CORAL REEF FORMATION

AND THE SCIENCES OF EARTH, LIFE, AND SEA, C. 1770-1952

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ABSTRACT

I argue that the search for a generally-applicable theory of coral reef formation began in the 1770s and that the pursuit of this type of explanation continued to orient reef research until 1952. The most influential (and still most famous) of these theories was the one proposed by Charles Darwin after the voyage of the *Beagle* (1831-1836), drawing on his knowledge of hydrography and the work of Alexander von Humboldt. I examine the sources and arguments of this and alternative theories, up to the moment when, by general consensus, Darwin's theory was proved correct by deep drilling on the atoll of Eniwetok [now Enewetak] in 1952. I interpret the Eniwetok drilling not as a straightforward proof of Darwin's theory, however, but as the moment when the principle that a single theory would explain all reefs was decisively undermined.

I show that reefs could not easily be classified by the categories of animal, vegetable and mineral, and living and fossil, that oriented much of the study of science, and use my long-term case study to examine the arrangements and re-arrangements of scientific disciplines with respect to these categories. By examining the different practical approaches to studying reef formation, moreover, I show how new "ways of knowing" were integrated with older ones in a continuous tradition of inquiry.

This dissertation analyzes the theories of reef and atoll formation presented by Johann Reinhold Forster on Captain James Cook's second Pacific voyage, Charles Lyell, Charles Darwin, James Dwight Dana of the United States Exploring Expedition, John Murray of the British Challenger expedition, and Americans Alexander Agassiz, Alfred Goldsborough Mayor, Thomas Wayland Vaughan, William Morris Davis, Reginald Aldworth Daly, and many more. The narrative culminates in work done at Bikini Atoll during Operation Crossroads (1946) and the Bikini Scientific Resurvey

(1947) by Harry Ladd, Joshua Tracey, Jr., and Roger Revelle, followed by the drilling at Eniwetok. I trace the role of coral reef science in the development and practice of the scientific disciplines of natural history, natural philosophy, zoology, geology, biology, geomorphology, physical geography or physiography, geophysics, and ecology.

ACKNOWLEDGEMENTS

I owe this dissertation to the help and encouragement of some extraordinarily generous people. The best thing about submitting it is having the opportunity to recognize and thank them publicly. I received many intriguing suggestions and far more good advice than I have been able to put into action. The shortcomings of the text that follows are my own.

Long enough ago for it to have been presumptuous on at least two counts, I told Laura Stark that I couldn't wait to acknowledge her in my dissertation. Now I know that even my greatest expectations underestimated her. I could hardly have conceived how much I would eventually learn from Laura about doing scholarly work, or how much she would let me rely on her while I struggled at various moments. Let me not delay any further in saying thank you.

I am honored to have had Angela Creager and Graham Burnett as co-advisers of my dissertation, and I thank them for working so closely and enthusiastically together on my behalf. Angela has shepherded me through graduate school with more care and patience than I could possibly have deserved. I am profoundly grateful for her sound advice, her ready feedback, her well-timed nudges, and her amazing skill at blending tough love with unwavering optimism. She has suffered to help me finish this dissertation, and I owe her everything. Moreover, if it had not been for her learned supervision and patronage of my work on the nuclear weapons tests at Bikini and Eniwetok, this dissertation would literally have been lacking its proper ending.

I originally became interested in writing about coral reef science during my first semester at Princeton, as a member of Graham's graduate seminar on "Science across the Seas." One Thursday in November 2002, in a pre-dawn class meeting on the morning that we all had to fly to the History of Science Society meeting in Milwaukee, we discussed an article by David Stoddart on Darwin's coral theory. The delirium of the early morning was broken when Graham became very animated, even by his tireless standards, in response to one of my questions. On the spot he charged me to write an essay examining the relation between Humboldt's zonation of mountainside flora and Darwin's notions about the growth of coral around an island. Many iterations later this piece has become chapter two of the present dissertation. Graham was my inspiration and my imagined audience for much of what follows, and I am deeply indebted to him for following through on the excitement and intellectual promise of that early seminar. .

The other two readers of my dissertation, Michael Gordin and Janet Browne, have not only given me valuable comments and advice for future work, they have given me attention, guidance, and intellectual support for as long as I've known them. I thank them very much for their labor on my behalf.

There are many others who have helped to make this dissertation possible. The members of the Department of History and Philosophy of Science at Indiana University introduced me to the field and encouraged me to pursue it beyond graduation. The faculty of my master's degree course in London convinced me to make it a career. I was tremendously fortunate to be designated an advisee of Andrew Warwick , who has remained my mentor ever since. He and Rob Iliffe taught me how one might actually be a historian of science. Simon Schaffer and Jim Secord have been cherished advisors to me before, during, and since my long research trip to Cambridge, UK in 2006. I wrote most of the text that has ended up in this dissertation in Evanston, IL and Washington, DC. I am profoundly grateful to Ken Alder at Northwestern University and Pam Henson at the Smithsonian Institution Archives for giving me wonderful places to work. Thanks as well to the members of Northwestern's Science in Human Culture program, my friends at the SIA, and the Inter-Library Loan staff of the Northwestern and Smithsonian libraries.

I have been blessed to share my time at Princeton with my cohort-mates Doogab Yi and Jeris Yruma and many others, in particular Lindy Baldwin, Daniela Bleichmar, Tom Boeve, Dan Bouk, James Byrne, Jamie Cohen-Cole, Andy Graybill, Ole Molvig, Tania Munz, Joe November, Nick Popper, Jeff Schwegman, Jason Sharples, Suman Seth, and Matt Wisnioski. I owe a debt of gratitude to everyone who has participated in the History of Science Program Seminar and to Linda Colley for teaching me about British history while I was a general exam student and teaching assistant. Goodness knows where I would be without the counsel and administrative help of Reagan Campbell, Tina Erdos, Minerva Fanfair, Vicky Glosson, Judy Hanson, Lynn Kratzer, Pamela Long, Debbie Macy, Audrey Mainzer, Etta Recke, and Amy Shortt in the History Department, and Elizabeth Bennett and the rest of the library staff. Many thanks also to Charles Gillispie, Mike Mahoney, and Helen Tilley in History of Science, Jim Gould and everyone else I met at Mathey College, and to my fellow members of the Princeton cycling team.

I have received enormous contributions of time and expertise from many other members of the field. At Cambridge I benefited from time spent with members of the HPS department, especially those in the Cabinet of Natural History, and help from staff at the University Library and the Whipple Library. I am enormously grateful to Alison Pearn, Paul White, and everyone else at the Darwin Correspondence Project for giving me the opportunity to search their full text database. While I was in Britain, I received specific help on this project from Jon Hodge, Greg Radick, Brian Rosen, and Martin Rudwick. Sarah Dry was the best fellow-dissertation-writer and friend imaginable. In this country, I was helped in every way by the unbelievably generous Ron Rainger, and I learned a lot from conversations with Rob Kohler, Lynn Nyhart, Michael Reidy, Helen Rozwadowski, the participants at the 2006 Dibner Institute “Oceans and Atmospheres” Seminar in the History of Biology at Woods Hole, and many, many others. I must say a special thanks to David Stoddart for the warm reception he gave me when I appeared in

Berkeley and told him that I was working on a dissertation prompted by his writings on the history of coral reef science.

There would be a lot missing from this dissertation without the inventive help I received from archivists at the repositories listed at the front of the bibliography, and I thank them profusely. I am especially grateful for the extraordinary services rendered by (in chronological order) Ellen Alers at the Smithsonian, Godfrey Waller and company at Cambridge, Tricia Boyd in Edinburgh, Professor Jim Kennedy and Stella Brecknell at Oxford, and Guy Hannaford and Ann Browne at Taunton.

Since I began working on this project, I have been heartbroken by the unexpected losses of Mike Mahoney, who guided me through the writing of my prospectus and much else; Peter Lipton, who welcomed me as a visiting student at Cambridge; and my uncle Topper Sponsel, who would have insisted on reading my dissertation.

My research and writing has been very generously supported by the Graduate School at Princeton University and the Dean's Fund for Scholarly Travel, Angela Creager (for inviting me along with her junior research seminar students to the National Archives), the NSF (for Dissertation Research Improvement Grant SES 05-22664), the Whiting Foundation, the History Department at Princeton, and major financial contributions from Vi and Gary Morris, Violet Wright, and my parents.

When I was in England I could always rely on my Uncle John and Auntie Doreen, Richard Redmond, and the Wyatt family for anything imaginable. Wherever I am, I draw encouragement and amusement from my splendid sister Heather Lytle and my great friend Zac Lytle. I have been buoyed by the love of many family members and old friends, and I can't wait to tell them that this is finished!

Nobody has cheered and helped me more than my parents, Valerie and William Sponsel. I cannot begin to put my gratitude into words. I dedicate this dissertation to them.

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Introduction

This is a history of efforts to explain the development of coral reefs, a tradition of inquiry born of the first "scientific explorations" of the South Seas in the late eighteenth century and continued unbroken into the Cold War. I have paid particular attention to the different approaches that were taken to answering the shared question, how are coral reefs formed?, and I have, as Martin Rudwick encourages, "let the documents themselves, and especially the correspondence, lead to a snowballing reconstitution of a cast list that is...founded on the actors' own perceptions."[1] My narrative follows an actual discourse between individuals who, despite their manifold disagreements, conceived themselves as part of a common enterprise.

This is an enterprise worth studying not just for its longevity, but because it provides a cross-section of the sciences as they were organized and reorganized. The actors who sought to explain coral reef formation shared no single disciplinary identity and had no institution or field site in common, which makes it possible to ask whether particular "ways of knowing" had histories that cut across those of individual disciplines. While those who debated reef origins formed a methodologically and philosophically diverse group, it was small enough to allow me to produce an archive-based history that analyzes a high proportion of the members' activities and discussions while bridging the eras of gentlemanly natural philosophy and so-called big science.

In working with manuscripts and published primary sources, I have sought evidence of the specific research questions, practices, and implements that shaped my actors' interpretations of coral reefs. When possible I have studied notes made in the field and at other "private" sites where reef knowledge was generated. Given the evident care with which my actors sometimes crafted later accounts of their motivations

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1. Martin J.S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge Among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985), 9.

for, and execution of, research into coral reefs, I have tried to be circumspect in the instances when I was forced to base my analysis exclusively on publications or reminiscences.[2] There were several types of information that I sought to ascertain about each of my actors using whatever sources I did have available. What was an individual's training and how was he introduced to the debates over coral reefs?[3] When an author introduced a new "theory" of coral reefs, what precisely was the question to which this theory was meant to provide an answer?[4] Where, how, and for how long did each actor try to solve his personal version of the coral reef question? What factors made the study of reef formation a compelling or profitable occupation? As I will begin to explain below, these components of scientific practice lend themselves to comparison, which helps to reveal changes and continuities over time.[5]

It may be useful to sketch this history from beginning to end. The first effort to explain the origin of the "low islands," now called atolls, was made by J.R. Forster, the philosopher on James Cook's second Pacific voyage, who proposed in 1778 that these lagoon-encircling islands were built up by "animalcules" from the ocean floor. The notion that living creatures could spontaneously produce habitable land was dealt a blow by the French zoologists Quoy and Gaimard, who demonstrated that Forster's animalcules only grew at very limited depths. This led to the formulation of an endur-

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2. I have been particularly stimulated in this regard by the critical analyses of published work (versus examination scripts and laboratory notebooks, respectively) by Andrew Warwick and Gerald Geison. See Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003), chapter 1; Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton: Princeton University Press, 1995).
3. In no case in this periods that I have been able to identify did a woman publish on the question of coral reef formation. It seems likely that one particular barrier to female participation in this topic was that there were limited opportunities for reef fieldwork beyond participation in all-male naval expeditions.
4. Andrew Warwick has constantly encouraged me to keep this query in mind.
5. Several authors have lately called for attention to such "elements" of scientific practice as a means for drawing general meaning from case studies. Notable examples are Robert E. Kohler, "A Generalist's Vision," *Isis* 96 (2005): 224–29; David Kaiser, "Training and the Generalist's Vision in the History of Science," *Isis* 96 (2005): 244–51; John V. Pickstone, "Working Knowledges Before and After Circa 1800: Practices and Disciplines in the History of Science, Technology and Medicine," *Isis* 98 (2007): 489–516.

ing two-part question: how did corals that could only live in shallow water establish reefs that stood in the deepest parts of the ocean, and why did they form a ring-like shape that left a lagoon in the center? Quoy and Gaimard themselves proposed a widely adopted idea that every atoll had formed atop a scarcely-submerged volcano crater. These questions were so fascinating to men of science, and of such urgency to navigators who feared that newly formed reefs would make their charts obsolete, that in the 1830s the British Admiralty began to instruct commanders of surveying vessels like the *Beagle* to study the formation of coral islands.

Charles Darwin's experience on the *Beagle* led him to argue that atolls were formed when reef fringed islands sank while new coral growth kept the reef's surface near sea level. He claimed that barrier reefs, which encircled an island or continent at some distance, were intermediate stages in the process by which fringing reefs were converted to atolls, and that his theory therefore explained the form of virtually every coral reef on earth. Thus in his answer to the widely acknowledged puzzle of atoll formation, Darwin redefined coral reefs as unique records of earth's history, with implications for understanding the rate of geological change, the stability of oceans and continents, and the distribution of species. After the publication of the *Origin of Species* by Darwin, and of several competing explanations for reef formation by a new generation of voyagers, participants on both sides of the debate over evolution fixed on the so-called "coral reef problem" when they sought an empirical test of Darwin's version of past change. The subsidence theory of atoll formation predicted that a reef would be composed of shallow water corals, even thousands of feet beneath sea level. The coral reef problem became so central to late Victorian scientific debates that the Royal Society of London established a Coral Reef Committee, which programmed three expeditions in the 1890s to attempt the "experiment" of drilling down to the foundation of the atoll of Funafuti in the Ellice Islands. No single interpretation of the results proved convincing to all, however, and the coral reef problem remained controversial for yet another very active generation of scholars.

Two American geologists, Harry Ladd and Joshua Tracey, called the period from 1910-1939 the Thirty Years' War, referring to the increased tensions between competing styles of reef work and the standoff over Darwin's mechanism of subsidence. At Bikini Atoll in 1946-1947 Ladd and Tracey participated in the first US nuclear weapons test in the Pacific, helping to make a massive physical and organic survey that stood as the most comprehensive atoll study yet undertaken. Building on the promising results of a deep boring conducted at Bikini, Ladd in 1952 drilled 4,152 feet to the basement of the neighboring atoll of Eniwetok (itself then a nuclear test site), in a feat that was and is widely taken to have proved Darwin's theory.

This study is the first attempt to examine the full chronological span of the coral reef problem in the present detail. Reef science is a topic with a relatively small historical literature composed almost entirely of biographical writings and a few short case studies of a particular individual or expedition. Because few of these works engage with other secondary sources on the topic, moreover, this collection of material can be said to form a *literature* on the history of coral reef science in only the loosest sense. My work should enhance the collective value of these existing contributions by showing their relevance to a longer continuous history. I have also substantially expanded and diversified the literature's empirical underpinnings by giving giving the first manuscipt-based account of many important sections of the story.

Among the secondary works on coral reef science, the most important subset consists of those by David Stoddart, a distinguished reef geographer who also pioneered the historical study of Darwin's coral-related manuscripts.[6] He has written several articles analyzing the origin and publication of Darwin's coral theory and his relations with

6. For his transcription and analysis of Darwin's 1835 "Coral Islands" essay, see David R. Stoddart, ed., "Coral Islands by Charles Darwin," *Atoll Research Bulletin* 88 (1962): 1–20.

fellow reef enthusiasts Charles Lyell and James Dwight Dana, and three others on later episodes of the use, opposition, and support of that theory.[7] While Stoddart has brought a coral reef perspective to the history of Darwin, many Darwin scholars have analyzed the *Beagle* voyager's coral work as part of their wider efforts to understand his biography and intellectual development.[8] Generally speaking, however, the biographies of scientists who sought to explain coral reef origins (and my notes will reveal that I have made grateful use of many of them) are unreliable sources of information on the broader history of the coral reef problem even during their subject's lifetime.[9] Few

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7. David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs," *British Journal for the History of Science* 9 (1976): 199–218; David R. Stoddart, "Grandeur in This View of Life: Darwin and the Ocean World," *Bulletin of Marine Science* 33 (1983): 521–27; David R. Stoddart, "Darwin and the Seeing Eye: Iconography and Meaning in the *Beagle* Years," *Earth Sciences History* 14 (1995): 3–22; David R. Stoddart, "'This Coral Episode': Darwin, Dana, and the Coral Reefs of the Pacific," in *Darwin's Laboratory: Evolutionary Theory and Natural History in the Pacific*, eds Roy M. MacLeod and Philip F. Rehbock (Honolulu: Univ. of Hawai'i Press, 1994), 21–48; David R. Stoddart, "Joseph Beete Jukes, the 'Cambridge Connection,' and the Theory of Reef Development in Australia in the 19th Century," *Earth Sciences History* 7 (1988): 99–110; David R. Stoddart, "The Duke, the Professor, and the Great Coral Reef Controversy of 1887–1888," *Earth Sciences History* 7 (1988): 90–98; David R. Stoddart, "Theory and Reality: The Success and Failure of the Deductive Method in Coral Reef Studies--Darwin to Davis," *Earth Sciences History* 13 (1994): 21–34. Other noteworthy articles on episodes relating to the coral reef problem are Roy M. MacLeod, "Imperial Reflections in the Southern Seas: The Funafuti Expeditions, 1896–1904," in *Nature in Its Greatest Extent: Western Science in the Pacific*, ed. Roy MacLeod, and Philip F. Rehbock (Honolulu: University of Hawaii Press, 1988); Ronald Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," *Historical Studies in the Physical and Biological Sciences* 30, no. 2 (2000): 349–71; Daniel E. Appleman, "James Dwight Dana and Pacific Geology," in *Magnificent Voyagers*, eds Herman J. Viola and Carolyn Margolis (Washington, D.C.: Smithsonian Institution Press, 1985), 89–118.
8. See in particular Sandra Herbert, *Charles Darwin, Geologist* (Ithaca, NY: Cornell University Press, 2005); Patrick Armstrong, *Darwin's Other Islands* (New York: Continuum, 2004), and the earlier works of both scholars cited in the thesis. Also noteworthy are William Montgomery, "Charles Darwin's Theory of Coral Reefs and the Problem of the Chalk," *Earth Sciences History: Journal of the History of the Earth Sciences Society*, 1988, 7 1988; Jane R. Camerini, "Darwin, Wallace, and Maps," Ph.D. thesis (University of Wisconsin, 1987); Frederick Burkhardt, "Darwin's Early Notes on Coral Reef Formation," *Earth Sciences History: Journal of the History of the Earth Sciences Society*, 1984, 3 1984; Michael T. Ghiselin, *The Triumph of the Darwinian Method* (Mineola, NY: Dover, 2003).
9. There are several practical problems that are common to many of the biographies and memorials I have consulted, and which appear to derive primarily from their authors' lack of familiarity with the coral reef problem except via their subject's own work. This tends to encourage a one-sided view of disagreements and occasionally produces factual mistakes about rivals' work. It has also tends to make the biographical subjects appear to be on one extreme of a polarized discourse, sometimes inferring allegiance between two scientists who happen to share a common rival. Many of these flaws are to be found in the otherwise extremely useful biography of A.G. Mayor written by authors

works seek to examine the topic over the course of a timespan longer than a normal scientific career.[10] One noteworthy exception to this trend is a book by James Bowen and Margarita Bowen, which contains a long and occasionally very detailed historical section on coral reef science as it related to the Great Barrier Reef of Australia.[11] Several useful sources focus on the history of a single voyage.

These disparate works for the most part agree on a few general points. One is that Darwin's theory had an enduring role in the coral reef debates, remaining viable (though not always preeminent) from 1842 to the present day. The second is that a small number of key events were the main drivers of the history of the coral reef question, notably the publication of Darwin's theory, the controversy following the *Challenger* expedition of 1872-1876, and the Funafuti borings of the 1890s. The Glacial-control theory of reef formation, advanced in the 1910s by the American geologist R.A. Daly, is sometimes mentioned as important but has not been explored in depth. Finally, there is utter consensus that the debate over reef origins was unsettled until the boring at Eniwetok, which resolved matters by proving Darwin right. Indeed, it has become common for papers and books on Darwin's coral reef theory to include a coda about Eniwetok, even if they include no other mention of events in the twentieth century. This convention, combined with the general lack of attention to the period after 1897 save occasional mentions of Daly, suggests that the debate was largely dormant in the years

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evidently more familiar his other main field of research, the jellyfishes. Lester D. Stephens and Dale R. Calder, *Seafaring Scientist: Alfred Goldsborough Mayor, Pioneer in Marine Biology* (Columbia, SC: University of South Carolina Press, 2006).

10. An exception is the brief survey Daphne G. Fautin, "Beyond Darwin: Coral Reef Research in the Twentieth Century," in *Oceanographic History: The Pacific and Beyond*, eds Keith R. Benson and Philip F. Rehbock (Seattle: University of Washington Press, 2002), 446–49.

11. James Bowen and Margarita Bowen, *The Great Barrier Reef: History, Science, Heritage* (New York: Cambridge University Press, 2002), 173–282 On scientific research at the Great Barrier Reef, see also O.A. Jones, "The Great Barrier Reef Committee: Its Work and Achievements, 1922–66," *Australian Natural History* 15, no. 10 (1967): 315–18; Dorothy Hill, "The Great Barrier Reef Committee, 1922–82. Part I: The First Thirty Years," *Historical Records of Australian Science* 6, no. 1 (1984): 1–18; Dorothy Hill, "The Great Barrier Reef Committee, 1922–1982. Part 2: The Last Three Decades," *Historical Records of Australian Science* 6, no. 2 (1985): 195–221.

between Funafuti and Eniwetok.[12] This in turn leaves it far from clear how the Eniwetok borings came about, except that in the atomic age, "big science" simply swept in to spontaneously provide a technological solution to a puzzle that once vexed some gentlemen of nineteenth century science. In the present study it becomes clear that there were specific reasons why Eniwetok was drilled for an answer to the coral reef question, as well as myriad solutions to other puzzles posed by the existing histories of coral reef science.

My chapters proceed chronologically rather than thematically, and I continually seek to understand the coral reef debate as my actors understood it. I examine their frequent recourses to hindsight, but I try to avoid looking beyond what was for them the present tense. Therefore readers may find it helpful if I use the introduction to forecast several of the themes that will recur through some or all of the dissertation.

The foremost theme is the changing status of theory in coral reef science. It is abundantly clear that an individual's idea of how coral reefs were formed was an important factor in determining his approach to the study of reefs and his notion of what made a legitimate scientific problem. Already by the beginning of the nineteenth century, few voyagers sailed toward a coral reef without already knowing a theory of what it was and how it had been formed. No reader in a European or American library could encounter a description of a reef that did not incorporate some accounting of its origin. As I detail in chapter one, "coral reef" was from its inception both a descriptive and an explanatory

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12. Though on the pre-Darwin era, see the long discussions in Leopold Böttger, "Geschichtliche Darwstellung unserer Kentnisse und Meinungen von den Korallenbauen," *Zeitschrift für Naturwissenschaften* 63 (1890): 241–304; Siegmund Günther, "Die Korallenbauten als Objekt wissenschaftlicher Forschung in der Zeit vor Darwin," *Sitzungsberichte der mathematisch-physikalishcen Klasse der K.B Akademie der Wissenschaften zu München*, no. 14 (1910): 1–42. The best English-language summary of the published primary material is probably the opening pages of David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs", which cites these sources.

term, and theorizing about reef formation was not necessarily the antithesis of practical investigation.

I argue that the philosophy of coral reef theories had a historical arc with shifting implications for how the origin of reefs should be studied. J.R. Forster, who was the natural philosopher on Cook's second voyage and who wrote one of the first *thematic* (rather than chronological) travel narratives (1778), introduced the premise that the low islands—later called "atolls"—formed a natural type whose members originated by a common cause. Charles Darwin proposed (1842) a developmental history of atolls that linked their formation to that of barrier reefs and shallow-water reefs, which raised the standard for any prospective coral reef theorist by demanding that a single theory must be able to account for the origin of virtually all coral reefs. In the late-nineteenth and early-twentieth centuries several theories emerged as direct competitors to Darwin's, each in one way or another suggesting a general account of reef formation. This contest of generally-applicable theories fueled the notion that any given reef, if interrogated correctly, might serve up a decisive testimony. Because the competing theories happened to make different predictions as to the depth and makeup of an atoll's foundation, the ultimate test would be to bore a hole all the way through an atoll and recover a sample of whatever lay beneath the coral rock. Despite many drilling efforts, most famously those at Funafuti in the 1890s, this standard was not met until 1952 at Eniwetok. Other types of evidence collected in the meantime, however, made it impossible to consider Eniwetok a sufficient test case for the formation of all reefs, or even, perhaps, all atolls. Thus Eniwetok proved in retrospect not to have been a crucial experiment between different general theories of reef formation, but between the ideals of the general theory and the locally-specific explanation.

It is worth reviewing what was at stake in these contests as well. Not to be overlooked was the lure of solving a puzzle that had exercised many of the greatest savants of the previous generation. This applied as well in 1840, when Darwin could look back at solutions offered by voyagers like Forster and Adalbert von Chamisso and by theorists like Lamarck and Lyell, as in 1940, when reef workers saw themselves as heirs to Darwin, James Dwight Dana, and Agassiz père et fils. Such prestige, substantial as it might have been, does not explain the sustained importance of this particular problem, though. The larger issues at stake varied by participant and changed over time. For explorers and navigators of the tropical oceans, coral islands were at once a threat and a haven. To understand their growth would be to predict where and when these inconspicuous obstacles might be encountered, and to improve access to the calm anchorages and fresh water that they might provide to those who approached knowingly. For natural philosophers who wanted to understand the balanced "oeconomy" of the globe, establishing the origin of reefs would determine whether the growth of coral animals counterbalanced the deterioration of the earth's physical features. For geologists, the different theories ascendant in the 1830s, 1840s, and 1850s implied that atolls held the key to understanding, respectively, submarine volcanism, the systematic movements of the earth's crust, and the lifespan of oceans and continents. For eighteenth century naturalists the various shapes of coral reefs shed light on lower animals' instinctive reactions to physical stimuli; for their nineteenth century counterparts in zoology and botany, unraveling the history of coral islands would explain how organisms had been dispersed across the Pacific Ocean. For some late-Victorians on either side of the debate over evolution, a direct test of Darwin's coral theory was to be a referendum on the merits of all Darwinism. There were early-twentieth century geologists who thought that reef studies were the way to decode the fluctuations of climate and sea level in the last Ice Age, physiologists of the time believed that reef formations marked the rate of coral metabolism.

These widely ranging and constantly changing concerns called for (and relied on) a variety of approaches to they study of coral reefs. It might seem to be a given that the history of reef studies start and end with field research, but this is hardly the case. I demonstrate that throughout this long period, field study of reefs was only one of many available strategies for answering the coral reef question. Many fieldworkers supplemented their observations by viewing maps, charts, and the reports of other travelers, but some other scholars believed that the systematic examination of reefs on paper was the only reliable route to a generally-applicable theory of reef formation. Some cabinet-based reef workers were concerned with the distribution of reefs across the globe, others with their form or relation to other types of land. Meanwhile, studying coral reefs in nature was far from a unitary method. Not only were travelers divided by their preferences to study the animate constituents of a reef or the rocks they left behind, they also had divergent ideas on whether to prefer the evidence offered by fossilized reefs on dry land, which were amply displayed but eroded, or the living reefs that were intact but veiled by water. Some field workers chose to make close studies of a single reef; others to view as many as possible in the time available to them. Then they had to determine how to approach and examine the reefs with the tools available. In this long history, some studied the undersea profile of the reef with a lead-line; others with echo-soundings. Some dredged for deep-water specimens, some drilled into the reef, some used depth charges to make seismic surveys. When Charles Darwin made several reef traverses in 1836, he noticed the continuous change in its living constituents; Alfred Goldsborough Mayer did the same in the 1910s, but he quantified his results by surveying quadrats at points spaced along the line, and he extended the line by going underwater in a diving helmet.

It is now common to study past science by making micro-historical studies of such practices, as employed at one institution or within one discipline during a period of years or decades. Meanwhile, a handful of recent works has offered a more extended chronological account of developments in modern science based on the lessons of such case studies. A common conclusion of such surveys is that that the practice of science since the mid-eighteenth century has been characterized by a small set of distinct approaches to knowing nature. These were not totalizing paradigms, we are told, and they do not map directly onto the histories of individual scientific disciplines. John Pickstone, for example, identifies three "ways of knowing," natural history, analysis, and experimentalism, whose relative importance varied between different disciplines and at different periods in their individual histories.[13] Covering roughly the same chronology, Lorraine Daston and Peter Galison trace the histories of three "codes of epistemic virtue" that guided scientific representations of nature: truth-to-nature, mechanical objectivity, and trained judgment.[14] These were invented in turn and added to scientists' collective repertoire of knowledge making practices, each available but not simultaneously maximizable. Because these epistemic virtues did not replace each other wholesale, the making of modern science should not be viewed as the result of a series of zero-sum ruptures or paradigm shifts but as a story of continuity based on the accumulation and recombination of a limited set of knowledge-making practices.[15] As Pickstone argues, in "the 'longue durée' histories of various ways of know-ing...revolutionary changes in science may *displace* previous ways of knowing, but they do not wholly *replace* them."[16] How they may have complemented, rather than replaced, one another remains largely obscure, however, because these authors tend to

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13. John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Chicago: University of Chicago Press, 2001).
14. Lorraine Daston and Peter Galison, *Objectivity* (Brooklyn: Zone Books, 2007).
15. Pickstone and Daston and Galison are here building upon Thomas Kuhn's concepts of the paradigm and of "scientific traditions," and Michel Foucault's "archaeology of knowledge" (Pickstone more explicitly), but rejecting the notion that new paradigms or epistemes have entirely replaced older ones. See Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1996 [Third edition]); Thomas S. Kuhn, "Mathematical Versus Experimental Traditions in the Development of Physical Science," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 311–65; Michel Foucault, *The Order of Things* (New York: Vintage, 1970).

focus, at a given chronological moment, almost exclusively on scientists and sciences who embraced the newest epistemic virtue and often explicitly rejected the old. So while Daston and Galison tell us that it was the lasting viability of older epistemic virtues that made the history of modern science one of continuity rather than revolution, they do not show this act of integration in practice. If they have not quite recapitulated the positivist narrative of modern science from Linnaeus to particle physics, they have at least implied that eras in the history of science should be characterized in terms of the sciences then in the ascendant.

This dissertation asks how the history of knowledge-making practices would look when viewed from the perspective of a single scientific problem whose solution remained unsettled throughout the rise of modern science. Here, of course, I am modifying a favorite strategy of microhistory and other cultural studies of scientific knowledge. Nearly twenty-five years ago Martin Rudwick wrote, "What are needed, for a fuller understanding of the processes by which scientific knowledge is shaped, are empirical studies of science in the making...which focus not on one individual scientist but on a specific scientific *problem* that brought together some *group* of individuals in an interacting network of exchange."[17] Rudwick amply demonstrates that such microscopic examination of an intense episode like the Devonian Controversy of the 1830s may shed enormous light on the way in which scientific knowledge was shaped at a given time. In the present study I inquire into the long-term changes and continuities in the production of scientific knowledge by examining a controversy that remained vibrant for generations.[18] I find that a few particular ways of knowing reefs did indeed have great

16. Pickstone, *Ways of Knowing*, 25.
17. Rudwick, *The Great Devonian Controversy*, 6.
18. Of course, this presents a challenge that I have met imperfectly. As James Secord has noted, regarding the difficulty of following the Cambrian-Silurian dispute in British geology beyond the 1860s, "I shall not follow the detailed story of the debate into this later period. The relative scarcity of manuscripts, the increasingly international character of geology, the volume of publication, and the large numbers engaged in the study of the Lower Palaeozoic would dictate a different method of treatment from that used here." I have, of course, had to adapt my own methods to the deal with the varying scope and type of sources available from different periods. James A. Secord, *Controversy in Victorian Geology: The Cambrian-Silurian Dispute* (Princeton: Princeton University Press,

longevity from at least the early nineteenth to the mid-twentieth century, while there were shifts in the disciplinary identities of those who asked particular types of questions.

I therefore intend this study not only for those interested in the particular history of coral reef science, but also for those concerned with the development of new sciences (and their interrelations) and the histories of the individual disciplines or traditions from which coral reefs were studied, particularly natural history, zoology, biology, and geology. There are other audiences to which I want to direct this dissertion as well. The concern with "place" in the history of science is now a rather general phenomenon, but I hope to engage those interested in the topic in a few specific ways.[19] I attend to the relation between specific *types* of locations (laboratories, museums, observatories, map rooms) and particular forms of inquiry. Much work on this topic has been aimed at muddying common assumptions, for example that experiments have been conducted exclusively in laboratories, often by demonstrating that these stereotypes had origins in the programmatic claims and accusations of discipline builders; I add testimony on both counts.[20] However, in describing the construction of laboratories on extremely remote coral reefs and showing how voyagers exploited their ships' libraries on the way from one stop to the next, I also explore how these scientific places, and the forms of knowledge and credibility that inhabited them, were often re-placed into fertile new terroirs. This was distinct from the practice of declaring specific field locations "natural

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1986), 299.

19. A survey of recent ideas on the geography of science is David N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago: University of Chicago Press, 2003).

20. As examples of this literature, see Lynn K. Nyhart, "Natural History and the 'new' Biology," in *Cultures of Natural History*, ed. N. Jardine, J.A. Secord and E.C. Spary (Cambridge: Cambridge University Press, 1996), 426–43; Karin D. Knorr Cetina, "The Couch, the Cathedral and the Lab: On the Relationship Between Experiment and Laboratory Science," in *Science as Practice and Culture*, ed. Andrew Pickering (Chicago: Chicago University Press, 1992), 113–38; Dorinda Outram, "New Spaces in Natural History," in *Cultures of Natural History*, ed. N. Jardine, J.A. Secord and E.C. Spary (Cambridge: Cambridge University Press, 1996), 249–65; Robert E. Kohler, *Landscapes & Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002).

laboratories," which was common in this history from the late-nineteenth century onward. (It was also common for my actors to describe virtually any technically challenging attempt to gather information--notably the practice of reef drilling from the 1830s to the 1940s--as an experiment.[21]) I also contribute to the lately-burgeoning literature on the history of a larger space, the ocean in general, as a scene for and subject of scientific inquiry, and on the histories of scientific voyaging and exploration.[22]

It remains to indicate the periodization of the chapters. Chapter one explains how atolls came to the attention of European naturalists in the eighteenth century and how the question of their formation became a matter of standing interest for men of science and naval administrators by 1830. Chapters two and three together constitute a very detailed revisionist history of Darwin's coral reef work, private and public, up to the publication of his 1842 book, *The Structure and Distribution of Coral Reefs*. I was encouraged to zoom in so closely on a few short years of this long history by the extreme opportunities and obligations posed by the extraordinary Darwin literature. However, this level of detail also serves a purpose in the longer story because Darwin's theory was to be examined repeatedly and minutely in the years ahead. Chapter four covers the period from the U.S. Exploring Expedition (1838-1842) to the publication of the final report on the Funafuti expeditions in 1904. This chapter traces a rise and decline in Darwin's theory, as judged by later voyagers, and shows that the greatest sup-

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21. Helen Rozwadowski has showed that the same term was commonly used in the mid-nineteenth century to describe deep-sea sounding attempts and I did so unintendingly for solar eclipse observations in the early twentieth century. Helen M. Rozwadowski, *Fathoming the Ocean: The Discovery and Exploration of the Deep Sea* (Cambridge, MA: Harvard University Press, 2005); Alistair Sponsel, "Constructing a 'Revolution in Science:' the Campaign to Promote a Favourable Reception for the 1919 Solar Eclipse Expeditions," *British Journal for the History of Science* 35 (2002): 439–67.
22. The most important recent contribution to "ocean history" is Rozwadowski, *Fathoming the Ocean*. My work will also add to histories of the Pacific Ocean in specific as a site and a conceptual space for science, on which see Roy MacLeod, and Philip F. Rehbock, ed., *Nature in Its Greatest Extent: Western Science in the Pacific* (Honolulu: University of Hawaii Press, 1988); Roy M. MacLeod and Philip F. Rehbock, eds, *Darwin's Laboratory: Evolutionary Theory and Natural History in the Pacific* (Honolulu: University of Hawaii Press, 1994); Keith Rodney Benson and Philip F. Rehbock, *Oceanographic History: The Pacific and Beyond* (Seattle ; London: University of Washington Press, 2002).

port for Darwin's theory came from geologists who had not necessarily seen any coral reefs. In the fifth and final chapter, which takes the story to 1952, I demonstrate that pre-World War II reef students often clashed over the value of fieldwork and the relative value of biology and geology in solving the coral reef problem. I reveal the specific pathways by which the prewar reef debate led to the drillings at Bikini and Eniwetok, and illustrate how the larger reef studies of which the drillings were part resembled the so-called intensive strain of prewar reef study in ideals and execution. In the conclusion I examine how and for whom Eniwetok did bring about the end of the coral reef problem, and use this as an avenue to reflect on the long history of the debate.

CHAPTER 1
How Coral Reefs Became a Problem for European Savants, 1778-1831

Introduction

During the Pacific voyages of Bougainville and Cook in the late 1760s, the coral reefs of the South Sea were only a problem for those Europeans whose ships might run aground on one. By the beginning of the 1830s, however, these marine formations were widely recognized as mysteries of nature whose very existence presented a puzzle to be solved by thinkers on sea and land. The study of coral reef formation was by then a well known and important feature of travel to the tropical Pacific, and the questions associated with reefs were sufficiently conventional to be debated by European savants who had never seen one themselves. Over the course of these six decades, then, coral reefs entered the consciousness of geologists and naturalists as well as navigators and became defined as a distinct class of phenomena that had to be analyzed and not just documented.[1] This chapter explains how the coral reefs of the Pacific were first classified and made into the target of systematic investigation.[2]

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1. For brief reviews of the present topic, see David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs"; Karl Alfred von Zittel, *History of Geology and Palaeontology to the End of the Nineteenth Century* (London: Charles Scribner's Sons, 1901), 245–49.
2. To use Lorraine Daston's term, this chapter examines the birth of coral reefs as a "scientific object." I have also been stimulated by Hans Jorg Rheinberger's similar notion of an "epistemic thing." Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford, Calif.: Stanford University Press, 1997); Lorraine Daston, ed., *Biographies of Scientific Objects* (Chicago: University of Chicago Press, 2000).

The tradition of inquiry into reef origins whose beginning I examine in this chapter emerged from the Pacific Ocean in particular, and its rise coincided with the acceleration of European exploration in the Pacific beginning in the latter half of the eighteenth century. The distinctive way that the Pacific was explored mattered to the way that these islands were encounterred and understood, and the particularities of certain Pacific reefs turned out to be important to general ideas about landforms well beyond the South Sea. The increase of interest in coral reefs was doubtless encouraged by the greater European presence in the tropical Pacific in the decades after Bougainville and Cook. The ways that this interest was expressed surely owed much to the new expectation that state-funded voyages of exploration should include "scientific" personnel.[3]

One such scientific traveler was universally credited with bringing philosophical attention to bear for the first time on the formation of reefs. This was J.R. Forster, who held the position of naturalist on the second of Cook's three Pacific voyages (1772-1775). I will argue that the subsequent debate retained Forster's taxonomy of Pacific islands (1778) and his assumption that a common cause could be found for the origin of each island within a given class. The narrative of this chapter following my opening section on Forster will be oriented around a series of voyages. Chief among them are the 1815-1818 circumnavigation by Otto von Kotzebue and the French circumnavigation under Louis-Claude de Freycinet, each of which carried a pair of individuals who would contribute to the coral reef debate. I argue that the physical differences between the islands

3. The reasons for both of these trends, and the distinctive ways they were manifested in British and French voyages, have been discussed extensively elsewhere. See, for example, John Dunmore, *French Explorers in the Pacific*, 2 vols (Oxford: Clarendon Press, 1965), vol. 1, 31–53 and vol. 2, 3–8, 43–62; Pieter van der Merwe, ed., *Science in the French and British Navies, 1700–1850* (Greenwich: National Maritime Museum, 2003); Robert A. Stafford, "Exploration and Empire," in *Oxford History of the British Empire*, vol. V, ed. Robin W. Winks (Oxford: Oxford University Press, 1999), 290–302.

visited by Adelbert von Chamisso and J.F. Eschscholtz of the Kotzebue voyage, and J.-R. Quoy and Paul Gaimard with Freycinet, compared with those seen by Forster, helped to shape the nature of their disagreements with Forster's explanation.

Then I shift my attention to the Hydrographic Office of the British Admiralty, and argue that the assumption that low islands formed a natural type had important implications for navigators and the scientific gentlemen who accompanied them, for it meant that the production of knowledge about a particular coral island was simultaneously a contribution to the knowledge of innumerable known and unknown islands of the same type that lay across the tropics. Purportedly universal theories of coral island formation, in turn, compelled surveyors to turn their attention to particular features of the islands, which changed the standards for what it meant to know a given location. By 1840, hydrographers from a range of European nations had charted coral coastlines, interviewed natives, made magnetic studies, and identified underlying strata using well-drillers' apparatus, all with particular theories of island formation in mind. Forster's explanation for the low islands had been cast aside, but there was now a flourishing body of practical and theoretical knowledge created by individuals who were explicitly taking up Forster's claim that these islands demanded a scientific explanation.

I will describe three kinds of authors who contributed to this published discourse on the form and formation of coral reefs, and the works characteristic of each. The first were navigators like Otto von Kotzebue and Frederick William Beechey, who along with their officers produced charts that showed the location and form of coral islands, and books that gave narrative accounts of their surveys. The second group of authors were those who had visited coral reefs as "scientific gentlemen" aboard surveying vessels. Whether they were ships' surgeons (who were almost always members of the navy) or savants like Forster, these men produced detailed descriptions and conjectured histories of specific reefs. Building on the work of navigators and voyaging men of science was a third group of European authors who had not necessarily seen coral reefs themselves, but who discussed reefs in general within comprehensive works on the "theory of the earth" and, by the early nineteenth century, in systematic treatises in the science of geology .[4] I claim here that both the defining and the debating of coral reefs proceeded by the wide acknowledgement that certain features--in particular the frequency with which reefs were found to have a ring-like shape--were gradually acknowledged to be both characteristic of this class of objects and also theoretically problematic. All three types of authors came to view coral reefs as objects that demanded a causal explanation. Facts and ideas circulated between authors from all three of these groups during the first decades of the nineteenth century, so that there ceased to be any sharp distinction between observing coral reefs and theorizing about them. Hence the fact that hydrographic surveys of reefs--by all accounts an enterprise designed to produce accurate *locally specific* knowledge--was made to incorporate, and thus to conform, to theories of reef formation.

J.R. Forster and the role of corals in building reefs

The Enlightenment scientific voyagers Cook and Bougainville returned from the Pacific with charts showing that the ocean became suddenly shallow in areas inhabited

4. On "theories of the earth" as a literary genre in the late eighteenth century, and as the strain of thought from which a newly conceived science of "geology" emerged, see Martin J.S. Rudwick, *Bursting the Limits of Time: The Reconstruction of Geohistory in the Age of Revolution* (Chicago: University of Chicago Press, 2005), chapter 3.

by corals, fueling the imagination of philosophers and the terror of fellow navigators and the public.[5] Corals sat atop submarine walls springing from depths that were often literally unfathomable with the lengths of rope carried by eighteenth century explorers.[6] They were always steepest on their windward side, as if to deprive mariners under full sail of the slightest advance warning of a shoaling sea. Both Bougainville on his circumnavigation (1776-1779) and Cook on his first voyage (1768-1771) piloted through a cluster of atolls in the middle of the Pacific that Bougainville named the Dangerous Archipelago (now the Tuamotus), in reference to the peril of sailing among "low isles, surrounded with breakers and shoals."[7] Even near the mainland, these shallow areas of coral could be deadly. As Cook moved northward off the eastern shore of New Holland (Australia) on 11 June 1770, he recorded that a few minutes before eleven p.m., the depth was "17 [fathoms (102 feet)] and before the Man at the [sounding] lead could heave another cast the Ship Struck and stuck fast...upon the SE edge of a reef of Coral rocks."[8] He had discovered the Great Barrier Reef, a labyrinth from which the *Endeavour*'s crew needed two months to sail clear.[9]

Although both Cook and Bougainville referred to shallow areas near land as "coral banks" or "reefs of coral rock," neither had made any reference to corals in describing the composition of the low islands that stood barely visible in the open ocean J.R. Forster, the naturalist who zig-zagged the Pacific on Cook's second voyage was the first person to offer a theory of the origin of the low, lagoon islands and in doing so was

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5. L.A. de Bougainville, *A Voyage Round the World*, trans. J.R. Forster (London: J. Nourse and T. Davies, 1772). On the Bougainville expedition in the broader context of French exploration in the Pacific, see François Moureau, "Philosophes et marins français dans la Mer du Sud avant Baudin: l'exemple de Bougainville et de ses compagnons," *Australian Journal of French Studies* 41 (2004): 15–32.
6. Rozwadowski, *Fathoming the Ocean*, 32–33.
7. Bougainville, *A Voyage Round the World*, 208.
8. J.C. Beaglehole, ed., *The Journals of Captain James Cook on His Voyages of Discovery: Vol. 1, The Voyage of the* Endeavour, *1768–1771* (Cambridge: Cambridge University Press for the Hakluyt Society, 1955), 344.
9. J.C. Beaglehole, *The Life of Captain James Cook* (London: The Hakluyt Society, 1974), 236–46.

the first person to suggest that they were constructed by the growth of corals. Forster and his son, George, were brought aboard the *Resolution* as last-minute replacements for Joseph Banks and Daniel Solander, who had accompanied Cook as naturalists on the *Endeavour*. Forster's *Observations Made During a Voyage Round the World* (1778) was an unconventional publication, for its organization was thematic rather than chronological. Forster had intended to write a conventional narrative account of the voyage, and kept a private journal of his observations to serve as a basis for this text.[10] After he returned to Britain, however, the Admiralty denied Forster the opportunity to write the official history of the voyage, which meant that he was prohibited from authoring a competing chronology of the voyage. His response was to write the *Observations*, an extraordinarily diverse treatise on physical geography, natural history, and the ethnology of the humans encountered during the *Resolution* voyage.[11]

Cook's wide traverses of the tropical and southern Pacific during the second voyage, which were made in order to search for a purported southern continent, allowed Forster the opportunity to see a great number of islands. One of the enduring legacies of his *Observations* was the taxonomy of Pacific islands that Forster developed. The two main types of island were the high islands (such as Tahiti and Fiji in the tropics) and the low islands (such as those of the Dangerous Archipelago), which were only to be found in the tropics. Forster described the low islands as "narrow, low ledges of coral rocks, including in the middle a kind of lagoon."[12] The only parts that actually stood out of the

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10. The journal has been edited and published as Michael E. Hoare, ed., *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, 4 vols (London: The Hakluyt Society, 1982).

11. Nicholas Thomas, "Johann Reinhold Forster and His *Observations,*" in *Observations Made During a Voyage Round the World*, eds Nicholas Thomas, Harriet Guest, and Michael Dettelbach (Honolulu: University of Hawai'i Press, 1996), xv-xxii; Michael Dettelbach, "'A Kind of Linnaean Being': Forster and Eighteenth-Century Natural History," in *Observations Made During a Voyage Round the World*, eds Nicholas Thomas, Harriet Guest, and Michael Dettelbach (Honolulu: University of Hawai'i Press, 1996), lv-lxxiv.

12. J.R. Forster, *Observations Made During a Voyage Round the World, on Physical Geography, Natural History and Ethic Philosophy* (London: G. Robinson, 1778), 14.

water were “little sandy spots,” which were “here and there...somewhat elevated above [the] high-water mark, whereon coco nuts and a few other plants will thrive.”[13] In the places on the reef where no sand had been thrown up, he explained, “the ledge of rocks is so low, that the sea frequently flows over it at high and sometimes at low water.”[14]

Forster argued that the high and low islands were produced by entirely different causes. He claimed that high islands originated with the volcanic agency of “subterraneous fire.”[15] The low islands, on the other hand, he attributed to the growth of corals. To be clear, Forster was proposing that these reefs were not merely inhabited by corals near the surface, but consisted, through and through, of coral rock. Thus he considered low islands--atolls--“to be a production of the sea, or rather its inhabitants, the polype-like animals forming the lithophytes.”[16] He explained that “It is well known, and often remarked in the South Sea, that the animalcules forming the lithophytes...cannot live out of water, [and] never extend their struture higher than to the water[‘s] edge, at low water mark.”[17] Forster’s “animalcules” were what we might call *coral polyps*. “Lithophyte,” meaning a plant made out of stone, referred to the colony formed by their skeletons, or what is now generally called a *coral*.

There was nothing unconventional about arguing that coral rock itself was a product of living organisms. Forster had acknowledged that it was “well known” that animalcules formed lithophytes.[18] He considered their “materials” to be “a kind of lime mixed with some animal substance.”[19] Lithophytes could be “above 15 feet high,” and in Forster’s experience they were “commonly narrow below...[and] the more they grow, the more they spread above.”[20] Thus he had seen corals “expanding from a base of two

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13. Forster, *Observations*, 14–15.
14. Forster, *Observations*, 15.
15. Forster, *Observations*, 150–52.
16. Forster, *Observations*, 149.
17. Forster, *Observations*, 147.
18. Forster, *Observations*, 147.
19. Forster, *Observations*, 149.
20. Forster, *Observations*, 147.

or three feet, to 18 or 20 on the top," appearing like plants to "have as it were only one stalk."[21] Although this was offered as a general description, it seems to owe much to the appearance of the upraised corals he saw at Turtle Island in July 1774, which were "lithophytes of the abovementioned heighth and size."[22]

In stating that "The reef, or the first origin of these isles, is formed by the animalcules inhabiting the lithophytes," Forster did not make it entirely clear whether he considered reefs to be formed *like* lithophytes, or *of* lithophytes.[23] As he explained it, "animalcules raise their habitation gradually from a small base, always spreading more and more, in proportion as the structure grows higher."[24] Such a process would undoubtedly explain the appearance of lithophytes, branching upward and outward from a narrow trunk. However, he continued to say that "The animalcules inhabiting the lithophytes...raise their habitation within a little of the surface of the sea, which gradually throws shells, weeds, sand, small bits of corals, and other things on the tops of these coral rocks, and at last fairly raises them above water." This action, seemingly identical to the formation of an individual lithophyte, was his explanation for the formation of a reef.[25] The development of reefs along this trajectory was implied by the variety of low islands he had seen, for he claimed "I have seen these large structures in all stages, and of various extent."[26] The first of these stages was represented by "a considerable large circular reef, over which the sea broke every where, and no part of it above water."[27] At a later step in the development of low islands, "some parts, are above water."[28] In these cases, "the elevated parts are connected by reefs, some of

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21. Forster, *Observations*, 147.
22. Forster, *Observations*, 147.
23. Forster, *Observations*, 150.
24. Forster, *Observations*, 149.
25. Forster, *Observations*, 150.
26. Forster, *Observations*, 149.
27. Forster, *Observations*, 149.
28. Forster, *Observations*, 149.

which are dry at low-water, and others are constantly under water."[29] Thus, a fully developed low isle would have a continuous reef up to the level of low water, and some elevated parts--i.e., islands--atop it. These islands would be arrayed around a lagoon enclosed by the reef on which they stood. The dry land "consist[ed] of a soil formed by a sand of shells and coral rocks, mixed with a light black mould, produced from putrefied vegetables, and the dung of sea fowls"[30] Eventually the waves would "carry a coconut hither...and thus may all these low isles have become covered with the finest coco-nut trees."[31]

Forster went on to explain the most distinctive feature of the low islands, their ring-like shape. Because the annular form was a feature of the underlying reef, he argued, its cause lay in the behavior of the creatures who had built it. "The animalcules forming these reefs," he explained, "want to shelter their habitation from the impetuosity of the winds, and the power and rage of the ocean; but as, within the tropics, the winds blow commonly from one quarter, they, by instinct, endeavour to stretch only a ledge, within which is a lagoon, which is certainly entirely screened aganst the power of both."[32] Though he did not mention it in the text, his conviction on this count was almost certainly reinforced by the fact that the annular reefs of the Pacific were usually higher and more continuous on their windward sides, while the passages into lagoons were usually found to the leeward.[33] This consistent relationship between the direction of the wind and the orientation of the low island reefs seemed to ensure that the lagoon would be protected even if the reef were not complete. Thus, Forster concluded, "the method employed by the animalcules in building only narrow ledges of

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29. Forster, *Observations*, 149.
30. Forster, *Observations*, 149.
31. Forster, *Observations*, 150.
32. Forster, *Observations*, 150–51.
33. See, for example, Forster's notes in Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, vol. 2, 320–22.

coral rocks, to secure in their middle a calm and sheltered place...seems to me to be the most probable cause of THE ORIGIN of all THE TROPICAL LOW ISLES, over the whole South-Sea."[34]

Forster pointed out that low islands were surrounded by immeasurably deep water, which meant that despite their apparent insignificance from above sea level, they must have massive submarine structures. He justified this by explaining that "if the bottom of the sea is to be looked upon as land, these isles certainly are elevations or rising grounds."[35] This statement was not merely a semantic one; rather it reflected a genuine uncertainty in the late eighteenth century about the nature of the deep sea. As Forster explained in a later section, their best efforts to learn more about the bottom of the ocean had only reinforced the profoundness of their ignorance. "The Depth of the ocean is certainly one of the most remarkable circumstances," he concluded. "We now and then even out of sight of any land, tried to measure this depth; for instance in the year 1772 Sept. 5th. being near the line in 00º 52 North latitude, we could find no ground with 250 fathom[s (1500 feet) of line]."[36] Similar efforts elsewhere in the Indian and Pacific oceans, with between 150 and 210 fathoms of line, gave the same result, so that Forster indeed could do no more than speculate that there was solid ground beneath the water from which islands arose. He went on to argue that the high and low islands of the tropical Pacific, "as they are so near one another, and lying in the same direction...can be nothing but chains of [submarine] mountains."[37] Considering the apparently non-random distribution of high and low islands across the South Sea, he argued "If we consider this direction of isles or submarine mountains, it should seem they were designed to give greater solidity and strength to the compages of our globe."[38]

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34. Forster, *Observations*, 151.
35. Forster, *Observations*, 29.
36. Forster, *Observations*, 53.
37. Forster, *Observations*, 29.
38. Forster, *Observations*, 31.

The implication of such claims, taken in concert with his theory of the origin of low islands, was that lowly lithophytes were capable of producing, by their concerted action, enormous structures of rock that were similar in magnitude to the formations produced by subterraneous fire.

The theory Forster laid out in *Observations* contains inconsistencies that were apparent, as we shall see, to others who studied coral islands. He never gave a clear account of how the growth of a single lithophyte related to the growth of an entire reef. His ambiguous descriptions of upward and outward growth from a small foundation caused at least one nineteenth century reader to conclude that Forster had imagined an atoll as, in effect, a single massive lithophyte with a smaller diameter at the ocean floor than it had near sea level.[39] He had also pointed out in his taxonomy that many of the *high* islands of the tropical Pacific were encircled by reefs built of coral, without offering any explanation for why this might be so. It was also unclear, in *Observations*, whether the description of low and high islands as parts of a single chain of mountains was meant to imply that there was actually some connection between their forces that operated to produce them.

Forster's journal from the *Resolution* voyage reveals some of the details that contributed to his theory of island formation, and suggests that the views attributed to him by readers of the *Observations* were not entirely in keeping with his private ideas. He made his first reflections on low islands in August 1773, when they came "a breast of the small, low Isles discovered by the Endeavour, & Mr Bougainville."[40] Here in the Dangerous Archipelago, Forster described "a low Island with several Clusters of Trees on it, surrounded on its South & SW side by a reef extending from the Isle in the shape

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39. Zittel, *History of Geology and Palaeontology to the End of the Nineteenth Century*.
40. Entry for 10 August 1773. Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, Vol. 2, 320.

of half a moon & forming on the inside a large wide bason."[41] This early view of a low island with an interrupted reef may have guided him toward the idea that corals built progressively *around* the lagoon to enclose it (rather than the alternative possibility of forming an entire ring first and then raising it toward the surface). Three days later, having passed through to the (high) Society Islands, Forster described Maiatea (Mehetia) as "a high Peak with a flat top, [which] looks for all the world like a Hill, that has lost its summit by an eruption caused by an internal Conflagration." It resembled, therefore, "a great many high Islands...that I have seen, & which bear besides some other proofs of their Conflagration, as Lava & some other Slags, anochraceous, fertile, hot Soil, etc."[42] Having seen low and high islands juxtaposed in such a short time, he continued the entry by offering his first account of their formation:

> Thus we might account in a double manner for the formation of Isles. The high ones seem to be the work of fire & the low ones are the work of the Sea & its Inhabitants, for it is a well known fact that all the low Islands in the South-Seas are surrounded to the South & South East by a reef of rocks, which when examined, are nothing else but immense lumps of rocks of the *Lythophyta Class* viz. *Madrepores, Millepores* & *Cellepores*; on which afterwards the Shells, Oysters & Muscles form their banks & thus yearly increase & elevate these rocks; the winds carry with the waves from the bottom of the Sea Sand & Seaweeds on it, which together with the Dung of innumerable aquatic Fowls gradually form a layer of light but fertile Soil. The ridge of rocks to the North West is always formed first into Soil & between it & the SW. ridge there is frequently a bason. If the question be put, how it comes that the *Madrepores* form such circular or oval ridges of rocks; it seems to me that they do it by instinct, to shelter themselves the better against the Impetuosity & constancy of the SW winds: so that within the ridge there is allways a fine calm Bason, where they feel nothing of the Effects of the most blowing weather. But these remarks I put here down as mere Guesses & nothing else. He whosoever can say something more clever may do it, if he is able to support his Opinion by proofs.[43]

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41. Entry for 12 August 1773. The island was Marutea. Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, Vol. 2, 320–22.
42. Entry for 15 August 1773. Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, Vol. 2, 323.
43. Entry for 15 August 1773. Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, Vol. 2, 323–24.

This entry makes it clear that in August 1773, Forster already considered coral animals to be primary agents in the construction of reefs, and had in mind the connection between the direction of the trade winds and the shape of the low islands.

The following April, after having spent the southern summer on an exploration of the antarctic that took them farther south than any previous expedition, Cook brought the *Resolution* on a second sweep through the tropical Pacific .[44] As Forster recalled, "When we first came within the tropics in 1773, I applied particularly to study the constitution and nature of the tropical isles. When we visited them again in 1774, I added many more observations."[45] The private journal shows that with another opportunity to examine the South Sea islands, Forster was reassured of the merit of his earlier speculations. After seeing the geology inland at Dominica (Hiva Oa) in the Marquesas, he affirmed that "all the Islands in the South Sea are either formed by Earthquakes & Volcanos, or by the Coralls & submarine Animals; the first kind make the high Isles, the second the low ones."[46] Later that same month, viewing the low islands of Takaroa and Takapoto, Forster gave a new account of how animalcules would produce a circular reef; it was more detailed than any version he ever put into print. "Both [low islands] have the longer sides [oriented south-north] & the shorter ones [oriented east-west,] ie the long sides are exposed to the East-wind, which probably was the first origin of these curious Isles: for there being some little Elevation or inequality in the bottom of the Sea the Coral-Animals began to raise their large rocky branching out habitations; & as soon as the Animals came nearer to the surface of the Sea, they

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44. Cook reached latitude 71º 10' S on 31 January 1774. J.C. Beaglehole, ed., *The Journals of Captain James Cook on His Voyages of Discovery: Vol. 2, The Voyage of the* Resolution *and* Adventure, *1772–1775* (Cambridge: Cambridge University Press for the Hakluyt Society, 1961), lxxxv.
45. Forster, *Observations*, 148.
46. Entry for 7 April 1774. Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, Vol. 3, 485.

extended their branches more North, in order to shelter themselves against the Surf of the Sea, & at last this work formed a Lagoon in the middle."[47]

This passage makes it clear that Forster believed that some kind of pre-existing topographical feature of the sea floor determined the *location* of coral reefs, while the influence of the wind and water on the animalcules determined their *shape*. Forster's published version explained only the shape of circular reefs, and gave no indication that he believed there to be any necessary preconditions to their growth on the ocean floor. It is impossible to tell whether this reference to "some little Elevation or inequality in the bottom of the Sea" that underlay a coral reef was new in April 1774, or whether it was implicit in his previous accounts. It is also not clear whether he maintained this view implicitly through the publication of the *Observations*, or whether the had abandoned it by the end of the 1770s. It seems possible that Forster had these underlying features in mind as an explanation for why low islands were oriented within chains of submarine mountains that also included high islands. Certainly the upraised corals at Turtle Island, which he saw in July 1774, indicated to him that the seafloor beneath low islands was also subject to the subterraneous fire that caused earthquakes and produced high islands. These conjectures are only possible with access to Forster's journal, however. Ignorant even of the vague explanation in his private writings, many later readers of Forster's published works on coral reefs fixated on his apparent inability to explain why low islands were founded in any given place.

Nevertheless, the *Observations* held tantalizing implications about the importance of corals in the "oeconomy" of nature, suggesting that these animalcules were among the few agencies capable of remodeling the surface of the globe on a vast extent.[48] Forster had made it possible for European savants like te French zoologist

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47. Entry for 18 April 1774. Hoare, *The* Resolution *Journal of Johann Reinhold Forster, 1772–1775*, Vol. 3, 494.

48. On Forster's conception of "oeconomy," see Dettelbach, "'A Kind of Linnaean Being': Forster and

J.V.F. Lamouroux to contemplate the possibility that "reefs...may eventually block communication between the temperate zones of the two hemispheres."[49] As the sovereigns of Europe raced to master the Southern ocean, these concerns stimulated navigators and their philosophically minded shipmates into a wider range of reef studies. The British navigator Matthew Flinders, while extending Cook's survey of the Australian coast (1801-1803), concluded that "the reefs, which form so extraordinary a barrier to [northern] New South Wales...[extend] through 14º of latitude and 9º of longitude; which is not to be equalled in any other known part of the world."[50] Along with documenting the magnitude of corals' productions, Flinders offered a precise account of how successive generations might contribute to these structures. "It seems to me," he explained, "that when the animalcules which form the corals at the bottom of the ocean, cease to live, their structures adhere to each other, by virtue either of the glutinous remains within, or some property of salt water; and the interstices being gradually filled up with sand and broken pieces of coral washed by the sea, which also adhere, a mass of rock is at length formed. Future races of these animalcules erect their habitations upon the rising bank, and die in their turn, to increase, but principally to elevate, this monument of their wonderful labours."[51] Although the reefs Flinders encountered were not the ring shaped low islands that Forster had seen, he too saw evidence that coral polyps shared an "instinctive foresight" to coordinate their behavior against the power of the wind. He argued that "[t]he care taken to work perpendicularly in the early stages, would mark a surprising instinct in these diminutive creatures. Their wall of coral, for the most part in situations where the winds are constant, being arrived

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Eighteenth-Century Natural History".

49. Jean Vincent Félix Lamouroux, *Histoire Des Polypiers Coralligènes Flexibles, Vulgairement Nommés Zoophytes* (Caen: F. Poisson, 1816), lix. All translations are my own.

50. Matthew Flinders, *A Voyage to Terra Australis*, (2 vols. and atlas, facsimile of the 1814 London edition) (Adelaide: Libraries Board of South Australia, 1966), vol. 2, 101–2.

51. Flinders, *A Voyage to Terra Australis*, vol. 2, 115.

at the surface, affords a shelter, to leeward of which their infant colonies may be safely sent forth."[52]

Meanwhile, thanks to the collections and illustrations produced on the French expedition (1800-1804) under Nicolas Baudin, South Sea corals also began to colonize the pages of taxonomic works being produced by land-bound naturalists like Lamouroux.[53] François Péron, a zoologist on the expedition, also reported that he had found great masses of coral rock on high land. "In the midst of the mountains of the interior of Timor, in the deep bosom of the vallies, in the ravines of torrents, every where are found the wrecks of these astonishing animals, while the mind is inadequate to the conception by what means nature has been enabled to raise these plateaux of madrepores, of such huge dimensions, to so great a height above the present level of the sea."[54] Reporting to the mathematical and physical class at the Institut de France after his return, he argued that this elevated mass confirmed that madrepores had the capacity to construct enormous submarine mountains.[55] It was further evidence that corals had to be accounted for in any new theory of the globe.

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52. Flinders, *A Voyage to Terra Australis*, vol. 2, 115.

53. Lamouroux, *Histoire Des Polypiers Coralligènes Flexibles, Vulgairement Nommés Zoophytes*. On the distribution of François Péron's specimens from the Baudin voyage, see Richard W. Burkhardt, Jr., "Unpacking Baudin: Models of Scientific Practice in the Age of Lamarck," in *Jean-Baptiste Lamarck, 1744–1829*, ed. Goulven Laurent (Paris: Éditions du CTHS, 1997), 497–514. On the scientific results of the Baudin voyage, see Margaret Sankey, Peter Cowley, and Jean Fornasiero, "The Baudin Expedition in Review: Old Quarrels and New Approaches," *Australian Journal of French Studies* 41 (2004): 4–14; Michel Jangoux, "Les zoologistes et botanistes qui accompagnèrent le capitaine Baudin aux Terres australes," *Australian Journal of French Studies* 41 (2004): 55–78.

54. François Péron, "Historical Relation of a Voyage Undertaken for the Discovery of Southern Lands [1806]," in *A General Collection of the Best and Most Interesting Voyages and Travels in All Parts of the World*, ed, trans John Pinkerton (London: Longman, Hurst, Rees, Orme, and Brown, 1812), 804–5.

55. François Péron, "Memoire sur quelques faits zoologiques applicables à la théorie du globe." Quoted in Jean-Baptiste Lamarck, "Considérations sur quelques faits applicables à la théorie du globe, observés par M. Péron dans son voyage aux Terres australes, et sur quelques questions géologiques qui naissent de la connoissance de ces faits," *Annales du Muséum d'histoire naturelle* 6 (1805): 26–52. See also Rudwick, *Bursting the Limits of Time*, 452.

Kotzebue's voyage and the close study of coral islands

Expeditions like those of Cook, Flinders, and Baudin were undertaken at considerable expense to the finances and naval resources of Britain and France, and were justified in large part by the rivalry between the two nations. The competitive tension between the Englishman Flinders and and the Frenchman Baudin as each sought new discoveries on the coast of Australia was heightened by the possibility that the next news from Europe might bring word of war. Indeed, Flinders' report on the voyage, including his comments on coral reefs, was delayed because he was detained for six years by the French governor at the Ile de France (Mauritius) when he steered his leaking vessel there after war had broken out. The Napoleonic wars produced a hiatus in the European exploration of the South Seas.

The resumption of Pacific exploration came from neither France nor Britain, but with a voyage that was privately funded by the Russian Imperial Chancellor, Count Nikolai Romanzov (Rumyantsev). In the present context, it was a voyage made interesting because it reveals to us that not all low, lagoon islands were as easy to identify as the particular ones that Forster had seen, particularly not when viewed from the deck of the ship or from an island itself. It was also noteworthy because the unprecedented time that was spent surveying low islands in the middle of the Pacific enabled two members of the expedition to produce distinct explanations of coral island formation.

This circumnavigation (1815-1818) was commanded by Otto von Kotzebue, a lieutenant in the Russian navy. With the advice and assistance of Admiral Adam Johann (Iwan Fjodorowitsch) von Krusenstern, who had taken Kotzebue as a teenaged clerk on the first Russian circumnavigation (1803-1806), Romanzov dispatched the 28-year old Kotzebue with the twin objectives of seeking a Northwest Passage through the

Bering Strait and exploring the islands of the tropical Pacific.[56] The undertaking was conceived as a "scientific voyage," and Kotzebue carried a set of specific instructions for astronomical and physical observations, including the study of the tides, the depth and salinity of the sea, the horizontal refraction of light and the magnetic inclination during the voyage. These instructions, written by J.C. Hörner (another former member of the Krusenstern voyage), offered a "simple and never enough to be recommended rule, 'attentively to observe, and circumstantially to describe, every unusual appearance,' and especially *to measure everything mensurable*."[57] The ship carried two scientific gentlemen, the official naturalist, Adelbert von Chamisso, and the ship's physician and assistant naturalist, J.F. Eschscholtz.[58] Eschscholtz, who, like Kotzebue and Krusenstern, was a Baltic German born in Estonia, had studied medicine at the university in his birthplace of Dorpat (now Tartu).[59] Chamisso was from a French aristocratic family that had settled in Berlin, where he attended military school and became a lieutentant in the Prussian army. Encouraged by his acquaintance with Humboldt and Auguste de Staël, he became interested in botany around his thirtieth birthday, in the early 1810s, and spent three years studying science and medicine at the new university in Berlin immediately before embarking on the voyage.[60] While he was away from Europe, Chamisso achieved a substanital literary reputation with the success of his recently published novel of a man who traded away his shadow, *Peter Schleimihls*

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56. On Kotzebue and Krusenstern, see Dietmar Henze, *Enzyklopädie der Entdecker und Erforscher der Erde*, 4 vols (Graz: Akademische Druck - u. Verlagsanstalt, 1993).
57. J.C. Hörner, "Instructions for the Astronomical and Physical Operations on the Voyage to the North Pole, Under the Command of M. von Kotzebue." [20 June 1815]. In Otto von Kotzebue, *A Voyage of Discovery, Into the South Sea and Beering's Straits, for the Purpose of Exploring a North-East Passage, Undertaken in the Years 1815–1818, at the Expense of His Highness the Chancellor of the Empire, Count Romanzoff, in the Ship Rurick, Under the Command of the Lieutenant in the Russian Imperial Navy, Otto von Kotzebue* (London: Longman, Hurst, 1821), 41–83, quotation on p. 41. Original emphasis.
58. Krusenstern, "Introduction." In Kotzebue, *Voyage*, 1–40, 23.
59. L.J. Blacher, "Johann Friedrich Eschscholtz." C.C. Gillispie, ed., *Dictionary of Scientific Biography*, 14 vols., supplement, and index (New York: Charles Scribner's Sons, 1974–80).
60. Dorothea Rudnick, "Adelbert von Chamisso." Gillispie, *DSB*.

wundersame Geschichte. The captain, meanwhile, was the son of the German dramatist August von Kotzebue. He and Chamisso endowed the narrative of the voyage with vivid descriptions that offered rich details of the perils and rewards of studying coral reefs in the open ocean.

Kotzebue spent April of 1816, January to March of 1817, and parts of October and November of 1817 at the atolls of the present day Marshall Islands, in accordance with his instructions to "pass the winter months in the neighbourhood of the imperfectly known Coral islands, to make discoveries there."[61] Because his ship, *Rurick*, was small for a ship of exploration at 180 tons burthen, he was able to bring her closer to the reefs than most previous navigators had managed in their vessels.[62] Krusenstern pointed out after the voyage that "[t]he small size of [Kotzebue's] ship...favoured these researches; he has...seen more of the Coral islands, and examined them more carefully, than any of his predecessors."[63] The Admiral also praised Kotzebue for the "courage and ability he showed in minutely exploring these singularly formed islands" and drew attention to the "dangers he braved to penetrate into the almost hermetically closed basins" of lagoons. Indeed, Krusenstern credited him with being "the first seaman who has ventured to navigate these lakes, encircled with corals," arguing that for this achievement his seamanship should be "placed at the side of the celebrated Flinders."[64]

Kotzebue's narrative of the voyage suggests that he was so fascinated with coral islands that he could not resist risking his ship repeatedly among treacherous reefs. As he explained it, "[t]hese islands inspire great interest, merely by their nature, as they owe their origin entirely to marine animals; and I determined to hazard a great deal,

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61. Kotzebue, *Voyage*, vol. 1, 355.
62. On the size of the ship, and advantages and disadvantages of a small ship for a voyage of discovery, see Krusenstern's disccusion in Kotzebue, *Voyage*, vol. 1, 13–14 and vol. 2, 294.
63. Krusenstern, "Analysis of the Islands Discovered by the Rurick." In Kotzebue, *Voyage*, vol. 2, 291–313, on 294.
64. Krusenstern, "Analysis of the Islands Discovered by the Rurick." In Kotzebue, *Voyage*, vol. 2, 291–313, on 311.

before I gave up the plan of penetrating [them]."[65] Encountering their first coral islands of the voyage during their long journey around South America to Alaska, Kotzebue and his "scientific gentlemen" were "seized with a greater desire than ever to land," and they "unanimously resolved to satisfy it, in spite of every danger."[66] The *Rurick* stood to the lee of the reef overnight while the ship's people worked through the dark hours to assemble "all the boards and poles on board" into a "pram, which was large enough to carry one person with ease."[67] The next morning, two sailors swam toward the island with a rope connected to a boat anchored off the reef in forty fathoms of water, "effecting a communication with the shore." As Kotzebue explained,

> One now placed himself upon the pram, drew himself along the rope towards the surf, and left it to a rising wave to throw him on shore; the pram was drawn back as soon as the man who had passed over had got firm footing on the beach, and then another began the unsteady passage. At length we were all on shore...all of us more or less hurt, as we could not reach the shore without being washed by the surf over a sharp coral bank.[68]

In the four hours it took to explore the whole island, Kotzebue's party found signs that the island had been visited by natives, but they encountered none. Naming their new discovery in honor of Count Romanzoff, they celebrated with drafts of coconut milk and wine.[69]

For Kotzebue as for Cook, exploring the tropics was a seasonal occupation, and so he was obliged to hurry northward to the Bering Straits after the discovery of Romanzoff Island in April 1816. In the winter he guided the *Rurick* back to the same area of the Pacific for an extended survey. Here again the drama of Kotzebue's voyage was heightened, as his lieutentant, Schischmareff, found a tantalizing channel across the

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65. Kotzebue, *Voyage*, vol. 2, 15.
66. Kotzebue, *Voyage*, vol. 1, 149–50.
67. Kotzebue, *Voyage*, vol. 1, 150.
68. Kotzebue, *Voyage*, vol. 1, 151.
69. Kotzebue, *Voyage*, vol. 1, 151–52.

reef using one of the ship's boats. Determined to take the *Rurick* through on the following day, Kotzebue effected a risky plan to hold the ship at the spot overnight. "The Rurick now stood in the middle of the ocean, fastened to a coral reef, under the protection of God."[70] At four o'clock in the morning on 6 January 1817, the wind changed and brought the ship so close to the reef that Kotzebue had no choice but to abandon the anchor. As the sun rose the sails were spread and the *Rurick* ran with the wind into the channel. "A death-like silence prevailed on board the ship," he reported, for "we heard on both sides the roaring of the breakers, and every one was ready at his post."[71] The small ship was able to slip through unscathed, and the crew found themselves on the opposite side of the reef, in calm water of less than thirty fathoms' depth.

They had entered the lagoon of a large atoll, but this was not yet apparent from the perspective aboard ship. The smooth sea to the northeast of the newly named Rurick's Strait was bordered on the northern and western sides by a curving reef, and the stillness of the water implied that there must also be shelter somewhere to the east. In the following days, the views from the masthead and from atop trees growing on islets on the reef showed that there was indeed land in that direction, "whence we conjectured that we were in the midst of an archipelago of islands."[72] Over another week of careful surveying and exploring to the eastward, the archipelago began to take shape. On 16 January, Kotzebue and others in his landing party "climbed up a tolerably high tree, and saw land to the south-east, by which I was confirmed in my supposition that we were in a circle of islands."[73] Four days later, while aboard the ship, he "now also perceived land in the S[outh]," and "was more and more convinced that we were in

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70. Kotzebue, *Voyage*, vol. 2, 16.
71. Kotzebue, *Voyage*, vol. 2, 18.
72. Kotzebue, *Voyage*, vol. 2, 33.
73. Kotzebue, *Voyage*, vol. 2, 46–47.

a circle."[74] It was two weeks since the *Rurick* had passed through the reef, and Kotzebue remained at least slightly uncertain that they were in a lagoon.

This attests to the striking difference that could exist between the appearance of a reef in nature and in its form laid down on a chart. With a perspective limited to the trifling elevation that could be attained from a masthead or treetop, there was no single vantage point from which an atoll thirty miles across could look like one integrated structure. Only on the kind of chart produced by Kotzebue's monthlong survey of this atoll, also named for Romanzoff (but usually referred to by Kotzebue using the native name, Otdia [now Wotje]) could one see at a glance that the reef had an elliptical shape that clearly divided lagoon from ocean on all sides.

The ship remained within the reef during the month of January 1816, while, in addition to surveying, the philosophically inclined among the *Rurick*'s people made a profound physical and ethnological investigation of the atoll and its inhabitants. Schischmareff explored the lagoon by boat, while Kotzebue, the scientific gentlemen, and many of the other crew members examined one islet after another along the reef. They occasionally remained ashore for days at a time, meeting dozens of natives and striking up friendships through gifts of iron. The captain passed restless nights ashore; though he was "undisturbed," he "could not sleep for thinking of the discoveries I expected to make."[75] Kotzebue frequently mentioned the party's shared "curiosity [to] examin[e] the corals, as well on the island as on the reef."[76] One evening, when "Chamisso and Eschscholtz returned with a quantity of rare corals and marine animals," they sat "cheerfully round the tea-kettle" and discussed what they had found. Kotzebue reported that "their conversation upon [the corals] was instructive, and we listened with

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74. Kotzebue, *Voyage*, vol. 2, 59.
75. Kotzebue, *Voyage*, vol. 2, 38.
76. Kotzebue, *Voyage*, vol. 2, 35.

attention, till the rats and lizards disturbed us by stealing our biscuit."[77] Chamisso took the opportunity for such extended sojourns on the coral islets, which would have been the envy of Forster, to make the most intimate descriptions ever offered of the surface of a Pacific reef. He distinguished the branching shape of the madrepores and millepores that grew in the lagoon from the "lozenge" shaped *Astrea* that were "always met with in the constantly-watered hollows of the bottom, next to the breakers."[78] He attributed the "red colour of the reef, under the breakers" to "a *Nullipora*, which covers the stone wherever the waves beat."[79] Experiments with the living and skeletal forms of this stony encrusting organism (considered by others before and after to be a plant) led Chamisso to "ascribe this substance an animal nature."[80] He also explained that different corals consisted of distinctive polyps: some lithophytes had "a certain velvet-like appearance...with fine pores [when] in a living state," while others had "larger and more distinguishable polypuses." Of the latter sort, a *Caryophyllea* had its "end-branches" covered by "an animal, resembling the [sea anemone] *Actinia*."[81]

Among the benefits of learning to converse with the islanders was receiving their descriptions of the other islands in what they called the Radack group (a name that is still applied to the eastern chain of the Marshall islands). On the first such occasion, Kotzebue reported that his own "speaking and pantomime were long in vain: at length, however [a native, Lagediack] understood me, pointed with his hand to the south, saying *inga eni cef cef*, (yes, islands there); and my joy was the greater, as I owed the discovery of an unknown group to my knowledge of the language."[82] After Kotzebue showed his friend how he recorded information on a chart, Lagediack drew several circular groups

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77. Kotzebue, *Voyage*, vol. 2, 37.
78. Adelbert von Chamisso, "Remarks and Opinions of the Naturalist of the Expedition," in *Voyage*, Otto von Kotzebue (London: Longman, Hurst, 1821), vol. 3, 142.
79. Chamisso, "Remarks and Opinions," vol. 3, 142–43.
80. Chamisso, "Remarks and Opinions," vol. 3, 142–43.
81. Chamisso, "Remarks and Opinions," vol. 3, 143.
82. Kotzebue, *Voyage*, vol. 2, 68.

of islands and explained their distance in number of days' sailing. Kotzebue also described Lagediack's own "very clever method" of depicting the "geographical situation": "he drew on the sand a circle, nearly in the form of the group Otdia [the atoll Wotje], placed round the edge of it large and small stones, which represented the islands; and...marked the channels," and "explained [the other low islands] in the same sensible manner."[83] As Kotzebue reported, "I have in the sequel found his information to be perfectly correct," though in navigating by these directions he reached the neighboring low island to the south much more quickly than expected, which "proved that Lagediack's day's voyage was no standard for us" and that "the other groups were nearer than he had fixed."[84]

Although Kotzebue apologized that that "I cannot enter into a detailed explanation of the origin of the Coral islands" because "they belong to the naturalist," he in fact offered extensive reflections on the progressive formation of reefs.[85] He was convinced that "[t]he animal builds upwards from the bottom of the sea, and dies as soon as it reaches the surface; from this edifice there is then formed, by the constant washing of the sea, a grey calcareous stone, which seems to be the basis in all the islands."[86] Of an islet that consisted of "large dead blocks of coral, which are covered with a layer of mould not more than two inches deep at the most," he wrote:

> The spot on which I stood filled me with astonishment, and I adored in silent admiration the omnipotence of God, who had given even to these minute animals the power to construct such a work. My thoughts were confounded when I consider the immense series of years that must elapse, before such an island can rise from the fathomless abyss of the ocean, and become visible on the surface. At a future period they will assume another shape; all the islands will join and form a circular slip of earth, with a pond or lake in the circle; and this form will again change, as these animals continue building, till they reach the suface, and then the water will one day vanish, and only one great island will be visible. It is a strange feeling to walk about on a living island, where all

83. Kotzebue, *Voyage*, vol. 2, 84.
84. Kotzebue, *Voyage*, vol. 2, 84.
85. Kotzebue, *Voyage*, vol. 2, 27–28.
86. Kotzebue, *Voyage*, vol. 2, 27.

> below is actively at work. And to what corner of the earth can we penetrate, where human beings are not already to be found? In the remotest regions of the north, amidst mountains of ice, under the burning sun of the equator, nay, even in the middle of the ocean, on islands which have been formed by animals, they are met with![87]

When he saw with the aid of Lagediack's directions, how the other low islands of the chain shared Wotje's annular form, he concluded that their "uniformity...is probably not accidental; but this structure seems to be peculiar to the corals."[88]

Chamisso's account of the voyage described the low islands of the Radack chain as "table mountains, which rise perpendicularly from the depths of the ocean, and near which the lead finds no bottom. The surface of the table is below water; only a broad dam round the circumference of it (the reef), reaches the surface at low water, and bears on its ridge or back, the sand banks, (the islands,) which the sea throws up. [...] The reefs and islands, therefore, inclose an internal basin, or lagoon."[89] This description reveals that Chamisso contended with the possibility that the greater part of the submarine structure (the "table") might be of a different material or origin than the reef ("dam"). He reported that "As far as the dam can be examined, [the dam] consists of horizontal layers of a limestone (congeries) formed of coral sand, or fragments of madrepores."[90] He could only infer the composition of the reef's foundation from the material that the waves brought forth from the depths. Such "[m]asses of rock, often measuring a fathom, thrown upon the dam, are of the same stone, which often contains only larger fragments of madrepores, than the layers exposed to view above."[91] This encouraged Chamisso to form "the opinion, that the whole construction, the table or *plateau* which forms the basis of the group of islands, consists of this same kind of

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87. Kotzebue, *Voyage*, vol. 2, 36.
88. Kotzebue, *Voyage*, vol. 2, 116–17.
89. Chamisso, "Remarks and Opinions," vol. 2, 356–57.
90. Chamisso, "Remarks and Opinions," vol. 2, 357.
91. Chamisso, "Remarks and Opinions," vol. 2, 357.

stone. It is a species of rock of new formation, and which still continue[s] to be produced."[92]

Chamisso's understanding of the stone that formed coral reefs is worth further attention, because it was markedly different from that of Forster or Flinders. As described above, he was well aware that lithophytes were stones formed by the skeletons of colonial polyps. Unlike those previous voyagers, however, and seemingly at variance with Kotzebue's view as well, Chamisso considered the rock of the reef to be consolidated from the fragmented remains of madrepores (the most prominent group of lithophytes) rather than to be built continuously by the madrepores themselves. In describing "this reef-stone," which he had seen on the shores of high islands at Woahoo (Oahu), the Philippines, and Guahon (Guam) as well as at the low islands, Chamisso pointed out that "some local differences may arise" in its composition, owing to "the difference of the species of madrepores, of which they are chiefly composed."[93] Thus he argued that "the species which live on the spot, furnish the elements for the stone which is formed."[94] This did not mean that these madrepores formed the stone themselves, however, for in studying "the coral reefs, and the kind of rock of which they consist, we no where recognize the skeletons of the Lithophytes in their original places, and the spot where they lived and grew."[95] Chamisso acknowledged that this observation was at odds with those of Flinders ("[who] assumes, that the skeletons of the madrepores are converted into reef-stone on the spot where they grew") and Forster, whom Chamisso considered to have "touch[ed] this subject only slightly," remarking dismissively that "what he says of it is not worth notice."[96] Besides having seen no

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92. Chamisso, "Remarks and Opinions," vol. 2, 357 Original emphasis.
93. Chamisso, "Remarks and Opinions," vol. 2, 358.
94. Chamisso, "Remarks and Opinions," vol. 2, 358.
95. Chamisso, "Remarks and Opinions," vol. 2, 360.
96. Chamisso, "Remarks and Opinions," vol. 2, 360 Chamisso elsewhere made many references to Forster's work that revealed him to have made a careful study of this earlier account of the Pacific.

direct evidence that had convinced him that reefs were formed of corals in situ, Chamisso was skeptical that the submarine form of the reef could possibly have been generated by coral growth. As he argued, against Forster and Flinders, "The supposition that *polypi* producing limestone, live only in the walls of the already-existing reefs, and their internal lagoons, would not explain the first origin of these reefs, the perpendicular height of which cannot well be taken at less than a hundred fathoms."[97]

Having criticized previous theories of reef formation, Chamisso offered an alternative whose vagueness served to illustrate that processes occurring deep beneath the sea remained the stuff of speculation only. "[W]e cannot but believe," he argued, "that in those parts of the sea where the enormous masses of this formation rise, even in the cold and unillumined bottom of the ocean, animals are continually employed, in producing, by the process of their life, the materials for its indisputably continued growth and increase."[98] It must be admitted that Chamisso was on relatively firm ground in assuming that the bottom of the sea was cold and unillumined, for his shipmates Kotzebue and Eschscholtz carried out frequent measurements of water temperature at various depths, and Kotzebue made an innovative series of studies of the "transparency" of seawater by lowering a white board on a line until it could no longer be perceived.[99] As an explanation for how these coral materials were transformed from skeletons into solid rock, Chamisso offered only a metaphor: "the ocean between the

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97. Chamisso, "Remarks and Opinions," vol. 2, 360 Original emphasis. This estimate of the perpendicular height of the reef suggests the limit to which Chamisso felt confident in the hydrographers' descriptions of undersea topography.

98. Chamisso, "Remarks and Opinions," vol. 2, 360–61.

99. See, e.g., Kotzebue, *Voyage*, vol. 1, 180–85. His assumption that corals could live in such dire conditions was reinforced by evidence from a deep sounding recently performed in the arctic by John Ross, who, Chamisso pointed out, had "found in Possession Bay, in 73º 39' north latitude, living worms in the mud, which he drew up from a depth of one thousand fathoms, and the temperature of which was below the freezing point." Chamisso, "Remarks and Opinions," vol. 2, 361 Ross's sounding was reported in his narrative of 1819, which Chamisso could only have consulted after the voyage. See John Ross, *A Voyage of Discovery, Made Under the Orders of the Admiralty, in His Majesty's Ships* Isabella *and* Alexander, *for the Purpose of Exploring Baffin's Bay, and Inquiring Into the Probability of a North-West Passage* (London: John Murray, 1819).

tropics seems to us to be a great chemical laboratory of nature, where she confides an important office in the system of her economy to these imperfectly-organized animals that produce lime-stone."[100]

Equally uncertain, given the obscurity of the deep sea, was how significant a role these calcareous organisms really played in the economy of nature--that is, how much of the earth's terrain was actually made of coral rock. "Objects, it is true, appear magnified in proportion as the eye is near to them," Chamisso acknowledged, and so "he who, in the midst of these islands, contemplates their formation, may be disposed to assign to it greater importance in the history of the earth, than reality justifies."[101] Any desire that he or other Europeans might have to settle the matter was constrained by the relatively brief duration of their visits to coral islands. Though he reported that "the progressive growth of the reef does not seem to have escaped the natives" of the Sandwich Islands (Hawaii), the rate of accumulation of coral rock could hardly be studied directly in a stay of a few weeks.[102] For this reason, Chamisso concluded his discussion of reef formation with a call to action for naturalists who might follow him across what he called the "Great Ocean:" "An accurate description of the state of these reefs at different periods, for instance, at an interval of half a century, if it were possible and really undertaken, must contribute to throw light upon many points of natural history."[103]

The most widely noticed comments on coral reef formation to emerge from the voyage of the *Rurick* were not written by Kotzebue or Chamisso. They were the work of the physician, Eschscholtz, in an "Appendix by Other Authors" added to the back of

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100. Chamisso, "Remarks and Opinions," vol. 2, 361.
101. Chamisso, "Remarks and Opinions," vol. 2, 361.
102. According to Chamisso, the Sandwich islanders "who, at the king's order, fetched stones out of the sea, to build a wall, declared, while at their work, that it would grow, and increase of iself." Chamisso, "Remarks and Opinions," vol. 3, 238.
103. Chamisso, "Remarks and Opinions," vol. 2, 361.

Chamisso's "Remarks and Opinions of the Naturalist of the Expedition." Until 1892 this brief essay "On the Coral Islands" was attributed to Chamisso despite the title of the appendix.[104] Eschscholtz placed more emphasis than any previous traveler on the distribution of coral islands. They were entirely absent across tracts of tropical sea that, by Forster's theory, would have offered ideal conditions for reef-building corals. Rather, low islands were usually found in "rows...and large groups" just like archipelagoes of high islands, which suggested that the two types had a common underlying cause. Therefore, Eschscholtz argued, "the corals have founded their buildings on shoals in the sea; or to speak more correctly, on the tops of mountains lying under water."[105] Chamisso had mentioned that coral islands were "sometimes in rows, which seem to indicate a ridge in the bottom of the sea," but pursued the subject no further.[106] Eschscholtz diverged from Chamisso on a number of other points. He implied that reefs were built by corals in situ, and argued explicitly that the most massive corals "prefer the more violent surf on the external edge of the reef," whereas Chamisso had conceived that "the enormous masses of one growth," which gales sometimes threw upon the reef, had "probably formed in the tranquil depths of the ocean."[107] Eschscholtz believed that calm water, such as was found in the lagoon, was better suited for the growth of "smaller species of coral, which seek a quiet abode."[108]

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104. [J.F. Eschscholtz], "On the Coral Islands," in *Voyage*, Otto von Kotzebue (London: Longman, Hurst, 1821), vol. 3, 331–36 Although the appendix's authorship was anonymous in both the original German and the English translation of Kotzebue's *Voyage*, it was specifically credited to Chamisso when it was published in Britain as a stand-alone article, "On the Coral Islands of the Pacific Ocean. -- By Dr A. von Chamisso," *Edinburgh Philosophical Journal* 6 (1822): 37–40. Eschscholtz's authorship was asserted in C. Ph. Sluiter, "Die Korallentheorie von Eschscholtz," *Zoologischer Anzeiger* 15 (1892): 326–27. See David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs," 214, note 13.
105. [J.F. Eschscholtz], "On the Coral Islands," 331.
106. Chamisso, "Remarks and Opinions," vol. 2, 358.
107. [J.F. Eschscholtz], "On the Coral Islands," vol. 3, 331; Chamisso, "Remarks and Opinions," vol. 3, 143–44.
108. [J.F. Eschscholtz], "On the Coral Islands," vol. 3, 335.

Eschscholtz's intuition that the coral reefs had originated on submarine mountaintops combined with his conception of the differential growth of different types of corals to provide him an explanation for low islands' ring shape, which Chamisso never accounted for. Eschscholtz considered that as a solid mass of corals grew upward and outward from the peak of a submarine mountain, the largest types grew preferentially on the outside of the reef. As the level top of the coral formation approached sea level, the accumulation of wave-borne shells and coral fragments would choke off the growth of less hardy species inside, so that only "the exterior edge of a sub-marine coral edifice" would actually approach the surface of the water.[109] For this reason, Eschscholtz argued, "the island, therefore, necessarily has a circular form."[110]

Despite the typical presence of a lagoon on their interior, Eschscholtz pointed out that "in their external form, the Coral islands do not resemble each other."[111] The northern Pacific low islands explored by the Kotzebue expedition, for example, were much larger than those seen by Bougainville and Cook in the Dangerous Archipelago, and though all had the general shape of a full or partial ring, they were highly variable in detail. There was no obvious explanation for this variation if low islands had been built from indiscriminate locations on the bottom of the ocean. Eschscholtz's explanation was that the external size and shape of each one "probably depends on the size of the sub-marine mountain tops, upon which their basis is founded."[112]

The richness of the observations and the diversity of opinions produced by the members of the *Rurick* expedition illustrate several of the important points of debate around which the study of coral reefs might pivot. Alongside the questions suggested by Forster's work, of the composition of coral reefs and the reason for their circular

109. [J.F. Eschscholtz], "On the Coral Islands," vol. 3, 333.
110. [J.F. Eschscholtz], "On the Coral Islands," vol. 3, 333.
111. [J.F. Eschscholtz], "On the Coral Islands," vol. 3, 334.
112. [J.F. Eschscholtz], "On the Coral Islands," 334.

form, the distribution of reefs was now a key issue. It would also be necessary to determine why and in what ways reefs varied from one location to another. Both Eschscholtz and Chamisso thought it likely that the low islands and reefs of the Indian Ocean "belong[ed] to the same formation" as those of the Pacific, but each separately emphasized that such opinions were based on "imperfect and unsatisfactory accounts."[113] It also remained to be determined whether corals produced significant amounts of new land on a global scale.

Quoy and Gaimard's limitations on the growth of corals

The notion that corals played a major role in remodeling the earth's crust was dealt a severe blow by the work of J.R. Quoy and J.P. Gaimard, physicians on the French circumnavigation under Louis-Claude de Freycinet (1817-1820). This was a voyage made extraordinary in part by the fact that it crossed the tropical Pacific without ever coming in sight of a low island.

Freycinet had been a lieutenant under Baudin on the expedition of the *Geographe* and *Naturaliste*, and was dispatched this time in the *Uranie* to continue the surveys around Australia, the East Indies, and the western Pacific that were begun by the ill-fated voyages of La Pérouse and Baudin.[114] Recalling that the presence of large numbers of naturalists on the Baudin expedition had undermined naval discipline, Freycinet insisted that the scientific positions aboard the *Uranie* be filled by members of the navy.[115] Quoy, who was appointed Surgeon, had trained at the School of Naval

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113. Chamisso, "Remarks and Opinions," vol. 3, 359; [J.F. Eschscholtz], "On the Coral Islands," vol. 3, 334.

114. For a brief review of the hydrographical accomplishments of the voyage, see L.S. Dawson, *Memoirs of Hydrography, Including Brief Biographies of the Principal Officers Who Have Served in H.M. Naval Surveying Service Between the Years 1750 and 1885* (London: Cornmarket, 1969), vol. 1, 74–75.

115. Lorelai Kury, *Histoire naturelle et voyages scientifiques (1780–1830)* (Paris: L'Harmattan, 2001), 134; Hélène Blais, "Exploration and Colonization in the Pacific: French Voyages Under the

Medicine in Rochefort, and Gaimard, the Assistant Surgeon, studied at the naval medical school in Toulon.[116]

Based primarily on their work in Timor, Quoy and Gaimard claimed that reef building corals could only live in shallow water, which dirctly contradicted the visions of lithophytes growing on ocean floor that had been advanced in the works of Forster, Flinders, and Chamisso. In their "Memoir on the Accumulation of Lithophyte Polyps, Considered Geologically," read to the Academie des Sciences in 1823 on the auspicious date of July 14, Quoy and Gaimard sought to overthrow "all that has been said or ostensibly observed" on the growth of coral reefs.[117] They identified "the species which constantly form the most considerable banks," among which were the Meandrina, certain Caryophyllia, and especially the Astrea.[118] "The species of the genus Astrea," they explained, "alone capable of covering immense parts of the surface, do not begin their constructions at greater than twenty-five or thirty feet of depth."[119] Quoy and Gaimard offered three types of evidence for this claim. They described direct observations through the notoriously clear waters of the tropical seas, where Astrea were only to be seen in shallow areas.[120] More importantly, they asserted that neither the anchor nor the sounding lead ever brought up fragments of these species from any great

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Bourbon Restoration and the July Monarchy," in *Science in the French and British Navies, 1700–1850*, ed. Pieter van der Merwe (Greenwich: National Maritime Museum, 2003), 64–67.

116. Toby A. Appel, "Jean-René-Constant Quoy," in *Dictionary of Scientific Biography*, ed. C.C. Gillispie (New York: Scribner's, 1974); William Coleman, "Joseph Paul Gaimard," in *Dictionary of Scientific Biography*, ed. C.C. Gillispie (New York: Scribner's, 1974). For a list of personnel on the *Uranie*, see Jacques Arago, *Narrative of a Voyage Round the World in the* Uranie *and* Physicienne *Corvettes Commanded by Captain Freycinet, During the Years 1817, 1818, 1819, and 1820*, (Facsimile of the 1823 London edition) (New York: Da Capo Press, 1971), part 2, 295.

117. J.R.C. Quoy and Paul Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," *Annales des Sciences Naturelles* VI (1825): 273–90. Quotation on p. 274.

118. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 277.

119. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 284.

120. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 284.

depth.[121] The third type of evidence, which they believed was most decisive, was drawn from the study of corals that had been upraised above the level of the sea. As they explained, "[t]he impossibility of going underwater to examine the precise depth at which the solid zoophytes establish themselves, means that we must deal instead with times past, and the monuments that the ancient revolutions of the globe have revealed, which will serve to prove what occurs in our own time."[122] Timor, in the Dutch East Indies, was a high island where many of the shores were made of coral rock. On the Baudin expedition, Péron had concluded that the entire island was formed by madrepores. Though Quoy and Gaimard likewise found extensive areas of dry land formed of "banks of Madrepores that the sea has left uncovered...as it retired," they strenuously disagreed with Péron, explaining that upon traveling inland they found primitive rocks that must underlie the masses of corals.[123] Their observations suggested that the "zoophytes built on a previously existing base, and they occupy only its surface."[124]

Quoy and Gaimard believed that reef building corals required "the influence of light" for their growth, and argued that it was indeed unlikely that such species could possess "the prerogative to live in all depths, all pressures, and all temperatures, so to speak."[125] Along with the constraint on the depths where these madrepores could flourish, the Frenchmen argued that corals multiplied best in warm, shallow, quiet water that was not liable to suffer surges or regular breezes.[126] Given these limitations, the

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121. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 284.
122. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 280.
123. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 280.
124. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 283.
125. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 277.
126. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 276.

breaking sea would prevent corals on a reef offshore from a high island from increasing the mass of the reef to seaward. Instead, corals would "approach closer and closer to the shore," extending the reef inward, where the warm water and deadened waves favored their growth.[127] These newly offered principles of coral propagation were almost diametrically opposed to ideas argued by Chamisso and Eschscholtz respectively, that madrepores were sufficiently robust to live at substantial depths and to grow most favorably in heavily agitated waters.

Quoy and Gaimard's assertion that corals could grow only in calm and shallow waters meant that they could not have built "coral" reefs. Listing a series of low islands and reef-encircled high islands, they insisted that "[i]nstead of believing that [they] are in part or in total the work of zoophytes, we think on the contrary that all of these lands have for their bases the same elements, the same minerals, that contribute to form [other] islands and all the known continents."[128] Quoy and Gaimard acknowledged that corals were found growing atop tropical shoals that "rise like walls from great depths, where no bottom is to be found," but cautioned that "it would be false to say that these reefs were entirely formed by madrepores."[129] With the constraints that limited coral growth, it would be impossible for these animals to make solid masses starting at depths known to be greater than 200 fathoms.[130] Furthermore, they explained, because the breaking waves of the open sea would "destroy [the] fragile edifices" built by corals, "we acquire the moral certainty that these submarine escarpments are not the work of these animalcules."[131] Quoy and Gaimard buttressed these claims by pointing out that

127. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 278–79.
128. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 274–75.
129. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 276–77.
130. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 277.
131. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré

submarine cliffs were known to exist in parts of the ocean that were uninhabited by corals, meaning that corals were unnecessary to explain such cliffs, and by citing their own experience of seeing coral reefs interspersed among shoals composed of other kinds of rock.[132] All their evidence suggested that "corals form layers or crusts of only a few feet in thickness," and that "there are no significant, permanently inhabited islands that are formed entirely of coral."[133]

For Quoy and Gaimard, the most important conclusion to be drawn from their "geological" study of coral growth, as the title styled it, was that corals were relatively insignificant. All that had been said about "the immense works" produced by these animals was "inaccurate, always excessively exaggerated, and generally erroneous."[134] Among the "zoological phenomena relevant to the theory of the earth," they argued, the importance of corals could hardly compare to the testaceous molluscs "in the materials that [they] have supplied, and continue to furnish, to the earth's crust."[135] Corals were capable of choking harbors, but "what are their layers" the Frenchmen asked, "compared to the enormous volcanic peaks of the Sandwich Islands, Bourbon, the Moluccas, the Mariannes, [and] the mountains of Timor and New Guinea [on which layers of coral rest]?"[136] They suspected that many of the naturalists who attributed massive structures to the work of corals had simply misinterpreted the origin of certain rocks. It was crucial, they explained, to distinguish between masses of coral that had accumulated in situ by the uninterrupted growth of polyps, and "deposits known under the name of

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géologiquement," 278.

132. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 286–87.

133. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 275–76.

134. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 274.

135. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 273, 290.

136. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 290.

coral limestone," which were sedimentary rocks containing the water-worn debris of the former material.[137] (Their memoir gave no indication that they knew that Chamisso believed coral reefs to be built of just such "coral limestone.") Quoy and Gaimard urged savants who "aspire to produce grand hypotheses on the conformation of the globe" to hearken to the newly discovered limitations that nature had prescribed to the reef-building corals. "In reconsidering these zoophytes with more attention," they argued, "we will no longer see them filling the basins of the sea, raising islands, augmenting continents, and menacing future generations by forming a solid circle around the equator."[138]

Unlike any previous writing on the formation of coral reefs, Quoy and Gaimard's paper contained a sustained critique of the methods as well as the specific conclusions drawn by their predecessors. The Frenchmen positioned themselves against mariners who, "upon encountering shoals [in the tropics], are swayed by the conventional view, and don't hesitate to say that they are made by madrepores," but also against naturalists like Forster, who had "granted too much to the madrepores" and was therefore "one of the first to accredit the opinion that we oppose."[139] Quoy and Gaimard used the inconsistencies in Forster's *Observations* to their rhetorical advantage, gleefully pointing out the place where he had "provide[d] weapons against himself." When Forster claimed that coral reefs were the "first origin" of the low islands, they argued, he must have "forgotten" that he had also stated that he considered

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137. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 283–84 In addition to making this distinction, Quoy and Gaimard also clarified the terminology used to refer to lithophytes: "We know that the word "coral" is commonly applied, quite incorrectly, to all the stony polyps; "madrepore," which in zoology serves to designate a single genus, has acquired more or less the same meaning. We use both words to refer to these animals in general, without omitting to speak specifically when it becomes necessary." Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 276–77, note 1.

138. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 289–90.

139. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 286, 289.

the low islands part of a grand chain of submarine mountains.[140] Their primary target was Péron, whom they accused of drawing "overly general conclusions" from "a few isolated observations made at Timor and the Isle de France [Mauritius], the only places where he had been in range to observe the full work of the lithophytes."[141] They also critized his willingness to believe the accounts of Dutch colonists and "half savage" people at Timor who shared "his opinion on the important role played by lithophytes," instead of making closer observations himself. Thus, Péron had "advanced, on the faith of coarse natives, that the elevated mountains that he had seen from but ten leagues, were entirely made of madrepores."[142] Arguing literally and figuratively, they scoffed that only a "shallow study" could allow someone to believe that "madrepores clog the ocean basins and elevate low islands from the bottom of their abysses."[143]

Their judgments against Péron may be interpreted at more than one level. Some of the accusations that he had drawn faulty conclusions from superficial evidence were so formulaic as to suggest that Quoy and Gaimard's main objective was to burnish their reputations at the expense of a better known naturalist. Having succumbed in 1810 to a malady he had originally contracted during his southern voyage, Péron would offer no defense against the charge that "these claims about the accumulation [of corals]...are based on bad or superficially made observations[, and serve to] mask the truth and accredit error by the influence of famous names."[144] However, politically attuned contemporaries may have drawn another conclusion from the particular criticism of

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140. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 286–87.
141. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 274.
142. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 281.
143. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 275.
144. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 274.

Péron, whose clash with Baudin was so vitriolic that the naturalist had notoriously avoided mentioning the commander's name in his narrative of the voyage.[145] Quoy and Gaimard were perhaps implying, as members of the Navy, that the decision to banish civilian savants from French scientific voyages had not come at the expense of good observations.

In any case, it is not clear that their own methods of inquiry were distinguishable in kind from those for which they condemned Péron. Like Péron, and indeed all others who aspired to discuss the theory of the earth, Quoy and Gaimard supported their perspective by citing other authorities on the geology of places "that we have not visited ourselves."[146] "One must look and look again, and well note the facts," they argued, against what they saw as Péron's overzealous generalizing, "for if one hurries to draw general conclusions, our pride always finds ways to reconcile these same facts to our point of view."[147] Yet in claiming that there were no mountains at Timor formed exclusively of corals, they explained that "[h]aving coasted [the island] for about fifty leagues, close enough to see the geography, we were able to see that it was volcanic in several places."[148] I am not suggesting that their methods were inadequate; drawing an idea of the geology of a place based on its appearance from a boat was a common practice. Their investigation of Timor was at least marginally more detailed than Péron's had been, and it revealed primitive rocks underlying masses of coral, evidence that directly contradicted their predecessor's claim. The point is that on the strength of their first hand study of the island of Timor, Quoy and Gaimard had proceeded to judge

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145. For a brief review of the latest scholarship on the relationship between Péron and Baudin, and its effect on sullying Baudin's reputation, see Sankey, Cowley, and Fornasiero, "The Baudin Expedition in Review: Old Quarrels and New Approaches".

146. E.g., Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 275, 282–83. Quotation on p. 275.

147. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 282.

148. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 282.

the significance of corals in renovating the crust of the whole earth. This reveals that they held a basic assumption that the growth of corals and the formation of reefs proceeded in some universal manner that could be understood by reference to one or a few individual islands. Quoy and Gaimard shared this tacit belief with many of their contemporaries. As the combative tone of their paper signaled, in the decades since Forster's book was published the voices offering theories of, and based on, coral reef formation had grown numerous enough to become dissonant. Given that different coral reefs scattered across various parts of the tropics had been taken as paradigms for the individual theories in play, perhaps the most remarkable thing about the disagreement between Quoy and Gaimard, on the one hand, and Péron on the other, was that it was based on divergent interpretations of the same particular island.

The Frenchmen had not studied a single atoll. Among commentators on coral reef formation in the first decades of the nineteenth century, Quoy and Gaimard shared with Péron the distinction of having made all of their close studies of coral reefs in the neighborhood of high land. On the whole, they were skeptical of travelers' accounts that coral islands in the midst of the ocean. "[W]e have always found it extraordinary," they admitted, "what navigators say of these coral islands of the Great Ocean, which are covered with verdure [despite] being far removed from all other land."[149] Such descriptions seemed implausible to Quoy and Gaimard because "in these immense open spaces, the violence of the undeadened swells must prevent the work of the zoophytes."[150] They "d[id] not deny the existence of these islands," but believed that "it would be interesting to reexamine them carefully" to determine if they had really been formed by coral growth.[151]

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149. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 288.
150. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 288.
151. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré

A comment made in a footnote to this discussion revealed just how dissimilar atolls were from the coral formations that Quoy and Gaimard had found on and around the mountainous islands they had visited. "On glancing at the charts of Kotzebue's voyage," they declared, "we are struck to see many of these [low] isles grouped in circles, linked one to another by reefs that are apparently made of corals, and presenting by this arrangement a deep little internal sea, into which one may enter by one or more openings." These coral formations that sounded so unfamiliar to Quoy and Gaimard were the very low islands that Chamisso and Eschscholtz had studied on the voyage of the *Rurick*. Quoy and Gaimard's skepticism, born of their field study in locations that had convinced them that coral growth must be merely superficial, led them to offer an entirely new explanation for the shape of atolls despite having not seen one: "Might not this arrangement be due," they conjectured, "to submarine craters, upon whose rims the lithophytes have built?"[152] Whereas Eschscholtz and Forster had both believed that the annular shape of atolls was due to some general property of coral growth, Quoy and Gaimard believed that a ring-shaped reef was nothing more than a ring-shaped submarine mountaintop encrusted with a thin layer of corals. The obvious question, which would have to be answered if this suggestion of Quoy and Gaimard's were to be taken seriously, was whether the Pacific was actually studded with submarine volcano craters of up to thirty miles in diameter sitting just a few feet below sea level.

We may discern two important features of the tradition of inquiry into reefs, circa 1820, from Quoy and Gaimard's role in it. First, their work indicates that by this time "coral" reefs were a relatively stable type in the naturalist's mental cabinet of curiosities. The Frenchmen took it virtually for granted that low islands on the one hand, and reefs around high islands on the other, were fundamentally related and

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géologiquement," 289.

152. Quoy and Gaimard, "Mémoire sur l'accroissement des polypes lithophytes considéré géologiquement," 289.

comprised the same types of animals (as well as believing that the actual operations of the animals in both instances were fairly insignificant in proportion to the rock they carpeted). Second, their work reveals that coral islands (the future "atolls") were becoming such well known and puzzling phenomena that it was more or less incumbent upon any would-be explainer of any reef to account in some way for their characteristic annular shape.

Competing theories and new syntheses

Although Quoy and Gaimard's suggestion that low islands had formed atop submarine craters (which I will call the *crater-rim theory*) was literally just a footnote to their argument, it was quickly drawn from the margin to the very center of European debate over the formation of coral reefs. Morever, their convincing demonstration that massive corals could only live within a few fathoms of the surface indicated that there must be mountaintops underlying coral atolls, whether these peaks were crateriform or not. As the British hydrographer Frederick William Beechey wrote after returning from his Pacific survey of 1825-1828, "[t]he general opinion now is, that [coral islands] have their foundations [either] upon submarine mountains, or upon extinguished volcanoes, which are not more than four or five hundred feet immersed in the ocean; and that their shape depends upon the figure of the base whence they spring."[153] These alternatives

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153. Beechey understood Forster to have claimed that atolls "sprung from a small base and extended themselves laterally as they grew perpendicularly towards the surface of the sea [and] represented upon a large scale the form which is assumed by some of the corallines." Evidently convinced by the general principle (if not the specific figure) offered in Quoy and Gaimard's demonstration of the depth limit to coral growth, Beechey dismissed the theory "entertained" by Forster because "considering the extent of some of these islands, it is evident that if this be their form, the lithophytes...must commence their operations at very great depths, a fact which is doubted by naturalists." F.W. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait, to co-Operate with the Polar Expeditions: Performed in His Majesty's Ship Blossom, Under the Command of Captain F.W. Beechey, R.N., F.R.S. &c. in the Years 1825, 26, 27, 28.* (London: Henry Colburn and Richard Bentley, 1831), vol. 1, 191–92. For biographical details on Beechey, see William R. O'Byrne, *A Naval Biographical Dictionary: Comprising the Life and Services of Every*

approximated the explanations for the form of atolls advanced by Eschscholtz on one hand, and Quoy and Gaimard on the other. In Beechey's view, "[i]t would be immaterial which of these theories were correct, were it not that in the latter instance the lagoon that is formed in all the islands of this description might be occasioned by the shape of the crater alone, whereas, in the former, it must result from the propensity of the coral animals."[154] Beechey was quick to point out that either of these theories, if they were confirmed, would have implications that would resound through a number of different sciences. Thus, "the consideration of the nature of their foundation" was in fact "not immaterial."[155] If corals could be demonstrated to erect circular reefs by instinct, for example, this would "[form] a remarkable and interesting feature of their natural history."[156] Alternatively, if Quoy and Gaimard were correct, then "we shall have examples of craters of considerably larger dimensions, and more complete in their outline, than any that are known upon the land, which, if true, is a curious fact."[157]

Along with these reflections on previous accounts of reef formation, Beechey's *Narrative of a Voyage to the Pacific and Beering's Strait* described what he took to be typical features of reefs and lagoons, based on the most extensive hydrographic observations yet made of atoll morphology.[158] On his way through the Pacific to meet up with John Franklin's westbound Arctic expedition, Beechey had surveyed thirty-two atolls in Bougainville's Dangerous Archipelago. They ranged in size from as much as thirty miles in diameter to less than a mile across, and "all appeared to be increasing their dimensions by the active operations of the lithophytes, which appeared to be

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Living Officer in Her Majesty's Navy (London: John Murray, 1849), 66–67; Dawson, *Memoirs of Hydrography*, 111–15.

154. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 192.
155. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 192.
156. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 192.
157. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 192.
158. Beechey's general discussion of the "Peculiarities of the coral islands" is in vol. 1, 186-195. His descriptions of individual coral islands are found throughout his first volume in the order that he encountered them.

gradually extending and bringing the immersed parts of their structure to the surface."[159] While his primary task had been to document the obstacles to safe navigation, he claimed that he was "not inattentive to the subject [of atoll formation], and when opportunity offered, soundings were tried for at great depths, and the descent of the islands was repeatedly ascertained as far as the common lines would extend."[160] He offered the results of these "experiments" in a plate showing "a section of a coral island from actual measurement," and depicting the mastery required to make such measurements.[161] He reported that the lagoons that he had been able to survey had depths of 20 to 38 fathoms [120-228 feet], but he had seen others that were too shallow to navigate. He was "tolerably certain" that corals formed the base of these lagoons, which indicated to him that "unless depositions of sand or other substances, obnoxious to coral insects, take place, [lagoons'] depth must depend on their age."[162] Regarding the "rapidity of the growth of the coral," Beechey lamented that "[v]ery little offered itself to our notice, by which we could judge [it]." The problem was in effect a hydrographic one, for as he explained it, "the islands which we examined had never been described with the accuracy necessary for this purpose; and there were, consequently, no means of comparing the state in which they were found by us, with that which was presented to our predecessors."[163] Various pieces of anecdotal and unsystematic evidence were available, but none allowed him to make a final judgment. Likewise on the broader question of the origin of atolls, Beechey was convinced that resolution would lie in accumulation of more observations by future surveyors. "The subject of the formation of these islands is one of great interest," he observed, but it would "require a numerous and careful collection of facts before any entirely

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159. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 186–87.
160. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 193.
161. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, v. 1, 193.
162. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 190.
163. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 190.

satisfactory conclusion [could] be arrived at."[164]

Charles Lyell reproduced Beechey's sectional diagram in the second volume of his *Principles of Geology*, which came out within the year. The two had discussed coral reefs even before the publication of the first volume of the *Principles*, and Lyell felt able to speculate where Beechey had been unwilling. Approvingly citing Quoy and Gaimard's figures on the depth limits of coral growth, he ruled wholeheartedly in favor of the crater-rim theory, which dovetailed with his ideas about volcanic action. Lyell had already explained a mechanism by which large craters would be formed beneath the sea in areas inhabited by reef-building corals. He described an imaginary active submarine volcano undergoing successive eruptions: "[I]n hot countries coral reefs, must often, during long intervals of quiescence, obstruct the vent, and thus increase the repressive force and augment the violence of eruptions. The probabilities, therefore, in a submarine volcano, of the destruction of a larger part of the cone and the formation of a more extensive crater, are obvious." [165]

This was not the only way that Lyell integrated his discussion of coral reefs with the lessons of his earlier volume, in which he had argued that igneous forces modify the earth's crust "by depressing one portion, and forcing out another."[166] These compensatory movements occurred frequently, and a given area could undergo both elevation and subsidence, but long-term trends would result in sea beds being raised into continents, and vice versa. If a coral reef subsided, its surface would be restored to sea level by the growth of new corals, but the next elevation would turn the new part of the

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164. Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 192.

165. This explanation was advanced to dispute Leopold von Buch's paroxysmal theory of "elevation craters." Lyell description of successive submarine eruptions was meant to demonstrate that the features that Buch and Elie de Beaumont called "craters of elevation" could be explained by reference to actual (i.e., contemporary) causes despite their large size. Charles Lyell, *Principles of Geology, Being an Attempt to Explain the Former Changes of the Earth's Surface, by References to Causes Now in Operation* (London: John Murray, 1830–33), v. 1, 391–93.

166. Charles Lyell, *Principles of Geology* (1st Ed), v. 1, 167–68.

reef into a high island composed of coral. The scarcity of upraised coral islands reported by Beechey indicated to Lyell that in the Pacific "the amount of subsidence by earthquakes exceeds...at present the elevation due to the same cause."[167] Could they be studied, the submarine coral formations of the Pacific would probably be stratified by "the arrangement of different species of testacea and zoophytes, which inhabit water of various depths, and which succeed each other as the sea deepens by the fall of the land during earthquakes, or grows shallower by elevation due to the same cause."[168] Thus, for Lyell, coral reefs were a record of the tectonics of the Pacific ocean floor.

Two other prominent British geologists, John MacCulloch and Henry de la Beche, offered divergent views on the geological significance of coral reefs in the same year that Lyell's volume was published. Writing explicitly in the genre of "theories of the earth," MacCulloch argued that corals were distinctive because "[t]he strata which they form are at once living and fossil."[169] For the geologist, who usually could "only infer" that organic processes contributed to the formation of particular rocks, corals "filled up" what was usually a "complete chasm between the labour and its produce." He considered the work of corals to be "extend[ing] the dominion of man over a far wider range" by forming "new parts of our own earth, not mere preparations for a future one."[170] It had taken MacCulloch several years to get his manuscript published, and although it cited the work from Kotzebue's voyage it was in many ways outdated by the time it appeared in print.[171] Indeed, De la Beche's book, though published in the same year as MacCulloch's, reported that Chamisso's account from the Kotzebue voyage had already "been so often quoted that it must be familiar to most readers."[172] The content

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167. Charles Lyell, *Principles of Geology* (1st Ed), v. 2, 296.
168. Charles Lyell, *Principles of Geology* (1st Ed), v. 2, 295.
169. John MacCulloch, *A System of Geology, with a Theory of the Earth, and an Explanation of Its Connexion, with the Sacred Records* (London: Longman, Rees, Orme, Brown, and Green, 1831), vol. 1, 336.
170. MacCulloch, *System of Geology*, vol. 1, 334–36.
171. David A. Cumming, "John MacCulloch (1773–1835)," in *DNB* (2004).
172. H.T. De la Beche, *A Geological Manual* (London: Treuttel and Wuertz, 1831), 141.

of MacCulloch's book placed him among those whom De la Beche accused of holding "very exaggerated ideas" about the "relative importance" of coral reefs.[173] De la Beche was compelled by Quoy and Gaimard's arguments to doubt that corals had formed large masses of rock, and he deemed their crater-rim theory "far from improbable."[174] Unlike most of the commentators on their work, however, he pointed out that the Frenchmen had neglected to explain the origin of reefs that formed barriers at some distance from high islands or continents.[175] It was a criticism that would have applied equally to Quoy and Gaimard's rivals.

Sending a hydrographic survey through the coral seas

It should not be surprising, given the interest in coral reefs among naturalists, geologists, and hydrographers in 1831, to learn that a survey vessel dispatched to the tropics that year by the British Admiralty carried instructions to study coral islands. What was noteworthy was that these directions envisioned the survey as a direct test of the latest theory of atoll formation. They were part of the "Memoranda for Commander Fitzroy's orders" written by Francis Beaufort, a scientifically minded surveyor who had recently ascended to the administrative position of Hydrographer of the Admiralty. The first aim of this voyage, which would be undertaken in the brig *Beagle*, was to carry out a new survey of the southern coasts of South America, eliminating the "motley appearance of alternate error and accuracy" that bedeviled older Spanish charts.[176] After

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173. Beche, *A Geological Manual*, 140.
174. Beche, *A Geological Manual*, 141.
175. Beche, *A Geological Manual*, 142.
176. Francis Beaufort, "Memoranda for Commander Fitzroy's orders," 11 November 1831. Archive of the United Kingdom Hydrographic Office [UKHO] MB 2, pp. 2-24; reprinted in Janet Browne and Michael Neve, eds, *Voyage of the Beagle: Charles Darwin's Journal of Researches* (London: Penguin, 1989), 384–99. Quotation from p. 391. Many of the specifics of Beaufort's orders address concerns raised by Rear Admiral R.W. Otway from his post at Rio de Janeiro in August 1828, where his Flag Lieutenant was the twenty-three year old Robert FitzRoy. See Minute of 14 August 1828, UKHO MB 1, p. 206. On FitzRoy's first post in South America see Dawson, *Memoirs of*

completing the South American survey, the 26 year old Robert FitzRoy was to continue taking meridians "at some judicious chronometer stages" across the Pacific, making "the intervening islands...standard points to which future casual voyagers will be able to refer their discoveries or correct their chronometers."[177] The making of standard points was one of the most important jobs of the official surveys. It meant using all available scientific means to fix the geographical location of a spot like a port or an island. Other navigators who arrived there could use the surveyor's result to reestablish their own latitude and longitude, about which they might have accumulated a great deal of uncertainty.

Because this westward run of point-making away from the coast of South America would put the *Beagle*'s course directly among the "circularly formed Coral Islands in the Pacific," Beaufort instructed FitzRoy to investigate them along the same lines that Beechey had done: "While [your astronomical observations] are quietly proceeding, and the chronometers rating," he wrote, "a very interesting inquiry might be instituted respecting the formation of these coral reefs."[178]

Beaufort's directions to FitzRoy were aimed precisely at the point of contention between recent explanations for the form of coral islands. "A modern and very plausible theory has been put forward, that these wonderful formations instead of ascending from the [bottom of the] sea, have been raised from the summits of extinct volcanoes; and therefore the nature of the bottom at each of these soundings should be noted, and every means be exerted that ingenuity can devise of discovering at what depth the coral formation begins, and of what materials the substratum on which it rests is composed." In addition to the usual nautical charts that FitzRoy would produce,

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Hydrography, part 2, 14.

177. Beaufort, "Memoranda for Commander Fitzroy's orders." Browne and Neve, *Voyage of the Beagle*, 392.

178. Beaufort, "Memoranda for Commander Fitzroy's orders." Browne and Neve, *Voyage of the Beagle*, 397.

Beaufort demanded "An exact Geological map of the whole island [showing] its form, the greatest height to which the solid coral has risen, as well as that to which the fragments appear to have been forced. The slope of its sides should be carefully measured in different places, and particularly on the external face, by a series of soundings, at very short distances from each other, and carried out to the greatest possible depths, at times when no tide or current can affect the perpendicularity of the line."[179]

By instructing FitzRoy in the theoretically relevant features of any given reef, Beaufort set into motion a long range plan for the systematic study of coral islands, building on the foundation set by Beechey's recent survey.[180] As Beechey had implied, it was taken for granted that general knowledge would issue from the accumulation of specific facts from one survey to the next.[181] After they had discussed the newly issued orders, the Beaufort mailed FitzRoy "a couple of [tracings of] Beechey's Coral Isl[an]ds not with any view of your visiting these particular islands...but that you might see the humour of these formations."[182] In a public notice of the voyage the week it departed, the *Athenaeum* adverted that "The most interesting part of the *Beagle*'s survey will be

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179. Beaufort, "Memoranda for Commander Fitzroy's orders." Browne and Neve, *Voyage of the Beagle*, 397. Here I have followed the original wording and punctuation of the manuscript version, UKHO MB2, p. 21.

180. Randolph Cock, "Rear-Admiral Sir Francis Beaufort, RN, FRS: 'The Authorized Organ of Scientific Communication in England, 1829–55'," in *Science and the French and British Navies, 1700–1850*, ed. Pieter van der Merwe (London: National Maritime Museum, 2003), 99–116; Randolph Cock, *Sir Francis Beaufort and the co-Ordination of British Scientific Activity, 1829–55*, Ph.D. Thesis, University of Cambridge (2003). Cock's thesis served me as an indespensable guide to the complex organization of the UKHO archive.

181. Beechey's 1825-1828 voyage, though it was the source of his general observations on coral reefs, was undertaken before Beaufort became Hydrographer. His instructions stand in striking contrast to those of the 1830s. Although he was directed to particular coral islands (e.g., to "ascertain...whether Ducie's and Elizabeth Islands be not one and the same," p. x), and it was "expected that your visits to the numerous islands of the Pacific will afford the means of collecting rare and curious specimens in the several departments of [natural history]," (p. xii), Beechey's special attention to the form of coral islands was not mandated in his instructions. Beechey reproduced the memoranda for his instructions in Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, ix-xvi.

182. Beaufort to FitzRoy, 14 November 1831, UKHO LB 3, p. 280.

among the coral islands of the Pacific Ocean...and the hypothesis of their being formed on submarine volcanoes will be put to the test. [...T]he surveys of [the lagoons] will form, with those of Captain Beechey in his late voyage, the basis of comparison with others at a future period, by which the progress of the islands will be readily detected."[183] For the rest of Beaufort's tenure the wording of FitzRoy's instruction served as the template for a similar paragraph written into the orders of all surveyors dispatched to the coral seas.

Beaufort's agenda for reform was carried out with advice and cooperation from the metropolitan scientific elite at every step. The coral reef investigation was no exception. Lyell had become a frequent visitor to the Hydrographic Department and delighted in telling Gideon Mantell that "our new hydrographer, Beaufort, is very liberal to all geologists, and you may get what unpublished information you like from the Admiralty, and there is an immense deal there."[184] Surveyors' reports were an important source of material for Lyell's *Principles*, which one of Beaufort's officers vetted in "all the *nautical* or hydrographical parts."[185] This exchange was of mutual benefit, as Lyell wrote to his fiancée just after the *Beagle*'s departure, "for I find every day the hydrographers are coming to me for instructions. I have just drawn up some for Captain Fitzroy, who has my book, and is surveying in South America. Captains Hewett, Beaufort, King, Vidal, and others, are in continual communication."[186] For Lyell's part, he believed that the seeds he planted in the surveyors' instructions would yield an abundant crop of new facts to support his treatise, all the better to stifle the

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183. "Scientific Voyage," *Athenaeum* 217 (24 December 1831): 834–35. The topic of coral reef formation had received wide coverage that year in reviews of Beechey's narrative.

184. Lyell to Gideon Mantell, 15 February 1830. Katherine M. Lyell, ed., *Life, Letters and Journals of Sir Charles Lyell, Bart.* (London: John Murray, 1881), v.1, 261–62.

185. Lyell, "Journal to Miss Horner," 4 February 1831. Katherine M. Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, v.1, 368–69.

186. Lyell, "Journal to Miss Horner," 13 February 1832. Katherine M. Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, v.1, 371.

views of his geological opponents.[187] He must have been pleased to know that Beaufort had added the status of official policy to a test of the very coral theory that Lyell was sending to press.

There must be no doubt, however, that from Beaufort's perspective he stood to gain from incorporating the latest theory of coral reefs into FitzRoy's directions. Documenting the location of reefs was, of course, fundamental to the Hydrographical Department's practical mission to combat the dangers of navigation. However, it was acknowledged at the Admiralty that "there would be much of discouragement attached to such surveys if changes should be constantly & rapidly at work in [coral] seas." Therefore the Lords encouraged Beaufort to "direct [his officers'] attention more particularly to the formation and growth of coral reefs."[188] If it were possible to account for these processes, one could suggest where new coral islands were most likely to appear and indicate how long existing charts might be relied on before new reefs would render them obsolete. Beaufort lived by the principle that facts taken from the periphery of the known world were to be systematized by savants in the metropolis, who in turn would direct the next wave of collecting. As in his projects to bring the collective observations of surveyors to bear on magnetic variation and the tides, Beaufort married theorists' questions with Admiralty interests to pursue a philosophical solution to the navigator's practical problem of coral reefs.

Neither Beaufort nor Lyell rested patiently on the coral matter for the *Beagle* to return. In fact, FitzRoy was only just crossing the Pacific when Beaufort arranged for a different surveyor to make another test of the crater rim theory.[189]

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187. Lyell, "Journal to Miss Horner," 13 February 1832. Katherine M. Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, v.1, 371.

188. Beaufort reported these concerns from Lord Auckland, then First Lord of the Admiralty, to Owen Stanley on his dispatch to survey the perilous Torres Straits. Francis Beaufort to Owen Stanley, 28 November 1846. UKHO LB 14, pp. 214-215. Cited in Cock, *Sir Francis Beaufort*, note 420.

189. Darwin's diary entry for 19 December 1835 reads "In the evening we saw in the distance New Zealand.-- We may now consider ourselves as nearly without the limits of the Southern Pacific

In 1835 it had been seven years since the end of Beechey's voyage in the *Blossom*, and Beaufort prepared to send him westward again to keep up the South American survey wherever FitzRoy might leave off, and then like FitzRoy to press on into the Pacific. There he would fall in again among the coral islands whose "general humour" he knew as well as any other surveyor. Whereas FitzRoy was going to try to test the crater-rim theory by sounding outside the reef, Beechey would have an entirely novel means of putting the theory on trial. Arrangements for Beechey's departure were nearly sewn up on 7 December 1835 when Beaufort wrote in his official capacity to an H. Porter, Esquire:

> "Sir
> I am commanded by the L[ords] C[ommissioners of the] A[dmiralty] to procure a boring Machine to perforate coral to the depth of not more than 100 feet, and you will do me a great favour if with your usual friendly activity you will at once order for me whatever may be necessary for that purpose. But as the Ships will sail in 8 days there is no time to be lost. – Can a set be picked up in London[?[190]]

It is not clear that Beechey, who had already expressed serious disdain to Beaufort at the mere possibility of taking a "philosopher" like FitzRoy's Charles Darwin on the voyage, was particularly enthusiastic about this assignment. He was almost certainly not experienced in operating a boring machine, because Beaufort noted, in his letter to H. Porter, "Pray excuse all this trouble and add one more act of kindness by procuring for me (from any quarter) directions for using the said machine to people who are quite ignorant of the subject." Aside from mechanical advice, Beaufort also sought scientific guidance, encouraging Lyell, who was then president of the Geological Society of London, to "inform me whether there are any points connected with this [boring]

Ocean." Richard Keynes, ed., *Charles Darwin's Beagle Diary* (Cambridge: Cambridge University Press, 1988), 380 [Darwin's p. 650].

190. Beaufort to H. Porter 7 December 1835 UKHO LB6: 1834-36, p. 329.

experiment to which you would direct Captain Beechey's attention."[191]

Days later Beaufort handed down the instructions for Beechey. They were similar in structure and content to those issued to FitzRoy in 1831. The section on "Coral Formations" explained how Beechey was to go about testing the notion that coral islands sat atop extinct craters.

> In order by a satisfactory experiment to bring this question to a direct issue, their Ldps have ordered you to be supplied with a complete set of the Boring apparatus used by miners, leaving it to your own judgment to select any coral Island which may be well adapted to the purpose and which will lead you as little as possible from the line of your Survey. They wish you to fix upon a convenient spot of the island where the operation cannot be disturbed by the surf, and there to bore perpendicularly so as to perforate the whole thickness of the coral. and to enter the tool sufficiently deep in the rock on which it is based to furnish ample specimens for future analysis. You will of course keep a register of the contents of the Auger every time it is withdrawn, and if the structure or density of the Coral appear to change it will be desirable to have a series of such specimens also preserved and tallied with their corresponding depths.[192]

Beaufort evidently expected the crater-rim explanation to hold up to this new evidence. In his letter to Lyell he had expressed his confidence that Beechey would "bring up ample specimens of the rock on which [the coral] rests, as well as of the coral at different depths if it appears to alter in its structure or density," yet the boring machine he had requested was intended to penetrate just 100 feet. The idea that the foundation of a coral reef would be located within 100 feet of sea level could only have been based on the depth limits of coral growth set forth by Quoy and Gaimard seven years earlier.

Perhaps because the enterprise was only in the last days before departure, Beechey departed without being certain exactly where the boring experiement ranked in priority, or where exactly it should happen. Not long after setting sail Beechey wrote back to Beaufort from the Atlantic island of Madeira. "I am some what puzzled to

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191. Beaufort to Lyell, 13 December 1835. UKHO LB6: 1834-1836, p. 333.
192. Beaufort, "Memoranda for Capt[ain] Beechey's Orders," 19 December 1835. UKHO MB 2, pp. 218-246, quotations from pp. 241-242.

know how to run a course to this said 'coral island' It is quite clear that we must go on purpose, but I shall any way take care to waste no time."[193] Beechey said he was prepared to devote two months to the boring. The problem was that the Hydrographer had left it to Beechey's "own judgment" to choose an island for the experiment. Quite evidently, surveyors under Beaufort's orders were not accustomed to finding that it didn't matter where they decided to carry out their work. Beaufort's response, sent across the Atlantic, was hardly less enigmatic: "In your Madeira letter you express some doubt about the Coral Island at which it was intended you should operate, -- but it the orders were purposely left thus open that you might select which you like, -- for undoubtedly you must go direct to one of them, -- none lying in your tracks."[194]

The boring itself is not a topic for this chapter. When the miner's apparatus was finally put into action in 1840, Beechey had long since been invalided home. Dispatched from England to fill Beechey's place was the man who had served as Beechey's Assistant Surveyor on the *Blossom*, an enthusiast of geology, Edward Belcher.[195]

My purpose in describing these sets of surveying instructions has been to illustrate that the practice of surveying began to draw on the taxonomic understanding of islands and by extension the theories that set out to explain their origin. FitzRoy and Beechey (and subsequently Belcher) were dispatched to examine features that were irrelevant to navigation in the immediate term but which promised to be important over the long term if they helped to explain how, or how fast, reefs were formed and to predict where new reefs might occur. There were no "raw data" generated during these

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193. Beechey to Beaufort, January 1836. UKHO SL 113: Beechey 1830-39.
194. Beaufort to Beechey, 2 March 1836. UKHO LB6: 1834-1836, pp. 389-390.
195. Belcher made mineralogical collections during the voyage of the Blossom. On his "scientific attainments," see Beechey, *Narrative of a Voyage to the Pacific and Beering's Strait*, vol. 1, 133. For a chronology of Belcher's succession to the command of the *Sulphur*, see Edward Belcher, *Narrative of a Voyage Round the World Performed in Her Majesty's Ship Sulphur..1836–1842* (London: Henry Colburn, 1843), preface and chapter 1.

surveys of coral islands. What it meant to survey a location was becoming governed by theories of how it had been formed and, by extension, how it might change. I am not arguing that theory corrupted surveying. I contend that for Beaufort and company, a theory of landscape change was a tool to make surveying more efficient and valuable.

Conclusion

By the 1830s, offering a theory of coral reef formation meant attempting to answer straightforward questions. What caused the shape of atolls, and how did they originate in unfathomably deep waters? Their solution was seen to lie in a synthesis drawn from navigators' reports, inquiries into the conditions of coral growth, and the "theories of the earth" being expounded by geologists. Because of the particular explanations that had been offered for atolls, any solution would automatically have multiple implications. From Forster onwards, a theory of atoll formation must either support or refute the extraordinary claim that tiny animals were actively combating the powers of erosion by producing massive amounts of new rock. Likewise, Quoy and Gaimard had ensured that new features of the natural history of corals must be accounted for in any future synthesis. It was by then taken almost for granted that corals could build continuous masses of stone. There was less consensus as to how much stone they produced--that is, how far beneath sea level coral formations began--and why they built in particular shapes. Other imaginable questions, such as the cause of reefs that encircled high islands, were acknowledged to lie outside the main stream of debate, if they were mentioned at all. It is telling that Beechey could refer to a "general opinion" about the nature of coral reefs while he identified competing theories that claimed to explain their acknowledged features. Many of Forster's conclusions about the submarine composition of low islands had been rejected, but his formulation of the problem remained salient. Moreover, the perception that coral reefs were relevant to broader questions about the permanence of oceans and continents, and the rate and directionality of changes to the earth's crust, could legitimately be attributed Forster, whose theory had implied that corals could potentially raise virtually any spot of the tropical ocean floor into habitable land.

To a reader who stayed current with French or English literature in natural history, geology, or navigation in the years around 1830, the actors I have described in this chapter could have been readily identified. Few historians of science, however, have discussed the question of coral reef formation as a problem of sustained interest in this period. None to my knowledge has made a comparison of the field experiences (if any) and the practices employed by these mutually-recognized protagonists of the coral reef debate; nor do we have a detailed comparison of their published theories. A satisfactory work along these lines would fill more space than a single chapter allows, but this preliminary attempt nevertheless reveals several themes that might usefully guide further analysis, and which (as the following chapters show) remained important in subsequent coral reef study.

My actors' explanations for reef formation were inextricably linked with the practices they used to investigate the reefs. Their most widely discussed features--the superficial shape of individual reefs, their elevation with respect to sea level, and the distribution of reefs across the ocean--were those most readily documented by the hydrographic surveying practices characteristic of voyages of discovery. The work of hydrographers was readily acknowledged by geologists like Lyell who analyzed coral reefs by exploring the charts they had made, but the methods of surveying were also important for naturalists who accompanied hydrographers to coral reefs, as illustrated by Quoy and Gaimard's references to material brought up by the sounding lead.

Besides the obstacle of getting to reefs in the first place, there were two common difficulties that had to be overcome in the first hand study of coral reefs. One was the fact that these formations were changing at an undetermined rate, and the other was the problem of determining the situation underwater. Some idea of the pace of coral growth could be gathered from the testimony of natives who had spent their whole lives on coral islands, but both Chamisso and Beechey indicated that the best evidence would come from closely documented studies of the same location, carried out many years apart. Meanwhile, there were multiple different strategies for dealing with what Quoy and Gaimard called the "impossibility of going underwater." Presented with the exposed coral formations of Timor, they assumed that conclusions about ongoing submarine processes could be drawn from the geological record of lithophytes' works. Chamisso, on the other hand, had taken advantage of the uncommon opportunity to land on living reefs by studying the masses of coral that were thrown up from the outer reef onto its surface. Beechey worked from the assumption, which Beaufort clearly endorsed, that possible modes of formation could be judged using knowledge of the submarine topology of a reef that was determined by systematic use of the sounding lead.

As the works of Lamarck and Lyell indicate, having seen a coral reef first hand was not a prerequisite to offering a theoretical discussion of their shape, extent, or history. Most of those authors who had visited coral reefs, meanwhile, made general statements about reef formation that were guided by the particularities of individual islands. Flinders developed his views on coral growth from the reefs forming the Great Barrier of Australia, and especially from one location, Half Day Island. Péron and Quoy and Gaimard discussed the coral formations on and around the high island of Timor. Chamisso and Eschscholtz had made a careful study of one large atoll in the Radack chain, while Beechey discussed the smaller atolls of the Dangerous Archipelago and gave special emphasis to Whitsunday Island. While the common experiences and destinations of many of these voyagers should not be overlooked (recall that Flinders and Baudin expeditions crossed paths multiple times on the coast of Australia and the ships carrying Chamisso, Eschscholtz, Quoy, and Gaimard met at the Cape of Good Hope), the reefs they encountered were quite distinct. For most of those who saw atolls the annular shape of the reef was a matter of primary interest. Such was the theoretical significance of atolls' form that Quoy and Gaimard were compelled to address the matter although they had not seen one themselves. Beechey, de la Beche, and Lyell in turn cited Quoy and Gaimard's crater-rim explanation in such a way as to suggest that atoll formation had been one of the Frenchmen's main concerns.

For fieldworkers, the contingencies of studying a particular reef or archipelago factored into the shape of an individual's general perspective on the formation of reefs. But let me be clear that general theories in turn changed the type of knowledge that could be considered available in the study of a single locality. This is best illustrated in the case of the hydrographers. Even if we follow Latour in believing that a survey only became knowledge by being removed from its specific locality, we still tend to think of the resulting knowledge as knowledge about that locale. This was Beaufort's general perspective, anyway. He insisted that the knowledge compiled at the Hydrographic Office be a systematic and faithful representation of the surveyed locations. Yet in the case of reef surveys, Beaufort took nearly an opposite approach.

For some reason Beaufort, the high priest of exact geographical knowledge, told both FitzRoy and Beechey to devote substantial time and effort to the examination of a *random* coral island. It was clear in the official orders that the particular island surveyed was beside the point, and when Beechey still couldn't believe it Beaufort reiterated this uncharacteristic lack of concern with locality in private correspondence. Yet these were certainly not whimsical or insignificant undertakins, because they were potentially dangerous and two months of Beechey's surveying time, which he planned to devote to the boring, was an extremely expensive commodity. How can we explain this? A clue lies in Beaufort's comment to FitzRoy, that he might want to familiarize himself with the "general humour" of coral islands. As this suggests, Beaufort was convinced that the lagoon islands were a distinct class of natural objects. And because individual islands were instantiations of this consistent type, then the survey of one ought to provide generally-applicable knowledge about any other low island, known or as yet undiscovered. This was not simply theory-laden seeing, this was theory-laden choice of what to see. Forster's concept that coral islands had a general form that was owing to a general cause helped to give new practical and theoretical meaning to local surveys.

CHAPTER 2
Hydrography as a Resource for Zoology and Geology: The Origin of Darwin's Theory of Coral Reef Formation

Introduction

In his old age Charles Darwin was always proud to recall his first theory, which had secured him the affection of Charles Lyell and helped to make him a new star of London science after the *Beagle* voyage. In his memoir of 1876 he wrote that "it was thought highly of by scientific men, and...is, I think, now well established."[1] Darwin's *Autobiography* also contained a provocative claim about the coral theory: "No other work of mine was begun in so deductive a spirit as this; for the whole theory was thought out on the west coast of S. America before I had seen a true coral reef."[2] The *Beagle* departed from South America for the Galapagos Islands in September 1835, after fifteen months on the west coast. He claimed that when he had visited "living reefs" (at the Low or Dangerous Archipelago and the Society Islands of the Pacific in late 1835, and Cocos (Keeling) Atoll and Mauritius in the Indian Ocean in the spring of 1836) he "had therefore only to verify and extend my views."[3] According to these reflections, Darwin had "been incessantly attending to the effects on the shores of S. America of the intermittent elevation of the land, together with the denudation and the deposition of sediment. This necessarily led me to reflect much on the effects of subsidence, and it was easy to replace in

1. Charles Darwin, *The Autobiography of Charles Darwin, 1809–1882*, ed. Nora Barlow (London: Collins, 1958), 98.
2. Charles Darwin, *Autobiography*, 98.
3. Charles Darwin, *Autobiography*, 98.

imagination the continued deposition of sediment by the upward growth of coral. To do this was to form my theory of the formation of barrier-reefs and coral atolls."[4] By this account, therefore, the origin of the coral theory had nothing to do with living corals and everything to do with the geology of South America. Darwin offered no explanation for why he might have begun to contemplate coral reefs as he rambled over the western slopes of the Andes.

I will demonstrate that the study of coral reefs was a priority that had been made explicit by Darwin and members of the Navy long before the *Beagle* reached the west coast of South America. Corals and other "zoophytes," as Darwin called them, were in fact often at the forefront of his activities in the first years of the voyage. He incorporated this careful study of *Atlantic* marine invertebrates into his account of coral reef formation, and exploited his familiarity with such creatures when he did visit living reefs in other oceans. I will futher argue that Darwin's intense reflections on the geology of South America, which did play an important role in his thinking about coral reefs, were intimately bound up with his work in marine zoology. The two pursuits were separately encouraged, and inextricably connected, by the hydrographic surveying that constituted the main labor of Captain FitzRoy and his officers. The coral theory was a direct product of the maritime practice of sounding that Darwin discovered for himself while aboard ship. In the course of this argument it will become clear that Darwin did not possess a theory of coral reef formation, in the sense that he and his contemporaries would have defined it, until after he had left South America far behind.

If the coral theory was not really thought out on the coast of Chile, it was nevertheless prompted by a remarkably discrete moment of insight from a remote island. Darwin scholars have lately been at pains to distance themselves from the idea that Darwin's evolutionary theory was the product of a eureka moment at the Galapagos, so

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4. Charles Darwin, *Autobiography*, 98–99.

it may seem retrograde to argue, as I do, that he had one at Tahiti. Unlike the theory of natural selection, however, Darwin did develop a coral theory during the voyage. He wrote a lengthy account of it while aboard the *Beagle*, revised, expanded, and even circulated it to FitzRoy before arriving back in England. The fact that he became self-consciously aware of having a new theory of atoll formation immediately after being "forcibly struck" by an intuition does not mean that it cannot be explained, or that it remained static afterward. What he gained at this moment of insight was a way of applying things he already knew to a problem he had previously conceived as part of a different domain. The theory gave Darwin a new (metaphorical) point of view prompted by a literal, and geographically specific, new vantage point.

Historians have most often addressed Darwin's coral theory in one of two recognizably different ways. The first has been in the context of his contributions to geology, the science in which he was most eager for recognition after the voyage.[5] With its implications about the motion of the sea floor and the disappearance of innumerable islands or continents from the tropical ocean, the coral theory held an important place in Darwin's self presentation as a geologist. Moreover, it was integral to his theory of a "simple" global geology predicated on compensatory vertical movements of the earth's crust. The post-*Beagle* development, through collaboration with Lyell, of an explicitly geological coral theory gave the impression that Darwin had only ever contemplated coral reefs as an indicator of crustal movements. Yet a concern with coral reefs runs through many of the manuscripts that Darwin characterized ambiguously, or explicitly as pertaining to zoology. Understandably, these sources have received little discussion

5. See for example David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs"; Sandra Herbert, "Charles Darwin as a Prospective Geological Author," *British Journal for the History of Science* 24 (1991): 159–92; Sandra Herbert, *Charles Darwin, Geologist*; Michael T. Ghiselin, "Introduction," in *The Structure and Distribution of Coral Reefs* (Tucson: Univ. Arizona Press, 1984); Frank H.T. Rhodes, "Darwin's Search for a Theory of the Earth: Symmetry, Simplicity, and Speculation," *British Journal for the History of Science* 24 (1991): 193–229.

in histories of Darwin as a geologist, helping to reinforce the perception that his inquiries into coral reefs were fundamentally and originally about geology (whether defined by Darwin or historians). There is good reason to study the coral theory for its rich lessons regarding Darwin's assumptions about the earth's crust, but there is equally good reason to study the theory on its own terms. The latter approach indicates that a theory of geological change was not the determinate outcome of Darwin's first efforts to understand coral reefs, which in turn offers new insight into the range of inquiries that did contribute to Darwin's eventual theorizing about the changes to the face of the globe.

The second major historiographical theme that has encompassed Darwin's coral work has been the analysis of his development as a theorist, from the passenger on the *Beagle* to the author of the *Origin*.[6] The coral theory serves in these accounts as a type case for his "early" style of thought, which is studied for the sake of comparing it with the theory of natural selection. In one such work, Michael Ghiselin regards the coral theory as "an almost ideal model" of the hypothetico-deductive method of scientific inquiry.[7] He considers this version of the scientific method so central to, and consistent in, Darwin's thought, that he points out that it is "easier to evaluate [criticism of Darwin's theory of natural selection] with respect to coral reefs because the evolutionary theories, while similar in principle, are far more intricate."[8] There are a number of problems with this approach as a means of understanding the development of

6. Ghiselin, *The Triumph of the Darwinian Method*; Howard E. Gruber, "Going the Limit: Toward the Construction of Darwin's Theory," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 9–34. Let me be clear that Ghiselin and Gruber take distinct approaches to studying this question. Gruber is much more attentive to Darwin's unpublished notes, and to the differences between the content of his private papers and his published works. Gruber gives a criticism of Ghiselin's method in Howard E. Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity: With Darwin's Early and Unpublished Notebooks Transcribed and Annotated by Paul H. Barrett* (New York: Dutton, 1974), 131.

7. Ghiselin, *The Triumph of the Darwinian Method*, 24.

8. Ghiselin, *The Triumph of the Darwinian Method*, 30.

Darwin's coral theory. Ghiselin relies on the 1842 book, supplemented by the *Autobiography*, for a static version of the coral theory that may be stylized for comparison with Darwin's other projects. In doing so he neglects to account for Darwin's rhetorical strategy and intended audiences, treating these works as equally reliable sources of "Darwin's original idea[s]."[9] Thus Ghiselin unsatisfyingly interprets the careful fashioning of Darwin's written arguments as evidence of a remarkably logical and consistent method in his original research. This second approach then, is even more explicitly beholden to Darwin's later expositions and retrospections about the coral theory.

My account of the origins of Darwin's interest in coral reefs, and his development of a theory to explain them, might be considered a study of his "private science."[10] I have drawn on the enormous, and (to my immense good fortune) increasingly well catalogued, collection of Darwin's papers at Cambridge.[11] The field and specimen notes, essays, and correspondence from the *Beagle* voyage demonstrate that Darwin's views on coral reefs were more numerous, mutable, and contingent than he suggested in his published recollections. This focus on manuscript material is far from novel in Darwin scholarship. My footnotes record a massive debt, not least to Richard Keynes and Sandra Herbert for their respective achievements in elucidating Darwin's zoological and geological notes.[12] In treating the coral theory for its own sake, however, I have identified several important strands that have been

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9. Ghiselin, *The Triumph of the Darwinian Method*, 23. For an eloquent demonstration of how a scientific paper disguised, rather than revealed, the authors' motivations for undertaking the work and the means by which he produced his results, see Warwick, *Masters of Theory*, chapter 1.

10. Geison, *The Private Science of Louis Pasteur*. Martin J.S. Rudwick, "Charles Darwin in London: The Integration of Public and Private Science," *Isis* 73 (1982): 186–206.

11. These are catalogued under DAR at Cambridge University Library (hereafter, CUL). I am grateful to have had the opportunity to use Nick Gill's remarkably detailed DAR inventory in the manuscript reading room at Cambridge.

12. Richard Keynes, ed., *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle (Cambridge: Cambridge University Press, 2000); Sandra Herbert, *Charles Darwin, Geologist*.

underemphasized by those who have focused on Darwin's geology or who have sought foremost to understand his progression to the theory of natural selection.[13] The payoff is not only a new version of the coral theory, but also a better understanding of this important period in Darwin's life, of his dependence on maritime culture, and his view of the sciences he practiced.

Darwin's background in marine zoology and geology

As I argued in chapter 1, the formation of atolls had become a well defined problem for European naturalists since Cook's voyages. Thus the Hydrographer of the Admiralty, Francis Beaufort, gave instructions to FitzRoy that included the assignment to study coral islands in such a way as to analyze the theory that they were founded upon volcano craters just below sea level, which had been proposed by Quoy and Gaimard and promoted by Lyell. There is no sure evidence for when Darwin first became aware of the curious problem of atoll formation and the possible geological implications of coral growth. Certainly, he was familiar with these issues when the ship sailed from England, by which time he taken an idiosyncratic education, curricular and otherwise, that offered him a wide variety of approaches to the study of the natural world. James Secord, Phillip Sloan, and Jonathan Hodge, among others, have overturned the perception that Darwin departed on the *Beagle* as an inexperienced naturalist.[14] In two

13. David Stoddart has considered Darwin's coral theory from a number of perspectives in several articles among his any contributions to the history of coral reef science. David R. Stoddart, "Darwin and the Seeing Eye"; David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs"; David R. Stoddart, "Grandeur in This View of Life"; David R. Stoddart, "Theory and Reality"; David R. Stoddart, "This Coral Episode". Stoddart also transcribed and edited Darwin's first manuscript statement of his coral theory. David R. Stoddart, "Coral Islands by Charles Darwin".

14. James A. Secord, "The Discovery of a Vocation: Darwin's Early Geology," *British Journal for the History of Science* 24 (1991): 133–57; Phillip Sloan, "Darwin's Invertebrate Program, 1826–1836: Preconditions for Transformism," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 71–120; Phillip Sloan, "The Making of a Philosophical Naturalist," in *The*

years as a medical student at Edinburgh, and three more at Cambridge, where he passed his examination for the bachelor of arts degree in January 1831, Darwin had studied zoology, botany, chemistry, and geology from men with diverse, and sometimes mutually antagonistic, perspectives. In lecture halls, museums, and field excursions, Darwin was exposed to the extreme viewpoints of Wernerian and Huttonian geology.[15] He saw models of the scientific lifestyle that ranged from the pious curiosity of the parson naturalist to the subtle ruthlessness of a junior academic. Most importantly, he collaborated in genuine research with adepts in marine zoology and field geology.

As a diversion from his medical studies in Edinburgh, Darwin apprenticed himself to the zoologist Robert Grant, who had studied in Paris with Cuvier and Geoffroy.[16] Grant was fascinated by the marine "zoophytes," plant-like colonial creatures--including corals--that he considered to straddle the boundary of the animal and vegetable kingdoms.[17] He extended recent work by Lamarck and Lamouroux in an effort to discern whether the organization of these animal-plants represented truly

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Cambridge Companion to Darwin, eds M.J.S. Hodge and Gregory Radick (Cambridge: Cambridge University Press, 2003), 17–39; M.J.S. Hodge, "Darwin as a Lifelong Generation Theorist," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 207–44.

15. Secord, "Discovery of a Vocation"; Sandra Herbert, *Charles Darwin, Geologist*, chapter 1; Janet Browne, *Charles Darwin: Voyaging* (Princeton, N.J.: Princeton University Press, 1996), 65–72.

16. Adrian Desmond, "'Grant, Robert Edmond'," in *Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2004). See also James A. Secord, "Edinburgh Lamarckians: Robert Jameson and Robert E. Grant," *Journal of the History of Biology* 24 (1991): 1–18.

17. Phillip Sloan has explained the contemporary taxonomic understanding of coral and coral-like organisms: "The discussions in the 1820s and 1830s about the possibility of a true 'zoophytal' creature had involved workers in invertebrate zoology in a search for defining criteria of plants and animals that particularly concerned the status of the colonial invertebrates and infusoria. Another taxonomic group drawn into this problem was the coralline algae, curious forms occurring primarily in warm waters, and often involved in coral-reef formation. Currently these are placed unambiguously among the plants, but they were a subject of substantial debate in the early nineteenth century. On the one hand, many of these forms occurred in branching tufts with a calcareous skeleton, or even in spreading fungus-like forms, strongly reminiscent of some of the corals, and in this were unlike any known plant. On the other hand, they lacked evident polyps, and showed none of the other animal functions. Generally Darwin's authorities placed the 'Corallina' (as distinguished from the animal 'corallines') among the colonial animals, but there was clearly much uncertainty on this." Sloan, "Darwin's Invertebrate Program," 98.

intermediate forms.[18] Edinburgh was becoming the focal point of a new British discipline of marine zoology (embodied most famously during the next decade in the work of Edward Forbes), in which naturalists co-opted the tools and skills of fishermen in order to gather specimens from deeper and more distant waters than before.[19] Along with a fellow student, John Coldstream, Darwin joined Grant in collecting along the shore of the Firth of Forth and on occasional boat rides. He followed Grant's research program into zoophytes keenly, striking out on an independent study of their reproduction with a microscope borrowed from his mentor. He was a keen enough observer to gain insights worthy of a short scientific paper, which he read to the student-run Plinian Society in 1827.[20] Bitterly for Darwin, his conclusions on the "ova" of the zoophyte genus *Flustra* were also sufficiently interesting that Grant preempted him by mentioning them, without attribution, in a presentation of his own to the Wernerian Society three days earlier.[21] Darwin soured on his teacher as a result of this episode, but retained Grant's lessons that these marine organisms demanded further explanation and could be a potent source of broader theoretical insights.[22] Darwin carried this informed curiosity about the zoophytes with him on the voyage, along with a wealth of experience collecting and studying the marine fauna of Edinburgh.

Darwin's second university career, at Cambridge, also led him into original fieldwork with a much more experienced companion than himself, when he accompanied the professor of geology Adam Sedgwick on his first study of north Wales

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18. Adrian Desmond and Sarah E. Parker, "The Bibliography of Robert Edmond Grant (1793–1874): Illustrated with a Previously Unpublished Photograph," *Archives of Natural History* 33, no. 2 (2006): 202–13.
19. Philip F. Rehbock, "The Early Dredgers: Naturalizing in British Seas, 1830–1850," *Journal of the History of Biology* 12, no. 2 (1979): 293–368; Rozwadowski, *Fathoming the Ocean*, chapter 4, especially p. 100.
20. Paul H. Barrett, ed., *The Collected Papers of Charles Darwin* (Chicago: University of Chicago Press, 1977), 285–91.
21. Browne, *Voyaging*, 80–88.
22. Sloan, "Darwin's Invertebrate Program"; Hodge, "Darwin as a Lifelong Generation Theorist".

in the summer of 1831. The two were introduced by John Stevens Henslow, who had taken Darwin under his wing and welcomed him into an extracurricular world of scientific rambles and intellectual dinners with the "learned men" of the university. Henslow and Darwin shared an enormous enthusiasm for Humboldt and together dreamed of retracing his footsteps over the volcanic rocks of Tenerife, a plan that was only superseded when Henslow nominated Darwin for the *Beagle* voyage. With their original objective in mind, however, Darwin remained in Cambridge after taking his degree, learning Spanish while Henslow "crammed" him in geology.[23] Henslow encouraged Darwin to get a clinometer for measuring the direction and inclination of geological strata, but it was Sedgwick who taught him, over a week or more together, how to analyze stratification in the field.[24] It was an opportunity to see how terminology and concepts that Darwin had learned in Edinburgh and Cambridge would be applied by a competent geologist when he made his first visit to a new landscape.[25] From Sedgwick, as from Grant, Darwin learned the practical skills and unwritten rules of contributing new knowledge to a discipline with established methods and conventions. His most advanced practical and theoretical work during the voyage came in the two sciences, geology and marine zoology, that he had learned as an assistant in research. The two pursuits intersected in his study of corals and reefs.

Robert Jameson, the professor of natural history at Edinburgh, probably introduced Darwin to contemporary debates over the origin of coral islands in 1826-1827, the same time Grant was stoking his enthusiasm for zoophytes. Jameson's five-month lecture course, though the subject of derisory comments in Darwin's

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23. Henslow also had field experience as a geologist, and was professor of mineralogy for three years before taking the botany chair in 1825. On Henslow's geology, see Sandra Herbert, *Charles Darwin, Geologist*, 36.
24. Sandra Herbert, *Charles Darwin, Geologist*, 39–47. Darwin and Sedgwick set off together on 5 August 1831 and separated on, or shortly after, 11 August.
25. Secord, "Discovery of a Vocation," 144–50.

autobiography, had a remarkable scope that was unparalleled at any British university.[26] In lecture notes that appear to date from the 1810s and 1820s, Jameson refers to the "coral riffs that surround the islands in the south sea " as contributors to the "unequalities of the bottom of the sea, or that part of the globe which is still covered with water."[27] Another lecture analyzed their process of formation by "the myriads of calcareous zoophytes," which "effect a chemical change on one of the mineral products carried into the sea by every river that flows through a limestone district. [...] The most important productions by the apparently insignificant race of the Polypi are the accumulations of these calcareous skeletons of the Anthozoa, which form the coral islands and reefs, the dread of the navigator."[28] Jameson was also responsible for perhaps the most comprehensive review of coral reef knowledge available in the English language until Darwin's *Structure and Distribution of Coral Reefs* appeared in 1842. In the fifth British edition of Cuvier's *Essay on the Theory of the Earth,* Jameson included a twenty page appendix on coral islands that contained long extracts from Forster and Flinders, along with nearly complete translations of Eschscholtz's report (credited to Chamisso) and Quoy and Gaimard's 1825 article (on all of whom, see chapter 1).[29] Darwin bought the book when it came out in 1827, the same year as he was taking

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26. Charles Darwin, *Autobiography*, 52. Darwin undoubtedly paid more attention to Jameson's lectures and museum specimens than the Autobiography implies, as illustrated by the discussions in Secord, "Discovery of a Vocation," 134–42; Sandra Herbert, *Charles Darwin, Geologist*, 32–36.
27. Jameson lecture notes, Edinburgh University Library Special Collections (EUL) GEN 122. As this reference to the receding universal ocean indicates, Jameson was (to Darwin's retrospective disgust) a noted devotee of the "Neptunist" geology of Abraham Gotlobb Werner.
28. Jameson's lecture notes, EUL GEN 122. This lecture is not in Jameson's handwriting, and may have been copied or translated by a secretary from another author's published work (as was the case with several other sets of notes on the same distinctive blue paper).
29. Robert Jameson, "On the Growth of Coral Islands," in *Essay on the Theory of the Earth. By Baron Cuvier*, ed, trans Robert Jameson (Edinburgh: William Blackwood, 1827), 379–98. Jameson also contributed shorter appendices on coral island formation to the first (1813) through fourth British editions of the book, which were translated by Robert Kerr. The 1827 edition was "translated from the last French edition" by Jameson (although it in fact contained many passages identical to those in Kerr's translation).

Jameson's course.[30] Jameson kept proof sheets of the coral islands appendix among his lecture notes, which suggests that Darwin also heard the material delivered aloud in Jameson's dry style.[31] In his course at Cambridge, Sedgwick too described coral reefs as one of the "great agents by which the earth's surface is modified", though the extent of Darwin's attendance at these lectures is not clear.[32] On the other hand, if he had not already heard Sedgwick expound on the topic before their excursion together in Wales, Darwin may well have received the same information in the field in connection with the fossil corals to be found in formations there.[33]

Regardless of exactly how long Darwin had known of the theoretical questions associated with coral reefs, we can say with confidence that he was pleased with their inclusion in Beaufort's instructions. Darwin arrived in London in early September after gaining his father's permission to accept the *Beagle* invitation. At the time it was not yet confirmed whether FitzRoy would be ordered to complete a circumnavigation after finishing his work on the west coast of South America, or whether he would be ordered directly home by the Atlantic. Unaware of Beaufort's own eagerness to give FitzRoy the liberty to cross the Pacific, Darwin made repeated visits to the Admiralty to lobby for the circumnavigation and its tropical island itinerary. Indeed, with the point in question Darwin witheld his final acceptance of a place on the ship. "The only thing

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30. Browne, *Voyaging*, 72.

31. Jameson's lecture notes, EUL GEN 122. The proof sheets in this folder appear to be from one of the first three editions, as they contain no mention of Chamisso or Quoy and Gaimard.

32. Sedgwick, *Syllabus of a Course of Lectures on Geology*, quoted in John Willis Clark and Thomas McKenny Hughes, *The Life and Letters of the Reverend Adam Sedgwick* (Cambridge: Cambridge University Press, 1890), v. 1, 288–89. Darwin claimed in his autobiography that he had not attended Sedgwick's lectures, though other students recalled differently. See Secord, "Discovery of a Vocation," 143.

33. In a letter written to Darwin after they had separated (in response to a letter from Darwin that is not extant), Sedgwick explains, "Your information did not however surprise me, as madrepores [stony corals] are quite as likely to be met with as terebratulae, which seem to occur here and there thro' ye Snowdonian chain." Adam Sedgwick to Charles Darwin, 4 September 1831. Frederick Burkhardt et al., eds, *The Correspondence of Charles Darwin* (Cambridge: Cambridge University Press, 1985–2006), vol. 1, 137.

that now prevents me finally making up my mind," he reported to his family after almost a week in London, "is the want of *certainty* about [the] S[outh] S[ea] Islands, although morally I have no doubt we should go there whether or no it is put in the instructions: Cap. Fitz says I do good by plaguing Cap Beaufort: it stirs him up with a long pole."[34] Meanwhile, he was gathering advice for his trip from "several great guns in the Scientific World," including useful hints from Grant (who had since moved from Edinburgh to the University of London) on preserving zoophytes in a state fit for dissection.[35] Grant's other former acolyte John Coldstream, who was still in Edinburgh, responded to a request from Darwin by sending illustrations on the construction and use of a dredge for collecting marine specimens, along with a ringing endorsement of Beechey's *Narrative* and other hints on the latest Scottish methods of "obtain[ing] a rich supply" of "the rarest...zoophytes."[36] By the time Beaufort's instructions were entered into the record, Darwin had accepted his place and traveled on to Devonport, where he awaited the *Beagle*'s departure alongside the crew. The Hydrographer's knowledge that FitzRoy was to be accompanied by this young savant with a special enthusiasm to investigate the South Sea islands could only have encouraged him to include the coral reef instruction.[37] Darwin at last felt assured that he would one day go where even Humboldt had not, to explore the tantalizing mysteries of the tropical Pacific.

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34. Darwin to Susan Darwin, 9 September 1831. Burkhardt et al., *CCD*, v. 1, 145.
35. Quotation from Darwin to FitzRoy, [10 October 1831]. Burkhardt et al., *CCD*, vol. 1, 174–75. One of the instructions from Grant was to "kill [zoophytes] by gradual additions of fresh water. so that polypi hang out[, and] Actineae by pouring boiling water in their interiors." DAR 29.3:78.
36. Coldstream to Darwin, 13 September, 1831. Burkhardt et al., *CCD*, vol. 1, 151–53.
37. Sandra Herbert has referred to Beaufort's coral reef instruction as "Darwin's only direct assignment as a geological author." Sandra Herbert, "Charles Darwin as a Prospective Geological Author," 187.

Marine zoology and the origin of Darwin's interest in hydrography

After three eye-opening weeks spent battling seasickness and examining the marine life captured by a net he had thrown over the ship's stern, Darwin's zeal for the sciences he had learned from Grant and Sedgwick was quickened by the *Beagle*'s first landfall.[38] Ashore on the Cape Verde islands in the tropical Atlantic, he wrote "I do not think the impression this day has made will ever leave me.-- The first examining of Volcanic rocks must to a Geologist be a memorable epoch, & little less so to the naturalist is the first burst of admiration at seeing Corals growing on their native rock." As he was to do throughout the voyage, he conceived of tropical corals as the grandest members of the zoophyte group he had begun studying five years earlier. "Often whilst at Edinburgh, have I gazed at the little pools of water left by the tide: & from the minute corals of our own shore pictured to myself those of larger growth: little did I think how exquisite their beauty is & still less did I expect my hopes of seeing them would ever be realized."[39] He was inspired to take out a boat to dredge for corals in the Edinburgh fashion.[40] Over three weeks at the islands he adopted a "usual occupation of collecting marine animals in the middle of the day & examining them in the evening," when he sketched them as they appeared under his microscope.[41] He collected and observed marine fauna, in other words, in the way he had learned from Grant, and examined them for their form, behavior, and "irritability."[42]

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38. Of his plankton net, Darwin wrote, "I proved to day the utility of a contrivance which will afford me many hours of amusement & work.-- it is a bag four feet deep, made of bunting, & attached to [a] semicircular bow [which] by lines is kept upright, & dragged behind the vessel." Entry for 10 January 1832. Keynes, *Diary*, 21. Keynes remarks that this appears to have been among the first uses of such a net for scientific collecting at the water's surface (rather than by dredging along the bottom as Coldstream had recommended).
39. Entry for 17 January 1832. R.D. Keynes, ed., *Charles Darwin's Beagle Diary* (Cambridge: Cambridge University Press, 1988), 24. For other mentions of his coral collecting at the Cape Verdes, see the diary entries for 28 January and 3 February 1832.
40. Entry for 6 February 1832. Keynes, *Diary*, 34.
41. Keynes, *Diary*, 33.
42. On the link between the methodology of Darwin's invertebrate researches at Edinburgh and those

Meanwhile, the geological skills he had acquired from Sedgwick seemed, at these islands, to reinforce the theory espoused in the first volume of Lyell's *Principles*, which he had received from FitzRoy as a present.[43] Darwin believed he saw evidence of a gradual, cyclical change in the relative level of land and sea, similar to that proposed by Lyell in his iconic discussion of the Temple of Serapis at Pozzuoli, Italy.[44] His first letter back to Henslow emphasized "how much I am indebted to [Sedgwick] for the Welch expedition," which had made him capable of discerning that "the geology [of St. Jago, Cape Verde Islands] was preeminently interesting & I believe quite new [with] facts on a large scale...that would interest Mr Lyell."[45] As the voyage progressed, Darwin came to regard the author of the *Principles* as his geological mentor.[46] In doing so, Darwin was casting Lyell in a role that existed because Sedgwick had created it.

Although in a moment of humility he confessed to being anxious "whether I note the right facts & whether they are of sufficient importance to interest others," Darwin already had ample confidence in the novelty and relevance of his observations in the science he knew best. He trusted his zoological training well enough to draw enthusiasm, rather than unease, from anomalous findings. Of a scleractinian coral, he wrote, "I examined pretty accurately a Caryophyllea & if my eyes were not bewitched former descriptions have not the slightest resemblance to the animal."[47] Between the thrill of reconstructing the geological history of a new place and the pride of knowing that he could identify known marine organisms and produce original insights to their

during the voyage, see Duncan Porter, "The *Beagle* Collector and His Collections," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 974–75.

43. On the geology of the Cape Verde Islands as Darwin's inspiration to pursue geology as a vocation, see Secord, "Discovery of a Vocation". For a detailed account of Darwin's collection of specimens and study of the landscape there, along with his considerations of elevatory movements and the origin of "diluvium," see Sandra Herbert, *Charles Darwin, Geologist*, 141–58.

44. Charles Lyell, *Principles of Geology* (1st Ed), v. 1, 453–59 and frontispiece. See also Sandra Herbert, *Charles Darwin, Geologist*, 152–56.

45. Darwin to Henslow, 18 May-16 June 1832. Burkhardt et al., *CCD*, vol. 1, 236–39, on p. 236.

46. As Sandra Herbert has pointed out, Darwin continually characterized his relationship to Lyell as that of a reader to an author. Sandra Herbert, *Charles Darwin, Geologist*, 63–65.

47. Darwin to Henslow, 18 May-16 June 1832. Burkhardt et al., *CCD*, vol. 1, 236–39, on p. 237; Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 14–15.

microscopic structure, the beginning of the journey only reinforced his two strongest disciplinary affiliations: "Geology & the invertebrate animals will be my chief object of pursuit through the whole voyage."[48]

As the officers began to survey the shore of South America, Darwin saw that his zoological inquiries could be materially enhanced by studying this hydrographic work. Off the coast of Brazil, he recorded in his diary that "the labours of the expedition have commenced.-- We have laid down the soundings on parts of the Abrolhos, which were left undone by Baron Roussin." Determining the extent of these shoals was among the first of Beaufort's official instructions to FitzRoy, and the systematic accumulation of data caught Darwin's attention.[49] "The scene being quite new to me was very interesting.-- Everything in such a state of preparation; Sails all shortened & snug: anchor ready to let fall: no voice or noise to be heard, excepting the alternate cry of the leadsmen in the chains."[50] Darwin was curious to analyze the surveyors' results, so on the back of a sheet of zoology notes he began compiling a "table of thermometrical changes during crossing and recrossing the bank."[51] He recorded the time of day when each sounding was taken, the water temperature (to the quarter of a degree Fahrenheit), and the depth measured in fathoms. He had already learned to follow the hydrographic convention for indicating that no bottom had been found at a given depth, by underlining the length of the sounding line in fathoms and placing a dot beneath it. By his third day paying attention to the surveyors' work, he discovered something that made him begin to record hydrographic findings on the front side of the page, in the thick of his zoological notes.

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48. Darwin to Henslow, 18 May-16 June 1832. Burkhardt et al., *CCD*, vol. 1, 236–39, on p. 237.
49. Beaufort, "Memoranda for Commander Fitzroy's orders." Reprinted in Browne and Neve, *Voyage of the Beagle*, 386.
50. Entry for 27-28 March 1832. Keynes, *Diary*, 48.
51. These entries were made on 26-28 March 1832. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 31–32.

Before the sounding line was cast, the concave bottom of the sinker, or lead, (pronounced "led") was "armed" with soft tallow that would either record an impression of a hard bottom or capture a sample of any loose material on the seafloor. The arming allowed chart makers to include sufficient detail about the type of bottom that navigators could place their location, if necessary, by sounding in the same waters.[52] It also presented a wealth of information for the opportunistic naturalist. In his notes for 28 March 1832, Darwin recorded that "10 miles West of Abrolhos; there came up with the lead (17 Fathoms) a piece of Fucus.-- on which were growing numerous minute tufts of a Conferva." Under his microscope, the harvest of this one sounding was considerable. The conferva had "stems simple cylindrical white transparent jointed; end truncate; length 1/10 of inch, diameter 2/3000." Looking closer, he saw that "On this minute plant & on a small coralline were crowded together a forest of numerous species of Bacillareès & Anthrodieès"[53] It revealed to him that any cast of the lead might produce a ready made specimen. What is more, it would be presented complete with knowledge of the depth of the water it inhabited and the precise geographical coordinates of the spot where it was found. These details of an organism's location and vertical distance from sea level were just the sort that Humboldt had championed in his study of terrestrial biota.[54] Darwin had learned that laws about the habitats and interrelationships of organisms were to be derived from just such practices of exact collecting. Lying off shore from the continent that had made Humboldt famous, this

52. Descriptions of the bottom were important parts of navigators' accounts of their explorations. Bougainville, for example, wrote in the narrative of his circumnavigation, "I shall add, for the use of those who may be plying here in thick weather, that a gravelly bottom shews that they are nearer the coast of Terra del Fuego than to the continent; where they will find a fine sand, and sometimes oozy bottom." Bougainville, *A Voyage Round the World*, 132.

53. This was recorded as specimen "392 not [in] spirits." Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 33.

54. Michael Dettelbach, "Humboldtian Science," in *Cultures of Natural History*, eds Nicholas Jardine, James A. Secord, and E. C. Spary (Cambridge: Cambridge University Press, 1996), 286–304; Janet Browne, *The Secular Ark: Studies in the History of Biogeography* (New Haven: Yale University Press, 1983).

one cast of the line offered Darwin everything a philosophical naturalist could desire. At this moment, ninety days into the voyage, the sounding lead joined the dredge and the microscope among the tools with which Darwin's zoological knowledge was built.

Marine invertebrates became an increasing fixation for Darwin. During a long shore leave at Rio de Janeiro, he was "busily employed with various animals; chiefly however corallines," in contrast to which he considered the local "Geology [to be] uninteresting, [and the] Botany and Ornithology too well known."[55] But the research continued apace because he was able develop unexpectedly rich collections even when he could not leave the ship. In late 1832 he begged Henslow to "[r]ecollect how great a proportion of time is spent at sea" as he regaled him with descriptions of the "new & curious genera" of pelagic animals caught in the trawl and the "interesting" zoophytes hauled up by the lead. "As for one Flustra," he raved, "if I had not the specimen to back me up, nobody would believe in its most anomalous structure." He was proud to say, of his time on the *Beagle*, "It has been a splendid cruize for me in Nat[ural] History."[56] He remained on or near the ship as FitzRoy surveyed Tierra del Fuego and the Falkland Islands in early 1833, "during [which] time," he noted in his diary, "I have been very busy with the Zoology of the Sea." Thanks in large part to his deck-top collecting methods, he was struck with the opinion that "the treasures of the deep to a naturalist are indeed inexhaustible."[57]

Among these marine treasures, Darwin puzzled to understand a variety of organisms that he counted under the heading of zoophytes. In applying this general name , he followed Grant's use of a term applied by Cuvier and derived from the name "Zoophyta," by which Linnaeus designated the taxon between the animals and plants.[58]

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55. On Darwin's study of corallines at Botafogo Bay, see the *Diary* entries for 8, 15, 20-22, 27 June 1832.
56. Darwin to Henslow, 26 October-24 November 1832.
57. Entry for 4 April 1833. Keynes, *Diary*, 149.
58. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, xiii.

Among the creatures Darwin considered zoophytes were a number of colonial invertebrates, including those he called corals (sometimes "coralls") and the smaller "corallines." At various times in the voyage he referred to reef building corals as "lamelliform" (plate-like) or "Corall forming" corals, or "lithophytes."[59] "Corallina," on the other hand, was his name for a group of encrusting organisms that he came to believe, in early 1834, did not have any "connection with the family of Zoophites" and were probably better placed in "the grand division of plants."[60] Nevertheless, he collected these algae, and studied their physiology and means of propagation, in the same ways that he investigated the colonial invertebrates. That his invertebrate work built on his training from Grant is explicit in his discussion of zoological specimen 983 (in spirits of wine), a moss-like encrusting zoophyte collected in mid-1834.[61] Among the notes of his dissection, which included five illustrations, he wrote, "I examined the Polypus of this very simple Flustra, so that I might errect at some future day, my imperfect notions concerning the organization of the whole family of Dr Grants paper."[62] This refers to the paper in which Grant preempted by three days Darwin's first Plinian Society publication in 1827, suggesting that the young naturalist recognized his *Beagle* studies as being intellectually continuous with those he began in Edinburgh. In re-engaging with his erstwhile mentor's analyses, he considered himself capable of far more than simple description of specimens.

Over several weeks in the summer of 1834, Darwin made explicit his ambitious plan for the study of zoophytes. Informative as they were to many of the questions that interested him best, he reported to his family that, "Amongst Animals, on principle I

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59. See, e.g., Down House Notebook 1.18, "Santiago Book" (CUL MS Microfilm 532), p. 6; DAR 37.2:791 verso; DAR 38.2:893 verso; David R. Stoddart, "Coral Islands by Charles Darwin," 9.
60. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, xiv, 187–88.
61. Keynes, *Diary*, 232–33.
62. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 232–33, and discussion on p. xv.

have lately determined to work chiefly amongst the Zoophites or Coralls." He explained, "it is an enormous branch of the organized world; very little known or arranged & abounding with most curious, yet simple, forms of structures."[63] His manuscripts of the period, flush as they were with new discoveries, explain much of his optimism about this work among the colonial marine invertebrates. But this letter also hints that his enthusiasm was equally based on professional opportunism, fed by his perception that this taxon had yet to be mastered. It was surely telling that the two most famous naturalists of the day, Cuvier and Lamarck, disagreed over the propriety of the term zoophyte and the unity of the group. Lamouroux had addressed the uncertainty of the field as stimulus for his own contribution to it: "What we know [of the natural history of Polypes] pales in comparison to what we don't know: it is therefore to draw the attention of men instructed in these new objects that I have published this work, in which I have sought to gather all that has been said on the Polypiers by previous authors, and to expand the domain of science through some new observations."[64] A passage in the second volume of Lyell's *Principles* (which contained the chapter on coral reefs, and which Darwin had received by mail from Henslow in November 1832) must have seemed even more inspirational. "The ocean teems with life--the class of *polyps* alone are conjectured by Lamarck to be as strong in individuals as insects. Every tropical reef is described as bristling with corals, budding with sponges, and swarming with crustacea, echini, and testacea; while almost every tide-washed rock is carpeted with fuci and studded with corallines, actiniae, and mollusca. There are innumerable forms in the seas of the warmer zones, which have scarcely begun to attract the attention of the naturalist[.]"[67] It is not difficult to imagine Darwin believing that this passage,

63. Darwin to Catherine Darwin, 9-20 July 1834. Burkhardt et al., *CCD*, v. 1, 391–94.
65. Jean Vincent Félix Lamouroux, *Histoire Des Polypiers Coralligènes Flexibles, Vulgairement Nommés Zoophytes* (Caen: F. Poisson, 1816), lxxxiv.
67. Charles Lyell, *Principles of Geology* (1st Ed), v. 2, 182.

written shortly before the *Beagle* sailed, had been intended for him personally.

In Valparaiso, Chile, the day after the *Beagle* made her first landfall on the west coast of mainland South America (and just after announcing his concentration on zoophytes to his family), Darwin boldly laid out the details in a long letter to Henslow.[68] He believed he had evidence for major taxonomic revisions. He explained that he had examined two species of the genus *Sertularia* "taken in its most restricted form as by Lamouroux," and found that "the Polypi quite & essentially differed, in all their most important & evident parts of structure." With this compelling discovery, Darwin had "already seen enough to be convinced that the present families of Corallines, as arranged by Lamarck, Cuvier &c are highly artificial. -- It appears they are in the same state which shells were when Linnaeus left them for Cuvier to rearrange." His audacious disagreement with the highest authorities went beyond morphology. During one of his dissections he had managed to stimulate a collective reaction by multiple polyps of a "little stony Cellaria." He took this as a remarkable indication of coordination from one polyp to the next. "This fact, as far as I see, is quite isolated in the history...of Zoophites. -- it points out a much more intimate relation between the polypi, than Lamarck is willing to allow." There can be little doubt that Darwin already intended to continue this line of study on the reef-building corals of the Pacific when FitzRoy finished the South American survey and set sail for the west to complete their circumnavigation.

Lyell had indicated that the tropics virtually begged to be studied by such a zoophyte expert as Darwin aspired to be, and specifically noted that few sites offered more promise than the "reef[s] bristling with corals." As Darwin devoted himself to studying the marine zoology of the Atlantic, there can be no doubt that coral reefs were

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68. Darwin to Henslow, 24 July-7 November 1834. Burkhardt et al., *CCD*, v. 1, 397–402. From internal context, this part of the letter appears to have been written on, or shortly after, 24 July.

indeed never far from his thoughts. Writing to Henslow while “sea-sick & miserable” on a bleak run from the Falkland islands to the mainland in April 1833, he affirmed “I trust that the Corall reefs & various animals of the Pacific may keep up my resolution.”[69] Three months later, in the dead of the southern winter and facing at least one more season of surveying before they might cross to the “glorious Pacific,” he reiterated the spell that coral reefs cast on his imagination. “I am ready to bound for joy at the thoughts of leaving this stupid, unpicturesque side of America. When Tierra del F[uego] is over, it will all be holidays. And then the very thoughts of the fine Coralls, the warm glowing weather, the blue sky of the Tropics is enough to make one wild with delight.”[70]

The coral reefs of the Pacific enlivened more than Darwin’s idle yearnings, however, for he envisioned them to be the culmination of his voyage-long program of marine invertebrate zoology. As he explained to Henslow, from whom he had just received the *Report* of the second meeting of the British Association of the Advancement of Science (BAAS), “for my second *section* Zoology.-- I have chiefly been employed in preparing myself for the South sea, by examining the Polypi of the smaller Corallines in these latitudes.”[71] What could this mean? To say that he was “preparing himself” implied that he had some responsibility to uphold in the tropical Pacific. The most obvious task was what Sandra Herbert has called Darwin’s “only direct assignment as a geological author,” namely, Beaufort’s instruction to study coral reef formation. As his earlier letters to Henslow indicate so strongly, coral reefs were the main feature Darwin associated with the South Seas. However, this letter makes clear that Darwin did not view their study as a “geological” assignment. Rather, he was

69. Darwin to Henslow, 11 April 1833. Burkhardt et al., *CCD*, vol. 1, 306–8.
70. Darwin to Henslow, 18 July 1833. Burkhardt et al., *CCD*, vol. 1, 321–23.
71. Darwin to Henslow, March 1834. Burkhardt et al., *CCD*, 368–71. The BAAS was divided into “sections” relating to different areas of science, hence Darwin’s use and emphasis of the word to describe the work that he considered fundamentally zoological.

especially pointed in classifying this work as “zoology,” a distinction that he happened to emphasize on this occasion because he was copying the BAAS division of the sciences into different “sections.”[72] Given that it would be FitzRoy, not Darwin, whose soundings were expected to determine whether the Pacific was dotted with submarine volcanoes, it makes sense that in March 1834, Darwin saw his future contribution to the question of atoll formation lying in zoology. The crater rim theory was predicated on Quoy and Gaimard’s novel proposition about the limits on coral growth, and at appears that Darwin’s point of view at this stage was that the study of coral reefs would hinge on understanding this organic process. Hence the study of Atlantic corals would prepare him for his expected inquiry into the origin of Pacific atolls. In the retrospective light cast by Darwin’s subsidence theory, it is difficult to imagine his coral reef work as a venture modeled more on the scientific practices of Grant than Lyell. In mid-1834, though, that is how Darwin saw it.

Geology and Darwin’s views on subsidence

Darwin’s curiosity about changing levels of sea and land and his enthusiasm for Lyell, first piqued at the Cape Verde islands, were redoubled by the geology of South America. Not surprisingly, given the importance of marine remains in the study of stratigraphy, his zoological knowledge proved valuable to his interpretation of geological history.[73] Less obviously, but with important consequences, hydrography also became a central resource for Darwin’s geological thinking.

72. The “Transactions of the Sections” in the BAAS *Report* for 1832 show geography and geology grouped into one section and zoology, anatomy, and physiology in another. *Report of the First and Second Meetings of the British Association for the Advancement of Science* (London: John Murray, 1833), viii.

73. Alan C. Love, “Darwin and *Cirripedia* Prior to 1846: Exploring the Origins of the Barnacle Research,” *Journal of the History of Biology* 35 (2002): 251–89; Sandra Herbert, *Charles Darwin, Geologist*.

As FitzRoy surveyed southward, Darwin began to learn the geology of eastern South America from his frequent excursions on shore. The Pampas and Patagonia consisted of great terraces of land, level to the naked eye, that stretched over hundreds of miles between the Atlantic and the Andes. Most of these terraces, which now stood tens or hundreds of feet above sea level, were characterized by distinctive marine remains such as the eponymous shells of the "great oyster bed." of Patagonia. Clearly, they had previously been submerged. It seemed more likely to Darwin that the land had been elevated than that the sea had receded to such an extent, but he wondered how such large tracts of the earth could have been raised up without any apparent deformation. Another puzzle was posed by a vast bed of gravel with which these oyster remains were associated, and which consisted of distinctive porphyry pebbles whose "Parent rock" had perhaps been to the northwest in the Andes.[74] What agency, Darwin wondered, could have transported a thin layer of pebbles so evenly across an area that Darwin had "traced for more than 700 miles?[75] In an essay, "Reflection on Reading my Geological Notes," written around March 1834, he considered the possibility that after a "vigorous elevation" of the sea floor these pebbles had been carried "by the retreating waters" from the "West foot of the Cordilleras [Andes]" to "a deeper sea." Whatever the exact cause, Darwin felt sure that they had been distributed in a "short period" because they were not "encrusted by stony small corallines.-- (Which I always have noticed to be the case in these seas)."[76] This telling remark suggests that Darwin was reasoning from knowledge gained by examining the armings of the sounding lead.

Indeed, in the course of studying the zoophytes that emerged on the lead, Darwin had begun to remark the rocks of the seafloor, to which the organisms were often

74. Sandra Herbert, ed., "From Charles Darwin's Portfolio: An Early Essay on South American Geology and Species," *Earth Sciences History* 14, no. 1 (1995): 30 (Darwin's f. 2).
75. Darwin to Henslow, 10 March 1834. Burkhardt et al., *CCD*, v. 1.
76. Sandra Herbert, "From Charles Darwin's Portfolio," 31 (Darwin's f. 5–6). On the date of the manuscript, see pp. 25-27.

adhered. In this way, the survey not only provided Darwin with submarine geological and zoological specimens that might be of isolated interest, but also provided a general familiarity with the range of constituents and physical conditions across different parts of even the deep sea floor. Thus Darwin was able to write of the Patagonian porphyry pebbles, "Whatever their origin, they mark a great change in the inhabitants of the ocean [for] during a succession of elevations [subsequent to the elevation of the gravel bed, and each producing another, lower plain] such shells as now exist-- flourished on the successive lines of beach & were scattered over the bottom."[77] In other words, a series of elevations since the era when the great oysters lived had converted new parts of the sea floor into dry land in the geologically recent period when the present inhabitants of the waters FitzRoy was plying were already extant. Indeed, Darwin told Henslow, "the most curious fact is that the whole of the East coast of [the] South part of S. America has been elevated from the ocean, since a period during which Muscles [i.e., mussel shells] have not lost their blue color."[78] The remains of sea creatures, some identical to those yet living and captured by the armings, became his index of successive elevations. His familiarity with such creatures owed much to the soundings, and his conclusions about the environments in which these organisms must have lived--drawn either from knowledge of their actual present day habitat or by analogy from living species to similar extinct ones--informed his judgment of the physical conditions in which successive beds had been laid down. This was precisely what Lyell championed in the *Principles*, namely reasoning about the past from knowledge of processes active in the present day, and the survey made it possible.

This same period, in the first half of 1834, was a crucial time in shaping Darwin's eventual view that the elevation of South America must have been offset by

77. Sandra Herbert, "From Charles Darwin's Portfolio," 31 (Darwin's f. 6).
78. Darwin to Henslow, 10 March 1834.

subsidence elsewhere. In "Reflection on Reading my Geological Notes," Darwin posited three possible types of elevatory force: "It becomes a problem. how much the Andes owes its height. to Volcanic matter pouring out?.-- how much to horizontal strata tilted up.? how much to these horizontal elevations of the surface of continents?"[79] Two further essays, on the "Valley of S. Cruz,"[80] and "Elevations of Patagonia,"[81] both written before the end of the southern winter (i.e. mid-1834), reveal that his ideas about elevation were extraordinarily malleable. Time and again after laying out evidence from the soundings or his inland observations and coming to a careful conclusion, Darwin would trail off into a series of challenges, questions, and contradictions to himself. Gradually, by 1835, he became convinced of the importance of "horizontal elevation," which he also described as elevation "concentric" with the earth. Both terms were slightly misleading; they referred to bulging of the earth's crust on the order of thousands of square miles.[82] Unlike localized injections of molten rock beneath the crust, or tilting of strata, gradual "horizontal" elevation would result in a more equable uplifiting of beds such as he saw in Patagonia. Thus by early 1835, he conceived of the entire continent having been uplifted in this manner. Carried upward by this movement had been the Andes themselves, which pre-dated the continent and had formerly existed as a chain of islands. Darwin gained confidence that the continent continued to be uplifted by gradual degrees in February 1835, when FitzRoy documented that an earthquake had elevated the coast of Chile by several feet.

A significant implication of elevation of this sort was that it must be offset by subsidence of another part of the earth's crust. The alternative would be that the whole globe was expanding when horizontal elevation occurred, which Darwin found

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79. Sandra Herbert, "From Charles Darwin's Portfolio," 32 (Darwin's f. 8).
80. DAR 34.2:131-152, and "Valley of S. Cruz," DAR 34.2:104-110.
81. DAR 34.1:40-60. On dating the essay see Sandra Herbert, *Charles Darwin, Geologist*, 399, note 70.
82. In May 1834 he wrote, "NB When I say concentric. I mean not truly so. – but an enlargement of the curve of the world." DAR 34.2:110 Verso.

untenable in itself, and which would have diverged wildly from the conventional wisdom of continental geologists who imagined that the inequalities of the earth's crust were wrinkles caused by the ongoing *shrinking* of the globe (caused by secular cooling).[83] The obvious location for this necessary sinking of the crust was the bed of the great Pacific ocean. As he had ranged into the Andes from the west coast in August 1834, Darwin had seen "immense flat valleys" that reminded him of the pebble beds of Patagonia. It seemed obvious that these level-bottomed valleys had, like the Patagonian terraces, also previously been on the sea floor when the mountains that towered above them had been islands. Thus, "The basin of Aconcagua [had] most clearly [been] marine with Islands."[84]

On 18 August 1834, in what I believe to be Darwin's first inkling that the bed of the Pacific must have been subsiding, he wrote in his tiny field notebook, "With respect to [the] great valleys," which he saw as uplifted sea beds, "Perhaps in Pacific if seen, wonder would be reversed."[85] The words "if seen" could be taken as irony, because there was no obvious way to look at the present day Pacific floor and see if it had sunk while these level valleys had been elevated. There is a chance, though, that Darwin had a notion that he might one day "see" the bed of the Pacific via hydrography, a practice that had already proved so useful off the Atlantic coast. What is beyond doubt is that by 29 May 1835 Darwin had decided to look for evidence that the bottom of the Pacific had subsided. On that day he wrote a letter to a geological acquaintance in Valparaiso. Darwin's letter is not extant, but Robert Alison's response referred to the *Beagle*'s impending departure from South America by saying "I wish much to hear of your report respecting the islands in the Pacific, and it will be curious if you find a sinking of the

83. See, e.g., Mott T. Greene, *Geology in the Nineteenth Century: Changing Views of a Changing World* (Ithaca: Cornell University Press, 1982), chapter 2.
84. Down House Notebook 1.15. CUL MS Microfilm 532.
85. Down House Notebook 1.15. CUL MS Microfilm 532.

land there, & a rising here."[86] Most likely, Darwin was not optimistic that he could prove the action of a process whose effects would be hidden beneath the waves. One week after his letter to Alison he wrote to his sister Catherine, saying "I have lately been reading about the South Sea -- I begin to suspect, there will not be much to see."[87]

While he remained for his final months in South America, however, where uplift had exposed former sea beds to view, Darwin was still contemplating this problem. Having previously been confounded by the consistent distribution of pebbles on the former sea bed of Patagonia, he had realized that it could be explained by a gradual, not a violent, horizontal elevation. He knew from his observations of sounding data, which he had begun assembling as "Observations on the Bottom of the sea betwen the Falkland Islands & S[anta] Cruz" in April 1834, that the action of the currents at the sea bed was minimal.[88] This was demonstrated by the fact that pebbles, like those in the porphyry gravel, came up in the arming still covered by living corallines with their delicate parts intact. Perhaps in part for this reason, he knew that under actual (i.e., present day) conditions, such matter was never found on the sea floor at points very distant from the coast from which it was eroded. Thus it seemed to him that porphyry pebbles could only be distributed over a long distance perpendicular to the coast during an ongoing period of elevation, when the sea bed would, in effect, migrate across the coastline on its way to becoming dry land. The longer this process occurred, the more thinly and widely would a bed of erosional materials be redistributed onto the upraised land.

Darwin moved from this insight about the probable history of an exposed plain covered with gravel to musings about the probable history of beds that were not exposed, but rather were preserved as strata of sedimentary rock. It stood to reason that

86. R. Alison to Darwin, 25 June 1835. Burkhardt et al., *CCD*, v. 1, 567.
87. Darwin to Catherine Darwin, 31 May 1835. Burkhardt et al., *CCD*, v. 1.
88. DAR 34.1:87-92.

beds containing similar rocks to the porphyry bed, such as conglomerates of pebbles, must have been formed during a period of similar crustal motion. Thus, in (southern) fall or early winter, 1835, Darwin noted in his "Santiago [note]Book" that "I believe much conglomerate [in a stratum of sedimentary rock] is an index of [the sea's] bottom coming near the surface." He went on to state this more clearly in the form of a rule of stratigraphic interpretation: "May we not imagine [that] each band of conglomerates [in a succession of strata] marks an epoch when that part of the ocean's bottom was near to a continent or shoal water[?]"[89]

In the same passage of notes from the Santiago Book, Darwin also considered what composition a sedimentary rock could have that would signal to the stratigrapher that it had been deposited on a sinking, rather than a rising, foundation. It occurred to him that in this case it would be marine organisms, rather than any product of dry land, that would be characteristic of such strata. The most plentiful organisms in the deep ocean that had sufficiently hard parts to be preserved in a sedimentary rock would be the massive corals living at the surface of a coral reef. Thus he wrote, "As in [the] Pacific a Corall bed. forming as land sunk. would abound with. those genera which live near the surface (mixed with those of deep water) & which would more easily be told the Lamelliform. Corall forming. Coralls.-- I should conceive in [the] Pacific. wear & tear of Reefs must form strata of mixed. broken. sorts & perfect deep-water shells (& Milleporae)." This passage refers not to the constructional part of a coral reef, but to the composition of sediment that would be laid down on the seafloor in proximity to a living reef. Thus, the deep water shells that lived closer to the seafloor would be "perfect," while the "genera which live near the surface" would only be deposited when they had been removed by "wear & tear," and drifted to the sea floor in a "mixed. broken" condition. Unlike strata containing conglomerate, Darwin was not certain that

89. Down House notebook 1.18 "Santiago Book." CUL MS Microfilm 532. Darwin's pp. 7, 12.

he had actually seen a layer of rock that answered to this description. He pondered whether "such appearance correspond[s] to any of the great Calcareous [i.e., limestone] formations of Europe." Though he was not certain of the answer, he knew that if such a bed consisted of organic remains of which a "*large* proportion [was] those Coralls which only live near [the] surface," then "we may suppose [that] the land [was] sinking" when it was deposited.[90]

After making some reading notes that may indicate the passage of hours or weeks while this train of thought was interrupted, Darwin resumed writing on the topic. At this stage he stopped thinking only of sedimentary rocks that might contain the remains of reef building corals, and began to imagine the possibility of strata actually "containing Corall reefs."[91] He realized that these might offer irrefutable evidence of the direction of the land's movement: "The test of depression <<in strata>> is where [a] great thickness has. shallow. coralls growing in situ: this could only happen when bottom of ocean was subsiding."[92] The sinking ocean floor would allow corals to grow one atop another to a thickness roughly equal to the amount of subsidence.

It is important to recognize that Darwin made these observations in answer to his questions about stratigraphy. His notes show clearly that this "test of depression" was meant to guide the interpretation of rocks on land, and there is no evidence that he saw any way to apply it to his observations at the islands of the Pacific, where depressed strata would be hidden underwater. The test of depression certainly built upon Darwin's expectation that widespread gradual subsidence was a genuine phenomenon of the present and the past, which itself was based upon his belief in horizontal elevation. Moreover, these notes prove that Darwin was aware before mid-1835 of Quoy and Gaimard's claimed depth limit for the growth of reef building corals, his assent to their

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90. Down House notebook 1.18, "Santiago Book." CUL MS Microfilm 532. Darwin's pp. 6-8.
91. Down House notebook 1.18, "Santiago Book." CUL MS Microfilm 532. Darwin's p. 14.
92. Down House notebook 1.18, "Santiago Book." CUL MS Microfilm 532. Darwin's p. 15.

rule perhaps being encouraged by his by-now wide experience studying zoophytes belonging to various depths.

These notes in the Santiago Book do not, however, prove that Darwin's coral theory was thought out on the west coast of South America, notwithstanding the several historians who have cited these passages, and the letter from Alison, in support of this claim from Darwin's *Autobiography*.[93] We may identify the move made here, from discussing the accumulation of sedimentary beds that contain broken corals to discussing the preservation of intact coral reefs, as the process he described in his autobiography as "replac[ing] in imagination the continued deposition of sediment by the upward growth of coral." But these notes, which so forcibly corroborate that part of Darwin's reminiscence, do not support the following claim that simply "To do this was to form my theory of the formation of barrier-reefs and coral atolls." Indeed, the notes imply that Darwin was, at this time, entirely incapable of imagining that he would apply the notion of upward growth of corals on a sinking foundation to his study of a living reef, because a few pages later we find him still casting about for any subsidence-based prediction that he might actually be able to study in the Pacific: "If the Pacifick Isl[ands] have subsided there ought to be a peculiar vegetation."[94] These ideas did contribute to his explanation of atolls, but not until he was actually in the Pacific looking at a living reef.

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93. E.g., Frederick Burkhardt, "Appendix V. Darwin's Early Notes on Coral Reef Formation," in *The Correspondence of Charles Darwin*, eds Burkhardt and et al (Cambridge: Cambridge University Press, 1985), 567; Frank J. Sulloway, "Further Remarks on Darwin's Spelling Habits and Dating the *Beagle* Voyage Manuscripts," *Journal of the History of Biology* 16, no. 3 (1983): 369; Sandra Herbert, *Charles Darwin, Geologist*, 169.
94. Down House notebook 1.18, "Santiago Book." CUL MS Microfilm 532. Darwin's p. 21.

To the Pacific and a new theory of coral reef formation

After five weeks at the Galapagos Islands in September and October 1835, FitzRoy brought the *Beagle* across the Pacific to Tahiti, a precipitous mountain island encircled by a coral reef that lay, in places, more than a mile offshore. To English minds, this was the most tantalizing and historic of the South Sea islands, where the botanist Joseph Banks had deflowered himself while Cook observed the transit of Venus in 1769. This was where Darwin would have his first chance to look for evidence of the subsidence that he felt sure had depressed the Pacific ocean floor.

The previous week, FitzRoy had threaded the *Beagle* through the atolls of the "Low or Dangerous" Archipelago [now the Tuamotus]. Presented with his first opportunity to see the coral islands of the Pacific, Darwin had climbed the mast in search of a bird's eye view. That vantage point had shown him Noon Island, a "trifling" ring of coral reef encircling a "great lake of water...about 10 miles wide."[95] Now at Tahiti after nearly a month as a speck on the seemingly limitless ocean he again sought perspective on his surroundings, this time by hiking up the nearest ridge. Ascending to a height of several thousand feet he realized that he had climbed through a series of discrete zones of vegetation. The climb up from sea level gave him the uncanny feeling that he was crossing great portions of the globe, moving from the equator back toward his home. Halfway up, after coarse grass had succeeded the dwarf ferns below, Tahiti began to look strangely familiar. "The appearance was not very dissimilar from that of some of the hills in North Wales; and this so close above the orchard of Tropical plants on the coast was very suprising."[96] He pressed onward, until "trees again appeared...tree ferns having replaced the Cocoa Nut."[97] This was a remarkable experience, but one that

95. Entry for 13 November 1835. Keynes, *Diary*, 365.
96. Entry for 17 November 1835. Keynes, *Diary*, 368.
97. Entry for 17 November 1835. Keynes, *Diary*, 368.

he might have expected on such a steep climb, for the first pages of Darwin's favorite book asserted that "each group of plants is placed at the height that nature has assigned." "These regions," Humboldt argued in the *Personal Narrative*, "form the natural divisions of the vegetable empire; and in the same manner as the perpetual snows are found in every climate at a determinate height, [plants] have also their fixed limits."[98] Humboldt's plant geography, as with his study of snowlines, famously illustrated that climbing in elevation was equivalent to climbing in latitude.

Darwin had doubtless seen Humboldt's iconic depictions of the succession of flora on the flanks of Chimborazo, then believed to be the highest mountain in the world, and knew of Humboldt's claims to have determined "according to barometrical measurement, in more than 4000 plants of the equinoctial region, the height of each station above the level of the sea."[99] However, he had never seen such a vibrant illustration of Humboldt's point as he did on the slope of Tahiti, where a succession of characteristic flora ringed the mountain in a series of living contour lines.

"Two or three thousand feet" above Matavai Bay, Darwin turned west toward the island of Eimeo [Moorea]. Lying 15 miles distant, it was a shrunken version of the jagged mountain he had just climbed. "The island is completely encircled by a reef...at this distance a narrow but well defined line of briliant white where the waves first encountered the wall of coral, was alone visible; Within this line was included the smooth glassy water of the lagoon, out of which the mountains rose abruptly."[100] He likened the island to an engraving, framed and matted by the reef and lagoon.

With this view before him, he began to ponder the subsidence of the ocean floor. As he had done elsewhere during the voyage, he envisioned how the scene before him

98. Alexander von Humboldt, *Personal Narrative of Travels to the Equinoctial Regions of the New Continent During the Years 1799–1804, Vol. 1,* trans. Helen Maria Williams (London, 1818), xxvi.
99. Alexander von Humboldt, "[Essay on the Geography of Plants (Translation)]," *Edinburgh Philosophical Journal* VI (0279 1822): 279.
100. Entry for 17 November 1835. Keynes, *Diary,* 369.

would be transformed if the past changes he conjectured continued to act on the present day landscape. In notes written shortly thereafter, he recorded his thoughts of this moment: "Viewing the Ei Meo from the heights of Tahiti I forcibly struck with this opinion...Remove the central group of mountains, & there remains a Lagoon I[sland]."[101] If Eimeo were drawn downward below the sea, he would be left with the same view as from the masthead at Noon Island, of an atoll enclosing nothing but an unbroken lagoon.

Perhaps this realization was aided by seeing the severity of Eimeo's slope. If such an inclination extended below the sea, a reef circling a mile beyond the shore was already standing in water deeper than the lower limit of coral growth.[102] Perhaps he immediately recalled his views on stratigraphy, which had helped him to imagine that a bed of limestone could contain a "great thickness" of corals "in situ," if they had accumulated while the "bottom of the ocean was subsiding." Whatever the case, his reasoning that the reef could remain near the surface while Eimeo sank out of sight depended on corals continuing to grow in the zone of water just below sea level. It is worth recalling that Darwin pondered the vertical distribution of plants the entire time he hiked up to this vantage point, because by this viewpoint, rings of coral were no different from the bands of flora that encircled Tahiti above the sea. Corals grew like a turf of vegetation wherever there was a suitable foundation within their vertical station. Just as plants must migrate up the mountainside if Eimeo sank, corals would grow upward and remain in their own zone. Like the snow line that marked a boundary in Humboldt's mountainside diagrams, the waterline was a fixed limit for corals. Darwin had realized that the same principles that constrained the geography of plants on the

101. David R. Stoddart, "Coral Islands by Charles Darwin," 7 (Darwin's p. 5).
102. This was an idea he expressed in later notes, as in notes from Mauritius in early May 1836. DAR 38.2:900-901.

Andes were applicable to the vegetative growth of zoophytes on the flanks of submarine mountains.

After several days of exploring the heights of Tahiti, Darwin made his first close study of a coral reef in an outrigger canoe paddled by hired men.[103] Farthest from shore was a "mound of Coral rock, strikingly resembling an artificial (but low) breakwater," fronting the open ocean.[104] Inside the line of whitecaps marking the reef's highest point was a broad tract of uneven coral. "In this low part there are little narrow twisting channels & holes of deep water, & on the other hand many points. where the Coral reaches to the surface."[105] This inner reef reached anywhere from 100 yards to a mile shoreward from the breakers. It was covered by the calm lagoon waters, which deepened toward shore as the reef disappeared, leaving "harbors where a ship can anchor in a fine Sandy bottom."[106] He suspected that the freshwater and sediment that ran off Tahiti into the lagoon presented obstacles to coral growth near shore.

Darwin was especially attentive to what Humboldt would have called the "geography of corals," examining whether the different parts of the reef were inabited by different types of reef builders. The main constituents of the inner reef were "stony & branching generas" and "Fungia & Caryophillia."[107] The calm water of the lagoon seemed a haven for "admiring the pretty branching Corals."[108] He collected a fungia and kept it alive long enough to study the "considerable powers of contracting & motion" of the polyps under his microscope.[109] He hoped to compare these specimens with corals living on the other side of the reef, in the water of the open ocean, but his men were unable to take him there "owing to the surf...breaking violently on the outer

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103. Entry for 22 November 1835. Keynes, *Diary*, 378.
104. David R. Stoddart, "Coral Islands by Charles Darwin," 10 (Darwin's p. 11).
105. David R. Stoddart, "Coral Islands by Charles Darwin," 11 (Darwin's p. 13).
106. David R. Stoddart, "Coral Islands by Charles Darwin," 10 (Darwin's pp. 11–12).
107. David R. Stoddart, "Coral Islands by Charles Darwin," 11 (Darwin's p. 13).
108. Entry for 22 November 1835. Keynes, *Diary*, 378.
109. "Fungia," specimen 1334. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 301.

margin, continuously pump[ing] over in sheets the water of its waves."[110] Instead he relied on the testimony of the Tahitians themselves. "Showing [the lagoon corals] to some intelligent natives, I was assured that such kinds never grow on the outside of the reef or compose solid reefs.-- From their descriptions. I imagined the prevalent kinds, so situated are such as Porites. Millepora. & some Meandrina & Astrea. Anyhow, they considered that there is a wide distinction in the two cases."[111] All his experience studying the contents of his dredge and the armings of the sounding lead encouraged him to believe his native informants. "Analogy. from the habits of all other marine animals would lead one to suppose that the same species would not flourish in two such different localities, as the foam of furious breakers. & shallow placid lakes."[112] He was convinced that the massive reef builders could only inhabit the outer margin, meaning that they would grow upward, and not inward over the reef flat, if an island they fringed subsided beneath the lagoon.

Tahiti was a revelation to Darwin. When the details of the reef offered nothing to contradict his speculations from the mountainside, he confided to his diary that "It is my opinion, that besides the avowed ignorance concerning the tiny architects of each individual species, little is yet known, in spite of the much which has been written, of the structure & origin of the Coral Islands & reefs."[113] All his ambitions are revealed in this brief note. He had decided to study corals--the "tiny architects"--precisely because he relished the opportunity to become expert in an area of "avowed ignorance." Now, unexpectedly, he could also challenge the "much which ha[d] been written" on a glamorous theoretical problem that had animated Beaufort, Beechey, and Lyell when the voyage departed. The coral reefs of the Pacific would keep up his resolution indeed.

110. David R. Stoddart, "Coral Islands by Charles Darwin," 10 (Darwin's pp. 11–12).
111. David R. Stoddart, "Coral Islands by Charles Darwin," 11 (Darwin's p. 14).
112. David R. Stoddart, "Coral Islands by Charles Darwin," 11 (Darwin's p. 14).
113. Entry for 23 November 1835. Keynes, *Diary*, 378.

Recording the new theory

Confident that his new views had merit, Darwin spent the passage from Tahiti to New Zealand in December 1835 writing a detailed statement of the theory, the twenty-page "1835 Coral Islands" essay.[114] They were crossing the very waters where Beaufort had expected FitzRoy to carry out his coral island surveys, Beechey having already charted the islands of the eastern Pacific. Exhausted by the survey of South America, however, FitzRoy hurried on without stopping. Unable to add to his stock of first-hand knowledge, Darwin turned to the books on board for further information about reefs. Along with philosophical travel narratives like Forster's *Observations*, and the recent geological texts by Lyell and de la Beche that drew on the work of Quoy and Gaimard, the *Beagle*'s library contained all the "voyages," of which Beechey's was the most recent example. With ready access to the whole canon of reef science, Darwin felt emboldened to generalize beyond his own narrow experience at Tahiti, laying out the implications of his new theory for the geology of the globe. "Although I have personally scarcely seen anything of the Coral Islands of the Pacifick Ocean," he began, "I am tempted to make a few observations regarding them."[115]

The argument of the "Coral Islands" essay started with a taxonomy of islands synthesized from the descriptions of Forster and Beechey (discussed in chapter 1). This contained (i) high islands without coral reefs, such as the Sandwich [Hawaiian] Islands; (ii) high islands encircled by coral reefs, such as Tahiti, Eimeo and other Society Islands; (iii) low islands made of coral (i.e., atolls), such as those in the Low or Dangerous Archipelago; and (iv) islands of dead coral in the shape of an atoll but

114. The original manuscript is at DAR 41:1-12 (Darwin's pp. 1-22). DAR 41:13-22 is a fair copy probably made by Darwin's servant, Syms Covington, which contains a small number of annotations by Darwin and FitzRoy not transcribed in David R. Stoddart, "Coral Islands by Charles Darwin".

115. David R. Stoddart, "Coral Islands by Charles Darwin," 5 (Darwin's p. 1).

uplifted from the water, such as Beechey's Elizabeth Island. Darwin pointed out that these types were gathered in archipelagoes that often shared a general orientation from northwest to southeast, which suggested that a single *vera causa* had acted in the original formation of both high and low islands.

He went on to declare that the conventional taxonomy was faulty, being based on an "artificial" distinction between the rings of coral that encircled islands and those that did not. Extending this point, he argued that if you removed the central landmass from an encircling reef (as he had imagined at Eimeo), you would be left with an actual atoll, identical in "structure & origin" to those now existing.[116] Evincing a sequence of intermediate forms from Beechey's *Narrative* and Krusenstern's *Atlas*, including "those extraordinary barriers of Coral, which front for so many leagues the coast of Australia," Darwin demonstrated that no "essential character" remained to distinguish the reefs of class (ii) from those of class (iii). He finished this line of argument by showing that there was a plausible mechanism by which high islands could be removed from within an encircling reef, namely subsidence of the ocean floor. "If the proofs of the identity in nature of the two kinds of reefs, are considered as conclusive," he resolved, "there is no necessity that the Lagoon I[slands] should be based on [submarine] Craters."[117]

In the second part of the essay Darwin shifted his focus from the morphology of reefs to the corals that constructed them. He described everything he knew about the factors limiting the growth of different kinds of stony corals and explained their distribution on the circular reef at Tahiti. He believed that Quoy and Gaimard were mistaken to imply that the same kinds of corals lived inside and outside the reef. He then carried out a kind of thought experiment, imagining "an Island situated in a part of the ocean. which we will suppose at last becomes favourable to the growth of Corall,"

116. David R. Stoddart, "Coral Islands by Charles Darwin," 6 (Darwin's p. 4).
117. David R. Stoddart, "Coral Islands by Charles Darwin," 8 (Darwin's p. 8).

so that “Corall would immediately commence to grow on the shore & would commence Sea-ward as far as the depth of water. would permit its rising from the bottom.”[118] He explained that an island fringed by a reef of this kind would “essentially differ from those in the South Sea, in the depth of the water...beyond the Wall not suddenly becoming excessive.”[119] If there were no subterranean movement, corals could never grow into a reef like that of Tahiti, where soundings beyond the breakwater showed a precipitous change of depth. However, he pointed out, coral growth combined with subsidence of the sea floor would produce structures resembling, in turn, the sequence of real islands he had already described as moving from class (ii) to class (iii) in the taxonomy. If, on the other hand, coral growth were interrupted by subterranean elevation (or the equivalent, a drop in the level of the sea), the result would be a fringe of dead coral rock like that of the rare class (iv). He recalled, in agreement, that Lyell believed the small proportion of upraised coral in the Pacific implied that subsidence had outweighed elevation in the recent history of the ocean floor. Lyell had not related this observation to the shape of the reefs, however. If Darwin was correct about they way corals grew in different conditions, then he could be confident in his new conclusion, “that the direction of the movement determines the structure of the reef.”[120]

Here, then, was the answer to Beaufort’s question. The size of an atoll did not depend on the extent of a submarine crater, but on the former shoreline of a sunken island. The same explanation was “referrible to those reefs which front a continent.”[121] The geography of corals was key. Their distribution across the globe determined which islands or continents might form the foundation of a reef. Their local distribution in horizontal and vertical space was the basis for a given reef’s outline. Thus, the shape

118. David R. Stoddart, “Coral Islands by Charles Darwin,” 14 (Darwin’s p. 16).
119. David R. Stoddart, “Coral Islands by Charles Darwin,” 14 (Darwin’s p. 17).
120. David R. Stoddart, “Coral Islands by Charles Darwin,” 16 (Darwin’s p. 20).
121. David R. Stoddart, “Coral Islands by Charles Darwin,” 15 (Darwin’s p. 19).

and breadth of the reef would be determined by the direction and inclination of this foundation where it passed through the several-fathom vertical zone in which reef building corals could live. No mere enthusiast of Humboldt's romantic language, Darwin had absorbed the concepts of a three-dimensional plant geography and set them into motion through a conjectured geological past. This insight at Tahiti had removed "much of the difficulty in understanding Coral formation" that he believed had confounded Forster, Quoy, Gaimard, and Lyell.[122]

When he had tried to determine the geological history of South America from the composition and thickness of various layers of sedimentary rock in the Andes, Darwin had supposed that "the Test of depression <<in strata>> is where great thickness has. shallow. coralls growing in situ." Now he could perform the Lyellian coup de grace, for he had evidence--in the horizontal shape of the world's living coral reefs--of an ongoing process that must be producing just such massive thicknesses of coral rock. To his original insight on interpreting past strata, he had added the requisite analogy to actual causes. In the "Coral Islands" essay he modified his words from the Santiago Book to emphasize how the present could serve as key to the past: "When in any formation there should be found, a great thickness composed of Coral & the genera of which resembled those, which now build the reefs, we might also conclude. that during its successive accumulation, the general movement, was one of depression."[123]

In the final pages of the "Coral Islands" essay, Darwin meditated further on the connection between his new theory and the geological questions that had exercised his mind since his discovery of recent shells on the high plains of Patagonia. He had only hypothesized Pacific subsidence because of his conclusions about the nature of elevation in South America. Now the resulting prediction, that reef fringed islands

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122. David R. Stoddart, "Coral Islands by Charles Darwin," 13 (Darwin's p. 15).
123. David R. Stoddart, "Coral Islands by Charles Darwin," 17 (Darwin's p. 22a).

would turn into encircling reefs and atolls, was confirmed by nature. At least, it was confirmed by FitzRoy's charts of the Pacific, which were inscriptions of nature laid down by hydrographers on journeys just like the *Beagle*'s. At this point he reversed his line of thought, taking the existence of encircling reefs and atolls as evidence for subsidence in the Pacific.

If he understood the propagation of corals correctly, and if submarine movement truly did determine the structure of reefs, then the lessons that could be read from the shape of reefs around the globe would be most "important to Geology." "For then we might assume that groups of Lagoon Is[lands] clearly showed that a chain of Mountains had there subsided."[124] In South America he had seen sea beds elevated into terraces and mused, "Perhaps in Pacific if seen, wonder would be reversed." Now he had a glorious confirmation. "The general horizontal uplifting which I have proved has & is now raising upwards the greater part of S. America...would of necessity be compensated by an equal subsidence in some other part of the world.-- Does not the great extent of the Northern & Southern Pacifick include this corresponding Area?" It went without saying that Lyell would approve of this conclusion. Instead, Darwin quoted the great Humboldt, who "carrys a similar idea still further," and "considers that 'the epoch of the sinking down of Western Asia coincides with the elevation...of all the ancient systems of Mountains, directed from East to West.'"[125]

Darwin's existing ideas about Pacific subsidence and tests for depression in strata glimmered with new meaning and importance in the light cast by his views on the

124. David R. Stoddart, "Coral Islands by Charles Darwin," 17 (Darwin's pp. 22–22a) Though he did not draw the link explicitly, this conclusion was supported by the observation with which he opened the essay, that archipelagoes of high and low islands alike seemed to share some connection to an original cause that produced mountains in a northwest-southeast direction.

125. Humboldt thought the orientation of the main axis of mountain chains was an indication of their age (i.e., mountain chains whose axes were the same direction had been uplifted in the same period). Darwin's French was poor, and may have been guided to this passage in Humboldt's *Fragmens Asiatiques* by a brief summary of the work in Conybeare's address on the state of geology in the BAAS report.

shape of coral islands. Now that he had seen the significance of those notions formed in South America for understanding the morphology of reefs, and the implications of his coral studies for geology, he would never fail to see them from this new perspective. It was true that those ideas were "thought out on the west coast of S. America before [he] had seen a true coral reef," but it was only the insight at Tahiti that made them part of a theory of coral reef formation. Only then were they incorporated into an explanation of the shape and origin of atolls.

Though he did not do it in the "Coral Islands" essay, Darwin felt compelled to give some consideration to evidence that he had seen earlier that year, and which seemed to give credence to the possibility of submarine craters underlying atolls. The crater rim theory not only postulated an enormous number of submarine volcanoes in the Pacific, but it required that their craters were of a surprisingly uniform elevation, not quite reaching sea level, but lying in that narrow vertical zone where madrepores could grow. It was little more than a month since Darwin had seen the "vast & almost infinite number of Craters" that gave the Galapagos Islands their "singular & highly characteristic aspect"[126] He cast his mind back to them now on the long passage to New Zealand, adding several new pages to his existing run of geological notes on the Galapagos. Three of the "great Volcanic mounds...surmounted by craters" had been found, by angular measurements, to have almost identical elevations, between 3720 and 3730 feet. "Inspecting the chart," he admitted, "one is tempted to exclaim; on such foundations, ready placed at an equal height, the Lithophytes, might soon raise to the surface, their circular ridges of Coral rock."[127] Lower craters made of sandstone were also noteworthy, because they shared with many atolls the characteristic of being slightly taller and steeper on their windward sides. "I am so much the more bound to

126. DAR 37.2:787 (Darwin's p. 41).
127. DAR 37.2:791v.

point out this coincidence," he noted, "as I am no believer in the theory of Lagoon I[slands]...being based on the circular ridges of submarine craters."[128] He reassured himself that because the southwest swell of the Pacific disrupted the relationship between the directions of the wind and surf in the coral zones, "the case of the Sandstone craters & that of the Lagoon I[slands] is not entirely similar."[129] Sandra Herbert, who noticed these comments on coral islands among the Galapagos notes, took them to indicate that Darwin had already formulated an alternative to the crater rim theory before reaching Tahiti. There are several kinds of evidence to support my view, however, that these notes were added only after the insight at Tahiti.[130]

Darwin was also deeply puzzled by "the entire absence of all [coral] reefs" at the Galapagos, despite their being "situated in the Pacifick and under the Equator."[131] Darwin wondered whether it was due to a "deficiency of [the] Calcareous matter" with which corals built their skeletons, but FitzRoy suggested the alternative possibility that

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128. DAR 38.2:791. See Herbert, 2005, p. 170.
129. DAR 38.2: 791, Darwin's p. 42.
130. These comments are written on the front and back of p. 42 of Darwin's Galapagos notes, which he numbered pp. 1-46 (with two pages numbered "1" and four pages "41"). The material on p. 44 could not have been written any time immediately after the Galapagos visit, because it quotes information from the *Beagle* weather journal about the temperatures two days after the departure from Tahiti. The question, therefore, is whether the writing was interrupted by the visit to Tahiti before Darwin wrote p. 42, or between pages 42 and 44. Two kinds of clues aside from the intellectual content suggest that Darwin had been to Tahiti before he wrote p. 42. The first is the page headings. Whereas the pages from 1-6 are headed "1835 October Galapagos Id," and those up to the second page 41 are headed "1835 Galapagos Isd," the page in question is headed only "Galapagos Id." The second indication is in Darwin's habitual misspellings. In the zoological notes of a dissection he performed on a Galapagos specimen (which must have been written there because the polyp was still alive), Darwin still used his characteristic South American spelling of "corall." The crater notes use the "coral" spelling that he picked up while writing the Coral Islands essay, and also used the spelling "Pacific" (as opposed to "Pacifick) that he otherwise failed to use for several months after leaving South America. In one of his original articles on the diagnostic study of Darwin's misspellings, Frank Sulloway specifically mentioned that this page of the Galapagos notes had usage inconsistent with the Galapagos period, but seems not to have considered that it was written later. However, given that the spellings and the ideas are both characteristic of the post-Tahiti period, I believe there is good reason to believe that these notes were written after Darwin was turned into an active opponent of the crater rim theory by his view of Eimeo. See Sulloway, "Further Remarks on Darwin's Spelling Habits and Dating the *Beagle* Voyage Manuscripts," 364, note "p.".
131. DAR 38.2:792.

reef builders could not withstand the cold water that surged up from great depths against the western shore of South America. Darwin at first thought that testing this "ingenious idea [would] require extended observation."[132] Instead, he got a hold of FitzRoy's weather journal and took advantage of the trove of empirical data that accumulated on a surveying ship, to conduct a retrospective experiment. He assumed that since "the whole ocean, near Tahiti abounds with Coral animals...we may presume the temperature of the Sea there [to be] perfectly favourable to their growth."[133] Thus he compared the mean and low values of 99 water temperature measurements at the Galapagos with 44 measurements taken amongst the Low and Society islands to evaluate whether those at the Galapagos could potentially be suitable for coral reefs. He found not only that the Galapagos mean and low temperatures were colder by 9.5º F and 18º F, respectively, but they were also much more variable than those of the coral seas, whose temperature never registered more than one degree below the mean. "It may easily be believed," he concluded, that a marine inhabitant of the tropics could "never flourish" in temperatures as cold and variable as those of the Galapagos.[134] Therefore, apt conditions for coral growth could not be assumed from latitude alone, no doubt confirming a trend that FitzRoy had already noticed in the course of recording the measurements. Darwin added to these conclusions with citations on ocean temperatures from three separate volumes of Humboldt's work.[135] For Darwin, as indeed for FitzRoy and Beaufort, extracting a natural law from a geographic array of precise measurements was the height of Humboldtian philosophy, and the survey was an exercise in gathering just such data.

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132. DAR 38.2:792.
133. DAR 37.2:793.
134. DAR 37.2:793 Verso.
135. DAR 37.2:793 Verso.

Testing the new theory at Keeling and Mauritius

Darwin admitted that he had “scarcely seen anything of the Coral islands in the Pacifick Ocean” when writing out his essay in December 1835. Nevertheless, he had integrated his ideas in a way that would at once answer the Admiralty’s coral reef query and could bring him into conversation about the earth’s crustal movement with his heroes Humboldt and Lyell. With the Pacific behind him after three months at New Zealand and Australia, he had still never set foot on a coral island.[136] Only in the Indian Ocean did the chance finally come. Perhaps at Darwin’s urging, FitzRoy elected to call at the Cocos (Keeling) Islands, a pair of atolls 700 miles southwest of Java and Sumatra.[137] Sailing into the lagoon through a channel in the reef, the Beagle anchored for ten days at the larger, southern island. It was “one of the low circular Coral reefs, on the greater part of which matter has accumulated & formed strips of dry land.”[138] Just a few hundred yards wide and barely standing above the level of high water, this ring of land was home to about a hundred former slaves from the Malay Archipelago and a handful of English settlers. From the anchorage, Darwin could see that the land was broken into a chain of islets, each one interrupting his view of the unbroken arc of the reef. Thus the “brilliant expanse [of the lagoon], which is several miles wide, is on all sides divided either from the dark heaving water of the ocean by a line of breakers, or from the blue vault of Heaven by the strip[s] of land crowned...by Cocoa nut trees.” With his own new theory of atoll formation in mind, Darwin turned his attention toward

136. The *Beagle* touched Australia well south of the tropics and never came close to the barrier reefs on the northeast coast.

137. Beaufort’s instructions mentioned stopping at the Keeling Islands to “settle their postition,” but only if a winter crossing forced FitzRoy to pass north of Australia through the Torres Strait. FitzRoy decided sometime early in 1836 to make the stop at Keeling despite taking the southerly route. Beaufort, “Memoranda for Commander Fitzroy’s instructions.” Browne and Neve, *Voyage of the Beagle*, 393. Armstrong and Herbert think it plausible that Darwin’s interest in seeing a coral island may have encouraged FitzRoy to deviate from the path in the itinerary. Armstrong, *Darwin’s Other Islands*, 196; Sandra Herbert, *Charles Darwin, Geologist*, 234–35.

138. Entry for 1 April 1836. Keynes, *Diary*, 413.

the points of evidence that might determine whether this structure had taken its shape from a submarine crater or from the subsidence of a reef fringed island.

If subsidence had occurred, then the reef's annular shape must have been maintained over many generations of coral growth. The "Coral Islands" essay had advanced a view of coral propagation different from Quoy and Gaimard's. Though Darwin almost instinctively believed in the basic truth of their assertion that reef building corals could only grow in shallow water, he rejected their claim that tranquil waters suited corals best. After viewing the Tahiti encircling reef, he also felt unable to accept their failure to differentiate the corals growing in the different habitations around the reef. "M. Quoy & Gaimard state, 'that the species, which constantly formed the most extensive banks, belong to the genera. Meandrina, Caryophyllia. & Astrea' & that the Saxigenous polypi increase most considerably in shallow & quiet water. I am not aware, Darwin continued, "whether they suppose, that these same species for the outer parts of the reefs."[139] On inspecting the Tahiti encircling reef and discussing its composition with the islanders Darwin had reinforced the belief, born of his earlier zoophyte studies, that the inner and outer inhabitants of the reef must be different creatures. This was critical for Darwin's theory, for he expected to find differential growth of these distinct coral populations that maintained the circular shape of the reef and kept the level of the breakwater higher than the inner flat and the lagoon floor. At present, however, he had expounded little more than a truism based on this conviction that organisms were narrowly adapted to specific environments: "Those species of Lithophytes, which build the outer. solid wall, flourish best, where the sea violently breaks."[140] Having been prevented from approaching the outer reef at Tahiti, he had "not pretend[ed] to conjecture concerning the cause of this prediliction, whether the

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139. David R. Stoddart, "Coral Islands by Charles Darwin," 11 (Darwin's p. 14) The statement of Quoy and Gaimard's views quoted by Darwin comes from Beche, *A Geological Manual*, 141.
140. David R. Stoddart, "Coral Islands by Charles Darwin," 13–14 (Darwin's pp. 15–16).

motion of the fluid, or the quantity of insolved air. is favourable; or whether the light and heat, which must pervade still shoal water is injurious to the growth of their Species."[141] At Keeling he sought a more detailed explanation of the constraints and inducements to coral growth at different parts of the reef that might explain the uneven heights they attained on its surface.

Darwin had phrased the most important open question about atolls thusly: "Within the lagoon all detritus accumulates, & if as according to M. Quoy and Gaimard. the Coral grows there also most rapidly; how comes it that the Lagoon is not more commonly filled up?"[142] Seeking a specific answer at South Keeling, Darwin transected the atoll many times on foot, from the shore of the lagoon, over an islet, and through the ankle deep water that covered the reef flat at low tide. Approaching the outer margin, Darwin vaulted "by the aid of a leaping pole...very far into the breakers."[143] Poised atop "great masses" of living coral, he studied the composition of the breakwater as the ocean foamed around him. The "chief masses" were of "living Astrea," solid corals "with a curvilinear outline up to 8 ft in diameter."[144] Where they had reached the level of the water, the tops were flattened and no longer growing. Instead, "the Astrea. extend[ed] laterally" in such a way that his "Specimens [would] show. a layer. additional on the sides as compared to the top."[145] Between these great knolls was "an exceedingly strong net work" of intersecting "Millepora...in thick vertical plates."[146] Branching corals flourished in the gaps, so that "the interstices [were] soon to be filled

141. David R. Stoddart, "Coral Islands by Charles Darwin," 13 (Darwin's p. [X15c]).
142. David R. Stoddart, "Coral Islands by Charles Darwin," 12 (Darwin's p. X15[b].
143. DAR 41.41.
144. DAR 41.41, Darwin's pp. 3 and 4. Though he referred to these masses as Astrea throughout his field notes, Darwin subsequently identified them as Porites. In his 1842 book he described the outer margin of Keeling as "almost entirely composed of a living Porites, which forms great irregularly rounded masses (like those of an Astraea, but larger) from four to eight feet broad, and little less in thickness." Charles Darwin, *The Structure and Distribution of Coral Reefs: Being the First Part of the Geology of the Voyage of the Beagle, Under the Command of Capt. Fitzroy, R.N. During the Years 1832 to 1836* (London: Smith Elder and Co., 1842), 6.
145. DAR 41:41, Darwin's p. 4.
146. DAR 41:41, Darwin's p. 4.

up & form solid masses."[147] This was what he had been led to expect at Tahiti. However, Darwin was "most surprised to see. the enormous quantity of matter. which the succesive paper like layers of Corallina [had] accumulated."[148] These stony, encrusting algae appeared capable of enduring "exposure for some time to the air," so that "instantly the surface of the Astrea dies. it is occupied by Corallina [to about] two feet above the level [of] the living solid Corals."[149] The coats of rock laid down by coralline algae were evidently integral to the reef's ability to resist the waves, for he found that "3 inches beneath the general level of the Corallina, the breakwater [was] excessively hard."[150] Working "by chissel [and] pixaxe" he "at last attained a fragment & strongly suspect[ed] it [was] Corallina petrified."[151] Enumerating what he took to be "The four Bulwark agents" of the reef, he listed two types of algae along with the corals Astrea and Millepora.[152]

Working through the course of the visit over multiple paths from the breakers to the lagoon, Darwin collected 24 zoological and 18 geological specimens (as recorded in the respective series of notes) that showed the horizontal composition of the reef. Thus the zoological list included "layers of a pale red encrusting Corallina; from the extreme breakers"; "Astrea [from] the midst of the outer breakers"; "Millepora [from the] Outer reefs in the most exposed places"; "Coral...common in holes on the outer reefs"; "Madrepore, in the lagoon"; "Seriatopora. common in the Lagoon"; "White branched Madrepore, exceedingly common in lagoon"; and "One of the commonest Corals in the lagoon: when alive yellow."[153] The description of the geological specimens reiterated Darwin's interest in distribution across the reef, but also showed his eagerness to

147. DAR 41:42, Darwin's p. 5.
148. DAR 41:42, Darwin's p. 5.
149. DAR 41:41-42.
150. DAR 41:47, Darwin's p. 15 verso.
151. DAR 41:47, Darwin's p. 15 verso.
152. DAR 41:44 verso. He was unable to name one of the two types of corallina, and identified it only by a small icon that he first used on DAR 41:42.
153. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 418–19.

document another continuum, from living corals to different kinds of rock. Thus he collected a fragment of “Astrea. the commonest Coral. block on the outer coast”; another of the “next most abundant kind...partially petrified”; some “Carb[onate] of Lime. probably Coral petrified”; a specimen that was unambiguously “white petrified Coral”; a “Yellowish white. vesicular stone...consisting of particles of shells & Corals. intimately united & blended together”; a similar specimen “which apparently [had] been a lamelliform Coral with the cells. completely filled up”; a breccia that was “very solid [with] fragments [of] a good many branching Coral[s]. from [the] reef”; and an “Astrea converted into <<snow>> white Calcareous rock [with a] glittering Crystall[ine] fracture.”[154]

The rocks and organisms Darwin collected were not scattered indiscriminately across the islets and the reef, but lay in an order from which he interpreted the origin and development of the sparse arcs of dry land. The reef flat was “composed of a very hard solid rock.-- which is petrified Coral & hard Calcareous sandstone.”[155] This had been formed when “channels between the living Coral. [had] gradually been filled with detritus [and]...petrified & smoothed by the action of the tides.”[156] Moving from the reef flat to an islet, one met a beach of “rounded fragments of solid Coral” underlain by a low ledge of breccia (a hard conglomerate rock of calcareous sand and pieces of coral “cemented by the action of atmosphere and tides”) that sloped “just perceptibly to seaward.”[157] On the windward side of the atoll, a “succession of beaches” was being laid down over the reef flat, “form[ing] the outer parts. of the strips of land.”[158] The islets, therefore, were expanding in a seaward direction over the reef. Meanwhile, the land toward the lagoon was made up of ever finer fragments of coral that had been

154. DAR 41:47.
155. DAR 41:42, Darwin’s p. 6.
156. DAR 41:42.
157. DAR 40:40-41.
158. DAR 41:40, Darwin’s p. 2.

thrown further inland by waves rushing over the reef flat.[159] The inner beaches of the islets were of a powdery coral sand that became beds of mud below the water level of the lagoon.[160] He agreed with a suggestion from FitzRoy that the finest sand in the lagoon was coral pulverized by strong-jawed fish as they grazed on the reef.[161]

Darwin continued to find order in the "under-water forests of the Keeling islands" that presented, to FitzRoy's eye, "more difference than between a lily of the valley and a gnarled oak."[162] Of the corals that lived in the lagoon, Darwin found that "the most abundant kinds are the branching sorts."[163] Though he noted with evident surprise that some species within the genus Astrea were to be seen there,[164] their form was recognizably different from the "bulwark species [and] the two other kinds [of Astrea] which are found outside."[165] Corallinas, so essential to the breakwater, were likewise "not abundant" in the lagoon[166] It was true that the conditions in the lagoon cultivated a richer diversity of coral genera, but they almost all grew "brittle & soft."[167] In striking contrast to the algae-encrusted Astrea that he had leaped to on the outer reef, Darwin found that when "standing on [the lagoon corals] a person breaks through them to some depth."[168] Unlike the resilient network being formed amidst the most turbulent water, the "dead [lagoon] Coral, showed no signs of adhering & forming as rocks. but rather of wasting."[169] This difference between the inner and outer reef suggested, contrary to the implication drawn from Quoy and Gaimard, that even "independent of repeated depressions," it was "difficult to imagine how [the lagoon] would ever entirely

159. DAR 41:40.
160. DAR 41:45.
161. DAR 41:47, Darwin's p. 14 verso.
162. Robert FitzRoy, *Narrative of the Surveying Voyages of His Majesty's Ships* Adventure *and* Beagle *Between the Years 1826 and 1836. Volume 2* (London: Henry Colburn, 1839), 634.
163. Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, 418–19.
164. DAR 41:40 verso.
165. DAR 41:44, Darwin's p. 10.
166. DAR 41:44, Darwin's p. 10.
167. DAR 41:44, Darwin's p. 10.
168. DAR 41:44, Darwin's p. 10.
169. DAR 41:45, Darwin's p. 11.

be silted up."[170] "Moreover," Darwin pointed out, again diverging from the Frenchmen's notion that calm, shallow water was most favorable for coral growth, if "the lagoon was nearly filled up the impurity of the water might [further] slow [the] growth of corals."[171] Without the fresh seawater supplied at the breakers, sand and mud settling on existing corals in the lagoon "must be fatal," and new corals would be unable to establish themselves on such a "slippery bottom. of sand or mud."[172] Combined with the lesser bulk of branching corals, these causes "must retard the growth of the Coral in the lagoon as compared to the outer [reef]."[173] Though Darwin found it difficult to distinguish clearly between the effects of subsidence, the wind, and the "comparative growth of corals," it was clear to him that present day processes were helping to preserve the annular shape of the reef.[174]

On the basis of his observations from sea level Darwin drew several sections showing the atoll's superficial structure. He also sketched a pair of diagrams to illustrate what he imagined to be the structure of the entire reef. At the bottom, roughly 1000 fathoms (6000 feet) deep, he hypothesized a foundation of "Greenstone?", his term for dark volcanic rocks.[175] The image demonstrates that Darwin conjectured this substrate to have sunk in a series of discrete subsidences, each followed by the growth of coral back to low water and the consolidation of a layer of breccia atop it. Corals could grow only up to the level of low water, while breccia could be formed from material deposited up to the level of high water, so the thickness of the breccia layer would depend on the difference in elevation between high and low tide. In the image

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170. DAR 41:45, Darwin's pp. 11 and 12.
171. DAR 41:45-46.
172. DAR 39.2:135 verso. Despite the obvious implication, I cannot find any statement in Darwin's Keeling notes to suggest that he thought branching forms would be more resistant to mortality from settling sediment.
173. DAR 39.2:135.
174. DAR 41:45, Darwin's p. 12 verso.
175. Sandra Herbert, *Charles Darwin, Geologist*, 115–16. See also "Glossary," Charles Lyell, *Principles of Geology* (1st Ed), v. 3.

Darwin figured this as three feet.[176] Surmounting the greenstone foundation, and fully enclosed by the ring of corals and breccia in situ, was a massive thickness consisting of the remains of branching corals, fragments, and calcareous sand, which he knew to be the main substance of the lagoon floor. Surrounding the entire constructional part of the reef on all sides was sand created by the pulverization of corals, which made a cone of detritus standing on the floor of the ocean.

Evidence that could reveal the atoll's longer history lay beyond the reach of the leaping pole and the pickaxe, in the surveyors' domain below sea level. Beaufort's instructions to FitzRoy had urged him to use "every means...that ingenuity can devise of discovering at what depth the coral formation begins, and of what materials the substratum on which it rests is composed." "The slope of its sides" was to be "carefully measured...by a series of soundings, at very short distances from each other." Beaufort (and Lyell) had envisioned these tests providing the crucial evidence between the crater-rim theory and Eschscholtz's explanation that corals had built upon deeper foundations, but they would serve equally well to evaluate Darwin's theory. If the foundation of the reef came close to the surface and had the outline of a volcano, or if subaqueous lavas were found at little depth, it would offer support for Quoy, Gaimard, and Lyell. Because there appeared to be a limit on the angle at which lava could harden into rock on the side of a volcano, a foundation inclined more steeply than the slopes of known volcanic cones would suggest that the reef had been built up by corals. Convinced that corals could not accumulate at any significant depth, Darwin seems never to have given serious consideration to any possible explanations besides the crater-rim theory and his

176. I diverge here from David Stoddart's analysis of these diagrams, in which he takes the "3 ft" to refer to an emergence of the "corals in situ" three feet above sea level. The note scribbled below (not mentioned by Stoddart) reads, "NB. If tides had been very small. when such a mass had accumulated, there would be very little Breccia." My figure also revises several small details of Stoddart's transcription of the writing on the diagrams, and offers readings of a handful of words he considered illegible. See David R. Stoddart, "Darwin and the Seeing Eye," 11–15.

own. If the surveyors were to find a steep inclination of the reef's foundation, it would stand as strong evidence that corals had grown upon a subsiding foundation in the manner he envisioned. Over the last four years Darwin increasingly found ways to learn from the *Beagle*'s hydrographic enterprise; finally here at Keeling his objectives had become almost indistinguishable from those of the officers.

Darwin's Keeling field notes reveal a naturalist collaborating intimately with the hydrographers at work. Besides recording the detailed results of 46 individual soundings and summarizing the findings of several times that many, he noted more than a dozen comments about the atoll from FitzRoy and Lieutenant B.J. Sulivan. Even where the water was relatively shallow, sounding among strong corals made for a difficult job. Just outside the breakers "Mr. Stuart [FitzRoy's mate, Peter Benson Stewart] carried away his anchor in 13 [fathoms] & [his] lead in 16. F: The Capt[ain] when sounding in 10 & 12 fathoms. frequently had the lead jammed. so as not to be without much difficulty to extricate it.-- How then rough the bottom must be."[177] FitzRoy himself declared that he "was anxious to ascertain if possible, to what depth the living coral extended, but [his] efforts were almost in vain, on account of a surf always violent, and because the outer wall is so solid that [he] could not detach pieces from it lower down than five fathoms." The captain spared no effort, however, and "Small anchors, hooks, grappling irons, and chains were all tried—and one after another broken by the swell almost as soon as we 'hove a strain' upon them with a 'purchase' in our largest boats."[178] It devolved upon Darwin to diagnose both the type and the present condition of the deeper corals by the impressions they left in the "tallow hardened with lime" that FitzRoy stuck to the bottom of his broadest lead. In his notes he gave a special mark of emphasis to one of FitzRoy's early soundings on the outer margin.

177. DAR 41:53.
178. FitzRoy, *Narrative*, 634.

From a depth of eight fathoms (48 feet), the tallow came up "Beautifully marked with Astrea," the bulwark coral that Quoy and Gaimard had claimed must live within 30 feet of the surface. Seeing that it was "probably alive" (because the tallow was "quite clean," and free of the sand that could accumulate freely on a surface of dead coral), this sounding alone demanded a small revision to current zoological knowledge.[179] Gradually Darwin drew more general conclusions about the extent of the living reef: to a depth of 12 fathoms (72 feet) he found the "armings clean [showing] Millepore [and] Astrea."[180] Beyond the zone of bulky corals he found "a fathom or two of fragments" that occasionally contained smaller bits of animated matter.[181] Below 20 fathoms (120 feet) there was only sand, with "no sign of any thing hard.-- in [the] soundings." These observations showed that the reef had a shallow "first inclination" from the breakwater to a depth of about 30 fathoms, with its breadth of 100-200 yards corresponding to a band of "discoloured water" that could be seen beyond the breakers.[182] From the 30 fathom mark, Darwin could see that the bottom "suddenly incline[d]" into deep blue water.

Anxious as he was to learn the secrets of the blue water, Darwin lamented that strong winds "rendered the most important part [of the survey], the deep sea sounding, scarcely practicable."[183] FitzRoy made the best of the conditions, "eagerly tak[ing] advantage" of "two moderate days...to go round the whole group in a boat."[184] Sulivan managed to achieve a small number of measurements between two and three hundred fathoms.[185] More remarkably, at "only a mile from the southern extreme of the South

179. DAR 41:53.
180. DAR 41:56.
181. DAR 41:55, Darwin's p. 24.
182. DAR 41:54 and 41:57.
183. Entry for 7-11 April 1836. Keynes, *Diary*, 417.
184. FitzRoy, *Narrative*, 630.
185. See, for example, Darwin's list of "Sulivans. outside deep soundings" on DAR 41:53. High winds and strong currents made very poor conditions for sounding because an accurate measurement required the lead to drop straight down and the line to remain vertical. For one sounding of 770 fathoms, Darwin added a later note marking the "depth doubtful (360 really)." DAR 41:55. On the

Keeling" FitzRoy found no bottom at 1200 fathoms.[186] With nearly one and a half miles of line played out, it was among the deepest soundings that had ever been taken. The thought of it boggled Darwin's mind and challenged his mathematical skills. At the bottom of one scrap of Keeling notes are two attempts to multiply 1200 by 6, with the second yielding 7200 for the depth of FitzRoy's sounding in feet.[187] Working from a list of deep soundings and their "estimated distance" from shore, he pre-empted the officers by making his own preliminary calculations of the slope.[188] By obscure methods, Darwin found the angle to be 48º at its steepest inclination.[189] This was similar to "Beechey[ʻs] mean slope" given in the sectional diagram of the atoll Bow Island, which "from the 20 fathom line appears nearly 45[º]."[190] He sought general principles by comparing measures from different sides of the atoll, and eventually by consulting with the officers, whose facility with trignometry far exceeded his own. Considering the shallower slopes out to 30 fathoms, he found "no law about the extension of the discoloured water[, which was] at least not less on the leeward. than windward side."[191] Regarding the deeper water, Darwin found it "clear from Mr. Sullivans sections" that there was also "no law with respect to [the] Windward & Leeward...shape of [the] lower Mountain."[192] Nevertheless, the foundation was obviously exceptionally steep. Sulivan explained to him that on some attempts the

difficulties of keeping the lead line "dead up and down," see Rozwadowski, *Fathoming the Ocean*, chapter 3.

186. FitzRoy, *Narrative*, 630.

187. DAR 41:56.

188. DAR 41:54.

189. DAR 41:51. Darwin was notoriously bad at mathematics, and in many cases I cannot see how his slopes could have been produced from the measurements he was working with. It is worth noting that when he lived in London after the voyage, Darwin commissioned his brother Erasmus to calculate angles of inclination for him (see the notes by Erasmus in DAR 39.1:28-30, e.g., "14 ½ miles base with 200 ft gives 0°7'48""). Given horizontal displacements measured in miles, and elevation changes measured in feet, Erasmus returned angles indicating that he figured 6080 feet to the mile. This shows that Darwin's measurements were in nautical miles (the distance of one sixtieth of a degree of longitude at the equator) rather than statute miles.

190. DAR 41:56.

191. DAR 41.57.

192. DAR 41:52.

sounding line had been severed between 500 and 600 fathoms, suggesting that it caught the edge of a cliff at that depth. Darwin considered it the “precipice of [an] unfathomable wall” that may have been cut by the “Action of [the] sea.”[193] Altogether the deep soundings plainly contradicted the diagram he had drawn earlier in his visit, which depicted a mound of sand surrounding the atoll. Darwin noted that the “very great inclination between the 2 soundings on the SE side [is] so steep that it must be rock.”[194] It is not clear that he ever entirely reconciled these contradictory ideas, and rather that he abandoned his effort to explain, or explain away, the presumptive talus of debris outside the atoll.[195] Perhaps he was willing to do so because the discovery of such a steep foundation was more damaging to the crater-rim theory than to his own. Thus, on Humboldt’s authority, Darwin noted that “Cones of Volcano[es] have a medium slope <<from>> 33º to 40º <<Even the steepest parts but little exceeding. these numbers.>>”[196] FitzRoy’s soundings showed the atoll to be steeper than a volcano, and Darwin took confidence from them even though they left many of his original questions unanswered.

On departing Keeling, Darwin wrote his explanation of atoll formation into his diary, wherein he had previously only hinted at its existence. Up until then, the coral

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193. DAR 41:56. This cliff cutting action would occur near sea level, so Sullivan’s findings implied subsidence on the order of hundreds of fathoms.

194. DAR 41:49.

195. In the 1842 book, Darwin wrote “I at first concluded that the whole [of Keeling atoll] consisted of a vast conical pile of calcareous sand, but the sudden increase of depth at some points, and the circumstance of the line having been cut, as if rubbed, when between 500 and 600 fathoms were out, indicate the probable existence of submarine cliffs.” Several pages later, after discussing the inclination of several atolls studied by Beechey, Moresby, and others, he added “Here then occurs a difficulty;--can sand accumulate on a slope, which, in some cases, appears to exceed fifty-five degrees? [...] M. Élie de Beaumont has argued, and there is no higher authority on the subject, from the inclination at which snow slides down in avalanches, that a bed of sand or mud cannot be formed at a greater angle than thirty degrees. [...] I must conclude that the adhesive property of wet sand counteracts its gravity, in a much greater ration than has been allowed for by M. Élie de Beaumont. From the facility with which calcareous sand becomes agglutinated, it is not necessary to suppose that the bed of loose sand is thick.” Charles Darwin, *The Structure and Distribution of Coral Reefs*, 9, 23.

196. DAR 41:56.

theory was recorded only in the "Coral Islands" essay, so it was a sign of newfound self assurance that he decided to add it to the permanent record of the voyage.[197] This version emphasized the results of FitzRoy's recent deep sounding in arguing, "Hence we must consider this Is[land] as the summit of a lofty mountain."[198] He acknowledged that "to how great a depth or thickness the work of the Coral animal extends is quite uncertain."[199] But he was more convinced than ever that "we must look at a Lagoon Is[land] as a monument raised by myriads of tiny architects, to mark the spot where a former land lies buried in the depths of the ocean."[200]

Darwin's inquiries at South Keeling island were aimed at connecting zoology to geology via hydrography. He was interested in which corals formed the greatest masses of rock, how the shape of a reef was maintained, and how deep the corals went. He sought to understand the present day transition from coral animal to coral rock, and the relationship of the present day formation of coral rock with the geological history of the reef. Methodologically, he was doing as Lyell inveighed: learning present day processes and using them to determine the likely conditions in the earth's deep past.

Darwin's most highly developed use of hydrography in the aid of zoological and geological inqiries came three weeks later, and is recorded in his little-noticed field notes from Mauritius, a volcanic island with a fringing reef. Unlike South Keeling, where Darwin had exploited the fact that FitzRoy was carrying out a full survey, Mauritius was a common way-station. FitzRoy had no need to make a systematic underwater investigation of such a well-charted island, and stopped only to accord with Beaufort's instructions to "determine the difference in longitude from [the southwest

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197. On Darwin's *Beagle* diary, or "Log Book," see Browne, *Voyaging*, 194.
198. Entry for 12 April 1836. Keynes, *Diary*, 418.
199. Entry for 12 April 1836. Keynes, *Diary*, 418.
200. Entry for 12 April 1836. Keynes, *Diary*, 418.

coast of Australia] to the Mauritius."[201] Standard charts, however, could not provide Darwin with the specific details he needed. The only solution was to take to the water himself. Along with at least one unidentified accomplice, Darwin "pull[ed] out to seaward," where, as he recorded, "I sounded repeatedly with a lead, the face of which was formed like a saucer with a diameter of four inches."[202]

Darwin was by now intimately familiar with the reef building corals of the Indian Ocean. With specificity unmatched in his previous accounts, Darwin identified four discrete zones as he moved from the beach out to deep water. From the mounds of coral that formed the breakwater out to eight fathoms (48 feet), the "arming invariably came up deeply cut by the branching Madrepores & marked with the impressions of Astreas; its surface was also, without a single exception perfectly clean. not bringing up a particle of sand."[203] From eight to 15 fathoms (48-90 feet), the arming was again clean of sand and "beautifully marked with impressions of Astreas...some species of Madrepore, Seriatopora, & fragments of branching Millepora & I think Porites as figured by Lamouroux."[204] The next zone, to 20 fathoms, contained extensive beds of the Seriatopora and was free of the massive reef builder, Astrea. Finally, from 20 to 33 fathoms (120-198 feet) most of the soundings showed a sandy bottom. To be clear what this entailed, Darwin was by now confident in his ability to distinguish between several genera of corals by the indentations they left in an otherwise completely clean disc of wax four inches in diameter.

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201. Beaufort, "Memoranda for Commander FitzRoy's Instructions." Browne and Neve, *Voyage of the Beagle*, 392. FitzRoy's narrative contained less than a sentence about Mauritius, confirming that it was by then a well described destination requiring little comment in a journal of exploration: "We anchored in Port Louis, at the Mauritius, on the 29th of April: sailed thence on the 9th of May: passed near Madagascar—thence along the African shore—and anchored in Simon's Bay, at the Cape of Good Hope, on the 31st."
202. DAR 38.2:894.
203. DAR 38.2:894.
204. DAR 38.2:895.

Armed with the knowledge from his private survey, Darwin's conclusions exuded confidence in matters on which he had previously been willing only to "conjecture." He argued, referring to the inhabitants of the zone from eight to 15 fathoms, "that the limit of 25-30 ft fixed upon by M. Quoy & Gaimard as the extreme depth at which the genus Astrea grows, is three times too little."[205] He went on to enumerate a series of principles of reef growth. No longer was he merely pointing out common features of reefs, he was willing to give an explanation of their immediate causes. He concluded that even at an island like Mauritius, "coral does not usually grow attached to the shores. a fact which probably originate[s] from the want of <<a solid>> foundation & <<the>> injurious tendency of the loose matter washed about by the sea."[206] As to the reason why "the highest part of the reef is situated at the outer margin; this must be owing either to the greater motion of the water. or to its greater purity."[207] These factors were so important that, in direct contradiction of Quoy and Gaimard, he claimed that "reefs composed of solid stone are only formed in <<a>> turbulent sea."[208] Finally, he explained that "the distance of the outer margin of the reef from the shore depends on the original inclination of the bottom."[209] "In this island," he remarked, "the thickness of the coral [at the outer margin] need not much exceed the depth at which it is believed coral can spring up from the bottom."[210] The implication was that subsidence was unnecessary to explain the reef at Mauritius, whereas "the relation of the breadth of the reef to the general angle of inclination is very important with respect to the theoretical origin of the coral reefs of the Pacifick."[211]

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205. DAR 38.2:895.
206. DAR 38.2:899.
207. DAR 38.2:900.
208. DAR 38.2:900.
209. DAR 38.2:900.
210. DAR 38.2:901.
211. DAR 38.2:900-901.

It is telling that Darwin listed these principles of reef growth in a geographical sequence from shore to deep water. Sounding had become more than a source of specimens and data. The work at Keeling and Mauritius show that by his fifth year on the *Beagle*, the practice and logic of hydrographic surveying ordered, in every sense, Darwin's understanding of coral reefs.[212] I have argued that Darwin's subsidence theory drew substantially upon the culture of maritime surveying in which he was immersed while aboard the *Beagle*. Hydrography endowed Darwin with an awareness of the undersea environment, hitherto underestimated by historians, that primed him in many ways for his insight at Tahiti. It also provided him with a practical way of testing many of his predictions about coral growth and reef morphology. In the process, hydrography became for Darwin an essential way of knowing coral reefs. Having developed this reliance at Keeling Atoll he needed Mauritius to be rendered for him in the same medium, even if it meant casting a lead himself.

Conclusion

I have argued that Darwin viewed his general study of zoophytes during the first years of the voyage as part of a larger program that would, when the opportunity arrived, include research on reef building corals and coral reefs. From Darwin's perspective

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212. It is noteworthy that in his contribution to the *Manual of Scientific Enquiry; Prepared for the Use of Her Majesty's Navy: and Adapted for Travellers in General* (1849), Darwin still adhered to this view. He wrote, in part: "The most important point with respect to coral reefs, which can be investigated, is, the depth at which the bottom of the sea, *outside the reef*, ceases to be covered with a continuous bed of living corals. This can be ascertained by repeated soundings with a heavy and very broad bell-shaped lead, armed with tallow, which will break off minute portions of the corals or take an exact impression of them. [...] There is reason to suspect that different species of corals grow in different zones of depth; so that in collecting specimens, the depth at which each kind is found, and at which it is most abundant, should be carefully noted. It ought always to be recorded whether the specimen came from the tranquil waters of the lagoon or protected channel, or from the exposed outside of the reef. [...] Whenever it is practicable, soundings ought to be taken at short ascertained distances, from *close* to the breakers in a straight line out to sea, so that a sectional outline might be protracted on paper." Reprinted in Paul H. Barrett, *Collected Papers*, 246–48.

while he was doing it, this zoology would be integrated with his investigation of reef formation. The zoological work was far from irrelevant once he had begun to think of coral reefs in connection with his geological work. As is abundantly clear from the "Coral Islands" essay and Darwin's field notes from Tahiti, Keeling, and Mauritius, the strength of the theory was immeasurably enhanced by his ability to distinguish types of living corals and his studies of their growth. Such investigations helped him to explain the original morphology of a fringing reef, the maintenance of its superficial structure when the foundation subsided, and the relative permanence of the lagoon. Helen Rozwadowski's work on nineteenth century practices of studying the deep sea suggests that the 1830s were characterized by a fundamental physical and intellectual division between hydrographers and marine zoologists.[213] Thus, Darwin's employment of the sounding lead as a source of data and living specimens for himself, if not entirely unprecedented, is worthy of note by historians of ocean science.

There is, moreover, reason to regard Darwin's marine zoology as a prerequisite for the genesis of the subsidence theory in particular. Darwin's sensitivity to the habits and habitations of marine organisms, particularly the constraining role of depth, informed his Santiago Book conclusions about the accumulation of thick beds of coral rock. Moreover, though, Darwin's determination that South America had undergone uniform elevation (and by extension that the Pacific may have subsided at a rate more gradual than that of upward coral growth) owed a non-trivial debt to his knowledge of the present day seafloor. This included, of course, facts about organisms that derived from his hydrographically-aided zoological investigations, but also the knowledge of undersea rocks and physical processes to which he was first led by his preoccupation with zoophytes.

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213. Rozwadowski, *Fathoming the Ocean*.

My account of the coral theory, then, yields the following, more general, argument about the importance of hydrography to Darwin's work during the *Beagle* voyage. He departed sufficiently well trained to make original observations and theoretical conclusions in the sciences of marine invertebrate zoology and geology, which he recognized as distinct from one another, and which he learned from two different mentors. During the voyage he identified himself as working on programs of research in these two areas, which he consistently distinguished, for example in his letters to Henslow and in his separate categories of notes and specimen lists. However, insights from one area informed his conclusions in the other, and also implied questions or hypotheses in the other, as in his thoughts about coral growth and subsiding foundations. Many authors have recognized this in greater or lesser detail, but few have recognized something that as a matter of practice was crucial to his pursuit of marine zoology and geology, and which served to integrate them. This was Darwin's "careful attention" to the officers' hydrographic surveying. Any given cast of the sounding lead could provide zoological and geological specimens, and would present them as interrelated products of a particular set of natural conditions corresponding to a particular depth and geographical location. Learning these present day organic and inorganic constituents of future sedimentary rocks provided Darwin with the key to interpreting the geological past recorded in the upraised sedimentary beds of South America. His sensitivity to the subtle differences existing in the present seafloor due to various conditions gave him clues to the geodynamic processes of elevation and subsidence that must have existed when certain types of beds were deposited in the past. Moreover, his knowledge of present day marine zoology (again significantly a product of his concern with the products of the sounding) informed his decisions of the relative ages of geological formations containing fossil marine organisms. Thus he used hydrography as a kind of stratigraphy of the present, which he then used, in Lyellian fashion, to decode the history of rocks and organisms.

If Darwin's debt to hydrography was so large, why has it previously escaped our attention? One reason is that, although they informed one another at almost every step, Darwin continued to recognize zoology and geology as separable pursuits, to be presented to distinct audiences based on their association with different theoretical puzzles of the day. Thus his publications after the voyage made neater distinctions between these disciplines than the complicated record of his fieldwork would always suggest. This suggests one reason why historians have often overlooked the importance of hydrography and the role of the surveyors in Darwin's work. They did not fit exclusively into one set of notes, nor naturally into his organization of ideas, nor easily into Darwin's mode of exposition. The lessons of hydrography were omnipresent, and sometimes implicit, in Darwin's thoughts throughout the voyage, and for that very reason fail to stand out in retrospect.

Darwin's publications in particular tended to gloss over the contributions of the surveyors. Along with his servant Syms Covington, these men were Darwin's invisible technicians, allowing him to imply in print, for example, that he had made a sounding of 86 fathoms (516 feet) by himself.[214] Having been a participant observer of the assistance Darwin received from the officers, FitzRoy was outraged that they were so scantily acknowledged in his narrative of the voyage.[215] Even the manuscript essays written late in the voyage (and phrased, as Sandra Herbert has shown, with an eye toward publication) are rarely explicit in their detail of the surveyors' contributions.[216]

Another reason why this debt may hitherto have been overlooked, even by those historians who have delved deeply into the manuscript record, is that Darwin's notes on hydrographical subjects often appear very elementary, containing laborious attempts to work out simple calculations like the conversion from fathoms to feet. This may have

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214. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 80–82.
215. Browne, *Voyaging*, 370–71.
216. Sandra Herbert, *Charles Darwin, Geologist*, chapter 5.

implied that they were little more than the doodles of a novice toying with questions that FitzRoy dealt with professionally. He tended to record the results of soundings on small scraps of paper, many of which contain little more than a few columns of numbers and pithy descriptions. Usually the meaning invested in these observations was recorded elsewhere, divorcing the interpretation from the data.

Whatever the reason that hydrography has been obscured in accounts of Darwin's work, its absence from virtually all accounts points to continued trend of exceptionalism in Darwin studies. We have more often figured Darwin as a weak-stomached passenger while on board, perhaps reading and thinking carefully, but *learning* little new during shiptime from what went on in the daily activity around him. Though no historian would countenance the myth that (in its most grotesque conception) the *Beagle* was Darwin's ship, taking him around the world to discover evolution, many have portrayed him as sufficiently absorbed in his trajectory toward the *Origin* that the actual mission of the officers proved to be largely irrelevant to Darwin's important conclusions.

On the contrary, Darwin was in many ways not exceptional. As an aspiring man of science using a voyage as his higher education and his entree into scientific society, Darwin was following a well understood convention (sufficiently obvious that his father complained that going on the voyage would constitute a change of career).[217] And more particularly, as I have shown in chapter 1, mulling atoll formation while embarked on an Admiralty survey was far from extraordinary. It was part of the job description. My account suggests two things that may have helped to make Darwin distinctive among naturalist in the 1830s. One was his access to the innovation and comprehensiveness of FitzRoy's own survey, which would have assured the captain's fame as a surveyor and

217. Though for an alternative interpretation of Robert Waring Darwin's opposition to the voyage on this count, see James R. Moore, "Darwin of Down: The Evolutionist as a Squarson-Naturalist," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 445.

explorer even if Darwin had not been on board. The other was his experience under the research tutelage of Grant and Sedgwick prior to joining an Admiralty cruise, which aided his capacity to argue from very specific zoological evidence to geological conclusions, and vice versa.

The surveys overseen by Beaufort during his years as Hydrographer have been described as exemplars of "Humboldtian science," a term popularized by Susan Faye Cannon to describe the derivation of natural laws from the mapping of precision-measured field data.[218] By Cannon's standards, Darwin's *Structure and Distribution of Coral Reefs* (described in the next chapter), which featured the only distribution map he ever published, must be considered his most Humboldtian work. This chapter, however, has illustrated the central role that concepts learned from Humboldt played in Darwin's conception and elaboration of the coral theory. Darwin thought of islands as mountains with their lower flanks submerged, and I have suggested that his insights into the habitations of corals were a development of what would now be called Humboldt's mountain biogeography. Using sounding to map corals on a submerged mountainside, I would argue, was the same type of venture as using a barometer to determine the elevation of plants. Whether or not it was in his own hand, Darwin used the sounding line as a tool of precision measurement. The larger scale distribution of different types of reefs was equally significant to Darwin's confidence in the new theory as he consulted charts on the passage from Tahiti to New Zealand. Darwin drew as well on Humboldt's geological views, of the parallelism of mountain ranges and the likelihood that a large area of Asia had subsided to compensate for the uplifting of neighboring areas. As with data from sounding, Humboldt's views were so integral to Darwin's thoughts on coral reefs that he often referred to them unremarkably, or only implicitly.

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218. Cock, *Sir Francis Beaufort*; Susan Faye Cannon, *Science in Culture: The Early Victorian Period* (New York: Science History Publications, 1978), chapter 3.

The lesson to be drawn here is not simply that Darwin considered this study to be evocative of Humboldt, which he surely did, but also that Darwin had recognized the survey as a rich source of Humboldtian data. After the voyage he wrote directly to Humboldt with details of his inquiry into the relative water temperatures around the Galapagos and the coral islands of the Pacific.[219] More broadly, this conclusion may be extended, to suggest that the type of voyage Darwin joined, though it was dissimilar from the conditions of Humboldt's own travel, encouraged and enabled him to manifest his admiration of Humboldt in specific lines of inquiry as well as romantically inflected descriptions of nature. "Humboldtian" ideals were valued as much by FitzRoy and Beaufort as by Darwin, though perhaps for slightly different reasons. Humboldt represented disinterested scientific investigation, a desirable cachet to add to the generation of knowledge by money-making (and therefore "interested") professional surveyors. I would speculate further that since the security of naval surveyors depended on an international acquiescence that scientific undertakings were for the common good, and not to be disturbed even in wartime, Beaufort could have found no better individual to symbolize his campaign than Humboldt, a Prussian who had resided in Paris and traveled under a Spanish passport.

For those concerned with Darwin's career as a geologist, I propose that in addition to illuminating the role of hydrography, my account also sheds new light on his intellectual and methodological debt to Lyell. In the *Principles*, Lyell lamented that progress in geology was stunted by insufficient knowledge of natural history. Darwin had convinced himself by mid-1834 (perhaps partly encouraged by Lyell's comment about the need for knowledge of tropical zoophytes, cited above) that the natural history of corals was lacking, and that he would be the world expert on the subject by the end of the voyage. Just over one year later he decided that "besides the avowed ignorance

219. Darwin to Humboldt, 1 November 1839. Burkhardt et al., *CCD*, v. 2.

concerning [corals], little is yet known...of the structure & origin of the Coral Islands & reefs." Here Darwin has made a Lyellian move, from knowledge of present day organic processes (and their relation to physical conditions) to interpretation of past physical processes. For Lyell it was a fundamental truth of methodologically sound geology that knowledge of the organic world (especially the calcareous organisms that got preserved in the fossil record) must precede the interpretation of the past, because one must assume that the earth's history was analogous to the present. Darwin shared this view and willingly played both roles of naturalist and geologist. The coral theory was the ultimate, but not the only, achievement of this synthesis, for here Darwin explained major formations still in the process of being laid down.

For those interested in Darwin as a theorizer, my account supports his reputation as a master rhetorician and manager of the public presentation of his theories. The coral theory was undoubtedly the most significant idea of the voyage that Darwin did *not* report in a letter to Henslow. This omission was perhaps informed by Darwin's anguish at discovering that Henslow had published extracts of the letters he had received from earlier ports of call. It suggests that Darwin's perfectionism for his theories and his absolute desire to be in charge of how and when and where they were presented were traits that existed before the 1840s, when by at least some accounts he held back the evolutionary theory. But in particular, my suggestion that the *process* of developing the coral theory was so different from its published *products* implies that Darwin did see a great value in selective and targeted presentations. The years 1836-1842 were, as we well know, a period when Darwin revised and expanded many of his thoughts from the voyage. In consultation and comparison with others, he gave new meanings to the bones and birds that he had collected.[220] Yet his subsequent arguments from this

220. See e.g., Frank J. Sulloway, "Darwin and His Finches: The Evolution of a Legend," *Journal of the History of Biology* 15, no. 1 (1982): 1–53; Stan P. Rachootin, "Owen and Darwin Reading a Fossil: *Macrauchenia* in a Boney Light," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 155–83.

evidence often implied that his theories had been permanently and inherently embedded in his specimens and observations (even if he protested to have gathered them in "Baconian" fashion). The same process occurred with his knowledge of corals and coral reefs. Each successive presentation that Darwin made of the coral theory had specific goals, private and public. Thus, as we shall see in chapter 3, he freely inverted the claimed order of his original inference to suit his purposes. In his first public presentation of the theory, the 1837 paper to the Geological Society, he argued that reef shapes and distribution were evidence of widespread subsidence in the geological past. Only in writing the *Autobiography*, when he had subordinated his aspiration to produce a grand geological synthesis to the shaping of his scientific legacy, did he refer to his South American work as a source for his original insight into the formation of living reefs.

Finally, for those interested in the places of science and the practices of scientists, we see Darwin learning things from his time at sea to match what he learned in his "voyage on land." We learn how he transcended the limitations of his physical location, as with his study of charts and voyages. Finally, we see how he drew upon the resources and encouragements of those around him. Far from being a "lone" genius, far from being untrained, he was a sponge of information and insights and he attacked questions to which his patrons and heroes wanted answers.

CHAPTER 3
Disciplining Charles Darwin: Charles Lyell and the creation of a geological coral theory, 1836-1842

Introduction

If the origins of Darwin's coral reef theory lay in the diverse practices described in chapter 2, how did it become published and remembered as a geological theory? The answer must be found in process through which the "private science" I examined in the last chapter was incorporated into public statements made in Darwin's 1837 coral reef paper and his 1842 book, *The Structure and Distribution of Coral Reefs*.

Seeking to understand the integration of Darwin's public and private science during these years, Martin Rudwick has plotted his scientific output on a "scale of relative privacy."[1] On a diagram resembling a geological section, Rudwick shows the order in which several of Darwin's "cognitive enterprises" were introduced into public circulation by way of a sequence of activities including theoretical notemaking, semi-private discussion, debating with experts, and the publishing of articles and books.

Except for the case of Darwin's species theory, these transitions have been curiously under-studied. The years from 1836, when Darwin returned from the *Beagle* voyage, to 1842 have often been called his "notebook years," in recognition of the personal notebooks in which he sketched and developed his theories on the transmutation of species. Far from being defined

1. Rudwick, "Charles Darwin in London" See also Howard Gruber's discussion of three distinct levels of scientific expression in Gruber, *Darwin on Man*, chapter 1.

solely by private rumination, however, these years also contained the most significant public activities of Darwin's career. Having returned as a celebrated scientific voyager, he entered rapidly into the elite of London science and, for the five years until he moved out of the city in 1842, remained a prominent figure in the company of men like Charles Lyell and at institutions like the Athenaeum club and the Geological Society.

This chapter is a detailed study of one of Darwin's cognitive enterprises, namely his effort to explain the shape of coral reefs, as it moved from the pages of Darwin's private notes to the leaves of his first monograph.[2] If we want to understand this period of his life, it makes sense to study this particular scholarly pursuit, which absorbed more of his time than any other project while he was living in London.[3] Even those whose primary concern is with the private-to-public emergence of Darwin's species theory ought to recognize that the coral project was his orientation in the role of public theorist. It is the more relevant because, as I argue, the barrier between Darwin's cognitive enterprises was highly permeable. Although Darwin's notebooks were nominally devoted to narrow topics, the expansive notes within them show that his ideas on reef forms, crustal movement, organic distribution, and species formation were tightly and consistently integrated.

The first main conclusion of this chapter is that Darwin continued to modify and adapt the coral theory after the voyage had ended. The 1837 paper and the 1842 book were written to fulfil distinct goals, and I demonstrate how each publication was shaped

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2. In Rudwick's original formulation, Darwin's coral reef work was included as part of the cognitive enterprise of "crustal mobility" (alongside the other enterprises of "species origins" and "man and mind"). Despite my narrower definition of the cognitive enterprise here, one goal of this chapter is to illustrate not only how the coral reef work fit within the enterprise of crustal mobility, but also to show its linkages with the nominally separate enterprise of species origins.

3. Darwin himself wrote in 1842 upon completing the coral book, "I commenced it 3 years & 7 months ago, & have done scarcely anything besides -- I have actually spent 20 months out of this period on it! & nearly all the remainder [on] sickness & visiting!!!" For my reference in preparing this chapter, I sketched a timeline of Darwin's activities while he lived in London based on his correspondence and journals. This diagram shows a total of roughly nineteen months from 1838 to 1842 when working on the coral manuscript appeared to be Darwin's primary occupation.

by Darwin's peculiar ambitions, insecurities, and professional obligations. Such factors help to explain why the fruits of Darwin's multi-disciplinary coral research were published in a book directed to a geological audience. When he began to revise his 1837 paper, he envisioned the resulting text as a single section of the book he intended to publish on the geology of the voyage. This part of the manuscript ballooned into a broader and deeper work, inflated by what Darwin found during months of coral reef research conducted not in the tropics, but in the map rooms and libraries of London. The resulting monograph became his first submission to the publishing house of Smith and Elder, which had contracted to bring out Darwin's *Beagle* geology. This book, which was by design and by definition a work of geology, has remained the ultimate statement of Darwin's coral program because he never wrote another planned work on the zoology of corals.

My second main finding is that Lyell's role in the professionalization and publicization of Charles Darwin was much more hands-on than has previously been recognized. Like a master instructing his disciple, Lyell exerted an active influence on the strategy and substance of Darwin's work. This apprenticeship gave Darwin discipline and it gave him *a* discipline, for Lyell coached him in the lifestyle of a gentlemanly specialist in science and also channeled him into the specialist realm of geology.

Historians have often remarked that Darwin's subsidence-based explanation of atoll formation was more "Lyellian" than the crater-rim doctrine that had been advocated by Lyell himself. David Stoddart, for example, has illustrated "how much better Darwin's theory fitted Lyell's general philosophical position than did [Lyell's theory]," insofar as it established the continuity between past and present processes, both organic and inorganic, and showed that "small causes could lead to great consequences."[4] In Sandra Herbert's judgment, Darwin's coral theory "adopted Lyell's ideas but transformed them" by showing "Lyell's notions of elevation and subsidence [at work] on a larger scale."[5] Martin Rudwick has recently maintained that "Lyell suffered the loss of his own theory quite cheerfully, for he recognized that Darwin had out-Lyelled him."[6] In fact, it was no coincidence that the new theory seemed on balance to strengthen, rather than undermine, Lyell's principles of geology. As I demonstrate in this chapter, Lyell choreographed his own strategic defeat on the issue of reef formation and he helped Darwin to craft the 1837 paper, a publication that portrayed the coral theory as an epitome of the Lyellian geological method.

Understanding that the the 1837 coral reef paper was a joint production of Lyell and Darwin helps to make sense of my third main conclusion, that Darwin struggled mightily in his effort to bring the coral reef book to print. Whereas the *Structure and Distribution of Coral Reefs* has often been portrayed as an almost effortless triumph of elegant reasoning (especially by historians and philosophers of science who see his coral theory as the simple precursor to his species theory), the project in fact tormented Darwin over matters of theory and method. This chapter demonstrates that Darwin felt highly ambivalent about following through on some of the more speculative assurances that he made during his period of high Lyellism in the late-1830s. It also suggests that the harsh lessons about theorizing in public that Darwin learned from publishing the coral theory permeated the barriers between Darwin's ostensibly distinct cognitive enterprises, thus serving as an object lesson when he considered how and when to publish a theory on the origin of species.

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4. David R. Stoddart, "Darwin, Lyell, and the Geological Significance of Coral Reefs," 206–7.
5. Sandra Herbert, *Charles Darwin, Geologist*, 242–43.
6. Martin J.S. Rudwick, *Worlds Before Adam* (Chicago: University of Chicago Press, 2008), 492.

Darwin's professional ambitions

To understand what became of Darwin's coral reef work, it is necessary first to examine his professional ambitions as the voyage was coming to a close. He no longer anticipated for himself the life of a country parson; instead, he planned to be a man of science in Cambridge or London, where he would be well positioned to distribute his specimens from the voyage to experts in zoological, botanical, and mineralogical classification. Darwin expected that the science of geology would reward him best for taking upon himself the burden of travel. From Mauritius he wrote to his sister Caroline that "I am in high spirits about my geology. [...I]t is a most dangerous task, in these days, to publish accounts of parts of the world, which have so frequently been visited. It is a rare piece of good fortune for me, that of the many errant (in ships) Naturalists, there have been few or rather no geologists. I shall enter the field unopposed."[7] Although elite geologists relied as much as did zoologists and botanists on specimens and observations collected by lesser-credentialed observers, they were also committed to undertaking field work for themselves.[8] The Geological Society of London followed a seasonal calendar with meetings held in the winter and spring, while the summer and autumn were reserved for field trips.[9] Each of the leading lights of the society was experienced in the field, and most of them could be identified by the locations where they had done especially meaningful work.[10] Lyell had written that "If it be true that delivery be the first, second, and third requisite in a popular orator, it is no less certain that to travel is of three-fold importance to those who desire to originate just and

7. Charles Darwin to Caroline Darwin, 29 April 1836. Burkhardt et al., *CCD*, vol. 1, 494–97.
8. On "the social and cognitive topography of geology in the 1830s," see Rudwick, "Charles Darwin in London".
9. Secord, *Controversy in Victorian Geology*, chapter 1.
10. On the territoriality of British geological practice at this time, see Secord's extended discussion of the importance of, and ultimate controversy over, the "collaborative boundary" between the Welsh research sites of Sedgwick and Murchison. Secord, *Controversy in Victorian Geology*.

comprehensive views concerning the structure of our globe."[11] That Darwin considered geological work to depend on access to the field is evident from notes he made in 1838 when he contemplated settling down to marry: "If *not* marry Travel [...] If I travel it must be exclusively geological [...which] Depend[s] on health & vigour & how far I become Zoological[.]"[12]

Darwin planned to write a book about the geology of the world, using his findings in South America to illustrate more general arguments. British geologists of the time were engaged primarily in the description and classification of strata, an undertaking that in the previous two decades had become increasingly based upon the identification and correlation of organic fossils.[13] As James Secord has observed, geologists in the early nineteenth century were more concerned with identifying sandstone than with imagining the history of the primordial sea in which it had been formed.[14] Darwin, however, wanted to use his interpretation of the terrain to decode the history of geological changes. This focus on dynamics was no doubt partly inspired by Lyell's *Principles*, but it was also surely drawn from the example of Humboldt and other continental theorists like Elie de Beaumont and von Buch.[15] In the final two years of the voyage he made increasingly careful, and ambitious, plans for his career as a writer. Included among the theoretical queries and reading notes in his Red Notebook, which he began in the first half of 1836 after filling the pages of his Santiago Book, were instructions to himself about how to present and organize his forthcoming book.[16]

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11. Charles Lyell, *Principles of Geology* (1st Ed), vol. 1, 56–57. See also Secord, *Controversy in Victorian Geology*, 25.
12. Burkhardt et al., *CCD*, vol. 2, Appendix IV, p. 443. Darwin added "If I dont travel.-- Work at transmission of Species[.]"
13. Secord, *Controversy in Victorian Geology*.
14. Secord, *Controversy in Victorian Geology*, 30.
15. On European geology, and particularly on theories of mountain building, see Greene, *Geology in the Nineteenth Century*.
16. Paul H. Barrett, et al., *Charles Darwin's Notebooks, 1836–1844: Geology, Transmutation of Species, Metaphysical Enquiries* (London and Ithaca: British Museum (Natural History) and Cornell University Press, 1987), 17–81 On Darwin as a "prospective geological author," see Sandra Herbert, *Charles Darwin, Geologist*, e.g., 164–73; Paul H. Barrett, et al., *Notebooks*, 18.

In a passage written at or shortly after leaving Mauritius, he indicated that he envisioned a book that was no less than global in scale: "In a preface, it might be well to urge, geologists to compare whole history of Europe, with America." He strategized how he could draw rhetorical strength from the geographical limitations of his work, supposing that "I might add I have drawn all my illustrations from America, purposely to show what facts can be supported from that part of the globe: & when we see conclusions substantiated over S. America & Europe. we may believe them applicable to the world."[17] He believed that the striking evidence of elevation in South America and subsidence in the Pacific would serve as keys that would make the "Geology of [the] whole world...turn out simple."[18]

With the *Beagle* moving homeward, from the coral formations of the Indian Ocean to the Cape of Good Hope and into the Atlantic, the majority of Darwin's time was spent organizing his specimen lists and preparing his manuscripts for publication.[19] As he described these days, "My occupation consists in rearranging old geological notes: the rearranging generally consists in totally rewriting them. I am just now beginning to discover the difficulty of expressing one's ideas on paper. As long as it consists solely of description it is pretty easy; but where reasoning comes into play, to make a proper connection, a clearness & a moderate fluency, is to me, as I have said, a difficulty of which I had no idea."[20] He also began to ask his family and friends to make arrangements for his return. Although he preferred the countryside around Cambridge, he imagined that living in London would "in every respect turn out the most convenient," because there he would have ready access to the learned societies where

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17. Paul H. Barrett, et al., *Notebooks*, 25 (RN p. 18).
18. Paul H. Barrett, et al., *Notebooks*, 42–44 (RN pp. 68e-73) On Darwin's visions of a "simple" geology, see Frank H.T. Rhodes, "Darwin's Search for a Theory of the Earth", and especially Sandra Herbert, *Charles Darwin, Geologist*, chapter 7.
19. See Browne, *Voyaging*, 334–39; Sandra Herbert, *Charles Darwin, Geologist*, chapter 5, especially pp. 173–76.
20. Charles Darwin to Caroline Darwin, 29 April 1836. Burkhardt et al., *CCD*, vol. 1, 494–97.

scientific business was transacted among gentlemen.[21] For a man with Darwin's ambitions, the most important of these metropolitan establishments would be the Geological, the Zoological, and the Linnean Societies. He was "very anxious to belong to the Geolog: Society," and wrote to Henslow asking him if he would "be good enough to take the proper preparatory steps."[22]

In fact, Henslow had already taken steps to place Darwin's name and his work in front of potential audiences, and by late 1835 many members of the English scientific community were animated by the thought of his return. On 16 November 1835, Henslow read extracts from Darwin's letters at a meeting of the Cambridge Philosophical Society.[23] Published in a pamphlet two weeks later, they described Darwin's studies in marine zoology, his overland excursions, and his fossil finds, from the beginning of the voyage up to his trip across the Cordillera earlier that spring. Although the extracts were heavy on description, Henslow also included some of Darwin's interpretations of the phenomena he had studied, such as his criticisms of Lamarck on the natural history of zoophytes, and his conjectured history of the Andes. Henslow told Darwin's father, when he passed along copies of the pamphlet of letters, that he believed Charles would "take [his] position among the first Naturalist[s] of the day."[24] Two days after Henslow's presentation in Cambridge, Darwin's other mentor at the university, Adam Sedgwick, discussed the contents of these letters at length before the Geological Society of London.[25] He pointed out that Henslow had by now received

21. See Charles Darwin to W.D. Fox, 15 Febrary 1836. Burkhardt et al., *CCD*, vol. 1, 491–92.

22. Darwin reported that Sedgwick had told him before the voyage that he would be willing to propose Darwin as a member of the Geological Society. Charles Darwin to J.S. Henslow, 9 July 1836. Burkhardt et al., *CCD*, vol. 1, 499–501.

23. *Extracts from Letters Addressed to Professor Henslow by C. Darwin, Esq.*, [Pamphlet published 1 December 1835] (Cambridge, 1835). Reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 3–16.

24. J.S. Henslow to R.W. Darwin, quoted in the letter from Caroline Darwin to Charles Darwin, 29 December [1835]. Burkhardt et al., *CCD*, vol. 1, 473–75.

25. Charles] [Darwin, "Geological Notes Made During a Survey of the East and West Coasts of South America, in the Years 1832, 1833, 1834, and 1835, with an Account of a Transverse Section of the Cordilleras of the Andes Between Valparaiso and Mendoza," [Communicated to the Geological

a large number of specimens from Darwin, and privately remarked that Darwin "has already sent home a Collection above all praise...& if God spare his life, he will have a great name among the Naturalists of Europe."[26] The weekly journal *The Athenaeum*, which had commented on the good prospects at the beginning of the *Beagle* voyage, reported on Darwin's work in South America, and Erasmus Darwin found that his society acquaintances in London were interested in his younger brother's travels.[27] At home, Darwin's sisters avidly catalogued these signs of Charles' "fame & glory" and delighted in his future prospects.[28] At the Admiralty, Francis Beaufort had begun to think of him as "Dr Darwin," the *Beagle*'s "Geologist and philosopher general," and was encouraging Beechey to take "such a personage" along when he set off to continue FitzRoy's survey on the coast of South America.[29]

No man of science awaited Darwin's return more eagerly than did Charles Lyell, who in his first address as president of the Geological Society said that "Few communications have exerted more interest in the Society than the letters on South America addressed by Mr. Charles Darwin to Professor Henslow."[30] Lyell's personal views on the widespread elevation of land had been disputed by a previous president of the society, George Greenough, and one point of particular contention was whether the coast of Chile had been uplifted by an earthquake in 1822.[31] Darwin's letters to Henslow implicitly supported Lyell by maintaining that uplift had occurred in 1822, and Lyell had heard from Robert Alison in South America that FitzRoy and Darwin were

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Society by Adam Sedgwick, 18 November 1835], *Proceedings of the Geological Society of London* 2 (1833–38): 210–12. Reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 16–18.

26. Sedgwick to Samuel Butler, as reported by Susan Darwin in her letter to Charles Darwin, 22 November 1835. Burkhardt et al., *CCD*, vol. 1, 469–70.

27. "Geological Society," *Athenaeum*, no. 421 (21 November 1835): 875–76. On Erasmus Darwin, see Susan Darwin to Charles Darwin, 22 November 1835. Burkhardt et al., *CCD*, vol. 1, 469–70.

28. Susan Darwin to Charles Darwin, 12 February 1836. Burkhardt et al., *CCD*, vol. 1, 488–89.

29. Beaufort to F.W. Beechey, 14 October 1835. UKHO LB6, 1834-36.

30. Lyell's anniversary address of 19 Febrary 1836, quoted in Leonard G. Wilson, *Charles Lyell: The Years to 1841: The Revolution in Geology* (New Haven: Yale University Press, 1972), 424.

31. On Lyell's dispute with Greenough over elevation, see Sandra Herbert, *Charles Darwin, Geologist*, 217–18.

witnesses to further elevation of that coastline during the earthquake of 20 February 1835.[32] What was more, Darwin's letters contained explicit praise for Lyell's *Principles* and revealed that he had already, despite having received the second and third volumes only during the voyage, adopted Lyell's new terminology for describing the age of tertiary deposits.[33]

After Darwin's work had been communicated to the Geological Society, Lyell told Sedgwick, " How I long for the return of Darwin! I hope you do not mean to monopolise him at Cambridge."[34] In the meantime, he appealed to the Hydrographer, Beaufort, for more advance news of Darwin's findings. Beaufort was in the process of dispatching Beechey, the surveyor who had studied the coral islands of the Low Archipelago in the late 1820s, to continue the South American survey where FitzRoy had left off. In December 1835, Lyell wrote, "If Capt. Beechey can see & communicate with Mr Darwin who was with Captain Fitzroy in the Beagle beg him to learn from Mr D[arwin] all the latest intelligence of the Geology of Patagonia & Chili which he can."[35] By that month, however, the *Beagle* was already beyond Tahiti and Darwin was at work on his first essay on coral reef formation.

That 1835 Coral Islands essay was one of the documents that Darwin revisited in the closing stages of the voyage as he readied his notes for publication. He had his servant, Syms Covington, recopy the manuscript into finer handwriting, and FitzRoy served as a preliminary audience. The captain annotated the fair copy with a question about the effect of "earthquake waves" and a note about channels that interrupt encircling reefs.[36] That Darwin planned to use this manuscript as the basis for a future

32. On Lyell's correspondence with Robert Alison in South America, see his letter to Francis Beaufort, 28 November 1835. UKHO Incoming Letters pre-1857, box 1, L242.
33. *Extracts from Letters*. Reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 3–16.
34. Lyell to Adam Sedgwick, December 6, 1835. Katherine M. Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, vol.1, 460–61.
35. Lyell to Francis Beaufort, 15 December 1835. UKHO Incoming Letters pre-1857, box 1, L244.
36. DAR 41:13-22. For a physical description of the manuscript, see David R. Stoddart, "Coral Islands

publication is evident from the notes he added alongside FitzRoy's. Some illustrate how he intended to improve the clearness and fluency of his argument, as when he reminded himself to "Give the reason first" and "amplify the expression."[37] Another note, "Here perhaps introduce the sentence of Polypi making a monument," reveals that he must have been pleased with the sentence he wrote in his diary after visiting Keeling Atoll, which said "Under this view, we must look at a Lagoon Is[land] as a monument raised by myriads of tiny architects, to mark the spot where a former land lies buried in the depths of the ocean."[38] He now expanded this thought on the sheet facing the final page of the manuscript, jotting "Polypi [are] historians...not only of time, but of...movem[ent]. a point on which evidence [is] so deficient."[39] As he explained to Caroline, "The subject of Coral formation has for the last half year, been a point of particular interest to me. I hope to be able to put some of the facts in a more simple & connected point of view, than that in which they have hitherto been considered." He declared that the "idea of a lagoon Island, 30 miles in diameter being based on a submarine crater of equal dimensions, has alway appeared to me a monstrous hypothesis," and he was now in a position to offer Lyell and Beaufort a superior alternative.[40]

Yet at this late stage of the voyage Darwin also still saw himself as a future expert on the zoology of corals, as evidenced by several newly written pages of notes on the physiological relations between the seprate polyps of a colony.[41] Correcting Cuvier and Lamouroux was evidently an attractive proposition as well. Of the multiple

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by Charles Darwin," 4.

37. DAR 41:18.

38. DAR 41:22. The original sentence is in the diary entry for 12 April 1836. Keynes, *Diary*, 418.

39. DAR 41:22.

40. Charles Darwin to Caroline Darwin, 29 April 1836. Burkhardt et al., *CCD*, vol. 1, 494–97.

41. DAR 5:B98-99. These notes are discussed in Sloan, "Darwin's Invertebrate Program"; Keynes, *Charles Darwin's Zoology Notes and Specimen Lists from H.M.S.* Beagle, Introduction.

sciences toward which his study of corals might lead, it remained an open question which would prove to be the most welcoming.

Return to England and alliance with Lyell

Darwin acted very quickly to connect himself and his collections with the relevant scientific authorities when the *Beagle* returned to Britain. It turned out that the most receptive and helpful man of science would be Lyell. Darwin disembarked at Falmouth on 2 October 1836 and traveled overland to visit his family in Shrewsbury while the ship was sailed to the mouth of the Thames and up the river to Woolwich, where the cargo would be unloaded and the crew paid off. From Shrewsbury he wrote to Henslow to report his arrival and to ask his "advice on many points." He had to find zoologists, botanists, and mineralogists who would be willing to describe and classify the contents of his collections, both those he had sent home early to Henslow for caretaking, and the material that remained stowed aboard the *Beagle*.[42] He traveled from Shrewsbury to Cambridge, where he saw Henslow in person, and thence to London where he began immediately "calling on various naturalist people."[43] He told his sister that "I do not think mortal man ever talked more than I have done during the last three days," but he found that his plans had "only become more perplexed instead of any clearer."[44] The problem was that despite the interest showed in him and his collections, he found very few people willing to volunteer for the laborious task of ordering the mass of material he had gathered. He began to realize with dismay that "the collectors so much outnumber the real naturalists, that the latter have no time to spare." Darwin's old mentor and sometime nemesis Robert Grant showed a short-lived

42. Darwin to Henslow, 6 October [1836]. Burkhardt et al., *CCD*, vol. 1, 507–8.
43. Darwin to Caroline Darwin, 24 October [1836]. Burkhardt et al., *CCD*, vol. 1, 509–10.
44. Darwin to Caroline Darwin, 24 October [1836]. Burkhardt et al., *CCD*, vol. 1, 509–10.

interest in examining Darwin's corallines.[45] Adrian Desmond and James Moore have speculated that it was actually Darwin who rejected Grant for this job, because he did not want be associated with Grant's radical politics and indelicate behavior.[46] The anatomist Richard Owen, whom Darwin met at a tea party hosted by Lyell, was willing to dissect some of his vertebrate specimens. But most of the "great men" were overwhelmed with their own work, and the museums were were already bulging with animal specimens that had yet to be catalogued. In disappointment, he reported to Henslow that he was "out of patience with the Zoologists," who cared so little for specimens and who seemed, when he visited the Zoological Society, to spend their time "snarling at each other, in a manner anything but like that of gentlemen."[47]

The effect of this experience was to make Darwin identify even more closely with the geologists. He got a "most cordial reception" from William Lonsdale, the secretary of the Geological Society, and an especially gratifying response from the society's president. As he told Henslow, "If I was not much more inclined for geology, than the other branches of Natural History, I am sure Mr Lyell's & Lonsdale['s] kindness ought to fix me."[48] Darwin was delighted to find that Lyell had decided, "in the *most* goodnatured manner, & almost without being asked," to make Darwin his protege. The author of the cherished *Principles* flung open the doors to a wealth of professional and social opportunities while counseling Darwin against accepting too many official responsibilities while there was so much important work to be done.[49] What Lyell did want Darwin to do was to enter into the debate over the elevation of

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45. Though Darwin met Grant while both were at the University of Edinburgh, Grant had held the combined chairs in Comparative Anatomy and Zoology at the newly founded University of London. Desmond, "'Grant, Robert Edmond'".
46. Adrian Desmond and James Moore, *Darwin: The Life of a Tormented Evolutionist* (New York: Warner Books, 1991), 203.
47. Darwin to Henslow, [30-31 October 1836]. Burkhardt et al., *CCD*, vol. 1, 512–15.
48. Darwin to Henslow, [30-31 October 1836]. Burkhardt et al., *CCD*, vol. 1, 512–15.
49. Lyell to Darwin, 26 December 1836. Burkhardt et al., *CCD*, vol. 1, 532–33.

South America. Darwin's dream of becoming the geological authority on that continent was perfectly aligned with Lyell's need for an ally in the ongoing dispute, which was partly about the specific case of Chile and partly about the larger issue of Lyell's gradual elevations. Lyell could see that if Darwin were deemed a creditable geologist, he would be a potent advocate for the contention that continents were elevated by causes on the order of those presently in effect. Whereas Lyell, Greenough, and others had based their disagreements about the interpretation of Chilean earthquakes on the strength of reports from observers on the ground, Darwin could argue the case using evidence he had gathered himself. Less than two months after his return, Darwin was voted into the Geological Society, where his alliance with Lyell quickly became common knowledge. As William Whewell wrote to John Herschel (who remained at the Cape of Good Hope, where the *Beagle* had called in June) at the beginning of December, "Darwin, who was with Capt. Fitzroy, and who visited you, is come home. He has made great natural history collections, and is become an extreme Lyellist in geology."[50]

With his patron urging him on, Darwin announced his plan to "set to work: tooth and nail at the Geology."[51] He had not yet told Lyell his views on coral reefs.[52] By late

50. Isaac Todhunter, *William Whewell: An Account of His Writings with Selections from His Literary and Scientific Correspondence*, in *Collected Works of William Whewell*, 16 vols, ed. Richard Yeo (Bristol: Thoemmes, 2001), vol. 16, 250.

51. Darwin to W.D. Fox, 6 November 1836. Burkhardt et al., *CCD*, vol. 1, 516–17.

52. Darwin was working on the topic in private and seeking feedback on his theory from a safer audience. He convinced his brother, whose house in London he shared for eight weeks after the voyage, to translate for him the latest work by Christian Ehrenberg about the formation of reefs in the Red Sea, and he allowed his family to read the section of his *Beagle* diary in which he had summarized his views. His cousin Hensleigh Wedgwood responded, "I liked your account of Keeling island, but your theory of the lagoon islands seemed to us not quite clearly enough explained." For a description of Erasmus Darwin's efforts to translate "a certain German pamphlet on the corals of the red sea," see Elizabeth Wedgwood to Hensleigh Wedgwood, [16] November [1836]. Burkhardt et al., *CCD*, vol. 1, 519–21. The pamphlet was probably Christian Gottfried Ehrenberg, *Über die Natur und Bildung der Coralleninseln und Corallenbänke Im Rothen Meere* (Berlin: Königlichen Akademie der Wissenschaften, 1834). On Hensleigh Wedgwood's comment, see his letter to Charles Darwin, [20 December 1836]. Burkhardt et al., *CCD*, vol. 1, 530. Wedgwood also annotated the diary manuscript. See Keynes, *Diary*, 419, note 2. See also Charles Darwin to Caroline Darwin, [7 December 1836], on his expectation of "detailed criticisms" from Hensleigh and Fanny Wedgwood. Burkhardt et al., *CCD*, vol. 1, 524.

December he had finished writing a paper on the elevation of South America, which he sent to Lyell so that he could review it before it was to be read aloud at the upcoming Geological Society meeting.[53] Lyell responded with delight, saying "The idea of the Pampas going up, at the rate of an inch in a century, while the Western Coast and Andes rise many feet and unequally, has long been a dream of mine. What a splendid field you have to write upon!"[54] He invited Darwin to visit him at home in London before the society meeting so that they could discuss the paper in person, Darwin having taken lodgings in Cambridge earlier that month. Lyell wanted him to elaborate on several passages and to alter "a word or two" of the paper prior to the public presentation. They met on the second day of the new year and it was almost certainly during the course of this discussion that Darwin first told Lyell about his reef theory.

One might wonder why Darwin neglected to talk to Lyell about coral reefs at any earlier opportunity. It seems likely that he was hesitant to jeopardize his newfound friendship by arguing against Lyell's view of atoll formation, even though he privately thought it a "monstrous hypothesis." However, with Lyell lapping up his views on the elevation of South America, Darwin must finally have revealed that coral reefs seemed to show that there had been compensatory subsidence in the Pacific. Lyell was stopped in his tracks, doubling over to rest his head on the seat of a chair while he absorbed what the younger man had said.[55] He then sprang up in a "state of wild excitement," and, to the nervous Darwin's immense relief, began to "encourage [him] with vivid interest."[56]

53. This paper became Charles Darwin, "Observations of Proofs of Recent Elevation on the Coast of Chili, Made During the Survey of His Majesty's Ship *Beagle*, Commanded by Capt. FitzRoy, R.N. [Abstract of the Paper Read to the Geological Society of London on 4 January 1837]," *Proceedings of the Geological Society of London* 2 (1838): 446–49.

54. Lyell to Darwin, 26 December 1836. Burkhardt et al., *CCD*, vol. 1, 532–33.

55. Charles Darwin, *Autobiography*, 100.

56. On Lyell's "wild excitement," see J.W. Judd, "Darwin and Geology," in *Darwin and Modern Science*, ed. A.C. Seward (Cambridge: Cambridge University Press, 1909), 358. Darwin recalled his surprise at Lyell's encouraging reception to a view that differed from his own in his autobiography. Charles Darwin, *Autobiography*, 100.

Indeed, Lyell put off other activities so that he could begin working together with Darwin to begin working on a new statement of the coral theory. He wrote to Charles Babbage on 6 January 1837, saying “I have been working so hard both with Darwin’s paper & since with his new views on Coral reefs, then with the Geol. Soc. Council [that he had not time to do more than begin reading Babbage’s *Ninth Bridgwater Treatise*].”[57] This hard work, it seems, consisted of helping Darwin to reshape the argument of the 1835 “Coral Islands” essay to align it more closely with Lyell’s arguments about the geological history of continental land masses.

From Lyell’s perspective, the value of this view of atoll formation was the support it would provide for his well known claim that whole continents had risen above the oceans and then fallen gradually below them, in cycles that continued into the present day. As he told Darwin, “I could think of nothing for days after your lesson on coral reefs, but of the tops of submerged continents. [...] Your lines of Elevation & subsidence will deservedly get you as great a name as De Beaumont's parallel Elevations, & yours are true, which is more than can be said of his.”[58] Lyell saw the possibility of a glorious compromise: he would sacrifice his opinion that atolls were formed atop the craters of submarine volcanoes, and in exchange he could advocate a theory that supported his much grander speculations. He had battled short-handed for seven years on behalf of his *Principles*, and now Darwin had come direct from the other side of the world to fight beside him for the slow oscillations of the earth’s crust. His congratulation to Darwin bore the traces of his own struggle: “It is all true, but do not flatter yourself that you will be believed, till you are growing bald, like me with hard work, & vexation at the incredulity of the world.”[59]

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57. Quoted in Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 435.
58. Lyell to Darwin, 13 February 1837. Burkhardt et al., *CCD*, vol. 2, 4–5.
59. Lyell to Darwin, 13 February 1837. Burkhardt et al., *CCD*, vol. 2, 4–5.

Darwin was tugged in several directions during the first half of 1837. While Lyell was eager for him to publish on coral reefs, others were fascinated by Richard Owen's new announcements about the gigantic mammalian fossils that Darwin had found in South America. Lyell praised Darwin and Owen in his second annual presidential address to the Geological Society, on 17 February, announcing that their "striking results" illustrated a law of morphological relations between the present and extinct mammals of a given locale.[60] The London zoologists were encouraging Darwin to draw together into a single treatise all the descriptions that were being made of his animal specimens from the voyage.[61] In March, he moved from Cambridge to London, where his most pressing task was to finish turning his *Beagle* diary into what he envisioned as "a kind of journal of a naturalist, not following however always the order of time, but rather the order of position."[62] He maximized his time by writing "abstracts" about fossil mammals and coral reefs that would be inserted into the journal and could also be circulated by themselves.[63]

It was at this moment of the most intense public activity of his life that Darwin's jottings in the Red Notebook began to include speculations on the possibility that one species might be transformed into another.[64] The plurality of the notes Darwin made in

60. Charles Lyell, "Address to the Geological Society, Delivered at the Anniversary, on the 17th of February, 1837," *Proceedings of the Geological Society of London* 2 (1838): 510–11.

61. See Darwin to Leonard Jenyns, 10 April [1837]. Burkhardt et al., *CCD*, vol. 2, 15–17.

62. Darwin to W.D. Fox, [12 March 1837]. Burkhardt et al., *CCD*, vol. 2, 10–12. This description is reminiscent of J.R. Forster's approach to writing his *Observations*, on which see chapter 1.

63. See Darwin to Henslow, 18 [May 1837], Burkhardt et al., *CCD*, vol. 2, 17–19, and Charles Darwin, *Journal of Researches* (London: Henry Colburn, 1839), 554, note. The abstracts were themselves abstracted as Charles Darwin, "A Sketch of the Deposits Containing Extinct Mammalia in the Neighbourhood of the Plata," *Proceedings of the Geological Society of London* 2 (1838): 542–44 and Charles Darwin, "On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations [Abstract of the Paper Read to the Geological Society of London on 31 May 1837]," *Proceedings of the Geological Society of London* 2 (1838): 552–54. These articles are reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 44–45 and 46–49.

64. Noteworthy discussions of the content and style of the Red Notebook entries are Sandra Herbert, ed., "The Red Notebook of Charles Darwin," *Bulletin of the British Museum (Natural History)* 7 (1980): 1–164; Alan G. Gross, *Starring the Text: The Place of Rhetoric in Science Studies*

this book, however, were about the oscillations of the earth's crust and the underlying constitution of the globe. This was the context in which he pondered the succession of one organic type by another, either across horizontal space like the two species of South American rhea that occupied distinct ranges, or across time, as with the extinct and living species of Guanaco.[65] As these examples suggest, when Darwin contemplated the creation of new species during the first half of 1837 he was stimulated primarily by questions about their geographical distribution. These in turn sprung from his reflections on changes to the earth's geography, as in the case of his queries on how newly elevated islands and continents would become inhabited by plants and animals. But his main preoccupation was to determine the underlying cause of these physical changes. Darwin was motivated to determine the laws underlying the succession of species by the prospect of adding a new type of evidence to this study of geographical change. If organic forms had a determinate response to changes in the earth's crust, then the history of animals and plants could reveal the history of the globe.

Meanwhile, the law-like response of coral reef shapes to geographical changes already offered a promising way to characterize the globe's internal workings. In early 1837, around the time when he and Lyell had been discussing the implications of his coral theory, Darwin noted that coral reefs superseded Lyell's best case study of the gradual rising and falling of the crust, the Temple of Serapis at Pozzuoli, Italy. Evidence from Pozzuoli indicated very localized oscillations, but Darwin believed that in South America and the Pacific he had seen evidence for vertical movements so widespread that they could only have been effected by a profound "final cause," such as the "circulation of [a] fluid nucleus" inside the globe.[66] Thus he wrote, "The great movements...agree with great continents." Darwin considered the fact that these

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(Carbondale: Southern Illinois University Press, 2006), chapter 6.
65. Paul H. Barrett, et al., *Notebooks*, 62–63 (RN pp. 130–33).
66. These phrases come from Paul H. Barrett, et al., *Notebooks*, 56–57 (RN pp. 111–12).

movements were not limited to “mere patches as in Italy” to have been “proved by [the] Coral hypoth[esis].”[67] In the second volume of the *Principles*, Lyell had sought to use both the history of organisms and the phenomenon of coral reefs as clues for decoding the movements of the earth’s crust. Now, under his guidance, Lyell’s premier disciple was working feverishly on these same phenomena with the same goal.

It is difficult to overstate the signficance of Darwin’s coral reef researches in the development of his scientific persona.[68] Lyell wrote to Herschel to praise the young man, and to report that “I am very full of Darwin’s new theory of Coral Islands.”[69] Lyell admitted that the newly returned traveler had convinced him that he “must give up [his] volcanic crater theory for ever, though it costs me a pang at first, for it accounted for so much.” Lyell could not resist enumerating all the evidence in favor of his former view. Nevertheless, he admitted, “[my] whole theory is knocked in the head, and the annular shape & central lagoon have nothing to do with volcanos, nor even with a crateriform bottom.” Echoing the language of Herschel’s *Preliminary Discourse on the Study of Natural Philosophy*, Lyell indicated that Darwin’s theory was based on a deeper phenomenon, a *vera causa*.[70] “Perhaps Darwin told you when at the Cape what he considers the true cause?” He explained to Herschel the factors that limited the growth of corals, and with the aid of three diagrams explained the effects of subsidence

67. Paul H. Barrett, et al., *Notebooks*, 58 (RN p. 117).

68. When his sister Caroline was curious to learn “on what points it is that Lyell ‘fully agrees with your [Charles’] views,’” she was certain that “[t]he Coral islands I know was one subject.” Caroline Darwin to Charles Darwin, [21 February 1837]. Burkhardt et al., *CCD*, vol. 2, 7–8. Henslow tried to contribute to Darwin’s springtime research on reefs by sending his former pupil a “chart and account of Diego Garcia,” which Darwin considered “a beautiful instance of a Lagoon Island.” It is acknowledged in Darwin’s letter to Henslow, [28 May 1837]. Burkhardt et al., *CCD*, vol. 2, 21–22.

69. Lyell to J.F.W. Herschel, 24 and 26 May 1837. RS HS 11.422 and 11.450. This letter is partially transcribed in Katherine M. Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, v. 2, 12–13 See also Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 447–49.

70. For an overview of Herschel’s philosophy of science, especially as it related to Darwin’s work in the 1830s, see David L. Hull, “Darwin’s Science and Victorian Philosophy of Science,” in *The Cambridge Companion to Darwin*, Darwin’s science and Victorian philosophy of science (Cambridge: Cambridge University Press, 2003), 168–91.

and elevation on a hypothetical “granite island round which coral is growing.” As evidence in favor of Darwin’s theory, Lyell pointed to the fact that reefs were known to exist in all intermediate states, and to Darwin’s “proof” that encircling reefs did not exist in sites of elevation. “So then,” Lyell concluded admiringly, “the coral islands, are the last efforts of drowning continents to lift their heads above water. Regions of elevation and subsidence in the ocean may be traced by the state of the coral reefs.” Lyell reported that he had “urged [President of the Geological Society William] Whewell to make him read it at our next meeting, and he “hope[d] a good abstract of this theory will soon be published. In the meantime,” he encouraged Herschel, “tell all sea-captains and other navigators to look to the facts which may test this new doctrine.”[71]

Through Lyell’s intervention, Darwin did indeed arrange to make his first public comments on coral reefs at the next meeting of the Geological Society. As he happily wrote to Henslow, “I am going to read a short account of my views of the whole affair, and Lyell I believe intends giving up the crater doctrine.-- so that I am just at present full of interest on the subject.”[72] Lyell’s letter to Herschel indicates that he was already intimately familiar with Darwin’s very latest progress on his coral reef theory. Lyell’s hand-drawn diagrams and accompanying explanations of the initial growth of a shore reef reflected a view contrary to Darwin’s 1835 essay, one prompted only by the late-voyage study of the Mauritius reefs. Likewise, any “proof” Darwin had of encircling reefs being excluded from areas of elevation was the result of work done after the voyage. As we shall see, Darwin was shortly to make the results of recently conducted

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71. Herschel did in fact heed this advice. In December of that year, in a letter to the astronomer Gipps, who was at the Paramatta Observatory near Sydney, he wrote “There is one <u>Geological</u> datum which it would be desirable to possess in every seaport...viz: the mean level of the sea as referred to some absolutely permanent & identifiable mark on the land – the recent discoveries of Mr Darwin relative to the <formation of> Coral formations of the Pacific render this a point of some considerable interest on a sea coast so peculiarly beset with these formations as that of Australia.” Herschel to Gipps, 26 December 1837. RS Herschel papers. I thank Simon Schaffer for pointing out this letter to me.

72. Darwin to Henslow, [28 May 1837]. Burkhardt et al., *CCD*, vol. 2, 21–22.

geographical comparisons into the centerpiece of his published theory. The key beneficiary of this development would be Darwin's new confidant and collaborator.

Darwin's 1837 paper on coral reefs

At eight-thirty p.m. on the last day of May, 1837, Darwin stood before the Geological Society at Somerset House and made his first public statement on coral reef formation.[73] It was just eighteen months since he had gazed upon the island of Eimeo from the heights of Tahiti, barely a year since he had pounded the corals of Mauritius with the bell shaped sounding lead, and less than eight months since he had returned to England. With his former shipmate B.J. Sulivan attending as a guest, Darwin gave a presentation that bore the distinct marks of the time and the venue in which he delivered it. As is evident both from the abbreviated version that appeared in the *Proceedings of the Geological Society* and the full text published in the *Journal of Researches* (which was typeset shortly thereafter, though it did not appear until 1839), Darwin went to great lengths to praise Lyell and to show how the form of coral reefs reinforced lessons great and small from the *Principles of Geology*.[74] As indicated by the title, "On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations," Darwin implied that it was the study of coral reefs that had led him to broader conclusions about the vertical motions of the earth's crust. Obviously, this runs contrary to the tale he told more than forty years later in his

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73. See the meeting announcement in the *Athenaeum* 500 (May 27, 1837), p. 388.

74. At the beginning of the section on coral reef formation in the *Journal of Researches*, Darwin included a footnote (p. 554) saying "This sketch was read before the Geological Society, May, 1837." Because these pages were printed so soon after the talk was delivered, and because it follows so closely the summarized versions given in the *Athenaeum* and the *Proceedings*, I have quoted it as a reliable approximation of what Darwin actually said at the Geological Society on 31 May 1837. The abbreviated versions are [Charles Darwin], "Geological Society [Report of Darwin's 'Areas of Elevation and Subsidence' Paper]," *Athenaeum*, no. 503 (17 June 1837): 443; Charles Darwin, "On Certain Areas of Elevation and Subsidence..as Deduced from the Study of Coral Formations". The *Proceedings* version is reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 46–49.

autobiography, but it also differs noticeably from the argument of his 1835 “Coral Islands” essay. Part of this change can be attributed to Darwin’s intervening visits to Keeling and Mauritius, but the strategy of presentation clearly indicates the impact of Darwin’s collaboration with Lyell in the previous eight months. More broadly, it illustrates how Darwin reshaped the work of the voyage for specific audiences in order to serve specific professional goals.

The differences between the essay written on the *Beagle* and the one prepared for the Geological Society consisted not only of a rearrangement of evidence but also a reorganization of the line of argument. In 1835 he had argued from a geographical premise to an explanation of reef forms. He began that essay by emphasizing the similar geographical orientation of different Pacific island groups, along with the similarity of individual Pacific coral reefs, which he considered virtually indistinguishable except by the presence or absence of land inside them. He then went on to propose that you could get from the simple phenomenon of a group of reef-fringed high islands to the complicated phenomenon of a group of lagoon islands if there were simply some way to remove the encircled land, and he proposed subsidence as this mechanism. When he wrote the 1835 essay, of course, he had yet to set foot on an atoll or to sound on the outer margins of a reef.

He began his 1837 paper to the Geological Society, however, by discussing the growth of reef building corals. This was perhaps the one topic on which he was unquestionably the Society’s foremost expert. Based on his personal experience “carefully examining the impressions on the soundings” at Keeling and Mauritius, the genera of corals that were capable of forming a reef could not live below ten or twelve fathoms.[75] Given this fact about the natural history of corals, Darwin explained, the form of lagoon islands [atolls], encircling reefs, and barrier reefs were all extremely

75. Charles Darwin, *Journal of Researches*, 554.

problematic. None could be expected to have grown up from their present foundations, because their outer margins stood in water too deep for corals to grow. Darwin considered these three classes of reefs to be structurally identical, so that the only way to distinguish between them was "in the absence or presence of neighbouring land, and the relative position which the reefs bear to it."[76] He contrasted these forms with a fourth type, which he called "fringing reefs." Like encircling reefs, they also made rings some distance from the shoreline of an island, but the difference was that they "extend only so far from the shore, that there is no difficulty in understanding their growth."[77] In his 1835 essay (see chapter 2), Darwin had assumed that if an island provided the necessary physical conditions, "Corall would immediately commence to grow on the shore." At Mauritius, he had been deeply puzzled by the fact that corals did not in fact appear to grow in this manner, writing, "Reef very seldom attached to shore [...] I do not understand this."[78] If corals never grew up to the shore, even on a "fringing reef," this meant that the only distinction between encircling and fringing reefs was in the depth of the water outside the reef, and whether it exceeded the depth limit of coral growth. In principle, therefore (although Darwin did not say it explicitly), the classification of a given reef was entirely dependent on the accepted value of the depth limit, and any change in this value would require fringing and encircling reefs to be reclassified. No wonder, then, that he began by establishing his own expertise on the zones of coral growth and by setting the depth limit in his terms.

Having described these four types of reefs and their distinctive relationships to any adjacent land, Darwin argued that "no explanation can be satisfactory which does not include the whole series."[79] He seems to have been implying that the striking

76. Charles Darwin, *Journal of Researches*, 556.
77. Paul H. Barrett, *Collected Papers*, vol. 1, 46.
78. Down House Notebook 1-3. Darwin also recorded his struggles with this question in DAR 38.1:891, DAR 38.2:892, and DAR 29.3:48b.
79. Charles Darwin, *Journal of Researches*, 557.

similarities between types demanded a common explanation. Thus he proposed subsidence as a mechanism that could explain the shape of each of the first three reef types, lagoon, encircling, and barrier. This had the added benefit of relating all four types to each other, thereby explaining their similarities and differences in one fell swoop. He gave his most succinct description of his "theory" of coral reefs as follows: "[the reefs'] configuration has been determined by the kind of subterranean movement."[80]

Darwin went on to argue that changes in the level of the land, such as would cause the transformation of one type of reef into another, were entirely likely to have taken place. As evidence he cited his own earlier paper on the elevation of South America, claiming that if that continent was rising insensibly, it was not improbable that the floor of the Pacific was subsiding in the same manner. He also cited the chapter on coral reefs in Lyell's *Principles*, echoing his point that subsidence was the most likely reason why the Pacific contained so little land despite the fact that volcanoes and corals both served to create it. Yet Darwin went beyond Lyell, indicating that subsidence was "rendered almost necessary" by the "inconsiderable depth at which corals grow."[81] The alternative, that every lagoon island was underlain by a submarine mountain just a few fathoms underwater, was simply too implausible to be true.[82]

When he read the paper to the Society, Darwin apparently used cross-sectional diagrams of different types of reefs to illustrate that if the premise of subsidence were accepted, "a simple fringing reef would thus necessarily be converted by the upward growth of the coral into one of the encircling order, and finally, by the disappearance

80. Charles Darwin, *Journal of Researches*, 561.
81. Charles Darwin, *Journal of Researches*, 557–58.
82. On Darwin's reasoning using the "principle of exclusion," see Martin J.S. Rudwick, "Darwin and Glen Roy: A 'Great Failure' in Scientific Method?" *Studies in the History and Philosophy of Science* 5 (1974): 97–185.

through the agency of the same movement of the central land, into a lagoon island."[83] If the reef-fringed shoreline of a continent subsided, the result would be a barrier reef, making it simply an "uncoiling [of] one of those reefs which encircle at a distance so many islands."[84] The diagrams were not published in the *Proceedings* or the first edition of the *Journal of Researches*.

Darwin's next move was to illustrate that there were actually gradations in nature between his taxonomic classes of encircling reefs, barrier reefs, and atolls, which supported the notion that they were transformed from one to another. In doing so he followed closely on from the argument he had written in his 1835 essay, claiming that "there exist[s] every intermediate form between a simple well characterized encircling reef, and a lagoon island," as well as links between encirling and barrier reefs.[85] As evidence that subsidence had occurred at the locations where these reefs were found, he mentioned four points. First was the "juxtaposition" of the types he associated with subsidence: for example, the ocean beyond the Australian barrier reef contained encircled islands and "true lagoons." Second, his personal examination of Keeling Island had revealed superficial evidence of subsidence, in the form of trees whose roots were undermined by seawater and a tide-washed storehouse that had been built seven years earlier above the high water mark. Third, he described reports of earthquakes from the encircled island of Vanikoro and the atoll of Keeling, which he took to be caused by episodes of subsidence. In the case of Keeling, he linked local earthquakes to those felt 600 miles away at the high, reef fringed island of Sumatra, where there was evidence of elevation. "One is strongly tempted to believe," he argued, "that as one end of the lever [i.e. Sumatra] goes up, the other [Keeling] goes down: that as the East Indian archipelago rises, the bottom of the neighbouring sea sinks and carries with it

83. Paul H. Barrett, *Collected Papers*, 47.
84. Charles Darwin, *Journal of Researches*, 559.
85. Paul H. Barrett, *Collected Papers*, 47.

Keeling Island, which would have been submerged long ago in the depths of the ocean, had it not been for the wonderful labours of the reef-building polypi."[86] Fourth, and perhaps as an afterthought (for it is not mentioned in the summary version), Darwin recast the work of the French crater-rim advocates Quoy and Gaimard to show how their observations could be taken as evidence in favor of his theory. He marveled that although they had crossed both the Pacific and Indian oceans, every one of the reefs they had described were associated with high land, and skirted the shore closely enough that they must (in Darwin's taxonomy) be considered fringing reefs.[87] Then he pointed out that in the case of each reef that Quoy and Gaimard had taken to be characteristic of "the general structure" of reefs, the Frenchmen had mentioned independently "in different parts of [their] account," that the islands they fringed had recently been elevated. In his mind, the fact that Quoy and Gaimard had documented the coincidence of fringing reefs with areas of elevation, independently of Darwin's prediction that they should be found together, carried "the same weight as positive evidence."[88]

With the subsidence theory fully sketched out, Darwin anticipated his critics by addressing another of the issues that had exercised him before and during his visit to Keeling. "It may be said," he acknowledged, "granting the theory of subsidence, [that] a mere circular disc of coral would be formed, and not a cup-shaped mass." This, in other words, was the question of why a lagoon would remain open after the encircled island had finally subsided entirely beneath the ocean (see chapter 2). Again, Darwin drew attention to his own field study of the Keeling reef, where he had acquired more first

86. Charles Darwin, *Journal of Researches*, 564.

87. That is, as he said, that the reefs described by Quoy and Gaimard, "do not require a foundation at any greater depth than that from which the coral-building polypi can spring." Charles Darwin, *Journal of Researches*, 561. See chapter 1 for my analysis of the reefs visited by Quoy and Gaimard, and for my demonstration that their paper was employed primarily as a statement on the formation of atolls despite their never having seen a reef of this type.

88. Charles Darwin, *Journal of Researches*, 561. Note that throughout his analysis of Quoy and Gaimard's joint publication, Darwin referred by name solely to Quoy.

hand experience of the economy of a living reef than any European naturalist except perhaps Ehrenberg. The reasons he listed were identical to those he had established in the spring of 1836. Fringing and encircling reefs always had at least some channel between them and the shore, and the strongest reef building corals (what in the field he had called the "bulkhead" corals) grew only on the outer reef. These factors encouraged the presence of a lagoon, while two others worked to keep it from being filled in. The closer the delicate corals of the lagoon came to filling it in by their own growth, the less favorable became the conditions in which they lived and so the slower they grew. Meanwhile, the absence of high land inside the reef meant that there would be no inorganic sediment that could fill the lagoon.

At this point of the paper Darwin made an important shift from trying to explain the form of reefs to using the form of reefs as an explanation for other phenomena. Whereas he initially sought to make a convincing case that certain reef shapes were the product of subsidence, in the second half of the paper he began to take subsidence as a given. To this moment, Darwin had barely mentioned the geographical distribution of islands, which had been his point of departure in the 1835 essay. But now, having used a discussion of coral growth to establish subsidence as the most likely cause underlying the development of encircling, barrier and atoll reefs, he introduced his geographical evidence as an independent "test" of "the truth of the theory." By implying that geographical evidence was unnecessary in the first place as proof for his explanation of reefs, he could suggest that it was legitimate to use reefs to explain geographical patterns.

He argued that if subsidences occurred in the same manner as the elevations he had encountered in South America, that is, if they "act[ed] over wide areas with a very uniform force," then his theory would be confirmed if the reefs that were characteristic of subsidence were located together in areas distinct from those where there existed islands with beds of raised shells and corals, and "mere skirting reefs," both of which stood as "proof of elevation." He declared that "I think it can be shown that such is the case in a very remarkable degree; and that certain laws may be inferred from the examination [of reef distribution], of far more importance than the mere explanation of the origin of the circular or other kinds of reefs."[89] Now Darwin began to pursue what he called "the main object of the paper," which was to use the form of coral reefs as an index to the movements of the Pacific and Indian Ocean floors.

This was the Lyellian project of cataloguing areas of elevation and subsidence in the ocean, a task which he argued was now possible by reference to his independently-formed theory of coral reef formations. In order to illustrate how such an enterprise would work, Darwin systematically described the distribution of reefs from east to west, starting at the west coast of the Americas. In his presentation at the Geological Society he displayed a map on which their locations had been marked, although no such illustration accompanies the full version published in the *Journal of Researches*.[90] The written text instead guided the reader on a hypothetical journey, "commencing on the shores of South America," "passing over the space of ocean," "continuing with our examination," and so on, which was made possible by the detailed charts that he had encountered as they were being made and used during the *Beagle* voyage.[91] Properly viewed, such charts allowed viewers who had never left Britain to experience the same type of perspective he had enjoyed when he stood high on Tahiti and imagined the island of Eimeo to be sinking: "Now if we look in a chart, at the prolongation of the

89. Charles Darwin, *Journal of Researches*, 562.

90. In Whewell's Presidential Address of 1838 he reminded the society that "We have had placed before us the map, in which Mr. Darwin has [...] divided the surface of the Southern Pacific and Indian oceans into vast bands..." William Whewell, "Address to the Geological Society, Delivered at the Anniversary, on the 16th of February, 1838," *Proceedings of the Geological Society of London* 2 (1838): 644.

91. For a brief discussion of "maps as instruments of thought" for Darwin, see Jane R. Camerini, "Evolution, Biogeography, and Maps: An Early History of Wallace's Line," in *Victorian Science in Context*, ed. Bernard Lightman (Chicago: University of Chicago Press, 1997), 70–109.

reef towards the northern end of New Caledonia, *and then complete the work of subsidence*...”[92] He speculated about the kind of landforms that underlay various reefs, judging that the atolls of the Low Archipelago had each been “moulded round the flanks of so many distinct islands,” while the whole group of the Maldives seemed to sit atop one mountainous island that “formerly occupied that part of the ocean.”[93]

Darwin claimed that plotting reefs by type across the Pacific and Indian oceans revealed groupings that could be divided into seven different linear bands, four of which were areas of subsidence and three of which were areas of elevation. The bands of movement were roughly parallel to each other, running from southeast to northwest, with the regions of subsidence separated from one another by the tracts that showed signs of elevation. Through the course of describing the limits of each of these geographic zones, he repeatedly demonstrated that “the three classes [of reefs] supposed to be produced by the same movement are found...in juxtaposition.” He likewise illustrated that such reefs were rarely found closely juxtaposed with any signs of elevation. In areas of the world that contained no lagoon islands despite the known presence of reef-building corals, he was able to demonstrate the likelihood that elevation had recently occurred. He urged, “Excepting on the theory of the form of reefs being determined by the kind of movement to which they have been subjected[,] it is a most anomalous circumstance...that the lagoon structure being universal and considered as characteristic in certain parts of the ocean, should be entirely absent in others of equal extent.”[94]

The two key features of these “linear spaces of great extent” were that the bands of elevation and subsidence alternated, and that they were “undergoing movements of an

92. Charles Darwin, *Journal of Researches*, 566 Emphasis added.
93. I discuss Darwin’s geographical imagination below, in the section on his 1842 book.
94. Charles Darwin, *Journal of Researches*, 565. Darwin claimed that this anomaly “has never been attempted to be solved.” In fact, this was Eschscholtz’s stated reason for his argument (against Forster) that lagoon islands must be underlain by submarine mountains.

astonishing uniformity."[95] What Darwin meant by "uniformity" in this context was (in my words) "of small magnitude." These "uniform" episodes of subsidence might have been geographically widespread, but their vertical magnitude had not been great enough or rapid enough to draw the living part of the reef beneath the shallow zone of coral growth. Darwin therefore employed his coral theory to argue against the notion of paroxysmal subsidences that produced more than a few fathoms of movement at a time. In other words, Darwin argued, downward movement of the crust sufficient to draw entire islands or continents to unfathomable ocean depths had been produced by the accumulation of changes of no greater magnitude than the recently-witnessed earthquake in Chile.

To those in the inner circle of the Geological Society, the similarity must have been obvious between this claim and Lyell's statements earlier in the decade on hypothetical changes of level. When Lyell had written "let a series of two hundred earthquakes strike [a] shoal, each raising the ground ten feet; the result will be a mountain two thousand feet high," he had also posited that elevation and subsidence were compensatory movements that occurred simultaneously on different parts of the globe.[96] Now Darwin too proposed a causal relationship between elevation and subsidence. Such a view was supported by two kinds of evidence generated by the coral theory. First was the general pattern of alternation between upward and downward movements shown by plotting reef types on the globe. Second were specific cases like that of the relationship between Keeling Island and Sumatra, where simultaneous earthquakes occasioned opposite movements in these islands several hundred miles apart. These points, combined with the apparent unlikelihood that there was any

95. Paul H. Barrett, *Collected Papers*, 48.
96. Charles Lyell, *Principles of Geology* (1st Ed), v. 1, 475–77.

alternative explanation for reef shapes, led Darwin to insinuate that he had revealed a "general law" underlying the formation and distribution of coral reefs:

> When we consider the absence both of widely-encircling reefs and lagoon islands in the several archipelagoes and wide areas, where there are proofs of elevations; and on the other hand the converse case of the absence of such a proof where reefs of those classes do occur; together with the juxtaposition of the different kinds produced by movements of the same order, and the symmetry of the whole, I think it will be difficult (even independently of the explanation it offers of the peculiar configuration of each class) to deny a great probability to this theory.[97]

The tactics of Darwin's presentation suggest that he was aware that this argument might be viewed as a tautology, which would explain why he limited his use of geographical evidence in the first part of the talk.

In this paper Darwin advertised a preference for "general" explanations, suggesting that the greater the number of phenomena a theory could explain, the more likely the theory was to be correct (about any of the phenomena). The type of proof Darwin was aiming at bears some similarity to the "consilience of inductions" described by the man who was presiding over the meeting, William Whewell. Trinity College, Cambridge's polymathic mathematician-cum-moral philosopher was at this point already at work on his *Philosophy of the Inductive Sciences* (1840), in which he asserted that "If we take one class of facts only, knowing the law which they follow, we may construct an hypothesis...which may represent them...[and] when the hypothesis, of itself and without adjustment for the purpose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of its reality, which has never yet [failed]."[98] If this was the type of argument that Darwin and his

97. Charles Darwin, *Journal of Researches*, 566–67.

98. Whewell coined the term "consilience," which literally meant a "jumping together" of facts from different classes of knowledge. William Whewell, *The Philosophy of the Inductive Sciences, Founded Upon Their History*, vol. 3–5 of *Collected Works of William Whewell*, 16 vols, ed. Richard Yeo (Bristol: Thoemmes, 2001 [1847; second edition]), vol. 5, 67–68. On Darwin's use of Whewell's ideas in the philosophy of science, see Michael Ruse, "Darwin's Debt to Philosophy: An Examination of the Influence of the Philosophical Ideas of John F.W. Herschel and William

contemporaries would have approved, it helps to explain why Darwin worked hard in 1837 to portray geographical zones of elevation and subsidence as a class of facts that he had not contemplated in constructing his explanation of reef forms.

Darwin stated these ideas in an almost aggressively Lyellian language, mixing direct references with allusions that would have been easily recognized by those in attendance. Lyell had opened the first volume of the *Principles* with a lengthy analogy between the work of a geologist and that of a historian.[99] Darwin portrayed reefs as legible historical records, modifying his earlier personification of corals as "historians...not only of time, but of...movem[ent]" so as to echo Lyell's construction. Thus he claimed that the "importance [of the coral theory], if true, is evident: because we get at one glance an insight into the system by which the surface of the land has been broken up, in a manner somewhat similar, but certainly far less perfect, to what a geologist would have done who had lived his ten thousand years, and kept a record of the passing changes."[100] Darwin's reading of this historical record had produced a "law almost established, that linear areas of great extent undergo movements of an astonishing uniformity, and that the bands of elevation and subsidence alternate."[101] This conclusion pressed him to venture into a speculation about figure of the globe itself, musing that the cause of these alternating bands was "a fluid most gradually propelled onwards, from beneath one part of the solid crust to another."[102] The Cambridge mathematician William Hopkins had lately been striving to model the

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Whewell on the Development of Charles Darwin's Theory of Evolution," *Studies in the History and Philosophy of Science* 6 (1975): 159–81; Hull, "Darwin's Science and Victorian Philosophy of Science".

99. Charles Lyell, *Principles of Geology* (1st Ed), vol. 1, pp. 1–4. It should be recalled that Lyell's desire to determine the historical circumstances in which geological formation had been laid down was not intrinsic to the British tradition of stratigraphy, which had for most of its practitioners been a taxonomic rather than a historical science.

100. Charles Darwin, *Journal of Researches*, 567.

101. Charles Darwin, *Journal of Researches*, 567.

102. Charles Darwin, *Journal of Researches*, 567.

mechanical effects of such a fluid on the overlying crust, while Lyell's allies Herschel and Babbage were known to be speculating on the effects that the topography of the crust had on the distribution of temperature within this molten layer.[103]

Darwin closed the paper by enumerating the geological and zoological lessons that could be "deduced" from the grand conclusions of his reflection on the growth of corals. He demonstrated that every active volcano in the Pacific and Indian oceans lay in one of his areas of elevation, and proposed another "law," that volcanism and elevation were linked consequences of the same "propulsion of fluid matter" to particular locations beneath the crust. This meant that stratigraphers--that is to say, almost everyone in the audience--could draw from Darwin's coral theory "a means of forming some judgment of the prevailing movement [i.e., elevation], during the formation of even the oldest series where volcanic rocks occur interstratified with sedimentary deposits." To this new conclusion formed from his study of the comparative distribution of reefs and volcanoes, Darwin added an insight that he originally conceived on the west coast of South America (see chapter 2), pointing out that "we may feel sure, where a great thickness of coral limestone occurs, that the reefs on which the zoophytes flourished, must have been sinking." Darwin also seemed to imply the possibility of correlating the organic remains in a thick limestone deposit with those of other well characterized formations, and thereby judging "what were the prevailing movements at different epochs."[104] Three months earlier in his Presidential address at the Society's anniversary meeting, Lyell had publicly reminded his colleagues that "Evidence of a sinking down of land, whether sudden or gradual, is usually more

103. For brief summaries of the significance of this work by their contemporaries, see Charles Lyell, "Anniversary Address, 1837," 487 and Whewell, "Anniversary Address, 1838," 646–47. For Darwin's reaction to Herschel and his reliance on Hopkins' publications of the late 1830s, see Sandra Herbert, *Charles Darwin, Geologist*, 210–15.

104. I am not certain that Darwin had this paleontological implication in mind.

difficult to obtain than the signs of upheaval."[105] Now, in a paper that Lyell had helped him to fashion, Darwin reiterated that "Any thing which throws light on the movements of the ground is well worthy of consideration; and the history of coral reefs may...elucidate such changes in the older formations." He once again asserted the lessons that his work might yield for his colleagues' research, arguing that the general laws he had adduced from studying coral reefs would make it more feasible to "speculate with...safety on the circumstances under which the complicated European formations...were accumulated."[106]

As if interpreting the record of crustal movements and deducing the constitution of the earth's nucleus were not sufficient reason to care about coral reef formation, Darwin closed by conjecturing that this topic might also provide the key to unraveling the history of life. First, his theory suggested an explanation for the "uniformity" of flora between remote islands of the vast "Indio-Polynesian" region, a puzzle that had been raised by René Lesson after the voyage of the *Coquille* in the early 1820s. Resurrecting the cherished monument metaphor from his Diary, Darwin explained that the problem was less intractable "[i]f we believe that lagoon islands, those monuments raised by infinite numbers of minute architects, record the former existence of an archipelago or continent in the central part of Polynesia, whence the germs could be disseminated."[107] Speaking on global terms, Darwin believed that such insights would allow his coral theory to "illustrat[e] those admirable laws first brought forward by Mr Lyell, of the geographical distribution of plants and animals [being] consequent on

105. Charles Lyell, "Anniversary Address, 1837," 506.
106. Charles Darwin, *Journal of Researches*, 568.
107. Darwin's undated reading notes on Lesson's *Voyage*, at DAR 29.3:48b, include "May we think the continent of which Tahiti was a peak had a peculiar vegetation & that the trade wind drifted seeds to E. Indian Isd. ??" These notes were probably written at or near St. Helena, on the leg of the *Beagle* voyage between the Cape of Good Hope and the final return to Britain. Darwin's Red Notebook contains a reference to Lesson at p. 62, shortly before the first notes mentioning St. Helena.

geographical changes."[108] In this light, it is noteworthy that Darwin's solution to the origin of the Indio-Pacific flora did not claim the former presence of a land bridge between continents currently divided by the Pacific, but rather posited a continent or group of islands with its own flora that had stood in the place where the Pacific was now located. In volume two of the *Principles*, Lyell had claimed that the distribution of continents and oceans had been relatively transient, and that such changes produced shifts in climate that ushered new epochs of life. Darwin argued to his geological colleagues that his newly determined method for determining whether a given island was in an area of elevation or subsidence "will directly bear upon that most mysterious question, whether the series of organized beings peculiar to some isolated points, are the last remnants of a former population, or the first creatures of a new one springing into existence."[109]

This sentence was almost certainly an allusive reference to the question of "the replacement of extinct species by others," which John Herschel had called "that mystery of mysteries."[110] Given that the theory of species formation that Darwin was privately working out at this time was based on a mechanism of geographical change (not natural selection), what Darwin seems to have had in mind was that using coral reef shapes as a key to understanding past vertical movements would in turn give insight into the changes that had driven the production of present species. In his B Notebook, which he opened in July 1837, he wrote "Species [are] formed by subsidence [...] elevation & subsidence [are] continually forming species."[111] An example of the way he envisioned this happening was for a piece of land to subside until it had been divided into two islands, where formerly identical members of a given species on each island would

108. Charles Darwin, *Journal of Researches*, 568.
109. Charles Darwin, *Journal of Researches*, 569.
110. John Herschel to Lyell, 20 February 1836. Quoted in Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 438–39.
111. Paul H. Barrett, et al., *Notebooks*, 191 (B p. 82).

change differently in response to unequal conditions. If the conditions produced suffient change, the new types would “keep distinct” even if the islands were later “elevate[d] & join[ed].” Such a process would result in “two species made.”[112] The B Notebook reveals that Darwin was as eager to use species as a gauge of geographical change as he was to use geographical knowledge derived from coral reefs as a key to the origin of species. “If my [species] theory [is] true,” he noted, “we get (1) a horizontal history of earth [...] (2^d) By character of any <<two>> ancient fauna, we may form some idea of [...] connection of those two countries.”[113] Probably writing in early 1838, he explained that “With [the] belief of <change> transmutation & geographical grouping we are led to endeavour to discover causes of change [...] change of species does not measure time but physical changes ”[114]

Darwin closed his address with a sentence that simultaneously reminded listeners of his own field experience and implied that the source of his allegiance to Lyell’s geological program was this very empirical evidence: “The traveller who is an eyewitness of some great and overwhelming earthquake, at one moment of time loses all former associations of the land being the type of solidity, so will the geologist, if he believe in these oscillations of level (the deeply-seated origin of which is betrayed by their forms and vast dimensions), perhaps be more deeply impressed with the never-ceasing mutability of the crust of this our World.”[115] As abstracted in the Geological Society’s proceedings, the sentence gained an even more recognizably Lyellian accent by referring to these crustal modifications as “the endless cycle of changes.”[116]

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112. Paul H. Barrett, et al., *Notebooks*, 191 (B p. 82). He had a specific location and pair of species in mind in this example, namely the rhinoceroses of Java and Sumatra. As his coral paper revealed, he believed these islands to be in the process of elevation. On Darwin’s studies of the distribution of East Indian fauna, see Camerini, “Evolution, Biogeography, and Maps”.
113. Paul H. Barrett, et al., *Notebooks*, 227 (B pp. 224–25).
114. Paul H. Barrett, et al., *Notebooks*, 227, 247 (B pp. 227 and 246).
115. Charles Darwin, *Journal of Researches*, 569.
116. Paul H. Barrett, *Collected Papers*, vol. 1, 48.

From start to finish, then, Darwin's first public account of his coral reef theory was consciously styled as an homage to Lyell's principles of geological work and an extension of Lyell's specific views on the causes and consequences of geological change. Although Darwin contradicted the crater rim theory that Lyell had championed, he went out of his way to explain why that explanation had formerly made sense, and suggested that it was only rendered implausible by his own recent determination of corals' depth limit (although by this logic, Quoy and Gaimard's narrower depth limit rendered the theory even less plausible). He referred to Lyell in connection with the likelihood of Pacific subsidence and the recent elevation around the Red Sea, along with praising his "admirable laws" of organic change. Of the eighteen sources of information named in the paper, Lyell was the only one who had never seen a living reef.[117] Even more significant than these explicit references was the undisguised and unapologetic insistence on seeing and describing the world in Lyell's terms, from the "symmetry" and "astonishing uniformity" of ongoing crustal motion to the unceasing "cycle of changes" that produced it. Darwin's paper delivered more, therefore, than a theory of coral reef formation and a road map to its implications. It was also a partisan statement of allegiance to Lyell and a declaration that in the territorial game of geology, the Pacific and Indian oceans must now be tinted in "Uniformitarian" colors.[118]

It is impossible to know exactly what role Lyell played in the composition of Darwin's coral reef paper of 1837. It is clear, as I described above, that he had

117. The sources Darwin named in the full text printed in the *Journal of Researches* were FitzRoy (twice), Kotzebue, Beechey (twice), De la Beche (twice), Flinders (twice), Dampier, Lyell (three times), Dillon (twice), Quoy (three times), Forster, Lesson (twice), Labillardiere, Bennett, Blight, Bougainville, Cook, Ehrenberg, and William Owen. (He also mentioned La Peyrouse in connection with his shipwreck at Vanikoro.) De la Beche, though mainly remembered for his geological work on British soil, had seen coral banks when he lived in Jamaica that he discussed in the source Darwin cited. See Beche, *A Geological Manual*, 141–42.

118. Whewell coined the term "Uniformitarian" in his review of the second volume of Lyell's *Principles*. [William Whewell], "[Review of C. Lyell, *Principles*, Vol. II (1832)]," *Quarterly Review* 47, no. 93 (March 1832): 126.

previously offered editorial help on what was to be Darwin's first paper to the Geological Society, on the elevation of Chile. Lyell's January letter to Charles Babbage about "working so hard [on] Darwin's [Chile] paper" gives the strong impression that Darwin's "new views on Coral reefs" were also being.developed collaboratively.[119] Darwin's kid-glove treatment of the crater rim theory and his gratuitous references to the *Principles* hardly require explanation beyond the fact that Lyell was his new social and professional patron. What deserves further comment, however, is a more subtle change in Darwin's presentation of the theory that reveals how it became more sympathetic to Lyell.

It is first necessary to point out how the accumulation of new evidence about the kinds and distribution of reefs forced Darwin to change exactly what he was arguing. The 1835 essay was limited geographically to the Pacific, whereas Darwin's 1837 scheme extended the analysis of reefs and the classification of vertical movement clear across the Indian Ocean as well, encompassing "more than a hemisphere."[120] In 1835, moreover, he had held the view that almost all Pacific reefs were of the encircling or atoll kind, and he implied that the "great extent of the Northern and Southern Pacific" was subsiding essentially as a single unit, in a way that "compensat[ed]" for the "general horizontal uplifting" of the "greater part of S. America." In 1837, having spent much more time examining other travelers' charts and accounts, he offered a more precise description of the Pacific as a zone that contained multiple alternating regions of subsidence and elevation. Describing these areas as "symmetrical," however, saved the argument that elevation and subsidence were *compensatory* crustal motions of the sort he had initially envisioned, and which of course underlay much of Lyell's conjectured physical and organic history of the earth.[121]

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119. Quoted in Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 435.
120. Charles Darwin, *Journal of Researches*, 568.
121. On compensatory movements hypothesized by Lyell, see Charles Lyell, *Principles of Geology* (1st

There was also a noticeable shift in the way Darwin characterized the areas that had sunk. Despite now claiming that subsidence affected discrete bands of the Pacific Ocean rather than the entire basin, he flirted in 1837 with the new idea that entire continents had been submerged. In the 1835 essay he stated that groups of lagoon islands indicated the presence of "a chain of Mountains [that] had there subsided."[122] In the 1836 diary entry written upon departure from Keeling, he imagined an "island" subsiding until only the coral reef "mark[ed] the spot where a former land lies buried."[123] Later in the voyage, or shortly after arriving home, Darwin began to contemplate the possibility that there had been a "continent of which Tahiti was a peak."[124] Such thinking would have appealed best to Lyell, for whom the richest payoff of the coral theory would be the demonstration of large scale revolutions in geological history, entire cycles in which the earth had been "remodeled" by the replacement of oceans with continents and vice-versa.[125] In Lyell's words, written to Herschel, "coral islands, are the last efforts of drowning *continents* to lift their heads above water."[126] In the Geological Society paper, as we have seen, Darwin as well began to speak of "continental subsidences" and the "former existence of an archipelago or continent." Drowned continents and drowned individual, or grouped, islands were not mutually exclusive, of course. The highest peaks of a sinking continent would necessarily become separate islands before they disappeared altogether. However, it was only when he found himself working side by side with Lyell that Darwin stopped defining the subsided land as part of an oceanscape (i.e. as former islands) and began to speak of the ocean itself as an impermanent feature of the earth's surface.

Ed), vol. 1, 113–17 and 473–77.

122. David R. Stoddart, "Coral Islands by Charles Darwin," 17 [Darwin's pp. 22–22a].

123. Diary entry for 12 April 1836. Keynes, *Diary*, 418.

124. See note 107, above.

125. For Lyell, "*One* complete revolution" would have taken place in the cycle of geological changes when "various causes of change both igneous and aqueous [had] remodel[ed]...the entire crust of the earth." Charles Lyell, *Principles of Geology* (1st Ed), v. 2, 271.

126. Lyell J.F.W. Herschel, 24 and 26 May 1837. RS HS 11.450. Emphasis added.

An English geological elite and its response to Darwin's paper

With its compelling new solution to the problem of atoll formation and its audacious theoretical conclusions, Darwin's paper struck even the best-informed members of the Society like a blow from a geological hammer. As the ambitious stratigrapher Roderick Impey Murchison hurriedly wrote to Herschel, "Last wednesday's Geological brought with it a paper from Mr C Darwin...which astonished us all."[127] As far as Murchison was concerned, Darwin had "proved, that [coral islands] were always formed by subsidence of continents & islands, & never upon elevating points...as had been supposed." He reported with no apparent disagreement that Darwin had "summed up by laying it down, that all the land of the Corallian Seas was sinking -- all that of S America rising; the one compensating the other." Because the content of formal discussion at the Geological Society was intentionally withheld from the public record, such correspondence is the only available clue to what took place after Darwin had finished his paper.[128] Darwin himself announced to his friend William Darwin Fox that he had felt "favourably received by the great guns."[129] There is indeed no evidence that any opposition was raised. Quite to the contrary, the person whom many felt would challenge any new coral theory made a grand gesture of support. Thus Sulivan, observing such a meeting for the first time, was astonished to hear Lyell "[giving] in his adhesion to [Darwin's] views at once."[130] Knowing that Lyell was the foremost advocate of the "monstrous hypothesis" that the *Beagle* shipmates had sought to undermine at Keel-

127. R.I. Murchison to J.F.W. Herschel, 3 June 1837. RS HS 12.391.
128. On the tradition of discussion at the Geological Society in this period, see Rudwick, *The Great Devonian Controversy*, 25–26 and Secord, *Controversy in Victorian Geology*, 14–24.
129. Darwin to W.D. Fox, 7 July [1837]. Burkhardt et al., *CCD*, vol. 2, 29–30.
130. B.J. Sulivan to Darwin, 29 November 1881. DAR 177. This letter was prompted by the publication of Katherine Lyell's *Life, Letters and Journals of Sir Charles Lyell, Bart.* Sulivan was eager to find out "if [Lyell] described in any way the Meeting at the Geological S[ociety] when you read your 'Coral Island' paper...the only meeting of that Society I ever attended."

ing Atoll, Sulivan perhaps expected a more adversarial response from the author of the *Principles*. Thus, the young Navyman's overwhelming memory of the meeting was, as he told Darwin, witnessing "your views being so readily accepted." Even the Fellows of the Society, to whom Darwin's flourishing reputation as an "extreme Lyellist" was well known, were astounded to hear the mentor conceding to his protege on any point. As Murchison told Herschel, "This Darwin is an immense addition to our stores. & so Lyell thinks, for he abjured on the spot all his dear theory on this subject."[131]

After the meeting, Lyell continued to promote Darwin, but also to cultivate him as a semi-official spokesman for his proprietary method of geological theorizing. While he was away that summer for a field trip to Scandinavia and Germany, for example, Lyell briefed Darwin on how he should respond to any criticism leveled *at Lyell* during the upcoming meeting of the BAAS.[132] Before leaving the country he had arranged for "the Mr. Darwin who has studied coral reefs so much & who read so splendid a paper on them at the G.S. on Wed[nesday]" to meet the missionary John Williams at the Geological Society's Somerset House.[133] Williams, who returned in 1834 from eighteen years in the Pacific, had published his *Narrative of Missionary Enterprises in the South Sea Islands* just weeks before, in April 1837.[134] This work contained a several-page geological taxonomy of island types and a detailed and well-informed argument against the "received opinion" that coral islands were formed by the growth of corals. Williams pointed out that the upraised atolls like Henderson's Island (discussed in my chapter 1) and Mangaia, which consisted of limestone formations up to 300 feet tall, could not

131. R.I. Murchison to J.F.W. Herschel, 3 June 1837. RS HS 12.391.
132. Lyell to Darwin, 29 August and 5 September 1837. Burkhardt et al., *CCD*, vol. 2, 41–43.
133. For the arrangement and the information about Darwin's specimens and diagrams, see Lyell to John Pye-Smith, 1 or 2 June 1837, published on 17 September 1935 in a letter to the *Times* of London by A.S. Pye-Smith. A copy may be found in DAR Add 8904.3:138.
134. Rev. John Williams, *A Narrative of Missionary Enterprises in the South Sea Islands* (London, 1837). On the date of publication, see John Gutch, *Beyond the Reefs: The Life of John Williams, Missionary* (London: Macdonald, 1974), 110–11.

originally have been produced by the polyps that were agreed by “scientific authorities” to inhabit a vertical range no greater than thirty feet. “The inference to be drawn from this,” he asserted, “is, that [either] the [coral] insects do exist in greater depths than are now assigned to them, or that these solid masses are not the effect of their labour: the one or the other must be the case.” Darwin’s recent paper, of course, offered a compelling third possibility, and it is easy to imagine him seizing upon Williams’ apparent paradox as nothing less than a proof of his own theory that atolls formed by subsidence.[135] Nor is it difficult to imagine Lyell’s amusement at the idea of sending his young bulldog to meet the churchman, whose book ridiculed “Lyell[’s] reasoning” on the rate of coral growth, which would have required the fantastical sum of “fifty or sixty thousand [years]” to form Mangaia, “and only that portion of [it] which appears above the water!” At Somerset House they would be able to consult Darwin’s coral specimens and the sectional reef diagrams he had used in his talk, which remained on display.[136] In the end it proved to be a profitable meeting for Darwin. Although he never engaged with Williams’ theoretical reasoning in print, the missionary became one of Darwin’s main sources of descriptive data on the geography of the region and information about the natives’ histories of their islands and reefs.[137]

This episode with Williams serves to illustrate the stratified social relations within British geology in the 1830s, as described in the early 1980s by Martin Rudwick.[138] Lyell, as the most recent president of the Geological Society, former Professor of the subject at King’s College, London, and author of an important treatise,

135. In this case, of course, the subsidence must have been followed by elevation in order for the coralline strata to have emerged several hundreed feet above sea level. This was precisely how Darwin interpreted the history of Williams’ prime example, the island of Mangaia, in his 1842 book. See Charles Darwin, *The Structure and Distribution of Coral Reefs*, 132 and 139.

136. See Lyell’s letter to Pye-Smith.

137. Darwin cited either the *Narrative* or personal communication with Williams a total of 22 distinct times in his 1842 book.

138. Rudwick, “Charles Darwin in London.”

unquestionably belonged at the center of Rudwick's social and cognitive map of the science, at the highest level of "ascribed competence." Along with Sedgwick, Murchison, De la Beche, and two or three others at this time, he could presume to interpret geological phenomena that lay outside his own field experience and to arbitrate, along with his mutually-recognized elites, professional controversies over theory or method.[139] As Rudwick points out, these were the men, amongst those referred to by Darwin as the "great guns" of science, whose scientific pursuits were conspicuously and almost exclusively geological. Thus individuals like John Herschel and William Whewell, who contributed to geological debate by way of their primary expertise in other sciences, lay outside this exclusive group.[140] Further from the center of Rudwick's map might be found those with deep, but narrow, expertise in particular geological formations or geographic regions. Beyond them were the gentlemen who formed the bulk of Geological Society membership and the science as a whole, true amateurs of geology who might be relied on for facts by the men of higher scientific status. Williams, as we can see, lay at this fringe, in the area occupied by "Scriptural geologists." Despite having infinitely more personal experience of coral reefs than did Lyell, and although he had read and deployed the same authoritative texts as the ones quoted in the *Principles*, Williams was effectively blocked from contributing to theoretical debates. In print, Darwin simply ignored the fact that Williams had offered any analytical ideas about coral reefs at all. Yet Williams' status as a geological *observer* was entirely secure, and Darwin not only relied on him for geographical descriptions, but also evidently trusted his judgment on certain questions of lithology.[141] Far from being paradoxical, this treatment may be seen as a

139. Rudwick, "Charles Darwin in London," 190–91.

140. Upon Whewell's induction as President of the Geological Society, Murchison referred obsequiously to these two as "philosophers in higher walks, who did not disdain to look upon the earth." Murchison reported this toast to Herschel in a letter of 2 February 1837. RS HS 12.390.

141. See, for example, Darwin's descriptions of the Cook and Austral Islands. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 132 and 155.

highly conventionalized way of reinforcing relatively new social and literary boundaries. Williams' *Narrative* was part of a genre that also included the *Polynesian Researches* of his missionary colleague, William Ellis, which likewise became a source for Darwin's 1842 book.[142] By tacitly disqualifying Williams and Ellis from the realm of geological theory, Darwin reinforced his own affinities with the geological elite.[143] Williams geologized, but he was not recognized as a a geologist.

Understanding this social terrain of British geology, including the factors that allowed one to move toward its central peak, is essential for interpreting the strategy of Darwin's 1837 paper. Through the course of the voyage, and on the foundation of his training with Sedgwick, Darwin had established himself well within the middle ground of geology. He was a creditable observer whose interpretation of the South American strata deserved to be taken seriously. Upon Darwin's return to England, however, Lyell quite evidently began to groom him as a potential member of the geologists' inner circle, making himself, in Darwin's words, "a most active friend."[144] The coral reef paper was Darwin's first attempt to contest matters of general theory, and as we have seen, it was very carefully chaperoned. Lyell encouraged Darwin and aided him in the writing of the paper. He also pressed Whewell into scheduling the paper before his own departure for the continent, and then staged a choreographed renunciation of his previously held view.[145] By making such brash forays into geological theory, Darwin was unmistakably announcing his candidacy for entry into the highest level of professional-

142. William Ellis, *Polynesian Researches During a Residence of Nearly Six Years in the South Sea Islands*, 2 vols (London: Fisher, Son and Jackson, 1829).

143. In the same week as Lyell's departure, the *Athenaeum* published a review of Williams' book that revealed a more general secular disdain for "the habitual unconscious arrogance of one who fancies himself in the immediate guidance of heaven." "[Review of] *A Narrative of Missionary Enterprises*..by John Williams," *Athenaeum*, no. 502 (10 June 1837): 413–14.

144. Darwin to W.D. Fox, 7 July [1837]. Burkhardt et al., *CCD*, vol. 2, 29–30.

145. The 31 May meeting was the penultimate Geological Society meeting of the season. The final one was held on 14 June, after Lyell's departure. The society reconvened on 1 November, when Darwin read his paper on the formation of mold. For the schedule, see announcements in the 1836 issues of the *Athenaeum*, e.g. 515 (9 September 1837), p. 660; 526 (25 November 1837), p. 866.

ized geology. We must recall, however, that being accepted into the geological elite was a process of conscious and conspicuous specialization. This helps to explain why Darwin presented his zoological and hydrographic investigations as precursors to his geological arguments rather than as ends in themselves.

In his February 1838 presidential address to the Geological Society, William Whewell welcomed Darwin into this elite by explaining how his coral paper factored into the geological achievements of the previous year.[146] Ever the chronicler of the practice of science itself, Whewell liked to distinguish two enterprises within the study of geology. These were "descriptive geology," which was the practice of "[cataloguing] the strata and other features of the earth's surface as they now exist," and "geological dynamics," the science of "examining and reducing to law the causes which may have produced such phaenomena."[147] He likened the contemporary state of geology to the science of astronomy in Kepler's time, with true theory beginning to emerge out of a "vast store of facts of observation."[148] The eventual goal of geology, as Whewell saw it, was the development of a fully mathematized science of physical geology, by which terrestrial processes would be reduced to the orderliness of celestial mechanics. "There can be no doubt," he admitted, "that the greater part of us shall be more usefully employed in endeavouring to add to the stores of descriptive geology, than in [the] abstruse and difficult investigations [of geological dynamics]."[149] Among the year's contributions to descriptive geology he cited Darwin's first two papers to the Society, on South American elevation and the extinct mammalia, which led Whewell to remark that

146. Desmond and Moore suggest that Whewell's high praise for Darwin was intended, at least in part, to convince him to accept a labor-intensive position as one of the secretaries of the Society. Desmond and Moore, *Darwin*, 235–36.
147. Whewell, "Anniversary Address, 1838," 632–33 For a brief discussion of his first use of the term "geological dynamics," in response to the first volume of Lyell's *Principles*, see Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 305.
148. Whewell, "Anniversary Address, 1838," 632.
149. Whewell, "Anniversary Address, 1838," 647–48.

"I cannot help considering his voyage round the world as one of the most important events for geology which has occurred for many years."[150] Indeed, he announced that the society had awarded its Wollaston Medal to Owen "for his general services to Fossil Zoology, and especially for his labours employed upon the fossil mammalia collected by Mr. Darwin."[151]

Nevertheless, he believed that Darwin's most impressive achievement lay not in the descriptive realm, but in the causal explanations demanded by geological dynamics. Whewell listed gradual, long term elevation and subsidence of the crust as principal examples of the "proximate causes" of geological phenomena. He imagined that the future of geological theory lay in the mathematical analysis of "ulterior causes," which might be construed as the "subterraneous machinery" that drove the proximate causes "by which islands and continents appear and vanish in the great drama of the world's physical history."[152] Referring "especially to his views respecting the history of coral isles," Whewell credited Darwin with bringing to view the proximate "class of events, its evidence, extent, and consequence [...] with a clearness and force which has, I think I may say, filled all of us with admiration."[153] Whewell cited Grant, Henslow and Sedgwick by name for their roles as Darwin's instructors, and although he had been a thoughtful but sharp critic of Lyell's system-building approach in the *Principles*, he reserved special acknowledgement for Lyell as the inspiration for Darwin's coral reef work.[154] He implied that it was not Lyell's treatment of the subject matter of coral reefs that had led Darwin forward. Rather, Darwin's advances were a product of his adherence to Lyell's geological method, and the result was that his insights were

150. Whewell, "Anniversary Address, 1838," 643.
151. Whewell, "Anniversary Address, 1838," 624.
152. Whewell, "Anniversary Address, 1838," 643–45.
153. Whewell, "Anniversary Address, 1838," 644.
154. On Whewell's reviews of *Principles*, see Leonard G. Wilson, *Charles Lyell: The Years to 1841*, chapters 9–10; Rudwick, *Worlds Before Adam*, sections 23.1 and 24.4.

applicable to geological theory-making in the broadest sense. "Guided by the principles which he learned from my distinguished predecessor in this chair," Whewell judged, "Mr. Darwin has presented this subject under an aspect which cannot but have the most powerful influence on the speculations concerning the history of our globe, to which you, gentlemen, may hereafter be led."[155] Although the president continued by praising "the large and philosophical views" expressed at the close of Darwin's paper, wherein he had gestured toward "the laws of change of climate, of diffusion, duration and extinction of species, and other great problems of our science which this voyage has suggested," Whewell implied that the foundation of these speculations was not yet secure. Instead, these tantalizing comments led the president to "look with impatience to the period when this portion of the results of Captain Fitz Roy's voyage shall be published, as the scientific world in general looks eagerly for the whole record of that important expedition."[156]

Nobody was more eager to see the *Beagle*'s intellectual cargo come to press than was Darwin himself, as he wrote repeatedly to his friends and mentors. Early success only reinforced his constant sense of urgency.

Darwin's coral reef paper and the acclaim it earned him marked the public high point of his first year back in England. He had deployed his most exciting idea from the voyage in a manner calculated to bring him favor with Lyell and the other geologists, and had been richly rewarded by the admiration of those he most sought to impress. As Whewell's address makes clear, Darwin's coral paper also brought honor upon Lyell's geological principles by illustrating their pedagogical value. This course of action ensured that his coral reef work, including his views on the habits and distribution of

155. Whewell, "Anniversary Address, 1838," 644–45.
156. Whewell, "Anniversary Address, 1838," 645 It must be noted that when he gave this speech, Whewell had read an advance copy of Darwin's *Journal of Researches*. See Darwin to Henslow, [21 January 1838]. Burkhardt et al., *CCD*, vol. 2, 69–70: "I have sent a copy of my journal, as far as complete, to M^r Whewell, for him to review the Geolog: part, in his anniversary speech."

coral animals, would be identified as a geological undertaking. In the process he had also given public voice to many of his boldest private speculations. For the moment, this was a great triumph. Yet in developing and publishing the coral theory this way, he had also created expectations that were to trouble him in the coming years. Through the long process of gathering and interpreting data for his coral reef book, Darwin began to recoil against the provocative tone and ambitious theorizing that had seemed so attractive and come so easily in 1837.

Studying coral reefs from London

Darwin's focus shifted to two distinct publishing enterprises once his journal of the voyage was drafted by the summer of 1837. One was the the zoology of the voyage, of which he would be the editor, and the other was the geology. The effort of finishing the journal caused him to "feel respect for every one who has written a book, let it be what it may, for I had no idea of the trouble, which trying to write common English could cost one."[157] At this stage he envisioned his geology as a single octavo volume, but by the beginning of 1838 he was canvassing Henslow for advice on the possibility of dividing the material.[158] Shortly thereafter, the publishing house of Smith, Elder & Co. advertised a forthcoming work by Darwin that was entitled *Geological observations on volcanic islands and coral formations*, which was an indication that plans to publish on South American geology had been postponed.[159] Almost immediately, though, he began to find "rather to [his] grief," that the projects were expanding beyond any schedule he had imagined.[160] With Lyell restlessly wondering when the book would appear, Darwin

157. Darwin to W.D. Fox, 7 July [1837]. Burkhardt et al., *CCD*, vol. 2, 29–30.
158. Darwin to Henslow, [21 January 1838]. Burkhardt et al., *CCD*, vol. 2, 69–70.
159. See Burkhardt et al., *CCD*, vol.2, 70, note 3.
160. Darwin to Caroline Wedgwood, [May 1838]. Burkhardt et al., *CCD*, vol. 2, 84–85.

began to worry about running into him before it was finished.[161] “I am very much obliged to you for sending me cards for your parties,” he told their mutual friend Charles Babbage, “but I am afraid of accepting them, for I should meet some people there, to whom I have sworn by all the saints in Heaven, I never go out, & should, therefore, be ashamed to meet them.”[162] He told his childhood friend Charles Whitley, “Of the future I know nothing[.] I never look further ahead than two or three Chapters -- for my life is now measured by volume, chapters, & sheets & has little to do with the sun.”[163] Yet, as his correspondence of the following four years reveals, the book consistently remained three or four maddening months from completion.[164]

Darwin had ample reasons for delay. His health began to deteriorate in 1838 and he lost months at a time to illness before any of his geological work was published in book form. By the time that happened he had also married his cousin, Emma Wedgwood, and become a father twice over. More than anything else, though, his books were delayed because he divided his working hours between so many different projects. Besides the writing on coral reefs and volcanic islands, he worked up some of his own animal specimens and superintended the publication of the *Zoology*, composed a preface and addenda to his *Journal*, made brief geological field trips and wrote a paper on the parallel “roads” of Glen Roy, Scotland, and quietly began to accumulate enormous masses of data on the origin of species. He told Lyell in September, 1838, “I have lately been sadly tempted to be idle, that is as far as pure geology is concerned, by the delightful number of new views, which have been coming in, thickly & steadily, on the classification & affinities & instincts of animals -- bearing on the question of species

161. Lyell to Darwin, 6 and 8 September 1838. Burkhardt et al., *CCD*, 99–102.
162. Darwin to Babbage, [1838]. Burkhardt et al., *CCD*, vol. 2, 67.
163. Darwin to C. Whitley, [8 May 1838].
164. See e.g., Darwin to Caroline Wedgwood, [May 1838]; Darwin to Lyell, [14] September [1838]; Darwin to W.D. Fox, 24 October [1839]. Burkhardt et al., *CCD*, vol. 2, 84–85, 104–8, 234–35.

-- note book, after note book, has been filled."[165] He managed a bout of effort on the "coral paper" in late 1839 that was halted by his wedding and resettlement into accommodation suitable for a married couple.

It was during the week he turned thirty, in February 1839, that he put himself back to work on the coral reefs with a diligence that lasted into the summer. The greatest task of this phase was a map of coral reef distribution, which would eventually be the most striking feature of his 1842 book. In the mistaken certainty that this was all that stood between him and publication, he put off responding to inquiries about his work. When he finally answered a letter from Leonard Jenyns, who had been classifying Darwin's ichthyological specimens from the voyage, he had to confess "I admire the ingenuity, with which you perceive a fishy smell about my book, my silence, & [I] daresay the very name of me." Having seen his thirtieth birthday come and go while he was mired in the ever-expanding middle of his first monograph, he acknowledged, "it is very pleasant easy work putting together the frame of a geological theory, but it is just as tough a job collecting & comparing the hard unbending facts." Lately, he "ha[d] been for the last six weeks employed over one map to illustrate [his] views on coral formations."[166]

As it turned out, the anticipated first volume of his geology never came to fruition. By October 1839, he had despaired of publishing even the coral reef and volcanic island material together. Instead, he began to "hope in a couple of months to have a very thin [octa]vo volume on Coral Formations published."[167] This reassessment fueled two months of hard work on the subject, which were followed by two months of illness.[168] After those four months he was still unwilling to show even the coral

165. Darwin to Lyell, [14] September [1838]. Burkhardt et al., *CCD*, vol. 2, 104–8.
166. Darwin to Leonard Jenyns, 15 July [1839]. Burkhardt et al., *CCD*, vol. 2, 206–7.
167. Darwin to W.D. Fox, 24 October [1839]. Burkhardt et al., *CCD*, vol. 2, 234–35.
168. Of his working routine during those two productive months, he wrote "One of my days is as like another as two peas. [...] I will give you a specimen; which will serve for every day---- Get up

material to Lyell because, as he apologized, "My M.S. is in such confusion."[169] The pattern continued, as the spur from Lyell prompted another bout of work, but then a full thirteen months of poor health kept him from making any further progress.[170] In July 1841 he began what turned out to be the final push on his coral reef material, devoting most of his working hours to the subject until he began to send his manuscript to the printer in January of 1842. When he had completed his proof reading in May, he could not hide his relief: "I have just finished correcting the last Page of Index of my small volume on Coral Reefs, wh. rejoices the inward cores of my heart."[171] His wife and children had left London for a holiday with her family while he was finishing, so he wrote to Emma to let her know the good news that he had completed the book that had occupied him since before they were married. "I will give you statistics of time spent on my coral-volume," he told her, "*not* including all the work on board the Beagle -- I commenced it 3 years & 7 months ago, & have done scarcely anything besides -- I have actually spent 20 months out of this period on it! & nearly all the remainder [on] sickness & visiting!!!"[172]

In the twenty months of land-bound time that Darwin spent actively working on his "coral-volume," he was not simply writing up a longer version of a static theory. The greater part of his effort was devoted to the research that would allow him to produce a comprehensive account of the world's coral formations. He desired to classify every known reef according to his taxonomy and to provide an explanation for

punctually at seven leaving Emma dreadful sleepy & comfortable, set to work after the first torpid feeling is over, and write about Coral formations till ten." Darwin to Caroline Wedgwood, [27 October 1839]. Burkhardt et al., *CCD*, vol. 2, 235–37.

169. Darwin to Lyell, [19 February 1840]. Burkhardt et al., *CCD*, vol. 2, 253–54.

170. See Darwin's journal entry of 28 May 1841. Burkhardt et al., *CCD*, vol. 2, p. 434.

171. Darwin to Leonard Jenyns, [9 May 1842]. Burkhardt et al., *CCD*, vol. 2, 319–20.

172. Darwin to Emma Darwin, [9 May 1842]. Burkhardt et al., *CCD*, vol. 2, 318–19. Darwin also bemoaned the expense of printing the volume, which contained a large colored map (see below). Given its estimated cost of £130 to £140, he feared that they would have to take £200 or £300 out of the family budget to supplement the printing of his other geological volumes.

every apparent anomaly. As he attested to the readers of his 1842 book, he had consulted “as far as [he] was able, every original voyage and map” that contained information on the structure and distribution of coral reefs.[173] After joining the Royal Geographical Society in 1838, he availed himself of their library to study the works of navigators like Beechey, Horsburgh, Dumont d’Urville, Lütke, Tromelin, and Duperrey.[174] He took advantage of Beaufort’s willingness to let him study charts at the Admiralty.[175] His coral research also relied, as did so many of his other projects, on correspondents who could provide him with geographical information or specialized knowledge that was unavailable in published works.[176] As he wrote to William Henry Smyth, a naval officer and one of the founders of the Geographical Society,

> I am engaged in drawing up an account of the Coral formations of the Pacific & Indian seas, and I observe it is said in Krusen[s]tern’s memoir, that you were in the [ship] Cornwallis, when Smyth’s Isl^d in the Northern Pacific was discovered. -- I am particularly anxious to know, whether the low islets & reefs, of which the group is composed, form a ring surrounding a lagoon, like so many other isl^ds. in the Pacific, and the atolls in the Indian ocean: -- or, if it has not a lagoon, then is one central island of greater height, & apparently of different constitution from the other low islets on the reef, & surrounded by a channel of deepish water: -- in short whether it has any peculiar structure. -- As I cannot obtain this information from any other quarter, if you would spare me a few minutes & send me an answer, I should feel extremely obliged and I trust you will excuse my having ventured so far to trouble you.[177]

Darwin was wondering, in short, whether this formation should be classified as a lagoon island or an encircling reef. Chief among the informants for his coral work was John Malcolmson, a Scottish surgeon (and fellow of the Geological Society) who had worked

173. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 119.

174. See Darwin to J. Shillinglaw (Secretary, Royal Geographical Society), [1839-May 1842], Darwin to Librarian of the Royal Geographical Society, [19 March 1839], Darwin to John Washington, 14 October [1839], and Darwin to John Washington, [14 October 1839]. Burkhardt et al., *CCD*, vol. 2, 153, 177–78, 229–30.

175. Charles Darwin, *The Structure and Distribution of Coral Reefs*, Preface.

176. On Darwin’s reliance on correspondents, see Janet Browne, *Charles Darwin: The Power of Place* (Princeton: Princeton University Press, 2002), e.g., 10–11.

177. Darwin to William Henry Smyth, 7 August [1839]. Burkhardt et al., *CCD*, vol. 2, 212–13. Smyth’s reply was inconclusive. See Charles Darwin, *The Structure and Distribution of Coral Reefs*, 158.

in India and visited Arabia and Sinai.[178] In a series of protracted letters written during the summer and autumn of 1839, Malcolmson told Darwin about the composition of islands in the Indian Ocean, informed him about elevated coral beds on the shores of the Red Sea, alerted him to experiments on the rate of coral growth performed by a Dr. Allan at Madagascar in the early 1830s (which had just been reported in an Edinburgh University thesis), and provided him with citations to dozens of published references on reefs and other subjects.[179] With the libraries of London at his disposal, and the testimony of gentleman travelers just a letter away, Darwin was enabled to compare countless islands he had never seen to the structures he had witnessed at Keeling, Tahiti, and Mauritius.

Although it receives little mention in most accounts of his coral theory, it is impossible to overemphasize the importance of Darwin's dry-land coral reef research. Through the whole of the *Beagle* voyage he had seen only a handful of coral formations, yet he professed to have a theory that would explain the origin of every reef in the world. All his pretensions of generality relied on establishing that the type specimens he had seen in person were truly representative of lagoon, barrier, and fringing reefs. For every day that he had spent examining reefs in the field, therefore, he spent a month poring over the reefs described in other travelers' narratives or inscribed on the Admiralty charts produced by other voyages. He compiled notes on every reef and tried to classify each of them as one of the types mentioned in his 1837 paper--lagoon island, encircling reef, barrier reef, or fringing reef. These were plotted with a color-coded entry onto his working copy of the distribution map.[180] He had brought this laborious task upon himself with the ambitious speculations of his earlier paper. Now the long-

178. See Burkhardt et al., *CCD*, vol. 2, 529 and 217, note 5.
179. John Grant Malcolmson to Darwin, 24 July 1839, 31 August 1839, 7 October 1839, [after 7 October 1839], and 30 November 1839. Burkhardt et al., *CCD*, vol. 2, 207–12, 215–18, 223–27, and 244–46. See also Charles Darwin, *The Structure and Distribution of Coral Reefs*, 77–79, 134–37, 194.
180. On these working maps, see Camerini, "Darwin, Wallace, and Maps," 95–98.

term fate of his coral theory rested on that unglamorous taxonomic work of "collecting & comparing the hard unbending facts." By pole-vaulting his way about Keeling Atoll, Darwin had propelled himself to the very limit of what any one naturalist could learn about any one reef. On the other hand, mastering all the reefs of the globe would require him to travel to a place beyond the reach of his leaping pole. Darwin found such a location half a world away from Keeling, in the imperial map rooms of the world's foremost maritime nation. It was there in London, and only there, where he accumulated the knowledge that let him erect a truly global theory of coral reef formation.[181]

Lyell's apprentice

Darwin's constant taskmaster and intellectual mentor in this work was Charles Lyell, who had more than one reason for wanting the young man to marshal his evidence and get it into print. He was not only keen to nurture his protege in the difficult task of producing a book, he also considered Darwin's coral reef work to be an important part of his own larger program of research and publication. In 1836 and 1837, Lyell was working to draft a textbook, the *Elements of Geology*.[182] This book systematically described the rocks and fossils of the geological record, whereas the *Principles* focused on the present-day physical and organic processes that were shaping the crust of the earth. After the *Elements* came out in 1838, he continued at work revising his *Principles* for what would be the sixth edition of that theoretical treatise

181. I have intentionally echoed Bruno Latour's metaphor of the Archimedean lever, which he used to describe the power of the laboratory. Here I am ascribing similar power to another location where Latour considered knowledge to be not merely consolidated, but created: the chart rooms at the center of maritime empires. See Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, MA: Harvard University Press, 1987), chapter 5.

182. Charles Lyell, *Elements of Geology* (London: John Murray, 1838).

(1840). Like any master, he was eager to strengthen his own compositions by incorporating the best of his apprentice's work.

The way Lyell appropriated Darwin's work for the *Elements* is particularly fascinating because it illustrates both how quickly he was able to act, and how the coral theory could be used to shed light on a wide range of existing puzzles. Three months before he and Darwin had ever even spoken about coral reefs, in October 1836, Lyell had already completed his first draft of the *Elements*. Yet by the time it was printed in 1838, the subsidence theory was present as a crucial component of the most important theme that ran through the book. Lyell described Darwin's coral theory when explaining how to interpret the presence at high altitudes of rocks that contain marine fossils, introducing it as a corollary to Darwin's views on the elevation of South America.[183] This was only a foreshadowing of the use to which Lyell put Darwin's expertise on coral reefs, however.

As a textbook and manual for beginning geologists that was written in terms borrowed from the *Principles*, the *Elements* was meant to inculcate the lessons of Lyell's more overtly theoretical work among a different book-buying audience.[184] Over the course of more than a hundred pages in the second half of the *Elements*, Lyell defined and analyzed the Secondary beds of Europe in ascending order of age, from the Chalk to the Old Red Sandstone.[185] A series of sedimentary beds that ranged in composition from the nearly pure carbonate of lime of the Chalk to the alternating clay, limestone, and sandstone of the Oolite and Lias groups, the Secondary deposits were staple fodder for early-Victorian geologists, who had a heavy appetite for stratigraphy.

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183. Charles Lyell, *Elements*, 96–97. In the Preface, Lyell reported that he had consulted Darwin's fully printed (but not yet published) journal of the voyage.
184. Leonard G. Wilson, *Charles Lyell: The Years to 1841*, especially p. 506.
185. For useful tables illustrating Lyell's 1838 nomenclature for the fossiliferous rocks, see Charles Lyell, *Elements*, 280–81.

The Chalk, which was found in locations right across Europe, posed a thorny problem for Lyell.[186] Because of its wide range and homegeneity, he explained, "geologists have often despaired of finding any analogous deposits of recent date."[187] It was, of course, the basic principle of Lyell's geological method that the origin of sedimentary rocks should be explained by analogy to present-day processes of deposition. In a paper read to the Geological Society in November 1837, Darwin commented that he believed large portions of the Chalk might have been formed from the powdered coral produced by reef fish and boring molluscs.[188] Lyell duly cited Darwin's description of the fine coral mud in the Keeling lagoon, along with similar samples collected at Bermuda by the Navy Lieutenant Richard Nelson, as a demonstration that deposits almost indistinguishable from the Chalk were being laid down in the present day.[189] To draw the connection more firmly, Lyell pointed to a

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186. William Montgomery has examined the relation of Darwin's coral theory to ongoing geological debates over the Chalk. He argues that Lyell's interest in coral reefs was driven by his desire to find an alternative to the Wernerian notion that the Chalk had formed as a chemical precipitate, and claims that Lyell's theory of the organic deposition of chalk (derived from corals) remained operative alongside Darwin's theory of reef formation. Montgomery is right to point out that the problem of the Chalk was one of the arenas in which Darwin's theory was disputed, but my research leads me to conclude that he has overstated his case. In contending that the debates over reef formation were essentially arguments about the origin of the Chalk, Montgomery ignores the long-standing concern (dating from J.R. Forster) over the potential role of corals in building new continents and thereby changing the proportions of land and sea. The issue of the Chalk was important to Lyell and others, but it should not obscure the fact that the study of reefs was brought to bear on even more fundamental questions about the remodeling of the earth's crust. See Montgomery, "Charles Darwin's Theory of Coral Reefs and the Problem of the Chalk".

187. Charles Lyell, *Elements*, 329.

188. [Charles Darwin], "Geological Society [Report of Darwin's Paper 'On the Formation of Mould']," *Athenaeum*, no. 526 (25 November 1837): 866. This comment was incorporated into the version printed in the Geological Society's *Proceedings*, but not the one in the *Transactions*: Charles Darwin, "On the Formation of Mould," *Proceedings of the Geological Society of London* 2 (1838): 574–76; Charles Darwin, "On the Formation of Mould," *Transactions of the Geological Society of London* 2nd ser., 5 (1840): 505–9. See also Paul H. Barrett, *Collected Papers*, vol. 1, 53, note 4. William Buckland had advised Darwin to withdraw the passage in his referee's report to the Geological Society, because it "introduc[ed] a very disputable matter into a paper that is otherwise unexceptional, & which if establishd, would be well deserving to form the Subject of a separate Communication." Buckland's report (9 March 1838) is transcribed in Burkhardt et al., *CCD*, vol. 2, 76.

189. Charles Lyell, *Elements*, 320–21.

limestone in Denmark that contained fossils characteristic of the Chalk, but which "consists of an aggregate of corals, retaining their forms as distinctly as the dead zoophytes which enter into the structure of reefs now growing in the sea."[190] Regarding the wide range of the Chalk, he drew on a recent private exchange with Darwin. In the first edition of the *Principles*, Lyell had claimed that the calcareous formations currently being laid down in the coral areas of the Pacific were "the most extensive of the groups of rocks which can be demonstrated to be now in progress."[191] His preferred explanation for the thousand-mile exent of the Chalk was on a direct analogy with the present-day coral seas. But Darwin had warned him that "It will be difficult for you to talk of great areas abounding with corals. -- People's ideas of the Pacific are ***most false***. -- In the thick archipelagoes -- in a long days sail, you will often only see one or two islands."[192] Although Darwin believed that corals had built masses of great vertical thickness during periods of subsidence, he knew from experience that the combined horizontal surface area of all the Pacific reefs paled into insignificance when compared to the vastness of the ocean. Heeding this warning, Lyell quoted almost verbatim (but without attribution) information from Darwin's letter about the geographical range of the great coral archipelagoes, and he acknowledged that "the islands in these spaces may be thinly sown."[193] But he saved his analogy by pointing out that there was no evidence of the Chalk having been laid down uniformly across the whole of the space now occupied by Europe. There may only ever have been "patches of [Chalk], of various sizes, thoroughout the area," Lyell argued, just as the analogous present day deposits being laid down around widely spread coral islands might be homogenous without being continuous.[194]

190. Charles Lyell, *Elements*, 324.
191. Charles Lyell, *Principles of Geology* (1st Ed), vol. 2, 298.
192. Darwin to Lyell, [19 December 1837]. Burkhardt et al., *CCD*, vol. 2, 65–66.
193. Compare Darwin's letter of 19 December 1837 to Charles Lyell, *Elements*, 329–30.
194. Charles Lyell, *Elements*, 329.

In the culmination of his disquisition on the Secondary strata, Lyell used analogies with actual (that is, present day) physical and organic processes to reconstruct the history of Europe as it had been recorded in these highly fossiliferous aqueous deposits. Lyell made Darwin's observations of living reefs, and his conclusions about the ongoing subterranean movements implied by reef shapes, integral to this impressive demonstration of reasoning from actual causes. In order to explain how extensive beds of calcareous deposits had been interstratified with thick deposits of what appeared to be shallow-water mud, Lyell invoked extended periods of slow elevation and subsidence that had borne and extinguished entire continents. In lighthearted anticipation of his critics, Lyell acknowledged that "we are half tempted to speculate on the former existence of the Atlantis of Plato." Turning serious, he asserted that "The story of the submergence of an ancient continent, however fabulous in history, may be true as a geological event."[195] Darwin's coral reef theory, so recently revealed to Lyell himself, emerged among the chief evidence for this claim. "If we now endeavour to restore, in imagination, the ancient condition of the European area at the period of the Oolite and Lias," he explained, "we must conceive a sea in which the growth of coral reefs and shelly limestones, after proceeding without interruption for ages, was liable to be stopped suddenly by the deposition of clayey sediment." Alternations of subsidence and elevation, producing and denuding new continents, accounted for these changes in conditions. Thus, "In order to account for [a] great formation, like the Oxford clay...covering one of coral limestone, we must suppose a sinking down *like that which is now taking place in some existing regions of coral between Australia and South America* [until] the occurrence of subsidences, on so vast a scale...caused the bed of the ocean and the adjoining land throughout the European area, to assume a shape

195. Charles Lyell, *Elements*, 362–63.

favourable to the deposition of another set of clayey strata."[196] For the observation that I have italicized, Lyell credited Darwin's unreleased *Journal* in a footnote. But Darwin's work had not simply provided the relevant analogy to prehistoric subsidences on the order of thousands of feet: his explanation of the limits on coral growth had dictated that such subsidence must be slow and uniform. This in turn helped Lyell to account for the fact that entire faunae had come and passed from existence in the period when the Secondary deposits were laid down. "Both the ascending and descending movements may have been extremely slow, like those now going on in the Pacific; and the growth of every stratum of coral...may have required centuries for its completion, during which certain organic beings may have disappeared from the earth, and others have been introduced in their place; so that, in each set of strata, from the Upper Oolite to the Lias, some peculiar and characteristic fossils were embedded."[197]

Lyell gave the very first copy of the *Elements* to Darwin, who read it straight through and was filled with admiration and perhaps envy. "I read with much interest your sketch of the secondary deposits," he told the author. "You have contrived to make it quite 'juicy', as we used to say as children of a good story. [...] I am in a fit of enthusiasm; & good cause I have to be, when I find, you have made such infinitely more use of my journal than I could have anticipated."[198] In this letter and in the continuing correspondence between Darwin and Lyell, the younger man seems to be torn between delight that Lyell found his work so useful and terror at the realization that with every new book, Lyell was chipping away at the potential novelty of his own, as yet unpublished, works. Although there is no doubt that his feelings of affection and indebtedness to Lyell remained strong, Darwin's concern may have been heightened by the past dissapointment of having his first publication preempted by his original

196. Charles Lyell, *Elements*, 402.
197. Charles Lyell, *Elements*, 403.
198. Darwin to Lyell, 9 August [1838]. Burkhardt et al., *CCD*, vol. 2, 95–99.

scientific mentor, Robert Grant (see chapter 2). If Lyell discerned this, it did not dissuade him from continuing to adapt Darwin's work for the enrichment of his own.

Darwin's fear of writing a speculative book

No sooner was the *Elements* published than Lyell began peppering Darwin with coral reef questions pertaining to his revision of the *Principles*. In this instance the young man was invited to reason along with Lyell. Calling himself "your adviser," Lyell wrote to Darwin asking whether the coral theory seemed to augur in favor of Elie de Beaumont's theory of mountain building, which stated that parallel chains had been elevated in dramatic simultaneous upheavals.[199] Lyell, of course, objected to any doctrine that relied on paroxysmal movements of the crust, so he planned a chapter-long denunciation of the Frenchman's "supposition that nature was formerly parsimonious of time and prodigal of violence." Lyell envisioned two ways that Darwin's coral theory might help his cause. The first, and less promising, was because it offered a way to determine if parts of the crust that were undergoing simultaneous upward or downward movement happened to be parallel. "If I remember right," Lyell inquired hopefully, "some of your lines are by no means parallel to others, although many are so." The second avenue was more exciting, because it might undermine the Frenchman's reasoning altogether. Elie de Beaumont had argued that the Pyrenees had been elevated rapidly within the short time *after* the end of the Cretaceous, as evidenced by the presence of (Cretaceous) Chalk deposits uplifted on their flanks. Lyell had argued that the mountains might have been formed over a much longer period of time *before* the Cretaceous fauna had died out, with some Chalk formations being uplifted while others continued to form on the seafloor. He believed that Darwin's coral paper had

199. Lyell to Darwin, 6 and 8 September 1838. Burkhardt et al., *CCD*, vol. 2, 99–102.

demonstrated just such a process, because it showed that at one geological period (that is, the present one) some reefs were being uplifted into dry land while living reefs in subsiding areas were accumulating strata of indefinite thickness: "Now in your lines of elevation, there will doubtless be coralline limestone carried upwards, belonging to the same period as the present, so far as the species of corals are concerned. Similar reefs are now growing to those which are upraised, or rising." By this "point of view," Lyell contended, "you grand discovery proves...in the most striking manner, the weight of my principal objection to the argument of De Beaumont."[200]

Darwin's response to this interrogation reveals that he was growing ambivalent about his role as a theorist. "With respect to the question how far my coral theory bears on De Beaumont's theory," he quickly replied, "I think it would be prudent to quote me with great caution, until my whole account is published, & then you (& others) can judge how far there is foundation for such generalization."[201] In truth, he hoped that Lyell would not have the opportunity to quote him at all before his own book was published.[202] But Darwin was not concerned only with priority; he was increasingly uncertain that his book could support the weight of theorizing that his short promissory essays had implied that it would. Privately, he was more than happy to agree with Lyell's "generalization." He admitted, "I do not doubt its truth...I do not believe a more utterly false view *could* have been invented than great straight lines, being suddenly thrown up." Darwin's concern was how far he could afford to stretch his evidence in public, and so he sought to temper Lyell's enthusiasm for quoting Darwin's work in progress. "[T]he extension of any view over such large spaces from comparatively few facts must be received with much caution." This fear--that all his views were based on

200. Lyell to Darwin, 6 and 8 September 1838. Burkhardt et al., *CCD*, vol. 2, 99–102.
201. Darwin to Lyell, [14] September [1838]. Burkhardt et al., *CCD*, vol. 2, 104–8.
202. "I should like my volume to come out before your new edition of Principles appears. Besides the Coral theory, -- the volcanic chapters, will, I think, contain some new facts." Darwin to Lyell, [14] September [1838]. Burkhardt et al., *CCD*, vol. 2, 104–8.

just a handful of reefs--was the very concern that drove him to the library of the Royal Geographical Society in the following winter and spring.

Darwin's skittishness about theorizing too grandly may have dated back to March 1838, when he presented a paper that marked the culmination and theoretical climax of the five papers he read to the Geological Society in 1837-1838.[203] It proved to be the occasion when he received his first serious critique, and it stung him badly. As Sandra Herbert, Frank Rhodes, and others have argued, this "Connexion" paper was the closest that Darwin came to publicly revealing the full extent of his private speculations on the figure of the earth and the true cause of geological phenomena.[204] In arguing that volcanic eruptions, earthquakes, and the elevation of mountain chains were related secondary effects of the "one motive power" that had uplifted the continent of South America, Darwin was elaborating the heirarchical relation of the geological causes that were hinted at toward the end of his 1837 report on coral reefs. Lyell considered the 1838 work to be "a paper...in support of my heretical doctrines," and he believed it had been an unalloyed success. As Lyell reported to his father-in-law, the geologist Leonard Horner, "[Darwin] opened up on [Henry] de la Bêche, [John] Phillips & others...his whole battery of the earthquakes and volcanoes of the Andes & argued that...all depended on a common cause." In the discussion that followed, Lyell "was much struck with the different tone in which my gradual causes were treated by all, even including de la Bêche[,] from that which they experienced in the same room 4 years ago when Buckland, de la Bêche, Sedgwick, Whewell and some others treated them with as much

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203. Charles Darwin, "On the Connexion of Certain Volcanic Phaenomena, and on the Formation of Mountain-Chains and Volcanos, as the Effects of Continental Elevations," *Proceedings of the Geological Society of London* 2 (1838): 654–60; Charles Darwin, "On the Connexion of Certain Volcanic Phaenomena in South America; and on the Formation of Mountain Chains and Volcanos, as the Effect of the Same Power by Which Continents Are Elevated," *Transactions of the Geological Society of London* 2nd ser., 6 (1840): 601–31.

204. Sandra Herbert, *Charles Darwin, Geologist*, chapter 7, especially 225–30; Frank H.T. Rhodes, "Darwin's Search for a Theory of the Earth".

ridicule as was consistent with politeness in my presence."[205] After years of fighting seemingly alone against the men Murchison called "practical geologists of the highest rank," Lyell was elated to find that his new ally had received only glancing blows from their mutual antagonists.[206] But whereas Lyell's soft speaking voice belied the fact that he had trained as a barrister and was accustomed to the vigor of Geological Society debates, Darwin was unnerved by his first skirmish.[207] After a Sunday meeting between the two, Lyell was forced to add a surprised postscript to the end of his triumphant letter to Horner: "I found that Darwin, who was with us yesterday evening, had felt very differently in regard to Wed[nesday]'s discussion[,] for[,] not being able to measure the change of tone in the last 4 years[,] he translated de la B's & Co.'s remarks into a vigorous defiance instead of a diminishing fire & an almost beating of retreat." Lyell concluded happily, however, with a pun on the righteousness of his geological search for the *vera causa* of geological phenomena, "But I have restored him to an opinion of the growing progress of the true cause."[208] Yet when Darwin sent the paper to John Phillips for review, he delicately maintained that "since I wrote it...[I] set less value on theoretical reasoning in geology."[209]

Let me be clear that Darwin did not abandon theorymaking. Even in the letter to Phillips, he insisted that "I have grown older...& therefore, I hope, a little wiser...but I even yet think there is some weight in the argument."[210] As I have suggested, moreover,

205. Lyell to Leonard Horner, 12 March 1838. Katherine M. Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, vol. 2, 39–41.

206. In a letter to John Herschel of February 1838, Murchison described the critics of Lyell's theory of the metamorphosis of rocks as "the practical geologists of the highest rank, including Sedgwick, Buckland, &c. & even some theoretical writers such as [George Poulett] Scrope." RS HS 12.390.

207. For the insight that Lyell "never speaks above his breath, so that everybody keeps lowering their tone to his," see Emma Darwin to Elizabeth Wedgwood, 2 April 1839. Quoted in Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 459. On Lyell's training and career in law, see Wilson's chapters 5 and 6.

208. Lyell to Horner, 12 March 1838. Postscript quoted in Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 456.

209. Darwin to John Phillips, [November 1840]. Burkhardt et al., *CCD*, vol. 2, 273–74.

210. Darwin to John Phillips, [November 1840]. Burkhardt et al., *CCD*, vol. 2, 273–74.

Darwin's willingness to theorize with Lyell or in private was never in doubt. He continued to hypothesize boldly in his personal species notebooks, indeed so boldly that according to some scholars he was beginning to have nightmares about being executed if his belief in transmutation were revealed.[211] In talks and periodicals, too, he was willing to "enter on speculative grounds," as he did before the Royal Society at the end of his major paper on Glen Roy.[212]

Darwin's hesitancy was tied to one particular type of public presentation, the book. There appear to be several distinct reasons for this. One problem was the scale of the work and the attendant desire to be comprehensive. There was, as he had told Jenyns, an enormous number of "facts" to be dealt with. It seems likely that this was exacerbated by the slow pace of work itself, because it forced him to abandon his plan to treat the coral theory as a subordinate part of the geology of the voyage. As the planned scope of his first book narrowed from a broad geological treatise to a work on the single topic of coral reefs, then, it seems that his intended coverage of coral reefs expanded to justify this monographic treatment. Most importantly, however, Darwin was haunted by the expectations that had been generated by the over-eager speculations of his 1837 coral paper.

As he worked on the topic through bouts of illness in 1838 and 1839, Darwin was almost infatuated by the ambitiousness of his earlier conclusions and the forbidding task of justifying them. He was sickened by the thought of even looking at the 1837 paper, and the longer he waited, the more he fetishized his earlier speculations. It was not until February 1840 that further probing from Lyell, who was still revising the *Principles*, forced him to reread it. Although there is no record of Lyell's letter, it

211. See Gruber, *Darwin on Man*, 3–4.

212. Charles Darwin, "Observations on the Parallel Roads of Glen Roy, and of Other Parts of Lochaber in Scotland, with an Attempt to Prove That They Are of Marine Origin," *Philosophical Transactions of the Royal Society* (1839): 39–81 Reprinted in Paul H. Barrett, *Collected Papers*, 89–137, quotation on p. 128. On Darwin's reasoning in the Glen Roy paper, see Rudwick, "Darwin and Glen Roy".

appears that he had despaired of receiving Darwin's book on coral reefs and had asked whether to cite the original publication. Darwin had to admit that despite having "set [his] heart" upon having the coral book completed before Lyell's new edition, Lyell was justified in his pessimism.[213] Having no choice but to look back and remind himself what he had written, Darwin reviewed the version published in the *Journal of Researches* and offered Lyell a list of the "two or three points, which will be different in my volume." Darwin put on a brave face and reported that "I find I am prepared to stand by almost everything. -- it is much more cautiously & accurately written, than I thought." Yet his list of revisions indicated quite the contrary. Darwin had in fact adopted a much more conservative position than he had held in 1837.

This preview of the forthcoming coral reef volume revealed that Darwin had abandoned many of his most ambitious conjectures, and was now almost exclusively oriented toward bolstering the claim that certain kinds of reefs had formed in areas of subsidence. He had been informed that coral reefs in the Red Sea lived deeper than previously believed, but he assured Lyell that "[t]he argument...that there must have been subsidence in the large areas, scattered with reefs, stands firm." After working hard at his reef map, he placed greater weight on the distribution of coral islands, explaining that his subsidence theory would hold, "even should coral-reefs be hereafter found to live at much greater depths [than] I suppose; for I find the areas are immense in which every island is low, & of coral-formation." In a little-noticed effort to solidify the subsidence theory, he had also refined his "classification of reefs." The first change was one of nomenclature. Darwin resolved to called his first class of reefs "atolls" instead of "lagoon islands," adapting the native name for the annular island groups of the Maldives.[214] Secondly, he combined encircling reefs and barrier reefs into a single

213. Darwin to Lyell, [19 February 1840]. Burkhardt et al., *CCD*, vol. 2, 253–54.
214. For earlier uses of "atoll" as a geographically specific term, see James Horsburgh and W.F.W. Owen, "Some Remarks Relative to the Geography of the Maldiva Islands, and the Navigable Channels (at

class. The significance of this step should not be overlooked, because it indicates that Darwin's taxonomy was no longer based on the shape of reefs, but on their proposed mode of formation. Without Darwin's theory, there was little to justify placing the annular reefs of the Society Islands and the long, straight barrier of Australia into the same group. Finally, in a stoic understatement, he told Lyell that "I shall have only very slightly to modify my general conclusions." In fact, he was retreating from what had been the paper's most strongly worded plaudits for Lyell's *Principles*. Thus, he admitted that he would be "speaking rather less positively -- & using the words alternate areas more frequently than 'parallel bands,'" and he confessed that he would "not be able to throw any light on [the] distribution of organic forms in the Pacific as [he] had hoped."[215]

This letter shows just how much Darwin's writing project had changed in less than three years. As he narrowed the scope of his geology book to include only coral reefs, he was being forced to consider abandoning the arguments that had prompted Lyell and Darwin to make coral reefs the centerpiece of his geological career in the first place.

Coral reef formation and Lyell's new *Principles*

Like the *Elements* before it, Lyell's new edition of the *Principles* was indebted to Darwin's private contributions when it was published in the summer of 1840. Once again Lyell's preface offered a generous acknowledgement to Darwin, and it explained

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Present Known to Europeans) Which Separate the Atolls from Each Other," *Journal of the Royal Geographical Society of London* 2 (1832): 72–92; [Robert Moresby], "Extracts from Commander Moresby's Report on the Northern Atolls of the Maldivas," *Journal of the Royal Geographical Society of London* 5 (1835): 398–404. In his 1842 book, Darwin followed Owen in quoting the seventeenth century account of François Pyrard de Laval on the "atollons" of the Maldivas. See Owen, pp. 83-88 and Charles Darwin, *The Structure and Distribution of Coral Reefs*, 2.

215. Darwin to Lyell, [19 February 1840]. Burkhardt et al., *CCD*, vol. 2, 253–54.

that the latter's "new views...have induced me to renounce the hypothesis which I formerly advocated, that [circular] reefs were based on submerged volcanic craters."[216] As he had forecast the previous year, Lyell employed Darwin's reef observations to expose Elie de Beaumont's "faulty induction" about the date and speed at which the Chalk had been uplifted by the formation of the Pyrenees. He also countered the Frenchman by arguing that "all the existing continents and submarine abysses" could have been formed by gradual movements comparable to the subsidence known to be occurring in "parts of the Pacific and Indian oceans, in which atolls or circular coral islands abound."[217] Whereas Darwin had asked to be quoted "with great caution" in the chapter on Elie de Beaumont, Lyell opted not to mention the young man's name at all. Although these passages alluded to Darwin's "Connexion" paper, and referred to the elevation of South America using words repeated from Lyell's first congratulatory letter to Darwin, in addition to discussing the formation of coral reefs, this chapter did not contain a single citation to Darwin's work.[218]

Lyell's revised chapter on coral reefs, on the other hand, proceeded not only to cite Darwin's forthcoming work, but to strike off independently from it. This chapter was double the twenty-page length of its counterpart from the first edition, and the title was changed from "Corals and Coral Reefs" to "Formation of Coral Reefs." This subtle modification reflected the fact that Lyell now placed a much greater emphasis on the mutability of reef structures and the causes thereof. What was not subtle, however, was the way that Lyell declared his own important role in the development of the subsidence theory. After "abandon[ing]" his submarine volcano theory (but not without explaining

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216. Charles Lyell, *Principles of Geology*, (3 vols) 6th ed (London: John Murray, 1840), vol. 1, xii.
217. Charles Lyell, *Principles of Geology* (6th Ed), vol. 1, 309–12.
218. Charles Lyell, *Principles of Geology* (6th Ed) Compare vol. 1, page 315 to Darwin's "Connexion" paper, and vol. 1, page 314 to Lyell to Darwin, 26 December 1836. Burkhardt et al., *CCD*, vol. 1, 532–33.

all the good reasons why "it was formerly embraced"[219]) he described Darwin's "new opinion" on the formation of reefs. He then rehearsed the evidence that had been presented in its favor in Darwin's Geological Society paper. But after giving this supportive rendering of Darwin's work, Lyell made a breathtakingly explicit declaration of his own priority in this area. Ironically, this statement employed the term "atoll" in the generic sense that Darwin had proposed in their private correspondence that spring:[220]

> When the first edition of this work appeared in 1831, several years before Mr. Darwin had investigated the facts on which his theory is founded, I had come to the opinion that the land was subsiding at the bottom of those parts of the Pacific where atolls are numerous, although I failed to perceive that such a subsidence, if conceded, would equally solve the enigma as to the form both of annular and barrier reefs.[221]

Lyell proceeded to give three full pages of quotations from his own first edition that he claimed would support this contention. To be precise, however, what they showed was that he had previously surmised that the amount of geologically recent subsidence in the Pacific appeared to have exceeded the amount of elevation over the same period.[222] The selected quotations emphasized "alternate elevation and depression of the same mass," and not--as he now implied--a prevailing "downward movement in the bed of the ocean."[223] He evidently could not help reading and remembering his own past words in

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219. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 379.
220. Lyell also added the term "atolls" to his glossary for this edition of the *Principles*, defining them as "Coral islands of an annular form, or consisting of a circular strip or ring of coral surrounding a central lagoon." (Neither "atoll" nor "lagoon island" appeared in the glossary to the first edition). Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 409.
221. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 385. In the first edition, Lyell had only used the term "atoll" in its geographically limited sense applying to the Maldives. See Charles Lyell, *Principles of Geology* (1st Ed), vol. 2, 286.
222. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 385–90. The extracted sections may be found (in the order presented in the sixth edition) in Charles Lyell, *Principles of Geology* (1st Ed), vol. 2, 296, 293, and 293–94.
223. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 388.

light of the new theory.[224] Lyell went on to explain the "important generalization" that Darwin had derived by correlating reef forms with submarine movement, namely that the globe "might be divided into areas of elevation and subsidence, which occur alternately."[225] Given the geological and zoological significance of such an observation, and knowing Lyell's role in shepherding it to prominence, it is not difficult to see why Lyell wanted to claim the credit he believed he was due.[226]

But with Darwin's full treatment of the coral reef theory unpublished, and indeed unavailable for perusal, Lyell decided not to stop there. "Having laid before the reader this brief analysis of Mr. Darwin's theory," he declared, "I shall next endeavour to trace out some of the other natural consequences to which it appears to me to lead."[227] In a footnote that sounds almost scolding in light of his many private appeals for Darwin to finish drafting his book, he added, "I know not how far the conclusions deduced in the remainder of this chapter may agree with those at which Mr. Darwin has arrived, and which he will explain in detail in his forthcoming work on Coral Formations."[228] In the course of nine pages, Lyell went on to analyze the structure of

224. For a similar example of an anachronistic, theory-laden self reading, see my discussion of Darwin's *Autobiography* account of the genesis of the coral theory in chapter 2. A good discussion of this phenomenon may be found in the closing chapters of Secord, *Controversy in Victorian Geology*.

225. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 391.

226. In his March 1842 lecture on coral reefs at the Broadway Tabernacle in New York, Lyell offered a more graceful (if slightly self-contradictory) justfication for pointing out his former comments, namely that they provided independent support for Darwin's theory. "I may as well mention," he explained, "that [the] theory of subsidence was not invented for the purpose of explaining these phenomena [i.e., the shape of coral islands]. Long before Darwin had made his examinations of these coral islands...I published my opinion upon this point, that the sinking down of the Pacific *might be in excess*: that its depression might be greater than its upheaval. [...] The theory [of Pacific subsidence], then, was not made for the purpose of fitting the facts [of reef shapes] -- though it is a perfectly legitimate reason for adopting a theory that you find it will explain all the known phenomena which no other theory will explain. -- Still, it is somewhat more satisfactory if the principle was not formed expressly to suit the facts of the case." Charles] [Lyell, "Mr. Lyell's Fourth Lecture on Geology," *New-York Tribune*, 28 and 29 March 1842. For a discussion of public science in New York in the first half of the nineteenth century, see D. Graham Burnett, *Trying Leviathan: The Nineteenth-Century New York Court Case That Put the Whale on Trial and Challenged the Order of Nature* (Princeton: Princeton University Press, 2007).

227. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 391–92.

228. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 392, footnote.

reefs that might be produced on an island undergoing different types of submarine movement. With the aid of woodcut diagrams he illustrated, for example, that intermittent episodes of rapid subsidence would produce a much smaller atoll than uniformly slow subsidence of the same island.[229] For this reason, he explained, it would be impossible to estimate the dimensions of an atoll's base from its circumference at the surface. He concluded that it was also impossible to "calculate...what may have been the height of [an] island now changed into an atoll," or to "estimate the thickness of coral with has accumulated."[230] Yet by considering the amount of subsidence that would be necessary to submerge the highest points of present oceanic islands like the Canaries, he ventured to guess that if the Pacific were laid dry it would reveal mountains capped with calcareous formations that were as much as ten or eleven thousand feet thick. "Thus," he pointed out in a silent reference to the debate over the origins of the Chalk, "a recent cretaceous formation may now be in progress in many parts of the Pacific and Indian oceans."[231]

There is no evidence of Darwin's reaction to Lyell's incursion into coral reef territory. It may well be that he found it quite demoralizing, because he abruptly ceased his own work on the topic. He had been writing exclusively on coral reefs from 26 March 1840 until sometime in the summer. When he returned to the manuscript over a year later, on 26 July 1841, he noted that it was the first time he had done so in thirteen months.[232] Counting back shows that the moment he had abandoned the project coincided with Lyell's publication date of June 1840. On the other hand, it also coincided with his summer holiday to visit relatives on both sides of the family. He was also in generally poor health throughout this period, although he continued to make

229. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 392–95.
230. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 398.
231. Charles Lyell, *Principles of Geology* (6th Ed), vol. 3, 400.
232. See entries in Darwin's journal, quoted in Burkhardt et al., *CCD*, vol. 2, 434.

progress on the zoology of the voyage and he wrote a paper for the Geological Society on the distribution of erratic boulders. He was also preoccupied with his private notekeeping on the species question.

Not surprisingly, it was Lyell himself who dragged Darwin back to coral reefs. He and his wife had made arrangments for a long trip to the United States, where Lyell would give public lectures on geology in Boston, Philadelphia, and New York.[233] He intended one of the lectures in each series to be on fossil and living coral reefs, for which the recent revision of the *Principles* provided plenty of material.[234] In order to illustrate the alternate bands of elevation and subsidence, he wished to display a map similar to the one Darwin had shown when delivering his 1837 paper to the Geological Society. With this in mind Darwin deposited his color coded charts with Lyell and left London in late May 1841 to make a palliative visit to the countryside.[235] In the month before his own mid-July departure for the U.S.A., Lyell colored a coral reef map based on Darwin's charts so that he might include it among the outsize diagrams and painted landscapes with which he planned to illustrate each of his lectures.[236]

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233. For an overview of Lyell's American lectures, see Robert H. Dott, Jr., "Charles Lyell in America--His Lectures, Field Work, and Mutual Influences, 1841–1853," *Earth Sciences History* 15 (1996): 101–40. On Lyell's travels in the United States and his life in the 1840s, see Leonard G. Wilson, *Lyell in America: Transatlantic Geology, 1841–1853* (Baltimore: Johns Hopkins University Press, 1998).

234. Like the *Elements* and the *Principles*, this lecture was to discuss fossil and living coral reefs. In a field trip to Wales in June 1841, the month before his scheduled departure, Lyell saw Silurian beds containing fossil corals in their position of growth. He believed that these formations, much older than the Chalk, had also been formed by subsidence similar to what Darwin had demonstrated. Darwin responded with constructive criticism for this view in his letter to Lyell of 6 [July 1841]. Burkhardt et al., *CCD*, vol. 2, 297–99 See also Leonard G. Wilson, *Charles Lyell: The Years to 1841*, 516.

235. Lyell informed Darwin that his clerk would be returning the "charts on Coral reefs you kindly lent me" in a letter written on the eve of his departure for America, *c*. 16 July 1841. Burkhardt et al., *CCD*, vol. 2, 299–300.

236. All the lectures were to be illustrated with large views and sectional diagrams, as had been the ones he delivered during his short tenure in the geology chair at Kings College. Some of the picture used in the 1841-1842 lectures were painted in the United States to illustrate examples from Lyell's recent fieldwork there. On Lyell's teaching illustrations see Dott, "Charles Lyell in America--His Lectures, Field Work, and Mutual Influences, 1841–1853," especially 105–6, and Rudwick, *Worlds Before Adam*, 362–64. On geological illustrations of the period, see Martin J.S. Rudwick, "The Emergence

At this stage there remained several reefs that Darwin had not yet classified, namely the problematic formations of the Red Sea, the West Indies, and Bermuda. While at his father's house in Shrewsbury, Darwin received a letter from Lyell suggesting the possibility of using a "neutral tint" to demarcate these known but unclassifiable reefs. Apologizing that "I can give you no precise information without my notes (even if then)," Darwin nevertheless proceeded to provide Lyell with detailed analyses from memory of his research on all three locations, elaborating as his recollections became clearer. He discouraged Lyell from prematurely adding a code-color for anomalous reefs, explaining that he did not consider them to be ultimately unclassifiable. "I advise you to leave the Red Sea quite uncoloured," he advised, "for I have not yet considered all the data I have collected." Nor had he "finally considered [his] portfolio of notes on the West Indies."[237]

Besides illustrating Darwin's practice of collating facts for his coral book by region, the letter also revealed his continued ambivalence about the proper venue for theorizing. After offering Lyell a conjectural history of the Red Sea reefs that featured complex local oscillations, denudation, and coral growth that "will I believe make Ehrenbergs, Moresby's & other accounts all harmonize," Darwin admitted that he was unwilling to introduce it in his manuscript. "I doubt whether I shall make any allusion to this view [in the coral reef book], as it will appear so hypothetical -- though to you & your pupils, as a mere theoretical case, it might have been expected to have somewhere occurred."[238] This confession is striking because it confirms that Darwin's aversion was not to private hypothesizing, but to *appearing* hypothetical in his book.

Darwin's unwillingness to publicize these views might be seen as a means of putting space between himself and Lyell. The slightly distant reference to "your pupils,"

of a Visual Language for Geological Science, 1760–1840," *History of Science* 14 (1976): 149–95.
237. Darwin to Lyell, 6 [July 1841]. Burkhardt et al., *CCD*, vol. 2, 297–99.
238. Darwin to Lyell, 6 [July 1841]. Burkhardt et al., *CCD*, vol. 2, 297–99.

moreover, was a marked change from Darwin's former eagerness to describe *himself* as Lyell's subordinate and student.[239] The letter closed on a valedictory note, bidding the Lyells a safe trip to America and offering "my warm thanks for all the friendship you have shown me." The tone was apt, because the Darwins had themselves decided to move away from London, not for a holiday but for a quieter home outside town. Thus when Lyell departed for his year abroad in the summer of 1841, it marked the breaking point of what had been nearly five years of intensive personal collaboration and mutual inspiration. Although their friendship and correspondence were to remain vigorious throughout their lives, the relationship between master and student was replaced by one between colleagues. That Darwin reached his independent standing among Britain's geological elite by way of Lyell's patronage and mentorship was never forgotten.

Darwin's colored charts were awaiting him when he got back to London from his convalescence in Shrewsbury. Maybe he felt unbound by Lyell's departure. Perhaps his thoughts had been stimulated by having had to respond to Lyell's questions without digging into the minutiae of his notes. For some reason, he resumed his coral reef work immediately and he did not stop until he had finished writing his book.

Darwin's 1842 *Structure and Distribution of Coral Reefs*

The Structure and Distribution of Coral Reefs was an epic compilation of geographical facts, zoological and hydrographical information on coral growth, and natives' knowledge of the history of particular corals and reefs. It contained a vigorous argument in favor of Darwin's explanation for the form of coral reefs. As I shall

239. For Darwin's former adulation at its most colorful, see his letter to Lyell of [14] September [1838]. On Lyell's stated "hope" that the *Principles* would stand the test of time, for example, Darwin responded "[B]egin to hope,; why the possibility of a doubt has never crossed my mind for many a long day: this may be very unphilosophical, but my geological salvation is staked on it." Burkhardt et al., *CCD*, vol. 2, 104–8.

illustrate below, Darwin not only advanced this causal theory of reef formation explicitly, he also used it to order his presentation of the material that was ostensibly factual and untheoretical. Yet the argument was circumscribed to rule out the topics of almost all the wider speculations that had troubled Darwin ever since he included them in his 1837 paper.

In his brief introduction, Darwin laid out three objectives for the book. He would (i) describe all kinds of reefs, with particular emphasis on those in the open ocean, and (ii) explain the origin of their forms. He promised to address not only the widely known puzzle of lagoon-island formation, but also to explain the equally puzzling, but little remarked, barrier reefs. Finally, he would (iii) examine whether geographical facts supported his "theory of their origin." Roughly the first third of the book was devoted to a chapter on each of the three classes of reefs found in his newly revised taxonomy: "Atolls or Lagoon-Islands;" "Barrier-Reefs;" and "Fringing or Shore Reefs." The fourth chapter covered the growth of corals and the distribution of coral reefs. In chapter five he offered a theory to explain the form of all classes of reefs, and in chapter six he revisited the topic of reef distribution, this time "with reference to the theory of their formation." This final chapter was, in effect, a thematic discussion of the color-coded map of the world that was inserted into the book. In a single appendix, which was half as long by itself as the entire rest of the book, Darwin explained the reasoning that lay behind his classification of each reef, as colored on the map, and revealed the individuals and publications he had consulted in settling each case.[240]

240. Because of its closely-set type, the appendix is even longer, in comparison to the rest of the book, than it appears when consulting the table of contents. I performed an electronic word count of the book based on the online copy at www.darwin-online.org.uk, which is transcribed from the 1842 London first edition. The text of the appendix is about 25,000 words, while the main text of the book, from the beginning of the introduction to the end of chapter 6, is roughly 54,000 words. Chapter 6 is 12,000 words, meaning that the total proportion of the book devoted to discussion of the map is about 47 percent: 37,000 of 79,000.

Chapters one to four, then, constituted a natural history of coral reefs. The first and third chapters began with detailed descriptions of reefs that he considered exemplary of their respective classes. In each case they were the only one of the sort that he had seen himself, namely the atoll of Keeling and the fringing reef at Mauritius. His information on the outline and composition of these reefs was based largely on his leaping-pole aided traverses and his examination of the matter imprinted onto the armed sounding leads. Darwin's rendering of the barrier (formerly called encircling) reef at Tahiti in chapter two was comparatively sketchy, reflecting not only the shortness of time he had spent on his examination there, but also the superficiality of this field work. From his mountain vantage point in Tahiti he had acquired, literally, an overview of the reefs, but he had not had access to soundings there and his only knowledge of the outer margin came from natives' descriptions. Thus the uneven accounts in his book mimicked the evolution of his methods of reef study as the voyage visited each of the three types of reef. Each of the first three chapters expanded outward from these exemplars to consider the general form of each type of reef and to describe especially noteworthy or problematic examples, such as the dissevered atolls of the Maldives. These discussions revealed Darwin's heavy debt to the charts and descriptions of other travelers. In chapter one, for instance, Darwin cited fourteen authors or correspondents (including one European resident of an atoll) in his descriptions of thirty-four named reefs and five groups of coral islands.[241]

241. The individual reefs were (in Darwin's spellings) North and South Keeling, Vliegen, Bow, Clermont Tonnere, Rimsky Korsacoff, Menchicoff, Caroline, Oulleay, Namonouito, Peros Banhos, Great Chagos Bank, Diego Garcia, Elizabeth, Egmont, Cardoo, Ducie's, the Australian barrier, the Red Sea reefs, Bermuda, Gambier, Vanikoro, Matilda, Whitsunday, Romanzoff, Milla dou-Madou, Suadiva, Mahlos Madou, North and South Nillandou, Male, Powell's, Horsburgh, and Heawandoo Pholo. The groups described collectively (of which some of the above are part) were the Low Archipelago, the Marshall Islands, the Carolines, the Maldives, and the Chagos group. The sources named in chapter one were Liesk (the resident at South Keeling), Beechey, Kotzebue, Chamisso, Nelson, Lütke, F. Bennett, Freycinet, Cook, Powell, Moresby, Prentice, Flinders, and Ehrenberg. He also mentioned the Cape Verde Islands and cited Lyell on the likelihood of reef channels becoming obstructed. Charles Darwin, *The Structure and Distribution of Coral Reefs*, chapter 1.

Darwin's **chapter four** surveyed the distribution and accumulation of coral rock in three dimensions. He began by describing the general distribution of coral reefs across the surface of the globe, concluding that there was no simple rule, for example relating to the variance in water temperature or the quantity of carbonate of lime dissolved in seawater, that could explain why some areas of the tropical oceans contained reefs and others did not.[242] He then turned to the distribution of corals upon reefs, arguing against Quoy and Gaimard's claim that stony corals flourished only where the water was calm. "This statement has passed from one geological work to another," Darwin complained, even though "the protection of the whole reef undoubtedly is due to those kinds of coral, which cannot exist in the situations thought by these naturalists to be most favourable to them." The problem, according to Darwin, lay in the tendency to confuse the diversity of a location's coral species with the vigor and structural strength of the corals themselves: "If the question had been, under what conditions the greater number of *species* of coral, not regarding their bulk or strength, were developed, I should answer, -- probably in the situations described by MM. Quoy and Gaimard."[243]

Building from this critique of Quoy and Gaimard, who were almost certainly the most widely cited experts on reef building corals, Darwin argued the need for a more complex understanding of the relations between these organisms. "In the vegetable kingdom every different station has its peculiar group of plants," he explained, in a silent reference to the works of de Candolle and Humboldt, "and similar relations appear to prevail with corals."[244] He spent several pages describing the living economy of a coral reef and explaining that different "zones" on the Keeling reef were

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242. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 60–63. In the text and a long footnote (p. 61) Darwin described his belief that cold water currents determined the absence of reefs at the Galapagos (see chapter two of this dissertation), but he explained that this view could not account for the lack of coral reefs in many other districts.

243. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 65.

244. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 67.

characterized by their own particular kinds of corals and algae. He argued that the way reefs responded to a disturbance, for example an earthquake that caused them to subside, might be determined by the happenstance of which groups of corals prevailed in the new conditions. "In an old-standing reef, the corals which are so different in kind on different parts of it, are probably all adapted to the stations they occupy, and hold their places, like other organic beings, by a struggle one with another, and with external nature." If Keeling atoll were to subside by just a few feet, however, "[t]he Nulliporae [that] are now encroaching on the Porites and Millepora" would find "that the latter would, in their turn, encroach upon the Nulliporae."[245] Depending on the characteristic bulk and growth rates of the genera that prevailed on a given reef, it might regain the surface or it might languish below water because it was "covered with luxuriant coral[s], [that] have no tendency to grow upwards."[246] Darwin pointed out that just these contingencies might explain why, despite occupying apparently identical physical conditions, some living atolls of the Indian Ocean remained several fathoms beneath the surface, while others grew right up to sea level.[247] Such differentiae might also explain the wildly varying estimates that had been offered by different authors for the rate at which corals grew and reef rock accumulated. Here he reviewed the evidence presented by other voyagers--who had collected it by direct observation, comparisons with earlier surveys, and interrogation of long-lived natives--and gathered that while local conditions often precluded it, it was possible for reefs to grow rapidly in comparison to "the average oscillations of level in the earth's crust."[248]

245. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 76.
246. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 69.
247. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 67–71.
248. "[I]t may be concluded, first, that considerable thicknesses of rock have certainly been formed within the present geological aera by the growth of coral and the accumulation of its detritus; and, secondly, that the increase of individual corals and of reefs, both outwards or horizontally and upwards or vertically, under the peculiar conditions favourable to such increase, is not slow, when referred either to the standard of the average oscillations of level in the earth's crust, or to the more precise but less important one of a cycle of years." Charles Darwin, *The Structure and Distribution of Coral*

He closed the chapter by reiterating and expanding another point whose significance would be revealed when he explained his theory in the following chapter, namely that the growth of reef building corals was constrained by water depth. Reviewing *particular* details offered by navigators Moresby, Wellstead, King, Beechey, and Stokes, along with those of the naturalists Ehrenberg and Couthouy, he quibbled with the *general* depth limit of thirty feet that had been given by Quoy and Gaimard.[249] Relying on the soundings he had carried out himself at increasing depths from the coast of Mauritius (see chapter 2), he argued that this systematic inquiry extablished a trend, of reef building corals disappearing below twenty fathoms (120 feet) or so, that was more important than the exact depth where it happened. He again pointed out the analogy with terrestrial plants struggling to maintain their stations, as (his readers would know) had been described by de Candolle.

> The circumstance of a *gradual* change...from a field of clean coral to a smooth sandy bottom, is far more important in indicating the depth at which the larger kinds of coral flourish, than almost any number of separate observations on the depth, at which certain species have been dredged up. For we can understand the gradation, only as a prolonged struggle against unfavourable conditions. If a person were to find the soil clothed with turf on the banks of a stream of water, but on going to some distance on one side of it, he observed the blades of grass growing thinner and thinner, with intervening patches of sand, until he entered a desert of sand, he would safely conclude, especially if changes of the same kind were noticed in other places, that the presence of the water was absolutely necessary to the formation of a thick bed of turf: so may we conclude, with the same feeling of certainty, that thick beds of coral are formed only at small depths beneath the surface of the sea.[250]

Reefs, 79.

249. It is a small but interesting point that Darwin was able to rely on information from the United States Exploring Expedition (1838-1842) before it had even ended. Joseph Couthouy, the erstwhile conchologist to the expedition who had been sent home by Commander Wilkes, learned of Darwin's coral theory from Lyell's lectures in Boston in the fall of 1841. This prompted Couthouy to send Darwin a paper containing his observations on the Pacific reefs, which the latter mentioned in footnotes added after the publishers had already begun printing the book. I will discuss Couthouy at greater length in chapter 4.

250. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 82.

"I have endeavoured to collect every fact," he assured his readers, "which might either invalidate or corroborate this conclusion."[251]

In **chapter five**, Darwin offered a theory to explain all the foregoing facts. Having primed his readers with his version of the natural history of coral reefs, he was able to dismiss his predecessors' theories of atoll formation in the space of a page.[252] Having also delivered some of his most important evidence in the descriptive chapters, he was able to introduce his own alternative as an inescapable consequence of the facts of coral growth and of reef distribution:

> What cause, then, has given atolls and barrier-reefs their characteristic forms? Let us see whether an important deduction will not follow from the consideration of these two circumstances, -- first, the reef-building corals flourishing only at limited depths, -- and secondly, the vastness of the areas interspersed with coral-reefs and coral-islets[.][253]

What, he asked, could provide the foundations for the atolls that formed the great archipelagoes of the Low Islands, the Gilberts, the Marshalls, the Carolines, and the Laccadives? He began to eliminate alternative explanations: banks of sediment; chains of broad-summited mountains that all reached within 180 feet of the surface; and the leveling-off of individual mountains by waves as they were each elevated close to the surface.[254] There was only one remaining possibility: "If, then, the foundations of the many atolls were not uplifted into the requisite position, they must of necessity have subsided into it; and this at once solves every difficulty[.]"[255]

251. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 82.
252. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 89.
253. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 90.
254. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 92–94. In the long footnote on p. 94, he also pointed out what he considered a particularly absurd shortcoming of Forster's theory that corals formed annular reefs by instinct: "According to this latter view, the corals on the outer margin of the reef instinctively expose themselves to the surf in order to afford protection to corals living in the lagoon, which belong to other genera, and to other families!"
255. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 94.

As he had done in 1835 and 1837, Darwin asked his audience to imagine what would happen to a reef-fringed island as coral growth kept pace with a subsiding foundation. Whereas in the *Journal of Researches* he had admitted that "without the aid of sections [such as he had shown when he read the paper to the Geological Society] it is not very easy to follow out the result," in the 1842 book he based his entire discussion on a pair of carefully designed sectional diagrams included as woodcuts on the same page as the text.[256] The first showed the transition from a fringing reef to an encircling barrier reef, and the second showed the transition from that barrier reef into an atoll.[257] The rhetorical key to these diagrams was that the barrier reef stage depicted was not, in fact, imaginary. Rather, it was a vertical section of the recently-surveyed island of Bolabola [Bora Bora] in the Society Islands. Thus the conjectural fringing reef and atoll stages were revealed to be just short steps in either direction from the actual conditions of a typical reef-encircled island. This point was driven home by another graphical technique presented in Plate 1 of the volume, entitled "Shewing the resemblance in form between barrier coral-reefs surrounding mountainous islands, and atolls or lagoon-islands." Here he juxtaposed charts (i.e., views from overhead) of reef-encircled islands with those of similarly-shaped atolls. In all ten cases shown on this plate, the reef was tinted orange while the high land was only hatched in black. The result was to draw the viewer's attention to the shape of the reefs, which illustrated the striking parallel between the form of barrier reefs and atoll reefs, and to suggest that the islands that the barrier reefs surrounded were ephemeral. This helped him to conclude that "the close similarity in form, dimensions, structure, and relative position...between fringing reefs and encircling barrier-reefs, and between these latter and atolls, is the necessary result of the transformation, during subsidence, of the one class into the other."[258]

256. Charles Darwin, *Journal of Researches*, 558.
257. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 98, 100.
258. Charles Darwin, *The Structure and Distribution of Coral Reefs,* 102.

Of the evidence that Darwin presented in support of the possibility that subsidence could occur on such a vast scale, much was new, or newly applied to the coral reef question, since 1837. Chief among the new material was a series of observations indicating that the sea had transgressed on present day atolls (presumably due to subsidence), some drawn from local traditions reported by Williams and FitzRoy, others showing that the form of islands had been modified between successive visits of European voyagers.[259] He used the remainder of chapter five to address a series of special cases that might present problems for his theory. These irregularly shaped islands and degraded reefs were all explicable, in Darwin's view, by "the action of...nicely balanced forces during a progressive subsidence...like that [implied] by our theory" and simply "modified by occasional accidents which might have been anticipated as probable."[260] Thus he claimed that every known reef in the world, even those that "differ[ed] from the *type* of the class to which they belong[ed]" could be "included in our theory."[261]

Chapter six contained an analysis of the third, and most compelling, piece of graphical evidence, the large distribution map that unfolded from the front of the book. The base map was copied from the 1835 "Carte Hydrographique des parties connues de la terre," a Mercator projection by the Frenchman C.L. Gressier.[262] The version presented by Darwin showed the 270 degrees of longitude that encompassed the Indian and Pacific oceans and the Caribbean, from 30º east of Greenwich to 60º west of Greenwich, with the longitudes explicitly adapted to the English standard. As the map

259. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 95–98.
260. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 103–13, quotations from pp. 109 and 114.
261. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 114 Italics added.
262. A marked up copy of Gressier's map hangs in the "old study" at Down House, Kent (English Heritage provenance number 88202837). Given his close connections with Francis Beaufort at the British Hydrographical Office, it is curious that Darwin chose to use a French hydrographic map. I am not sure why he did so.

legend indicated, the locations of reefs had been marked and color coded according to their classification. Because atolls and barrier reefs were indistinguishable from each other "as far as the actual coral-formation is concerned," they shared the color blue, with atolls being marked in a darker tint and barrier reefs denoted by pale blue. Fringing reefs, on the other hand, were colored red. Thus the map was meant to call attention to the division between "two great types of structure:" the coral reefs whose foundations lay within the possible depth of coral growth (colored red) versus those whose foundations were believed to lie below that depth limit (colored in one or the other shade of blue). Along with the reefs, the location of active volcanoes was indicated by vermilion spots.

If Darwin's subsidence theory were true, however, then the red and blue tints held a deeper meaning. Reefs colored blue had subsided during the time since corals began to grow, and those colored red had either stayed stationary or been elevated. Analyzing the "grouping" of different colored reefs, he pointed out that the blues and reds were "not indiscriminately mixed together."[264] Atolls were often seen clustered amongst themselves, as were barrier reefs; these types were also found in close proximity to one another, which "would be the natural result of both having been produced during the subsidence of the areas in which they stand." Only in rare instances were red and blue dots found close together on the map, and in these areas Darwin believed there was evidence of "oscillations of level," which was the term he used for *relatively* brief episodes of alternating uplift and subsidence. Darwin treated this apparent orderliness in the distribution of reefs, the systematic segregation between the "two great types of structure" represented by blue/subsiding and red/not subsiding areas, as a prediction made by his theory. He deemed the patterns on the map to be proof that the subsidence explanation was correct. "[T]he grouping of the different

264. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 124.

kinds of coral-islands and reefs," he was thus prepared to argue, "is corroborative of the truth of the theory."[265]

By the 1840s, thematic maps of geology and of plant and animal distribution were well established parts of the naturalist's repertoire. As Jane Camerini has demonstrated, there were many precedents for Darwin's use of this technique, though applying it to the distribution of coral reefs was novel. Like other creators of thematic maps, Darwin relied on the existence of satisfactory base maps on which to plot information. As Camerini shows in her examination of what she calls Darwin's "visual thinking," by which she means cartographical or geographical thinking in particular, Darwin often referred to imagined "mental maps" as he formulated, tested, and presented his theories of coral reef formation and the origin of species.[266] He also, of course, marked up maps for his own use, as well as publishing a handful of maps to accompany his *Beagle* works. However, Camerini's concern with visual *thinking* is the grounds for her disagreement with claims by David Stoddart and Martin Rudwick that Darwin was relatively non-visual. Whereas Stoddart has argued that Darwin was "the least cartographic of men," Camerini argues that the many verbal references to maps and distribution in Darwin's notes and publications count as visual thinking.[267] She is therefore arguing a slightly different point from Stoddart, who was concerned with the fact that Darwin rarely *made* maps, and that when he did they were rarely

265. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 124.

266. Camerini follows Ghiselin in arguing that "his geological work evinces the same formal properties that characterize his later research" and she argues that one of the primary roles of maps in Darwin's thinking was to help him "make the transition from geology to biogeography." Thus she argues that the distributional and cartographic aspects of the coral theory "provided both practical and cognitive frameworks for his work on biogeography, which in turn formed the basis of his first theory of speciation." One important weakness of this argument is that it glosses over the *simultanaeity* of these developments by ignoring the fact that Darwin worked on the coral reef book and created its map after he had begun his species project. Camerini, "Darwin, Wallace, and Maps," 105.

267. Camerini was reacting to a pre-publication version of David R. Stoddart, "Darwin and the Seeing Eye".

cartographically innovative.[268] As the only distribution map that Darwin ever published, the coral reef map has drawn the attention of scholars, whether it is seen as exceptional (by Stoddart) or as the most obvious manifestation of his deeply ingrained geographical sensibility (by Camerini). What is difficult to determine is the role that this map played for Darwin himself. At various times Camerini refers to it as “the visual representation of his theory,” as “proof of the theory, rather than a stimulus for it,” and as the “consummate test of Darwin’s theory”[269] This ambiguity reflects a tension between the role played by the map in the argument of the book itself, where it was indeed offered as proof of the theory, and the role of the concepts portrayed on this finished map, of the location and relative orientation of coral reefs. Darwin contemplated these matters as early as the 1835 essay, in which he discussed what would be found “[i]n looking at a chart” showing coral islands.

It is worth reflecting for a moment on the way in which the classification of reefs worked to support the theory of their formation. Darwin claimed that his reef taxonomy reflected travelers’ instinctive groupings, and thus implied that these classifications were independent of theory. “Without any distinct intention to classify coral-reefs,” he averred, “most voyagers have spoken of them under the following heads: ‘lagoon-islands,’ or ‘atolls,’ ‘barrier,’ or ‘encircling reefs’ and ‘fringing,’ or ‘shore reefs.’”[270] This may have been literally true, in the sense that each of these six terms had previously been used to describe one reef or another. Yet the order imposed on these terms by Darwin’s theory was already evident: not only did he list these ostensibly natural classes in a sequence suggested by his theory, he also implied that certain terms were straightforwardly synonymous, as with “atoll” and “lagoon island.” Yet both Chamisso and Eschscholtz had been uncertain whether annular reefs of the Indian and

268. Camerini, “Darwin, Wallace, and Maps,” especially 102–3 and 194–95.
269. Camerini, “Darwin, Wallace, and Maps,” 88, 96–97, 101.
270. Charles Darwin, *The Structure and Distribution of Coral Reefs,* 1.

Pacific oceans were the same phenomena (see chapter one of this dissertation). In his second chapter, Darwin admitted that lumping encircling and barrier reefs into a single class was in fact his own taxonomic innovation, one which he justified based on their similarities "in structure, and in position relatively [sic] to the land."[271]

Why was it important for Darwin to imply that his taxonomy of reefs was simply the one that any observer of reefs would give? He had given a taxonomy based on their two-dimensional form and then he showed how this superficial taxonomy compared to an analysis that depended on, in effect, cutting down into the reefs. He thus showed that a true analysis of coral reefs must rely on their deeper structure and on what might be called the physiology of their growth (i.e., the growth of the individual corals). This was how he showed that fringing, barrier, and atoll reefs were essentially identical, and their underlying identity was the basis for his argument that a theory of reef formation must explain all types of reefs. As I showed in chapter 1, Darwin was far from the first person to point out the importance of the sub-surface study of reefs. Yet by accompanying FitzRoy he had been able to do as much as anyone else to pursue this line of inquiry. Presenting a taxonomy based on superficial characteristics, only to undermine it by excavating deeper into the reefs, was a promising strategy in early nineteenth century natural history because it echoed the exemplary anatomical work of Cuvier and Owen. This transition from a focus on superficies to a concern with sub-surface features reflected a shift that had occurred in his own coral reef fieldwork. At the Society Islands, Darwin had an extraordinarily productive overview of reef shapes and their horizontal relation to high land, while he could only speculate about the terrain beneath sea level. In his visits to Keeling and Mauritius, he sought to replace his two-dimensional perspective with a three-dimensional knowledge of the reefs, gained from

271. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 41.

sounding, paddling in the lagoon, and peering into the water at the reef's outer margin.[272]

Curiously, the outcome of this deeper comparison was to show that there were many fundamental similarities between the structures of fringing, barrier, and atoll reefs. Reefs that did not share these features were rejected from his taxonomy altogether. This practice reveals the theory-ladenness of Darwin's reef classifications, and was most evident in a series of statements explaining that there were certain reefs that "resembled" atolls, but were not "true" atolls. For him, it was axiomatic that atolls were surrounded by deep water. A true atoll's structure had to pose the question that his theory had been designed to answer: how could shallow water organisms establish a reef in a location where the ocean floor was too deep for them to live? It was indeed possible, he admitted, that coral growing in shallow seas or around banks of sediment might "sometimes assume, (and this circumstance ought not to be overlooked,) the *appearance of atolls*" if there happened to be "more vigorous growth of coral on the outside."[273] Such reefs had no place in his classification. Indeed in the appendix, discussing three annular reefs that lie off the coast of what is now Belize, he wrote, "these reefs have so completely the form of atolls, that *if they had occurred in the Pacific, I should not have hesitated about colouring them blue*. [...Yet] I consider it more probable that the three foregoing banks are the worn-down bases of upheaved shoals, fringed with corals, than that they are *true atolls, wholly produced by the growth of coral during subsidence*."[274] This desire that all members of a class should share the

272. My interpretation here was stimulated by Graham Burnett's contention that nineteenth-century whalemen practiced a highly specialized natural history of whales and the sea, which was based on "superficial" characteristics visible from the deck or mast of a ship (for example, identifying different cetaceans by the characteristic exhalations from their blowholes). Burnett contrasts this approach to the ethos that prevailed among comparative anatomists of the period, whose taxonomies were based, when possible, on internal features of the animals. See Burnett, *Trying Leviathan*.

273. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 57–58 Emphasis added.

274. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 202. Emphasis added. The reefs were Turneffe, Lighthouse, and Glover. Darwin made the same point about offshore reefs in the Red

same mode of formation was evident in his dismissal of the prior theories of Quoy and Gaimard and Chamisso. Regarding the Frenchmen's crater-rim theory, he explained that "I am far from denying that a reef like a perfectly characterized atoll might not [sic] be formed [atop a submarine crater]; some such, perhaps, now exist; but I cannot believe in the possibility of the *greater number* having thus originated."[275] On Chamisso's (actually Eschscholtz's; see chapter 1) "earlier and better theory" that reefs assume a ring shape because the sturdiest corals flourish on the rims of coral masses, he acknowledged that "I believe some such exist in the West Indies[, b]ut a difficulty of the same kind with that affecting the crater theory, renders...this view inapplicable to the *greater number* of atolls."[276] In his description of the colored map, he took this reasoning a step further and argued that "true atolls" were only those reefs to which his theory applied: "[Even] if I had means of ascertaining the fact, I should not colour a reef merely coating the edges of a submarine crater, or of a level submerged bank [in the color reserved for an atoll]; for such superficial formations differ essentially, even when [they do] not [differ] in external appearance, from reefs whose foundations as well as superficies have been wholly formed by the growth of coral."[277] In other words, if it could be proved that an annular reef had been formed in the manner predicted by the rival theory of atoll formation, then Darwin would consider that reef by definition *not* to be an atoll.

Darwin ended his final chapter by turning to examine the implications of the theory. Rather than using the book as an opportunity to expand on the speculative conclusions offered in his 1837 paper, he narrowed the scope of his closing reflections

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Sea (p. 195), "which approach in structure to the true atolls of the Indian and Pacific Oceans; but they present only imperfect miniature likenesses of them."

275. Emphasis added. I believe the double negative in this sentence was unintended. Charles Darwin, *The Structure and Distribution of Coral Reefs,* 89.

276. Emphasis added. Charles Darwin, *The Structure and Distribution of Coral Reefs,* 89.

277. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 122.

considerably. He addressed two main themes: first was the apparent link between volcanic action and elevation, which he had proposed in the 1837 coral paper and elaborated in the 1838 "Connexion" paper, and which was supported by the absence of active volcanoes in the blue-colored areas of the map. The second theme was the relation between the areas of subsidence and elevation. He pointed out that the portions of the earth's surface that had been raised and lowered were "immense," and he explained that the long and narrow shapes of the blue areas on the map might themselves represent only small proportions of the true geographical extent of subsiding areas. These bands of atolls and barrier reefs potentially reflected the orientation of mountain chains (or parts of mountain chains) that ran across larger areas that had subsided.[278] What he considered "perhaps, the most interesting conclusion in this volume" was that "the whole vast amount of subsidence, necessary to have produced the many atolls widely scattered over imense spaces [had been produced by] movements [that] must either have been uniform and exceedingly slow, or have been effected by small steps, separated from each other by long intervals of time."[279]

Darwin stopped short of examining the ulterior causes of elevation and subsidence. He was willing to characterize movement of the crust, which by Whewell's definition was a proximate cause, but not to explain it. Darwin declined to speculate on the internal matter of the globe, and he spoke of the relation between elevation and subsidence in decidedly measured tones, as when he pointed out that "[a] view of the map will show that, generally, there is a tendency to alternation in the parallel areas undergoing opposite kinds of movement; *as if* the sinking of one area balanced the rising of another."[280] He summed up chapter six by stating that "the subterranean

278. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 143–45.
279. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 145.
280. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 145, emphasis added. In the 1837 paper, cited above, Darwin had said "One is strongly tempted to believe, that as one end of the lever goes up, the other goes down."

changes which have caused some large areas to rise, and others to subside, have acted in a very similar manner," which indicated that elevation and subsidence were only proximate causes of another phenomenon upon which he would not elaborate.[281]

In the three-paragraph "Recapitulation" of his argument, Darwin underscored the way in which his coral theory, and the map that he had developed along with it, could be used as tools for the imagination. "[W]hen the two great types of structure, namely barrier-reefs and atolls on the one hand, and fringing-reefs on the other, were laid down in colours on our map, a magnificent and harmonious picture of the movements, which the crust of the earth has within a late period undergone, is presented to us." Just as he had previously imagined the island of Eimeo in motion, when he looked at his colored reef map he saw this "picture of movements" as though it were animated.

> We there see vast areas rising, with volcanic matter every now and then bursting forth through the vents or fissures with which they are traversed. We see other wide spaces slowly sinking without any volcanic outbursts; and we may feel sure, that this sinking must have been immense in amount as well as in area, thus to have buried over the broad face of the ocean every one of those mountains, above which atolls now stand like monuments, marking the place of their former existence.[282]

Here he signaled the climax of his argument by deploying once again the "monument" phrase that had pleased him since he first entered it in his diary in 1836.

The last sentence of the book declared that his coral theory was the product of a narrowly-motivated attempt to explain the characteristic shapes of coral reefs. It was a claim that would stand in complete contradiction to his later *Autobiography* account of the origin of the coral theory. In the 1842 book, he wrote:

> Reflecting how powerful an agent with respect to denudation, and consequently to the nature and thickness of the deposits in accumulation, the sea must ever be, when acting for prolonged periods on the land, during either its slow emergence or subsidence; reflecting, also, on the final effects of these movements in the interchange of land and ocean-water, on the climate of the earth, and on the

281. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 146.
282. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 147–48.

> distribution of organic beings, I may be permitted to hope, that the conclusions derived from the study of coral-formations, originally attempted merely to explain their peculiar forms, may be thought worthy of the attention of geologists.[283]

This strategic claim, that he had undertaken the explanation of reef forms without any more general geological or zoological problems in mind, was the final thrust of his book-long effort to portray his views on elevation and subsidence as conclusions that were independent of his theory of reef formation. The point was not to give a true accounting of the genesis of his theory, but to avoid the appearance of a circular argument. This closing sentence also made one final allusion to the Lyellian cycle of changes that affected the earth's crust, climate, and organized beings. Last, it made explicit a point that has been one of my main arguments in this chapter: that whatever were the origins of Darwin's theory of coral reef formation, it was presented in 1842 as a contribution to the science of geology.

Response to the 1842 book

With the book finally published, Darwin worked to increase its visibility among men of science and the reading public, with the primary goal of encouraging people to buy it. He requested that Smith and Elder send copies for review to the *Athenaeum,* the *New Edinburgh Philosophical Journal*, and the *Philosophical Magazine*, and he told them that he was sending some of his presentation copies to "foreigners, who I thought by noticing the work, would aid its sale."[284] The other recipients were libraries and "people who had materially aided" Darwin in producing the book. He drafted at least two lists of intended recipients (some of whom may not ultimately have been sent a copy), which serve as useful illustrations of the audience he perceived for the work.[285]

283. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 148.
284. Darwin to Smith, Elder & Co., [17 May 1842].
285. DAR 69:A108.

Among the eminent foreigners he contemplated were his hero Humboldt; the navigator and cartographer Krusenstern; Lyell's two greatest European rivals, the geologists Léonce Élie de Beaumont and Leopold von Buch; and Christian Ehrenberg, the German naturalist and microscopist who had studied the Red Sea reefs. Absent from the lists were Quoy and Gaimard, the French naturalists who had proposed the crater-rim theory, as was Chamisso, who had died in 1838. The institutions to receive the work were the British geographical and geological societies, the Geological Society of Paris, and the "Public Libraries" of Britain. The individuals Darwin planned to reward for their assistance included FitzRoy, Beaufort, Moresby, Beechey, Sedgwick, and Allan. He eventually counted Lyell among those helpers as well, although his name was forgotten on the first draft. These lists suggest that Darwin envisioned geologists as the book's primary audience, while also demonstating the profound debt he felt to practicing hydrographers. Not coincidentally, Beaufort arranged for three copies of the new book to be delivered to the Admiralty Hydrographic Office.[286]

The book attracted two reviews by the end of 1842. One, by the sitting Secretary of the Royal Geographcial Society, Colonel Julian Jackson, was a highly complimentary piece that recommended the work to "the geographer, the navigator, and the *savant*."[287] Significantly, given Darwin's keen desire to avoid being seen as overly speculative, Jackson framed his review around the question of when theories might legitimately be established to describe collections of evidence. He opened by pointing out that while it was conventional "to deprecate the precipitancy with which theories and systems are

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286. A.B. Becher to Darwin, 1 September 1842. UKHO LB10 1841-1842, p. 510.

287. Julian Jackson, "[Review of] The Structure and Distribution of Coral Reefs: Being the First Part of the Geology of the Voyage of the 'Beagle,' Under the Command of Capt. Fitzroy, R.N., During the Years 1832–1836. By Charles Darwin, M.A., F.R.S., F.G.S., Naturalist to the Expedition," *Journal of the Royal Geographical Society of London* 12 (1842): quotation on p. 120. It seems that Jackson was initially undecided whether to place the review in the journal of the RGS or whether to submit it to a popular periodical. Darwin saw more "dignity" in the journal, but believed that a magazine might publish the review more quickly. See Darwin to Julian Jackson, 13 October [1842]. Burkhardt et al., *CCD*, vol. 2, 338–39.

raised upon the insufficient foundation of a few isolated facts," the opposite problem was equally troubling, namely that "an immense number of valuable observations on the most interesting and important subjects remain dispersed, and therefore almost useless, long after there is more than enough from which to deduce some satisfactory conclusion."[288] Jackson believed that Darwin's greatest contribution was in the organization of previously disconnected facts, praising his mastery of other travelers' accounts and arguing that this work of systematization was an adequate warrant for the theoretical conclusions at the end of the book. "From the manner in which he has grouped the facts, and then reasoned upon them, the mind remains satisfied that he has detected the law, or rather the process, of nature in [coral reef] formation." In describing the structure of the book, Jackson commented that in the first four chapters, "Mr. Darwin has confined himself generally to the arrangement and detail of facts." That Jackson, or any reader, should have considered these chapters to be mere description, from which the theory laid out in chapter five emerged, was of course Darwin's goal.

Less easily persuaded was the Scotsman Charles Maclaren, the author of the other review. Maclaren drew attention to the fact that the distinction between barrier and fringing reefs discussed in the second and third chapters, far from being pure description, "has reference chiefly to theoretical considerations."[289] Nevertheless, Maclaren accepted Darwin's premises about the ideal characteristics of theories themselves, concurring that it would be preferable in principle to have a single theory

288. Jackson, "[Review of] The Structure and Distribution of Coral Reefs," 115.

289. Charles Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," *Edinburgh New Philosophical Journal* 34 (1843): quotation on p. 39. This essay was an abridgment of a two-part review that appeared in the periodical that Maclaren edited, the *Scotsman*, on 29 October and 9 November 1842. The full-length text from the *Scotsman*, along with the woodcuts that appeared as illustrations in both versions mentioned above, appeared as a separate pamphlet, an example of which may be seen at the NHM Earth Sciences Library, Murray Collection, Section 15, 19.

that could explain the origin of all types of coral reefs.[290] In laying out Darwin's argument for his readers, Maclaren was also willing to assent that even if the subsidence of reef foundations could not be directly proved, the incorporation of this cause into the theory was yet acceptable, and "involves no inconsistency," because "Geology...renders it certain that some portions of the earth's surface have sunk to a lower level."[291]

Both Maclaren and Jackson described Darwin's color-speckled reef distribution map as a collation of evidence from which fresh theoretical conclusions could be drawn, rather than as a mere illustration or manifestation of a pre-existing theory. This too was a vindication of Darwin's careful segregation of his two main types of evidence in the manner first employed in the 1837 paper, with facts relating to the growing conditions of corals used as support for the subsidence theory, and those related to the distribution of entire reefs reserved for interpretation in light of the theory. On Jackson's viewing of the map, "the direction of the spaces, coloured red on the map, and which represent the areas raised [sic], is such relatively to the spaces coloured blue and indicating the depressed areas, that their co-relation of effect seems evident on simple inspection, though their synchronism of action cannot in all cases be fully established."[292] Meanwhile, Maclaren himself speculated for several lines upon the nature of the landforms that underlay the coral archipelagoes, and on the probable effects of a continued uplifting of the reef-fringed areas of the East Indies, which might one day "unite that vast chain of islands to one another, and to the continent of Asia...converting the Chinese sea into a vast inland lake."[293]

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290. Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," 40.
291. Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," 42.
292. Jackson, "[Review of] The Structure and Distribution of Coral Reefs," 118 To be precise, the red-colored areas of the map were meant to be areas where the form of reefs suggested an absence of subsidence (as opposed to the presence of elevation), although Darwin had pointed out that in most of these locations there was independent evidence of elevation in the form of upraised shells and corals.
293. Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," 46.

Despite believing that Darwin's theory "explains the phenomena under consideration better than any other which has been proposed," Maclaren closed his review by pointing out what he took to be three of its deficiencies. He first mentioned the imperfect segregation between signs of elevation or stasis and signs of subsidence on the map.[294] The point of this criticism was to show that it would sometimes require very selective boundary drawing to make these contrary indicators belong to separate geographical areas. His next objection was based on the rarity of sites that showed stages of transition between living fringing reefs and coral rock upraised on dry land. If so many areas of the coral seas had been elevated, Maclaren protested, there should be more cases where recognizable fringing reefs were found uplifted and in various states of degradation. His final criticism was the one that he believed to be "the most serious objection to the theory." If the subsidence-built foundations of atolls were formed of coral rock two or three thousand feet thick, as Darwin's diagrams suggested, then there should somewhere be upraised masses of coral rock of similar thickness. In the cycle of geological changes upon which the book was premised, some such reefs ought to be upraised into terra firma. Yet nowhere in the world had "a bed or formation of coral, even 500 feet thick, been discovered, so far as we know."[295]

Darwin's responses to Maclaren's final objection revealed that for all his efforts to avoid overextending his theory, he was still sensitive about appearing over-speculative. In a letter to Lyell, who had "alluded to [Maclaren's] criticisms," Darwin admitted that it was improbable that coral growth on a given reef could continue uninterrupted over the time required for several thousand feet of subsidence.[296]

294. Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," 46.
295. Maclaren, "On Coral Islands and Reefs, as Described by Mr. Darwin," 47.
296. Darwin to Lyell, [September-December 1842]. Burkhardt et al., *CCD*, vol. 2, 328–30. On Lyell's having mentioned Maclaren's criticisms to Darwin, see Charles Darwin, "Remarks on the Preceding Paper, in a Letter from Charles Darwin, Esq., to Mr. Maclaren," *Edinburgh New Philosophical Journal* 34: 47–50 (1843): 171–74. Reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 171–75, quotation on p. 172.

However, he considered this admission "no ways fatal to the theory," because subsidence on such a massive scale was only necessary if one wanted to account for the disappearance of entire continents beneath the Pacific. "In the areas, where the large groups of atolls stand, & where likewise a few scattered atolls stand between such groups, I always imagined that there must have been great tracks of land, and that on such large tracks there must have been mountains of immense altitudes," he explained. "But now it appears to me, that one is only justified in supposing that groups of islands stood there." In a direct reply to Maclaren's review that was published in the *Edinburgh New Philosophical Journal*, Darwin conceded that "In my volume, I rather vaguely concluded that the atolls, which are studded in so marvellous a manner over wide spaces of ocean, marked the spots where the mountains of a *great continent* lay buried, instead of *merely separate tracts of land or mountainous islands*; and I was thus led to speak somewhat more strongly than warranted, of the probable vertical amount of subsidence in the areas in question."[297]

If Darwin had been indirect on this point in his book, however, it was hardly an oversight. The nature of "continental" subsidence and the question of what lay beneath the coral archipelagoes had been issues of central theoretical importance in the provocative version of the theory presented in 1837. Of course, at that time it had been Lyell who advocated most strongly that "the coral islands, are the last efforts of drowning continents to lift their heads above water." It was just such extensions of the theory, and the question of whether they had a place in monographs as well as papers, that had so paralyzed Darwin when he wrote the book. After five anxious years of writing and rewriting, the "bold" conclusions of his 1837 paper had become the very "vague" conclusions that Darwin now abjured.

297. Paul H. Barrett, *Collected Papers*, vol. 1, 174 Emphasis added.

While Darwin's anxious reaction to the feedback generated by his papers of 1837 and 1838 had discouraged him from expounding the implications of the theory as far as he had originally intended, he remained attached to his answer to the limited question of what determined the shape of coral reefs. Darwin's personal faith in this theory, and in the type of theorizing it represented, held strong. As he said in closing his response to Maclaren, "The case, undoubtedly, is very perplexing; but I have the confidence to think, that the theory explains so well many facts, that I shall hold fast by it, in the face of two or three puzzles, even as good ones as your third objection."[298] Far from being empty boosterism, this statement portended the confidence that Darwin would place in his coral work in the years to come.

For Darwin, the oceans and shorelines of the world remained colored in reds and blues. The coral reef map was not only, as Jane Camerini has noted, "a representation of the base map, so to speak, that was in his mind during and after the exciting and profound developments in the so-called transmutation notebooks."[299] It was also the resource that he literally unfolded and consulted as his primary reference on matters of tropical geography, geology, and natural history. His correspondence and notes reveal that he made particularly frequent use of the coral reef distribution map in contemplating the dispersal of animals and plants across the oceans. This was the topic of an involved correspondence in the mid-1840s between Darwin and the young botanist Joseph Dalton Hooker. Darwin advised Hooker that "If you will look at the map in my Coral-volume, you will see that probably much more land existed within geologically recent times than now exists," and he annotated the letters he received with similar reminders to himself, such as "Islands like Mountains -- Isl^ds of Pacific most puzzling [...] Look at my Coral Is^d Map & see whether most peculiar on Blue or Red."[300] This

298. Paul H. Barrett, *Collected Papers*, vol. 1, 174.
299. Camerini, "Darwin, Wallace, and Maps," 89.
300. Darwin to Hooker, 11 March [1844], and annotations on Hooker to Darwin, 28 October 1844. See

was a pattern of behavior that was repeated for decades by Hooker and Lyell as well as Darwin, each of whom treated the coral volume as a source of enduring intellectual stimulation as well as a compendium of data on tropical landforms.[301] Lyell's scientific journals show that he labored in the years to come to think through the effects of subsidence on the scale that Darwin's book implied, even while impishly delighting that Darwin had shown how a "tremendous catastrophe" could be "brought about by what Sedgwick called 'Lyell's niggling operations.'"[302] After the *Origin of Species* was published, Lyell used Darwin's coral theory as a tool for reasoning on the relative speeds of geological and organic change, assuming that species on a subsiding island or continent might become adapted to their new conditions, writing "[A]s to atolls...if they subsided very slowly, the absence of volant & amphibious forms of reptile & mammifer in such a region proves rate of transmutation slow, even as compared to revolutions in physical geography."[303] To those in the position to judge, the coral volume self-evidently belonged to the Uniformitarian canon.

It turned out that Darwin was never really sure where he had fallen on the question of whether groups of atolls were underlain by mere archipelagoes or entire continents. Although he had followed Lyell by making statements that suggested the subsidence of whole continents, he often went on to claim that the opposite was true. This was provoked at least in part by the fact that Edward Forbes and others invoked the former existence of land bridges to explain the distribution of organisms, which Darwin viewed as unsupportable speculation. He exclaimed self-righteously to Hooker, "I never made a continent for my Coral Reefs."[304] Later he told Lyell, "With respect to

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also Darwin to Hooker, [6 March 1844] Burkhardt et al., *CCD*, vol. 3, 16–17, 19–20, 68–73.

301. See e.g., Darwin to Lyell, 5 July 1856 and 3 March 1868.

302. Leonard G. Wilson, ed., *Sir Charles Lyell's Scientific Journals on the Species Question* (New Haven: Yale University Press, 1970), e.g. 61 and 135. Lyell quoted Sedgwick in a letter to Darwin, 17 June 1856.

303. Lyell to Darwin, 8 September 1860.

304. Darwin to Hooker, 19 July 1856.

<permanence> <<long endurance>> of our existing continents, I formed my opinion chiefly from facts of geographical distribution, to which I allude in Origin -- & partly from views given under Coral Reefs."[305] Hooker for his part, could "not find a reference to the permanence of continents in [Darwin's] "Coral Reefs", -- a book by the way that shook my confidence in that theory more than all others put together, & the effect of which it has required years of thought to eliminate or rather to overlay."[306] No doubt Darwin's protestations were partly a case of revisionist history, of remembering what he wanted to remember. But they also reflect the fact that in the process of recasting and reassessing his theory, during and after the *Beagle* voyage, Darwin ended up saying and writing things that were in tension, if not downright contradictory. As the differences between the texts of 1835, 1837, and 1842 reveal, there was not just a single, static, coral theory. For this reason, it is no wonder that Darwin had trouble remembering whether he thought "the theory" had been too speculative or not enough, and whether it had proved the permanence of continents or the exact opposite.

Conclusion

My central argument in this chapter has been that Darwin's ideas about coral reefs were molded into a new geological theory after the *Beagle* voyage. In particular, I have focused on the relationship between Darwin and Lyell, arguing that Lyell played an active role in shaping the coral work that saw the light of publication, rather than merely inspiring it by his writings and making it possible by his patronage. Finally, I have shown that the coral book Darwin wrote afterward was a relatively conservative sequel to the highly theoretical, highly Lyellian manuscript of 1837. Darwin's efforts to bring a coral theory into publication left him a willing, but nervous, theorist. This should be a

305. Darwin to Lyell, 3 March 1868.
306. Hooker to Darwin, 11 August 1881.

useful insight for biographers of Darwin, who generally characterize him either as a person who spent twenty years working on his species theory without considering it finished, or as one who held on to a finished theory for twenty years for fear of revealing his secret.[307] Before he had even sketched his 1842 species draft, he had already agonized over the possibility of being too speculative in his long-postponed coral reef book.

This chapter has described how two men who first encountered each other indirectly through written texts became allies and collaborators. It is widely acknowledged that Darwin's admiration for Lyell came about through his reading of the *Principles* during the *Beagle* voyage. Lyell too was enthusiastic about Darwin before the voyage had ended, as a result of the letters that Darwin sent home to Henslow. From late 1836 until Lyell's departure for the United States, the two met frequently and worked together to orchestrate Darwin's new career, with Lyell in the role of master to Darwin's apprentice. What Lyell provided was not instruction in the practice of field geology, but mentorship on the role of the gentlemanly specialist in the science of geology.[308] Lyell vetted and revised Darwin's work before it was presented in public, he interceded on Darwin's behalf to schedule talks and arrange meetings, and he appointed Darwin to act on his behalf, most importantly through the arguments of his publications. I have also illustrated that in Lyell's publications, he enjoyed almost proprietary right to Darwin's unpublished findings; his correspondence suggests that he considered Darwin's work to be an impressive but derivative product of Lyell's "own heretical doctrines." In these ways he played a major role in both the form and content

307. The best argument that Darwin feared publishing his theory is made in Desmond and Moore, *Darwin*. For a study of Darwin's ongoing work on species between the first sketch of 1842 and the publication of the *Origin* in 1859, see Dov Ospovat, *The Development of Darwin's Theory* (Cambridge: Cambridge University Press, 1981).
308. On "gentlemanly specialists," see Rudwick, *The Great Devonian Controversy*.

of Darwin's first ventures into the realm of geological theorymaking, ushering Darwin to publish dramatic works of Lyellian geological speculation from 1837 to 1839.

An interesting study might yet be made comparing Lyell's treatment of Darwin with Darwin's later treatment of his own younger colleagues. Certain parallels to the Darwin-Hooker relationship spring immediately to mind, particularly that of the master pumping the disciple for information to use in his books. Even more intriguing are parallels between Lyell's response to Darwin's coral theory, as I have portrayed it here, and Darwin's response to the species theory of Alfred Russel Wallace.[309] In each instance, a man who was well-connected and well-respected in the metropolis was confronted by a junior traveler who offered a compelling but potentially inconvenient theory. In each case, the elder man claimed to have been convinced or chastened by the new individual while imposing near-complete control over the circumstances in which the younger man's theory would be made public. And in both cases, the senior colleague succeeded in bolstering his own reputation by absorbing the newcomer's findings into a larger project that would continue to carry the better-established man's name.

Having first followed Lyell's example implicitly, Darwin shrank from it after receiving a taste of the partisan sniping that such bold speculations could engender from other geologists. He gradually expressed other concerns as well. Fitting a book's worth of details to a general theory was more difficult than coming up with a theory in the first place. Aside from that, what made a proper monograph in natural history was the comprehensive systematization of "certain" empirical knowledge, not speculation. As he abandoned first the original plan of writing a single big book on the geology of the voyage, and then divided the work from two into three parts, the particular claims he made in 1837 were too ambitious and topically broad to be backed up in one book about

309. I thank Simon Schaffer for encouraging me to draw this parallel.

coral reefs. Meanwhile, as his own reputation and solid place in London science were secured, he had less need to impress Lyell or anyone else with the grandeur of his theorizing. Now he had to prove that he could do the methodical work that served as the true currency for the practical men like Sedgwick. Finally, there was the object lesson of Lyell himself, who it seemed had never written a book he wasn't planning to revise. Moreso than any other geologist in the country, Lyell had made a career of authoring profitable geology books, which he revised and sold again with the help of his publisher, John Murray.[310] In Darwin's balance-sheet notes, written while he pondered his professional and marital options in 1837 or 1838, Darwin wrote, " I could not go on as Lyell does, correcting and adding up new information to old train." This lack of enthusiasm for the prospect of updating and rewriting his books may also, therefore, have given Darwin pause, when he considered just how much to let the massive empirical content of his first monograph appear to rest on a theoretical foundation that others might view as merely provisional.[311]

Despite the self-confident fondness he expressed for the coral book throughout his life, he constantly returned to the possibility that he had been overzealous in his speculations. While each recollection was aimed at a different audience and was meant to serve a different purpose, it is nevertheless instructive to survey how Darwin managed this issue. In some instances he defended the rigor with which he had guarded against error. He explained to C.H. Smith in 1845 that he had taken a highly skeptical approach to the sources he used in compiling his reef distribution map because "Every one knows how greedily a theorist pounces on a fact [that is] highly favourable to his

310. For an overview of Lyell's career as an author, see James A. Secord, "Introduction," in *Principles of Geology*, Charles Lyell (London: Penguin, 1997), ix-xliii.

311. At least in the time shortly after it was published, Darwin made it clear that he had no plan ever to revise the coral reef book. In a letter to Hooker of 7 January 1845, he wrote "Thanks for your offer of collecting facts about coral-reefs, but I will not trouble you, as I shall never publish a second Edition." Twenty years later, he had done so (see chapter 4).

views."[312] When Henry de la Beche, during his 1848 presidential address to the Geological Society, asked whether atolls might in some instances form atop banks of sediment, Darwin responded with a letter restating the arguments made in the book and amplifying the judicious approach he had taken therein. "I remark," he reminded de la Beche, "'The evidence from a single atoll or a single encircling barrier-reef, must be received with some caution, for the former may possibly be based on a submerged crater or bank, and the latter on a submerged margin of sediment or of worn-down rock.' -- Whether you consider my remarks satisfactory or not, I trust that you will find that I have not proceeded without consideration of the sources of error: I assure you, I did not spare time or labour in examining thousands of charts & all voyages. -- But forgive the length of this letter; a man is as tender of his theories as of his child<<ren>>." When, on the other hand, Darwin was responding to new reef studies that seemed to support his theory, he was then only too willing to concede that he had speculated, albeit correctly. Writing to Joseph Beete Jukes (see chapter four), whose examination of the Australian barrier reef had led him to declare Darwin's "the true theory of coral reefs," he affected this confessional tone. "I have always felt that my coral-reef book was too bold & speculative & therefore you will not easily imagine how gratified I am when anyone, who has had opportunities of observation, does not give his verdict against it."[313] After James Dwight Dana of the U.S. Exploring Expedition reported on the findings of that most comprehensive trek through the Pacific islands, Darwin gloated to Lyell that "I am astonished at my own accuracy!! if I were to rewrite now my coral book, there is hardly a sentence I sh^d. have to alter [...] Dana talks of agreeing with my theory in most points; I can find out not one in which he differs. -- Considering how infinitely more he

312. Darwin to C.H. Smith, 26 January 1845.

313. Darwin to J.B. Jukes, 8 October [1847]. The Jukes quotation comes from Joseph Beete Jukes, *Narrative of the Surveying Voyage of H.M.S.* Fly, *Commanded by Captain F.P. Blackwood, R.N.*, 2 vols (London: T. & W. Boone, 1847), vol. 1, 347.

saw of Coral Reefs than I did, this is wonderfully satisfactory to me; though really I think it some little reflection on him, that he did find other & new points to observe."[314] (I discuss Dana in the next chapter.) These recollections touch obliquely on various delicate points: the riskiness of basing conclusions on the first-hand study of just a single atoll, the relation of speculation to evidence, and the reputation of a "theorist."

This chapter has demonstrated that Darwin's 1837 and 1842 coral texts *were* geology, in that they were written by a self-identified geologist, for the consumption of other geologists, as part of an explicitly geological publishing campaign. As the cumulative evidence presented in the last two chapters reminds us, however, this is not the same thing as saying that Darwin's study of corals and reefs began as an exclusively or inherently geological enterprise. Just as it was not inevitable that he would explain reef shapes by reference to coral growth limits and subterranean movement, it was not a foregone conclusion that Darwin's only coral publications would be geological ones. Having aspired to become the authority on zoophytes, he avoided distributing the marine invertebrate specimens from the *Beagle* voyage to another collector. It was only once the *Beagle* geology had expanded to three books, and he had become absorbed in studying the origin of species, that he began to concede that he would never fulfill his original plan to publish on the zoology and taxonomy of corals.[315] As he wrote in 1849 to Dana, a man who did publish on both coral zoology and reef formation, "I have always felt much interested in regard to your classification &c of the corals; I dissected enough to see what a famous field there was open. Indeed I had intended working on this subject, but my miserable health for the last ten years, (which has lost me much more than half my time) has interrupted all my former hopes & designs."[316] At the

314. Darwin to Lyell, 4 December 1849.
315. On Darwin's plans for a work on the invertebrate zoology of the voyage, and their abandonment, see Love, "Darwin and *Cirripedia* Prior to 1846".
316. Darwin to J.D. Dana, 8 October 1849. Burkhardt et al., *CCD*, vol. 4, 265–67. See also Darwin to Peter Martin Duncan, 18 July 1861.

same time, the frequent juxtaposition in his private notebooks of topics that were eventually published as either zoology or geology shows that the barriers between these sciences were more social and institutional than intellectual. In other words, studying the origin of species was not a narrowly zoological undertaking any more than studying coral reefs was inherently geological.[317] It was no more inevitable that Darwin would present his species theory in 1858 as zoology (i.e., at the Linnean Society) than it was preordained that his studies of coral growth would first see the light of day in a meeting of the Geological Society.

Just as later events have obscured the path by which Darwin's coral theory became geology, his later fame as a "biologist" has also diminished the apparent role that geology played in providing him with enduring research questions, a mentor, and an audience. Darwin himself was responsible for helping to efface his debt to geology, as indicated by comparing the closing sentence of the *Origin of Species* with that of his 1842 species sketch. The *Origin* ended with the observation that "There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved."[318] The 1842 text ended with a similar sentence that did not cite gravity as the most important fixed law, but instead referred to the way that "land and water, in a cycle of change, have gone on replacing each other."[319] Why did the *Origin* lose this geological allusion to Lyell? There is more than

317. As is indicated by the programmatic statements that Whewell and Lyell made when each was president of the Geological Society, the origin of species was seen as a central question to geologists, and indeed was widely considered to be a puzzle that could only be answered by geological evidence. (This is not to say that geological evidence was guaranteed to reveal a cause or mechanism of speciation, only that it would answer the historical question of when, where, and how it occurred.)

318. Charles Darwin, *On the Origin of Species* (London: John Murray, 1859), 490.

319. Francis Darwin, ed., *The Foundations of The Origin of Species. Two Essays Written in 1842 and 1844* (Cambridge: Cambridge University Press, 1909), 52.

one possible reason: he knew that Lyell disagreed with his species theory, and his own reputation was now based as much on his study of barnacles as it was on his work elucidating these geological changes. It does not require the retrospective knowledge that this phrase was removed to notice the purposes it served in 1842, however, of highlighting Darwin's best known original research, accentuating the importance of what he called "geographico-geological" knowledge in the development of his theory of natural selection, and implying that as sources of reliable knowledge, geology was equivalent to physics and Lyell was on a par with Newton.

That nearly all other historians have treated the coral theory as essentially static in the post-voyage years must stem in part from their faith in Darwin's autobiographical statement that the whole theory was thought out during the voyage before he had seen a true coral reef (on which, see chapter 2).[320] Yet it also grows from the tendency to treat the coral theory as a precursor to, and formal analogue of, the species theory. I have shown this view to be unsatisfactory, among other reasons because it ignores the fact that Darwin's views on coral reefs were being modified at the same time that he was working on solving the geology of the globe and the origin of species. Indeed, as Darwin's ideas about species changed, so did his veiled allusions to this private work in his publications on coral reefs. In the 1837 paper he publicly announced that knowledge of geographical changes derived from studying coral forms would shed light on the history of organisms. In the same months that he was writing and revising that paper, his current species theory relied on geographical changes as the engine behind organic

320. Among those who are explicitly concerned with how Darwin's theoretical ideas changed, see, for example, Patrick Armstrong's table showing the development of the coral theory, which implies that it was unchanged after the voyage. Armstrong's remarks, that the 1837 paper was the "First formal presentation of the theory," and the 1842 book was the "Publication of [a] full statement," are similar to those of many other Darwin scholars. Howard Gruber, in a work designed to show that Darwin had *multiple* theories of the origin of species during his London years, nevertheless states that the 1835 "Coral Islands" essay gave "the [coral] theory in its essentials." Armstrong, *Darwin's Other Islands*, 249; Gruber, *Darwin on Man*, 102, note 11.

variation. In the B Notebook he wrote “Species [are] formed by subsidence.” However, after his 1839 reading of Malthus and his invention of a new mechanism for speciation, natural selection, he ceased to imply that the mechanism that formed new species of coral reefs and the one that formed new species of organisms were one and the same. Instead of dwelling solely on the possibility that knowledge of subsidence was the key to understanding speciation, his 1842 book was rife with discussions about the struggle for existence between different corals, and between corals and algae.[321] I will readily grant that it does not require a theory of natural selection to prompt reflections on the struggle between organisms of the same and different genera; nevertheless, it seems clear that Darwin’s coral publications were infused with thoughts related to his species work, and were indeed sown with arguments that could later have been used to promote or reinforce the species work when it was published.

My point has not been to invert the conventional argument by claiming that the species theory served as a model for Darwin’s coral reef work. Rather, this chapter has demonstrated that during Darwin’s London years, the influences of one of his “cognitive enterprises” upon another were never simply mono-directional. This was true of work “within” his coral reef project, where knowledge of reef distribution reinforced conclusions about crustal movement, while hypotheses about crustal movement lent meaning and plausibility to his explanation of reef movement. But it was equally true of the highly permeable relationship between the coral project, the geology project more broadly construed, and his species work. Darwin was constantly eager to use mutually relevant classes of facts to draw insights about *each* class of facts. Thus while his coral book implied that one benefit of determining “the [vertical] movements, which the crust of the earth has within a late period undergone” was to shed light on the “final effects of

321. By contrast, in 1836 he had been captivated by the struggle between corals and the ocean. See his Diary entry for 6 April 1836. Keynes, *Diary*, 417.

these movements...on the distribution of organic beings," his species essay of the same year declared, "How interesting does the distribution of all animals become, as throwing light on ancient geography."[322] For Darwin, there were no static forms of evidence. In defining the consilience of inductions, Whewell had supposed that a hypothesis meant to explain "one class of facts only" would inspire confidence if it also provided "the rule and reason of [another] class of facts not contemplated in its construction." In practice, as Darwin pondered the connections between the history of organic forms, geological movements, and organic distribution (including the distribution of corals in the form of a ring), there ceased to be any class of relevant facts that had not already been contemplated in the construction of his hypotheses. When he claimed otherwise, as he did in his coral publications by segregating conclusions based on facts about growth from those based on facts about distribution, he was following rhetorical and logical conventions, not describing the actual history of his own intellectual labor.[323]

With this last point in mind, it is worth coming back to my claim in chapter 2 that Darwin's autobiography is not a reliable account of his work on coral reefs. On the question of whether he had any theory of coral reef formation before he "saw a true coral reef," the 1837 and 1842 publications *imply* that the subsidence theory was prompted by contemplating the previously unexplained phenomenon of encircling reefs. As it happens, the account of Darwin's thoughts that I reconstructed from his voyage notes in chapter 2 suggests much the same, with the encircling reef in question being that of Eimeo. The autobiography account, on the other hand, offered an entirely different account of how the theory was derived, which was based on the (accurate) premise that Darwin had reflected on subsidence and thick coral deposits while he was still in South America. In this chapter I am not trying to analyze the degree of accuracy

322. Francis Darwin, *Foundations*, 51.
323. My thinking on these matters has been shaped by Warwick, *Masters of Theory*. See especially p. 9.

with which Darwin’s published versions of the coral reef theory reflect the way he “really did” come up with “the theory.” Rather, by showing that the arguments were so different (from 1835 to 1837 to 1842 to 1881) I hope to explode altogether the idea that Darwin’s publications describe his method of theorizing, and to demonstrate that they instead reflect simply what he considered the best or most appropriate form of argument and the most convincing evidence at a given time and for a particular audience.

CHAPTER 4
Zoology, Geology, and the Emergence of a "Coral Reef Problem"

Introduction

This chapter opens in an era of wooden sailing vessels that carried men of science on several-year surveying expeditions and closes at a time when professional scientists availed themselves of steamships and could choose to measure their reef field trips around the world in seasons rather than years. Among these trends, one that particularly shaped the practice and the discussion of reef science was that by the turn of the twentieth century, virtually all the participants had a specialist training in a particular scientific discipline. In the study of hybrid objects like coral reefs all manner of disputes came to be characterized and lamented as inter-disciplinary problems. The biological and geological approaches that were available to Darwin as choices of how to work ceased to inhere in single individuals. For geologist James Dwight Dana and zoologist Josepth Pitty Couthouy of the U.S. Exploring Expedition (1838-1842), the problem was that each was competent and interested in the other's "department." This issue on later voyages and in later controversies was that individuals were <u>incapable</u> of seeing the problem from another disciplinary perspective.

I trace this story from the U.S. Exploring Expediton through the British *Challenger* voyage (1872-1876) to the early twentieth century responses to a trio of reef boring expeditions that were sent to the Pacific atoll of Funafuti. Through this period that, technologically speaking, looks like the rise of modern oceangoing, I argue that certain types of expedtions seemed to suggest certain types of solutions to what became known in the 1880s as the "coral reef problem," with particular attention to shore-hugging surveys versus the oceanographic expeditions like that on the *Challenger* which allowed the zoologist John Murray to concentrate on the mysteries of the deeper water and to attribute a smaller role to effects governed by the meeting point between land and water level.

U.S. Exploring Expedition: A tale of two departments

The story of the US Ex. Ex. shows how personal conflict between two scientific specialists could arise from the perception that explaining coral reef formation legitimately belonged to each man's "department" of science. While the records of their coral reef studies during the voyage itself are ambiguous at best, there is no doubt that after the expedition the overlapping boundary between zoology and geology was a key region of contention between Joseph Pitty Couthouy and James Dwight Dana as they battled over the propriety and accuracy of their respective publications on reef formation.

Not surprisingly for such a costly undertaking, the Exploring Expedition served different purposes for different constituencies. The historical literature on the expedition shows that the dispatch of a Naval squadron to explore the Pacific and Antarctic was conceived in part as a commercial venture, aimed at protecting and expanding the interests of the American whalers and sealers who were already plying the Great Ocean, and also as a political gesture meant to emulate and supersede French and British voyages of "scientific exploration."[1] What both objectives required in

1. William R. Stanton, *The Great United States Exploring Expedition of 1838–1842* (Berkeley: University of California Press, 1975), 1–40; Nathaniel Philbrick, *Sea of Glory: America's Voyage of Discovery, the U.S. Exploring Expedition, 1838–1842* (New York: Viking Penguin, 2003), 3–42; Herman J. Viola, "The Story of the U.S. Exploring Expedition," in *Magnificent Voyagers*, Herman J. Viola and Carolyn Margolis (Washington: Smithsonian Institution Press, 1985), 9–23. For a selection of relevant primary documents, see Nathan Reingold, ed., *Science in Nineteenth-Century America: A Documentary History* (New York: Hill and Wang, 1964), 108–26.

practice were surveys of islands that might be frequented by American vessels. For this venture, the discovery of new lands was secondary to the goal of reducing the uncertainty about the location and navigability of innumerable islands and shoals that had already been reported. The tropical Pacific portion of this work was the largest task of the voyage, though it has often been overshadowed by the squadron's penetration into the Antarctic and the surveys of the Columbia River on the west coast of North America.[2] What made the tropical Pacific so treacherous, of course, were the low islands and barrier reefs that formed so many of its strand lines. The examination of coral formations was by definition central to the mission. Anticipating the danger of navigating these shores, as well as the possible hostility of their inhabitants, upon being given command of the expedition, Lieutentant Charles Wilkes devised a "Method of Surveying the Coral Islands" that could be executed without landing.[3] The system exploited the multitude of vessels and officers that Wilkes had at his disposal and relied on the use of ships' guns to measure distance by sound. With observers in separate boats "occupying all the points of a trigonometric survey simultaneously," baselines would be established by sequential firing of guns, beginning with a ship standing off the island.[4] Vessels would then move systematically around the island in either direction until they met up again to join their triangles on the other side.

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2. Mary E. Cooley, "The Exploring Expedition in the Pacific," *Proceedings of the American Philosophical Society* 82 (1940): 707–19; Ralph E. Ehrenberg, John A. Wolter, and Charles A. Burroughs, "Surveying and Charting the Pacific Basin," in *Magnificent Voyagers*, Herman J. Viola and Carolyn Margolis (Washington: Smithsonian Institution Press, 1985), 164–87.

3. Wilkes distributed copies of this document to each of the vessels in the squadron. It is reprinted in Charles Wilkes, *Narrative of the United States Exploring Expedition During the Years 1838, 1839, 1840, 1841, 1842* (Philadelphia: C. Sherman, 1844–74), vol. 1, appendix xli, and expanded in Charles Wilkes, *Hydrography [U.S. Exploring Expedition Scientific Volume XXIII]* (Philadelphia: C. Sherman, 1861), 6–13. On the use of this technique in practice, see D. Graham Burnett, "Hydrographic Discipline Among the Navigators," in *The Imperial Map*, ed. James Akerman (Chicago: University of Chicago Press, forthcoming).

4. Wilkes, *Hydrography*, 6.

The Wilkes expedition was outfitted with a large corps of scientific specialists on the model of the Baudin expedition, rather than with one or two surgeon naturalists, gardeners, or philosophers-general, as had been the case on many of the more recent Pacific surveys, including those of Kotzebue and Beechey. Before the positions for men of science had been filled, the Secretary of the Navy Mahlon Dickerson wrote to the leaders of four of the nation's scientific institutions to request nominations for experts to join the expedition and advice on the topics to which their attention should be directed. In a letter to Peter DuPonceau, the president of the American Philosophican Society in Philadelphia, and in similar entreaties to the nearby Academy of Natural Sciences, the Naval Lyceum of New York, and the East India Marine Society of Salem, Massachusetts, Dickerson sought the names of gentlemen well acquainted with the fields of "Geology and Mineralogy, -- with Botany, with Zoology in all its numerous branches, with meteorology, magnetism, electricity and other subjects connected with natural history," along with a philologist and a portrait painter.[5] The APS committee appointed to respond to the Secretary's request declined to nominate specific individuals, but they produced a thirty-page statement of scientific objectives for the voyage. J.K. Paulding, who succeeded Dickerson as naval secretary in the Van Buren administration, transmitted this "learned and comprehensive Report" to Wilkes along with the Navy's official 1838 "Instructions to the Commander" of the voyage, and declared the scientific directions to form an official part of the commander's orders.[6]

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5. Mahlon Dickerson to [Peter S. DuPonceau], 31 August 1836. Quoted in Edwin G. Conklin, "Connection of the American Philosophical Society with Our First National Exploring Expedition," *Proceedings of the American Philosophical Society* 82 (1940): 520. See also James A.G. Rehn, "Connection of the Academy of Natural Sciences of Philadelphia with Our First National Exploring Expedition," *Proceedings of the American Philosophical Society* 82 (1940): 543–49; G.S. Bryan, "The Purpose, Equipment and Personnel of the Wilkes Expedition," *Proceedings of the American Philosophical Society* 82 (1940): 551–60.

6. J.K. Paulding, "Instructions to the Commander." Quoted in Conklin, "Connection of the American Philosophical Society with Our First National Exploring Expedition," 539.

In a move that helped to ensure that there would be friction between Couthouy and Dana, the APS instructions directed both the zoologists and the geologists of the voyage independently to tackle the question of coral island formation. This was not simple duplication of an order, however. The instructions revealed that it was possible, and indeed desirable, to approach the matter from distinct zoological and geological perspectives. With the issue defined as it had been in the last decade by Quoy and Gaimard, Beechey, and Lyell, the origin of coral islands could plausibly be examined by studying the conditions of organic growth or by seeking knowledge of submarine geology. I am aware of no previous voyage or expedition in which the study of coral reefs was formally divided between multiple specialists.[7] The zoology instructions by Titian Peale, who as it turned out served as a naturalist on the expedition himself, sought a solution in the habits of coral-forming animals. The zoologists were ordered "to dredge in deep as well as shallow water for the numerous inhabitants of the ocean, and to ascertain as nearly as possible, the different depths at which those animals exist; the depths from which the various species of Zoophytes erect their fabrics and form Islands, many of which in after-times become the residence of Man; to ascertain the time requisite for the maturity of such; their food; and in fact to collect all the information which can be reasonably obtained of that race of animals, which though among the smallest, hold notwithstanding one of the most important places in the chain of created beings."[8] For geologists, according to the instructions written by H.D.

7. Although Quoy and Gaimard published jointly on coral reefs after their voyage on the *Uranie*, and Eschscholtz and Chamisso produced separate statements on reef formation after their voyage with Kotzebue, I have not seen any evidence of a formal division of labor within either pairing. The Yale professor Benjamin Silliman proposed the outlines of a similar arrangement in a letter to Jeremiah Reynolds of 30 May 1836. He cited "Coral animals" as a desideratum of "Zoology," and "Coral reef islands; above or under water" as a topic for "Geology, Mineralogy, &c," in describing "objects...deserving of especial notice in [a] voyage towards the South Pole." The letter is transcribed in Jeremiah N. Reynolds, *Address on the Subject of a Surveying and Exploring Expedition to the Pacific Ocean and South Seas* (New York: Harper & Brothers, 1836), 112–15.

8. Titian Peale, quoted in Conklin, "Connection of the American Philosophical Society with Our First National Exploring Expedition," 530.

Rogers, the key feature was to determine what lay beneath the corals. "The circular figure and deep water of the Coral Islands having given rise to the conjecture that these fabrics of the Zoophytes are based upon the craters of submarine volcanoes, the collection of any facts calculated to throw light upon this subject will form one of the interesting duties of the Geologists."[9] The expedition was also outfitted with well drilling equipment that would enable the direct study of this question by boring through a reef.[10]

That these two visions of coral reef study were drawn from the European traditions of scientific navigators and theorists of the earth that I described in chapter 1, and were not independent products of the North American commercial presence in the Pacific, is indicated by the APS committee's "*List of Books*, recommended to be taken on the Expedition for the use of the Officers and Scientific Corps."[11] The thirty-eight works listed in this bibliography were presumably among those used by the committee members themselves in determining the possible and desirable topics that might be studied during the voyage. Among them were Beechey's *Narrative*, which detailed his comparative surveys of the coral islands of the Low Archipelago and summarized the relevant philosophical literature; the *Voyage* of "Kotsbue," whose volumes included the reef theories of Chamisso and Eschscholtz, and the *Narrative* of Freycinet, who had

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9. Henry D. Rogers, quoted in Conklin, "Connection of the American Philosophical Society with Our First National Exploring Expedition," 533.

10. At Rose Island, Couthouy wrote in his journal that "A more eligible locality for making some experiments by boring, for which we have the necessary apparatus in the Expedition, can hardly be expected to occur during the cruise." Entry for 7 October 1839. BMOS Archives 22, p. 289.

11. The list is reprinted in Conklin, "Connection of the American Philosophical Society with Our First National Exploring Expedition," 537–38. The great advocate of a U.S. national exploring expedition, Jeremiah Reynolds, compiled a catalogue of "Islands, Reefs, and Shoals, in the Pacific Ocean, &c" by consulting the logbooks of New England whalemen and sealers. On the origin of these islands, Reynolds wrote, "From all the accounts I have received of the islands, reefs, rocks, &c., in these seas, I draw the inference that most of them are of volcanic origin, and have arisen, in the lapse of ages, in groups or single islands, as it has pleased the great Creator of the universe to call them into existence; and by the same great engine of nature they may be constantly changing." Reynolds, *Address*, 193–230, quotation on p. 196. See also Burnett, "Hydrographic Discipline," 193–98.

couriered Quoy and Gaimard through the East Indies and the Pacific. Also present were the British geological textbooks of Lyell and De la Beche, which examined the question of reef formation from the perspective of earth history, and the works of the continental geologists Leopold von Buch and Elie de Beaumont, whose theories of mountain building and crater formation were directly relevant to contemporary views on ocean reefs.

These zoological and geological responsibilities were assigned to Joseph Pitty Couthouy and James Dwight Dana, respectively. They were among nine civilians who formed the scientific corps of the Exploring Expedition, which was charged with investigating natural history, philology, and ethnology.[12] Initial plans called for an even larger civilian scientific presence on the five ships of the expedition, but these were scuppered by Wilkes, who insisted that the physical and navigational sciences be the exclusive province of navymen.[13] As one of the least-experienced lieutenants in the Navy, Wilkes owed his command of the Expedition in large part to the fact that his achievements and ambition in those areas outshone those of his fellow officers, and during the voyage he added control of these scientific departments to his long list of personal duties.

Dana, who was 25 when the Wilkes Expedition departed in 1838, began lobbying for the position as mineralogist and geologist two years earlier, when he feared that political favoritism would leave him in "dis-appointment."[14] However, as a protegé

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12. On the appointment of the civilian members of the expedition, see Stanton, *The Great United States Exploring Expedition of 1838–1842*, chapter 3.
13. In a memorandum of 1838, Wilkes excluded the civilians from "[a]ll the duties appertaining to Astronomy, Surveying, Geography, Geodesy, Magnetism, Meteorology, and Physics generally." After the voyage, Wilkes wrote an often-cited justification for this decision: "I felt that the Navy was justly entitled to all these departments embraced as they are within the limits or scope of the profession, and that they ought not to be, attached to such an undertaking to act simply as the 'hewers of wood and drawers of water' as was the case in the original organization." (16 July 1842). These quotations are drawn from Reingold, *Science in Nineteenth-Century America*, 124.
14. See Dana to Asa Gray, December 1836. HU-GH Historic Letters File.

of one of the nation's foremost men of science, Yale's Benjamin Silliman, and with prior exposure to both the U.S. Navy and European geology gained during his tenure as a shipboard mathematics instructor to midshipmen on the Mediterranean service, Dana was an excellent candidate for the scientific corps.[15] This experience, and his good fortune to be assigned to the sloop of war *Peacock* rather than to Wilkes's flagship *Vincennes*, helped him to evade the Ex. Ex. commander's hostility toward his civilian passengers much better than did his ill-fated colleague, Couthouy.

Had Joseph Couthouy remained with the Expedition for its entire duration, he might share with James Dana the distinction of having examined more Pacific coral reefs than any other naturalist in the age of sail. Given the recklessness of his desire to study the shore of every coral island he saw, regardless of pounding surf or threatening natives, there is also a good chance he would have become a martyr to his science, with a name even more obscure than it is today. As it stands, Couthouy is one of the most enigmatic figures to be involved in the Exploring Expedition, or indeed in the whole history of coral reef science. He was an impassioned former merchant captain who riddled his journal with classical allusions and perpetually advocated on his own behalf.[16] His business evidently took him to the Mediterranean and to the coral reefs of

15. Although few American scientists have ever been more prominent in their field than was Dana in geology during the second half of the nineteenth century, the only full-length biographies devoted to him are the1899 life-and-letters compilation by Daniel Coit Gilman and the 1978 Ph.D. thesis by Michael Prendergrast. For these and other biographical sketches relevant to this chapter, see Daniel Coit Gilman, *The Life of James Dwight Dana, Scientific Explorer, Mineralogist, Geologist, Zoologist, Professor in Yale University* (New York, London,: Harper & Brothers, 1899); Appleman, "James Dwight Dana and Pacific Geology"; David R. Stoddart, "This Coral Episode"; Michael Laurent Prendergast, "James Dwight Dana: The Life and Thought of an American Scientist," Ph.D. thesis (Los Angeles: University of California, Los Angeles, 1978).

16. There is scant biographical information on Couthouy. The best source is William H. Dall, "Some American Conchologists," *Proceedings of the Biological Society of Washington* 4 (1888): 95–134. See also Stanton, *The Great United States Exploring Expedition of 1838–1842*, 48; Frederick M. Bayer, "The Invertebrates of the U.S. Exploring Expedition," in *Magnificent Voyagers*, eds Herman J. Viola and Carolyn Margolis (Washington, D.C.: Smithsonian Institution Press, 1985), 71–88. I have found only one manuscript letter written by Couthouy before the voyage, a strident complaint about his vexation with the Secretary of the Navy sent to Asa Gray, 19 March 1837. HU-GH Historic Letters File.

the Caribbean, for he had contributed specimens from both locations to the natural history cabinets of Boston.[17] He pursued his scientific interests in home waters as well, helping to expand the marine catalogue of the Massachusetts zoological survey.[18] He was more than a collector, however, because his published taxonomic work on local mollusks and zoophytes was deemed authorititative.[19] His reviews of others' scientific work were carried in London penny periodicals.[20] After appealing directly to President Andrew Jackson for a place on the expedition (reportedly telling him, "Well, General, I'll be hanged if I don't go, [even] if I have to go before the mast!"[21]), he was appointed the squadron's conchologist and placed under Wilkes's supervision on the *Vincennes*.[22]

Although the manuscript records of the Exploring Expedition are vast, we have only an odd assortment of those that belonged to Couthouy and Dana. Unfortunately for my purposes, we have no contemporaneous sets of field notes that would allow us to compare how the two colleagues approached any given coral island. The single extant volume of Couthouy's journal spans only the first ten months of 1839, while Dana's

17. Jerome V.C. Smith, *Natural History of the Fishes of Massachusetts, Embracing a Practical Essay on Angling* (Boston: Allen and Ticknor, 1833), 296–97; D. Humphreys Storer, "An Examination of the 'Catalogue of the Marine and Fresh Water Fishes of Massachusetts, by J.V.C. Smith, M.D.,' Contained in Professor Hitchcock's 'Report on the Geology, Mineralogy, &c. of Massachusetts.'," *Boston Journal of Natural History* 1 (1837): 355.

18. "[Review Of the Reports of the Commissioners on the Zoological Survey of Massachusetts]," *Tne North American Review* 47, no. 100 (July 1838): 253; Augustus A. Gould, *Report on the Invertebrata of Massachusetts: Comprising the Mollusca, Crustacea, Annelida, and Radiata* (Cambridge, MA: Folsom, Wells, and Thurston, 1841).

19. Joseph Pitty Couthouy, "Descriptions of New Species of Mollusca and Shells, with Remarks on Several Polypi, &c. Found in Massachusetts Bay," *Journal of the Boston Society of Natural History* 2 (1838): 53–111. It is cited as an authorititative text in *Proceedings of the American Philosophical Society* 1, no. 2 (March and April 1840): 187.

20. Joseph Pitty Couthouy, "Luminous Appearance of the Sea (Part I)," *The Saturday Magazine* 437 (27 April 1839): 159–60; Joseph Pitty Couthouy, "Luminous Appearance of the Sea (Part II)," *The Saturday Magazine* 439 (4 May 1839): 171–72.

21. Dall, "Some American Conchologists," 109.

22. According to Wilkes' *Autobiography*, written long after Couthouy was dead, the commander was suspicious of Couthouy from the outset of the voyage. "He had much glibness of tongue and very little truth or fact to accompany it, and in making the selection of his quarters, I assigned him to the *Vincennes* instead of the *Peacock*, where I could more readily control his peculiar disposition, as I understood it would require." Charles Wilkes, *Autobiography of Rear Admiral Charles Wilkes, U.S. Navy, 1798–1877*, eds William James Morgan, et al. (Washington: Naval History Division, Department of the Navy, 1978 [manuscript written 1871–1875]), 382.

journals from that early part of the voyage are missing (perhaps having been lost in the wreck of the *Peacock* at the Columbia River in 1841).[23] Couthouy's account covers the beginning of the Expedition's first cruise across the tropical Pacific, including their pass through the Low Archipelago, the Society Islands, and the Samoas on a track from South America to Australia. It gives the impression that he and Dana shared theoretical ideas (and disagreements) and cooperated well in building their respective collections of specimens.[24]

Couthouy's journal entries during the voyage indicate that he approached the question of coral island formation from the zoological perspective instructed by Peale, meaning he paid careful attention to the depths at which reef building corals could grow. He was a tireless worker who initially showed signs of being irrepressible. At Clermont Tonnerre, the first coral island encountered by the squadron, Couthouy leapt into the heavy breakers from a ship's boat that was unable to reach shore and jubilantly swam to dry land, rejoicing in being "probably the first white man" to set foot on an island where "neither Beechey or Dupperey [sic] landed, finding the surf too heavy."[25] (This was a Pyrrhic victory, because he infuriated Wilkes by remaining too long at his collecting on shore). When he was confined to a boat, he examined the corals that could be seen through clear waters beneath the boat, occasionally diving to obtain specimens and always observing the soundings if not making some himself.[26] His primary work when he was able to get ashore lay in collecting marine invertebrates. He took his specimens

23. The Couthouy journal that is held at the Boston Museum of Science is almost certainly a duplicate journal that he sent home to Boston from Sydney. It is neatly written and terminates abruptly with an entry written near "Opolu" on 29 October 1839, which corresponds with a description he gave of those "duplicate minutes of the most important of my observations from the time of our leaving the United States, to our arrival at Upolu in the Samoan group." Joseph Pitty Couthouy, "Reply of J.P. Couthouy, to the Accusations of J.D. Dana, Geologist to the Exploring Expedition, Contained on Pp. 130 and 145 of This Volume," *American Journal of Science and Arts* 45, no. 2 (1843): 380.
24. See, e.g., Couthouy journal entries for 22 July and 31 August 1839. BMOS Archives 22.
25. Couthouy journal entry for 14 August 1839. BMOS Archive 22.
26. On diving for specimens and sounding, see Couthouy's journal entry for 25 August 1839 (at Wytoohee [Napuka]in the Disappointment Islands). BMOS Archives 22.

into his cabin aboard the *Vincennes* to make sketches and descriptions of them; on more than one evening his sense of urgency saw him laboring "till near midnight merely taking rough notes of such things as cannot live until morning."[27] The squadron sighted dozens of coral islands during August and September 1839, and as Couthouy became more familiar with their general appearance he devoted an increasing proportion of his journal to comparisons between reefs. The key experience that led him to connect investigations into the depth of coral growth with the interpretation of reef formation came when he encountered Aurora Island [Makatea] on 9 September 1839. He described it as "totally different from any of the islands previously seen. It presents on all sides a perpendicular wall of coral conglomerate at least 300 feet high in some places rising directly from the sea with blue [i.e., very deep] water at a hundred yard's distance. [...] Half way or more up the cliff was an interrupted belt of excacations exactly similar to those at present worn away at the base of the cliffs by the action of the surf. The summit of the isle presented a broad plateau or table land somewhat lowest at the centre [...] the appearance of the island altogether being that of a coral reef raised up by some powerful agency to its present elevation."[28]

As Quoy and Gaimard had done two decades earlier, Couthouy examined upraised coral in hopes of drawing general conclusions about the habits of living corals. Unlike the missionary John Williams, who used Quoy and Gaimard's stated depth limit of coral growth to argue that such creatures could not have built tall islands like Aurora, Couthouy used the height of the coral rock as evidence that the Frenchmen had simply got the depth limit wrong. Working on the assumption that Aurora's perpendicular wall had originally been formed by growing corals, Couthouy asserted that the island "proves

27. Journal entry for 31 August 1839. BMOS Archive 22.

28. Journal entry of 9 September 1839. BMOS Archive 22. Dana later wrote that on approaching the island, he had supposed the cliffs to be basaltic (i.e., of volcanic origin), and found them to bear "much resemblance to the Palisades of the Hudson." James D. Dana, *On Coral Reefs and Islands* (New York: G.P. Putnam, 1853), 35.

conclusively that the calcareous Polypi construct their dwellings at [...] a depth much greater than it has been of late supposed they could exist." In his view, the island had been uplifted by two distinct events of elevation, each raising it by about 150 feet. In his journal he argued that "if the island has in this manner been elevated at two remote periods, it shews that the saxigenous Polypi construct their domiciles at a depth below the surface at least five times greater than that given by Quoy & Gaimard as the result of their experience which is 25 or 30 feet for the Astreas, the only ones capable of covering any large extent of surface." As if he had not previously realized that his observations of living corals had also long since contradicted the figure given by his French counterparts, Couthouy went on to note that "The estimate of Q & G. is certainly too low in regard to the number of feet at which Astreas are now found below the surface, as since our cruise among these islands I have myself frequently observed them in from 7 to 10 faths water [42-60 feet] in great abundance."[29]

After Aurora Island, Couthouy became fascinated by new questions that he, and the authors of the scientific instructions to the expedition, considered to lie more properly within the realm of geology. Four weeks later at Rose Island in the Samoan Group, where there were "boulders of a very heavy cellular lava" scattered across the reef, he indulged for the first time in speculation about the foundation of a coral island. He considered the boulders to be "strong evidence that the base on which the corals here rest is a volcanic rock at no great distance below the surface, since it [i.e. the volcanic foundation] was not below the action of the surf, the only imaginable power that could have placed these boulders in their present situation."[30] The notion that this coral island might have a particularly shallow foundation seemed to beg for a direct test of the strata below foot. "A more eligible location for making some experiments by boring,"

29. Journal entry of 9 September 1839. BMOS Archive 22.
30. Journal entry of 7 October 1839. BMOS Archive 22.

Couthouy remarked, “for which we have the necessary appraratus in the Expedition, can hardly be expected to occur during the cruise.” There is no record of whether he made a direct appeal to Wilkes to allow for boring through the reef, but in any case the squadron departed the island the same day.

Later that week, however, Couthouy was presented with another island that lured him into geological speculation. Aunuü was a steep-sided island that stood two hundred feet high with what appeared to be a crater in its center. He believed that “An examination of this island would be of much interest *in a geological point of view*.”[31] What was extraordinary about it was the submarine topography on its flanks. “It is a singular fact,” Couthouy reported in his journal, “that notwithstanding the abrupt manner in which this volcanic isle rises from the sea, there are soundings at 2 & ½ miles distant on a <u>coral</u> bottom distinctly visible.” Although there was no hint of it at the surface, there was an underwater shelf of coral ringing the island. Couthouy’s mind raced back to the reef-encircled islands of Tahiti and Eimeo, which the squadron had visited just two weeks earlier. He found that the coral shelf “had every appearance of being similar in nature to the reefs surrounding the Society Islands, although centuries may elapse ere the labours of the Polypi shall raise it as near the surface as are those at the present day.” If the comparison were apt, then it was possible that Aunuü was surrounded by a younger version of the reefs that encircled Tahiti and Eimeo at the surface, and this in turn might shed light on the relative ages of the high islands of the Pacific. “If this could be ascertained by a proper examination, to be the fact, would it not prove that the Polypi have been at work a much shorter period than at the Society & other barrier islands & by inference, that this group was elevated at a much later date?” Although he had noted the relevance of these thoughts for the department of geology, Couthouy was here getting right to the crux of his zoological instructions to

31. Journal entry of 11 October 1839. BMOS Archives 22. Emphasis added.

“ascertain...the depths from which the various species of Zoophytes erect their fabrics and form Islands...[and] ascertain the time requisite for the maturity of such.”

Couthouy’s attention to the conditions in which the reef forming corals were growing at the Pacific islands led him to conclude, however, that the very premise of this zoological instruction was misguided. That same day, in a private manuscript whose contents have otherwise been lost, Couthouy argued that it was temperature, and not merely depth, that limited the ability of zoophytes to form islands. “This shelf has satisfied me that that suspicion entertained on our first entering upon the Paumotus is correct,” he wrote,

> and that the growth of corals (to a certain extent) depends on temperature as much as depth. It also shows conclusively that they flourish to a depth at least three times as great as that given by Quoy and Gaimard as the result of their investigations [...] How far *below* the estimated temperature here obtained, the Polypes continue to build, can only be determined by many experiments of a more accurate nature than any I have been able to make. Probably (judging from the temperature at the islands already visited) they thrive best between 80º and 76º or 77º, and gradually decrease as the water falls below this last.[32]

This idea, that temperature might be the key to patterns of coral growth, proved central to the later tension between Couthouy and Dana.

Aunuü turned out to be just one of the many locations where Couthouy’s ambitions were thwarted and his questions were left unanswered because the squadron sailed on too soon for his liking. Part of the problem was that Wilkes’ method of surveying coral island coastlines was so intensive and speedy that it frequently left little time to actually study the islands. As Couthouy noted grimly after just a week in the Low Archipelago, “At present our opportunities are made entirely dependant on the amount of surveying that is necessary.”[33] He directed much of his anger at Wilkes,

32. Transcribed by Couthouy in Joseph Pitty Couthouy, “Remarks Explanatory of the Extent of His Views Relating to the Influence of Temperature on the Development of Corals, as Compared with Those Entertained by Jas. D. Dana, Esq,” in *Abstract of the Proceedings of the Fifth Session of the Association of American Geologists and Naturalists* (New York: Wiley and Putnam, 1844), 32.

33. Journal entry for 21 August 1839. BMOS Archive 22.

publicly as well as in the pages of his journal, for the commander's apparent disregard for the scientific men's desire to go on shore. On many occasions when Couthouy saw plenty of opportunity to land, the officers would make no boat available to the naturalists, or Wilkes, who was still angry about Couthouy's tardiness at Clermont Tonnerre, would withhold permission for them to leave the ship until the day's surveying was almost finished, at which point he would allow them on shore for the final hour or two. As Couthouy complained in his journal,

> Our greatest point of interest so far has been Honden I[sland] and there no natives could interfere with us. Yet although we were off the island from the evening of the 13th to that of the 21st, from a defect somewhere in the arrangements we were only on shore about half an hour during which time we could not move out of sight of the boats. Such opportunities were certainly never imagined by any of us to be such as would be offered by an Exploring Expedition. After it was ascertained on the 20th that the island was uninhabited had we been left on shore provided with a tent under which to pass the night, we might have commenced early the next morning and enjoyed at least 9 hours of undisturbed examination before the Surveying operations were concluded and our return on board was necessary, as it was after two in the afternoon when the Captain left the shore. If ever a case can be supposed where it was especially desirable to explore, surely this was one, not only on account of the islands being uninhabited and therefore in untouched freshness, but because from its isolated position there can be no doubt that its productions vary materially from those of the more central islands of the archipelago. It is only at places of this kind that we can expect to accomplish any thing worthy of the preparations which have been made on the account of Natural History.[34]

Speaking of the Wilkes Expedition, which has often been portrayed as a story of antagonism between scientific and naval priorities, Graham Burnett has written "in at least one critical domain of the expedition's scientific work--its hydrographic surveying (its most important knowledge-seeking endeavor)--naval discipline, and indeed naval violence, were by no means an impediment to scientific zeal."[35] Burnett makes a very convincing case as relates to hydrography *as a science*, which Wilkes certainly considered it to be. But to Couthouy's mind, at least, surveying and exploring were

34. Journal entry for 21 August 1839. BMOS Archive 22.
35. Burnett, "Hydrographic Discipline," 216.

distinct enterprises, and exploring was the true scientific pursuit. Wilkes seemed to be doing his best to make the two enterprises mutually exclusive.

> Until we came to this region, we visited no one place that had not been repeatedly examined, while within the last few days five unexplored islands have been surveyed, the whole amount of the collections from which made on board of this ship will not fill a cigar-box. This may be unavoidable, but it is certainly a misnomer at present to denominate this an Exploring Expedition.[36]

At Honden, Couthouy endured the irony of having to describe the productions of the island by studying specimens that Wilkes himself had collected on shore.[37]

Couthouy also felt that Wilkes was sabotaging his efforts even to carry out the most basic requirement of his zoological appointment, the collection and study of corals. Having stayed up late after taking a dozen specimens from inside the lagoon at the island of Raraka, he awoke the next morning to find that Wilkes had decided that the dying corals "endangered the health of the crew by producing malaria" (even twenty years later Wilkes recalled it as "one of the most nausiating smells," an aroma that "tainted the Ship in every place and was exceedingly unwholesome"[38]), and the commander had made an order that "no specimens of coral, live shells, or anything else that may produce a bad smell, will be taken below the spar deck, or into any of the rooms" of the *Vincennes*.[39] The order was disastrous for Couthouy because it precluded his evening shipboard work of describing and drawing, which in turn would seriously limit the number of specimens he would have time to treat. Wilkes later clarified that he expected Couthouy "to procure only one specimen of each species of coral which is to be as small as is consistent with the determination of its characters," which the

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36. Journal entry for 21 August 1839. BMOS Archive 22.
37. Journal entry for 21 August 1839. See his entry for 2 September for another example of Wilkes procuring specimens for Couthouy's department. BMOS Archive 22.
38. Wilkes, *Autobiography*, 431.
39. Couthouy journal entry of 31 August 1839. BMOS Archive 22. As Couthouy attests, "The words underlined here are so in the original." Wilkes reproduced the order in the appendix of Wilkes, *Narrative*, vol. 1.

conchologist considered "just the reverse of what has always been considered desirable in regard to specimens of this kind." As far as he was concerned, this was further evidence that this American expedition was not the truly scientific undertaking that its French and British predecessors had been, and he told Wilkes so. The commander responded that he "did not care a d__n for what had been done in previous Expeditions."[40] This ugly episode was the beginning of the end of Couthouy's tenure with the squadron.[41]

What Dana was thinking about coral reef formation in the meantime is not entirely clear. One certainty is that he had his own difficulties getting ashore despite being a degree removed from Wilkes by traveling aboard the *Peacock*. According to his subsequent publications on coral reefs, he complained of his limited chance to study Raraka, and admitted that he had never set foot upon Clermont Tonnerre or Rose Island, meaning that he missed two coral islands on the westward Pacific cruise that Couthouy had found instructive.[42] Dana remained vastly more tactful in his relations with Wilkes than the undiplomatic conchologist, however, saving his comments about the scientific corps' "Naval servitude" at the coral islands for a private letter to Silliman.[43] According to reminiscences by both men, Dana and Couthouy had by this time developed a firm friendship and a mutual pleasure in collaborating.[44] They had camped together on an

40. Couthouy journal entry of 31 August 1839. BMOS Archive 22.

41. There were several further conflicts between Wilkes and Couthouy in the final weeks of the Pacific cruise, notably over the custody of officers' collections and the privacy of Couthouy's notes. Couthouy's perspective is given full voice in his manuscript journal. Wilkes had his say in the *Narrative* and in his polemical and factually dubious *Autobiography*, all cited above. For a comparatively neutral account synthesized from these and other sources, see Stanton, *The Great United States Exploring Expedition of 1838–1842*, 116–47.

42. See Dana, *On Coral Reefs and Islands [1853]*, 41, 45, 128.

43. Dana to Benjamin Silliman, 12 September 1839. Quoted in Stanton, *The Great United States Exploring Expedition of 1838–1842*, 137.

44. James D. Dana, "Reply to Mr. Couthouy's Vindication Against the Charge of Plagiarism," *American Journal of Science and Arts* 46, no. 1 (1844): 129–36; Joseph Pitty Couthouy, "Review of and Strictures on Mr. Dana's Reply to Mr. Couthouy's Vindication Against His Charge of Plagiarism," *American Journal of Science and Arts* Appendix to vol. 46, no. 2 (1844); James D. Dana, "Reply of J.D. Dana to Foregoing Article by Mr. Couthouy," *American Journal of Science and Arts* Appendix to 46, no. 2 (1844).

exhausting inland journey at Tahiti, and they willingly collected specimens for one another. Subsequent events suggest, however, that Couthouy had not revealed to Dana the extent of his speculations on the foundations and relative ages of coral reefs.

The squadron arrived at Sydney, New South Wales on 29 November 1839. Couthouy was in poor health, "confined to [his] room by severe illness, the result of exposure [at Samoa], from which the physicians...pronounced recovery more than doubtful," but Dana's prospects were brightening.[45] He was introduced to the Reverend W.B. Clarke, a recent graduate of Cambridge and former student of Adam Sedgwick who had arrived in the colony earlier that year. Wilkes was planning for the expedition's second push into Antarctic waters, where he expected the scientific corps to be superfluous. He banished Couthouy to the Sandwich Islands to convalesce apart from his colleagues, and ordered the rest to remain in Australia, where they should apply themselves as they saw fit until the polar expeditionaries returned. Dana took this opportunity to join Clarke on a series of extremely profitable inland geological rambles.[46] At some point during this long southern summer, Dana learned that in England another of Sedgwick's former students, Charles Darwin, had recently offered a new explanation for the foundation of coral islands and barrier reefs.

It is generally accepted, anyway, that Dana first heard of Darwin's coral theory around the end of 1839. In the popularized 1872 revision of his coral reef book, he offered a story claiming that by sheer happenstance he had learned of it while he was in Sydney.

> Soon after reaching Sydney, Australia, in 1839, a brief statement was found in the papers of Mr. Darwin's theory with respect to the origin of the atoll and barrier forms of reefs. The paragraph threw a flood of light over the subject, and called forth feelings of peculiar satisfaction, and of gratefulness to Mr. Darwin,

45. On Couthouy's illness, see Couthouy, "Reply," 380.
46. On the collaboration between Dana and Clarke, see Appleman, "James Dwight Dana and Pacific Geology," 95–98; David R. Stoddart, "This Coral Episode," 26–27.

which still come up afresh whenever the subject of coral islands is mentioned.[47] There is no contemporary evidence to confirm Dana's account, and in searching the Sydney newspapers David Stoddart has found no notice of Darwin's theory either before or during Dana's stay there. There is ample evidence, however, that Dana gave a plausible date for this event, even if he mis-identified the means of transmission. Stoddart has demonstrated that Sedgwick told Clarke about Darwin's coral theory before the latter emigrated to Australia, and it seems likely that the subject would have come up during Clarke and Dana's field excursions.[48] John Herschel had also written of Darwin's theory (though without explaining it) to the astronomer Gipps, who was stationed outside Sydney at the Paramatta observatory (see chapter 3). By this time, the abstract of Darwin's theory had long since been published in the *Athenaeum* (1837) and the *Proceedings of the Geological Society of London* (1838). It had taken Clarke himself four months to make the passage from London to Sydney, so ample time had passed for a copy of either periodical to make its way to New South Wales.[49] One text that had undoubtedly made its way into Dana's hands was Charles Lyell's 1838 *Elements of Geology*, which he received by mail at Valparaiso, Chile the previous June, and which credited Darwin with demonstrating that "in those seas where circular coral islands abount, there is a slow and continued sinking of the submarine mountains on which these masses of coral are based."[50] For all this, the records of the voyage give little indication that Dana experienced any epiphany about coral islands at Sydney. If

47. James D. Dana, *Corals and Coral Islands* (New York,: Dodd & Mead, 1872), 7.
48. David R. Stoddart, "This Coral Episode," 26; David R. Stoddart, "Joseph Beete Jukes," 101–2.
49. Clarke's travel time (27 January to 27 May 1839) is given in David R. Stoddart, "Joseph Beete Jukes," 101. It was also possible for the notice of Darwin's theory in the *Athenaeum* to have reached Dana in the United States well before the U.S. Ex. Ex. departed. Dana's mentor Silliman re-published the *Athenaeum* abstracts from the Geological Society in his *American Journal of Science and Arts*, and Darwin's abstract on the mammalian fossils of La Plata (delivered and published less than a month before his coral paper) made it into the January 1838 number of Silliman's journal. "Geological Society," *American Journal of Science and Arts* 33, no. 1 (1838): 208–11.
50. Charles Lyell, *Elements*, 402 For Dana's reciept of the book at Valparaiso, see Prendergast, "James Dwight Dana," 165.

anything, the evidence suggests that Dana was not in a position to experience Darwin's theory as an epiphany because unlike Couthouy he had not begun to think seriously about coral reef formation during his first traverse of the Pacific.

Couthouy's journal would seem to suggest that, for his part, he either did not know Darwin's theory or did not consider it useful. On the first Pacific cruise, Couthouy's primary new opinions on coral reef formation were based on his discovery that reef building corals could live at much greater depths than was then acknowledged, and it was that shallow depth limit which created the puzzle that Darwin's theory was designed to solve. And Couthouy's comments about the relative ages of reefs seem to indicate that he imagined reefs to become established at some depth and grow upward, rather than commencing at the surface as Darwin did.

In his 1872 recollection, Dana went on to say that he began to work with Darwin's theory in mind when he returned to the tropical Pacific after his stay in Australia.

> On reaching the Feejees, six months [after arriving at Sydney], in 1840, I found there similar facts [to those Darwin had invoked from the *Beagle* voyage] on a still grander scale and of more diversified character, so that I was afterward enabled to speak of his theory as established with more positiveness than he himself, in his philosophic caution, had been ready to adopt.[51]

This passage has been widely cited ever since, to suggest that Dana confirmed Darwin's theory during the 1840 Pacific cruise. However, to my knowledge no scholar has yet attempted to use Dana's field notes from that second Pacific cruise, which are among the few of Dana's Exploring Expedition documents to have survived, to actually examine the development of his thoughts on reef formation.

Dana's two extant Expedition notebooks are not journals. They contain undigested field notes scribbled in handwriting that Dana intended only for his own

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51. Dana, *Corals and Coral Islands*, 7.

eyes. The first is entitled "New Zealand, Tonga to veejees, Sandwich Islands," but begins with an entry made at the Fijis on 6 July 1840. The second, which Dana inscribed as "Note Book no 4" and titled "Sandwich Islands, Samoa, Kingsmills," was begun where the previous one ended: "off Hawaii. Nov. 13, 1840."[52] Thus the notebooks are the product of the very period that Dana cited as pivotal in his acceptance of Darwin's theory, and more compellingly, of the Pacific island groups that formed the primary empirical examples given in his subsequent publications on coral reefs.

The first lesson to be drawn from Dana's 1840 field notebooks is that the impact of the Australian interlude on his subsequent reef studies probably had less to do with Darwin than it did with Couthouy. Because Couthouy was detached from the squadron at Sydney, Dana entered his second cruise among the coral islands with the new responsibility of tending to Couthouy's zoological department. Thus, along with descriptions of the landscapes and lithology of the high islands of the Fijis, Dana's notebooks also contain evidence that he had inherited Couthouy's obsession with collecting corals and observing their conditions of growth. On the extensive reefs of Fiji Dana noted how the corals responded to differences in tide levels, described the relative locations of various coral genera, and identified "the species wh[ich] can grow in fresh[er] water than others."[53] In noting the effects of fresh water, he added a tidbit learned from J.H. Eagleston, a merchant captain from Salem, Massachusetts who frequented the islands on the beche-de-mer trade, who told him that the anchorages most certain to be free of coral were those breaks in the reef where a river emptied from the islands.[54] Dana followed his own ship's surveyors' work with interest when it yielded specimens or provided clues as to the depth at which the reef builders were

52. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frames 388-466 and 467-513.
53. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frames 409-411.
54. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 411. On Eagleston and the "Feejee" trade, see C.S. Osgood and H.M. Batchelder, *Historical Sketch of Salem* (Salem, Massachusetts: Essex Institute, 1879), 168–73.

growing. Apparently unconstrained by the limitations that Wilkes had placed on Couthouy's collecting, Dana used methods including a "grapling iron" and a dredge to acquire a massive collection of Fiji corals.[55] The "Annotated lists of specimens received from U.S. Exploring Expedition" that are are now held at the Smithsonian indicate that one homebound shipment contained fifteen *boxes* of corals, nearly all of which came from that archipelago.[56]

The second revelation of the field notebooks is that when Dana did engage with existing interpretations of coral reef formation, he appears to have done so not in order to confirm Darwin's theory, but rather with a view to disproving the earlier crater rim theory that Darwin opposed. In the first of the two notebooks, after a set of entries from the Fiji surveys of May and June 1840, Dana made a page of notes that he subsequently headed "Coral." It included the simple statement "Submarine volcanoes have not crater form -- or lose it soon after eruption."[57] The relevance of this observation to "coral" was almost certainly that it undermined the main assumption of the crater rim theory of coral island formation suggested by Quoy and Gaimard and popularized in the English language by Beechey and Lyell. If volcanoes that erupted underwater did not produce craters, then there would be no annular foundations for shallow-water corals to encrust and turn into islands.

Dana's idea on this head probably emerged from his observations on the shape and erosion of the high islands around Viti Levu, Fiji, which were the topic of field notes written at the front end of the book. Here he remarked on the similarity of these landforms with the sharp ridges of Tahiti, pointing out the depth to which rivers had carved the volcanic rock. (This was a phenomenon whose explication was itself to be

55. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, e.g., frame 410.
56. SIA RU 7186, Box 2, Folder 6, "U.S. Exploring Expedition Annotated lists of specimens received from U.S. Exploring Expedition. 1839-1842."
57. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 461.

one of Dana's main contributions to Pacific geology, and which has also not previously been traced to these field notebooks. The notes written at this stage do not agree, however, with his later-published on the dissection of volcanoes by subaerial erosion.)[58] On a page of notes made at "Viti Lebu," Dana wrote that "Submarine...volcanic summits cannot be high [i.e., conical]-- wrong to draw conclusion of their height [or] form from subaerial summits."[59] The relevance of this specific point for Dana was likely prompted by the fact that he believed that the high islands of Fiji had been elevated from below water after being formed as submarine volcanoes.

Although the page of "Coral" notes is undated, the main entries were almost certainly written before 24 July, when Lieutenant Joseph Underwood was killed by natives at the island of Malolo in the aftermath of a failed bartering attempt. The notes refer to recent communication with Underwood and Passed Midshipman Blunt, who gave Dana information drawn from their earlier coral island surveys during the 1839 Pacific cruise. Underwood told him about the submarine topography at Clermont Tonnerre, while Blunt reported on the shape and depth of corals determined by his soundings at Rose Island. After noting these facts, Dana penned a query similar to the one that Couthouy had made, coincidentally the week the squadron had actually been at Rose Island, about the influence of temperature on coral growth. Dana's question was, "Does it not appear that the principle obstacle to large corals growing at great depths is owing to cold temperature[?] -- this is shown by not finding corals in cold latitudes."[60]

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58. It has been widely stated that the geology of Tahiti and the Sandwich Islands were the main source of Dana's views on this topic, though Stoddart has criticized Hoffmeister for assuming that the ideas were fully formed by Dana's climb inland at Tahiti. The closest study of Dana's Pacific geology, by Appleman, makes no mention of his study of subaerial landforms at the Fijis. David R. Stoddart, "This Coral Episode," 32; J. Edward Hoffmeister, "James Dwight Dana's Studies of Volcanoes and of Coral Islands," *Proceedings of the American Philosophical Society* 82 (1940): 725; Appleman, "James Dwight Dana and Pacific Geology".

59. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frames 405-407, quotation on frame 407.

60. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frames 461-462.

Dana was proposing, in other words, that the depth limit of coral growth was merely a function of the fact that water temperature declined as the depth increased.

With this prospect in mind, and having discarded the possibility that submarine craters could underlie coral islands, Dana seized upon a new explanation for that familiar puzzle of how reefs of shallow-water corals had become established in the deepest parts of the Pacific. "May not the temp[erature] of [the] seas [have] been warm when corals of coral islands first commenced to grow" he asked, "& will this account for the depth from which they are built up?"[61] Dana did not record whether he was troubled by the origin of lagoons, which was the other question to which the crater rim theory had provided an answer. Instead, in comments interlined with his promising new suggestions, he began to consider how he might strengthen his view. "What is the coldest temp[erature] of water in Lat[itude] 28º or 30º[?]" Speaking of the Australian barrier reef, where corals flourished so prolifically, he noted, "water flows from Equator along <<E>> Coast of N[ew] Holland. Coral stops at 23º or 24º [south latitude]." Finally, he underlined his hypothesis about the possibility that deep-standing reefs had been established when seas were warmer and wrote an instruction to himself: "Examine Beechey's voyage for temperature of seas."[62]

In the next set of entries, which were headed "Miscellanea," Dana finally referred to the work of Charles Darwin. But Dana did not mention the subsidence theory of coral formation here; rather, he reflected on Darwin's claim that the elevation of the east coast of South America had produced the distinctive topography there. Dana's comment was that "Darwins theory of formation of the Pampas of La Plata -- may not be correct -- the action of sea on present coast produces just the reverse effect & a gradual elevation &c."[63] Darwin had argued that the level plains of the Pampas

61. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 462.
62. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 462.
63. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 462.

were formed during gradual elevation of the continent. As Charles Lyell phrased it in the book Dana had at hand, “It appears from the observations of Mr. Darwin and others that very extensive regions of the continent of South America have been undergoing slow and gradual upheaval, by which the level plains of Patagonia, covered with recent marine shells, and the Pampas of Buenos Ayres have been formed.”[64] Dana’s perspective on the formation of the Fiji islands is revealed by his criticism of Darwin: he believed that the heavily dissected landscape at the Fijis was a product of the sea, and that a gradual elevation would further dissect it. “Submarine <<Islands of>> igneous origin when elevated rarely have distinct craters,” he wrote, “they are intersected by deep vallies wh[ich] perhaps may have been in part excavated by the agitation of the water at the time of the eruption.”[65] At this stage of the voyage, then, it would appear that Dana’s perspective on the erosion of volcanic islands was almost diametrically opposed to the one for which he is now remembered. For at this moment, he was fixated on the islands’ history of elevation as an explanation for their jagged valleys and uneven coastlines.[66]

These were the views that Dana brought to the Sandwich Island of Oahu at the end of September 1840, where he discovered that Couthouy had already been hard at work collecting widely across the archipelago. According to retrospective accounts by

64. Charles Lyell, *Elements*, 96 Although it is not certain that Dana was consulting this book as he pondered elevation, his notes on this page contain the term “metamorphic rock,” recently coined by Lyell and advanced in the *Elements*. Dana queries on elevation included, “[Are there] No inclined strata on flanks of Andes? [Are] Metamorphic rocks of Andes easily distinguished from the true igneous?” Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 461.

65. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 461.

66. Dana’s notes also mentioned the island of San Lorenzo, off the coast of South America, where he had previously argued with Couthouy over the nature of its elevation. As Couthouy’s journal entry of 22 July 1839 explains, Dana believed that superficial deposits of recent shells found up to an altitude of 100 feet at San Lorenzo had been “elevated to their present position at the period of the soil on which they rest being being [sic] forced up from the sea.” (Couthouy journal, BMOS Archive 22). In the present notes about the erosion caused during elevation, Dana appeared to be searching for a reason why San Lorenzo had not suffered greater degradation. His answer was that “faults probably formed when underwater & the island [was] smoothed down to [its] present surface by [the] action of [the] Sea.” Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 462.

each man, the geologist and the zoologist exchanged specimens and notes during Dana's stay at Oahu. Dana's manuscript inventory of material sent home from the Sandwich Islands confirms that he received 474 geological "Specimens collected by Mr J.P. Couthouy," many of which came from islands that Dana never had the chance to visit.[67] Another of Dana's inventories shows Hawaiian specimens collected jointly by "J.P.C. & J.D.D.," which indicates that they also made field excursions together.[68] I have found no contemporary record of the content of their discussions at Oahu, however, while the retrospective accounts are all highly partisan and therefore highly suspect. Wilkes claimed in his autobiography that Couthouy attempted to resume control of the conchological department that Dana had tended ever since Couthouy parted company with the expedition in Australia. According to Wilkes's often self-aggrandizing text, Couthouy "obtained possession of [the conchological specimens, drawings, and notes] from Dana and made claim to them." Wilkes, who favored Dana and detested Couthouy, instructed Couthouy to return the material to Dana and "issued an order...stat[ing] that [Couthouy] had nothing to do with them as he no longer belonged to the Expedition." To being finally and officially barred from the expedition, Couthouy responded (in Wilkes' words) "like a fool and a mad man in his abuse and determination to hold to what he had done and the results of his labours," until Wilkes ordered some officers to impound and seal Couthouy's scientific materials in tin cases and return directly to Dana the specimens that he had collected.[69] "It was only in this way," Wilkes recalled, that "I saved the results of the Expedition."[70] By Couthouy's

67. SIA RU 7186. Box 2, folder 3, "U.S. Exploring Expedition, Original Catalogue of Geological Specimens Collected at Sandwich Ids. By James D. Dana."
68. SIA RU 7186. Box 2, folder 5(D), "Original invoices and other official papers of the U.S. Exploring Expedition."
69. I have not seen the text of any such order, but Couthouy later claimed to have been "ordered home from Oahu" on 3 November 1840 and he definitel did not have possession of his journals when he traveled back to the United States. Couthouy, "Review of and Strictures on Mr. Dana's Reply," 8.
70. Wilkes, *Autobiography*, 480–81.

account there was less coercion required, for he said his "journals and note-books...were at the time I delivered them up in Oahu, secured each by several seals, bearing the impress of my own *private signet* as a safeguard against any imporper tampering with their contents."[71]Even at the height of their later enmity, Dana and Couthouy never alluded to any such open conflict, or even competition, between themselves at the Sandwich Islands. The composite picture painted by the two was of amiable cooperation, willing redistribution of specimens to the most appropriate recipient, and most intriguingly, the decision to collaborate in the preparation of a report on coral formations.

From each man's perspective, the decision to coauthor a report on corals and their productions was justified because the subject could not be confined wholly within either zoology or geology. According to Dana (writing against Couthouy in 1844), upon arriving in Oahu from the Fijis on 30 September 1840 he "la[id] before [Couthouy]" a manuscript of "over *seventy written pages*" and drawings of "more than one hundred species" of "coral animals," which the two spent nearly six hours reading together.[72] "After presenting him all my ideas and showing him the drawings," Dana claimed, "I proposed...that we should unite our labors and bring out a report together on the whole subject of corals." He deemed it a reasonable course "in view of what I had done in this branch of science [i.e. the science of corals]--the zoological part of which belonged rightly to him, and the geological to me."[73] Couthouy (writing against Dana in 1844) concurred that "I [had] neglected no opportunity of making observations on the

71. Couthouy, "Review of and Strictures on Mr. Dana's Reply," 9.

72. I have not found any such document among Dana's papers, but his "Oregon Notebook" of late 1841 contains a tally of his manuscripts which does include an entry for one called "Corals" that comprised "80 [pages] + 2 sketches." (YUL Dana Family Papers, Reel 6, frames 527-528.) This does not, of course, confirm that the coral manuscript really existed in late 1840 when Dana had last seen Couthouy. It should also be recalled that by the time Dana reached made landfall in Oregon, he had already lost manuscripts in the wreck of the *Peacock*.

73. Dana, "Reply to Mr. Couthouy's Vindication," 131.

geological structure of reefs and islands for Mr. D[ana]'s information, and it was his knowledge of htis which led to the proposition by him to publish on this subject jointly with me." He recalled that Dana had proposed authoring a joint report even earlier, "just prior to our parting in Sydney."[74]It is easy to imagine reasons why each man might have believed that he would profit from an agreement to collaborate on a report. As their notebooks reveal, each had by this time come up with exciting leaps of interpretation that might rightly be considered to belong to the other's department: Couthouy on the foundation of coral islands, and Dana on the factors limiting the growth of coral animals. According to their shared convention (and the instructions' stated protocol for the behavior of the scientific corps), such insights should rightly be given over to the man to whose department they fell. If they authored a volume together, Dana and Couthouy would each be able to have their names attached to the first publication in which their own ideas were presented. According to Dana, they spoke again on the topic after Couthouy had been permanently detached from the squadron. Notwithstanding Wilkes' apparent determination that Couthouy have nothing more to do with the expedition or its publications, the two remained committed to the plan. If indeed, as Dana claimed, "the importance [was] discussed of [Couthouy's] making observations in the West Indies, towards the joint report," they saw the conchologist's banishment as an opportunity to broaden the scope of their eventual collaboration. By both accounts, the men "parted when leaving the Sandwich Islands" with "peculiar intimacy" and "warm expressions of regard."[75]

Having settled matters with Couthouy, Dana ventured back out into the Pacific inspired by a set of new ideas born of his two months in the Sandwich Islands. Having attended particularly closely to the chain's volcanic landscape, especially during fruitful

74. Couthouy, "Review of and Strictures on Mr. Dana's Reply," 7.
75. Dana quoting Couthouy, in agreement. Dana, "Reply to Mr. Couthouy's Vindication," 130.

visits to the crater of Diamond Hill [now Diamond Head] on Oahu and the active volcano of Kilauea on Hawaii, Dana became fascinated by the spatial distribution and relative ages of the islands that made up such groups. As a number of scholars have previously explained, after comparing the broad, intact dome of Kilauea with those of extinct volcanoes in various states of erosion, Dana concluded that the time since each volcano's last activity increased sequentially from the southeastern end of the Hawaiian chain to its northwestern extremity.[76] This had several implications for his broader efforts to interpret the geology of the Pacific. First, it led him to abandon the notion that the deep valleys characteristic of igneous high islands like the Societies and the Fijis were a product of marine erosion during their elevation.[77] Instead, he saw evidence at Kilauea and Mauna Loa that igneous islands could emerge from the sea without being intersected by the deep valleys he was accustomed to seeing, and hence to conclude that such features were the product of subaerial erosion, primarily by running water.

This in turn led Dana to an insight that has since been heralded as his greatest original contribution to the subsidence theory of barrier reef and atoll formation.[78] Again contrary to the view he recorded in the Fijis, he argued that the heavily embayed shorelines of islands like Tahiti could not have been derived if the island's only motion relative to sea level had been elevation. Rather, such deep bays could only have been formed as a result of subsidence, which would allow ocean water to flood valleys that had already been formed and widened by subaerial erosion when the island stood higher. As Dana was later to argue, this interpretation offered independent evidence

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76. Notable contributions to the study of Dana's volcano work include Appleman, "James Dwight Dana and Pacific Geology"; Greene, *Geology in the Nineteenth Century*; Hoffmeister, "Dana's Studies of Volcanoes and of Coral Islands"; Prendergast, "James Dwight Dana".

77. Thus he never published this view held during the middle portion of the expedition, which is evidently why it has not been described by previous Dana scholars.

78. This claim was made first, and most vigorously, by William Morris Davis, *The Coral Reef Problem* (New York,: American Geographical Society, 1928). It is echoed in the works cited above, along with David R. Stoddart, "This Coral Episode".

that many barrier-reef encircled islands had been sites of subsidence. If Dana made this connection during the voyage, however, his manuscripts do not reflect it.

The second of Dana's Pacific notebooks demonstrates that his obsession during the first half of 1841 was to document the geographical orientation of individual islands and of island chains. During a brutally intensive surveying cruise that took the *Peacock* from Oahu back to the Samoas, and thence to a staggering number of low islands including those of the Ellice group, the Kingsmills, the Radack chain, and the Pescadores, before returning to Oahu on half-rations of water, Dana was enabled to compare a panoply of coral formations. Dana's opportunities to set foot on the islands were brief and infrequent, rarely exceeding two hours. Often his notes were based on observations taken from the deck as the ship skipped past islands that went unsurveyed. Revealing priorities that may have been dictated by the fact that he got short introductions to so many islands, Dana adopted a concise and consistent note-taking style focused on recording a handful of key variables for each locale.[79] Most important of these was the direction of what he called the "trend" or "longest axis" of an island or atoll. Throughout the notebook he underlined these facts: "trends NE & SW," "<<did not land>> trends NNE & SSW," and so on.[80] In April 1841, referring to charts and to his own notes, he began writing entries that described the direction of entire island chains and compiling the trend of each island within them. His frequent result was to illustrate that the islands often "correspond[ed] closely in their direction" to that of the chains in which they lay.[81] Although the purpose of these efforts is not spelled out in the notebook, it seems clear that Dana's object after visiting Hawaii was to uncover the

79. The style is well illustrated in Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frames 473-476.
80. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frame 475.
81. Dana Family Papers, YUL. Box 10, folder 134. Microfilm reel 6, frames 498-500.

link between the force that created islands with the one that gave them their systematic geographical arrangement.

Meanwhile, in what was the result of a baffling decision by Wilkes, another portion of the squadron was undertaking the expedition's most intensive study of a coral island without either Couthouy or Dana being present. While Wilkes' *Vincennes* remained at Hawaii so that the commander could carry out physical and meteorological observations atop Mauna Loa, and the *Peacock* and *Flying Fish* were dispatched to the islands of the west-central Pacific, the *Porpoise* under Cadwallader Ringgold was ordered back to the Paumotus to settle the location of seven or more low islands whose existence was uncertain. Wilkes' written order to Ringgold also instructed him to land a party "under a careful officer" so that they could attempt to bore through a coral reef. "You may select such an island as may seem to you most advantageous to use the boring apparatus," Wilkes ordered. "[S]pecially instruct [the officer] in its use, carefully preserving the borings for every foot in depth, in boxes properly marked; the armourer and forge will of necessity accompany the party." At the same island selected for the boring, Ringgold was to obtain a transverse section showing "the level [of the island] and soundings inside and out[side the lagoon], on all the points of the island where it is possible to obtain them, in connexion with an accurate survey of the same." On an expedition that was already running short of time, Wilkes was prepared to devote serious manpower to this study. "Between thirty and forty days is deemed ample time to secure the success of the boring experiment, which it is recommended should be continued throughout the twenty-four hours, which may easily be effected by a proper division of the force employed."[82] Lieutenant Robert Johnson was the unfortunate officer who drew this duty, "on account," as Wilkes explained, "of his ingenuity,

82. Wilkes orders to Cadwallader Ringgold, 14 November 1840. Transcribed in Wilkes, *Narrative*, vol. 4, appendix VII.

perseverance, and mechanical contrivance."[83] Though he and his party of fifteen remained for 36 days upon Carlshoff Island [Aratika], Johnson found mainly frustration with the boring apparatus. Because of "the looseness of the sand, and the falling in of the coral stones," repeated attempts to drill produced the same result, that "the pipes became choked, broke, and were thrown out of the perpendicular."[84] Although the party had comparative ease boring multiple holes to a depth of ten or eleven feet, further progress was reported to be as little as one foot per day to a maximum of twenty-one feet. This strange episode in the history of the expedition was, as far as I can tell, little remarked by Dana or anyone else, and was unmentioned in the Geology report.[85] Wilkes' rather uncharacteristic decision to order the sounding may have been prompted by a desire to outdo Edward Belcher, whom Wilkes found rude when they met at Ovalau in the Fijis just four months after Belcher had struggled to carry out Francis Beaufort's instruction to bore into a coral island (on which, see chapter 1). The similarity between Wilkes' instruction to Ringgold and Beaufort's to Belcher is suggestive, as is Wilkes' dismissive reference to Belcher's boring in the *Narrative* of the Ex. Ex., but I do not have any evidence that the two surveyors had discussed reef boring during their interview of August 1840. In any event, Wilkes' competitive feelings toward Belcher were no secret.[86]

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83. Wilkes, *Narrative*, vol. 4, 286.
84. Wilkes, *Narrative*, vol. 4, 286.
85. It should be noted that later in his life, Dana was quite scathing about the boring attempt. See, e.g., James D. Dana, "Origin of Coral Reefs and Islands," *American Journal of Science* 3rd ser., vol. 30 (1885): 104.
86. Wilkes did, however, admit that after Belcher refused to compare magnetic observations at Rewa, Wilkes had made "observations at the Same place [Belcher] had experimented with his needles and obtained the comparison after he had [published] his report." Wilkes considered "this conduct [to be] somewhat churlish on [Belcher's] part." Wilkes, *Autobiography*, 463.

The Dana-Couthouy controversy

While the Exploring Expedition was stretching into its unplanned fourth year, Joseph Couthouy made his way back to Boston in time to attend Charles Lyell's lecture on Darwin's new theory of coral reef formation. Finding Lyell's second-hand descriptions of coral islands to be fundamentally mistaken, and learning that Darwin was planning to publish a book on coral reef formation, Couthouy resolved to publish his own views immediately. Of course, the notes and journals containing his observations on coral reefs were sealed up in Wilkes' possession somewhere in the Pacific ocean. But Couthouy was the member of the scientific corps who had once been so determined to take soundings of a mountain lake at Samoa that he had, according to Dana's eyewitness report, "paddled himself across on two logs lashed together, and used a vine loaded with a stone for a lead."[87] He would not be deterred on this occasion either. Couthouy read a paper containing his "Remarks upon coral formations in the Pacific" to the Boston Society of Natural History on 15 December 1841. Dana was at that moment on the *Vincennes*, a little over half way through the passage from San Francisco to Manila via yet another stop at the Sandwich Islands.

Drawing primarily from his experiences at the Paumotus, but marshalling a remarkable range of other data as well, he argued that subsidence had played a role in forming barrier reefs and coral islands. Even as he implicitly criticized the paucity of Darwin's own field study of coral islands, Couthouy claimed that his months of experience examining low and reef-fringed high islands had impressed upon him "a conviction of the correctness of the theory here advanced by Mr. Darwin."[88] He implied

87. James D. Dana, *Geology*, vol. 10 of *United States Exploring Expedition. During the Year 1838, 1839, 1840, 1841, 1842. Under the Command of Charles Wilkes, U.S.N.* (Philadelphia: C. Sherman, 1849), 321.

88. Joseph Pitty Couthouy, "Remarks Upon Coral Formations in the Pacific; with Suggestions as to the Causes of Their Absence in the Same Parallels of Latitude on the Coast of South America," *Boston Journal of Natural History* 4 (1842): 76–77.

that he knew of Darwin's theory only by way of Lyell's lecture at the Lowell Institute, never once mentioning the long account published two years previously in Darwin's *Journal*.

Couthouy's paper was directed at what he considered to be an explictly geological phenomenon, the form and origin of "the countless coral isles and reefs, which stud the equatorial seas, especially in the Pacific and Indian Oceans."[89] The tone of the paper suggested that it was intended as a corrective for geologists, meaning Lyell in particular, and Lyell as a proxy for the science as a whole. Couthouy reviewed the reasons why coral formations had long appealed to "the researches of the geologist," citing their relevance for understanding, among other desiderata of geology, the earth's former climate, the origin of limestone and chalk formations, and the agency of small forces when allowed to act over immense time. The result of these geological researches had been "a variety of theories upon the mode in which such innumerable masses of coral have risen from the bottom of 'the vasty deep.'"[90] Of these he singled out the crater rim theory for extended consideration and criticism becase it had "obtain[ed] the sanction of some distinguished names among the geologists of Europe" (not least of whom was Lyell), and because he considered it to represent a misguided approach to the problem. Aside from the theory's shortcomings at explaining the form and distribution of coral islands, which Couthouy enumerated enthusiastically, it was unsatisfactory in principle to have a theory that could account for only a single one of the several distinct forms of coral reef.[91]Couthouy's proposal to geologists invoked subsidence as a cause of the great thickness attained by many coral formations, and as a way to explain the relation between barrier reefs and atolls, but from this point of departure he also offered several distinctive new claims. With apologies for being

89. Couthouy, "Remarks Upon Coral Formations," 66.
90. Couthouy, "Remarks Upon Coral Formations," 67.
91. Couthouy, "Remarks Upon Coral Formations," 72–76.

without recourse to his notebooks, and "rely[ing] upon memory alone," he cited masses of evidence from the Paumotus and Samoas in substantiation of a new historical account of the formation of Pacific that involved horizontal movement of the sea floor in both directions. He argued the low islands of the Pacific marked the location where a great equatorial land mass or group of islands had subsided in spurts interrupted by periods of quiescence. Evidence from Rose island and elsewhere indicated that the downward movement had ceased relatively recently, and had been followed by periods first of repose, and then of re-elevation. Introducing a major point of difference between his description of reef morphology and Darwin's, Couthouy explained that most islands with barrier reefs *also* had fringing reefs growing in the lagoon from near the shoreline toward the inner edge of the barrier reef. Again pressing the geological inadequacy of geologists' understandings of coral reefs, he said of Lyell,

> Of a fact so important in its geological bearings as the co-existence of a fringing and lagoon-enclosing reef at [Tahiti], the distinguished lecturer was, I presume, not aware, inasmuch as it was in no manner alluded to by him. [...] It would be nearer to the truth [than the Lyell-Darwin description of Tahiti], to state that instead of a continuous lagoon, there is a nearly continuous fringing reef, surrounding the island and varying from a few yards to more than a mile in width, and that the lagoons merely form canals between this and the sea reef.[92]

That sufficient time had elapsed for fringing reefs to grow almost to the point of becoming united with barrier reefs beyond them indicated to Couthouy that a period of rest had intervened between the cessation of subsidence and the beginning of "the re-elevatory process."[93] The many locations where Couthouy had witnessed level flats of coral at heights of two or three feet above high water indicated to him that the re-elevation had commenced recently and was ongoing.

Although Couthouy's first-hand experience of coral reefs was much more extensive than Darwin's, neither he nor Dana had spent as long at any single coral island

92. Couthouy, "Remarks Upon Coral Formations," 139.
93. Couthouy, "Remarks Upon Coral Formations," 140.

as Darwin did at Keeling Atoll. His account of coral reef forms differed from Darwin's as a consequence. Darwin, in the book manuscript that he was to finish the month after Couthouy read his paper, gave an extremely detailed account of Keeling and explained other reef forms as variants on this exemplary location. Couthouy, in his eighteen-page section on reef form and topography and coral zonation, on the other hand, described the general appearance of all the reefs he had seen and used different locations to illustrate separate features. He acknowledged that each of his visits to a coral island had been so "very brief" that he had never been able to study the daily course of the tide at any such location. (Recall, by contrast, how important Darwin's knowledge of the tide at Keeling had been for his interpretation of the formation of the islets' breccia layer; see chapter 2). On many points, including the means by which lagoons were conserved, the slow rate of coral growth, and the roles played in reef construction by different types of corals and calcareous algae, Couthouy and Darwin proved to be in complete and presumably independent agreement. On the other hand, while Couthouy claimed that he had "more than two years ago" come to "similar conclusions" to Darwin's on the origin of coral islands, he said that he had never "entertain[ed] [Darwin's] opinions respecting limited and definite areas of subsidence and elevation."[94]

Couthouy closed his paper by attending to the zoological question that had been his official assignment on the voyage, namely determining the factors that limited coral growth. It was as subject that had, in his opinion, "by no means received, hitherto, an attention commensurate with its importance."[95] As he had done in the notes written at Aunuü, he argued for the important role played by water temperature in dictating the abundance of reef-building corals, and proposed that Quoy and Gaimard had stated an erroneously shallow depth limit for coral growth because they had "not sufficiently

94. Couthouy, "Remarks Upon Coral Formations," 77.
95. Couthouy, "Remarks Upon Coral Formations," 158.

taken into consideration the variations of temperature at small depths, produced by accidental causes."[96] He went on to argue further that water temperature was the the main determinant of the global distribution of coral reefs. Unlike Darwin, who had sought in his 1837 coral paper to relate reef distribution to patterns of vertical crustal motion, Couthouy considered the placement of reefs to be intimately connected with the patterns of hot and cold water currents in the oceans. Giving the range of 77º to 81º Fahrenheit as the temperatures in the areas of the Pacific where corals were in "their most lavish display," he reported much colder temperatures in tropical areas where reefs were absent. This conclusion prompted him to call for a nationalized effort to gather temperature and depth data from a set of key locations including the coasts of Africa and Australia. He believed that a "connected series of observations...by direction of the Navy Department, and published in the form of tabular reports" would shed light not only on the question of reef distribution, but also on "questions relative to oceanic and (as connected with these,) atmospheric phenomena, our knowledge of which is yet in its infancy."[97]

Darwin got his hands on a copy of Couthouy's article before his book was published and he added lengthy footnotes citing favorable data from Couthouy and disputing his evidence for recent elevation (see chapter 3).

The rest of the U.S. Exploring expeditionaries returned home in June 1842 to be met by an odd combination of pubic apathy and scandalous controversy. While their achievements were less noticed or celebrated than the participants hoped they would be, mutual accusations between Wilkes and several of his subordinates were followed by courts martial that did attract wide attention.[98] Couthouy testified that the Wilkes'

96. Couthouy, "Remarks Upon Coral Formations," 74.
97. Couthouy, "Remarks Upon Coral Formations," 161–62.
98. The court martial proceedings are described in the histories of the voyage cited above. On the significance of alleged surveying errors in the proceedings, see Burnett, "Hydrographic Discipline".

language had been "offensive in the highest degree," though his ability to document the charge was compromised by the fact that he could not cite entries in his journals, which Wilkes claimed to have lost.[99] Dana did not attend the proceedings.[100]

Just as the recriminations involving Wilkes have been the source of spectacular insights into the practice of a scientific survey under naval discipline, there is much to learn about the standards of geology and zoology by studying the fracas that ensued when Dana learned that Couthouy had already published on coral formations.[101] Dana struck back at Couthouy in April 1843, at the meeting of the Association of American Geologists and Naturalists in Albany. In open discussion of a paper asserting the refrigeration of the globe, Dana pointed out that reef building corals could not grow in low temperatures, and used this datum to argue that the distribution of fossil corals well beyond the tropics indicated that the ocean had cooled during the tertiary period. Probably aware that Couthouy had made this same point in his 1842 paper, Dana raised Couthouy's name himself, and, according to the published abstract of proceedings,

> took the occasion to remark that Mr. Couthouy was indebted to himself (Mr. D[ana]) for the views there advanced by him with regard to temperature limiting corals: and added that the temperature 76º F. was a mistake by Mr. Couthouy for 70º, the limit fixed upon by Mr. Dana when the views were communicated by him to Mr. Couthouy.[102]

Four days later Dana presented two of his own papers on his study of coral reefs, which appeared in print with recognition of his status as "Geologist of the United States Exploring Expedition." In his first paper, "On the temperature limiting the distribution of corals," Dana gave a more detailed accusation. After stating that he had

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99. Couthouy quoted in Stanton, *The Great United States Exploring Expedition of 1838–1842*, 284.

100. Dana had hastened to New Haven after the voyage, where he stayed at the Silliman home. While the court martial was in session in August, Dana became engaged to Silliman's daughter, Henrietta. On Dana's non-attendance, see Couthouy, "Review of and Strictures on Mr. Dana's Reply," 3. On Dana's visit to New Haven, see Prendergast, "James Dwight Dana," 282.

101. On the former topic, see Burnett, "Hydrographic Discipline".

102. "Abstract of the Proceedings of the Fourth Session of the Association of American Geologists and Naturalists," *American Journal of Science and Arts* 45, no. 1 (1843): 145.

“ascertain[ed] the influence of temperature on the growth of corals,” which he now placed at 66º F., and claiming that this allowed him to explain the anomalous lack of coral reefs in the seas surrounding the tropical-but-cold Galapagos, and the equally anomalous presence of reefs at the extratropical-but-warm Bermudas, he insisted that

> in justice to myself I may state here, that this explanation, which was published some two years since by another, was originally derived from my manuscripts, which were laid open most confidingly for his perusal, while at the Sandwich Islands in 1840.[103]

A footnote to the published version, which appeared in Silliman’s journal, identified Couthouy as the author in question. Dana gave no critique of the substance of Couthouy’s paper. By disputing only a single digit of Couthouy’s sixty-six page essay, he tacitly indicated that the rest of its content was both accurate and plagiarized.

In the second, longer paper delivered by Dana at Albany, he brought his Expedition observations to bear on a more transparently geological topic, the question of subsidence. He addressed Darwin on his own terms, by engaging with the stated objective of Darwin’s 1837 paper to discern alternate regions of crustal movement. Dana declared that Darwin’s “theory [of the role played by subsidence] with regard to the formation of atolls, or annular coral islands, has been fully confirmed by the investigations of the Exploring Expedition.” However, in the next breath he attested that Darwin had made mistakes in delineating “his regions of subsidence and elevation,” and in drawing “the conclusion that these changes are now in progress.” Possessed of so much more field experience in the coral seas than Darwin had enjoyed, Dana charged Darwin with making generalizations that had been “deduced without sufficient examination.”[104] He believed that it would take a series of observations over time to

103. James D. Dana, “On the Temperature Limiting the Distribution of Corals,” *American Journal of Science and Arts* 45, no. 1 (1843): 130.

104. James D. Dana, “On the Areas of Subsidence in the Pacific, as Indicated by the Distribution of Coral Islands,” *American Journal of Science and Arts* 45, no. 1 (1843): 131.

determine whether vertical movements persisted to the present day, but he was willing to characterize not only the locations of past subsidence in the Pacific, but also the relative amount of downward motion in different areas. Dana promised to revise several further points of Darwin's coral work, but deferred doing so because he was not yet at liberty to discuss facts that were bound to appear first in a government publication. On balance the paper was favorable to Darwin's position, however, because the man who had now studied more coral formations than any other living naturalist was compelled to declare that "every theory" of coral reef formation must henceforth assume "extensive subsidence."[105]

Couthouy was absent from the Albany meeting where Dana made his accusation, so he responded in the next issue of Silliman's journal. He argued that it was not only plausible that he could have determined the effect of temperature on coral growth independently of Dana, but that it was in fact his obligation and his right to do so as the executor of the zoological instructions. "It must be borne in mind," he told his fellow men of science, "that in the distribution of the various departments of natural history among the naturalists attached to the expedition, the corals were specially assigned to me. Their habits, growth, distribution and all else connected with their history, were consequently the objects of my particular attention."[106] Couthouy pointed out that they had once respected each other's departments by collecting for one-another, and lamented the fact that Dana had first accused Couthouy in public despite their former intimacy. He protested that he had only published on coral reefs so hastily because he feared based on Lyell's lecture that his independent views would be preempted by Darwin's rumored "elaborate work on their distribution, &c." Unlike Mr. Dana, he

105. Dana, "On the Areas of Subsidence in the Pacific," 132.
106. Couthouy, "Reply," 379.

needled, "I deemed it highly probable that another person, observing the same facts as myself, might draw precisely the same inferences."[107]

In a daring final move, Couthouy revealed that he could prove that he had determined the role of temperature first, and independently of Dana. Even though Wilkes had "lost" the official journals and field notes seized at Oahu, Couthouy promised to provide exonerating evidence in the form of a set of "duplicate minutes" of his notes from the period before he fell ill at Samoa, which he had privately sent to friends in Boston in case his ailment proved fatal. Divulging the existence of his previously undetected manuscript was an admission that Couthouy had probably not taken lightly, because distributing such information about the expedition had been strictly prohibited. And nevertheless he could not actually produce any notes to document his claim, he explained, because this private journal was also out of his possession at present. Thus was the door left open for another set of accusations.

Although Michael Pendergrast has characterized Couthouy's letter and those that followed as the stuff of a priority dispute, the zoologist had in fact raised an issue distinct from those of priority and plagiarism.[108] He had invoked the sanctity of the scientific departments with which each man had been entrusted, and the boundary between these departments was to be the most fiercely contested terrain in the next round of the battle between Dana and Couthouy. In his reply, Dana went on the offensive again, arguing that if a department had been violated, it was his own. Pointing out that there was not only "a zoological part" but also a "geological" one in the "branch of science" that dealt with corals, Dana accused Couthouy of failing to recognize that the zoological study of corals had its limits. "Mr C[outhouy] claims in his vindication that the whole subject of corals was in his hands, much to my surprise, and no doubt to

107. Couthouy, "Reply," 381.
108. Prendergast, "James Dwight Dana," chapter 6.

the surprise of all, who know that the structure of coral islands is so far a *geological* question as to constitute an important chapter in all geological treatises. The point was considered so far settled at sea as never to have been mooted."[109] Dana neglected to mention that his original charge of plagiarism had not been in reference to the structure of coral islands, but to the growth of corals, which was almost as self-evidently a zoological question. Instead he forged on with the new accusations that simply by "publish[ing] on corals," Couthouy was "in violation of an implied agreement" to collaborate, and that the zoologist had "trespass[ed]...on the department of a friend [by] giving to the public numerous geological facts observed abroad besides those on coral islands."[110]

The disagreement could no longer be contained by the regular pages of Silliman's journal, so the editors printed a special appendix to the 1844 volume that contained a paper each by Couthouy and Dana. Silliman and company declared that "*science is no longer* the theme of discussion," and availed themselves of "the opportunity publicly to inform the parties interested, that this controversy will not again be permitted, under the covers of this Journal."[111]

Quite to the contrary, however, the vocation of science, and the question of how its labor should be divided, had now become the central theme of the dispute. Couthouy was forced to retreat from Dana's terrain and attempt to reinforce the ramparts around his own field. He took his scientific department to pertain, he said, "to living corals, to corals zoologically considered." He believed that this naturally included "the influence of temperature upon their growth." As to Dana's charge of trespassing on geological turf, he protested that it had been precisely "for Mr. D[ana]'s information" that he had "neglected no opportunity of making observations on the geological structure of reefs

109. Dana, "Reply to Mr. Couthouy's Vindication," 131 Original emphasis.
110. Dana, "Reply to Mr. Couthouy's Vindication," 132.
111. "Editorial Remark," *American Journal of Science and Arts* Appendix to 46, no. 2 (1844).

and islands."[112] Though he admitted to discussing "geological subjects" in his article on coral formations, this had been an "entirely incidental and unpremeditated" act that Couthouy lamented had "expand[ed] under my hands to an extent far beyond my original idea, which was simply to point out an erroneous statement by Mr. Lyell, in regard to the structure of the reefs bordering Tahiti."[113]

On the offensive again, Dana fired back with a criticism of Couthouy's zoological work. He noted derisively that when they had compared notes at Oahu, Couthouy possessed illustrations of no more than a dozen coral species, while Dana's portfolio, bolstered by his efforts at the Fijis while Couthouy was absent, contained drawings of more than a hundred.[114] Dana seemed barely able to restrain himself from saying that his decision to make Couthouy his co-author was made because he wanted access to, if not control over, the zoological as well as the geological publication. If that was the motivation behind his offer at Oahu, then the gesture was unnecessary. Couthouy's days with the squadron were numbered, and his banishment ultimately extended to the Ex. Ex. publications as well.

Dana also spoke dismissively of the geological observations that Couthouy had made on coral reefs when they compared notes at Oahu, remarking that "I had seen Mr. C.'s drawings, but had never given his geological investigations on corals another thought."[115] This was probably because Couthouy had withheld his best material, whether geological or otherwise. Couthouy's surviving journal (which is almost certainly the set of "duplicate minutes" written at Samoa) provides ample evidence that he had long been deeply intrigued by the question of coral islands' foundations. They also indicate plainly that he conceived of this as a geological topic, but it seems unlikely

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112. Couthouy, "Review of and Strictures on Mr. Dana's Reply," 7.
113. Couthouy, "Review of and Strictures on Mr. Dana's Reply," 4.
114. Dana, "Reply of J.D. Dana to Foregoing Article by Mr. Couthouy," 11.
115. Dana, "Reply of J.D. Dana to Foregoing Article by Mr. Couthouy," 11.

that Dana saw them at Oahu. Moreover, Couthouy had indeed devoted considerable energy to studying the effect of temperature on the growth of corals. In what Couthouy called a "manly acknowledgement," Dana readily confirmed this fact at the 1844 Geologists and Naturalists meeting after Couthouy had finally presented him with the long-lost notes.[116] Yet it also seems unlikely that Couthouy ever shared this information with Dana during the voyage. Dana's Fiji notes offer convincing evidence that he drew his conclusions on temperature there without Couthouy's input or any intuition that the ideas were not original. It is conceivable that Couthouy never discussed the matter with Dana at Oahu because he considered it a purely zoological topic. It is also easy to sympathize with Couthouy's protestation that he had been utterly preoccupied at Oahu by the fact that Wilkes was on the verge of seizing his papers and detaching him from the expedition. For this very reason, and given that Dana was already the interim custodian of Couthouy's department, sharing his best work with Dana was probably Couthouy's last intention.

James Dwight Dana and the coral sciences at mid-century

Couthouy ended up winning the battle over priority, but Dana prevailed in his covert war for the department of zoology. Couthouy foresaw this in his talk at the 1844 meeting, where he took the opportunity of Dana's withdrawal to state what he considered his original contributions to be. Couthouy claimed to have originated two "theories or principles" relating to corals: that their growth was limited by water temperature, regardless of depth, and that the absence of corals in certain areas was

116. James D. Dana, "Acknowledgement of J.D. Dana Relative to a Charge of Plagiarism," in *Abstract of the Proceedings of the Fifth Session of the Association of American Geologists and Naturalists* (New York: Wiley & Putnam, 1844), 30; Joseph Pitty Couthouy, "Acknowledgement of J.P. Couthouy Relative to a Charge of Plagiarism," in *Abstract of the Proceedings of the Fifth Session of the Association of American Geologists and Naturalists* (New York: Wiley & Putnam, 1844), 30.

caused by cold currents. But Couthouy admitted that Dana had completed the expedition, and that for this reason it would be Dana, and not Couthouy, who would be in the position to judge the validity of these principles. "The time and opportunity for more extensive observation which were denied to me, it was the peculiar good fortune of Mr. Dana to enjoy...*His* is the rich harvest of facts, and their application in a wide field of observation."[117] Couthouy's statement was gracious, but it was also self-interested. It was not simply what he had seen during the voyage that would make Dana the judge, it was what he could see after the voyage. The specimens and drawings were government property, and it was Dana who would have access to them all, Couthouy's and his own. It was Dana, who had always been as tactful toward Wilkes as he had been tactless toward Couthouy, who ended the voyage in good graces and received the commission to write not only the official volume on geology, but also the one on the zoophytes.

Owing to the decades-long fiasco that was the production of Ex. Ex. scientific volumes, the record of Dana's publications on corals, coral reefs, and general zoology is extremely convoluted.[118] The official documents were his Expedition reports on zoophytes (1846) and geology (1849).[119] Because these publications were limited by Congress to a printing of 100 copies, Dana took a series of steps to make the contents accessible to a wider range of his colleagues. The contents of the zoophyte volume, which re-ordered the entire taxonomy of corals despite injunctions by Wilkes and his congressional allies that Dana limit himself to describing specimens collected during the

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117. Couthouy, "Remarks Explanatory..to the Influence of Temperature," 34–35.
118. The best sources of information on the publications of the Ex. Ex. are two bibliographies: Harley Harris Bartlet, "The Reports of the Wilkes Expedition, and the Work of the Specialists in Science," *Proceedings of the American Philosophical Society* 82 (1940): 601–705; D.C. Haskell, *The United States Exploring Expedition and Its Publications 1844–1874* (New York: New York Public Library, 1942).
119. James D. Dana, *Zoophytes,* vol. 7 of *United States Exploring Expedition. During the Year 1838, 1839, 1840, 1841, 1842. Under the Command of Charles Wilkes, U.S.N.* (Philadelphia: C. Sherman, 1846); Dana, *Geology*.

voyage, were reprinted in serial in Silliman's journal over the course of two years.[120] As Dana complained to Asa Gray, "in a Science in such an unsettled state as that of Zoophytes, patching on new species was unpracticable without a thorough revision. [...] You know the merits of the case. The law authorized printing 'Expedition discoveries[']"[121] In Dana's mind, and indeed Gray's, the new classification was an expedition discovery of even greater note than that of any individual new species of coral. Far from being a purely taxonomic work, however, the zoophyte volume offered a full natural history of the "habitudes" of polyps, with special attention to the forms of coral stone and the way it was produced by the polyp. Dana took evident pleasure in skewering the "poets" who romantically described corals as the homes of the polyps and coral reefs as the product of the polyps' "labors." The stony secretion known as coral, he explained, was entirely internal to the living polyp, and no more a product of its labor than Dana's skeleton was a result of his own industry.

Dana's monumental geology volume contained, besides several hundred pages analyzing the localities visited on the voyage, interpretive chapters on coral formations and the geology of the Pacific ocean that comprised another two hundred pages. Dana serialized the coral reef material in Silliman's journal in the early 1850s and released it as a stand-alone book unaffiliated with the Exploring Expedition volumes.[122]

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120. James D. Dana, "On Zoophytes," *American Journal of Science and Arts* 2, no. 1 (1846): 64–69; James D. Dana, "On Zoophytes, no. II," *American Journal of Science and Arts* 2, no. 5 (1846): 187–202; James D. Dana, "On Zoöphytes, no. III," *American Journal of Science and Arts* 3, no. 7 (1847): 1–24; James D. Dana, "On Zoöphytes, No. IV," *American Journal of Science and Arts* 3, no. 8 (1847): 160–63; James D. Dana, "On Zoöphytes, no. V," *American Journal of Science and Arts* 3, no. 9 (1847): 337–47.

121. Dana to Gray, 8 March 1846. HU-GH.

122. James D. Dana, "On Coral Reefs and Islands; Part I," *American Journal of Science and Arts* 11, no. 33 (1851): 357–72; James D. Dana, "On Coral Reefs and Islands; Part II," *American Journal of Science and Arts* 12, no. 34 (1851): 25–51; James D. Dana, "On Coral Reefs and Islands; Part Third," *American Journal of Science and Arts* 12, no. 35 (1851): 165–85; James D. Dana, "On Coral Reefs and Islands; Part Fourth," *American Journal of Science and Arts* 12, no. 36 (1851): 329–38; James D. Dana, "On Coral Reefs and Islands; Part Fifth," *American Journal of Science and Arts* 13, no. 37 (1852): 34–41; James D. Dana, "On Coral Reefs and Islands; Part Sixth," *American Journal of Science and Arts* 12, no. 38 (1852): 185–95; James D. Dana, "On Coral

By the end of the 1840s, Dana could fairly consider himself the world's foremost authority on all scientific matters relating to Pacific corals and their formations. He had taken full advantage of the breadth of research topics allowed him by Couthouy's dismissal from the voyage, and of the breadth of experience presented by his extended cruises through the Pacific's richest coral zones, to acquire a familiarity with atolls and barrier reefs that dwarfed Darwin's first hand knowledge. He had also taken advantage of the financial support grudgingly provided by the U.S. Congress for specialist work on the Ex. Ex. collections, which enabled him to devote the better part of a decade to full-time research and writing on zoological and geological topics. The intensity and duration of his labors startled his colleagues. What his coral reef work lacked in originality by appearing after Darwin's was compensated by the weight and quality of evidence that Dana commanded.

When Darwin read the coral reef sections of Dana's *Geology* in 1849, he did so knowing that the American had done in person what Darwin had been forced to do by studying charts. Before Darwin had even finished reading the volume, he wrote to Dana to tell him, "You cannot imagine how much gratified I have been, that you have, to a certain extent agreed with my coral-island-notions." The value of Dana's opinion lay in his obvious experience. As Darwin congratulated him, "your range of research [has been] a wide one."[123] Writing to Lyell, Darwin could not restrain his "exuberance of vanity." Having read only through the "coral reef part," he confessed, he could find no point upon which Dana disagreed with him. "Considering how infinitely more he saw of Coral Reefs than I did," Darwin reported, "this is wonderfully satisfactory to me." In

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Reefs and Islands; Part Seventh," *American Journal of Science and Arts* 13, no. 39 (1852): 338–50; James D. Dana, "On Coral Reefs and Islands; Part Eighth," *American Journal of Science and Arts* 14, no. 40 (1852): 76–84; James D. Dana, "On Changes of Level in the Pacific Ocean," *American Journal of Science and Arts* 15, no. 44 (1853): 157–75; James D. Dana, "On the Consolidation of Coral Formations," *American Journal of Science and Arts* 16, no. 48 (1853): 357–64; Dana, *On Coral Reefs and Islands [1853]*.

123. Darwin to J. D. Dana 8 October 1849.

fact, he thought, it reflected poorly on Dana that he had found so little to add to Darwin's theory.[124] Darwin had not yet read the whole book, as he admitted to Dana in another letter several weeks later, "but in the descriptive part, our agreement has been eminently satisfactory to me, & far more than I ever ventured to anticipate.-- I consider that now the subsidence theory is established."[125] Gradually, however, he began to notice that Dana had found room to disagree with Darwin, notably by stating (as Couthouy had) that many atolls showed signs of recent elevation and by commenting unfavorably on Darwin's use of fringing reefs as evidence that subsidence had not occurred. Darwin was most agitated that Dana "does not condescend to notice my explanation for such appearances," as he told Lyell in a huff, "Dana puts me in a passion several times by disputing my conclusions, without condescending to allude to my reasons...He strikes me as a very clever fellow; I wish he was not quite so grand a generaliser."[126] Given that Darwin coral book was based on much narrower field experience than Dana's, and that he had quivered with anxiety that his book would be deemed too speculative, this comment may have rung hollow to Lyell.

Notwithstanding their awkwardness in assigning credit to one another, Darwin and Dana became warm correspondents as each turned his attention away from coral reef studies. As Stoddart has pointed out, their mutual interest in mollusks soothed the tension and provided the main substance of their interaction and growing mutual respect in the 1850s.[127] At the end of the decade, however, when Darwin passed from his work on barnacles to his publications on the origin of species, he knew he would have to tread carefully with the devout Yankee. Because Dana's health had crumbled, there was all

124. Darwin to Charles Lyell 4 December [1849].
125. Darwin to J. D. Dana 5 December [1849].
126. Darwin to Charles Lyell [7? December 1849].
127. David R. Stoddart, "This Coral Episode."

the more reason for Darwin to send a delicate letter to accompany the presentation copy of his new book in November 1859.

> I have sent you a copy of my Book (as yet only an abstract) on the Origin of species. I know too well that the conclusion, at which I have arrived, will horrify you, but you will, I believe & hope, give me credit for at least an honest search after the truth. I hope that you will read my Book, straight through; otherwise from the great condensation it will be unintelligible. Do not, I pray, think me so presumptuous as to hope to convert you; but if you can spare time to read it with care, & will then do what is far more important, keep the subject under my point of view for some little time occasionally before your mind, I have hopes that you will agree that more can be said in favour of the mutability of species, than is at first apparent. It took me many long years before I wholly gave up the common view of the separate creation of each species.
>
> Believe me, with sincere respect & with cordial thanks for the many acts of scientific kindness which I have received from you, My dear Sir
>
> Yours very sincerely
> Charles Darwin[128]

Unlike his friend Gray, Dana never became an American bulldog for Darwin's principle of natural selection. But when, in the last years of Darwin's life, a new generation of scientists threatened their common views on reef formation, Dana was to prove a more than capable defender of what he was then willing to term a "Darwinian" theory.

In the meantime, however, the subsidence theory became mid-century orthodoxy with a place in the intellectual cargo of all new reef voyagers. Some treated it as a default explanation for reef formation, others sought more positive evidence of subsidence. But from the late-1840s, virtually every new scientific description of a reef included a declaration either for or against subsidence.

I have already mentioned (in chapter three) the positive testimony of Joseph Beete Jukes, a geological "son" of Adam Sedgwick who examined the Australian barrier during Francis Blackwood's hydrographic survey on the *Fly* (1842-1846).[129] In

128. Darwin to James Dwight Dana, 11 November [1859].

129. David R. Stoddart, "Joseph Beete Jukes". On the Hydrographic Office dispatch of the *Fly*, see Cock, *Sir Francis Beaufort*; Jordan Goodman, *The* Rattlesnake*: A Voyage of Discovery to the Coral Sea* (London: Faber and Faber, 2005), 12–13. Jukes identified himself as a "son" of Sedgwick; see Secord, *Controversy in Victorian Geology*, 58.

his 1847 narrative of the the voyage he claimed that he had "tried hard to find any substantial objection to [Darwin's subsidence] hypothesis, and must confess I failed to do so."[130] Contrary to what Darwin would have predicted, Jukes believed that during the most recent two or three thousand years the northeast coast of Australia and the barrier reef had remained stationary or been slightly elevated relative to sea level. But he did not consider this finding damning to Darwin's theory, "because, previously to this [2,000 to 3,000 year period], depression might have been taking place throughout a far more extensive period." Jukes argued that the subsidence hypothesis was the only plausible way to account for the relation between the coastline and the reef. He explained that the Admiralty charts of northeast Australia would show that the reef followed the "curves and flexures" of the shore with "quite sufficient conformability to show that the two are connected." The apparent correspondence between the coastal topology and the offshore pattern of reefs and channels led Jukes to declare that "every modification in the form and structure of the reefs is explicable by [the susidence] hypothesis, and many difficulties solved, which admit of no other explanation." Because he had found it impossible to disprove Darwin's explanation for the origin of reefs like the Australian barrier, Jukes concluded that it "rises beyond a mere hypothesis into the true theory of coral reefs."[131] I have not been able to examine anything Jukes wrote while he was actually at the Great Barrier Reef to gauge how Darwin's theory figured into his fieldwork, and I do not deny the likelihood that this junior member of the Cambridge scientific network may have found it professionally expedient to give Darwin's theory the benefit of the doubt in his published work.[132] But what this published work suggests is that Darwin's theory was considered sufficiently useful that

130. Jukes, *Narrative of the* Fly, vol. 1, 343–44.
131. Jukes, *Narrative of the* Fly, 347.
132. Jukes needed paid employment after the voyage. In 1846 he took a position under De la Beche with the Geological Survey. See Secord, *Controversy in Victorian Geology*, 205–6.

in the absence of new contradictory evidence, it would remain the default explanation for barrier reefs and atolls.

In contrast to Jukes, Harvard's Swiss-American naturalist Louis Agassiz decided that subsidence was simply unnecessary for explaining the reefs he studied off the coast of Florida for the U.S. Coast Survey in 1851. He argued that although "the idea of subsidence and upheaval is naturally connected with the features of coral reefs," the "natural consequence of the growth of reef building corals" could account for the production of the Florida barrier reef and the coral islands of the Florida keys.[133]

Reef formation was a theoretical problem for Jukes and Agassiz because British and American authorities considered the formation of new reefs to be a practical obstacle to safe and predictable maritime commerce. Jukes's Captain Blackwood was sent to survey the Torres Strait at the northern end of the Great Barrier because so many vessels had been lost after becoming "entangled within the reefs," and as Francis Beaufort exhorted him "Do not hurry over the hidden dangers which lurk *and even grow* in that part of the world."[134] Beaufort's American counterpart, the Superintendent of the U.S. Coast Survey Alexander Dallas Bache, had instructed Agassiz not merely to study the formation of reefs, but to determine "whether the growth of coral reefs can be prevented, or the results remedied, which are so unfavorable to the safety of navigation."[135] Agassiz argued that there was no possibility of checking the spread of reef building corals "beyond the bounds which nature itself has [already] assigned to their growth," and urged instead that a close knowledge of the natural tendencies of reef

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133. Louis Agassiz, "Extracts from the Report..on the Examination of the Florida Reefs, Keys, and Coast," Appendix 10 in *Annual Report of the Superintendent of the Coast Survey..for 1851*, Alexander Dallas Bache (Washington, 1852), 156.

134. "Proposed orders for Captain Blackwood." UKHO MB 3: September 1837-May 1842, pp. 409-416. Francis Beaufort to Francis Blackwood, UKHO LB 12, quoted in Goodman, *The* Rattlesnake, 13 (emphasis added).

135. Agassiz, "Florida Reefs," 158.

formation would allow mankind "at least to avoid the evil consequences."[136] For example, Agassiz declared, in the Floridian case he believed that the reefs would grow no farther out from shore because the slope of the sea bottom outside the present-day reef sank "rapidly to unfathomable depths" and left "no opportunity for the growth of a new reef."[137] In Britain, Roderick Impey Murchison hailed Agassiz's work in his 1853 presidential address to the Royal Geographical Society. "Professor Agassiz has successfully shown how all such surveys ought to be made in conjunction with naturalists. For, quite independent of the important additions to natural-history knowledge which are obtained, statesmen as well as hydrographers thus ascertain the causes of increase or decrease of coral reefs, and learn, that whilst no human power can arrest the growth of such reefs, there are channels amidst them which will remain deep for long periods of time, and the outlines of which, when well defined by lighthouses, may be the salvation of much life and property."[138]

The durability of Darwin's coral theory was reflected in the steady demand for copies of his 1842 book. In December 1873, the 64-year-old Darwin wrote to his former publisher, Smith & Elder, to inquire about reprinting it. He reported that his friends had been unable to buy copies when they tried, and that his son Horace had recently learned that a Cambridge bookseller spent three years trying to locate the book before finding a copy in Berlin.[139] Darwin proposed to "add a few notes" to the text, but warned that he would not have time to correct the proofs himself. At this time, his energies were devoted mainly to producing a second edition of *The Descent of Man* for the publisher John Murray. When Smith & Elder agreed to bring out a revised edition

136. Agassiz, "Florida Reefs," 158.
137. Agassiz, "Florida Reefs," 159.
138. Roderick Impey Murchison, "Presidential Address to the Royal Geographical Society of London," *Journal of the Royal Geographical Society of London* 23 (1853): cxvi-cxvii.
139. Darwin to Smith, Elder & Co., 17 December [1873]. DAR 96:159-160. See also Charles Darwin to Horace Darwin, 9 January [1874]. DAR 185.

of *Coral Reefs*, Darwin suddenly found himself in the same position as the hapless book merchant, looking for a copy of the first edition that he could use as the basis for his updated text. He wrote to Hooker to ask, "Did I give you a copy of my Coral reefs book? If so I wish you w[ould] give it me, & I will let you have a copy of a new edition; for I cannot buy one, & yet want a copy very much for correction."[140] Within ten days he had two copies in hand, courtesy of Hooker and Lyell.[141] Although he bemoaned the time he spent preparing it, Darwin dealt very rapidly with "the cursed 2d. Edit," and new sheets began rolling off the presses in March 1874.[142]

The bulk of the changes incorporated into the 1874 edition of Darwin's *Coral Reefs* were responses to the work of Dana and a few others. Some of the new parts were silent revisions, but many were annotations clearly meant to preserve on record the exact wording of the original edition. In a brief preface dated February 1874, Darwin declared that in the 32 years since his first coral reef book had been published, "only one important work on the same subject has appeared, namely, in 1872, by Professor Dana, on Corals and Coral-Reefs."[143]

Murchison's statement may also be read as an exhortation on behalf of individuals who wanted to take advantage of the scientific opportunities offered by such large state-funded projects as surveys and voyages of exploration.[144] Simultaneously, however, British marine scientists were beginning to organize oceangoing research on their

140. Darwin to Hooker, 8 January 1874. DAR 95:310.

141. See Darwin to Hooker, 18 January [1874]. DAR 95:311-312. He decided to use Lyell's copy, which had arrived first, and returned Hooker's copy immediately.

142. Darwin to Hooker, 4 March [1874]. DAR 95:313-316.

143. Charles Darwin, *The Structure and Distribution of Coral Reefs*, 2nd ed. (London,: Smith Elder & co., 1874), v.

144. Indeed, just two years later Murchison took the directorship of the Geological Survey of Great Britain in order to ensure that the institution would continue to hold a research orientation. Secord, *Controversy in Victorian Geology*, 273.

own behalf, first in private yachts and eventually in naval craft like the *Lightning*, the *Porcupine*, and most famously, the *Challenger*.[145]

The new British challenger to the subsidence theory

That the voyage of H.M.S. *Challenger* (1872-1876) should produce a foil to Darwin and Dana was only fitting. It was an undertaking commissioned in part as a direct investigation of natural selection in particular, and of the phenomenon of Darwinism in general.[146] As an expedition in which civilian scientists would direct the activities of a naval vessel, it was in an important respect diametrically unlike the voyages that had delivered Darwin and Dana to locations unexpected. It was also, profoundly, not a coastal surveying mission. The voyage manuscripts by Darwin and Dana attest, often in tones of profound anguish, the extent to which they were held hostage to officers who would neither land nor depart from some featureless coast. To be sure, they each endured many long passages under sail. But their shipboard experiences were exquisitely oriented toward that point of intersection between the surface of the sea and the edge of the land, and this nexus, not coincidentally I think, very often served as a target and a resource for their intellectual energies.

The five volumes of John Murray's *Challenger* diary reveal that this expedition was truly unmoored from the land and oriented toward the deep sea. Murray was a Canadian-born Scot, now remembered as a founder of oceanography but then considered a naturalist, who spent much of the four-year cruise studying bottom deposits brought up by deep soundings and dredging. It is perhaps not a surprise that

145. Rozwadowski, *Fathoming the Ocean*, chapter 4.

146. Margaret Deacon, *Scientists and the Sea, 1650–1900: A Study of Marine Science*, 2nd ed (Aldershot: Ashgate, 1997); Susan Schlee, *The Edge of an Unfamiliar World: A History of Oceanography* (New York: E.P. Dutton, 1973); Rozwadowski, *Fathoming the Ocean*.

this was where the action happened in Murray's theory of coral reef formation. Murray's focus on deep sea deposits encouraged him to envision new answers to the old question of what could lie beneath an atoll. He argued that there was a greater number and type of organisms whose remains could contribute to the structure of a deep ocean reef. Crucially, his argument against Darwin's subsidence theory was that subsidence was *unnecessary*.

Murray did comparatively little research on coral reefs themselves: arguably he spent even less time than Darwin had on firsthand reef study. In the early months of the voyage, at the Bermudas in March 1873, he "hired a Bermuda boat and with a Marine...examined the corals &c at the bottom...and fished up some specimens."[147] Other specimens were obtained by dredging, but the main work was deep sounding off the islands and studying specimens in the "work room" aboard the ship. Notably, the first impression he registered of reef formations in his entire journal was of the distinctive cup-shaped features that lie close to the shores of the large islands.[148] He examined these "little atolls -- about 20 feet in diameter" and "found them to be composed entirely of [the tube worm] Serpula not of coral."[149] These, then, were reef structures composed of organisms other than stony corals, and whose annular shape and central depression were apparently not a result of subsidence. I am not suggesting that this determined his future explanation for the formation of atolls, but it surely helped him to be aware that reefs did not have to be made entirely of corals, and that the combination of solution and erosion could make lagoons.

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147. *Challenger* diary of John Murray, ESL, NHM. Volume 1, 15 March 1873.
148. These are one of the only natural phenomena discussed in this dissertation that I have actually seen myself. I am grateful to Professor Jim Gould (Princeton) and the staff of the Bermuda Institute for Ocean Sciences for making this possible. For a recent analysis of their composition and formation, see R.N. Ginsburg and Johannes H. Schroeder, "Growth and Submarine Fossilization of Algal Cup Reefs, Bermuda," *Sedimentology* 20, no. 4 (2006): 575–614.
149. *Challenger* diary of John Murray, ESL, NHM. Volume 1, 14 March 1873.

When he was not writing about birds (especially albatrosses) in his journal, Murray spent the voyage obsessing over the nature of deep sea deposits. But for the profoundest depths, the bottom was often blanketed with calcareous and siliceous material derived from the remains of microscopic organisms that in life floated in the shallow pelagic zone of the ocean. From isolated dredgings at Oahu in August 1875, meanwhile, he began to suspect that reef building corals lived deeper than he had been led to believe.

The bulk of Murray's coral observations and reef study came during a few days at Tahiti in September 1875. On the 20th, he "Dredged off the reef in from 12 to 40 fms -- Feel sure some corals live to the latter depth."[150] The next day he accompanied Lieutenant Swire in the pinnace as Swire conducted a series of soundings that would allow them to "determine the slope of the reef." While Swire was working, Murray reported, "With the water glass I could see the coral growing to 18 fms." Synthesizing the information gained from dredging, peering, and sounding, Murray summed up his conclusions about the reef:

> At [a distance of] from 125 to 150 fms from the edge of the reef we generally got...to the limit of living coral in the lateral direction. And the depth was usually from 30 to 40 fms.
> I feel very certain that some of what are called the reef building corals live down to these last depth[s], for repeatedly the dredge brought up specimens from this depth. -- We took care to determine the depth when we put the dredge over, while it was over, and before we heaved in. The specimens have been preserved and labeled so that the species may be determined.[151]

These observations proved to be of no more than secondary importance when Murray published a new explanation for the formation of coral islands in 1880.

The key to Murray's theory was the organic detritus that had been the main

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150. *Challenger* diary of John Murray, ESL, NHM. Volume 4, 20 September 1875.
151. *Challenger* diary of John Murray, ESL, NHM. Volume 4, [note written on or after 1 October 1874].

target of his research during the voyage. "In order to clearly understand how a submarine mountain, say half a mile beneath the sea, can be built up sufficiently near the surface to form a foundation on which reef-forming corals might live," he explained, "it is necessary to consider attentively the *Pelagic Fauna and Flora of Tropical Regions*."[152] Murray reported that the *Challenger*'s scientifics had found the ocean to be full of pelagic plants and animals in the depths between sea level and about a hundred fathoms. These were carried by currents and provided food for corals, which explained the noted luxuriance of windward, as compared to leeward, reefs. But the key role that Murray had in mind for these planktonic organisms was played when they died and began to sink. Before their calcareous skeletons and shells could reach the bottom of the deepest parts of the ocean, he had determined, they were dissolved by the carbonic acid in seawater. But

> In the shallower depths--on the tops of submarine elevations or volcanoes--the accumulation of the dead silicious and calcareous shells is too rapid for the action of the sea water to have much effect. Long before such a deposit reaches sufficiently near the surface to serve as a foundation for reef-forming corals, it is a bank on which flourish numerous species of Foraminifera, Sponges, Hydroids, deep sea Corals...&c. All these tend to fix and consolidate such a bank [...] Eventually coral-forming species attach themselves to such banks, and then commences the formation of *Coral Atolls*.[153]

Darwin had admitted that reefs "not to be distinguished from an atoll" might form on submarine banks, but (as discussed in chapter 3) dismissed this as a possible cause for the true atolls of the deep ocean. Murray countered that the above mentioned accumulation of calcareous and siliceous matter meant that such banks were likely to be extremely common: "As here stated, recent deep-sea investigations have shown that submerged banks are continually in process of formation in the tropical regions of the

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152. John Murray, "On the Structure and Origin of Coral Reefs and Islands," *Proceedings of the Royal Society of Edinburgh* 10 (1880): 507 Original emphasis.

153. Murray, "On the Structure and Origin of Coral Reefs and Islands," 510.

ocean, and it is in a high degree probable that the majority of atolls are seated on banks formed in this manner."[154]

Murray needed a separate explanation to account for the formation of lagoons and barrier reefs. Lagoons he saw partly as the result of differential growth on the inside and outside of a bank of coral, and cited *Challenger* tow-net experiments that demonstrated "very much less Pelagic life (food) in the lagoon waters than on the outer edge of the reef."[155] But they were equally determined by processes of erosion and solution, and here he proved that the example from two-and-a-half years before visiting Tahiti had remained with him. "Complete little Serpula-atolls, with lagoons from 3 to 50 feet in diameter, and formed in this way without subsidence, were numerous along the shores of Bermuda."[156] Barrier reefs, he believed, were produced by the inevitable outward growth of a fringing reef as it colonized the platform (called a talus, after the ramps of dirt built up at the base of medieval fortifications) generated by the accumulation of coral rubble at its base. Speaking of the Tahiti barrier reef, "Everything appears to show that the reefs have commenced close to the shore and have extended seawards, first on a foundation composed of the volcanic detritus of the island, and afterwards on a talus composed of coral debris, and the shells and skeletons of surface organisms."[157] Behind this advance, the widening of the lagoon would be ensured by solution, erosion, and coral malnutrition.

Murray offered scant evidence to support the contention that his explanation reflected a true, and not merely a plausible, mechanism for the production of atolls and barrier reefs. Murray's only piece of evidence to suggest that ocean reefs actually had formed this way came from a passage in Wilkes' US. Ex. Ex. *Narrative*: "It is a

154. Murray, "On the Structure and Origin of Coral Reefs and Islands," 511.
155. Murray, "On the Structure and Origin of Coral Reefs and Islands," 511.
156. Murray, "On the Structure and Origin of Coral Reefs and Islands," 512.
157. Murray, "On the Structure and Origin of Coral Reefs and Islands," 514–15.

remarkable fact," Murray wrote, "that, in all coral atolls which have been raised several hundred feet above the sea, the base is generally described as composed of solid limestone, or 'of various kinds of coral evidently deposited after life had become extinct.'"[158] Meanwhile, he had found that subsidence did not seem to explain what he had seen at Tahiti. "Professor Semper, during his examination of the coral reefs of the Pelew group, experienced great difficulties in applying Darwin's theory. Similar difficulties presented themselves to the author in those coral reef regions visited during the cruise of the 'Challenger.'"[159]

Murray based his case in large part on a novel conception of parsimony as related to oceanic processes. He argued that it was simply "much more natural" to view atolls as a byproduct of the accumulation of other types of organic sediment than as the results of subsidence. He cited Jukes, Couthouy, and Dana for evidence of recent elevation of oceanic reefs, and argued that "this is what we should expect." "Generally speaking, all the volcanic regions which we know have in the main been areas of elevation, and we would expect the same to hold good in those vast and permanent hollows of the earth which are occupied by the waters of the ocean." Everything that subsidence had explained, Murray had accounted for "by quite other causes," all of which had the benefit of being "proximate, relatively well known, and continuous in their action."[160]

Murray's version of parsimony was based on his preference to emphasize organic, rather than inorganic, agencies. He reported that Darwin had relied on two types of facts, the "physiological" fact that reef building corals grew at a limited depth, and the "physical" fact that elevation and subsidence were continually occuring on the

158. Murray, "On the Structure and Origin of Coral Reefs and Islands," 513.
159. Murray, "On the Structure and Origin of Coral Reefs and Islands," 506.
160. Murray, "On the Structure and Origin of Coral Reefs and Islands," 518–19.

surface of the earth. Murray argued that Darwin had underestimated the role of physiological causes, and that it was this mistake that led him to complicate his theory by calling in superfluous physical agencies.[161] Respecting his own priorities, he claimed that "The distinguishing feature of the views now advanced is that they do away with the great and general subsidences required by Darwin's theory, and are in harmony with Dana's views of the great antiquity and permanence of the great ocean basin, which all recent deep-sea researches appear to support."[162]

Response to Murray's coral theory

Murray's theory met an advocate in Archibald Geikie, who had been one of his former professors at the University of Edinburgh and was now the Director-General of the Geological Survey of Great Britain. In his 1883 Presidential Address to the Royal Physical Society of Edinburgh, Geikie declared in support of Murray that "the existence of [atoll and barrier] reefs is no more necessarily dependent on subsidence than on elevation."[163] The great triumph of the *Beagle* voyage was threatened with obsolescence, but Darwin had died the previous year, profoundly unconvinced by Murray's theory, but unwilling to enter the fray.

In an 1885 paper, Dana stepped into the breach to respond to Geikie's claims. He described Darwin's theory and his own contribution to its success, and made a long case that subsidence had occurred in the Pacific, as evidenced by phenomena independent of reef forms. He used Challenger evidence introduced by Murray to show that subsidence had happened at Tahiti. And he discarded the Murray-Semper theory by demonstrating that there was no reason not to believe subsidence had happened, there

161. Murray, "On the Structure and Origin of Coral Reefs and Islands," 505.
162. Murray, "On the Structure and Origin of Coral Reefs and Islands," 517.
163. Quoted in Dana, "Origin of Coral Reefs and Islands," 89.

was no reason to think that the remains of pelagic organisms actually did raise banks to within the range of reef building corals, and that there was no reason why solution should occur in lagoons moreso than anywhere else on the reef.

A primary argument made by the new critics of Darwin's reef theory was that the *Beagle* voyage had not actually involved a great deal of coral reef research. Geikie had said that "It should be borne in mind that, compared with more recent explorers, [Darwin] did not enjoy large opportunities for investigating coral reefs," identifying Tahiti and Keeling, but not Mauritius, as places where he had conducted investigations.[164] Having been referred to by Geikie as a "competent observer" who had had "at least" as much opportunity to study coral reefs as Darwin, Dana turned the argument about limited field experience back against Darwin's opponents by pointing out that he, Dana, had seen reefs in the Society and Samoan groups, had spent three months studying those of the Fijis, and had landed on no fewer than fifteen additional coral islands in other archipelagoes.[165] He added that Darwin, by studying Tahiti and Keeling reefs, "followed up by a careful study of other atolls and reefs of the ocean through the maps and descriptions of former surveying expeditions," had in fact possessed "a broad basis for judgment and right conclusions."[166]

Contra Geikie and Murray, Dana argued that subsidence was not proposed solely because no other explanation seemed admissable. "Darwin can hardly be said to have 'invoked' subsidence. Subsidence forced itself upon his attention. He saw evidence that it was a fact, and the theory came ready-made to him."[167]

Dana dispatched Murray's arguments with relish, and pointed out that one of his main points of evidence could be turned against him. He claimed that "the chief interest

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164. Geikie quoted in Dana, "Origin of Coral Reefs and Islands," 170.
165. Dana, "Origin of Coral Reefs and Islands," 171.
166. Dana, "Origin of Coral Reefs and Islands," 171–72.
167. Dana, "Origin of Coral Reefs and Islands," 171.

of the Challenger soundings consists in their affording ‘direct’ proof, ‘positive’ proof, of *much subsidence*; a kind of proof that subsidence sinks out of sight, and which soundings may yet make available in many similar cases.”[168] Murray and Swire had described masses of coarse debris up to 30 feet long, at depths from 240 to 600 feet. These depths, Dana declared, “are far below the limit of forcible wave-action. They are depths where the waters, however disturbed above by storms, have no rending and lifting power.” This belt must originally have been formed when the reef now at 240 feet had been at sea level, Dana argued, the only place where the wave action was sufficient to dislodge such large blocks from the reef, and “hence the debris affords *positive proof of a large subsidence during some part of the reef-making era*.”[169]

He further argued that the periodic exceptionally steep inclinations found at great depths on the face of atolls by FitzRoy and Wilkes were the result of periods of rest before subsidence resumed. Dana argued that no such banks had been discovered in areas of otherwise-deep ocean, and that the observations by Semper and Guppy (showing that elevated reef corals were placed atop limestone abounding in the marine organisms indicated by Murray) did not disprove the likelihood of subsidence operating in areas where proof indications of it had been found. “Such observations have great interest, but they only prove that, in coral-reef seas, corals will grow over any basis of rock that may offer where the water is right in depth, and do not nullify any of the evidences of subsidence. This point should be kept before the mind in all future study of coral-reef regions”[170] In Dana’s mind, the only true test would be to bore through an atoll.[171] He also recommended a closer examination of the island of Metia (known to

168. Dana, “Origin of Coral Reefs and Islands,” 176.
169. Dana, “Origin of Coral Reefs and Islands,” 177.
170. Dana, “Origin of Coral Reefs and Islands,” 181.
171. “Deep borings in atolls with circular drills that would give a six-inch core would supply evidence as to the existence or not of beach-made coral rocks at levels below the surface. They would also determine the depth to which true modern coral-reef rock extends and the nature of the underlying

him during the Ex. Ex. as Aurora Island), to determine whether it did indeed consist of a 250 foot tall layer of "true coral-reef rock," as he had concluded 45 years earlier in an observation that he now considered "not as complete as it should have been."[172] Absent conclusive evidence against subsidence, Dana concluded "the hypotheses of objectors to Darwin's theory are alike weak. [...] Darwin's theory therefore remains as the theory that accounts for the origin of coral reefs and islands."[173]

Coral reef controversy

What had been a relatively parochial dispute between the theories of Murray on the one hand, and Darwin and Dana on the other, became a transatlantic phenomenon when it was used as a parable about the prevalence of "idolatry" among professinoal scientists. The Scots statesman and amateur geologist, George Campbell, Eighth Duke of Argyll, saw in the coral reef dispute a "great lesson."

Argyll's object lesson began with the career of Charles Darwin himself. The Duke painted an innocent picture of the Darwin who had yet to be corrupted by "preconceived theories," Darwin "before he was a Darwinian."[174] By Argyll's telling, the subsidence theory was the "one remarkable exception" to Darwin's open-minded exploration of nature during the *Beagle* years. He offered a dramatized version of Darwin's insight at Tahiti (erroneously describing Eimeo as an atoll) and of his subsequent development and publication of the coral theory, which had been an objectionably self-confident presentation that "took the scientific world by storm [and]

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beds, whether calcareous, volcanic, or of any other kind; this is hence a sure method for obtaining a final decision of the coral island question and should be tried." Dana added a footnote about the futile boring attempt during the U.S. Exploring Expedition. Dana, "Origin of Coral Reefs and Islands," 104.

172. Dana, "Origin of Coral Reefs and Islands," 105.
173. Dana, "Origin of Coral Reefs and Islands," 190.
174. Duke of Argyll, "A Great Lesson," *The Nineteenth Century* 22 (1887): 294.

was well calculated so to do."[175] Other scientific men had embraced the theory, and "for the space of nearly half a century it has maintained its unquestioned place as one of the great triumphs of reasoning and research."[176] And for Argyll, this was the real problem: Darwin's coral theory had become dogma. The "great lesson" of his title was that the *Challenger* voyage had proved Darwin wrong. "Darwin's theory is a dream," Argyll announced. "It is not only unsound, but it is in many respects directly the reverse of truth. With all his conscientiousness, with all his caution, with all his powers of observation, Darwin in this matter fell into errors as profound as the abysses of the Pacific. All the acclamations with which it was received were as the shouts of an ignorant mob."[177] In his enthusiasm, Darwin had failed to recognize that a hypothesis could be plausible and still not be true.[178] Argyll portrayed Murray as a new hero of science, a man "whose sagacity and candour of mind, are not inferior to those of Darwin." Murray had humbly availed himself of modern deep-sea technology and the facts acquired thereby had led him to "a new truth," a "generalization as magnificent as that of Darwin's theory."[179] Murray's theory was "supported with such a weight of facts and such a close texture of reasoning that no serious reply has ever been attempted."[180]

Instead of rejoicing in the exposure of a "long-accepted error," scientists had attempted to stifle Murray's theory. Argyll announced that under this "Reign of Terror," Murray had been "strongly advised against the publication of his views in derogation of Darwin's long-accepted theory...and was actually induced to delay it for two years."[181]

175. Argyll, "A Great Lesson," 300.
176. Argyll, "A Great Lesson," 301.
177. Argyll, "A Great Lesson," 301.
178. Argyll, "A Great Lesson," 302.
179. Argyll, "A Great Lesson," 305.
180. Argyll, "A Great Lesson," 305.
181. Argyll, "A Great Lesson," 307.

Argyll raised the "Bathybius" affair: "This is a case in which a ridiculous error and a ridiculous credulity we re the direct results of theoretical preconceptions. Bathybius was accepted because of its supposed harmony with Darwin's speculations. It is needless to say that Darwin's own theory of the coral islands has no special connection with his later hypothses of Evolution. Both his theory and the theory of Mr. Murray equally involve the agencies of vital, chemical, and mechanical change. Nevertheless the disproof of a theory which was so imposing, and had been so long accepted, does read to us the most important lessons. It teaches us that neither the beauty--nor the imposing character--nor the apparent sufficency of any explanation may be any proof whatever of its truth."[182] Argyll closed by insinuating that if the likes of Huxley were too closed-minded even to accept the revealed truth about coral reefs, they were to be distrusted all the more when they expressed their views "concerning far deeper and more complicated things...incapable of being reduced to proof."[183]

Responding in the same journal, Thomas Henry Huxley argued that Murray's theory, far from remaining unchallenged, as the Duke alleged, had been skewered by no less a personage than Dana, "one of the highest extant authorities on the subject."[184] Huxley professed to hold no opinon on Murray's theory himself, having decided that he was not current with the recent science of coral reefs. "I doubt if there are ten living men who, having a practical knowledge of what a coral-reef is, have endeavoured to master the very difficult biological and geological problems involved in their study. I happen to have spent the best part of three years among coral-reefs and to have made that attempt," he continued, but "until I had two or three months to give to the renewed study of the subject in all its bearings, I must be content to remain in a condition of

182. Argyll, "A Great Lesson," 308.
183. Argyll, "A Great Lesson," 309.
184. T.H. Huxley, "Science and the Bishops," *The Nineteenth Century* 22 (1887): 640.

suspended judgment." Instead, Huxley vouched for Dana as "the most competent person now living to act as umpire."[185] Argyll had claimed that Murray's views had met with only a "slow and sulky acquiescence" by scientists. Huxley responded that "they cannot be said to have met with general acquiescence of any sort, whether quick and cheerful, or slow and sulky."[186] Indeed, given the apparent shakiness of Murray's theory, any advice to postpone his publication that Murray may have received "would have been sagacious and kind."[187] There had been no "conspiracy of silence," Huxley declared, quoting a letter he had received from the young geologist, J.W. Judd. "It is difficult to imagine how any one, acquainted with the scientific literature of the last seven years, could possibly suggest that Mr. Murray's memoir published in 1880 had failed to secure a due amount of attention," Judd had marveled, noting Murray's high profile as superintendant of the *Challenger* publications and citing a half-dozen examples of recent British and European textbooks that had reported Murray's theory in detail. "If this be a 'conspiracy of silence,' where, alas! can the geological speculator seek for fame?"[188]

Responding to Argyll, the geologist T.G. Bonney published a defense of scientists in the journal *Nature*, in which he argued that "Mr. Murray has obtained distinguished converts, but with such differences of opinion among those best qualified to judge, it is certainly going further than is warranted by the facts to insinuate if not to assert that he has convinced the scientific public."[189] Following Bonney's entry into the fray, the controversy moved into the pages of *Nature*, with further contributions not only from Argyll and Huxley, but also a series of communications from those who were

185. Huxley, "Science and the Bishops," 635.
186. Huxley, "Science and the Bishops," 637.
187. Huxley, "Science and the Bishops," 637.
188. John W. Judd to T.H. Huxley, 10 October 1887. Printed in Huxley, "Science and the Bishops," 641.
189. T.G. Bonney, "A Conspiracy of Silence," *Nature* 37 (1887): 26.

active in the study of reefs. The controversy surrounding the Duke's comments has been well described by Stoddart. He concludes that the Duke's "Great Lesson" revealed nothing about coral reefs, but it aptly (if exaggeratedly) characterized science as an activity shaped by seniority, social networks, tradition, and allegiance to received wisdom in the form of theories. But the outcry from scientists that followed the Duke's defense of Murray also revealed more clearly than before a fault line that ran within the boundaries of professional science. If there was not a conspiracy against Murray, then surely there was an open question as to whose theory was correct, Murray's or Darwin's. And Argyll's polemic had made the coral question into a venue for the debate--both within the realm of science, and between scientists and a range of other interested parties including clergymen, statesmen, and amateur students of nature--over the merits of Darwinism. The disagreements over how to interpret the results from the *Challenger*, amplified by the controversy sparked by Argyll and Huxley, led to widespread calls for the direct test of boring through an atoll.

Conclusion

I have argued that during the U.S. Exploring Expedition, distinctions between zoology and geology were difficult to make precise. There was at this stage a notion that there were two approaches--based on first principles, and as instantiated in the instructions. But in practice, Couthouy and Dana were equally inclined toward, and similarly proficient at, lines of investigation that were nominally part of both departments. Couthouy and Dana might have been *assigned* separate *departments*, but they were not *indoctrinated* (as in Kuhn) into separate *disciplines*, at least as regards how to proceed during *fieldwork*. The problem was, moreover, that there were phenomena deemed to be the subject of one department (e.g., the structure of coral islands as geology) that could barely be described, let alone explained, without resorting to the other. Later in the chapter, there began to emerge distinct zoological and geological *disciplines* of coral reef study, which carried distinct assumptions, were instantiations of distinct practices, and rendered practitioners significantly incapable of communicating across the boundary.

Murray's theory of coral reef formation offered a way to account for the characteristics of coral reefs that geologists cared about without reference to geological processes. More than any since before Quoy and Gaimard (going back to Eschscholtz or even Forster) his was a theory that relied on organisms to explain both the depth and shape of coral reefs. By arguing that there were more types of organisms whose remains could contribute to the structure of a reef, he was able to argue that subsidence was *unnecessary* to explain the origin and morphology of coral reefs, and therefore to reject Darwin's deductions about how reefs must be formed. The fact that Murray's alternative was founded on his own extensive field experience was important. However, it is important to ask how much of Murray's field experience was actually with coral islands themselves, and how much was he also arguing about something being plausible, rather than arguing that he had demonstrable evidence about the nature of reef foundations? To the extent that Murray's theory was also largely conjectural, or based on demonstrating that something was possible rather than that it was actually true, this goes to show why there was such a push for direct investigation of reef foundations as a way to settle the debate between advocates of the two theories.

The controversy over Murray's coral reef theory shows that by the 1880s Darwin's coral reef theory was satisfactory only from the perspective of geology. While Murray, Guppy, and Semper criticized Darwin's relative lack of field experience, they also criticized his willingness to include the process of subsidence into his theory when it seemed unnecessary to do so. For geologists and/or land-lubbers, it was indeed parsimonious to include a process (i.e., subsidence) that was indicated by other evidence to have occurred in areas where coral reefs existed.

The calls for a drilling experiment, and the planning for Funafuti, showed that settling the coral reef question was intended to settle distinct questions in geology and zoology/biology. The answer to the narrow question of coral reef formation was in large part considered a geological issue. Whereas, the implications of this experiment for the strength of "Darwinism" broadly construed would make it relevant for zoological questions about gradual change, the age of the earth.

CHAPTER 5
The Bomb that Ended a "Thirty Years' War"

Introduction

On 18 July 1947 the Director of Public Information for the U.S. Navy dispatched a press release that read:

> Drillers from the oil fields of Oklahoma began working around the clock on Bikini Island today in an operation that may settle a one hundred and ten year old argument among geologists. The core drilling operation being carried out jointly by the U.S. Geological Survey and the Navy Department is designed to definitely establish the origin of coral atolls.
>
> Some of America's leading geologists, headed by Dr. Harry S. Ladd, of the U.S. Geological Survey, are cooperating with the Bikini Scientific Resurvey in this atoll study. One hundred and ten years ago Charles Darwin, the famous British naturalist, advanced the theory that coral atolls are formed by coral growing upward on reefs around a slowly sinking island. Since that time arguments pro and con have been raised by geologists in all parts of the world.
>
> Up to now, the question has never been settled. CDR Roger Revelle, USNR, oceanographer for Operation CROSSROADS and head of the Geophysics Branch of the Office of Naval Research, said he believes core samples taken in the drilling operations at Bikini this summer may prove whether or not Darwin was right.[1]

How did the U.S. Navy come to be advertising its role in the crucial test of Charles Darwin's coral theory in 1947? This chapter argues that it was because the scientists recruited to survey Bikini Atoll for the nuclear weapons test of 1946 brought with them the same research questions that had animated their careers before World War II. Darwin's theory may have been 110 years old, but it was still the most controversial aspect of a debate over coral reef formation

1. "Bikini Scientific Resurvey Press Release No. 12," 18 July 1947. L.P. Schultz Papers, SIA, Box 26, folder 3, "Bikini Scientific Resurvey, Correspondence, Press Releases, etc."

that remained as vigorous as it had ever been in the nineteenth century. The press release was right in noting that geologists around the world had raised arguments for and against Darwin's theory, but it neglected to mention that their colleagues in many other sciences had been just as active in disputing the role of subsidence in coral reef formation.

The splintering of professional coral reef scientists among different disciplines, described in chapter 4, increased in the first three decades of the twentieth century. I demonstrate that coral reef formation was a truly inter-disciplinary subject both in the sense that it formed an independent tradition of scientific discourse that overlapped the domains of multiple disciplines, and in the sense that the disputes were polarized in such a way that it seemed the disciplines themselves were the combatants. Yet unlike the decades of the late nineteenth century, the disciplines of zoology (or biology) and geology were not the only ones in play. Clustered around these fields were disciplinary perspectives that were new, or newly applied to the study of corals and reefs. On the zoological side were studies of coral physiology and reef ecology or "coral bionomics," while the geological approach was augmented by that of physical geography, or physiography. Those individuals who came closest to combining the perspectives of each side were paleontologists, who sought to apply information from studies of living corals and reefs to the interpretation of geological structures. Meanwhile, newly rearranged institutions for the science of oceanography offered to embrace the perspectives of nearly everyone involved in the coral reef theory.

Throughout what the authors of a 1949 review of reef literature called the "Thirty Years' War" over coral reef formation from 1910-1939, the question of research methods was primary. Alternatives ranged from the comparative "home study" of hydrographic charts and photographs of physical features to highly localized and labor intensive experimental studies of coral reproduction and growth. However, nearly all participants acknowledged that deep core drilling all the way to bedrock through a living reef, such as had been tried at Funafuti, would offer a qualitatively different, and enormously valuable, form of evidence. To be sure, there was disagreement as to how universal the results of any one bore might be, but core drilling was fetishized as the silver bullet of coral reef study. For this reason borings were frequently attempted in the first half of the twentieth century, including notable attempts at the Great Barrier Reef in the 1920s and 1930s.

This chapter culminates with the famous core drillings at Bikini and Eniwetok atolls in the Pacific in 1947 and 1952, the latter of which did reach basement rock almost a mile beneath the surface. This Eniwetok boring is widely remembered as the event that confirmed Darwin's subsidence theory, and in histories of Darwin's theory it is often described in a coda or epilogue to events in his own lifetime.[2] In this manner, the Bikini and Eniwetok borings have come to appear as inexplicably spontaneous events. While it is often pointed out (though, surprisingly, not always) that these projects were part of the U.S. nuclear weapons testing program at each atoll, such statements often serve further to alienate those particular borings from the "normal science" of coral reef formation. When contrasted by historians to the reef studies of Darwin, or even of the *Challenger* voyage or the Funafuti expeditions, these events seem almost ahistorical, as though they have no real connection to Darwin's theory except that they incidentally proved it correct. (This echoes histories of the atomic bombs themselves, which have often been described as exceptional weapons that held the inherent capacity to end World War II, when in fact, like the core drilling, they were employed along with other tactics as part of what was expected to be an ongoing

2. See, for example, Ghiselin, "Introduction"; Sandra Herbert, *Charles Darwin, Geologist*, 357.

conflict.[3]) Part of the object of this dissertation has been to document the profound connections between the temporally remote events of the *Beagle* voyage and the Bikini drillings, to argue that the mid-twentieth century drillings were relatively conventional extensions of ongoing coral reef research. This chapter demonstrates that core drilling was widely demanded--and performed--by reef scientists of all persuasions, and that in this respect the Bikini and Eniwetok borings were themselves normal science. Indeed, I reveal that core drilling became part of the research program at the Marshall Islands via recommendations for postwar Pacific science that were generated independent of the nuclear weapons program. The core drilling was just one component of a barrage of scientific studies of the atolls that were conducted in association with the nuclear weapons program. By revealing how these studies were consistent with the approaches to coral reef science that existed before 1946-1947, this chapter also demonstrates that postwar notions of how to characterize an atoll were the products of prewar sciences of coral reefs. Therefore, when the Army and Navy brought scientists to Bikini before Operation Crossroads in 1946 and told them to make a baseline survey of the atoll as a whole so that the effects of the bomb could be known, many of these scientists responded by doing more expensive and comprehensive versions of the same types of research that they already believed were essential to understanding reef formation.

It would be virtually impossible to give minute accounts of every expedition and every new idea in the history of reef studies during this extremely fruitful period. Therefore I have been forced to select cases for study in this chapter that constitute a much smaller proportion of the contemporary contributions to the discourse over reef formation than did those in previous chapters. I have chosen to concentrate on the story of American reef studies in this period. The Americans were blessed with strong

3. For this argument on the history of the bomb, which has helped me to clarify my thinking about the role that has been retroactively assigned to the core drilling, see Michael D. Gordin, *Five Days in August: How World War II Became a Nuclear War* (Princeton: Princeton University Press, 2007).

funding for reef research that gave them access to reefs of the Pacific as well as the nearby Atlantic-Caribbean, and their work was much less disrupted during the two World Wars than that of their European counterparts. The difficulties posed by the volume of available material, however, simply mirror the challenge faced by reef scientists at the time. As the participants in the early-twentieth century debates over coral reef formation frequently noted, it had become virtually impossible to review the entire literature relevant to the problem. The result was that few attempted to do so. For a community that was beginning to drown in particulars, there was all the more reason for an individual to crave a generally-applicable theory of reef formation and to find excuses to ignore the increasingly large bodies of knowledge represented by the different disciplines from which other reef scientists hailed.

The first two sections of the chapter are partly overlapping accounts of two different models of reef study in the first decades of the twentieth century, one that was intensely fieldwork oriented and the other that relied in much larger part on the study of published charts and books. The great advocates of fieldwork were the Americans Alfred Goldsborough Mayer (he changed his name to the less-Germanic "Mayor" in 1918) and Thomas Wayland Vaughan. The two proponents of chart-based studies of coral reef formation were William Morris Davis and Reginald Aldworth Daly. All four were trained at Harvard, though Davis already held the university's chair in physical geography by the time the other three commenced their studies. By 1910 Mayer and Vaughan had become two of the most active coral reef field researchers alive and Daly had published a short but important paper on the formation of coral reefs. Davis only began writing on coral reefs the year after his retirement in 1912, but by 1928 he had published an enormous monograph and nearly forty other papers on the topic and become a chief antagonist of all three of his junior colleagues. Mayer and Vaughan agreed that Davis had spent too little time studying coral reefs in person, but their friendly collaboration was strained by their own disputes over the relative importance of geological and biological methods to reef fieldwork. It will become clear that there were many factors that complicated the general division I have laid out here: Vaughan made extensive use of hydrographic charts in his analysis of reef formation, and Davis and Daly each traveled to the Pacific to examine coral reefs for themselves. But the very different insights that each man drew from charts or living reefs only serves to accentuate the differences between them. In the first section I follow the story of all four men up to about 1914 with a focus on the relationship between Mayer and Vaughan. In the second section my attention shifts to Davis and Daly, while Mayer and Vaughan remain important actors in the story.

In the late 1920s the conflict between advocates of geological and geographical approaches to the coral reef problem on the one hand, and those committed to biologically oriented solutions on the other hand, came to a vitriolic climax in disputes between Davis and John Stanley Gardiner. Each man published a book epitomizing his approach to the field, and each responded hyper-critically to the other's work. I then introduce Harry Ladd and J. Edward Hoffmeister, who were at home in the geological camp, but who, as paleontologists, advocated that their colleagues make better use of the zoological and ecological data that were available to them. Ladd remains the focal point of the chapter as he played an important role in planning American post-World War II geological research in the Pacific islands captured from Japan before leading the core drilling teams at Bikini and Eniwetok.

A.G. Mayer, T.W. Vaughan, and the intensive study of coral reefs

Alfred Goldsborough Mayer was the director of the Marine Biological Department of the Carnegie Institution of Washington (CIW), including its tropical laboratory on the Dry Tortugas islands in the Gulf of Mexico, from its founding in 1904 until his death in 1922. His contemporary, T. Wayland Vaughan was a paleontologist for the U.S. Geological Survey who made a series of annual visits to the Tortugas laboratory beginning in 1908. Mayer entered Harvard as a graduate student in zoology in 1892.[4] Vaughan took bachelor's, master's, and doctoral degrees at Harvard after enrolling in 1890 following a first undergraduate degree in physics at Tulane.[5] While in Cambridge, each was tabbed as an apprentice by Alexander Agassiz, Mayer for the study and illustration of jellyfishes and Vaughan for the identification of fossil corals. Eventually availing themselves of enormous financial support from the Carnegie Institution, Mayer and Vaughan became two of the leading exponents of localized field studies of coral reefs. Each man made seminal contributions to the experimental study of reef building corals, and they each applied the resulting physiological and ecological data to the interpretation of coral reef formation.

Mayer quickly became a close junior associate of Agassiz, working beside him at his laboratory at Newport and on a cruise in early 1893 to the Bahamas, which was undertaken in preparation for a book on the medusae of the east coast of North America. Mayer showed great skill at drawing medusae and was allowed to draft reports on several species of hydromedusae and siphonophores.[6] From March to June 1896 Mayer accompanied Agassiz on a cruise to Australia with stops at Hawaii and American Samoa, New Zealand, and Naples. Later that year, Agassiz made Mayer curator of

4. Mayer has been the subject of a recent biography based on his publications and correspondence. I have drawn on this work for the outlines of his work and as a useful guide to the locations of his widely-scattered manuscripts. Stephens and Calder, *Seafaring Scientist*.

5. For biographical information on Vaughan I have drawn on the following sources: H.B. Bigelow, "Presentation of the Agassiz Medal to Dr. Thomas Wayland Vaughan," *Science* 83 (1936): 474–75; T.W. Vaughan, "Response of the Medallist," *Science* 83 (1936): 475–77; W. Storrs Cole, "Thomas Wayland Vaughan, 1870–1952," *The Micropaleontologist* 6 (1952): 45–47; R.N. Ginsburg, "Formative Years of the Scientific Career of T. Wayland Vaughan," *[Geological Society of America] GSA Today*, November 1995, 233–34.

6. Stephens and Calder, *Seafaring Scientist*, 5.

Radiata, a division which included the corals, at his Harvard Museum of Comparative Zoölogy (MCZ). In spring-summer of 1897 Mayer went to the Florida Keys to collect medusae, at Agassiz's direction. This resulted in his first visit to Loggerhead Key, in the Dry Tortugas islands, where he was later to found the CIW laboratory. The Tortugas was a group of seven low islands that partially enclosed a shallow lagoon, situated offshore of the Keys, well beyond Key West.

From late 1897 to early 1898, Mayer traveled to the Pacific with Agassiz, visiting Fiji and Honolulu. He had already begun to struggle to negotiate publication credit with Agassiz for work done by Mayer under Agassiz's direction and funding.[7] On this trip Mayer became anxious about his position under Agassiz and began to plan for employment somewhere other than the MCZ.[8] In July 1898 Mayer went back to the Tortugas for more collecting. Meanwhile Agassiz retired as official director of the MCZ and put Mayer's rival William Woodworth in his place, reinforcing Mayer's view that he would not rise at the MCZ.[9]

Yet Mayer continued to work with Agassiz, with his schedule in 1899 establishing an annual pattern that he was to repeat many times in the 1910s and early 1920: springtime work at Loggerhead Key and another expedition to the South Seas. On this venture (Agassiz's cruise on the *Albatross*), Mayer deplored the hasty surveying that characterized Agassiz's comparative coral reef studies. For Mayer, the speed of Agassiz's travel placed enormous limitations on his zoological collecting. In his view, by rushing "from Island to Island," Agassiz had ensured that the expedition was "fore-doomed to be a failure from every standpoint excepting that of the study of coral reefs."[10] This fascinating comment offers some insight into how Mayer developed a

7. Stephens and Calder, *Seafaring Scientist*, 11.
8. Stephens and Calder, *Seafaring Scientist*, 14.
9. Stephens and Calder, *Seafaring Scientist*, 16.
10. Mayer to C.B. Davenport, 18 December 1899. Quoted in Stephens and Calder, *Seafaring Scientist*, 23.

lifelong commitment to intensive studies of a chosen locality, which ideally in his view should be revisited from year to year. Yet it also reveals that at this time Mayer, who was by 1899 already an accomplished marine fieldworker and taxonomist, did believe that "the study of coral reefs" was an enterprise that could succeed through the practice of relatively superficial field study. Through the course of his career, which was increasingly occupied with coral reef studies from late in the first decade of the 1900s, his perspective changed profoundly.

In 1902, when Mayer was working outside Agassiz's immediate reach as a curator at the Brooklyn Museum, the Trustees of the Carnegie Institution of Washington appointed a Committee on Zoology, with strong representation by marine zoologists, to determine how to use funds to promote research.[11] The presence of Alexander Agassiz and others helped to ensure that some of this money went toward establishing a new marine zoology station like the Marine Biological Laboratory at Woods Hole, Massachusetts. While it initially seemed that the CIW might take over the Woods Hole lab, Mayer campaigned for the establishment of a new tropical laboratory. Mayer argued that a permanent facility at the Tortugas, situated far off the mainland within twenty-five or thirty miles of the Gulf Stream, would provide the benefits of working from a land station with the opportunity to study "the life [carried by] the great tropical ocean current." This would be, he argued, "a combination of advantageous conditions which all who have been upon cruising expeditions will appreciate."[12] Mayer urged "American men of science" to "awaken to the fact that we have at our very door a

11. The best history of the origins of the CIW Department of Marine Biology is to be found in Stephens and Calder, *Seafaring Scientist*. See also James D. Ebert, "Carnegie Institution of Washington and Marine Biology: Naples, Woods Hole, and Tortugas," *Biological Bulletin* 168, no. (Supplement: The Naples Zoological Station and the Marine Biological Laboratory: One Hundred Years of Biology) (1985): 172–82.

12. Alfred Goldsborough Mayer, "The Tortugas, Florida, as a Station for Research in Biology," *Science* 17 (1903): 190.

tropical fauna far surpassing in richness that of Naples," the site of the foremost marine biological station.[13]

In December 1903 the CIW Department of Marine Biology was formally established, with plans to build a biological laboratory on Loggerhead Key. It opened in the spring of 1905, and immediately suffered various problems, including storm damage and researcher illness. (The problems of isolation and bad weather were omnipresent at Loggerhead Key, and Mayer quickly began to reconsider his decision to place the lab there.[14]) From the moment the Tortugas laboratory was founded, Mayer envisioned making zoological studies of the local corals with a view to solving a question whose origins were far from Florida. "No extended and constant study of the growth of corals under various conditions has yet been made. Such a study would lead to a more accurate knowledge of the mode of formation of coral Atolls."[15] Mayer's approach was not merely observational; he would make active interventions into coral growth, which he called experiments. "Suitable slabs of terra cotta should be sunken at carefully chosen points, and at intervals they should be raised to the surface and the corals growing upon them should be measured and photographed after which they should be replaced uninjured for further observations."[16]

Mayer hosted annual summertime visits of researchers to the Tortugas, and his efforts to recruit new visitors included attempts to lure geologists to his Marine Biological station. In 1907 Mayer invited the Canadian geologist Reginald Aldworth Daly, then working on the International Boundary Survey between the USA and

13. Mayer, "The Tortugas, Florida, as a Station for Research in Biology," 192.
14. Stephens and Calder, *Seafaring Scientist*, 53, 56–58.
15. "Detailed Plans of Work, Equipment and Expenses of the Carnegie Marine Laboratory for Research at the Tortugas, Florida" written by Mayer and dated January 28, 1904, CIW RG Marine Biology, Box 2, folder 2: "Tortugas Lab – Name, Plan, Scope, History, 1903-1926."
16. "Detailed Plans of Work, Equipment and Expenses of the Carnegie Marine Laboratory for Research at the Tortugas, Florida" written by Mayer and dated January 28, 1904, CIW RG Marine Biology, Box 2, folder 2: "Tortugas Lab – Name, Plan, Scope, History, 1903-1926."

Canada, to visit the Tortugas for a program of (in Daly's words) "work on sea-water and sea-critters." At the time Daly was unable to accept the offer, but he commended Mayer's efforts because he believed that geological interpretations would be enhanced by certain related types of zoological and chemical work. "Your proposition is both highly complimentary and also confirms my own thought," Daly wrote, "that much experimental and observational work needs to be done in the biological geology 'der Gegenwart' [i.e., of the present]. Geology is full of assumptions, quite unchecked by actual tests, as to the behavior of animals in the sea, as to their contributions to sediments, as to the history of marine salts as affected by organisms." Daly encouraged Mayer to begin the proposed experimental work that summer even if Daly himself were unable to participate; otherwise he hoped to visit in future seasons and address such matters with the assistance of "some bang-up graduate student who could handle the chemical side of the questions."[17]

For 1908 Mayer sought Vaughan's attendance at the Tortugas. Vaughan had been associated with the U.S. Geological Survey (USGS) since before finishing his Ph.D. at Harvard, and was now heading Coastal Plain investigations for the USGS. Along with his geological credentials, he was also a well established expert on the taxonomy of living and fossil corals. He had applied this knowledge to the paleontological study of several areas of the Caribbean, which in turn exemplified his longstanding interest in changes of relative level between the sea and the land.[18] For taxonomic as well as paleontological reasons, he was interested in the ecological relations between corals, and had criticized Gardiner in 1904 for "suppress[ing]...data that he possessed regarding the influence of environment on variation" in his

17. R.A. Daly to A.G. Mayer, 11 March 1907. A.G. Mayor papers, SU, Box 1, folder "D."

18. For a taste of Vaughan's early work on taxonomy, and on the reconstruction of land-sea levels, see T.W. Vaughan, "Review of Two Recent Papers on Bahaman Corals," *Science* 14 (1901): 497–98; T.W. Vaughan, "Evidence of Recent Elevation of the Gulf Coast Along the Westward Extension of Florida," *Science* 16 (1902): 514.

classification of Madreporarian corals. Vaughan also urged that experimental studies should be used to settle taxonomic questions, such as those posed by the variety of shapes that a given species of coral could assume depending on local water conditions, for example by planting separate pieces of a coral specimen in different locations and comparing the resulting morphology.[19] At the 1907 International Zoölogical Congress in Boston, August 19-24, Vaughan revealed the elaborate interconnections between the various strands of his work in his report on the Madreporaria of Hawaii, which showed that he used data on the distribution of living corals to interpret the conditions in which fossilized corals had lived, and to unravel the history of coral migrations within and between the "Indo-Pacific" region and the Atlantic.[20]

Mayer was enthusiastic about the prospect of a doing a general reef study with Vaughan, who already had field experience in the Caribbean-Gulf of Mexico region and who could attack the study of the Tortugas reefs from many angles. Mayer reported to Woodward that "I will try to show him the entire barrier reef of Florida, and also take him to Andros, Bahamas if he desires to go there."[21] Mayer's enthusiasm was genuine,

19. T.W. Vaughan, "[Review of] *Madreporaria*. Parts I. and II. By J. Stanley Gardiner, M.A," *Science* 20 (1904): 503–5; John Stanley Gardiner, "Corals [Response to T.W. Vaughan]," *Science* 20 (1904): 765–66; T.W. Vaughan, "[Review of] *Madreporaria*. Parts III. and IV. By J. Stanley Gardiner, M.A," *Science* 21 (1905): 984–85.

20. He gave bathymetric and thermal distribution data for each genus. He did not, in this memoir, attempt to give causes for each genus' distribution in depth and temperature, which he believed "probably have a physiological basis," but argued that the facts themselves were valuable for "the paleontologist...that he may reconstruct from a study of fossils the physical conditions that prevailed during past geological epochs." He compared the stony coral fauna of the Hawaiian Islands to that of Panama, and to that of the "Indo-Pacific region" as a whole, and concluded by forwarding the possibility that the Hawaiian genera comprised "an emigrant fauna," like that of Bermuda, and that its makeup had been determined by ability of the larvae of each genus to "be transported alive for great distances by currents." T.W. Vaughan, "Summary of the Results Obtained from a Study of the Recent Madreporaria of the Hawaiian Islands and Laysan," in *Proceedings of the 7th International Zoölogical Congress* (Cambridge: Harvard University Press, 1912), 935.

21. Mayer to Woodward, 22 January 1908 (from Naples). CIW RG Marine Biology, Box 1, folder 9, "Mayor, Alfred G. Reports, 1904-1913, 2 of 2." Woodward responded that Vaughan would be "a capital investigator to look into the origin of coral reefs." Woodward to Mayer, 3 February 1908 (while Mayer is at Naples). CIW RG Marine Biology, Box 1, folder 9, "Mayor, Alfred G. Reports, 1904-1913, 2 of 2."

for he began to invest enormous personal energy and thousands of dollars of Carnegie money into Vaughan's work. In this first year of their collaboration, Vaughan told Mayer "I believe that we shall ultimately get something worth while out of the study of the reefs and keys, but I also think that the principal credit will belong to you, for your sympathy, interest, and help."[22]

During that 1908 lab season Vaughan studied the development and distribution of reefs in the Florida Keys and the Bahamas. His strong views on the coral reef problem startled Mayer, who reported to his wife that Vaughan believed that Alexander Agassiz's work on reef formation was "not worth the powder required to blow it to H---!"[23] Vaughan himself wrote to Agassiz to tell him of his latest fieldwork, revealing that he was studying well drillings in Florida to determine the thickness of the local limestones while admitting that Agassiz had a "sort of prior claim on Florida Keys geology."[24] Meanwhile, Mayer was eager that the work of the laboratory as a whole should move beyond the model of zoological investigation favored by Agassiz. increasingly attend to "the laws governing life; rather than...the systematic collection of groups which have already been extensively studied."[25] When Agassiz died in early 1910, Mayer wrote a memorial for *Popular Science Monthly* in which he both praised and criticized his mentor.

Mayer's criticisms of Agassiz's reef work portrayed him as a foil for the "intensive" studies that Mayor was then coordinating. "He saw more coral reefs than has any living man and this very virtue of his exploration is its chief fault, for the study of coral reefs is a complex problem and it can not be solved by a superficial inspection

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22. Vaughan to Mayer, 6 August 1908. Hyatt and Mayer (Mayor) papers, PU MSS C0076, box 7, folder "Vaughan, T. Wayland (fl. 1914)."
23. Mayer to Harriet Hyatt Mayer, 10 May 1908, quoted in Stephens and Calder, *Seafaring Scientist*, 62.
24. Vaughan to Agassiz, 1908. MCZ, bAg 861.10.1: "T.W. Vaughan letters to A. Agassiz, 1897-1908." Vaughan's manuscript report on his 1908 fieldwork is in CIW RG Marine Biology, Box 1, folder 9, "Mayor, Alfred G. Reports, 1904-1913, 2 of 2"
25. Mayer 1908 report to the CIW trustees, quoted in Stephens and Calder, *Seafaring Scientist*, 61.

such as he was forced to make. No one realized this more fully than he did himself, but he believed that the subject should be approached by a superficial survey of all of the reefs of the world, and thus he might hope to discover places where the problem might afterwards be studied with decisive results. He aimed to point out only the broad aspects of the problem, leaving the elucidation of details to those who might follow him."[26] Mayer had gone above and beyond Agassiz in the study of medusae; now he was using his laboratory at the Tortugas and his access to Carnegie funds to investigate the coral reef details that Agassiz had failed to touch. "It is to be regretted," Mayer reflected, "that of the three great writers upon coral reefs Darwin saw only one atoll, Dana sailed past many but was permitted to land upon few...and Agassiz was compelled to cover such a vast field that certain of his conclusions, as he states himself, are still tentative; for the solution of some of the questions presented by these problems demands a more intensive and prolonged study than he was able to devote to them."[27] Despite this fault, Mayer added that he "believe[d] that science will come to see that [Agassiz] succeeded in showing that Darwin's simple explanation of the formation of atolls does not hold in any part of the world."

At the same time, Agassiz's old friend and ally John Murray traveled to Harvard to deliver a eulogy that sought to compare Agassiz's record of fieldwork to those of his late-nineteenth century contemporaries rather than to his active successors. He lauded Agassiz's commitment to first hand observation and deplored those men of science who would defend Darwin's theory "although [having] never seen or examined a coral reef."[28]

In July of 1910 Mayer got a part-time position at Princeton and moved his

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26. Alfred Goldsborough Mayer, "Alexander Agassiz, 1835–1910," *Popular Science Monthly* 77 (1910): 434.
27. Mayer, "Alexander Agassiz," 435.
28. John Murray, "Alexander Agassiz: His Life and Scientific Work," *Science* 33 (1911): 83.

family there. It was a year of transition for the zoologist. He published his master work of systematic zoology, *Medusae of the World*. In the introduction he wrote, "I have always felt that each working naturalist owes it as a duty to science to produce some general systematic work, and this has been an actuating motive in the production of this book. But chiefly I have been moved to the task through respect for the wishes of my generous friend and master in science, Alexander Agassiz."[29] In almost the same breath, however, he acknowledged that the type of work he had learned from Agassiz had encouraged was going out of fashion. In an April 1910 article for *Popular Science Monthly* meant to publicize "The Research Work of the Tortugas Laboratory." He wrote, "The era of finding and naming of animals which had its dawn with Linnaeus and its noonday of splendor with the great French naturalists has waned into its dignified decline. Not that systematic zoology will not accomplish much in the future, but the days of its great achievement are in the past. Therein, indeed, lies the opportunity of the Tortugas laboratory, for a new science has arisen phoenix-like above the ashes of the old. Modern biology is now but little concerned with the naming of dead things, but the study of the living has become of paramount importance."[30] The "intensive" research program at the Tortugas was oriented toward experimental work and the gathering of quantitative data in a range of new fields that exploited the juxtaposition of the laboratory and the field site, and were decidedly not systematic zoology. "At Tortugas some of the ablest investigators of our country have been directing their attention not only to the systematic study of the rich reef fauna of the region, but mainly to problems in physiology, ecology, regeneration and embryology."[31] Geology was not mentioned in this list, but Vaughan's research was highlighted among the studies "of the widest gen-

29. Alfred Goldsborough Mayer, *Medusae of the World* (Washington, D.C.: Carnegie Institution of Washington, 1910), vol. 1, 2.
30. Alfred Goldsborough Mayer, "The Research Work of the Tortugas Laboratory," *Popular Science Monthly* 76 (1910): 399.
31. Mayer, "The Research Work of the Tortugas Laboratory," 401.

eral interest." Vaughan had determined that there were distinct regional differences in the formation of the Florida Keys, ranging from the elevation of coral reef rock to the elevation of limestone mud. "In addition to his studies of the geology of the reefs, he is making the most accurate and extensive investigation of the associations, habits, rate of growth and constitution of corals ever attempted by any naturalist."[32] Vaughan himself urged descriptive zoologists to contribute to the new sciences with an "appeal" in the journal *Science*. He argued that the value of specimens would be enhanced by information on the "environment" in which they had lived. "As so many zoologists are engaged on the description of marine faunas, and as it is more or less habitual to give very meager data on the conditions under which the organisms described live, this appeal for more detailed information is made to the body of investigators."[33]

It was not only zoologists and geologists, however, who would contribute to solving the origin of reefs themselves. Marshall Avery Howe of the New York Botanical Garden cited Vaughan's definition of a coral reef, "a ridge or mound of limestone, the upper surface of which lies or lay at the time of its formation, near the level of the sea, and is predominantly composed of calcium carbonate secreted by organisms, of which the most important are corals," as embodying "the long-standing and still prevalent view as to the origin and composition of coral reefs."[34] But Howe argued that whereas it might be "at first sight...quite axiomatic that corals should be the most important constructional agents in the formation of 'coral' reefs," recent evidence indicated that lime-secreting plants were at least as important as corals in the structure of many

32. Mayer, "The Research Work of the Tortugas Laboratory," 408–9 On 27 April 1910, just as Mayer's paper appeared, Vaughan himself gave a summary of his forthcoming interpretation of the history of the Floridian Plateau to the Geological Society of Washington. T.W. Vaughan, "Sketch of the Geologic History of the Floridian Plateau," *Science* 32 (1910): 24–27.

33. T.W. Vaughan, "The Insufficiency of Data on Environment Given in Papers Describing Deep-Sea and Other Marine Organisms," *Science* 33 (1911): 250.

34. W. A. Setchell wrote a biographical memoir of Howe for the National Academy of Sciences, but I have not seen it.

"true coral reefs."[35] Howe cited the work of Hinde and Finckh on the deep borings of Funafuti, which showed that the algae *Lithothamnion* and *Halimeda* were more important constituents of that "typical" atoll than were either corals or foraminifera. He added that an equally crucial role was played by seaweeds in the formation of other reefs of both the Pacific and Indian oceans by Gardiner, "who, being a professor of zoology in Cambridge University, should be free from any suspicion of bias in favor of the plants."[36] Howe declared that it was not yet possible to determine whether more calcium carbonate was secreted and deposited by corals or algae (in early nineteenth-century terms, which was more "geologically significant") but he pointed out that the contest might indeed tip in favor of the plants because lime-secreting seaweeds could inhabit a much wider range of depths and water temperatures than the reef-building corals, and in some instances at least, the plants appeared to grow more rapidly.

Howe, who had studied marine algae in several locations across the Caribbean, indicated that zoologists and geologists concerned with reef formation shared some terminology that was distinctive to reef studies. Reef students of both disciplines often used the term "nullipore" to refer to the types of stony red algae also known since the early nineteenth century as corallines or corallina, whereas practicing botanists had long-since abandoned it. For plant specialists the term was obsolete because when the generic name *Nullipora* was originally proposed by Lamarck in 1801, he had classified its four species as animals. Since being identified as plants, botanists considered these species properly to belong to the genus *Lithothamnion*.[37] Meanwhile, Howe pointed out, the term nullipore remained as "the almost exclusive possession of the zoologists and geologists," who still used it descriptively in characterizing the constituents of

35. Marshall A. Howe, "The Building of 'Coral' Reefs," *Science* 35 (1912): 837.
36. Howe, "The Building of 'Coral'' Reefs," 838.
37. Howe pointed out that the genus *Lithothamnion* had more recently been subdivided. Howe, "The Building of 'Coral'' Reefs," 839.

living reefs.

By 1912, Mayer dreamed of expanding the reach of the Carnegie Institution's marine biology department from the Caribbean to the Pacific. He sent Woodward a proposal to spend $16,200 on the outfitting and prosecution of a field research trip to Australia. (By comparison, the annual operating cost of the Tortugas laboratory for 1913 was to be $15,690.)[38] He considered Pacific work as a necessary corollary to the work he and his colleagues had already done on the Atlantic side of North America. "It may appear that we are tending to dissipate our energies in desiring to go so apparently far afield as to Australia, but the fact is that intensive physiological and ecological researches such as those we aim to encourage are problems not usually of a locality but of world wide import. For example Vaughan has after many years study gained a fairly complete idea of the laws governing the growth and ecology of the Coral reefs of the Atlantic, but his work must remain inconclusive in respect to the corals of the <u>World</u> unless he can similarly study the Pacific Reefs." Mayer gave several other examples aside from Vaughan, and concluded that "I could enlarge upon this scheme of work, but perhaps it may suffice to say that every investigator who I hope may go to Australia has already exhausted the resources of the North Atlantic and Mediterranean and has had years of the best sort of preparation for such a trip. [...] Once, in say ten years, the studies carried out year after year in our tropical Atlantic lead to the Pacific for their final solutions." Mayer saw the Pacific as a place where the features of coral reefs would be in greater relief. "I realize that there is an immense field we have left unexplored in our immediate neighborhood at Tortugas, but by a single trip to the

38. A.G. Mayer to Robert S. Woodward (from Concord Junction, Mass.), 22 August 1912a. CIW RG Marine Biology, Box 1, folder 8, "Mayor, Alfred G. Reports, 1904-1913, 1 of 2." Mayer followed up the next day with another letter that argued that "The cost of the proposed Australian expedition 16 200 may seem excessive, but Mr. [Alexander] Agassiz's expeditions were all much more expensive ranging from 38 000 to over 100 000. He of course chartered steamers and therein lay the chief element of his expense account." Mayer to Woodward, 23 August 1912. CIW RG Marine Biology, Box 1, folder 18: "Tortugas Lab – Mayer's Trips, 1912-1920."

Pacific we are not deserting our home station but rather preparing ourselves for seeing things at home under new light gained from wide experience."[39]

Part of Mayer's master plan for coral reef research was to enable Vaughan to devote his full time to the topic. He broached with Woodward "the possibility of getting Vaughan away from the U.S. Geological Survey to enable him to devote the next twenty years to the study of the Coral Reef problem."[40] Mayer proposed that the CIW should fund Vaughan at a salary of $3000 for eleven years. Vaughan's directorship of the USGS project on the geology of the coastal plain of the eastern United States was nearly finished, "thus ending an epoch of his service in the Survey."[41] In trying to convince Woodward to pay Vaughan a full time salary, Mayer expressed his profound respect for the quality and distinctiveness of Vaughan's work. "His interest has for the past five years grown increasingly toward the coral reef problem for the study of which the Carnegie Institution has afforded him opportunities. Vaughan's preparation for this work is unsurpassed by any other student in the World, and he has the problem as well in hand that it would seem a loss to science were he prevented from devoting his major energies to its solution." The type of studies that Vaughan made exemplified the combination of localized fieldwork and experimental methods that exemplified modern science in Mayer's view. "No one has adopted the intensive methods he is applying to this work, for Vaughan's opportunity for this sort of work has been exceptional at Tortugas and it is this intensive rather than superficial study which I hope will characterize the researches conducted by all investigators under our auspices. Among all we have had at the Tortugas, Vaughan is perhaps the most successful in getting the funda-

39. A.G. Mayer to Robert S. Woodward (from Concord Junction, Mass.), 22 August 1912a. CIW RG Marine Biology, Box 1, folder 8, "Mayor, Alfred G. Reports, 1904-1913, 1 of 2."

40. A.G. Mayer to Robert S. Woodward (from Concord Junction, Mass.), 22 August 1912a. CIW RG Marine Biology, Box 1, folder 8, "Mayor, Alfred G. Reports, 1904-1913, 1 of 2."

41. Mayer to Woodward, 22 August 1912b. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

mental facts, and in being able to detect the general laws." What made Vaughan unique was that he grasped general laws in physiology, ecology, and geology. "For example," Mayer continued, "he now knows the actual growth-rate of nearly all the Atlantic Reef-Corals. He discovered that they feed exclusively on the animal life of the ocean, not upon floating plant life. He worked out the law of their distribution over the reefs so he can tell whether the water over a fossil coral reef was deep, shallow, cool, warm, stagnant or flowing, calm or agitated, merely by observing the corals. Moreover incidentally, he has I think solved the problem of the oölite, and found that the underlying rock of the Bahamas is Miami oölite, not Aeolian rock as was supposed by Alexander Agassiz. He is now studying the effects of temperature, and of exposure to the atmosphere upon the coral reefs. Altogether no student of this general subject has anything like the mental grasp upon the problem he has attained. His work upon corals already surpasses that of Dana, Darwin, A. Agassiz, Semper, Murray or Gardiner, for his studies are more accurate and detailed than theirs could be owing to the difficulties under which they laboured."[42] Mayer accompanied his letter with a proposal called "Project of T. Wayland Vaughan for an extended study of corals and coral reef areas, including the geologic history of the borders of the Gulf of Mexico and the Caribbean Sea."

Mayer's request for a trip to Australia was successful, but Woodward thought it impolitic to prise Vaughan entirely free from the USGS. Nevertheless, as of 17 December 1912, plans were afoot for an heavy year of fieldwork in 1913. Following the usual summer season at Tortugas, Vaughan was to leave for the Torres Straits in July of 1913, to be followed in August by Mayer and eight others.[43] But before even leaving for the Tortugas, Vaughan backed out of the Australia trip. Not only was Vaughan over-

42. Mayer to Woodward, 22 August 1912b. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

43. The plans are reported in Mayer to Woodward, 17 December 1912. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

whelmed by the backlog of publishing projects that faced him, he also could hardly bear the thought of further fieldwork in remote tropical regions. “As I am convinced that I should not attempt expeditions that require great physical or nervous strain I believe that I should undertake only tasks that lie within the recognized limits of my strength.”[44] Although I have not found the letter in which Vaughan made his initial withdrawal, Mayer’s response indicates that Vaughan had not been gracious about it.[45] As he confided to Woodward, “[Vaughan’s] statement that after we have spent thousands of dollars in forwarding his studies we ‘display a lack of interest’ is disquieting for it appears to indicate a certain morbidity of mind which may be associated with his present ill health.”[46]

Mayer, who was then in Freiburg, Germany, was disappointed that Vaughan would miss the opportunity to see the reefs of the Pacific, but he was even more concerned that the years of research Vaughan had done at the Tortugas would be wasted if his results went unpublished. Having only recently touted the general value of Vaughan’s work in the Tortugas, he told Woodward that the value of Vaughan’s work was severely limited if he failed to go to the Pacific.[47] Indeed, “his work upon Coral reefs has lost its most important purpose, for his conclusions must necessarily be confined to those derived from a study in a narrow and very special field, and cannot be of world wide significance. [...] His statement that in order to draw general conclusions respecting reefs we need study only those near at home is I think hardly logical and if he

44. Vaughan to Mayer, 20 January 1913. CIW RG Marine Biology. Box 1, folder 16, “Tortugas Lab – Employees, 1904-1913.”

45. The extant letters from Vaughan to Mayer were written too late to have reached him in Germany before the extant letters of reaction were written from Mayer was written to Woodward and Vaughan.

46. Mayer to Woodward, 26 January 1913b (from Freiburg, Germany). CIW RG Marine Biology, Box 2, folder 4, “Tortugas Lab – Reports, 1904-1939, 1 of 2.”

47. Mayer to Woodward, 26 January 1913b (from Freiburg, Germany). CIW RG Marine Biology, Box 2, folder 4, “Tortugas Lab – Reports, 1904-1939, 1 of 2.”

does not care to visit the Pacific I do not desire to recommend that any further large sums be expended upon his work."[48]

Although he was unwilling to travel there himself, Vaughan gave instructions for Mayer to take to Australia. These reveal how he had intended to link his Atlantic work with investigations in the Pacific. In the process he explained that the outstanding problems in coral reef research lay across multiple sciences: "You will, perhaps, recall that I have stated the so-called coral reef problem involves three lines of investigation: (1) The ecology of corals; (2) the study of geologic processes within the coral reef region; and (3) an investigation of the geologic history of the area in which the reefs occur, the elucidation of this being largely based on evidence acquired through the knowledge of the geologic processes." He went on to "state the condition of each aspect of the problem," and to describe the work that he would recommend to be carried out at the Great Barrier Reef in each aspect. He considered the study of Geologic Processes to be the most important for the Australian expedition because he believed that the general principles of coral ecology were already relatively well understood and that present interpretations of the geological history of the Great Barrier Reef was would be difficult to supersede without improvements in the knowledge of geological processes. Within this most important department of the investigation, Vaughan urged that there were two subjects that "above all others need elucidation," namely submarine planation and sub-marine solution, and he wrote, "I fear it will be more difficult to find someone who can make these studies than for any other aspect of the problem." Submarine planation was the process that might generate shallow water platforms upon which coral reefs could be established without requiring subsidence. In order to study this phenomenon, Vaughan believed, "a man must have the physiographic attitude of mind toward this subject and

48. Mayer to Woodward, 26 January 1913b (from Freiburg, Germany). CIW RG Marine Biology, Box 2, folder 4, "Tortugas Lab – Reports, 1904-1939, 1 of 2."

be able to see a great deal." Determining the rate of solution (and deposition) of calcium carbonate was another key to evaluating the theories of lagoon formation advocated by Murray and Alexander Agassiz. On this front "Very careful work will be necessary to ascertain whether destruction or construction is predominating in any particular lagoon."[49]

Vaughan was at a loss, though, to propose who might carry out all these varied operations. "From what precedes," he told Mayer, "you will see that the problem involves two entirely different kinds of work, one zoologic, the other geologic. If the investigation is to be done by one man he must be both a zoologist and a geologist." As Vaughan's phrasing made clear, scientists primarily identified themselves as one or the other. He continued, "Should this combination not be found it should then be divided between a zoologist and a geologist who will work in cooperation, the work of each man supplementing and supporting that of the other. In order that the investigation may go forward it seems to me that it would probably be better to have two men. I should think that you could find some able young man who would be glad of the opportunity to do the zoologic work, which in my opinion is far simpler than the geologic. For the geologic work I believe a competent man may be found either in the United States or in Australia, but I wish to say that the geologist who undertakes the investigation should be supported by a chemist. In fact, if he is not supported by a chemist his work cannot be conclusive."[50] The implication of Vaughan's instructions was that apart from employing one of a handful of uniquely qualified individuals, the only way a coral reef could be studied up to modern standards was collaboratively.

In March 1913, still hoping for full-time funding from the CIW, Vaughan gave a

49. Vaughan to Mayer, 20 January 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

50. Vaughan to AGM, 20 January 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

separate report to Woodward describing the scope of his intended project. The purpose of the investigations Vaughan described was to "work out…the geologic history of the peripheries of the Gulf of Mexico and the Caribbean Sea," and it incorporated his USGS Coastal Plain Investigations as well as his work in Central America, the West Indies, and northern South America. Regarding his work on corals, Vaughan said "My studies of corals are grounded in my paleontologic work. The investigations of these organisms have had three aims, namely: (1) A systematic account of coral faunas, especially fossil faunas, in order to procure information on the succession of coral faunas which are terminated by those living at the present time; (2) the description of the various fossil coral faunas according to their stratigraphic position, so as to render assistance in correlating geologic formations; (3) a study of the ecology of recent corals, thus obtaining some knowledge of the principles underlying the relations of these organisms to the environment in order that deductions may be made regarding the physical conditions under which the fossil organisms live. It was in order to obtain a sufficient basis for making deductions from fossil corals that nearly all my studies on the recent organisms have been made."[51]

According to Vaughan his work on living corals had been instrumental, aimed only at improving what he could do as a geologist. Yet Mayer, as a zoologist, considered those studies to be fundamental in their own right. Even as he struggled to manage Vaughan's dissatisfaction, Mayer never lost respect for these achievements, which, he told Woodward, "I regard as the most important study we have yet had the privilege of aiding in. [...] I regard his coral reef work as of the first importance – the only conclusive study ever made upon the growth rate and ecology of corals, and to lose it would be the saddest loss we could sustain."[52] The outlook was bleak, for "Vaughan's health

51. T.W. Vaughan, "Statement for Doctor Woodward. Coastal Plain Investigations." March 1913. CIW RG Marine Biology. Box 1, folder 16: "Tortugas Lab – Employees, 1904-1913."
52. Mayer to Woodward, 5 July 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab –

is decidedly broken – I hope not permanently but certainly for the moment seriously." Mayer asked Woodward if he could add a sum of one thousand dollars to his expense estimate for 1914 to be paid to Vaughan upon submission of a completed manuscript on his Tortugas coral studies. "We are not a charitable institution, but at times charity and progress go hand in hand and this I think is one of them."[53]

Vaughan clearly was disturbed during the summer of 1913, although his letters to the boss, Woodward, never approached the bile of his communications with the department head, Mayer. To Mayer, he expressed his insecurities in terms of a *disciplinary* rivalry between geology and biology. Even as Mayer was desperately trying behind the scenes to secure emergency funds to support Vaughan through his trials, Vaughan contacted him to express his disgust that Mayer did not recognize the overarching geological importance of his work.

> Our conversations, the knowledge of your association with Mr. Agassiz, and your suggesting expeditions here and there (including the one to Australia) [where Mayer was bound at the time of writing] led me to infer you were interested in corals and the coral reef problem in a broad way and wished to support investigations that would lead to the solution of the latter. Acting in this belief, I made studies preliminary for the Australian expedition and submitted a program for practically a world wide investigation of coral reef areas. This program comprised studies of the biology of reefs. of the geologic processes operative in the areas in which they occur, of the geology and the interpretation of the geologic history of the areas. The interpretation of the geologic history was to be especially directed toward elucidating the conditions under which the reefs developed. Although, in my opinion, no other method of study will give satisfactory results, I noted in discussing the Australia project with you that you seemed to have only the biologic phase in mind.[54]

Vaughan accused Mayor of having similar plans to excise geology from the Department's work in and around Tortugas as well. If, as Vaughan suspected, those

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Employees, 1904-1913."

53. AGM to Woodward, 5 July 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913." Woodward agreed to the request. See Woodward to AGM, 8 July1913, in the same folder.

54. Vaughan to Mayer, from North Hatley, Quebec, 29 July 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

efforts would be "confined to certain aspects of the biology of the Tortugas and Bahaman shoal water corals, and that the 'geological' studies be separated from the 'biologic,'" then Mayor would be going too far. To sideline the geological work would "actually [mean] removing the entire coral reef problem, except certain biologic studies of really narrow scope."

By the end of the summer, which he spent in Quebec on USGS detail, Vaughan's spirits had brightened, though he was still below his normal weight.[55] He had attempted some writing on coral reefs and found that "All that I know of coral reefs from my own observations or from the recorded observations of others falls into coördinate relation."[56] In doing so he determined that the most valuable reef data had been the results of intensive localized study. "Without that completely conclusive study of the Tortugas, this could not have been done – nor could it have been without the careful studies of Agassiz in the Paumotus and those he had Andrews make in the Fijis."[57] After being filled with gloom over the many publications he had promised, Vaughan's delineation of Florida's reef history had swung his mood to the other extreme: "I have been so elated over these results that I wish to tell you," he reported to Woodward, "for years of efforts (I worked in Cuba in 1901; and began publishing on the West Indian elevated reef corals in 1899) have been definitely successful. I have been fairly drunk with joy."[58]

Vaughan's interpretation of the Florida reef tract began with the determination that chemical precipitation of calcium carbonate had been a hundred times more effec-

55. Vaughan to Woodward, 18 August 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

56. Vaughan to Woodward, 15 September 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

57. Vaughan to Woodward, 15 September 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

58. Vaughan to Woodward, 15 September 1913. CIW RG Marine Biology. Box 1, folder 16, "Tortugas Lab – Employees, 1904-1913."

tive in forming the underlying limestones of Florida than had the growth of corals.[59] In concert with topographic studies showing that a shallow limestone plateau extended around Florida in places with and without living reefs, this fact convinced Vaughan that the Florida barrier reef stood on a platform that was formed independently of the growth of the reef proper. He went on to claim that the platform underlying the Great Barrier Reef, as depicted in Admiralty charts, was analogous to those of Florida, Cuba, and the Bahamas, all of which he had studied himself. These platforms must be antecedent to the growth of the present reefs, he concluded, and he pointed out that their depths coincided with the depths that Daly had given for platforms in the open Pacific (on which, see below).

Vaughan also discussed atoll formation. Along with Drew and Dole, he had determined via experiment at Florida that lagoons could not be created by solution. He argued that the rims of the two atoll-like islet groups off Florida, the Tortugas and the Marquesas, were both constructional (i.e., they had grown upward into their present orientation), and had been given their shape by the action of winds and currents rather than by the shape of their underlying platform. Comparing his Florida data to Agassiz's descriptions of the Paumotu atolls, Vaughan argued that both groups were formed by a thin crust of Recent corals growing on an older limestone foundation. He reckoned that the shape of deep-ocean atolls did depend on the shape of their platforms, arguing that his Florida coral growth experiments showed that sedimentation would check the activity of corals not living at the perimeter of the pre-existing foundation.

Vaughan's comparisons between Florida reefs and those of the Caribbean and

59. T.W. Vaughan, "Sketch of the Geologic History of the Florida Coral Reef Tract and Comparison with Other Coral Reef Areas," *Journal of the Washington Academy of Science* 4 (1914): 26–34. The original draft of this publication was sent to Woodward, and shows that Vaughan only added some of the comments that compared Florida coral reef formation to the process in the rest of the world at a later stage in composing the piece. It is attached to Vaughan to Woodward, December 26, 1913. CIW RG Marine Biology. Box 1, folder 16: "Tortugas Lab – Employees, 1904-1913."

the Pacific suggested that “the problem of the depth of barrier platforms is a world-wide one.”[60] As Vaughan explained it in a private letter to Woodward, **“**There now remains, in my opinion, only one aspect of the coral reef problem that needs further elaboration in order to get a complete basis for sound coral reef theory. That aspect is the platforms about which I have talked to you and written you.”[61] While Vaughan was rediscovering his enthusiasm, Mayer was--perhaps not coincidentally--steaming to the other side of the world

With Vaughan absent, Mayer’s 1913 expedition the Torres Straits, north of the Great Barrier Reef, marked the beginning of his own major work on coral reefs. Mayer departed for the Great Barrier Reef with Harvey, the MCZ zoologist Hubert Lyman Clark, Clark’s former student and a veteran of Loggerhead Key, David Tennent, and the engineer for the Tortugas laboratory John Mills on 23 July 1913. They were later joined by F.A. Potts, the zoologist from Gardiner’s home university of Cambridge.[62] Mayer’s choice to set up research at Thursday Island in the Torres Straits was based on his visit there in 1896 and his consultation of Saville-Kent’s 1893 book. However, silt had killed most of the corals and there were no echinoderms present. The party moved on to another location, Murray Island, where they worked for six weeks. His achievements there encouraged Mayer to brag to his wife that he had “done that which Vaughan has not done.”[63]

Mayer’s chosen approach to studying the massive expanse of the Great Barrier Reef was a continuation of the approach he took at the Tortugas. Rather than attempt-ing to survey the full length of the barrier, he opted for a careful and centralized study of

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60. Vaughan, “Sketch of the Geologic History of the Florida Coral Reef Tract and Comparison with Other Coral Reef Areas,” 33.
61. Vaughan to Woodward, 15 September 1913. CIW RG Marine Biology. Box 1, folder 16, “Tortugas Lab – Employees, 1904-1913.”
62. For events of this expedition, see Stephens and Calder, *Seafaring Scientist*, 108–10.
63. Quoted in Stephens and Calder, *Seafaring Scientist*, 109.

a single reef. As he reported to Woodward in a letter from the barrier, by setting up camp at the luxuriant site of Murray Island, "I was enabled to make the most detailed and intensive study of a coral reef yet made."[64] As at the Tortugas Mayer had presided over experiments on coral growth, but his Murray Island work showed an increased desire to produce quantitative results in all manner of investigations. "I surveyed the reefs and ran lines across them and then laid out 50 foot squares at 200 foot intervals and counted each and every coral on each square thus determining the actual frequency of each species at each situation from the shore out to the outer edge of the reef. The results come out in curves for each species. I then found the temperature limits the corals could withstand and also how much dilution of the sea water with rain water they could tolerate, and how long they must be covered with silt to cause death. I also got the temperatures at times and tides ranging from 4.17 AM> to sun-set and from low to high tide, and titrated the water from various parts of the reef. I will write to Vaughan and ask him if he wont have the paper with me under our joint names and I am sending a well labelled collection to him."[65]

From Celebes, Mayer wrote to Woodward that "This is the first time the physical factors determining the growth of a Pacific reef have ever been analyzed. Heat is a primary factor and ability to resist silt is associated with it. It is curious that those species which resist high temperature resist silting (being buried under the mud) in like proportion. This suggests that the effect of high temperature is to kill by asphyxiation. You know that Physiologists have been much puzzled to explain why animals die at temperatures so much below that of heat rigor – I think this explains it, but will carry out some CO_2 experiments at Tortugas next summer to settle it. I will send a copy of mss. to

64. Mayer to Woodward, from Thursday Island, 30 October 1913, CIW RG Marine Biology, Box 1, folder 8, "Mayor, Alfred G. Reports, 1904-1913, 1 of 2."

65. Mayer to Woodward, from Thursday Island, 30 October 1913, CIW RG Marine Biology, Box 1, folder 8, "Mayor, Alfred G. Reports, 1904-1913, 1 of 2."

Vaughan so he can be a joint author of the paper and can correct it and name the corals according to his own system of nomenclature."[66] Mayer's comments about settling a matter raised by Pacific reefs by experimenting on those in the Tortugas corresponded perfectly to the justification he had initially given to Woodward, that the work in the Pacific was an outgrowth of, and spur to, work at the Tortugas laboratory.

This global view of corals and reefs was a philosophical perspective made possible by the technologies of telegraph and steam ship. He had already made four Pacific excursions in his career, each lasting less much than a year. Unlike most nineteenth century students of the Pacific reefs, he had every reasonable expectation of returning again. As Mayer steamed home via British New Guinea and England, where he met with Gardiner in Cambridge, he alerted Woodward that "I will finish the paper upon Murray Island and Torres Straits coral reef before I reach Europe."[67] It was a comment that revealed the high pace at which reef expeditions could now go from conception, to execution, to print. Mayer's publication on "Ecology of the Murray Island coral reef" was in print by April 1915.[68]

Mayer learned of the death of John Murray just as he was leaving England for the final leg of his journey home, and the news of Murray's loss prompted him to analyze the distribution of reef knowledge among different workers. Murray had disagreed with statements Vaughan had made, based on second hand information, upon the nature of Pacific reefs, and Mayer wrote to Woodward that "I think [Murray] is right in so far as the Pacific is concerned and Vaughan is right respecting the Atlantic."[69] To Mayer,

66. Mayer to Woodward, 28 November 1913 (from S.S. *Houtman* at Celebes). CIW RG Marine Biology, Box 2, folder 4, "Tortugas Lab – Reports, 1904-1939, 1 of 2."

67. Mayer to Woodward, 28 November 1913 (from S.S. *Houtman* at Celebes). CIW RG Marine Biology, Box 2, folder 4, "Tortugas Lab – Reports, 1904-1939, 1 of 2."

68. Alfred Goldsborough Mayer, "Ecology of the Murray Island Coral Reef," *Proceedings of the National Academy of Sciences* 1 (1915): 211–14.

69. Mayer to Woodward, 24 March 1914 (from Maplewood, New Jersey). CIW RG Marine Biology, Box 2, folder 4, "Tortugas Lab – Reports, 1904-1939, 1 of 2."

there was a clear correlation between first-hand study of an ocean's reefs and correct interpretation of their origin. To wit, he reported, the three South-Sea travelers "Professor Stanley Gardiner, Sir John [Murray] and myself are in essential accord respecting the Pacific reefs." The great difficulty was that intensive fieldwork and wide travel were difficult objectives to combine. "It is most unfortunate" he lamented, "that not a living student knows both the Atlantic and the Pacific reefs <<(except myself)>> for there are marked and important differences. Vaughan should see the Pacific reefs before he draws conclusions respecting their formation, but this I fear he will never do, and thus he is in danger of being discredited for the really excellent work he has done so patiently and well upon the Atlantic reefs. I will however see him and attempt to steer him out of the way of the rocks."

Mayer's version of the intensive approach to reef study demanded not simply that the solution to the coral reef problem be based on data from a wide sampling of world's reefs, but that this sampling should be achieved by individual researchers. We might say that from Mayer's perspective, the reefs could not be made to "travel," so that if reefs from across the tropics were to be compared then it must be the researcher who moved from one field site to another.

Daly, Davis, and the physiographic approach to the coral reef problem

Mayer and Vaughan had contemporaries who believed that reefs could be made to travel, for whom personal observations of coral reefs were secondary to the systematic study of charts, maps, and photographs. The Harvard geologists William Morris Davis and Reginald Aldworth Daly were separated by a generation in age but they lived as next-door neighbors in Cambridge, Massachusetts, where Daly succeeded

Davis as the Sturgis Hooper professor of geology at Harvard in 1912.[70] Although Davis had been trained in geology, he was at least as widely known as a practitioner of physical geography (often called "physiography"), the subject in which he had originally been appointed an Assistant Professor (1885) and then as Professor (1890), before switching to the geology chair in 1899. His most important contribution to the science was his notion of a "cycle of erosion," in which reliefed landscapes were gradually reduced to plains by the action of running water and other effects, eventually to be uplifted again into unequal landscapes by tectonic action. The physiographic approach was to interpret a present-day landscape by imagining the possible former landscapes that could have produced it. Davis did not reject fieldwork, but he believed one should approach the field with previously established hypotheses in mind, developed from the study of maps, and should direct observation in the field toward specific diagnostic features. In his many articles on pedagogy and method, Davis evangelized this model of "multiple working hypotheses" developed by the American geologists Grove Karl Gilbert and T.C. Chamberlin.[71] Davis's physiographic work could be distinguished by his use of schematic "block diagrams," which depicted integrated sectional views of a landscape or feature at multiple stages of its supposed evolution. And for Davis, the

70. The following biographical details are drawn from Reginald A. Daly, "Biographical Memoir of William Morris Davis, 1850–1934," *National Academy of Sciences Biographical Memoirs* 23 (1944): 263–303; Isaiah Bowman, "William Morris Davis," *Geographical Review* 24 (1934): 177–81; Kirk Bryan, "William Morris Davis: Leader in Geomorphology and Geography," *Annals of the Association of American Geographers* 25 (1935): 23–31; Lawrence Martin, "William Morris Davis: Investigator, Teacher, and Leader in Geomorphology," *Annals of the Association of American Geographers* 40 (1950): 172–80; Robert P. Beckinsale, "The International Influence of William Morris Davis," *Geographical Review* 66 (1976): 448–66.

71. Henri Baulig, "William Morris Davis: Master of Method," *Annals of the Association of American Geographers* 40 (1950): 188–95. On Gilbert's advocacy of what is now know as the hypothetico-deductive method, and T.C. Chamberlin's schematization of this method of what he called "multiple working hypotheses," see G.K. Gilbert, "The Inculcation of Scientific Method by Example," *American Journal of Science* 31 (1886): 284–99; T.C. Chamberlin, "The Method of Multiple Working Hypotheses," *Journal of Geology* 5 (1897): 837–48; Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York: Oxford University Press, 1999), chapter 5.

“evolution” of a landscape relied on a genuine “inorganic natural selection” that determined, for example, which of multiple stream beds would become a region’s primary drainage channel.

During Davis’s tenure as professor of physical geography, Daly was at Harvard studying geology for his M.A.(1893) and Ph.D. (1896), after spending his undergraduate years at Toronto.[72] After two years studying at Heidelberg and Paris, he returned to Harvard briefly to teach under the man who had been Davis’s mentor in geology, Nathaniel Shaler. After turning down Mayor’s offer to spend the 1907 field season at the Tortugas (see above), Daly had taken a position at M.I.T. while working up the results of his Canadian boundary survey.

Daly became interested in coral reef formation during a 1909 field trip to study the volcanoes of the Hawaiian islands. He was to write many times that he had been struck by the narrow width of Hawaiian coral reefs, and that this led him to consider the possibility that they had only been growing since the most recent ice age, during the Pleistocene (or Glacial) epoch. His field notes from the trip reveal that this idea may actually have descended from his studies of dry-land physiography, which were themselves stimulated by his use of a new technology that allowed him to survey the landscape broadly and rapidly: the automobile. For most of July he examined volcano craters and the associated rocks, frequently using a “motor car” to explore the landscape. Around the end of July he began to give serious thought to the subaerial erosion he had seen (some of which suggested the former presence of a glacier on the high slopes of Mauna Kea) and to ponder, to a lesser extent, the sea cliffs. After seeing Maui during August, he got to Honolulu on August 25. Four days later he wrote notes on a single-

72. Biographical details of Daly’s life are drawn from Marland P. Billings, “Reginald A. Daly, Geologist,” *Science* 127 (1958): 19–20; James H. Natland, “Reginald Aldworth Daly (1871–1957): Eclectic Theoretician of the Earth,” *[Geological Society of America] GSA Today*, February 2006, 24–26.

day 95-mile motor car trip around Oahu, which was again given to noting island- and archipelago-wide phenomena. He was able to "Corroborate faulting at Pali as far as headland N side of Kaneoha Bay – fault-block clearly seen with nearly vertical dip, at ridge ca 2 miles NW of Waikane." Then to the northwest he found "Coral-sand dunes from Laie Pt. to Waimea fringe old sea-cliffs." He immediately wrote "Is this coral all post-Glacial?" This suggests that he was trying to explain how the cliffs could have been eroded if they were fringed by coral formations, and evidently his solution was that the corals had appeared only after the cliffs were formed. He then asked himself, "Corals could not live here in Glac[ial] Period?" On 6 September, he wrote, "Circumnavigate Kauai...Night back to Honolulu...All day debate origin of graded plateau...NE & E of island. highest points on it ca 300' above sea [...] Prob[ably] this is an uplifted marine beach." A possible objection to this plateau having formed was that there was "no coral li[mestone] known on it." Daly reasoned that this was "Per[haps] because corals could not live there eg. Glac[ial] Period?"[73] These observations suggest that Daly may have drawn his explanation of the narrow Hawaiian reefs from an independently formed opinion that some of the high-land formations he had seen would be more easily interpreted if he could postulate an absence of corals in the not-too-distant geological past.

In a paper that appeared directly following the Hawaii trip, Daly argued that while the last thirty years had seen much opposition to the Darwin-Dana subsidence hypothesis, none of the critics had sought in their own theories to account for the changes in tropical sea level that must have occurred during the last Ice Age. This phenomenon, Daly said, "seems to supply a missing link in the chain of argument used by Semper, Rein, Murray, Agassiz, and Guppy against the wholesale-subsidence

73. R.A. Daly field notebooks, Hawaii 1909. HU-P HUG 4315.55, vol. 2, pp. 29-30.

hypothesis."[74] He gave evidence to suggest that during the Pleistocene period, the formation of northern and southern polar ice caps had lowered global sea level by twenty to thirty fathoms, and that the gravitational pull of the caps would account for a further five- to eight-fathom negative shift in the equatorial seas.[75] During this low stand, Daly hypothesized, the simultaneous global cooling of the seas probably meant that the distribution of reef corals was severely limited. Unprotected by coral reefs, islands outside the very warmest parts of the tropics were subject to heavy abrasion that cut broad benches or platforms down to a depth of 15 fathoms; in many cases they were probably planed down into entirely submarine plateaus. The breadth of these flat-topped formations was increased by the deposition of a talus of eroded material on their outer margins. When the ice caps melted after perhaps 100,000 years, and both the level and the temperature of tropical seas rose, reef corals grew upward at the margins of the new wave-cut benches and plateaus. Over the course of a roughly thirty-fathom positive shift in sea level, fringing reefs, barrier reefs, and atolls would be formed in a manner "analogous to that imagined by Darwin and Dana on the subsidence hypothesis."[76]

Daly used bathymetrical and geographical data as his primary evidence for this new theory of reef development. He presented a large table containing the mean and maximum depths of sixty-five "representative atoll lagoons and barrier channels of the Pacific and Indian oceans," along with the extreme width and length of the submarine plateaus that each reef surmounted. The table showed that the average maximum depth of lagoons was about 35 fathoms, with surprisingly little variation, while the average depths of great submarine plateaus without coral reefs was about 45 fathoms. Given

74. Reginald A. Daly, "Pleistocene Glaciation and the Coral Reef Problem," *American Journal of Science* 4th Series, Vol. 30 (1910): 298.

75. Daly, "Pleistocene Glaciation and the Coral Reef Problem," 299–300 Daly drew his evidence from the work of Penck, Suess, Hess, and Archibald Geikie, and supplemented it with his own observations from the neighborhood of the 49th parallel.

76. Daly, "Pleistocene Glaciation and the Coral Reef Problem," 305–6.

that there must be some accumulation of calcareous detritus inside lagoons (and here Daly discarded the Murray-Agassiz contention that lagoons were places where limestone was dissolved), Daly assumed that this buildup of lagoon deposits accounted for the ten-fathom difference between the average depths of lagoons and unadorned plateaus. This meant that the depth of submarine plateaus was strikingly uniform. Furthermore, the 45-fathom figure apparently corresponded to the amplitude of the Pleistocene tropical sea level shift (30 fathoms) combined with the depth to which platforms were likely to be cut by waves (15 fathoms). Further evidence for the theory was presented by the horizontal breadth of living reefs. Daly was sure that the Hawaiian islands, which lie so near to the present northerly limit of reef growth, could not have had living reefs during the cold Pleistocene, and he attributed their uncharacteristically narrow reefs to the relative shortness of time since post-Glacial conditions had allowed them to begin growing.

According to Daly, neither the "Darwin-Dana hypothesis" nor Murray's view could account for the "remarkable flatness" and nearly uniform depth of the plateaus that bear atolls and barrier reefs. They were explained perfectly, however, by Pleistocene marine erosion, while the existence and form of the reefs themselves could be explained by the subsequent warming and rising of the seas. Yet he characterized his theory of 1909-1910 as a supplement to the Murray-Agassiz view that most atolls and barrier reefs were formed during periods of crustal repose, and not via the "enormous crustal displacements" that the subsidence theory required. "Correlating ice-caps and coral reefs," he declared, uses "the great discovery of Louis Agassiz to support a principle conclusion of Alexander Agassiz. To the father, a zoologist, geology owes the glacial theory; to the son, a zoologist, geology owes a matchless collection of facts, which not only illuminate the theory of coral reefs, but also profoundly affect the problem of crustal deformation in the oceanic areas."[77] Evidently Daly believed that both the senior and the junior Agassiz deserved only the title of zoologist, despite the fact that each man had been overtly and directly engaged with the coral reef problem that Daly considered to be part of geology.

In 1915 Daly offered a massively expanded version of the argument and gave it name, "the Glacial-control theory."[78] Relying upon a much wider body of ocean charts and literature on glaciation, sea level change, and coral growth than he had previously cited, Daly amplified the sample of quantitative data in his argument, although the resulting numbers showed the same correlations that had been drawn in his "preliminary" 1910 paper. There were two main substantive changes to the argument for Glacial-control. The first was to propose that old oceanic islands had been significantly truncated even before the Glacial period, perhaps because a protective reef fauna had not evolved before, or long before, the Pleistocene.[79] This meant that the truncation of large islands did not have to have occurred entirely within the relatively brief Pleistocene . The second change was to propose the principle of "mud-control," which postulated that lagoons and other features were partly the result of post-Glacial coral growth in the center of plateaus being inhibited by sediments that were stirred up when the warming sea rose.[80]

Daly also set out a series of criticisms of the Darwin-Dana subsidence theory that dealt in turn with points of substance, of method, and of presentation. He reiterated the claim that widespread equable subsidence on the order of thousands of feet was much less likely than general stability of the crust, which Daly proposed and which would have allowed for uninterrupted pre-Glacial truncation of the sort he had newly

77. Daly, "Pleistocene Glaciation and the Coral Reef Problem," 308.
78. Reginald A. Daly, "The Glacial-Control Theory of Coral Reefs," *Proceedings of the American Academy of Arts and Sciences* 51, no. 4 (1915): 155–251.
79. Daly, "The Glacial-Control Theory of Coral Reefs," 160.
80. Daly, "The Glacial-Control Theory of Coral Reefs," 211–12.

invoked. By 1915, however, Daly had all but ceased to associate crustal stability with the name of Murray or Agassiz, and he now treated "Glacial-control" as a full theory independently capable of competing against subsidence .

Daly's criticism of Darwin and Dana's method and presentation centered on a historically-oriented discussion of the ways in which coral reefs were represented on paper. "The subsidence theory," he argued, "was invented chiefly to explain the ground-plans, *maps,* of the surface reefs; that is, one topographic element was emphasized, and [from that perspective] the evidence of submergence is certainly good. But the same principle of questioning the existing topography -- portrayed in *charts*, full of soundings -- suggests as clearly that submergence has been strictly limited."[81] At this point in his text, Daly reproduced a portion of an Admiralty chart showing several hundred soundings over and around the submarine banks north of the Laccadives, which exemplified his argument that platform depths were uniform and relatively shallow, whether the platforms were rimmed by upgrown coral reefs or not. "No other banks better show the independent origin of reef and platform," Daly urged. "The topographic unconformity 'leaps to the eye.'"[82] The idea was that this topographic unconformity coincided with a true, but unobservable, stratigraphic unconformity (that is, a line of discontinuity) between the layer of modern reef rock that formed the atoll rim and the older, unrelated rock of the platform on which it stood.[83] In order for the subsidence theory to explain such an unconformity, Daly believed, its advocates would have to postulate a long pause in the sinking of the foundation. If such an important "accessory assumption" were added to the subsidence theory, then subsidence would actually be

81. Daly, "The Glacial-Control Theory of Coral Reefs," 240.
82. Daly, "The Glacial-Control Theory of Coral Reefs," 242.
83. "Unconformities" in the stratigraphic record were lines of discontinuity between one stratum and another, and they were taken to indicate the passage of some length of time during which deposition of new rock did not occur. In Daly's case, this interval would have represented the end of Pleistocene erosion and the beginning of post-Glacial reef building.

rendered a comparatively insignificant part of the theory. Because this was what modern bathymetric charts demanded, according to Daly, its advocates must face the reality that "The subsidence theory is not simple, as so often claimed."[84]

Daly argued that the subsidence theory had been made to *look* simpler than it really was. In his criticism of "the psychological influence of classic diagrams," he pointed out that Darwin and Dana's published woodcuts, which showed reefs being formed around a sinking island, were severely skewed in the vertical dimension. These exaggerated the slope of the original island and misrepresented what would be the eventual depth and volume of the lagoon. Daly claimed that the legends to these diagrams did not admit to the vertical exaggeration (although Darwin actually did do so in his text). "Undoubtedly." he concluded, "the subsidence theory has too long enjoyed the fictitious aid of imperfect diagrams, which have been studied in, or copied from, the classic works." This was a begrudging recognition of the rhetorical effectiveness of Darwin's 1842 sectional diagrams (upon which, see chapter 3).

Daly pointed out that there was also a possibly-unintended negative consequence of the Darwin-Dana diagrams. They portrayed encircling reefs contracting in diameter as they grew upward, whereas Daly believed, like many reef students of the previous three decades, that reefs grew upward and *outward* upon the outer talus. This meant that the Darwin-Dana diagrams gave the deceptive impression that a boring carried out on the rim of an atoll, as at Funafuti, could actually serve as a "vital test" of the theory, when in fact such a bore would pass through talus below the living part of the outer reef, *even if the reef had built up during subsidence*. For the "primary purpose of testing the Darwin-Dana theory," Daly explained, "[a] truly valuable test can be made by boring on a coral islet situated within the lagoon of a typical atoll, about midway between the main

84. Daly, "The Glacial-Control Theory of Coral Reefs," 244.

reef and the lagoon center."[85] Daly recommended a specific locality that would be ideal for such an examination: Breakfast Island, in the lagoon of Jaluit Atoll, one of the Marshall Islands. The Marshall Islands, which came under German control in 1885, were in fact an unpromising choice for a field site, having been seized by Japan the previous year. At the end of World War I, they were among the Pacific islands formally ceded to Japan by a League of Nations mandate.

Daly almost certainly identified Breakfast Island by studying a chart, and in this reliance on library-based research he had much in common with Darwin. Part of the objective of Daly's long paper had been to identify the points on which the subsidence theory and the glacial-control theory made different predictions about the nature of reefs, and to encourage field workers to make quantitative studies of the salient phenomena in order to rule in favor of one theory or the other.[86] Yet he concluded his paper by arguing that the truly decisive data would not be produced by any individual coral reef specialist. "The writer offers no apology for entering the coral-reef controversy," despite what might have appeared to be his lack of relevant field experience.[87] "The facts show that the problem cannot be solved merely and only from the data secured by intensive study of the reefs themselves [...] A life-time spent in a personal study of the reefs would add little to the easily accessible bathometric [sic] facts." Daly lamented "the all too common failure of writers on this problem to value properly the facts obtained by a host of nameless investigators, whose results appear on hundreds of hydrographic charts. Nor will years of field experience in the coral archipelagoes alone give the observer the facts which more and more clearly show that the history of reefs is bound up with the question of world climate during post-Tertiary

85. Daly, "The Glacial-Control Theory of Coral Reefs," 218–19.
86. See especially Daly, "The Glacial-Control Theory of Coral Reefs," 250.
87. Daly, "The Glacial-Control Theory of Coral Reefs," 250.

time."[88] For Daly, the solution to the coral reef problem lay in the detached compilation and consideration of the results of many others' fieldwork in many sites. He may have been lured into the study of reef formation by his experience in the Hawaiian islands, but in Daly's analysis the results of his field observations paled in comparison to the evidence he had marshalled at home.

In his 1915 general address to the American Association for the Advancement of Science, Daly went one step beyond this by specifying how fieldworkers could contribute best to home bound synthetic thinking. Gesturing to the success of Americans' cooperation in creating the Panama canal, which had been open just less than a year, he proposed a plan of "comprehensive exploration" of Pacific islands, to be undertaken by citizens in "private association."[89] What Daly envisioned was a systematic collaboration of specialist fieldworkers, whose results and collections would be gathered at a centralized institution where comparative study would eliminate "the waste of effort and the danger of positive error that are due to the compartment method of study." The compartment method was, of course, the very specialization required of fieldworkers. As Daly said, "analysis must precede synthesis and specialization is increasingly more necessary for the field worker. Hence the only economical way of reaching the truth of nature is to co-operate, first, last, and all the time." This in turn would demand a new method of field work, aimed at comparative study of many islands rather than isolated study of any one location. "A body of specialists, *together* questioning *all* of the Pacific oceanic islands, are sure to reach final results quicker than is possible to a much greater number of equally capable men who independently 'monograph' island or island group."[90] Daly recognized that his plan "implies that the

88. Daly, "The Glacial-Control Theory of Coral Reefs," 251.
89. Reginald A. Daly, "Problems of the Pacific Islands," *American Journal of Science* 4th Series, Vol. 41 (1916): 154.
90. Daly, "Problems of the Pacific Islands," 155.

specialist observers should be in the field a long time," and he pointed out how this would be advantageous compared to the "ordinary 'monographic' method of work." Specialists could have "ample *time* for...field studies," and eliminate "the loss the science involved in the normal obligation to leave the field at the end of one or two seasons."[91] They could make long-term series of "direct measurements" of coral accumulation, volcano activity, and island erosion; they could catalogue changes to fauna and flora; and they could undertake "[m]utational and experimental studies of endless variety [that] need to run several years before valuable results are attainable."[92] These data would each be more valuable because they were part of a larger set produced by multiple specialists. Henceforth scientists could avoid the problem of drawing general conclusions from the monographic study of what might be an atypical location, a style of research that presently gave "misleading results and represent[ed] energy worse than wasted."[93] It was, as Daly candidly described it, a plan with "a special advantage owing to its very bigness."[94] It was a dream scenario for someone who was trying to solve the scientific puzzles of the Pacific islands by compiling the scattered and inconsistent observations of fieldworkers, specialist and non-specialist alike, who had gone before.

Daly was not the only person in Cambridge, Massachusetts who was researching the coral reef problem this way. In 1914 the just-retired William Morris Davis, who had never seen a reef, published a provocatively titled three-part article on "The Home Study of Coral Reefs."[95] It was an essay on scientific method that happened to have

91. Daly, "Problems of the Pacific Islands," 156.
92. Daly, "Problems of the Pacific Islands," 156.
93. Daly, "Problems of the Pacific Islands," 155.
94. Daly, "Problems of the Pacific Islands," 156.
95. William Morris Davis, "The Home Study of Coral Reefs, Part I," *Bulletin of the American Geographical Society* 46, no. 8 (1914): 561–77; William Morris Davis, "The Home Study of Coral Reefs, Part II," *Bulletin of the American Geographical Society* 46, no. 9 (1914): 641–54; William Morris Davis, "The Home Study of Coral Reefs, Part III," *Bulletin of the American Geographical*

coral reef formation as its subject. It had grown out of a lecture that he had delivered to undergraduates at several universities, including Oxford and Cambridge in England, and it was aimed at an audience of earnest students and scientific readers in general "who have the ambition to become critical as to the sufficiency of scientific investigations, who are interested in the origin of coral reefs as well as in many other problems, and who may wish to form, each for himself, a carefully systematized method of procedure."[96] His case study was to demonstrate how "a geographer or a geologist, who has never seen any coral reefs [and] nevertheless has occasion to give some account of their origin" might generate an interpretation that was independent of the results of any particular fieldworker.[97] The method proceeded in several steps. The home student should gather and order observations made by past observers in order inductively to establish generalizations about the phenomenon of coral reefs. Then he should invent "as many provisional schemes, or 'working hypotheses,' as possible, each embodying an...imagined sequence of changes in past time" that would explain the general characteristics of present-day coral reefs. Next the student was required to deduce all possible consequences of each working hypothesis, and then to confront these predictions with known facts and to use them to direct an "observational search for previously unseen facts." Observations isolated from theory could not discover the origin of coral reefs, which "involves the action of unobservable processes during irrecoverable past time." Thus, Davis explained, the old adage "Go and see" should, "in studying problematic phenomena like coral reefs, be replaced with the newer adage: 'See and think.'"[98]

Society 46, no. 10 (1914): 721–39.

96. Davis, "Home Study I," 571.

97. Davis, "Home Study I," 561.

98. Davis, "Home Study I," 562–63.

Davis's home study of reefs revealed that there were eight hypotheses that might account for the origin of the general features of oceanic coral reefs.[99] None was original to Davis, as each one explicitly corresponded to a theory previously put forth by one or more earlier observers, including those of Lyell, Murray (on atolls), Murray (on barrier reefs), Agassiz, Wharton, Darwin, and Daly. He treated each hypothesis in turn, describing its postulates and the available facts that bore on them. Although he drew his hypotheses from prior authors, he did not hesitate to expand on their original deductions. Thus in the case of the subsidence hypothesis he explained that "Darwin's postulate of general subsidence over large areas may have to be modified in view of facts discovered since his time," referring to the discovery of many elevated reefs intervening in the coral areas of the Pacific.[100] He also pointed out that Darwin had failed to deduce two important conclusions of the subsidence hypothesis: that river valleys on high land would be drowned during subsidence, leaving an embayed shoreline within a barrier reef (already pointed out by Dana), and that the faunas of neighboring barrier reef-encircled islands should be similar because they would have derived from one ancestral stock that split only when the islands were separated by subsidence.[101] Davis nevertheless attributed the subsidence hypothesis with all its original and revised postulates, to Darwin as the man who had first stated its "essential principle."[102] Davis found that each of these predictions was supported by known facts, lending great support to the subsidence hypothesis. The only hypothesis that offered serious competition for subsidence, in Davis's view, was Daly's "exceptionally

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99. Davis wrote that reefs on continental borders could be treated as equivalent to those surrounding large islands. Davis, "Home Study I," 573.
100. Davis, "Home Study II," 648.
101. Davis specified that this consequence was applicable to the similarity of those types of plants and animals that could not have migrated from one island to another after they were separated. Davis, "Home Study II," 650–54.
102. Davis, "Home Study II," 648.

ingenious theory" of Glacial-control.[103] However, he declared known facts to be at best inconclusive when compared to Glacial-control's hypothetical consequences. For example, because Pleistocene sea level changes ought to have been global phenomena, Daly's postulated wave-cut benches ought to be found more universally than they had yet been.[104] Davis concluded that the Glacial effects might be "superposed" on the "dominating" effects of subsidence, leaving a solution that "would, geologically considered, take rank as a subordinate complication of Darwin's theory...but it would be of little import geographically [as compared to Darwin's unsupplemented theory] because its consequences would be so generally invisible."[105]

Davis's essay revealed that an ideal theory was highly general, but that real solutions to complex problems like the origin of coral reefs were likely to include underlying complexities. He believed that in order to be satisfactory, one theory must account for both atolls and barrier reefs. He again revealed that he identified theories with some essential tenet that kept them recognizable even if they had been modified. Thus, such "subordinate modifications" of a generally successful theory with the elements of less successful ones would "not lessen the value of a primary principle, but merely embroider secondary complications upon it, in the same way that the attractions of the planets cause minor perturbations in the earth's orbit, of which the general form is controlled by the dominating attraction of the sun."[106] This was a reminder not to generalize too quickly, even from the most decisive local case studies. "Even if a few selected reefs are perforated by many deep borings, so that their structure is well determined and their origin demonstrated," Davis explained, "it will not follow that all

103. Davis, "Home Study III," 728.
104. Davis, "Home Study III," 733–34.
105. Davis, "Home Study III," 734.
106. Davis, "Home Study III," 737.

other reefs are of the same structure and origin."[107] This sentence was telling, because it revealed that in Davis's opinion the ultimate test of the formation of any given reef, however typical, was deep boring.

Davis finished his article with a punch line that revealed why he had chosen coral reef formation to be the subject of his didactic methodological essay. He was planning to visit the Pacific himself. His home study had prepared him to enter the field with his mind open to multiple hypotheses and his eye trained on the details that would let him judge between them. Previous scholars, whether they had studied reefs in person or not, had been too willing to abandon old reef hypotheses for new ones "without asking for any test--such as drowned valley embayments or peculiar biotic relations--by which a discriminating choice might be made between the two theories."[108] Davis's home study would thus orient him when he arrived in the field.

In Davis's field excursion of 1914 he adopted a method of study that was not mentioned in his "home study" article, but which was logically consistent with its argument. This was the physiographic inspection of many different reefs in rapid succession. Traveling "for the most part in comfort" on commercial steamers, trading ships, and local sailboats, he saw a total of thirty-five islands, including the coral formations of the Fijis, the New Hebrides, and several other groups.[109] He learned of the impending disaster in Europe on 31 July 1914, three days after the Archduke was assassinated, upon arrival at Sydney. After spending the month of August 1914 at the meeting of the BAAS in Australia (the Association had supplemented the cost of his journey), he took a steam cruise up the Queensland coast in the lagoon of the Great Barrier Reef. On this journey, he wrote in 1928, "my attention was given chiefly to the

107. Davis, "Home Study III," 737.
108. Davis, "Home Study III," 735.
109. Davis, *The Coral Reef Problem*, 143–45; David R. Stoddart, "Theory and Reality"; William Morris Davis, "A Shaler Memorial Study of Coral Reefs," *American Journal of Science* (4th ser.) 40, no. 237 (1915): 224.

features of the coast. An overnight stop on one of the reef islands of Cairns was an entertaining experience but, as might have been expected, entirely fruitless as far as the origin of the reef is concerned."[110] On his return trip to the United States he spent a week at the Society Islands, where he discovered that the French colonial capital of Papeete had recently been bombarded by a pair of German cruisers.[111]

The paper on his voyage that Davis published the next year made plain that his faith in Darwin's theory was stronger than ever, and that he was less concerned with explaining any given location in detail than in bringing the collective observations of his extensive travels to bear on the existing general hypotheses of reef formation. He explained that he had focused his research (both personal observation and home study) on phenomena related to barrier reefs and elevated reefs, because the origin of fringing reefs was uncontested and that of atolls was "inscrutable unless [they are] penetrated by numerous and expensive borings, for they stand alone and bury their past."[112] It was not the barrier reefs themselves, however, that were worth studying. Because each serious theory of coral reef formation was "successful in explaining the visible features of sea-level reefs themselves," trying to judge between theories by looking at the sea-level reefs, Davis argued, was utterly futile. "I repeatedly took occasion to test the truth of this statement," he announced, "while wading upon a well-formed barrier reef, beaten by the surf on its exposed front, swept over by the foaming surge, and backed by the quieter waters of the lagoon. [...] Sea-level coral reefs, taken alone, do not afford any sufficient test [of their origin]."[113] In a single breath Davis asserted that he belonged among the fraternity of reef field workers who had ventured to stand amidst the breakers on a reef bulwark and declared that a man in this position was impotent to solve the

110. Davis, *The Coral Reef Problem*, 347.
111. Davis, *The Coral Reef Problem*, 145.
112. Davis, "A Shaler Memorial Study of Coral Reefs," 225.
113. Davis, "A Shaler Memorial Study of Coral Reefs," 224–25.

coral reef problem.

Because reefs themselves offered no testimony that might settle the question, Davis sought to interrogate "witnesses of some other kind, which were present when the reefs were forming and which are willing to testify about the events which then took place." These witnesses were the shorelines of the high lands adjacent to barrier reefs, and the structures of uplifted reefs. Davis discussed these phenomena in essentially the same way as he had done in his "Home Study" article, as diagnostic features of landscapes that need not themselves be explained. He allowed working hypotheses not only to direct his gaze in the field, but also to structure the way he reported the results of these observations.[114] His paper was primarily a logical exercise in working out the predictions of rival theories (with much repetition from the pre-expedition article), with facts about the locations he studied in the field deployed when needed, but not otherwise synthesized. More important to his exposition than any field site or group of field sites were the idealized block diagrams that illustrated the "deduced stages" of different reef types under different conditions.[115] Stoddart has analyzed these images and concluded that they were often at odds with the empirical evidence with which Davis had been confronted in the field.[116] Davis concluded that while questions remained to be answered about coral reef formation, submergence of foundations offered the best general explanation for the then-known facts. He further concluded that the required change in the relative levels of land and sea had most likely been effected by subsidence, as predicted by Darwin.

When Davis had returned from his long steamer cruise he found a copy of Mayer's ecological study of the Murray Island reef. It was, of course, based on a model

114. Davis did later publish monographic studies of individual Pacific locales; unfortunately I have not been able to examine them.
115. See, for example, Davis, "A Shaler Memorial Study of Coral Reefs," 239, 240, 251.
116. David R. Stoddart, "Theory and Reality."

of fieldwork in stark contrast to his own; the nearly simultaneous trips to the same part of the globe could hardly have employed less similar strategies. Mayer had used the weeks at his disposal to concentrate on a single location rather than to catch a glimpse of as much terrain as possible; he had studied the organic propagation of a living reef; and his first object in publication had been the characterization of a field site rather than the elaboration of general principles. In his response to Mayer in a private letter, Davis was candid about the gaps in his own knowledge, and he expressed a desire to expand his range. "I have just read [the Murray Island paper] with interest," he enthused. "It makes me regret my own ignorance of zoological matters."[117] It seems that this was more than an empty compliment, because he asked Mayer for help in identifying "what I suppose were zoophytes of some kind," and for advice in naming the reef feature of which they were the main constituent. He did also, to be sure tutor Mayer on physiographic arguments that he believed Mayer was overlooking, particularly those that weakened the Glacial-control theory. Within a year or two, for reasons that are not entirely clear, Davis abandoned the inclusive perspective that he entertained in this letter and helped to polarize the coral reef problem for participants on both sides of the Atlantic.

As he continued to publish on the coral reef problem, Davis increasingly began to frame his criticism of others' work in terms of their scientific discipline, while his arguments about reef formation itself remained quite consistent. Thus in his 1916 article on "Problems associated with the study of coral reefs" he specified that the scientist who would try ineffectually to determine the origin of coral formations from

117. W.M. Davis to A.G. Mayer, 2 December 1914. Hyatt and Mayer (Mayor) papers, PU MSS C0076, box 2, folder "Davis, William Morris, 1850-1934." Incidentally, this folder contains a handful of letters (including a very friendly one to "Miss Hyatt" of 11 July 1897, signed "Neighbor Davis") which suggest that Davis had known Mayer's wife, the daughter of the Boston zoologist and science administrator Alpheus Hyatt, for many years.

the study of living reefs was most likely to be a zoologist.[118] "One of the most striking features of coral reefs is their incapacity to reveal the conditions of there origin. True, the observer on a reef, whether he is a zoologist or not, may see the extraordinary luxuriance of coral growth, and may discover that the heavier forms grow under the surf on the outer slope, while the more delicately branching forms frequent the quieter waters of the lagoon. [...] If he be a zoologist, he will revel in the opportunity of study in so superb a natural aquarium [...] But so long as he confined his attention to the sea-level reefs, he will not be able to make sure which one of the eight or nine competing theories [of their origin] is the right one."[119] He criticized as well those geologists who were skeptical of plain physiographic evidence, quoting from an anonymous geologist who balked at the "cumbrous and entirely hypothetical series of upward and downward movements" required in order to explain the origin of upraised reefs by advocates of the subsidence theory. (The nameless foil was one of Davis's transatlantic rivals, Archibald Geikie, quoted from his 1883 address on the origin of coral reefs.[120]) "This pronouncement is, in view of its source, one of the most extraordinary that I have encountered in coral-reef literature," Davis exclaimed. "It tempts me to follow for geologists, the example of the late William James, who divided philosophers into two categories, the tender-minded and the tough-minded. Let us divide geologists into the same two classes, and among the tender-minded place those who hesitate to accept a theory of coral reefs... [because] terrestrial uneasiness is mentally distressing; and among the tough-minded, those who are perfectly ready to follow good evidence

118. William Morris Davis, "Problems Associated with the Study of Coral Reefs, Part I," *The Scientific Monthly* 2, no. 4 (April 1916): 313–33; William Morris Davis, "Problems Associated with the Study of Coral Reefs, Part II," *The Scientific Monthly* 2, no. 5 (May 1916): 479–501; William Morris Davis, "Problems Associated with the Study of Coral Reefs, Part III," *The Scientific Monthly* 2, no. 6 (June 1916): 557–72.

119. Davis, "Problems Associated with the Study of Coral Reefs, Part I," 313–16.

120. Compare Davis, "Problems Associated with the Study of Coral Reefs, Part II," 501; Archibald Geikie, "The Origins of Coral Reefs, II," *Nature* (1883): 127–28.

wherever it leads."[121]

Intensive fieldwork goes global

For those who wanted to follow good evidence back to field sites at the world's coral reefs, the Great War now posed a virtually insurmountable obstacle. In Europe, the war had long-since banished any pretense of normal scientific work or collaboration with international colleagues on the other side, as heartbreaking letters from Potts and Gardiner made clear to Mayer.[122] As an American who had cherished visits to Germany and who considered himself cursed with a German name, Mayer felt this problem acutely. As a man who wished to navigate his CIW research vessel (which was named after the founder of the Naples field station) through the tensely contested waters of the Atlantic, it was a serious practical difficulty. "The already hysterical Island of Jamaica certainly would explode," he told Woodward ruefully when he considered the possibility of a 1915 field season, "if it saw approaching it a powerful military looking boat named 'Anton Dohrn' commanded by a man named 'Mayer.' We can only lie on our oars and wait."[123] Mayer contributed to the war effort while at Princeton by writing a manual on navigation and teaching the subject to future sailors.[124]

Mayer did mange to work at his familiar Tortugas base in 1915, and he parlayed this visit into his first direct statement on theories of coral reef formation. In the resulting publication, "Sub-marine solution of limestone in relation to the Murray-Agassiz theory of coral atolls," he argued that his latest work at Tortugas provided the

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121. Davis, "Problems Associated with the Study of Coral Reefs, Part II," 501.
122. The two most poignant letters are Potts to Mayer, 5 August 1914. Hyatt and Mayer (Mayor) papers, PU MSS C0076, box 6, folder "Potts, Frank A., (fl. 1916)," and Gardiner to Mayer, 8 January 1915. A.G. Mayor papers, SU, Box 1, folder "G."
123. Mayer to Woodward, 17 January 1915 (from Princeton, New Jersey). CIW RG Marine Biology, Box 2, folder 4, "Tortugas Lab – Reports, 1904-1939, 1 of 2."
124. Stephens and Calder, *Seafaring Scientist*, 122.

first quantitative test of the notion that lagoons could be formed by solution. This was, he reported, the kind of data that Vaughan had never been able to offer. Mayer had stored pieces of mollusc shell in seawater under various conditions for a year in order to determine the rates at which seawater could dissolve a layer of limestone. Mayer believed that "many if not all atoll lagoons have been formed since the beginning of Tertiary times," which then-current estimates placed at about three million years ago.[125] Meanwhile, extrapolations from his experiment suggested that solution would require 19,250,000 (or more importantly, on the order of tens of millions of years) to dissolve a layer of calcium carbonate 120 feet thick, that being the depth of a moderate lagoon.[126] Therefore, he argued, "it appears that [atoll lagoons] owe their development to agencies other than that of solution by sea-water," while it seemed possible, but unlikely, that less voluminous barrier reef lagoons had formed this way.[127] Taking the opposite approach, though with very similar methodology, Vaughan argued that experiments on coral growth by himself and Mayer indicated that most living reefs could have grown up to their current thicknesses since the end of the last Ice Age (i.e., since long after the *end* of the Tertiary).[128]

125. For two roughly contemporary estimates of the chronological length of the geologic time scale, each dating the beginning of the Tertiary to three million years ago, see Henry Fairfield Osborn, *The Origin and Evolution of Life: On the Theory of Action Reaction and Interaction of Energy* (New York: Charles Scribner's Sons, 1918), 153; Gilbert van Ingen, "Palaeontology," in *The Americana: A Universal Reference Library*, ed. Frederick Converse Beach (New York: Scientific American Compiling Department, 1912), unpaginated. The CIW President, Woodward, was an Advisory Editor for the latter volume. It is nevertheless puzzling that Mayer believed that lagoons dated to the beginning of the Tertiary. I think this may have been a mistake by Mayor, and that he meant to follow Daly in dating lagoons to the *end* of the Tertiary, i.e., the beginning of the Pleistocene.

126. Note that Mayer's biographers have mistakenly described his conclusions in this experiment, as showing the time it would take to dissolve a layer of calcium carbonate *at* 20 fathoms' depth. Stephens and Calder, *Seafaring Scientist*, 118.

127. Alfred Goldsborough Mayer, "Sub-Marine Solution of Limestone in Relation to the Murray-Agassiz Theory of Coral Atolls," *Proceedings of the National Academy of Sciences* 2 (1916): 29–30 Many readers may know that the Cretaceous-Tertiary boundary is currently dated at ca. 65 million years ago, under which view Mayer's solution experiment would produce the opposite conclusion.

128. T.W. Vaughan, "The Results of Investigations of the Ecology of the Floridian and Bahaman Shoal-Water Corals," *Proceedings of the National Academy of Sciences* 2 (1916): 95–100 See also T.W. Vaughan, "The Present Status of the Investigation of the Origin of Barrier Coral Reefs [A Preface to

Notwithstanding the difficulties presented by the war, and despite struggling with a tubercular cough, Mayer made an extraordinary series of field expeditions in 1917 and 1918. He spent the early part of the 1917 in the Pacific, studying the reef-fringed volcanic island of Tutuila, Samoa, and then spent the summer at the Tortugas laboratory. He spent March to May of 1918 in the West Indies, and then returned to Samoa that summer. On his first visit to Tutuila he continued his field tests of the tenets of the Murray and Agassiz theories of reef formation. He determined (via laboratory analysis by Alexander Phillips) that the waters that ran off the land were alkaline, and so could not contribute to the solution of a lagoon as supposed by his mentors.[129] However, he still found reason to support their claims that a fringing reef could evolve into a barrier reef without subsidence. He conceived that this would happen by the combination of outward growth of living corals upon the fore reef talus and scouring of the shoreward parts of the reef flat caused by strong currents.[130] The present fringing reef was, he argued, "a mere veneer over [a] modern off-shore marine platform."[131] Samoa also provided a venue for further studies on the ratio of coral genera on the reef flat (initiated at Murray Island) and experiments in coral growth, including planting corals of different genera in locations of silty stream discharge to determine the relative effects of dilution and sediment on coral mortality.

On the return visit in 1918 Mayer gathered a series of linked observations that allowed him to estimate the net rate of growth for the reef flat. By reexamining corals that he had "measured, photographed, weighed, marked by numbered brass tags" and planted in the harbor of Pago Pago in 1917, he ascertained the mass of stone that was

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'Relations of Coral Reefs to Crust Movement in the Fiji Islands,' by E.C. Andrews.]," *American Journal of Science* Fourth series, Vol. XLI (1916): 131–35, which also cited the Tortugas research.

129. Alfred Goldsborough Mayor, "Coral Reefs of Tutuila, with Reference to the Murray-Agassiz Solution Theory," *Proceedings of the National Academy of Sciences* 3 (1917): 524.

130. Mayor, "Coral Reefs of Tutuila," 525.

131. Mayor, "Coral Reefs of Tutuila," 522.

added to the average coral head of each reef-building genus. Then, by way of the ecological survey that told him the approximate number of coral heads of each type on the reef flat, he derived a figure for the approximate average annual accumulation of limestone on the reef's upper surface by growth, about 840,000 pounds.[132] From this number, he subtracted the amount of limestone lost each year, which he estimated to be 100,000 pounds as sediment (determined by setting up a sediment trap off the reef flat) and 2.9 million pounds due to organic causes such as the destructive action of sea cucumbers (which he determined by cataloguing the number of such organisms on the reef flat and multiplying by the amount of sand they dissolved each year through their digestive process, which he had determined experimentally at Tortugas the previous summer).[133] The net accumulation of limestone on the reef flat was, therefore, negative. Mayer took this to reinforce the likelihood that reefs naturally advanced seaward from fringing to barrier forms through destruction of the reef flat and vigorous growth of the reef's outer margin. On the same visit, Mayer's engineer, Mills, bored through the fringing reef outside Pago Pago harbor and found the coral limestone to be 121 feet thick, lying on volcanic rock.[134]

Mayer's field research was not unaffected by the war. During his 1917 work at Tortugas, he studied the effects of explosive shock on the nervous systems of invertebrates and fishes.[135] In promoting research that was less obviously relevant to the crisis at hand, he became much more vocal about its practical benefits. In seeking a renewal of permission to visit American Samoa so that he could make his 1918 field trip, Mayer wanted the Secretary of the Navy to know that his scientific research would aid navigation during and after the war. "[W]e desire to return to determine the rate at

132. Alfred Goldsborough Mayor, "The Growth-Rate of Samoan Coral Reefs," *Proceedings of the National Academy of Sciences* 4 (1918): 390–91.
133. Mayor, "The Growth-Rate of Samoan Coral Reefs," 391–92.
134. Stephens and Calder, *Seafaring Scientist*, 128–29.
135. See Stephens and Calder, *Seafaring Scientist*, 122.

which Pacific Coral reefs grow upward -- a piece of information which might well be stated on charts [--] especially of the numerous openings in coral reefs which are now deep and useful as entrances in time of stress, but have coral heads in the bottoms. We now know little or nothing of the growth rate of Pacific corals."[136] At Tobago, during his 1918 West Indian trip, his marine research aroused suspicion and he was accused of subterfuge against the Allies. In the throes of the ensuing diplomatic crisis, which followed him back to the States, he had his surname officially changed to Mayor.[137]

In 1919 Mayor finally managed to get Daly to join him for fieldwork, not at Tortugas as they imagined in 1907, but in the southwest Pacific. Mayor had planned to develop a new field site at the Fijis, but his party was prevented from landing there because of quarantines intended to prevent the spread of that year's influenza epidemic.[138] Instead the party had to resume study of the American islands of Samoa, where they spent two full months. Mayor used a diving helmet to study the outer margin of the reef, where he planted more living corals down to a depth of 52 feet. He reported to Woodward that "We really entered a new world in so far as scientific study is concerned when we got down on the submarine precipice under the breakers, and many new facts have come to light."[139] Mills did another boring, finding "wave worn basalt" beneath the living reef, which showed that the present fringing reefs were not superimposed upon the remains of other pre-Glacial reefs.

136. Mayer to Woodward, 15 January 1918. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920."

137. Mayer's Tobagan disaster and its repercussions are covered by a series of letters in CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920." See also Stephens and Calder, *Seafaring Scientist*, 125–26.

138. On the initial plans: Mayor to Woodward, 9 May 1919. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920." On the influenza quarantines: Mayor to Woodward, 28 September 1919. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920."

139. Mayor to Woodward, 28 September 1919. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920." On earlier use of the diving helmet at Tortugas in 1915, see Stephens and Calder, *Seafaring Scientist*, 130.

A clue to the approach that Daly took to his work in Samoa is given by an article that went into press around the time he was in there. Here he argued that "The coral-reef problem now specially needs the attention of geologists, the zoologists largely having furnished their share of the required data. [...] Much of the voluminous literature on the coral-reef problem has little value because authors have not insistently done their utmost to imagine the tropical geography at stages receding the present, that is, to think geologically." He lamented that "Unfortunately, few geologists have had the opportunity to study reefs in the field or to absorb the information embodied in modern ocean charts and the facts recently acquired by the oceanographers." Daly's field notes indicate that when he was in the field, what he studied were the same sorts of features he would have examined on topographic maps and charts. He gave most of his attention to surveying landscape features on foot and by boat, keeping many records of the directions and heights of features. He also obtained series of soundings perpendicular to the reef front at several points. The specimens he collected are referred to by number in the field notes, but presumably were rock samples. He did do some sounding perpendicular to the front of a reef or two. Having seen little of actual reefs during his 1909 Hawaiian expedition, he frequently noted Mayor's opinion about what was typical of Pacific reefs.[140] Evidently, he was aided in his work by the use of photographic reproductions of manuscript charts of several of the American Samoan islands provided him by the U.S. Hydrographic Office.[141] Of Daly's fieldwork, Mayor reported to Woodward that he had done "a geological survey of the whole group of Islands in American Samoa except [the lone low island of] Rose Atoll."[142]

140. R.A. Daly field notebooks, Samoa, 1919 (2 volumes). HU-P HUG 4315.55. These notes are dense and heavily abbreviated, so rather than quoting specific experiences I have digested them as well as possible.

141. Reginald A. Daly, "The Geology of American Samoa [Report]," *Carnegie Institution of Washington Year Book*, no. 18 (1919): 192.

142. Mayor to Woodward, 28 September 1919. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920."

The following year Mayor returned yet again to the Pacific with a larger group of specialists, more closely approximating the scale of the pre-war Murray Island expedition. Among the participants were several of the members of that previous venture, including the botanist Setchell and the British zoologist Potts. The geologist was not Daly, but Rollin T. Chamberlin, the son of T.C. Chamberlin of "multiple working hypotheses" fame. Responding to a query from Mayor, Vaughan had endorsed him as "an able, careful, and critical geologist, and in my opinion, is one of the coming young men of the country who has already in large measure made good. If you could get him to go with you I am confident that you would make no mistake."[143]

The size of the crew matched Mayor's ambitious plans for the expedition. When Woodward had given the go-ahead for the expensive 1920 trip, even before the 1919 trip had begun, Mayor thanked him by saying "it now becomes our duty to get up a better volume on coral reefs than any that has yet appeared; and with the men I have in mind I am sure we can do it. [...] I feel quite like a school boy of 16 over the prospect of 1920 and the coral reefs!"[144] While returning home on the 1919 expedition he reiterated to Woodward that it was the breadth of his colleagues expertise that made these intensive studies so valuable. "I hope to visit Fiji and to go around the world next year when conditions of travel permit and I hope to have a good set of specialists with us in Samoa and Fiji next year so that we may gather material for a volume upon Pacific reefs that will be better than the recent Murray Island volume."[145] After spending time with Daly he had become all the more eager to link his quantitative zoological reef studies with the geologist's conjectured reef histories.

143. T.W. Vaughan to A.G. Mayor, 24 January 1920. A.G. Mayor papers, SU, box 5, folder "V."
144. Mayor to Woodward, 17 May 1919. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920."
145. Mayor to Woodward, 28 September 1919. CIW RG Marine Biology, box 1, folder 18, "Tortugas Lab – Mayer's Trips, 1912-1920."

> I think we can prove that the present Pacific reefs are all recent not more than 20 000 years old and all have developed since the last Wisconsin glacial period. Tutuila gives positive evidence on this point, and we will be able to tell the age of the reefs here with almost mathematical exactitude when we get up our extensive planting of corals, corroded limestones etc in 1920.
>
> This field in the Pacific has never been studied as we are now working upon it. We are the first to go down on the breaker-pounded outer face of reef precipices, and the first to really get the growth rate of Pacific corals, or to make an intensive study of the ecology and physiology and associations of corals over a Pacific reef.[146]

The 1920 party made it to the elusive Fijis as well as the Samoas. Mills drilled for core samples, Mayor worked in the diving helmet, and Potts studied invertebrate growth rates. In keeping with the approach of his fellow geologist, Daly, Chamberlin concentrated on the physiography and bathymetry of the volcanic islands of these groups in relation to reef formation. At the invitation of the (American) Governor of Samoa, Mayor visited the single atoll of the group, Rose Atoll, which had been undescribed in scientific literature since the works of Couthouy and Dana.[147] He found that the visible rock of the reef rim was composed almost entirely of calcareous algae, which proved to be one of Setchell's leading examples in an article that amplified Howe's 1912 argument that plants must be included among the important reef building organisms by declaring that "without nullipores no 'coral reefs' can be or would have been formed."[148]

Mayor's fieldwork of 1919 and 1920 lured him further into the debate over general theories of reef formation with the likes of Vaughan, Daly, and Davis.[149] He was on the whole critical of the genealogical component of Darwin's theory that made

146. Mayor to Woodward, October 6, 1919. CIW RG Marine Biology, box 1, folder 19, "Miscellaneous, 1911-1919 1 of 3."

147. Alfred Goldsborough Mayor, "Rose Atoll, American Samoa," *Proceedings of the American Philosophical Society* 60 (1921): 62–70.

148. William Albert Setchell, "Nullipore Versus Coral in Reef-Formation," *Proceedings of the American Philosophical Society* 65, no. 2 (1926): 140.

149. See, e.g., Alfred Goldsborough Mayor, "The Reefs of Tutuila, Samoa, in Their Relation to Coral Reef Theories," *Proceedings of the American Philosophical Society* 59 (1920): 224–36.

subsidence the agent of qualitative changes to reef form, and particularly of the fundamentalist version propounded by Davis. "Davis is the most active defender of Darwin's coral reef theory," he noted, "yet the sequence of fringing reefs being converted into barrier reefs through subsidence of the land or by rise of sea level, and finally the conversion of these barrier reefs into atoll rims has not been proven even in a single instance, although it is the crux of Darwin's theory."[150] Mayor's experiments on coral growth rates inclined him to share Daly's belief that living reefs were all post-Glacial, but he was unconvinced that Glacial sea level changed had played any major role in determining reef distribution or morphology. Rather, he believed that living Pacific reefs were primarily formed on platforms of erosion, that there had been little or no shift of level in their short existence, and that their shapes were in the main a function of differential coral growth and erosion.

After the decade of American-based coral reef debate sparked by Daly's first paper, its protagonists took stock in a series of letters to one another. Vaughan and Mayor maintained their correspondence, but each also had exchanges with Davis in which each was critical of his relative inexperience in the field. Daly and Mayor were personally sympathetic to one another, and difference between their methodological views had narrowed during their 1919 trip to the Pacific. Davis was the antagonist to all. He was critical of Mayor's unfamiliarity with geology and with Darwin's theory, and he disagreed with Daly's conclusions even when he supported his physiographic method.

In the summer of 1920 Davis and Daly began an unusual exchange, which was evidently the only occasion when they bothered to conduct a private debate on the topic in writing. Davis was planning a monograph on the coral reef problem that would, like

150. Mayor, "The Reefs of Tutuila, Samoa, in Their Relation to Coral Reef Theories," 234 Davis responded in William Morris Davis, "The Coral Reefs of Tutuila, Samoa," *Science* 53 (1921): 559–65.

his many essays, analyze the features of reefs and the adjacent lands and consider the evidence for and against each coral theory. He wrote, "My Dear Daly; One of the most difficult things that I know of is to criticize a theory in which one does not believe, in such a manner as to seem entirely fair to the author of the theory. [...] One of the reasons that I have found it difficult to criticize your theory is that it has changed so much in its successive presentations."[151] Davis complained that Daly had shifted from emphasizing coral mortality and platform abrasion during the Pleistocene, in 1910, to supposing that mud was the chief factor in 1915, that he had been unclear about where and when he believed subsidence may have happened in the Pacific, and that Daly had made inconsistent statements as to which island groups "exemplified" Glacial-control. Davis's work in progress contained a chapter of 150 manuscript pages on Daly's theory alone. "I debated much as to how to treat that chapter. My conclusion is that it should be written just as if we did not live next door; just as if I were in Australia and you here, or vice versa." An exchange of several letters followed; Daly believed that "On account of the complexity of the subject, it seems best to continue the discussion in writing."[152] He told Davis, "Of course I wish that you would criticize my theory impersonally; the question of being neighbors or in the relation of master and student surely ought not to arise when it is a case of scientific discussion. The curious thing is, that being so close, the difficulty of clearly grasping views is so great." He explained that the 1910 paper had been "merely a preliminary sketch," and that his ideas had been made "clearer and fuller" as a result of reading Davis's papers and discussing the problem with Mayor and Vaughan. The letters between the two dealt with many minute points of detail, especially relating to the history of elevation and subsidence at the Fijis, the origins of

151. W.M. Davis to Daly, 18 June 1920. HU-P, HUG 4315.5 "Reginald A. Daly Correspondence circa 1919-1955."

152. Daly to Davis, 21 June 1920. HU-P, HUG 4315.5 "Reginald A. Daly Correspondence circa 1919-1955."

the Tutuila fringing reef and the submarine platform there, and the platform of the Great Barrier Reef. They also haggled over the nature of the subsidence theory, with Davis criticizing Daly for misconstruing Darwin's ideas and Daly responding that the present version of the subsidence theory did not consist exclusively of things that Darwin had said. Davis was pessimistic that they would reach a resolution, explaining "As to your and my interpretations of various facts and theories, I have no expectation of bringing them into agreement; chiefly because our way of attack seems to me fundamentally different."[153] A chief point of difference, it seemed, was that Daly took his friend Mayor seriously even when Mayor was not writing about his own field of biology. Of some disputed cliffs at Tahiti whose presence might give evidence of a still-stand between the sea and land, and about which Daly quoted Mayor's description, Davis scoffed, "I doubt if Mayor would recognize such battered cliffs as cliffs at all. Remember, he is not very clear on physiography."[154] Daly, meanwhile, had cited the time he spent in the field with Mayor as the source of several of his refinements to the Glacial-control theory, and he turned his field experience to his advantage against the only coral reef author who gave more credence to physiographic data than had Daly himself.[155]

Criticizing Davis's lack of first-hand familiarity with growing reefs was a common tactic for all his opponents, but none did it so emphatically as the champion of intensive study, Mayor. Davis had irked him by disputing the way that Mayor had classified some reefs, pointing out to him that Darwin had defined the point of distinction between fringing and barrier reefs as the depth of the water they stood in (no

153. Davis to Daly, 27 June 1920. HU-P HUG 4315.5 "Reginald A. Daly Correspondence circa 1919-1955."

154. Davis to Daly, 27 June 1920. HU-P HUG 4315.5 "Reginald A. Daly Correspondence circa 1919-1955."

155. Daly to Davis, 21 June 1920 and 28 June 1920. HU-P, HUG 4315.5 "Reginald A. Daly Correspondence circa 1919-1955."

deeper than, and deeper than, the zone of coral growth, respectively), rather than their distance from shore. "I think you misunderstand Darwin here," Davis told Mayor. "You may not like this classification; I don't; but such it is...And being so, it follows that the barrier reef of Tutuila grew up from what Darwin would have called a fringing reef."[156] To Mayor this was pure obfuscation on Davis's part. On 7 October 1920 Mayor drafted a letter to Davis that declared, "I certainly cannot agree with any classification by Darwin or anyone which calls a Barrier reef a fringing reef for everyone knows that a fringing reef is so shallow that one can wade out straight from the shore to the outer edge. I do not know of a single reef in the Pacific in which there is any doubt as to whether it is a fringing reef and has grown outward from contact with the shore or a barrier reef which has had no contact with the shore."[157]

To a field-worker, Mayor believed, the current between a barrier reef and the shore made it impossible to mistake for a fringing reef. This current, indeed, was often so strong that it allowed new fringing reefs often to grow up along shorelines that were already encircled by an offshore barrier reef, whereas the existence of such fringing reef-barrier reef combinations was a fact that Davis, on principles learned from Darwin, vehemently denied. Mayor almost tauntingly wrote, "If you will use a diving suit and go down in the growing edge of these fringing reefs [inside barrier reefs] you will be convinced of – 1. the strong current hard to stand against – 2. the luxuriant growth of densely crowded Acropora, etc. I notice in your suppositions respecting the ease with which a reef may fill up you do not consider the currents. Take an Ekman meter and study them and see for yourself what the actual condition is." Whereas Davis had great admiration for Darwin's original coral publication, Mayor did not consider it authoritative, either as an account of coral reefs or as a statement of the possibility of

156. W.M. Davis to A.G. Mayor, 5 October 1920. A.G. Mayor papers, SU, Box 1, folder "D."
157. A.G. Mayor to W.M. Davis, 7 October 1920. A.G. Mayor papers, SU, Box 5, folder "Outgoing Correspondence."

barrier reefs and atolls forming by subsidence. "I see no advantage whatsoever in attempting to confuse the issue by clinging to Darwin's erroneous and hasty definition of a fringing reef [equals a] barrier reef when the two are wholly distinct in nature. Of course if you say that reefs with a lagoon between them and the shore have frequently arisen on previously submerged off shore platforms I agree but must insist on calling these barrier reefs as does also Vaughan, Andrews, Hedley and other modern students. Aristotle says flies have 4 legs but modern students do not state their legs as 4 [equals] 6 nor does a fringing reef [equal] a barrier reef. Pardon this abruptness I know you like a hot fight and are a fair fighter so come back at me!"[158] The letter dated 7 October that Davis actually received from Mayor (a letter found in the Davis papers rather than the Mayor papers) was very similar, but Mayor had toned down the taunting from "study them and see for yourself" to "You**...**seem not to have studied the reefs intimately," and he deleted the request for a "hot fight."[159]

Davis replied instantly (perhaps in fact anticipating Mayor) with a postcard of 8 October that exemplified his persistence, his delight in heckling, his adherence to Darwin, and his preference to generalize from the comparative study of maps. "As to the sub[merged] b[arrier] rf. having grown up from what Darwin called a fringing reef – an off-shore fr[inging] rf. if you like, that seems probable – D[arwin] retains that view of fr[inging] rfs in his 2^d Ed^n. 1874. I think it unfortunate that he used the name in that way – but as he did so, I believe your...theory for the Tutuila reef is not well formed – not so well as the reef! Nearly [all] charts I have exam^d show f[ringing] rfs. narrower than the b[arrier] rfs that enclose them."[160]

158. A.G. Mayor to W.M. Davis, 7 October 1920. A.G. Mayor papers, SU, Box 5, folder "Outgoing Correspondence."

159. Mayor to Davis, 7 October 1920. Davis papers, HU-H bAm 1798, 320.

160. .M. Davis to A.G. Mayor, 8 October 1920, from Cambridge, MA. A.G. Mayor papers, SU, box 5, folder "Unidentified Correspondence."

Mayor forwarded Davis's original letter to Chamberlin for a geologist's opinion and got a revealing reply. Chamberlin seemed to favor Mayor in principle, and he was critical of Davis's presumptuousness. "It struck me instantly as a bit strange that considering the relative experience of you two with actual growing reefs that he should attempt to instruct you upon the conditions of growth of fringing reefs. The actual case you cite near Suva and the experiences with the diving helmet ought to answer the theoretical objections raised. [...] The letter seems to me to be just a continuation of what you designated as 'arm chair geology.' It looks like starting from partial information and arguing from that basis, but without taking into account a lot of other actual factors which play an important rôle in the real case."[161] However, when Chamberlin responded to the specific arguments that Davis had made, in correspondence with himself as well as Mayor, he did so by arguing from a chart as well. "[Davis's] suggestion of a submerged alluvial plain around Tutuila seems to me exceedingly unlikely *from an inspection of the contoured soundings on the chart*. If our submerged fringing reef were in reality an alluvial plain it should naturally be best developed in the bays where streams deposited alluvium. It should build delta plains out flush with the ends of the promontories and then after that extend the alluvial plain out farther, opposite the mouths of streams, in true delta fashion. *The chart shows nothing of the sort*. On the contrary, the shallow tract at not a few points is best developed off the ends of promontories and rocky ridges from the island. It is in general irregular, *such as I should suppose a growing coral reef should be*."[162] Chamberlin added a note that seemed to imply that he believed that Mayor would have trouble beating Davis in a physiographic argument, even if Mayor's interpretation of the reef

161. R.T. Chamberlin (Dept. of Geology, Univ. of Chicago) to Mayor, 9 October 1920. A.G. Mayor papers, SU, Box 1, folder "C."
162. R.T. Chamberlin (Dept. of Geology, Univ. of Chicago) to Mayor, 9 October 1920. A.G. Mayor papers, SU, Box 1, folder "C." Emphasis added.

platform were the correct one: "P.S. Davis may reply concerning the steep outer slope of the submerged reef that that is what one might expect from a delta front. Delta fronts (the fore set beds) are steep, tho formed in still water, but they should be definitely related to the stream mouths. R.T.C."[163]

The name of Charles Darwin was never far from the debates over coral reef formation. Because his terminology of fringing, barrier, and atoll reefs was the standard jargon for all participants, Darwin's theory was constantly being raised by reef workers of the 1920s, even if only to dismiss it. Mayor found it frustrating to carry on conversation using terms laden with a theory that he did not support. He told Davis, "Modern students <u>divorce</u> barrier from fringing reefs, while to Darwin's mind the b[arrier] reef was a descendant of the f[ringing] reef. As for 'off-shore-fringing reefs' they do not exist except as an academic expression. The two sorts of reefs are <u>structurally</u> different. A fringing reef if suddenly placed in the position of a barrier ie moved 'off shore' would wash away in the first hurricane! it is so loosely organized. We find this out by our borings and it was a surprize [sic] to us to see how loose everything is on a fringing reef."[164] He wished that Davis would set aside the effort to determine what Darwin had meant by his original (or final) version of the theory. "To my mind the important question is not <u>an interpretation</u> of Darwin's views in which neither Vaughan, Andrews, Daly, or Chamberlin are quite able to follow you; but is Chamberlin <u>right</u> or <u>wrong</u> in his interpretation of the geologic history of the reefs of Tutuila?"[165] On this question, Mayor was delighted to refer Davis's complaints to Chamberlin himself, or to Daly, who also knew both the reef in question and the substance of Chamberlin's work. "I am of course <u>not</u> in any sense a geologist," Mayor

163. R.T. Chamberlin (Dept. of Geology, Univ. of Chicago) to Mayor, 9 October 1920. A.G. Mayor papers, SU, Box 1, folder "C."

164. Mayor to Davis, 21 May 1921b. Davis papers, HU-H bAm 1798, 320. (Note that Mayor sent Davis two letters with this date, each of which is in this folder of the Davis papers.)

165. Mayor to Davis, 18 May 1921. Davis papers, HU-H bAm 1798, 320.

admitted, “and the expression of geologic opinion at times has unfortunately been forced upon me there being no geologist in sight.”[166]

On 22 June 1922 at Loggerhead Key, where he had gone for his customary season of work at the Tortugas lab, Mayor was found lifeless at the edge of the beach. He was just 54 years old, but he had apparently succumbed to the tuberculosis that had plagued him for years and worsened since the last Pacific trip.[167] As the embodiment of the CIW Department of Marine Biology he had steered many colleagues and many thousands of dollars toward the study of coral reefs, and without Mayor’s hand on the tiller the direction of the department was uncertain. That autumn Setchell wrote to the president of the Carnegie Institution, Woodward’s successor J.C. Merriam, to explain the trajectory of Mayor’s recent work. “I thoroughly believe that Dr. Mayor felt it desirable to change somewhat his policy and that he felt it wise to emphasize more and more the work in the Pacific islands [over that at the Tortugas] and primarily the work towards a solution of the Coral Reef Problem.”[168] Setchell went on to recommend how he thought Mayor’s aims should be fulfilled. Among his suggestions was a shipboard “floating laboratory” with units that could be moved ashore, to be based at Papeete, Tahiti. “The whole variety of coral reef problems in the Pacific could thus be tackled from a central location and dealt with by means of the vessel employed and the portable laboratory units.” This plan would have had the effect of replicating in the Pacific the combination of field locality and laboratory resources available at the Tortugas and other marine stations. It may be that Mayor had been encouraged to consider Tahiti as a model reef for intensive study by a letter that he received from Gardiner in 1915, in which the Englishman was offering suggestions for reef work that would escape him for

166. Mayor to Davis, 18 May 1921. Davis papers, HU-H bAm 1798, 320.
167. Stephens and Calder, *Seafaring Scientist*, 145.
168. W.A. Setchell to J.C. Merriam (President of CIW), 19 October 1922. CIW RG Marine Biology, box 2, folder 2, “Tortugas Lab – Name, Plan, Scope, History, 1903-1926.”

as long as the European war continued. "Why not try Tahiti?" Gardiner had asked, "and in addition have a real good go to settle the question as to its formation. I have looked it up again & it strikes me as a place which should yield up its secrets. If I could go anywhere I would rather set down there for 3 months than anywhere I know, this is from the coral bionomics & reef points of view."[169]

Setchell continued his letter to Merriam by arguing that the integration of sciences that had occurred under the Department of Marine Biology during Mayor's tenure should continue. "I myself feel that the biology of the reefs and the surrounding shoals ought to be carried on in connection with a series of borings, made in such number and in such favorably situated localities as to yield sample sections in the cores obtained, and the study of these cores in connection with the biological examination of the living reef immediately connected ought to yield biological, geological, chemical, and physical facts sufficient to solve the general question of reef formation and go a long way toward solving the problem of reef histories." This description of the potential value of reef drilling helped to explain why specialists of so many different stripes had called for borings to be made. The future work of the Marine Biology Department, Setchell concluded, "should be a direct continuation of that of Darwin, Dana, Agassiz, and Mayor. It should be in the hands of men of broad sympathies and good executive powers." It turned out that despite Setchell's efforts, the department's geographical scope contracted after Mayor's death. Setchell's letter remains as a hint as to Mayor's objectives in raising the frequency and size of his Pacific expeditions: to establish the intensive style of reef study more permanently among the Pacific islands and thereby to pursue a conclusive solution to the coral reef problem.[170]

169. Gardiner to A.G. Mayer, 7 June 1915. A.G. Mayor papers, SU, Box 1, folder "G."

170. Mayor's last works on coral reefs were published posthumously in Alfred Goldsborough Mayor, *A Memorial to Alfred Goldsborough Mayor: Some Posthumous Papers of A.G. Mayor Relating to His Work at Tutuila Island and Adjacent Regions, Together with Reports of R.A. Daly, R.T. Chamberlin, and C.B. Lipman on Their Work in the Same Connection*, Papers from the Department of Marine

Biology, Geology, Physiography

While American coral reef scientists entered the 1920s with research programs that remained more or less intact through the Great War, their counterparts in Europe, Australia, and Japan were renewing old reef studies and initiating new ones only at the beginning of the decade.

In 1922 William Watts, who had been chairman of the Royal Society's Coral Reef Committee, wrote to Davis with an update on the state of British coral reef science. Evidently responding to an inquiry by the Harvard man, Watts told him "With regard to your question on Coral Reefs. I think there is no active geological work going on here on that subject except by the Biologists Gardiner & Wood Jones. Most of us are working from four to six months of the year to [illegible] the conflict of the war and its sequels, and what is then left does not run to coral-reefs."[171] Davis's reputation as a staunch defender of Darwin's theory and his geological and geographical orientation to the coral reef problem were well known to Watts and his British colleagues. As a geological sympathizer in the sharp disputes of the turn-of-the-century coral reef problem, Watts considered Davis an ally in an old cause. "I can't tell you how glad I am that your work is doing so much to confirm Darwin's theory, in which I have been an invarious believer; before the time when I was secretary to the Funafuti Committee and ever since."[172] From Watts's perspective, the divisions of the Funafuti era remained crystallized in Britain.

> Our attitude here is easily inflamed. There was endless discussion between the geologists on the one hand and the biologists & geographers who do not at bottom believe in geological processes, on the other. Each side spoke in terms

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Biology of the Carnegie Institution of Washington (Washington, D.C.: Carnegie Institution of Washington, 1924).

171. Watts to Davis, 19 March 1922. Davis papers, HU-H bAm 1798, 511.

172. Watts to Davis, 14 April 1922. Davis papers, HU-H bAm 1798, 511.

> the other did not really understand. Finding agreement on 'their' known facts did not impress the other side the Geologists. Bonney, Judd, Sollas, David, Wharton, & others determined to test a test case and the Funafuti boring resulted.
>
> Their results were so conclusive that I think the opposition expected us to shout about it. However Judd Sollas & Bonney were determined that the record should be a perfectly sober statement of fact – from which those who read the memoir carefully could not fail to see that the subsidence method had been settled for good and all in one case of a typical atoll. That having been done I think that on this side we were content to let the matter rest having erected a wall against which the biologists beat in vain. The Darwin method was a 'vera causa'.[173]

Watts believed that there were few outstanding elements of the coral reef problem. The Florida reefs were a special case that he felt had yet to be properly solved, and he thought further tests should be done to judge whether the deposition-solution mode of reef formation described by Murray could operate at all. He was not optimistic for Murray's theory, nor for the sorts of people who had supported it to see the light represented by Davis's work. "[M]ay I be pardoned for saying," Watts wrote of Davis's latest treatise on the Fiji Islands, "that though your arguments appeal to geologists & geographers, the biologists will be incapable of appreciating them. That is our great difficulty over here. Both physicists on the one hand and biologists on the other imagine that it is sufficient to take the <u>results</u> of geological work and that it is unnecessary to know anything of the methods as a criterion of reliability. Perhaps even this might be excused if they knew the <u>results</u> better."

The same week that Watts was bemoaning the divided state of British coral reef science, a plan for collaborative study of the Great Barrier Reef was sprung in the Commonwealth of Australia.[174] H.C. Richards, the professor of geology at the University of Queensland had convinced Sir Matthew Nathan, who was the new

173. Watts to Davis, 14 April 1922. Davis papers, HU-H bAm 1798, 511.

174. In addition to my research in the papers of the (British) Great Barrier Reef Committee and C.M. Yonge at the Natural History Museum, London, the following account relies on details from Bowen and Bowen, *Great Barrier Reef*; Hill, "The Great Barrier Reef Committee, 1922–82. Part I: The First Thirty Years"; Hill, "The Great Barrier Reef Committee, 1922–1982. Part 2: The Last Three Decades"; Jones, "The Great Barrier Reef Committee: Its Work and Achievements, 1922–66".

governor of the state and as such held the presidency of the Queensland branch of the Royal Geographical Society of Australasia (RGSAQ), to throw his support behind a plan for research into the "structure and growth [and] economic potential" of the Great Barrier Reef. What Richards had in mind was for Australian scientists to "do here what Mayor and Vaughan are doing in the Gulf of Mexico."[175] This led to the creation of a Great Barrier Reef Committee of the RGSAQ. The Committee (henceforth GBRC) concentrated its early efforts on organizing a deep bore through the reef, which was conducted at Michaelmas Cay, near Cairns, in April 1926. The crew, under Funafuti veteran Charles Hedley, drilled to the depth of 580 feet before running out of casing. At this depth the bore had penetrated only sediment of coral and foraminifera with no sign of bedrock. This was considered a failure, and Richards shifted the efforts of the GBRC toward the organization of marine biological research in hopes of redressing a disproportionate focus on geological work. Under advice from Gardiner in Cambridge, the GBRC in July 1926 invited F.A. Potts to submit a proposal for research.[176] Having worked with Mayor at Tortugas, Murray Island, and Samoa, Potts tellingly suggested "an intensive investigation" of a localized area of the Great Barrier Reef.[177] In the end it was not Potts but Charles Maurice Yonge, a physiologist of marine invertebrates, who led a year-long expedition to the reef in 1928-1929.[178] Yonge's twelve-member Biological Section of the expedition was based for the year at a prefabricated laboratory erected on a small islet, and an independently funded three-person Geographical Section under J.A. Steers ranged more widely. Yonge's work on coral physiology, and

175. H.C. Richards to Sir Matthew Nathan, 7 December 1921. Quoted in Bowen and Bowen, *Great Barrier Reef*, 233.

176. Gardiner's complicated role in the organization of the 1928-1929 Great Barrier Reef Expedition is discussed in Barbara E Brown, "The Legacy of Professor John Stanley Gardiner FRS to Reef Science," *Notes and Records of the Royal Society* 61 (2007): 207–17; Bowen and Bowen, *Great Barrier Reef*, chapter 15.

177. Potts quoted in Bowen and Bowen, *Great Barrier Reef*, 252.

178. B. Morton, "Charles Maurice Yonge, 9 December 1899–17 March 1986," *Biographical Memoirs of Fellows of the Royal Society* 38 (1992 November 1992): 378–412.

particularly on the role played in their metabolism by symbiotic algae, was the defining product of the expedition.[179] The GBRC organized a second reef boring expedition in 1937, at Heron Island at the southern end of the Barrier. This effort reached 732 feet, and like the Michaelmas Cay bore passed through sedimentary material without reaching basement. It was a curious feature of the major GBRC undertakings that they were segregated into explicitly biological and explicitly geological undertakings. This phenomenon deserves a deeper investigation than I am able to give it here.

On the return trip of the Great Barrier Reef Expedition to the United Kingdom in 1929, Yonge visited Wayland Vaughan at La Jolla, California, where he was now the director of the recently re-named Scripps Institution of Oceanography (formerly the Scripps Institution for Biological Research). After advocating unsuccessfully in Washington after the war for the establishment of a federal agency for oceanography, Vaughan had been chosen to administer the development of the nation's first dedicated oceanographic institution at Scripps in 1925. Vaughan did not view oceanography as an independent science, but, as Ronald Rainger has argued, as "the study of a particular place, the oceans, [by] scientists employ[ing] knowledge and methods from biology, chemistry, geology, and physics to study that place."[180] Vaughan's view of oceanography was, therefore, essentially consistent with the view of coral reef research that he proselytized to Mayer and Woodward in the 1910s, in which satisfactory knowledge of the phenomenon in question (reefs) would come from integrating the intensive work of individual workers who might have strong and distinct disciplinary

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179. It would be worthwhile to examine the work of the Yonge Expedition from the perspective of the debates over coral reef formation. I have gathered substantial material on the organization and prosecution of the venture, but at present I am unable to incorporate it into this chapter as presently conceived.

180. Ronald Rainger, "Adaptation and the Importance of Local Culture: Creating a Research School at the Scripps Institution of Oceanography," *Journal of the History of Biology* 36 (2003): 451–500. See also T.W. Vaughan, "Oceanographic Investigations of the Scripps Institution for Biological Research of the University of California," *Science* 63 (1926): 8–10; T.W. Vaughan, "Biological Research at the Scripps Institution of Oceanography," *Science* 63 (1926): 297.

identities. Vaughan was impressed with Yonge when they met. He tried to lure the physiologist to join the faculty at Scripps, and when Yonge opted to return to Britain Vaughan supplied him with copied notes on his own coral feeding experiments.[181]

In 1928 William Morris Davis, then 78 years old, finally published the book on coral reef formation that he had begun drafting in the 1910s.[182] Like Davis's journal articles on the topic, *The Coral Reef Problem* drew heavily on the physiographic approach, and it was a vehicle for an even stronger judgment in favor of the subsidence theory. As he had done before, Davis described what he took to be the salient physical features of coral reefs, and then evaluated competing explanations for them one after another. What was remarkable about Davis's book was not its argument or its structure but its magnitude. The "weighty monograph," as Daly called it, was nearly 600 pages long and a magnificent bibliography of 754 entries.[183] It was a testament to the breadth of empirical data that students of coral reefs now had at their disposal and a valuable document of the history and current state of the field. Indeed, the historian of science George Sarton reviewed the book at once for his journal *Isis* as an "important contribution to the subject and to its history."[184]

Davis's partisan zeal for the discipline of physiography had only sharpened in his old age, and he made utterly clear his conviction that other sciences had only subordinate claims on the coral reef problem. "It should here be emphasized," he wrote in the introduction,

> that independent evidence as to the value of competing theories of reef origin can be best obtained not from the reefs themselves, but chiefly from the

181. T.W. Vaughan to Yonge, 11 December 1929. Yonge Collection, NHM. E.49, GBRE correspondence.

182. Davis, *The Coral Reef Problem*.

183. Daly, "William Morris Davis," 278. I have followed R.T. Chamberlin's count of the number of bibliography entries. R.T. Chamberlin, "[Review of] *The Coral Reef Problem*. By William Morris Davis," *Journal of Geology* 38 (1930): 756.

184. George Sarton, "[Review of] William Morris Davis. -- The Coral Reef Problem," *Isis* 11 (1928): 399–401.

> physiographic features of the coasts, either insular or continental, that are bordered by fringing reefs or fronted by barrier reefs; and also from the structure of elevated reefs. It is for this reason that the following pages are so largely occupied with the physiography of land forms rather than with the biology of coral reefs. The biology and especially the symbiosis of the reefs are unquestionably important subjects in themselves, but the opportunity for the establishment and growth of reefs is so largely determined by the physiographic conditions of insular and continental coast, over which the reef-building organisms have no control, that those physiographic conditions necessarily assume the leading rôle in the problem under discussion."[185]

As Davis warmed to his argument, he adopted a blasé attitude toward those who studied corals themselves. This was justified in part because corals themselves were passive participants in the formation of a reef. "Let it be borne in mind that, while coral reefs react upon their surroundings as they grow, they are chiefly the consequences rather than the determinants of their environment...Hence coral reefs must be treated as items in the geological history of their areas."[186]

Furthermore, Davis simply refused to use biologists' terminology at all. "It may occasion surprise, not to say disappointment, on the part of some readers of this book that, although it is largely concerned with coral reefs, it nowhere contains the learned names by which to identify the members of the extraordinary symbiotic assemblages repeatedly seen on reef slopes and in lagoon waters," he wrote. He had the same opinion of petrological details and the terms used to describe them: they were the legitimate matter of science, but they were unnecessary for understanding the origin of coral reefs. "Had either biology or petrology been of essential importance in the phase of the coral reef problem here under discussion," he assured his readers, "I would have made an effort to 'book up' enough to give those sciences at least a fair treatment, but as a matter of fact they have no such importance."[187]

185. Davis, *The Coral Reef Problem*, 3.
186. Davis, *The Coral Reef Problem*, 142–43.
187. Davis, *The Coral Reef Problem*, 220–21.

To many readers, it seemed that Davis had chosen to make a highly partial argument in favor of the subsidence theory by systematically ignoring of biological evidence. He had cited biologists' work, but he had not attended to their arguments.

John Stanley Gardiner, the most accomplished student of reef building corals among all living commentators on reef formation, reviewed the book for the British *Geographical Journal*. The man from Cambridge, England congratulated his Cambridge, Massachusetts foil for compiling a comprehensive review of the coral formations of the Pacific Ocean, for "giv[ing] his conclusions honestly," and for "not profess[ing] to be what he is not, viz. a biologist."[188] Gardiner characterized Davis's approach as "the study of lands enclosed in fringing and barrier reefs from the geographical viewpoint," and judged it "doubly valuable as being the work of a competent geologist." But he introduced two main lines of critique. The first was that Davis's conclusions rested upon an insufficient foundation of first-hand fieldwork. Davis had erred by basing so many of his judgments on "materials collected by a very miscellaneous body of observers, few of whom have wide and long-enough personal experience of coasts, of reefs and of marine conditions to allow them to make assuredly accurate statements."[189] Whatever the potential strengths of the physiographic approach, it was only as credible as the sources of the physiographic data. Even the descriptions of sites that Davis had visited Gardiner believed to be too superficial and qualitative. "I know from experience the difficulties of the field," Gardiner assured his readers, "but I cannot for a moment believe that embayments cannot be established by figures of heights and depths, angles of slopes, photographs from two fixed spots, etc.; in effect, I ask for impressions to be replaced by detailed field work."[190] The second

188. John Stanley Gardiner, "[Review of] The Coral Reef Problem [by W.M. Davis]," *Geographical Journal* 72 (1928): 268.
189. Gardiner, "[Review of] The Coral Reef Problem [by W.M. Davis]," 268.
190. Gardiner, "[Review of] The Coral Reef Problem [by W.M. Davis]," 270.

theme of caution that ran through Gardiner's review related to the ideal of finding a unitary solution to the coral reef problem. Even if Davis could demonstrate that certain barrier-encircled islands had been submerged, there was no need to assume that similar causes operated in the formation of all reefs. "To-day our motto is 'each reef its own story.'"[191]

It was out of character for Gardiner, who was perhaps the most active proponent of biological approaches to the study of coral reefs in the world, to have allowed Davis's jabs at biology to go unparried. In fact, his response was only postponed. In 1930 Gardiner came to Davis's territory to deliver the annual series of Lowell Lectures on the topic of "Coral reefs and atolls." His approach remained subtle, but this was perhaps the approach best calculated to irk the man who thrived on "hot fights." According to the published edition of the lectures, Gardiner barely mentioned Davis throughout his entire engagement at Boston. When he did, it was to imply gently that Davis's book was prejudiced, by describing *The Coral Reef Problem* as a compendium of "almost all known facts that can be made to favour subsidence."[192] Moments later, without mentioning Davis's name, Gardiner needled, "it is interesting that all or nearly all recent investigators of coral reefs have been skeptical as to the subsidence theory, if they were in possession of the knowledge of the principles underlying organic life."[193] Finally, Gardiner had made his thrust at Davis's ignorance of biology.

Davis responded to Gardiner's precision needling with a shotgun blast in the general direction of British biology. Reviewing the published Lowell Lectures for the T.C. Chamberlin-founded *Journal of Geology*, Davis took it upon himself to evaluate the standards of scientific reasoning that had been employed by Gardiner and ten other

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191. Gardiner, "[Review of] The Coral Reef Problem [by W.M. Davis]," 273.
192. John Stanley Gardiner, *Coral Reefs and Atolls; Being a Course of Lectures Delivered at the Lowell Institute at Boston February 1930* (London,: Macmillan and Co. limited, 1931), 147.
193. Gardiner, *Coral Reefs and Atolls,* 148–49.

"British biologists or investigators of mainly biological training who have concerned themselves with the geological problem of the origin of coral reefs."[194] He explained the premise of this attack by noticing the historical circumstances that had compelled certain types of naturalists to try to explain the formation of reefs. "Coral reefs are the product of lime-secreting organisms, mostly polyps and algae, and it is probably for this reason that their origin has been more studied by biologists and voyagers of biological training than by geologists, and especially by British biologists, because they have voyaged so much." Despite this accident of history, Davis argued, this problem belonged to the science of geology because it required "the discovery of the conditions and processes of the past under which the massive reef structures have been formed."[195] Because reef origins were a geological problem, Davis reasoned, the coral reef question "cannot be successfully solved without the use of methods appropriate to geological investigation."[196] Therefore it would be reasonable to examine how rigorously these British voyaging biologists had followed a sound system of geological reasoning such as Chamberlin's method of multiple working hypotheses.

This setup gave Davis an avenue not merely to point out non-geologists' clumsy use of geological facts, but to ridicule their powers of reasoning. He related an anecdote about John Murray, who, "when once asked 'How about the shore-line embayments of the central volcanic islands?' [this being Davis's favorite point of physiographic support for the subsidence theory] retorted: 'What have they to do with it?'"[197] Davis reserved

194. William Morris Davis, "Gardiner on 'Coral Reefs and Atolls': A Discussion," *The Journal of Geology* 42 (1934): 202.

195. Davis, "Gardiner on 'Coral Reefs and Atolls'," 200.

196. Davis, "Gardiner on 'Coral Reefs and Atolls'," 201–2 Davis added the disclaimer that "The problem is, of course, like any other problem in science, open to study by anyone who wishes to study it; but prudence would suggest that if it be taken up by, for example, a philologist or a mathematician, he would do well to prepare himself by making close acquaintance with geological methods before embarking upon it." This implied that the problem with biologists was that their specialty was not sufficiently detached from the problem for them to recognize the inadequacy of their own discipline's methods to solve it.

197. Davis, "Gardiner on 'Coral Reefs and Atolls'," 203.

the bulk of his criticism for the final one of Gardiner's Lowell Lectures, on "The foundations of atolls," which he called "a very inconclusive, would-be geological discussion which, being appended to biological chapters of much higher grade, lowers the tone of the book to which it is given admission."[198] Davis squabbled with Gardiner's characterization of the reef phenomena that a theory need explain, he accused him of failing to accept subsidence as a working hypothesis, and he argued that Gardiner had failed to make key deductions about the theories of Wharton and Murray that were damning when "confronted" by readily available evidence. He took issue with Gardiner's statement that "If we regard the question of the formation of the foundations of coral reefs honestly, we are forced to admit that all our theories and considerations are mere camouflage for our lack of knowledge." To Davis, who believed that provisional theories, well tested, were the route to acquiring geological knowledge, this comment "betray[ed] an unscientific habit of mind."[199]

Davis reflected on the systematic differences between geological thought and British biological thought. "When naturalists collect animals or plants they find specimens of the same species at different stages of growth, representing youth, maturity, and age. Similarly, when geologists observe examples of like structures they soon discover that they show early, middle, and late stages of inorganic yet organized development."[200] Davis considered the existence of fringing reefs, barrier reefs, almost-atolls, and true atolls to be "one of the most striking examples of an apparently developmental series." In principle, biologists should find this compelling. But the problem with biologists, Davis reckoned, was that they had not been taught to see such connections. "The chief causes of their inadequacy may, perhaps, be traced back to their lack of training in making sound inferences concerning the invisible past as

198. Davis, "Gardiner on 'Coral Reefs and Atolls'," 205.
199. Davis, "Gardiner on 'Coral Reefs and Atolls'," 208.
200. Davis, "Gardiner on 'Coral Reefs and Atolls'," 206.

compared with their expertness in making accurate observations of the visible present."[201]

Davis was surely aware that there was a potentially fatal counter-example to his description of British biologists and their inadequacies when it came to making sound inferences about developmental processes in the invisible past. What of Darwin? Davis had his answer ready: "Darwin is not included because, in his youth, when he studied coral-reef origins, he was as much a geologist as a biologist."[202]

Ladd, Hoffmeister, and the antecedent platform theory

Among those who called for an integration of the biological and geological approaches to the coral reef question were two young American scientists, Harry Ladd and J. Edward Hoffmeister, who had a serendipitous meeting at Suva, Fiji in 1926. Ladd was then 27 years old, just a year removed from completing a Ph.D. in paleontology at the University of Iowa. Hoffmeister, who was exactly a month younger than Ladd, had previously studied West Indian corals with Vaughan, who reportedly told him "if you continue to study corals you will either become famous or you will lose your mind."[203] He was now doing paleontological studies of corals as a member of a party headed by Setchell and funded by Honolulu's Bernice P. Bishop Museum to make an "investigation of the geologic and biotic history of the Tonga archipelago."[204] After finishing their Tongan work, Setchell's party was delayed for a week at the Fijian island of Vitilevu on their steamer passage back to Honolulu. There they encountered Ladd, who was on a Yale University-Bishop Museum fellowship to study the relative ages of

201. Davis, "Gardiner on 'Coral Reefs and Atolls'," 204.
202. Davis, "Gardiner on 'Coral Reefs and Atolls'," 202.
203. The Vaughan anecdote is recounted in the script of Harry Ladd's toast to Hoffmeister as outgoing Treasurer of the Geological Society of America, 1963. Ladd's script is in Ladd Papers, SIA, Box 1, folder 4, "Correspondence 1925-1982."
204. William Albert Setchell, "The Tonga Expedition of 1926," *Science* 64 (1926): 440–42.

the upraised terraces and living reefs of Fiji.[205] Although Ladd's was a solo project, his aims coincided with those of Setchell and Hoffmeister, for his method was to compare fossils from the Tertiary and Pleistocene limestones of the island with the present constituents of the reef. His proposal for the Bishop Museum stated that "Such studies should throw considerable light on the age and geological history of the rocks involved and add to what is now known of Pacific faunas. In addition there is the possibility of contributing something toward the questions involved in the origin of coral reefs in general."[206] He and his wife were due to take the same steamer, the *Niagara*, to Honolulu, but Ladd seized the week with Hoffmeister as an opportunity for further field work. He took the newcomer to examine quarries around Suva, and Hoffmeister made his own collection of corals on the barrier reef in Suva harbor. Ladd and Hoffmeister swiftly began a more formal collaboration. By the last week of December 1926 they had delivered a jointly authored paper on the "Recent negative shift of strandline in Fiji and Tonga" at the annual meeting of the Geological Society of America. Ladd and Hoffmeister together made two further trips to the Fijis and Tongas, in 1928 and 1934, and each man produced a monograph for the Bishop Museum, Hoffmeister on the geology of Eua, Tonga, and Ladd on the geology Vitilevu.[207]

The results of their joint fieldwork also prompted Hoffmeister and Ladd to collaborate on a series of three articles on the coral reef problem for the *Journal of*

205. Setchell's article makes clear that they spent their week with Ladd after completing their work in Tonga. Ladd's recollections indicate that they had originally met in Fiji in May on the Setchell party's outbound journey. See Ladd toast to Hoffmeister.

206. Clipping labeled "Director's Report - Bishop Museum," probably from 1925. Ladd Scrapbook, Ladd Papers, SIA, RU 7396, Box 2, folder 4, p. 3 (henceforth "Ladd Scrapbook"). Ladd's field notes for his 1925-1926 trip are in Ladd Papers, SIA, RU 7396, Box 2, folder 7, "Diaries and Field Notes, 1915-1973 (1). Journal from Fiji trip 1925-6." See also "Oral History Interview with Harry Stephen Ladd...Conducted by Clifford M. Nelson," Ladd Papers, SIA, RU 7396, Box 2, folder 3, pp. 10-11." (henceforth "Ladd Oral History.")

207. J. Edward Hoffmeister, "Geology of Eua, Tonga," *Bernice P. Bishop Museum Bulletin* 96 (1932); H.S. Ladd, "Geology of Vitilevu, Fiji," *Bernice P. Bishop Museum Bulletin* 119 (1934) The 1928 trip also resulted in J.E. Hoffmeister, H.S. Ladd, and H.L. Alling, "Falcon Island," *American Journal of Science* 5th Series, Vol. 18 (1929): 461–71.

Geology. The first article, which appeared in 1935, was on "The foundations of atolls." The article was addressed to an audience of geologists, and it was relatively unusual in that its authors called for members of their own field to give more weight to work in other disciplines when trying to determine the origin of coral reefs, instead of using the pages of a general-interest journal like *Science* or *Nature* to urge members of other disciplines to attend more carefully to the authors' specialty. Hoffmeister and Ladd, who were then both members of the geology department at the University of Rochester, used their facility with paleontological evidence to show from a geologist's perspective how biological evidence was useful in interpreting the geological history of Pacific islands. In this argument, they used the just-deceased William Morris Davis as their foil. They quoted liberally from among Davis's most inflammatory accusations against British biologists, and while they acknowledged that "much of his criticism [was] justified" in their view, they declared that "Davis and some other geologists are at least as guilty of befogging the coral-reef problem as are the biologists, for they have, in their turn, ignored important biological considerations."[208]

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208. J.E. Hoffmeister and Harry S. Ladd, "The Foundations of Atolls: A Discussion," *The Journal of Geology* 43, no. 6 (1935): 653 Davis was an obvious target for this line of attack by virtue of his published writings, though it seems possible that Ladd's decision to study living as well as fossil faunas at Fiji had actually been stimulated by private interaction with Davis. While still a Ph.D. student, Ladd had written to Davis and several other American scientists with experience in the Pacific to ask where he might find highly fossiliferous raised reefs to study. Their replies are still extant, and the letter from Davis warned Ladd that "W.H. Dall has told me that the determination of age of raised reefs by their molluscan fossils is difficult, because te present mollusca of the mid Pacific islands is not well known." W.M. Davis to Ladd, 9 October 1924. Ladd Papers, SIA, RU 7396, Box 1, Folder 5, "Correspondence, 1924-1966." There is a letter from Dall to Ladd, 8 October 1924, in the same folder, which does not mention this problem. Fifty years later, Ladd recalled that he had also met Davis after a lecture in Iowa,

Hoffmeister and Ladd pointed out what they took to be the prime instances of geological narrow-sightedness and gave several examples from their fieldwork to show how they had integrated different types of evidence in revising previous geologists' interpretations. One of the most important cases in which "many geologists have woefully failed to consider biological factors" was in studying elevated limestones of the Pacific. Too often, they argued, geologists had failed to determine the organic composition of these rocks, and had simply "dubbed [them] a 'coral-reef limestone.'" This "inadequate observation" had led to "considerable confusion" in reef studies because many assumptions about the formation of reefs had been based on conclusions drawn from the thickness and shape of formations that were in fact probably not elevated reefs. They further criticized Davis for his accusation that Gardiner neglected to describe the volcanic foundations immediately beneath elevated limestones. "Davis apparently believed that the foundations of atolls and barrier reefs are synonymous with the foundations of the limestones," they wrote. "Nothing could be farther from the truth."[209] Echoing Vaughan's work in Florida, Hoffmeister and Ladd argued that some elevated coral limestones were founded on other limestones that were geologically much older. Indeed, at Eua Hoffmeister had found that a formation that Davis had called "the loftiest elevated reef in the open Pacific" (which made it for Davis a prime demonstration that reef limestones had accumulated to thicknesses that could only have been formed if their foundations had subsided) was in fact two formations. There was non-coralliferous Eocene limestone underlying Pliocene coral limestone, which meant that the island actually provided evidence of corals making a thin veneer over an independently-formed foundation. In this and several other instances, Davis had

where "he approved of my plans to go to Fiji and told me exactly what to study there, not paleontology but physiography, which is his field. And we didn't agree even in those days." Ladd Oral History.

209. Hoffmeister and Ladd, "The Foundations of Atolls: A Discussion," 655.

willfully ignored "good biological information which throws considerable light on the history of [an] island," and he appeared to have done so "in order that the physiography might be made to fit a particular theory of reef origin."[210]

As examples of the geological value of integrating different types of field investigations, Hoffmeister and Ladd offered revisionist interpretations of the islands of Mangaia and Tuvuthá, which subsidence advocates had interpreted as an elevated barrier reef and an elevated atoll. In each case they argued that apparently straightforward physiographic forms had been evolved after the islands were elevated. They did so partly by demonstrating that the characteristic central depressions of each island were the result of subaerial solution of the limestone rather than construction by corals, and partly by the use of ecological arguments. Not only did the limestone of Tuvuthá show few, if any, fossils in position of growth; it was doubtful that it had even been reef talus because "[t]he association of organisms peculiar to coral reefs is not present."[211]

There were two points to these demonstrations. The first was to drive home the potential value of biological evidence while skewering the species of geological research that barely even integrated geologically-oriented fieldwork. "It is thus interesting to note," they pointed out, "that, of the geologists who have actually seen Tuvuthá, not one has stated that the island represents an elevated atoll." The only geologists who had actually made this leap of interpretation had all "obtained their information second hand," and had been misled by the fact that "Physiographically, an island of this type looks exactly as a modern atoll would look if it were elevated above sea level."[212] Hoffmeister and Ladd's second point was to report to their geological colleagues that their fieldwork cast doubt on the subsidence theory. "There seems to be

210. Hoffmeister and Ladd, "The Foundations of Atolls: A Discussion," 657, 660.
211. Hoffmeister and Ladd, "The Foundations of Atolls: A Discussion," 661–62.
212. Hoffmeister and Ladd, "The Foundations of Atolls: A Discussion," 662.

a growing feeling among geologists, particularly those who have never worked on coral reefs, that Davis in *The Coral Reef Problem* wrote the closing chapters on this long-drawn-out discussion and that Darwin's theory of subsidence has at last been proved to be correct. This is far from being the truth."[213] Hoffmeister and Ladd advocated "detailed work, both geologic and biologic" to be carried out on "a number of key islands." Flying visits supplemented by examinations of hydrographic charts would not do. Davis had seen many reefs, but "he traveled too extensively, considering the time at his disposal." He had not given himself the opportunity, they argued, to really work out the history of any one location. They warned their colleagues that the work of biologists who had focused on both plants and animals--for example Setchell, Howe, Mayor, Gardiner, and Yonge--must be "fitted into any theory of reef formation." Yet they prepared for the possibility that progress would not make the problem any simpler. "Probably no single reef theory will explain all reefs. Certainly recognition of the complexity of the problem is essential to its solution. It does not belong within the realm of any one subject, but requires the attention of scientists of many fields, each contributing his share."[214]

The following year, Ladd and Hoffmeister provided further evidence of their willingness to draw upon evidence that they characterized as "biological" and "ecological," in their critique of Daly's Glacial-control theory.[215] Like Davis, they bemoaned Daly's seemingly mutually-exclusive iterations of Glacial-control, which they believed had introduced a confusing level of "mobility in the theory."[216] They accepted that there had been a Glacial low stand of the sea, and agreed that it was the rising post-Glacial waters, and not subsidence, that accounted for embayment of reef

213. Hoffmeister and Ladd, "The Foundations of Atolls: A Discussion," 664.
214. Hoffmeister and Ladd, "The Foundations of Atolls: A Discussion," 664–65.
215. H.S. Ladd and J.E. Hoffmeister, "A Criticism of the Glacial-Control Theory," *The Journal of Geology* 44, no. 1 (January-February 1936): 74–92.
216. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 78.

encircled islands. Yet they doubted Daly's interpretation of the effect that Glacial changes in sea level and temperature had on corals. They presented observations by themselves and Mayor from the silty delta of the Mba River at Vitilevu, where the reef builder *Porites* was abundant, to counter Daly's claim that "mud-control" was likely to have been an effective check on reef growth before or during the Pleistocene, but they were willing to accept that lowered temperatures had thinned the reef fauna of islands in the marginal reef belts. However, they strongly deprecated the possibility that these reefs could have been recolonized rapidly enough to grow upward in tandem with rising post-Glacial seas. They declared that rising post-Glacial seas could have encouraged development of the forms of reef associated with a shift in the strandline, but "we do not believe," they wrote, "that a rising sea level is essential to the formation of either barriers or atolls. Given a suitable foundation, either type of reef may be formed under stable conditions of depth by normal upward growth."[217]

With this statement, Ladd and Hoffmeister offered a named theory of their own, the "antecedent-platform theory." This theory held that "any bench or bank--even one not 'smooth'--that is elevated at a proper depth within the circum-equatorial coral reef zone can be considered a potential reef foundation, and that, if ecological conditions permit, a reef could grow up to the surface without any progressive change in ocean level. This is a general principle that applies to all reefs--preglacial, glacial, and postglacial."[218] They admitted that this was not a new theory, and cited earlier works by Vaughan and Chamberlin that had showed that many different types of platforms had indeed been found to underlie living reefs. The key distinction between the antecedent-platform theory and Glacial-control was that the theory of Ladd and Hoffmeister did not date modern reefs to any specific period or event in geological history.

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217. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 88.
218. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 89–90.

The antecedent-platform theory was in some respects an anti-theory, in that it simply removed the platform (meaning any foundation lying below the depths at which corals might be expected to grow) from the question of reef shapes, and it offered no single answer to the question of where any given platform came from, or when it might have appeared. This meant that "Each reef, living or elevated, must be studied in relation to its immediate surroundings."[219] Ladd and Hoffmeister readily admitted that in the case of open-sea platforms, "it is, perhaps, a question whether or not we shall ever acquire sufficient information to ascertain their origin with confidence."[220] They closed the paper by promising to elaborate on the theory "in the near future," using data from their field trips to Tonga and Fiji. In fact, the duo's next statement on the formation of these contested islands did not make it into print until 1944, by which time the reefs of the Pacific had become the grounds for actual battles. This paper opened by explaining the factors that had ensured that "The origin of coral reefs...still provokes discussion after more than one hundred years" of active debate. "The isolation of many flourishing coral-reef areas, the difficulties involved in examining more than the surface of a living reef, and the fact that most investigators have devoted only a very brief time to field work in any one locality--all have added to the complexity of the problem."[221]

The 1944 paper by Hoffmeister and Ladd concealed an argument *against* general theories of coral reef formation in the guise of a general theory. The authors did this by enumerating the wide range of causes that could produce a platform within the depth where reef building corals could grow. The fact that the resulting taxonomy of platforms included platforms of erosion, of deposition, of volcanic eruption, and of earth movements (including both subsidence and uplift) reinforced the fact that the antecedent-platform theory was one that begged for a locally specific explanation for the

219. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 92.
220. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 92.
221. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 388.

origin of each reef. Indeed, they provided instances of reefs growing atop each type of platform in the Fiji and Tonga islands alone.

The "theory" was so broad that it could aptly be called a rejection of the search for a universally-applicable coral reef theory. Yet Hoffmeister and Ladd chose to frame their contribution in terms that implied that the ideal of a single theory remained a good one, by declaring that "the theory is all-inclusive."[222] They had no doubt noticed that over the hundred years of debate to which they referred in their opening, much more notoriety in coral reef studies had been attached to those who offered global theories than to those who offered singular reports from remote and complicated field sites.

By the arrival of the 1944 paper, Ladd was Assistant Chief Geologist of the USGS, making him the administrator who supervised all of the Survey's basic science and areal studies. As the U.S. made westward headway in the Pacific theater of the war, Ladd was assigned to lay out a geological plan for studying Pacific Islands, particularly those of the post-World War I Japanese Mandate that had come under U.S. control during the course of the war. U.S. amphibious assaults in the Pacific had been seriously hindered by lack of available intelligence regarding reef topography, most notoriously at Tarawa in the Gilbert Islands (now Kiribati) in November 1943, where U.S. Marines sustained heavy casualties when their landing craft became held up on the reef after covering fire had been lifted.[223] Japanese scientists had done substantial research into Pacific islands, including those under mandate, during the 1920s and 1930s. At the upraised coral island of Kita-Daitô-Jima, about two hundred miles east of Okinawa, in 1934 and 1936 a Japanese expedition bored a single hole to an eventual maximum depth of 1416 feet, exceeding the Funafuti maximum but nevertheless failing to reach

222. Ladd and Hoffmeister, "A Criticism of the Glacial-Control Theory," 400.
223. United States Strategic Bombing Survey (Pacific) Naval Analysis Division, *The Campaigns of the Pacific War* (Washington, D.C.: U.S. Government Printing Office, 1946), 192.

basement rock.[224] However, while paleontological research evidently was accessible in American journals, the results of Japanese geophysical work had apparently been tightly controlled and was largely unavailable to scientists in the West until after 1945.[225]

In the first draft of the report, which he evidently produced several months prior to the Japanese surrender on 15 August 1945, Ladd proposed "that systematic geologic investigations of the Pacific islands now under U. S. military or civil control be undertaken immediately by the Geological Survey."[226] The plan enumerated both short-term priorities, which "should be financed by the War Department" at newly-captured islands, and long-term research that would be undertaken when possible at any "Pacific islands remaining under U.S. control or influence." The highest priority studies, called the "First Phase," were expected to take eighteen months and cost $600,000, to be paid entirely by the War Department. These would include geological mapping and determinations of the mineral resources of the islands, which would help to identify

224. On the Kita-Daitô-Jima boring, see Shôshirô Hanzawa, "Micropalaeontological Studies of Drill Cores from a Deep Well in Kita-Daitô-Zima (North Borodino Island)," in *Jubilee Publication in the Commemoration of Professor H. Yabe, M.I.A. Sixtieth Birthday* (Tokyo, 1940–41), Vol. 2, 755–802, plates 39–42; Toshio Sugiyama, "[On the Drilling in Kita-Daito Island]," (In Japanese), *Contributions from the Institute of Geology and Paleontology, Tohoku University* 11 (1934): 1–44; Toshio Sugiyama, "[On the Second Drilling in Kita-Daito Island]," (In Japanese), *Contributions from the Institute of Geology and Paleontology, Tohoku University* 25 (1936): 1–35. (I have copies of these articles but am only able to study the figures and diagrams.) On the extensive paleontological work done especially by Hisakatsu Yabe and his associates see Hisakatsu Yabe, Toshio Sugiyama, and Motoki Eguchi, *Recent Reef-Building Corals from Japan and the South Sea Islands Under the Japanese Mandate*, Science Reports of the Tohuku Imperial University, Second Series (Geology) (Sendai, 1936). On Yabe's career, see Renjirô Aoki, "Professor Hisakatsu Yabe," in *Jubilee Publication in the Commemoration of Professor H. Yabe, M.I.A. Sixtieth Birthday* (Tokyo, 1939), Vol. 1, iv-vii.

225. The USGS library contains several postwar translations of geophysical publications. For a paleontological publication that appeared in an American journal, see Hisakatsu Yabe and Toshio Sugiyama, "Geological and Geographical Distribution of Reef-Corals in Japan," *Journal of Paleontology* 9 (1935): 183–217, plate 21. Yabe did postdoctoral work in Germany, Austria, and the United States prior to World War I. See Aoki, "Professor Hisakatsu Yabe," iv. For an expression of American concern, by the mid-1930s, that Japan was fortifying mandated islands in violation of the League of Nations, see Paul H. Clyde, *Japan's Pacific Mandate* (New York: Macmillan, 1935), preface.

226. Ladd, "Geologic Investigations of Pacific Islands." Ladd Papers, SIA, Box 1, folder 3.

islands that were suitable for military installations or other development prior to the islands being allocated in any eventual peace settlements, and would provide strategic information in the event of islands ceasing to remain under U.S. control.

The so-called Second Phase of research would be a ten-year program building on the findings of the First Phase, costing roughly one million dollars, and incorporating a wider variety of basic scientific research alongside complete geological and topographic mapping. One component of the Second Phase plan was an investigation of the "Coral Reef Problem:"

> There are many geological and biological questions connected with coral reefs – both elevated and existing reefs – that remain unanswered. It is believed that many valuable data bearing on the general coral reef problem would be accumulated by the proposed island survey. It would be possible by using the proper type of drilling equipment to bore through a living coral reef at a strategic location and obtain a large core recovery. With a good recovery of large diameter core it would be possible to differentiate between transported material and reef rock made up of corals and algae in position of growth. Evidence of this sort has never yet been obtained and might be decisive at least as far as some coral reef theories are concerned.[227]

Ladd's report was circulated to the Army and the State Department, and was approved on 3 October 1845 with "geologists and specialists in related fields" dispatched to Japan by the end of the month to begin First Phase work.[228] Ladd himself was sent to the Pacific on assignment to the Military Geology Unit of the Survey, which had been formally instituted in 1942 to produce terrain reports.[229] He then composed a

227. Ladd, "Geologic Investigations of Pacific Islands," pp. 8-9 Ladd Papers, SIA, Box 1, folder 3.

228. For the timeline of approval of the 1945 report, see Ladd, "Suggestions for Proposed Long-Term Program of Pacific Geology," pp. 2-3. 4 January 1946. Ladd Papers, SIA, Box 1, folder 3.

229. On the Military Geology Unit (from 1945 the Military Geology Branch), see Maurice J. Terman, "Military Geology Unit of the U.S. Geological Survey During World War II," in *Military Geology in War and Peace*, eds J.R. Underwood, Jr. and P.L. Guth (Boulder, Colorado: Geological Society of America Reviews in Engineering Geology XIII, 1998), 49–54; Maurice J. Terman, "Military Geology Branch of the U.S. Geological Survey from 1945 to 1972," in *Military Geology in War and Peace*, eds J.R. Underwood, Jr. and P.L. Guth (Boulder, Colorado: Geological Society of America Reviews in Engineering Geology XIII, 1998), 75–82. On Ladd's role, see Ladd Oral History, p. 29, and Gilbert Corwin, "Engineer Intelligence and the Pacific Geologic Mapping Program," in *Military*

revised version of the proposal for long-term work, dated 4 January 1946.[230] In this report he advocated that the Second Phase work should begin immediately, citing "the possibility that some of the territory included in the proposed study may not remain in our custody indefinitely." He explained that "The aims of the long-term program though primarily scientific will be of real value to the armed forces. To insure an early appreciation of the program's usefulness, studies of several of the more promising areas should be completed as soon as possible. These first studies will serve as samples, illustrating the kind of work that the survey proposes to do."[231]

Reef boring remained on the list of long term objectives, though now with greater details on the challenges to doing so. Ladd pointed to the recent boring at the Great Barrier Reef as an object lesson in the type of drill bit to avoid using. He also refined his recommendation to include the suggestion of a specific site for atoll boring. The site that was "ideally located for a drill site" was Breakfast Island at Jaluit Atoll, Marshall Islands. This was the very spot where Daly had called for a boring in his 1915 paper. Because Breakfast lay near the center of the lagoon, there was no chance that the bore would be passing through fore-reef talus. If the bore penetrated continuous strata of shallow-water lagoon deposits to a great depth, subsidence would be confirmed for the atoll of Jaluit. If there was an unconformity between Recent lagoon deposits and a platform of another rock within the first three hundred fathoms Ladd had the whole Pacific at his disposal. Surely Breakfast Island would provide a crucial test of the subsidence theory.

Geology in War and Peace, eds J.R. Underwood, Jr. and P.L. Guth (Boulder, Colorado: Geological Society of America Reviews in Engineering Geology XIII, 1998), 67–74. See also Frank C. Whitmore, "The Pacific Island Mapping Program of the U.S. Geological Survey," *Atoll Research Bulletin*, no. 494 (December 2001): 1–9.

230. Ladd, "Suggestions for Proposed Long-Term Program of Pacific Geology." 4 January 1946. Ladd Papers, SIA, Box 1, folder 3.

231. Ladd, "Suggestions for Proposed Long-Term Program of Pacific Geology," p. 3. 4 January 1946. Ladd Papers, SIA, Box 1, folder 3.

Ladd's expanded description of the scientific proposal and explanation of the theory behind selecting particular islands for research bore strong resonances to the principles expounded in the research papers he wrote with Hoffmeister. Under the heading of "General Factors" relevant to the operation plan, his first comment was on the possibly limited applicability of results from one Pacific island to another. "Many of the geological studies to be undertaken involve regional problems, the solution of which may not be found in any one island or group of islands; many other problems, of course, are local."[232] He proposed to combat this problem by ensuring that "the first steps in the program be the detailed study and geologic mapping of certain key islands [...] known to have had a longer and more complicated geological history than their neighbors."[233]

That Ladd's recommendations for postwar geological work in the Pacific were shaped by the questions that had already stimulated him and other scientists in their professional research was proved not only by the content of his report, but also by the enthusiastic letters that he wrote to senior figures in the coral reef debate. "I know you will be interested to learn," he told Daly, "that your excellent paper on 'Problems of the Pacific Islands,' published nearly thirty years ago, was found to be exceedingly useful" in preparing his report. "We are hopeful that a program very similar to what you proposed will be carried out, at least in areas that are to remain under control of the United States."[234] To Gardiner, who had send Ladd comments on the 1944 Ladd-Hoffmeister monograph on the geology of Lau, Fiji, he wrote "Your suggestion that we try to arrange for sonic sounding in Lau comes at an opportune time. The Geological Survey is planning a broad program of geological work in the Pacific." He explained

232. Ladd, "Suggestions for Proposed Long-Term Program of Pacific Geology," p. 3. 4 January 1946. Ladd Papers, SIA, Box 1, folder 3.
233. Ladd, "Suggestions for Proposed Long-Term Program of Pacific Geology," p. 3-4. 4 January 1946. Ladd Papers, SIA, Box 1, folder 3.
234. Ladd to Daly, 5 November 1945. Ladd Papers, SIA, box 1, folder 5, "Correspondence, 1924-1966."

that although the forthcoming efforts would probably be concentrated on other islands that the United States had recently acquired, that the program did introduce the possibility for the Geological Survey of conducting such "oceanographic" work in collaboration with the U.S. Hydrographic Office.

Shortly after Ladd circulated his revised report, a new development changed the context in which any future scientific work would be conducted in the newly acquired Pacific Islands. Following President Truman's approval on 10 January 1946 of a Joint Chiefs of Staff plan to test the effects of nuclear weapons on a naval fleet, Bikini atoll was selected as the test location.[235] When he returned to Washington after two months in the Pacific organizing the Military Geology work, Ladd learned that he had been ordered straight back to the South Sea. He was to travel to Bikini to head a geological survey of the island that must be conducted prior to the atoll's use as the site for a weapons-effects test called Operation Crossroads.[236]

Able, Baker, Charles Darwin, and the core drilling at Bikini Atoll

Held in the summer of 1946, Operation Crossroads was the largest U.S. military operation that had ever been conducted at peacetime, involving 42,000 personnel and 251 ships.[237] It was planned as a "weapons-effects" test, meaning that nearly one hundred vessels (mainly U.S. Navy surplus, plus three captured German and Japanese ships) were anchored in Bikini lagoon as a target fleet for two devices like the bomb

235. According to the official report of Operation Crossroads, the requirements for any test site were "A protected anchorage at least six miles in diameter…a site which was uninhabited, or nearly so…a location at least 300 miles distant from the nearest city…a location within 1000 miles of a B-29 base…freedom from severe cold and violent storms…predictable winds directionally uniform at all altitudes from sea-level to 60,000 feet…predictable water currents of great lateral and vertical dispersion…[and] control by the United States." William A. Shurcliffe, *Bombs at Bikini: The Official Report of Operation Crossroads* (New York: W.H. Wise, 1947), 16–17.

236. On the timing of Ladd's order, see Ladd Oral History.

237. Shurcliffe, *Bombs at Bikini*; *Operation Crossroads 1946* (Washington, DC: Defense Nuclear Agency, 1984).

that had been dropped on Nagasaki in 1945.[238] Crossroads was meant to gauge both the might of the blast and the effects of radiation, not only on the ships in the target fleet but also on nearly 6000 test animals and the atoll itself.[239] The 1946 operation plan called for geophysical surveys of the reefs and surrounding waters, catalogues of native species, and ecological studies. The first bomb, code-named Able, was dropped onto the fleet from an unmanned plane; the second bomb, Baker, was suspended beneath one of the target ships and detonated underwater. The Baker shot produced a radioactive spray that contaminated the entire test fleet and much of the atoll, an event so disastrous that Operation Crossroads was cut short without carrying out a planned third detonation.

Crossroads was intended to test more than just the target fleet, however. The operation involved elaborate baseline surveys of the composition and condition of the flora, fauna, geology and lagoon water at Bikini prior to the first detonation. Ladd and his fellow geologists were part of a large contingent of scientists placed within the Oceanographic Section of Joint Task Force One (JTF-1), which was headed by Roger Revelle, a former Ph.D student under Vaughan at Scripps.[240] The data gathered by participants drawn from museums, universities, and scientific institutions around the country would provide a means for judging the physical and organic effects of the bomb

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238. Operation Crossroads was the first of seven US test series in the Marshall Islands comprising nearly 70 nuclear detonations between 1946 and 1958.The other test series at the Marshalls were Sandstone (1948), Greenhouse (1951), Ivy (1952), Castle (1954), Redwing (1956), and Hardtack (1958). U.S. Department of Energy Nevada Operations Office, *United States Nuclear Tests: July 1945 Through September 1992* (2000).

239. The test animals were 200 pigs, 60 guinea pigs, 204 goats, 5000 rats, and 200 mice. Shurcliffe, *Bombs at Bikini*, 84.

240. On Revelle's role in the organization of the test, see Ronald Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s"; Ronald Rainger, "'A Wonderful Oceanographic Tool:' The Atomic Bomb, Radioactivity and the Development of American Oceanography," in *The Machine in Neptune's Garden: Historical Perspectives on Technology and the Marine Environment*, Helen M. Rozwadowski and David van Keuren (Science History Publications, 2004); Gary E. Weir, *An Ocean in Common: American Naval Officers, Scientists, and the Ocean Environment* (College Station: Texas A&M University Press, 2001), chapter 11.

blasts.[241] It was conventional in 1946 and 1947 for the operation to be described as an experiment, as in the assertion made in the planning stage by Vice Admiral W.H.P. Blandy, who oversaw the tests as the Commanding Officer of JTF-1, that "This is not a combined or international operation, but rather a scientific experiment by the United States Government alone."[242] The operation plan revealed that the scientific surveys were designed explicitly to take the form of a controlled experiment. For Revelle and others, the strategy for determining the bomb's radiological effects was that the conditions would be determined before and after the bomb blasts not only on Bikini, but at a series of other "control atolls" in the Marshall Islands. For example, the biological operation would consist of

> a quantitative inventory of flora and fauna at and near (1) Bikini (location of burst), (2) a secondary experimental point likely to be affected by the bursts, as Eniwetok, and (3) a point unlikely to be affected by the burst at Rongerik. These inventories to be made (1) prior to the explosion of bombs, (2) as soon after explosion as safe and feasible, (3) at varying intervals for a considerable time subsequently in order to estimate long period effect and the rate of repopulation. [...] This work will be carried on in cooperation with the program of physical oceanography.[243]

Norman Holter, Revelle's colleague in the Navy Bureau of Ships, made the case even more explicitly in a conversation with Engleman in February 1946, "We will survey

241. Shurcliffe, *Bombs at Bikini*; Pamela M. Henson, "The Smithsonian Goes to War: The Increase and Diffusion of Scientific Knowledge in the Pacific," in *Science and the Pacific War*, ed. Roy M. MacLeod (London: Kluwer, 2000), 27–50.

242. Blandy quoted in "'Operation Crossroads:' The Effect of the Atomic Bomb on Naval Power," *The Bulletin of the Atomic Scientists* 1, no. 5 (1946, 15 February 1946): 1, 12. I have only found one contemporary objection to Bikini's status as a place of experiment. Writing in the *Bulletin of the Atomic Scientists* before the Crossroads test, Lee DuBridge asked "wouldn't science and engineering be far better off if the 100 million dollars or so which the tests will cost could be devoted to laboratory research under controlled conditions?" Lee A. DuBridge, "What About the Bikini Tests," *The Bulletin of the Atomic Scientists* 1, no. 11 (May 1946): 7, 16.

243. "Oceanographic Survey Program for Operation CROSSROADS--Summary of." Memorandum from Roger Revelle to Rear Admiral T.A. Solberg, U.S.N. and Dr. Ralph A. Sawyer. 15 February 1946. SIO Subject Files, AC 6, Box 6, Folder 27. See also Ronald Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s". I am grateful to Ronald Rainger for sharing the contents of this SIO source with me.

after both tests and in addition will survey the control atoll just as a scientific check control."[244] With the exception of surveys immediately after the Baker blast, Crossroads and the Resurvey were carried out just as described. The plan of operations--and by extension the expectations for what was knowable by these surveys--was based on the assumption that an atoll should be understood as a discrete unit. In the minds of those who determined how knowledge would be produced by Operation Crossroads, Bikini was not merely the location where the bombs were tested: Bikini *atoll* was what the bombs were tested on. The extent to which the bomb's effects over time would be distinguished from seasonal changes in the atoll biota depended on the extent to which an atoll could serve as an experimental system.

Thus in addition to the two months that Ladd and the geologists spent conducting surveys of surficial and subsurface geology at Bikini in 1946 prior to the bomb tests, Joshua Tracey of the USGS and Charles Bates of WHOI made comparative studies of the Marshall Island atolls of Eniwetok, Rongerik, and Rongelap. Tracey and Ladd's work on Bikini focused on reef morphology, and coral growth and zonation.[245] They conducted 26 traverses across the atoll rim, perpendicular to the reef front, recording the surface structure and the distribution of corals and algae within twenty-foot wide strips. Off the outer margin of the reef they studied growth and erosion by swimming with a face mask, while they examined inside the lagoon by diving and dredging. Among the benefits that came with studying the atoll as part of the massive Crossroads operation was access to air photographs of Bikini supplied by the Navy. Their colleague Kenneth O. Emery, known as K.O., undertook the study of Bikini's submarine geology. For him, too, the Crossroads infrastructure made available sources of comparative information

244. "Eng[l]eman talk with Holter February 13, 1946." SIO Subject Files, AC 6, Box 6, Folder 27. I am grateful to Ronald Rainger for sharing the contents of this SIO source with me.

245. Joshua I. Tracey, Jr., Harry S. Ladd, and J.E. Hoffmeister, "Reefs of Bikini, Marshall Islands," *Bulletin of the Geological Society of America* 59, no. 9 (1948 September 1948): 861–78.

that enhanced his own study. He believed that “the number of ships available and the staff made up of specialists in many fields of science virtually insured the making of new discoveries.” His subject of submarine geology, which he had studied under Francis Shepard at the University of Illinois and more recently at Scripps, was “one of the fields most likely to gain as a result of the trip, simply because the underwater portions of atolls had not been well studied previously.”[246] Emery seized the opportunity to get results using the “modern sampling methods” of echo sounding and underwater photography, but he made extensive physical bottom samples inside and outside the reefs. Emery’s 8000 miles of continuous-echo sounding records at Bikini showed the profile of the reef margin and outer slope, and revealed the presence of a flat-topped seamount that extended from the northwest side of Bikini at a depth of 680 fathoms. Emery too examined other atolls, making briefer surveys of Eniwetok, Rongelap, Rongerik, and Ailinginae in order “to determine whether Bikini is a typical atoll and to provide controls for estimating bomb damage to organisms.”[247] When the 200,000 echo-soundings taken by the Navy in Eniwetok lagoon were plotted, Emery found that its floor was terraced at 10 fathoms and studded with 2300 coral knolls, which gave it “an irregularity which departs markedly from some of the conventional ideas of billiard table flatness of coral lagoon floors.”[248]

When these studies and those by other Crossroads scientists were published they had a heavy focus on the nature of atolls as a discrete natural entity. This type of concern was uncommon in prewar coral reef literature, despite the longstanding tradition of treating individual reefs as proving grounds for general theories. The comprehensive-

246. Kenneth O. Emery, “Submarine Geology of Bikini Atoll,” *Bulletin of the Geological Society of America* 59, no. 9 (1948 September 1948): 858. On Emery’s training with Shepard, see Donn S. Gorsline and Kelvin S. Rodolfo, “Kenneth Orris Emery (1914–1998): Pioneer Marine Geologist,” *[Geological Society of America] GSA Today*, November 2003, 18–19.

247. Emery, “Submarine Geology of Bikini Atoll,” 856.

248. Emery, “Submarine Geology of Bikini Atoll,” 858.

ness of the Bikini study and the control-atoll element of the research program encouraged researchers to treat the atoll as an integrated whole to be compared to the other atoll-units.

Bikini emerged as more than a local field site; it became a model atoll. Thus Ladd, Tracey, Emery, and the coral paleontologist John W. Wells of Cornell co-authored a paper on the "Organic growth and sedimentation on an atoll." It began by declaring that "An atoll is a composite reef structure made up of a number of distinctive reef types..."[249] Though the authors acknowledged that "Atolls, like other types of coral islands, are not all exactly alike," they justified their schematic descriptions by adding that "We do know, however, that the major features of Bikini are quite similar to those of its near neighbors in the Marshall Islands, several of which were studied somewhat less intensively than Bikini was."[250] Perhaps the most striking example among the myriad atoll-level studies produced by Crossroads research was Marston Sargent and Thomas Austin's pioneering work on the "Organic Productivity of an Atoll."[251] They sought evidence on "how these coral structures remote from any terrigenous influence maintain themselves both as geographical features and as biotic communities supporting marine populations considerably denser than those of the surrounding waters."[252] Their

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249. Harry S. Ladd, et al., "Organic Growth and Sedimentation on an Atoll," *The Journal of Geology* 58 (1950): 410.

250. Harry S. Ladd, et al., "Organic Growth and Sedimentation on an Atoll," 410–11.

251. Marston C. Sargent and Thomas S. Austin, "Organic Productivity of an Atoll," *Transactions of the American Geophysical Union* 30, no. 2 (1949): 245–49. For examples of other atoll-level studies published shortly after Operation Crossroads, see Walter H. Munk and Marston C. Sargent, "Adjustment of Bikini Atoll to Ocean Waves," *Transactions of the American Geophysical Union* 29, no. 6 (December 1948): 855–60; William L. Ford, "Radiological and Salinity Relationships in the Water at Bikini Atoll," *Transactions of the American Geophysical Union* 30, no. 1 (1949): 46–54; Martin W. Johnson, "Zooplankton as an Index of Water Exchange Between Bikini Lagoon and the Open Sea," *Transactions of the American Geophysical Union* 30, no. 2 (1949): 238–44; Walter H. Munk, G.C. Ewing, and R.R. Revelle, "Diffusion in Bikini Lagoon," *Transactions of the American Geophysical Union* 30, no. 1 (1949): 59–66.

252. Sargent and Austin, "Organic Productivity of an Atoll," 245.

findings, based on comparing the rate of production of organic matter in the surface water (by measuring oxygen content) between bottles kept in light and dark, not only allowed them to calculate a maximum possible growth rate of the reef, but also reinforced the view that atolls were discrete, self-supporting entities. Their paper was a direct offshoot of the control atoll studies, and in fact, the generic atoll mentioned in their paper title was not Bikini but its upwind neighbor Rongelap, where Sargent and Austin gathered data both before and after the Able blast. Perhaps as much as any other, this paper contributed to the notion that an atoll was a self-contained ecological unit. In Howard and Eugene Odum's much better known 1955 paper on Eniwetok Atoll that was one of the founding statements of systems ecology, they cited Sargent and Austin's "ingenious" methods as inspiration for their own study of "these inherently stable reef communities"[253]

The radiological catastrophe of the Baker shot meant that the resurvey of Bikini and the control atolls had to wait until the summer of 1947. Ladd, Tracey, Emery, and many of the other scientists involved in Crossroads returned to Bikini as part of a second procedure, Operation Crosscheck. More often called the Bikini Scientific Resurvey, it was commissioned by the Joint Chiefs of Staff after strong lobbying from Revelle, who was now at the newly formed Office of Naval Research. At his urging, the Resurvey comprised a broader program of research than had Crossroads itself, and included scientists and support personnel in ten divisions. These included Geology (15 participants), Radiobiology (11), Fisheries (11), Biology (9), and Radiochemistry and Radiophysics (11); participants were drawn from among the Army and Navy, the Atomic Energy Commission, Scripps Institution of Oceanography, the U.S. Geological

253. Howard T. Odum and Eugene P. Odum, "Trophic Structure and Productivity of a Windward Coral Reef Community on Eniwetok Atoll," *Ecological Monographs* 25, no. 3 (1955): 291–320.

Survey, the Smithsonian, and nine universities.[254] The primary goals stated for the Resurvey were “Determinations of the amount and nature of the radioactivity remaining in the water, the lagoon floor, and the reef and land structures of the atoll, wherever such radioactivity exceeds the normal level of natural radioactivity and cosmic rays;” “Determinations of the concentration and kind of radioactive materials in the animals and plants of the atoll and the effects of this radioactivity on the organism[s];” and “Physiological, geological and oceanographic studies of organisms and reef building processes in order to gain basic information for better understanding of the possible biological effects of Operation Crossroads.”[255] Scientists on the Resurvey noticed a greater acceptance of their place on the mission than they had the previous summer. The Smithsonian Institution ichthyologist L.P. Schultz wrote in his diary, “This expedition was entered into by me with some skepticism as to the possibility of cooperation between the Navy—and civilian scientists after my experience last year on the [U.S.S.] Bowditch [the scientists’ base during Crossroads].” Of the Resurvey, he noted that even high ranking officers seemed to be “there to serve the scientists. […] It was the reversal of 1946 in every detail. The Navy must have profited from the 1946 experience and made this an experiment in cooperation.”[256]

Civilian scientists who had to be recruited to participate in the Resurvey were lured with promises that their own research careers would be advanced by the special opportunities it presented. Meanwhile, their home institutions had to be convinced that their employees’ time should be devoted to classified research. In confidential communication seeking leave for scientists on faculty at the University of California, the Secretary of the Navy James Forrestal emphasized the Resurvey’s practical value to the Navy

254. *Operation Crossroads 1946*.
255. See, eg, memo from Parsons to AEC, 4 June 1947, AEC General Correspondence, NARA II, RG 326, Entry 67A, Box 67, folder “AEC 800.92 Bikini (6-4-47).”
256. “1947 Notes,” L.P. Schultz papers, SIA, box 25, folder 7.

but also encouraged the view that it would be an occasion for research into existing problems in the basic sciences. Thus the Navy's primary concern was with the "transfer and accumul[a]tion of radioactive substances from water and sediment to the plants and hence to the animals [of Bikini lagoon]. Such studies may be of great importance in future planning for atomic defense." But he continued, "Moreover, the unusual physiology and environmental conditions of both the land and the marine plants of the atoll, combined with the presence of radioactive tracer substances in relatively large amounts, may make possible a unique contribution to basic problems of plant nutrition."[257] AEC Chairman David Lilienthal, also writing to university President Robert Sproul, echoed Forrestal in his presentation of the dual benefits to be accrued by study at Bikini.

> As you know, the possible biological effects of radioactivity from nuclear fission products are of great concern to the Atomic Energy Commission, not only as they directly involve problems of human physiology but also in connection with possible effects on agriculture and fisheries. The enormous amounts of fission products deposited on the reefs and in the waters of Bikini Atoll as a result of the underwater burst last summer, and the very large accumulations of these materials by plants and animals, make this area an important and unique field experiment station for radiobiological problems of plant nutrition and soil chemistry.[258]

Navy Captain Christian Engleman, the Resurvey Project Officer, simply told a newspaper that "we would be negligent in our duties if we did not return to Bikini for periodic research. It is the greatest laboratory in the world."[259]

To this laboratory Harry Ladd brought the ambition shared by coral reef researchers of all persuasions, to see a bore run all the way into the basement rock

257. Forrestal to Robert Gordon Sproul [President of the University of California], 2 June 1947, AEC General Correspondence, NARA II, RG 326, Entry 67A, Box 67, folder "AEC 800.92 Bikini (6-4-47)."

258. [Lilienthal] to Robert Gordon Sproul, [June 1947], AEC General Correspondence, NARA II, RG 326, Entry 67A, Box 67, folder "AEC 800.92 Bikini (6-4-47)."

259. "Bikini Called 'World's Greatest Laboratory'," *San Diego Union*, 12 September 1947, 1.

beneath an atoll. With the support and encouragement of Revelle, who had been interested in the deposition and distribution of organic sediments himself since the late 1930s, Ladd was placed in charge of a crew of oil drillers sent by the George E. Failing Company of Enid, Oklahoma. Failing also supplied the rig, a 1500 Holemaster, which had the drill mounted on the bed of pickup truck for portability. The funds were provided by Revelle's Navy Department Bureau of Ships.[260] The drill was to be run continuously by two three-man crews who worked in twelve hour shifts with a geologist on duty at all times. When they were not supervising the drilling, Ladd and Tracey resurveyed their 1946 reef traverses.[261] On Friday morning, 18 July 1947, on the islet of Bikini at the northeasternmost point of the atoll, the Project Officer Engleman christened the Holemaster with a can of cold beer and coring commenced.[262] This was reported in the press release quoted at the beginning of this chapter, in which Revelle declared that the Bikini drilling could potentially "prove whether or not Darwin was right."[263] The first hole was cored to a depth of 300 feet. Because the hole passed through mostly sandy or cavernous material, very little of the core (only twelve percent) was actually recovered. After getting similar results on a second shallow coring run, the crew switched to a non-coring drill bit and began to set their sights deeper. Meanwhile, press releases cascaded forth from the Navy public communications staff promising "daily announcements on [the] progress of the drilling."[264] Two deep holes were drilled

260. Harry S. Ladd, Joshua I. Tracey, Jr., and G.G. Lill, "Drilling on Bikini Atoll, Marshall Islands," *Science* 107, no. 2768 (16 January 1948): 51–55.

261. Joshua I. Tracey, Jr., "Subsurface Geology of Bikini Atoll, Marshall Islands," Ph.D. thesis (Yale University, 1950), 1–2.

262. Staff of the Project Officer of Bikini Scientific Resurvey, "Core Drill Bores Deeper Into Geological Background," *Bikini Backtalk*, 19 July 1947, 1. Ladd Scrapbook, p. 43. Except for the added paragraph describing the christening of the rig, the article cited is a reprint of Bikini Scientific Resurvey Press Release No. 12.

263. "Bikini Scientific Resurvey Press Release No. 12," 18 July 1947. L.P. Schultz Papers, SIA, Box 26, folder 3, "Bikini Scientific Resurvey, Correspondence, Press Releases, etc."

264. "Bikini Scientific Resurvey Press Release No. 14," 22 July 1947. L.P. Schultz Papers, SIA, Box 26, folder 3, "Bikini Scientific Resurvey, Correspondence, Press Releases, etc."

on the lagoonward side of the islet beside the location of the second test coring hole. The first of these reached 1,346 feet, but had to be abandoned after a piece of pipe was broken off in the hole.[265] On the second deep hole, called 2B, the drill was run for 190 hours, at which point every available section of drill pipe was in the hole. The bit had reached a final depth of 2,556 feet. During the drilling the hole was kept full of fresh water, which circulated the cuttings to the surface. With every five or ten feet of progress, which corresponded to the length of each section of drill pipe that had to be added to the string, the cuttings were recovered for later analysis. Studies of the included fossils, particularly those of microscopic foraminifera, would allow the cuttings to be dated, while lithologic analysis would help to establish the environment under which the material had been laid down. But at was immediately clear that the hole had not reached bedrock. Even at nearly half a mile below the surface of the atoll, the cuttings consisted of calcareous sand. A day or two after the drill string was removed from 2B, a civilian geophysicist, Joseph Chernock, began making seismic refraction tests using the hole.[266] With a geophone (a receiver of seismic vibrations) placed at a known depth in hole 2B, a charge was set off at a different depth in the adjacent hole 2. A series of 73 charges was made while the geophone was at various depths chosen to match the locations of the top and bottom of distinct strata recorded in the drillers' log.[267] This allowed for the calculation of the seismic velocities through rocks of known composition, and these figures in turn were used to refine the interpretation of the seismic survey of Bikini Atoll that had been conducted in 1946. The latest press release declared 2B to be "the deepest hole ever drilled in a Pacific

265. Tracey, "Subsurface Geology of Bikini Atoll, Marshall Islands," 17.
266. On the timing of the seismographic work, see Staff of the Project Officer of Bikini Scientific Resurvey, "Drillers Stop at 2557 Feet," *Bikini Backtalk*, 21 August 1947, 1. Ladd Scrapbook, p. 49.
267. M.B. Dobrin and Beauregard Perkins, *Seismic Studies of Bikini Atoll*, U.S. Geological Survey Professional Paper 260-J (Washington, D.C.: U.S. Government Printing Office, 1954).

Atoll."[268]

Why did the Navy care whether the Bikini drilling had set records, or even whether Darwin or Daly was closer to the truth about coral reef formation? The Bikini drilling was done because it would be useful to those conducting it and those funding it. The value of the study to a geologist like Ladd was obvious, and as I will describe in greater detail below, actual knowledge of subsurface geology was valuable to those detonating the bombs as well. But the kind of publicity that it received from the Navy emphasized it as a basic science puzzle and entirely obscured its relevance to the bomb tests (which among other things, sparked fears that the atoll's foundation would collapse), and this shows that the drilling could be made to serve another purpose as well. "Annex L" of the 1947 Operation Plan, which was approved (and was probably at least co-authored) by Revelle, showed that this value was apparent before the Holemaster had started turning:

> PUBLIC INFORMATION PLAN
>
> I. Certain obvious features of the BIKINI Scientific Resurvey must be considered in the formulation of a workable public information plan; these include:[...]
> D. The necessity for presenting to the American people in an intelligent manner the story of cooperation that exists between civilian and military agencies in the BIKINI Resurvey work. Proper handling of the BIKINI Resurvey story can do much to acquaint the American public with the long range value of OPERATION CROSSROADS.
> E. The importance of providing a continuing series of newsworthy press releases to the public, through established public information channels during the course of the operation. These press summaries should begin immediately following the official public announcement of the Resurvey. Interesting, newsworthy stories from BIKINI, that concern the operation, will forestall much press criticism and speculation of a harmful nature. [...including...] Science stories concerning the study to be made of the BIKINI reefs, and the geological structure of the atolls.[269]

268. "Bikini Scientific Resurvey Press Release No. 21," [15?] August 1947. L.P. Schultz Papers, SIA, Box 26, folder 3, "Bikini Scientific Resurvey, Correspondence, Press Releases, etc."

269. "Operation Plan" for Bikini Scientific Resurvey, pp. L-1- L-2. NARA II, RG 374, Defense Nuclear Agency, Entry 47B, Bikini Resurvey, Box 156, Folder A3 "Organization and Management"

Coming on the heels of Hiroshima and Nagasaki and conducted in peacetime, the 1946 bomb tests were highly controversial.[270] This section of the Operation Plan for 1947 suggests that part of the value of conducting the Resurvey was the creation of a venue to rehabilitate the reputation of the bomb tests. The Bikini press releases described the atoll in terms of the wealth of knowledge that the bomb test had sowed and which merely awaited harvest by opportunistic scientists. They also managed to portray Bikini as a benign paradise while still emphasizing what a high technical achievement the Crossroads tests had been. The Resurvey press release of 24 July begins:

> Sun-tanned sailors and scientists observed the anniversary of the world's first underwater atomic bomb explosion today by going swimming in the clear blue-green 84° warm waters of Bikini lagoon. They swam from beaches that one year ago were lashed by high and angry waves thrown outward from the explosion point.
> When the Baker Day atomic bomb exploded, half a million tons of water boiled up out of Bikini Lagoon in a mile high cylinder and fell back like rain on the CROSSROADS target fleet in what was probably the most impressive man made spectacle ever staged.
> One year later the scientists and military personnel now engaged in an intensive six week scientific resurvey of Bikini Atoll can find few visible effects of the that blast. Except for the activities of the 700 man Bikini Scientific Resurvey Task Group, Bikini is the same placid palm ringed lagoon on which King Judah and his subjects sailed in outrigger canoes.[271]

The core drilling at Bikini served to help portray Operation Crossroads as a study of nature and an opportunity to expand human knowledge. Like the widespread use of the language of experiment to promote the test, it implied that learning about the bomb and nuclear energy were the same kind of pursuit as learning about atoll formation, and you could use the one to learn about the other.

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270. Jonathan M. Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll* (Annapolis: Naval Institute Press, 1994).

271. "Bikini Scientific Resurvey Press Release No. 16," 24 July 1947. L.P. Schultz Papers, SIA, Box 26, folder 3, "Bikini Scientific Resurvey, Correspondence, Press Releases, etc."

The Bikini drilling was also a public relations coup for the Failing company. The Holemaster used in the Resurvey was designed to drill a to a maximum depth of 1500 feet, but with no basement rock in sight the Oklahomans had nearly doubled that standard. "2,557 FT. UNDER BIKINI," read the banner on a full-page advertisement for Failing's portable rigs, which ran in the November 1947 issue of the *Bulletin* of the American Association of Petroleum Geologists. "Coring the coral formations, loose sands, and conglomerates at Bikini was no cinch, but the Failing Holemaster came through with colors flying. [...] Despite this unusual load the unfailing Failing drill did not groan nor grumble. Down, down, down it went until the government geologists called 'enough!'"[272]

After the scientists had gone home from Bikini for a second time, the difficult work of analyzing the contents of the holes. Fossils were distributed for identification: the corals went to Wells, who had completed a revised the taxonomy of the scleractinian corals with Vaughan four years earlier.[273] The molluscs went to Ladd, larger Foraminifera to W. Storrs Cole, smaller forams to Rita Post and Ruth Todd, and calcareous algae to J. Harlan Johnson, who had also been at Bikini. Tracey, who had come onto Crossroads and the Resurvey as a member of the USGS, submitted his study of the cores and cuttings as a Ph.D. thesis at Yale before incorporating it into the final USGS report that he coauthored with Emery and Ladd.[274]

As soon as prelimary results were available, Ladd, Tracey, and a rotating cast of co-authors brought such material as could be declassified into print. In a paper they wrote with Hoffmeister they described the zonation of the living reef, but did not

272. A copy of this advertisement is inserted in Ladd Scrapbook, p. 53. The scrapbook also contains several clippings from the Failing company newsletter, "The Core Driller."

273. T.W. Vaughan and J.W. Wells, *Revision of the Suborders, Families, and Genera of the Scleractinia*, Geological Society of America, Special Papers, 44 (1943). Citation taken from William R. Brice, "John West Wells, July 15, 1907-January 12, 1994," *National Academy of Sciences Biographical Memoirs* 70 (1996).

274. Tracey, "Subsurface Geology of Bikini Atoll, Marshall Islands."

mention the drilling at all, and refused to offer any "[s]peculation as to the origin of the atoll" pending further investigation.[275] Ironically, their first statement on the drilling results, written with G.G. Lill of the new Office of Naval Research, was actually in print seven months before the reef zonation paper, by virtue of its placement in the high-frequency journal *Science*.[276] In this short article they discussed the lithology of the 2500 foot section and described the results of the seismic investigation, but they carefully avoided synthesizing these observations into a historical account of the atoll's formation. Even so, the physical description would have been striking to anyone familiar with the coral reef debate. After penetrating 65-75 feet of reef limestone, the drill had passed through five generally distinguishable zones of rock on its way to 2,556 feet. All were composed of calcareous sediment, much of which was sand or poorly consolidated limestone. One sandy section between 725 and 1,100 feet contained well preserved fossils belonging to a readily-identifiable shallow water fauna. A core run taken between 925 and 935.5 feet in hole 2A that could be dated to the upper Tertiary, i.e. pre-Glacial, period had yielded fossils of reef corals and mollusks. The excellent condition of these fossils indicated that when they were deposited they had not been transported far from their position of growth, and the characteristic association of corals, mollusks, forams reinforced this conclusion. "[T]he entire assemblage suggests a depth considerably shallower than that where it is now found."[277]

Equally suggestive were the geophysical observations. The seismic work indicated that a hard material with an irregular surface and a seismic velocity characteristic of igneous rock underlay the atoll, but that it only reached to within a range of 6,000 to 13,000 feet below sea level. Meanwhile the seismic velocity of the material above it corresponded with that of the calcareous sand found between 1100 and

275. Tracey, Ladd, and Hoffmeister, "Reefs of Bikini, Marshall Islands," 862.
276. Ladd, Tracey, and Lill, "Drilling on Bikini Atoll, Marshall Islands."
277. Ladd, Tracey, and Lill, "Drilling on Bikini Atoll, Marshall Islands," 52.

1800 feet in hole 2B. "It would appear, therefore, that the entire section above basement--a section 1-2 miles in thickness--is sedimentary in nature, probably composed of calcareous sediments not unlike those found in the lower part of the deep hole."[278] If this were true, then the shallowest part of the bedrock lay at least twice as far from the surface as the deepest reach of their drill.

Instead of even mentioning the possibility of subsidence, Ladd, Tracey, and Lill called for more research to be done. One reason was that the lithologic features of the Bikini sediments varied considerably from those cored at Funafuti and Kita-Daito-Jima. Another was the classic question of the foundation. "The character of the unknown basement rock," they argued, was "a matter of prime importance." It could be that Bikini was formed atop "a basaltic mound," meaning a volcano, "but this is not certain."[279] They advocated for the drilling of a hole to a depth of 8,000 to 10,000 feet near the center of Bikini lagoon, where according to geophysical evidence the basement should lie within that depth. Because there was no solid ground in the middle of Bikini lagoon, they proposed sinking a barge atop one of the broad-topped coral knolls that reached within a dozen feet of mean sea level in the lagoon. "A substantial foundation for a drilling platform could then be provided," they explained, by means of a portable drilling rig mounted on the barge. "Engineers and drillers with whom the plan has been discussed foresee no major difficulties."[280]

In 1949 the duo of Ladd and Tracey finally staked their claim as counterparts to the great reef theorists of the past with a *Scientific Monthly* article on "The Problem of Coral Reefs."[281] In this general-interest journal they traced their lineage as students of "the ancient and controversial problem of coral reefs" back to Darwin and Chamisso.

278. Ladd, Tracey, and Lill, "Drilling on Bikini Atoll, Marshall Islands," 53.
279. Ladd, Tracey, and Lill, "Drilling on Bikini Atoll, Marshall Islands," 54.
280. Ladd, Tracey, and Lill, "Drilling on Bikini Atoll, Marshall Islands," 54.
281. Harry S. Ladd and Joshua I. Tracey, Jr., "The Problem of Coral Reefs," *The Scientific Monthly* 69, no. 5 (1949 November 1949): 297–305.

The dubbed the three decades that had gestated the day's active reef researchers, from 1910 (when Daly's first paper on the effects of Pleistocene glaciation appeared) to 1939, the "Thirty Years' War." By their telling, the major conflict had been between advocates of the new Glacial-control theory and supporters of subsidence, led by Daly and Davis respectively. While these armies trenched in to fight their battle of "very definite ideas about reef origin" Vaughan took the diplomatic course available to one who "recognized the necessity of studying each reef in relation to its local environment and was aware of the dangers of generalizations based too largely on physiographic form." Gardiner, who like Vaughan was actually "familiar with reef organisms and their requirements," was praised for his 1931 book, which "show[ed] a more objective approach [than Davis's] and is probably the finest general presentation of the subject of existing reefs yet written."[282]

When they turned to the interpretation of present reef knowledge, Ladd and Tracey exemplified the virtuous characteristics that they had attributed to Vaughan: openness to diverse ways of studying reefs and concern for the particularities of local field sites. Here they called not only for another deep boring, but also for magnetic and seismic surveys, for geological mapping based on airborne photography, for ecological studies that would "indicate the amount of organic matter produced [and] the amount available for burial in reef sediments, and set limits to the rate of reef growth under existing conditions," and for chemical studies of the effectiveness on solution of several different types of reef limestones.[283] As to the interpretation of their own work at Bikini, they implied that it may have left reef scientists farther from a general theory of atoll formation than they had been before Operation Crossroads. "[When] the Bikini findings are compared with the results of drilling done on coral islands and reefs in

282. Ladd and Tracey, "The Problem of Coral Reefs," 298.
283. Ladd and Tracey, "The Problem of Coral Reefs," 303–4.

other parts of the Pacific," they declared, "[i]t is clearly shown that we are not yet ready to generalize or to predict what the next deep hole on a coral island will reveal. Thus, of the three deep holes drilled on islands in the open Pacific...the hole on Kita-Daito-Zima was consolidated in its upper part, the one on Funafuti was consolidated on its lower part, and the one on Bikini, the deepest, was not consolidated at all. The Kita-Daito section was dolomitized in its upper part, Funafuti in its lower part, Bikini not at all. The ages of the rocks penetrated likewise varied considerably."[284] And of course, they concluded, "One or more holes drilled through a coral atoll will certainly be required for a final solution to the reef problem."[285]

Writing for the much more private audience represented by his Ph.D. committee, Tracey revealed just what the Bikini geologists had been forced to acknowledge about the atoll's history. If the geophysicists were correct that the layer between known shallow water deposits and basement rock was as much as two miles thick, "A relative subsidence of 7,000 to 13,000 feet over a long geologic time is necessary to explain the rocks of the intermediate zone"[286]

Not every geologist was prepared to accept subsidence on a super-Darwinian scale, however. In a series of letters written to Ladd in response to the Bikini drilling paper, Daly suggested that the next boring should be situated about a third of the way from the atoll rim to the center of the lagoon. He did not believe in the seismological data, and expressed his "hunch" that a truncated volcanic cone was concealed inside the atoll, and that "volcanic material at say 1/3 of the distance [from atoll rim to lagoon center] should be found at a depth no greater than about 300 feet."[287] As he explained, showing more than thirty years' worth of exasperation, "At Funafuti...and Bikini the

284. Ladd and Tracey, "The Problem of Coral Reefs," 302–3.
285. Ladd and Tracey, "The Problem of Coral Reefs," 304.
286. Tracey, "Subsurface Geology of Bikini Atoll, Marshall Islands," 133.
287. Daly to Ladd, 19 July 1948. Ladd papers, SIA, box 1, folder 5. In the margin of this letter, Ladd wrote "I think hunch is wishful thinking."

expensive drilling was in the main atoll reef – the wrong place!" To Daly the solution was as clear as it had been in 1915: "Breakfast Island of Jaluit atoll would be a good place to bore – so far inside the reef that it would not cut merely talus."[288] Ladd wrote back assuring Daly that he agreed with the selection of Breakfast Island as an ideal drilling location, and revealing that he had originally proposed the site himself. His story was a striking demonstration of the way in which a longstanding research interest got incorporated into the Crossroads operation, and how it had to be compromised to suit the particularities of Bikini atoll.

> When Operation Crossroads developed, Bikini Atoll was studied intensely […]. Even after the completion of the first Bikini Survey in 1946, I still talked about the advantages of Breakfast Island as a drill site. Bikini was later agreed upon for purely practical reasons. Actually, there was no choice, for we found that, whereas we could obtain funds for a limited drilling program at Bikini by tying the work into the Crossroads Resurvey, we couldn't have gotten funds for any kind of an independent operation at Jaluit.[289]

Ladd may have preferred to drill at Jaluit in 1946, but by now Bikini had its own advantages. Like many of the other scientific participants in Operation Crossroads and the Resurvey, he had profited immensely from the wide range of studies that had been done on Bikini. His geological work had been made easier, and its meanings more clear, because every imaginable feature of the atoll and its inhabitants had been documented. As he and Tracey explained in their 1949 rallying cry for another drilling project there, "Bikini has been selected for the proposed operation because at the present time more is known about that atoll than any other."[290]

Shooting the moon at Eniwetok Atoll

Ladd, Tracey, and company drilled more than four times deeper than Darwin had imagined, in 1881, would be necessary to prove subsidence, and found nothing

288. Daly to Ladd, 16 February 1948. Ladd papers, SIA, Box 1, folder 5.

identifiable that was not the limy remains of a shallow water creature. Yet because they had failed to reach basement rock, there was still room to insert at least a sliver of doubt into the question of whether "Darwin was right" about Bikini's origin. Ladd set about raising money to take another try at drilling for the ultimate goal. He quickly received promises of support from the Office of Naval Research and the Geological Society of America, but the sum of their pledges fell short of the amount needed to fund a return to Bikini.[291] Meanwhile other scientists continued to appear at Bikini, which became an active site of radiobiological research by members of the University of Washington Applied Fisheries Laboratory.[292] In 1950 the joint Scripps-Navy Electronics Laboratory Mid-Pacific Expedition dredged on the outer slopes of Bikini and found basaltic rocks at depths between one thousand and two thousand fathoms (6,000-12,000 feet), which provided the first direct evidence that Bikini was underlain by volcanic rock.[293]

The year after the Resurvey, atomic testing moved to the atoll of Eniwetok, which Tracey had visited as part of his Crossroads research.. The three shots of Operation Sandstone there in April and May of 1948 were the sixth, seventh, and eighth atomic blasts in history, and the first tests since control over nuclear energy had been handed to the civilian Atomic Energy Commission (AEC) on 1 January 1947.[294]

In late 1950 a Coral Atoll Research Program was initiated by the Pacific Science Board of National Research Council. It was run by a committee with the mission of

289. Ladd to Daly, 24 February 1948. Ladd papers, SIA, box 1, folder 5.
290. Ladd and Tracey, "The Problem of Coral Reefs," 304.
291. Ladd and Tracey, "The Problem of Coral Reefs," 304.
292. Neal O. Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946–1951* (Seattle: University of Washington Press, 1962).
293. H.S. Ladd, et al., "Drilling on Eniwetok Atoll, Marshall Islands," *Bulletin of the American Association of Petroleum Geologists* 37, no. 10 (1953 October 1953): 2277, note 8; Gordon A. MacDonald, "Igneous Rocks," in *Geology of Bikini and Nearby Atolls: Part I, Geology*, U.S. Geological Survey Professional Paper 260-A, Kenneth O. Emery, Joshua I. Tracey, Jr., and Harry S. Ladd (Washington, D.C.: U.S. Government Printing Office, 1954), 120–24.
294. For a general history of the six Eniwetok operations between 1948 and 1958, see U.S. Department of Energy Nevada Operations Office, *United States Nuclear Tests: July 1945 Through September 1992*.

promoting broadly ecological atoll field studies that would aid in the U.S. administration of the Trust Territory of the Pacific Islands, established in 1947. To aid encourage quick dissemination of field data, the Pacific Science Board introduced the *Atoll Research Bulletin*, which was published on an ad hoc basis beginning in 1951 with botanist F.R. Fosberg as editor.[295] When Ladd was unable to attend an organizing symposium in December 1950 to speak on geological studies of atolls and lay out a program for future research, Tracey stepped in on his behalf to deliver a brief report. As the first priority for "future geologic work," he gave "Drilling. A deep hole to the basement rock should be drilled on an atoll that has been well studied. At Bikini such a hole would probably be 4000-7000 feet deep." On his own copy of the text, Tracey added a note that actually reaching that depth would be "shooting the moon."[296]

In 1951 Ladd was brought to Eniwetok by the AEC to oversee the drilling of a deep hole on the islet of Engebi, at the northern extremity of the atoll rim. The Holemaster rig had been brought over from Bikini to drill several shallow holes in 1950, and it was being run by the same Failing company foreman, V.C. Mickle, who had run the drill crew for the Resurvey. At Engebi they put a hole called K-1 down to a depth of 433 feet, and an adjacent one, K-1B, down 1,280 feet. It is difficult to follow Ladd's movements afterward; this enterprise seems to have been either tightly classified or anticlimactic, because it there is no material relating to it in Ladd's papers. Nor is there any in the papers belonging to Tracey, who was much more systematic than Ladd in preserving correspondence. However, Tracey did receive a carbon copy of a 15 February 1952 letter from Ladd to Emery (which had a hand written note added to the bottom by Ladd saying "J: The peripatetic Mister Ladd is still in town [i.e., in

295. For a recent brief history of the journal and a biography of Fosberg, see the special number of the *ARB*. David R. Stoddart, ed., "F. Raymond Fosberg and the *Atoll Research Bulletin 1951–1991*," *Atoll Research Bulletin*, no. 305 (May 1992).

296. Tracey Papers, SIA, Accession 02-021, box 1, folder "Coral Atoll Committee, Pacific Science Board, 1951.

Washington] for a few days, but even so, I take this opportunity to send greetings via our footnote correspondence!"). Emery, Tracey, and Ladd were the co-authors of the main volume on the geology of Bikini then in draft, so they habitually engaged in three-way correspondence by carbon copy, as evidenced by the evidently full collection saved in Tracey's files. In the main body of this letter, Ladd had written to Emery, "As you may have heard from Roger [Revelle] I have just returned from a quick trip to the Marshall Islands during the course of which we completed plans for additional drilling on Eniwetok Atoll. This phase of the project is not classified—at least not at the present time—but it is not, of course, being given any publicity. The work will probably be done during May and June, and I hope that this time we will really find out a great deal about the foundations of the atoll. Planning and preparation is naturally taking considerable of my time, but I am determined to see the main part of the Bikini report in the hands of the Survey editors—including modifications and additions by you and Tracey—before I take off. The findings of this latest venture will be written up in full as a later chapter [i.e., separate volume] of the Bikini professional paper."[297] Ladd's shadowy partners in planning the next Eniwetok venture were the AEC, Los Alamos Scientific Laboratory, and most importantly, the Armed Forces Special Weapons Project (AFSWP).[298] According to a notice of the drilling that Ladd contributed to the ONR's "Research Reviews," the "primary justification for this expensive operation was the need, on the part of AEC scientists, for factual data to support their geophysical interpretations."[299] In his 1977 oral history interview, Ladd said that the AFSWP

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297. Ladd to Emery, 15 February 1952, Tracey papers, SIA Accession 02-021, box 4, folder, "Ladd 51-52."

298. See "Acknowledgements," Harry S. Ladd and Seymour O. Schlanger, *Drilling Operations on Eniwetok Atoll*, U.S. Geological Survey Professional Paper 260-Y (Washington, D.C.: U.S. Government Publishing Office, 1960), 865–67.

299. Harry S. Ladd and Seymour O. Schlanger, "Down to Earth on Eniwetok," Office of Naval Research Research Reviews (Washington, D.C.: Department of the Navy, 1955), 13. There is a copy of this publication taped into Ladd Scrapbook at p. 108.

"provided the original sum of money for that drilling" because they "wanted to get some dope on the foundation of Eniwetok to enable them to interpret their seismic surveys more accurately. They did pretty well without a hole to the basement of Bikini, but they weren't satisfied."[300] Ladd hoped he would be able to take Tracey with him on "the interesting project that is being cooked up for the Marshall Islands," but Tracey was assigned to be the field chief of the Military Geology study of Guam in 1952 and 1953, and Ladd had to tell him that despite lobbying the Chief of the Military Geology Branch, Frank Whitmore, to get Tracey on board, he had "received word from Father Whitmore that after deep thought he has concluded that your services cannot be spared from Guam for even one month."[301]

Writing up the Bikini geology report and coordinating the production of chapters by other authors had weighed heavily on Tracey, Emery, and especially Ladd. Ladd was desperate to submit the manuscripts to the USGS before leaving for Eniwetok because he fully expected to gather data there that would be relevant to the Bikini report, which would tempt him to postpone publication still further in order to synthesize the results. As he wrote to Emery, "This, as you know, could go on endlessly because we will be working on the Marshall Islands collections for years to come."[302] Ladd was further encouraged to hurry out the Bikini paper because he believed, based on word from Lill at the ONR, that they would be able to obtain military clearance for the publications based on review of the abstracts alone.[303] On 24 April 1952 fourteen manuscripts designated for the USGS "Professional Paper" (the Survey's premier line of research publications) on "Bikini and nearby atolls," which had been edited by the Military

300. Ladd Oral History, p. 31.
301. Ladd to Emery, 28 February 1952, Tracey papers, SIA Accession 02-021, box 4, folder, "Ladd 51-52." On Whitmore, see Whitmore, "The Pacific Island Mapping Program of the U.S. Geological Survey". For a brief review of Tracey's work in Guam, see Joshua I. Tracey, Jr., "Working in the Pacific," *Atoll Research Bulletin*, no. 494 (December 2001): 11–20.
302. Ladd to Emery, 8 April 1952. Tracey papers, SIA Accession 02-021, box 4, folder "Ladd 51-52."
303. Ladd to Emery, 8 April 1952. Tracey papers, SIA Accession 02-021, box 4, folder "Ladd 51-52."

Geology Branch, were delivered with Whitmore's approval to the Director of the Survey for authorization to publish. Abstracts had been approved by the ONR, who judged that "all of the material was in the field of pure science and...none of it should be in any way restricted," and were being sent on the AEC. If that clearance was granted there remained just one more hurdle, review by the Navy. The Army had agreed to waive its right to review the material if it were approved by the Navy.[304]

The many-faceted Professional Paper on Bikini and nearby atolls exemplified the Crossroads scientific impulse to define atolls through a multi-disciplinary matrix of interrelated field and laboratory studies. The first batch of fourteen papers alone comprised two thousand pages of text, 130 figures, and twenty charts. As Ladd explained this unprecedented profusion of extra-geological material to Whitmore,

> The field investigations carried on during Operation Crossroads were on a large scale and scientists of all kinds took part in the work. It was an unique opportunity for intensive study of atoll geology and all of the fields closely related to geology. In assembling the present parts of the report, we have tried to present as complete a picture as possible of the atolls and their physical setting.

This geological goal was only achievable through catholic acceptance of other lines of research.

> The atolls cannot be clearly pictured nor their history well understood without some knowledge of the oceanographic environment in which they have been formed; hence, the inclusion of several chapters dealing with physical oceanography. Likewise, the reefs that make up the present day surfaces of the atolls and the limestone foundations on which they rest cannot be well understood without some knowledge of the rock-building organisms that are responsible for their construction; hence, the inclusion of special reports on such groups as the corals and the Foraminifera that are living in the area today.[305]

The human embodiment of this all-purpose approach to research was Roger

304. Ladd to "authors of the Bikini professional paper," 24 April 1952. Tracey papers, SIA Accession 02-021, box 4, folder "Ladd 51-52."

305. Ladd to Whitmore, 24 April 1952. Tracey papers, SIA Accession 02-021, box 4, folder "Ladd 51-52."

Revelle, who had been the driving force behind the assembly of the all-encompassing Bikini scientific teams. Ladd had been "needling" Revelle for months to write a foreword to the Professional Paper, which the Commander had long ago agreed to submit. Ladd wanted Revelle to frame the project because, as he told Whitmore, "With his broad interests, he is uniquely qualified to prepare a Foreword that would not only tell the story of how Operation Crossroads was planned and carried out, but would do a great deal to tie the numerous chapters of the present report closely together."[306] Exactly a week later, Revelle finally turned up at Ladd's house in Washington with a draft, which was forwarded to Tracey in Guam for review. Tracey responded impertinently that he liked it, but he thought Revelle's "first two paragraphs are a bit flowery and reminiscent of 'The Sea Around Us,'" referring to the 1951 bestselling book by Rachel Carson.[307] The long-suffering Ladd acknowledged that "The opening is a little fluffy but the more you read it the better you will like it. I think it is excellent & if it reminded you of 'The Sea Around Us' that is a real compliment!"[308] In its published form, at the head of a series that eventually ran to 35 papers, the beginning of Revelle's introduction read

> Of all earth's phenomena, coral reefs seem best calculated to excite a sense of wonder. And of all the forms of coral reefs, the atolls have appeared to men of science to be the richest in mystery and the most strange. Rising alone from the empty sea, these ancient structures, growing now slowly, now fast, toppling when the sea retreats and flung up in haste when sea level rises, are like a Gothic cathedral, ever building yet never finished, infinite in detail yet simple and massive in plan. Tiny plants and animals are their builders. Their architects are the giant ocean and the restless wind.[309]

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306. Ladd to Whitmore, 24 April 1952. Tracey papers, SIA Accession 02-021, box 4, folder "Ladd 51-52."

307. Tracey to Ladd, 5 August 1952 (from Guam). Tracey papers, SIA Accession 02-021, box 4, folder, "Ladd 51-52."

308. Ladd to Tracey, 12 August 1952. Tracey papers, SIA Accession 02-021, box 4, folder, "Ladd 51-52."

309. Roger Revelle, "Foreword," in *Geology of Bikini and Nearby Atolls: Part I, Geology*, U.S. Geological Survey Professional Paper 260-A, Kenneth O. Emery, Joshua I. Tracey, Jr., and Harry S. Ladd (Washington, D.C.: U.S. Government Printing Office, 1954), iii.

In the Foreword, Revelle gave a brief history of the organization of Crossroads and the Resurvey, and summarized each of the contributions in the Professional Paper. The general theme of the piece, supported by Revelle's able handling of these details, was that the studies performed during and as a result of the Bikini expeditions had combined to form a new kind of knowledge of the atoll. "Bikini Atoll was studied intensively," he wrote, "from many different points of view and with modern tools of exploration, so that it has become perhaps the most thoroughly known atoll on earth."[310] As an example, he pointed out that more species of corals had been identified at Bikini than at any other location in the world. "Since Bikini atoll lies far from the assumed centers of [coral] dispersal in the East Indies," he explained, "this probably merely reflects the intensity of the collecting done at Bikini."[311] The bomb tests given scientists this fertile field for research, and Revelle subtly repaid this debt by offering a bucolic final judgment of this atomic atoll. "Bikini Atoll appears, on the whole, to be a healthy, flourishing structure.. [...] The most important groups [of organisms] contributing to the atoll are...[the] same groups [that] have been building up the massive foundation of the present reefs at least since mid-Tertiary times."[312]

With the Professional Paper out of his hands for the moment, Ladd prepared excitedly for the trip to Eniwetok. His backers had come through with the money and materials that could make it the ultimate test. In a note scribbled to Tracey just before his departure on the eve of summer, 1952, Ladd buzzed "Don't pass it on <u>but</u> after long battles against what seemed like hopeless odds we are to drill our no. 2 hole on a coral knoll in the lagoon! And, Josh, we are <u>loaded</u> with equipment. We are all set for 5000 & there is an extra 1000 feet of drill pipe in case we have to rat-hole ahead beyond that

310. Revelle, "Foreword," iii.
311. Revelle, "Foreword," vii.
312. Revelle, "Foreword," vii.

depth in search of volcanic rock. Yes, we are loaded for [sure]."[313]

The Eniwetok plan called for two deep holes to be drilled.[314] The first would be on the islet of Elugelab, on the north-northwest side of the atoll rim in the direction of a submarine plateau that had been found adjacent to Eniwetok at 700 fathoms (4,200 feet). The second hole would be put down in the lagoon from a platform mounted on a coral knoll. A possible third deep hole would be put down on Parry Island, an islet on the windward, eastern, side of the atoll rim By 1952 Eniwetok was well developed as a more or less permanent outpost. Unlike Crossroads and the Resurvey, when the scientists were quartered aboard ships during the operations, Eniwetok was home to a land-based "all male society."[315] Ladd worked with a new drilling crew, this time made up of Texans headed by Willie Springer, who ran a trailer-mounted rig heavier than the 1500 Holemaster. Alongside Ladd were Kirk Stephenson of the AEC, who was there from Los Alamos, R.C. Fitzpatrick of ONR, and four colleagues from the USGS.

Drilling at Elugelab turned out to be extremely difficult because fully seventy percent of the section turned out to consist of either soft beds or apparently empty cavities. These cavities made it almost impossible to keep the hole, F-1, filled with circulating drilling mud, which was the means by which cuttings were meant to be returned to the surface for analysis. There was temporary loss of circulation in the hole four times on the way down to 2,135 feet, at which point the crew lost circulation

313. Ladd to Tracey, undated handwritten note. Tracey papers, SIA Accession 02-021, box 4, folder, "Ladd 51-52." The final word is illegible.

314. General details on the 1952 drilling are drawn from H.S. Ladd, et al., "Drilling on Eniwetok Atoll, Marshall Islands"; Ladd and Schlanger, "Down to Earth on Eniwetok"; Ladd and Schlanger, *Drilling Operations on Eniwetok Atoll*; Seymour O. Schlanger, *Subsurface Geology of Eniwetok Atoll*, U.S. Geological Survey Professional Paper 260-BB (Washington, D.C.: U.S. Government Publishing Office, 1963); Thomas W. Henry and Bruce R. Wardlaw, *Geologic and Geophysical Investigations of Enewetak Atoll, Republic of the Marshall Islands. Introduction: Enewetak Atoll and the PEACE Program*, U.S. Geological Survey Professional Paper 1513-A (Washington, D.C.: U.S. Government Printing Office, 1990).

315. Recollections of Eniwetok infrastructure are in Ladd Oral History, pp. 32, 34. (NB: Page 33 of the oral history text is missing, along with pp.23 and 25.)

altogether. They drilled on, though the hole was uncased below 1,973 feet and was periodically empty of any pressurized fluid, which normally acted to prevent the sides caving in. They were now receiving no cuttings whatsoever, but they pressed deeper with the boring bit and diagnosed the lithologic character of the section based on its "relative hardness," which was told by the rate of progress shown in the drilling-time record. After burning through their drilling mud much faster than expected, they put four pumps into action trying to keep the hole filled with untreated seawater. In this manner they improvised their way past the Bikini record depth, and then past 3,000 feet. When they occasionally reached zones that seemed to be composed of firmer rock, they let the pump pressure off and risked the "hazardous procedure" of running the core barrel down the hole and drilling "dry" for a few feet in hopes of getting good samples.[316] To the tantalizing depth of 4,000 feet they avoided any further mishap, and indeed, they struck several good sequences of comparatively solid limestone, which they cored successfully five times on the way to 4,553 feet.

At 4,533 feet the drillers put the boring bit back onto the string and made another 57 feet of progress with a drilling rate comparable to that of the last several hundred feet of limestone. Then at 4,610 feet the rock got appreciably harder. At 4,619 feet the drill string was backed out and the core barrel was put in the hole. It reached the bottom of the hole without difficulty, so they drilled it down into what could now only be the basement rock. When the core barrel reached 4,630 feet, disaster struck. The walls of the hole had of necessity been left uncased below the depth of 1,973 feet, more than half a mile of drilling ago. Now, with a sample of the precious bedrock almost certainly secure inside the core barrel, the hole collapsed on top of it. On 9 June Ladd wired the AEC man Stephenson, who had returned to Los Alamos from Eniwetok, with the bad news: "REACHED HARD BASEMENT ROCK AT FOUR SIX ONE

316. H.S. Ladd, et al., "Drilling on Eniwetok Atoll, Marshall Islands," 2263.

ZERO FEET[.] CORED FOUR SIX ONE NINE TO FOUR SIX THREE ZERO...FEET BUT CORE BARREL FROZEN[.]"[317] After three days of trying unsuccessfully to budge the core barrel, the only option was to abandon it. The crew lowered a charge of explosive down the hole in order to sever the drill string and salvage as much drill pipe as possible. (This was a minor indignity for the islet of Elugelab, which was the site of the first hydrogen bomb test five months later. This 10.4 megaton blast, the "Mike" shot, left a crater more than a mile wide in the spot where Elugelab had been.[318]) After recovering 3,750 feet of pipe, Ladd found himself on the receiving end of some more bad news. Drilling F-1 had been so expensive that there was not enough money left to drill the lagoon hole.

As Ladd later told the story, after dispatching emissaries who tried unsuccessfully to drum up more money, he "left the drill and flew to Honolulu and began using the telephone." His first call, to the USGS, was fruitless. Then he called Stephenson at Los Alamos, who began pressing a Navy general for funds. Ladd's message to the general was "that if you shut down this thing now--and he had sent me an ironical 'job well done' Navy business--I said, 'You have to come out and do it all from scratch. Now we have got the equipment out here and for another $80,000 I can drill a hole in the basement and bring up the basalt.' And Kirk worked on the General all night, apparently, and he got the money."[319] On 20 June 1952, a Friday, a message arrived at Eniwetok for the drilling crew chief, Springer: "O K TO PROCEED WITH DRILLING OF SECOND HOLE UNTIL APPROXIMATELY 18 JULY."[320]

With time now limited, the rig was directed to the Parry Island drill site instead

317. Inserted in Ladd Scrapbook at p. 85.
318. For a readily available account of the destruction of Elugelab, including the iconic photographic depiction of the missing island, see Richard Rhodes, *Dark Sun: The Making of the Hydrogen Bomb* (New York: Simon & Schuster, 1996), 511–12.
319. Ladd Oral History, p. 32.
320. Inserted in Ladd Scrapbook at p. 85.

of the theoretically more interesting lagoon platform. This section proved to be slightly better consolidated than Elugelab's, and though Parry's E-1 hole did lose circulation twice, caverns totaled only eight feet, compared to 122 feet in F-1. In periodic coring runs made on the way down, cores were recovered at a much improved rate of 56 percent. The Parry hole produced good cuttings beyond 3,000 feet, and Springer's men managed to case the hole all the way down to 4,109 feet. The swift progress of the E-1 run did create one difficulty: the drill bit was cutting so fast that in the lower parts of the hole it was introducing cuttings into circulation from one layer of rock well before those from the previous horizon had reached the collection point at the surface, where they were sieved from the drilling mud. To improve the accuracy with which the cuttings could be attributed to a particular depth, the crew conducted a series of four tests of the speed with which cuttings traveled back to the surface, which they did by attaching packets of water soluble dye to the bit after setting a new string of casing. Graphing the time lag between the resumption of drilling and the arrival of dye at the surface from the four depths gave a straight-line function showing the circulation time from a given depth at average pump pressure. Ladd and Seymour Schlanger used this information when they wrote up the Eniwetok report to correct the depths attributed to the cuttings in the field, which had been taken from the length of the drill string when the cuttings reached the surface..

Just short of 4,200 feet the drill struck solid rock. Once again the core barrel was sent down the hole. On 11 July Ladd composed a jubilant message that went out simultaneously to the AEC at Los Alamos and Sandia, the AFSWP in Washington, the director of the Geological Survey, and Roger Revelle, then director of Scripps. "PARRY DRILL HOLE REACHED BASEMENT ROCK AT FOUR TWO ZERO ZERO FEET[.] CORED SOLID BLACK BASALT BETWEEN FOUR TWO ZERO

EIGHT AND FOUR TWO ONE ONE[.] RECOVERY ONE ZERO ZERO PERCENT[.]"[321] The recovered basement core of a volcanic rock, olivine basalt, meshed perfectly with results of the geophysical investigations and offshore deep dredgings that suggested that the limestones of Eniwetok and Bikini were thick caps on an even taller submarine volcano. After the heartbreak of the F-1 hole collapse, E-1 had made the mission a perfect success. Ladd was justly delighted. Claude Coray, the Eniwetok project manager for Holmes and Narver, which was the civilian company contracted to operate much of the infrastructure at the atoll, sent a letter to Ladd's wife to express his delight that "the Doctor...has achieved something that has been his ambition for years." In the four days that had passed since E-1 had yielded basement rock, Ladd had been "as pleased and happy with that accomplishment as a child with a new toy."[322]

It turned out that there was a silver lining to the ill fate of F-1 as well, because it yielded another revelation about the atoll's structure. Measurements of the water level in the hole showed that it rose and fell in perfect synchrony with the tides on the ocean side of the island despite the fact that the hole was cased continuously from the surface down to almost 2,000 feet. This "one-to-one correspondence, both in phase and in amplitude" showed that the entire limestone cap was "as permeable as a sponge."[323]

Later came the long investigation of the four-thousand foot sedimentary section of Eniwetok, which revealed a twist in the history of the atoll. Like Bikini, the limestone was all of shallow-water formation. But the sedimentary sections of Eniwetok and Bikini each contained limestone that had been exposed to rainwater for a long time associated with the shells of freshwater animals and the pollen and spores of a

321. Inserted in Ladd Scrapbook at p. 85.
322. Claude Coray to Jane Ladd, [postmark] 15 July 1952. Ladd Papers, SIA, RU 7396, box 2, folder 6, "Photographs, 1947-1961."
323. H.S. Ladd, et al., "Drilling on Eniwetok Atoll, Marshall Islands," 2261–62; Ladd and Schlanger, "Down to Earth on Eniwetok," 13.

tropical deciduous forest. There had been thousands of feet of cumulative subsidence in the histories of Bikini and Eniwetok, but they had also both spent considerable periods as high limestone islands.[324]

Yet for many observers, the coral reef problem was more or less solved. Emery's mentor, the submarine geologist Francis Shepard, late of Scripps and once an undergraduate student of Daly's, wrote to Ladd to congratulate him. "I suppose you have already been told by many people that this represents what is really an epic in geology. When I think of the controversy that has been raging for a hundred years relative to the Darwin and Daly, etc. hypotheses, it certainly is amazing to think that the problem has now been so satisfactorily settled, at least insofar as the general principle is concerned. The implications of these deep holes and their shallow water faunas are so widespread that it will take a long time before they are all properly recognized."[325]

Ladd himself described these implications in his 1960 Professional Paper on Eniwetok, which formed the twenty-fifth chapter of the still-open Bikini series. "The deep drilling on Eniwetok...firmly established the fact that the foundation of that atoll is a basaltic volcano that rises 2 miles above the floor of the ocean. It thus confirmed one of the most important features of Darwin's subsidence theory [...] The 14 feet of fractured olivine basalt obtained from the hole drilled on Parry Island apparently formed from a single flow [of lava]...When volcanic action ceased the summit of the basaltic mound probably projected far above sea level. The first sediments to be laid down were Eocene in age, and they contained reef corals that do not flourish below 150 feet."[326]

That Ladd considered Eniwetok to have confirmed a *feature* of Darwin's theory, however, did not mean that he believed it had confirmed Darwin's theory. Many people

324. Ladd and Schlanger, *Drilling Operations on Eniwetok Atoll*, 900.
325. Shepard to Ladd, 22 January 1954. Ladd Papers, SIA, RU 7396, box 1, folder 7, "Correspondence, 1925-1973."
326. Ladd and Schlanger, *Drilling Operations on Eniwetok Atoll*, 899.

believed as a result of Ladd's drilling program that "Darwin was right," but the man they took to have proved the point did not share their view on the matter. In 1960, the same year as Ladd's Professional Paper on Eniwetok appeared, *Science* ran a review of the early volumes by the UCLA physiographer William C. Putnam. Putnam praised PP-260 for "the detailed information it provides on the foundations of Bikini," and contrasted the current state of knowledge to the coral reef controversies before World War II, when "there appeared to be almost as many theories proposed as there were investigators in the field."[327] Ladd responded "I would qualify only one statement in your excellent review. Bikini did not have an origin 'virtually identical with that proposed in Darwin's theory.' It had Darwinian subsidence (many times over) but its history was much more complicated than Darwin anticipated."[328] In the early 1970s, when Ladd and Tracey were called to consult on another special weapons project at Eniwetok (by then officially known as Enewetak), they were repeatedly at pains to explain that "During its long history, Eniwetok subsided much more than the amount called for by Darwin but it is not a 'classic example of the Darwinian concept of atoll formation' because, so far as known, it did not pass through fringing and barrier reef stages."[329]

For many other observers, Darwin's theory had become the textbook solution to the coral reef problem. While Ladd and Tracey worried about what parts of Darwin's theory were supported by evidence from the Marshall Islands, their work was being cited far and wide in nonspecialist works as the decisive proof of Darwin's theory.

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327. William C. Putnam, "[Review of] Bikini and Nearby Atolls [Chapters A-W]," *Science* 131, no. 3409 (1305–7 1960).

328. Ladd to Bill [William C.] Putnam, 5 October 1960. Tracey Papers, SIA, Accession 02-021, box 1, folder, "Correspondence, General, 1959-1960."

329. Ladd and Tracey to Robert Henny (Air Force Weapons Laboratory, DEV Kirtland Air Force Base, Albuquerque, New Mexico), [1972]. Tracey Papers, SIA, Accession 02-021, box 4, folder "Eniwetok Active Corr. 74."

Conclusion

I have argued that debates over coral reef formation in the first half of the twentieth century were especially potent because participants had many distinct ways to study coral reefs. This chapter indicates that the familiar cabinet-field and laboratory-field dichotomies fail to capture the places and methods of studying coral reefs in this period. There was patently more than one type of field work, ranging from what Daly called the "monographic" studies of a single reef or archipelago to the motor car and steam ship based studies of landscapes that Daly and Davis pursued. And there was equally a variety of different cabinet- or indoor-based approaches that graded into fieldwork, as illustrated by Mayor's trajectory. Mayor began his career as a taxonomist who spent long periods collecting specimens on the other side of the world from his museum; he became a laboratory physiologist, albeit one whose laboratory was situated at a remote field site; and he became the preeminent experimentalist of coral reef formation via work at reefs from home waters to the Pacific.

Overlying all this was an exceptionally outspoken dialogue pitting geology against biology, with other disciplines playing smaller roles. It is noteworthy that the participants in the interdisciplinary coral reef disputes did study reefs in ways that might be taken as characteristic of their home disciplines as a whole. The most obvious example lies in Davis's preference for investigating the generalizable contours of the landscape over locally distinctive details, which he had made an important feature of the American science of geomorphology. (Daly and, to a lesser extent, Vaughan shared Davis's faith in the method of comparative study of topographic maps and hydrographic charts). More broadly, though, these methods embodied the heavily theory-oriented approach to scientific questions that, according to Naomi Oreskes among others, was a feature of late-nineteenth and early twentieth-century American earth science as a whole.[330] Likewise, Alfred Goldsborough Mayor's work in coral reefs exemplified important trends in the history of the life sciences in the early twentieth century. Mayor dealt with fieldwork in ways that suggest he shared many of his fellow biologists' epistemic fears (to use a term from Daston and Galison).[331] Mayor's importing of a laboratory to his field site at the Tortugas, his affinity for experimental investigations, and his use of the quantitative transect-quadrat approach to studying the distribution of reef organisms, were characteristic gestures of rigor and objectivity in a science that had formerly embraced observational and taxonomic studies.[332] His vivid statements about the rise of "modern biology" and his faintly oedipal obituary of Alexander Agassiz suggested that he felt ambivalent about his debts to, and major contributions to, the morphological wing of zoology against which so many of his contemporaries were revolting.[333]

It is important to note the vastly increased pace and frequency of coral reef field trips in this period as compared even to that of the last chapter. For some of the most enthusiastic exponents of fieldwork in the first half of the twentieth century, annual coral reef research expeditions were the norm. This had several important consequences for the participants and for the effort to write their history. Excursions to even the most remote reefs of the Pacific and Indian oceans no longer needed to be several-year affairs. For men like Mayer and Vaughan, these trips were not singular events of a lifetime. Unlike Darwin and Dana, whose subsidence theory they disputed, they did not spend five years accumulating observations, specimens, and theories before getting the

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330. Naomi Oreskes, *The Rejection of Continental Drift*; Naomi Oreskes, ed., *Plate Tectonics: An Insider's History of the Modern Theory of the Earth* (Boulder: Westview Press, 2003), xi-27.
331. Daston and Galison, *Objectivity*.
332. Kohler, *Landscapes & Labscapes : Exploring the Lab-Field Border in Biology*.
333. Garland Allen, *Life Science in the Twentieth Century* (New York: John Wiley & Sons, 1979); Keith Rodney Benson, Jane Maienschein, and Ronald Rainger, *The Expansion of American Biology* (New Brunswick: Rutgers University Press, 1991); Ronald Rainger, et al., *The American Development of Biology* (Philadelphia: University of Pennsylvania Press, 1988).

opportunity to publish their work. On the contrary, results could be written up on the steam passage home and in print within the year. Few of the expeditions, and even fewer of the reef theories that were based on them, were chronicled in book-length narratives or arguments. In the careers not only of Vaughan and Mayer, but also of their comparatively sessile colleagues, the alternating intervals of fieldwork and publication were short, and most of their publications were correspondingly short. These phenomena made it possible to study coral reefs in new ways. Longitudinal studies of organic and geological changes became commonplace, especially at Mayer's research station in the Tortugas. It also became normal to publish on research in progress, and to publish incremental changes of ideas based on another season of work.

Because many of the disputes over coral reef formation were related to the narrowness of individual researchers' expertise, it was clear to many participants that the solution to an inter-disciplinary problem must come through collaborative research. This idea characterized the work at Mayor's Tortugas lab and on the associated CIW expeditions, but it was equally relevant to the organization of a survey of Bikini Atoll. As much as anyone in American science in the 1940s, Roger Revelle had the pedigree and the personal inclination to bring multidisciplinary approaches to bear on scientific problems.[334]

I have argued that when practicing reef scientists were called to Bikini Atoll, the currency of the coral reef problem helped to ensure that a scientific "survey" of the atoll would include attention to the features that might settle this dispute. I am not arguing that Crossroads scientists acted in bad faith, or that they simply took advantage of the operation to do work irrelevant to its mission, any more than I am arguing that the

334. Ronald Rainger's work on the Scripps "school" of oceanography that crystallized in the 1930s is particularly relevant to this point. Ronald Rainger, "Adaptation and the Importance of Local Culture".

government funding at Bikini corrupted the scientists' ability to do basic research.[335] The key point was that when they were compelled to characterize the atoll based on their expertise in a particular field, they did so by applying the methods of their disciplines. In the case of reef studies, core drilling was widely understood to be the ultimate source of data about an atoll.

The regular press releases about this "newsworthy...science stor[y]" were actually stipulated in the 1947 Operation Plan as a means of "forestall[ing] much press criticism and speculation of a harmful nature." This publicity campaign almost certainly helped to encourage a widespread belief that research into coral reef formation had been stagnant from Darwin's time until the heroic achievements at Bikini and Eniwetok, whereas the fact that the core drilling was done at all owed at least in part to the fact that reef studies had flourished as the subject of multiple scientific disciplines in the first half of the twentieth century. Indeed, I have showed that Ladd and his colleagues formed part of a continuous tradition of reef scholars from Darwin and Alexander Agassiz to Vaughan, Davis, and Daly. The existence of an ongoing debate over various theories of atoll formation not only helps to explain why the issue was ever taken up during the Crossroads test, it also clarifies why Ladd was always at pains in subsequent years to specify that the Bikini and Eniwetok sections showed that the atolls had indeed undergone "Darwinian subsidence," but that they did not appear to have ever passed through the fringing reef and barrier reef stages that Darwin postulated. Ladd was demonstrably not a supporter of Darwin's theory before the war, and his unwillingness to let Eniwetok stand, even in non-specialist literature, as a proof of Darwin's general theory was consistent with his previous role as a champion of individualized

335. Within the wide literature on the coercion and exploitation associated with military patronage of science, discussions particularly relevant to this episode are Ronald Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s"; Weir, *An Ocean in Common*.

explanations for each reef.

The question of whether a theory was best characterized by its essential core or its latest full statement was brought up repeatedly by reef scientists in the first six decades of the twentieth century, and it is not clear that many of them had a consistent position on the issue. Davis was equally absorbed by the tasks of criticizing Mayor for misusing the genuine Darwinian taxonomy of reefs and of crediting the modern synthetic "subsidence theory" to Darwin. Daly reapeatedly argued that what mattered was whether evidence supported the essence of his own theory, but that essence changed a great deal depending on the nature of the criticism being leveled at it.

Along these lines, determining the full significance of the basalt cores from Eniwetok is indeed complicated. Well before Ladd had got his core of basement rock, the combination of studies at Bikini demonstrated that enormous subsidence had taken place there. It appears that only the most self-interested opponent of Darwin's theory entertained the idea that the 2,500 foot rampart of shallow water limestones at Bikini was fore-reef talus. Meanwhile seismic data and deep water dredging already offered considerable proof that somewhere beneath the limestone was the kind of primary rock that Darwin predicted would underlie an atoll at some depth. Whereas the atomic bomb only became a war-ending weapon in retrospect, then, it seems that by the time Ladd went to Eniwetok in 1952, the result was highly overdetermined. Any deep boring that brought up a sliver of basement rock was destined to be the "final proof" that Darwin was right, because there was already ample evidence that Darwin almost certainly was right that subsidence had occurred where deep ocean atolls were found. To be sure, bringing up an actual piece of volcanic rock from 4,000 feet beneath Eniwetok reinforced that evidence. It gave great credence to the seismic studies, in particular. But it was this evidence that was already available that helped to give meaning to the basalt core. The basalt was not really necessary to prove subsidence, though; the 4,000 feet of shallow water limestone on top of it could do that. But those 4,000 feet of limestone told an almost identical story to the 2,500 feet of the same material that had already been drilled at Bikini. The basalt was worth studying in its own right, certainly, and it would have mattered if the basement had not been volcanic at all.

When it came to settling the dispute between rival coral reef theories, it would appear that the basalt core was as convincing as it was not just because it was inherently more informative than any of the other types of evidence just mentioned. Its value was *historical*. Since before Ladd was born, all disputants had agreed in the premise that when it came to the ocean atolls, the deep bore was the crucial test between Darwin's theory and everything else. Without the crucial testimony of a deep bore, no evidence would have been truly satisfactory.

CONCLUSION

Who said that Darwin was right about coral reef formation? According to a pair of recent publications, Harry Ladd did. In her 2002 review of twentieth century coral reef research, the biologist Daphne Fautin reported that Ladd had marked the E-1 drill hole at Eniwetok with a sign that read, "Darwin was Right."[1] Sandra Herbert took up this story and used it to poetic effect at the end of her 2005 book on Darwin as a geologist.[2]

These words may be a memorable epitaph for Darwin's coral reef work, but it is difficult to imagine Ladd uttering them without serious qualification. The anecdote fits not at all with the perspective on the coral reef problem that Ladd expressed at many other times in his career. Not only was he a non-supporter of Darwin's theory as far back as his 1920s work on the Fijis, but he expressed tacit skepticism with the possibility of a generally-applicable theory of reef formation in his publications with Hoffmeister of the 1930s and 1940s. His cautious interpretation of the Bikini holes five years earlier suggests that he believed strongly in making full petrologic and micropaleontological analyses of cuttings and cores before drawing conclusions like the one written on the sign. There is also abundant documentary evidence, some of which I have already cited, that he testily corrected people who claimed that the Eniwetok boring had proved Darwin's theory, as opposed to saying that it had shown a lot of subsidence along with periods of emergence). Ladd adhered to a notion expressed at various times by Vaughan and Daly, that the essence of Darwin's theory was the claim that fringing reefs, barrier reefs, and atolls shared a

1. Fautin, "Beyond Darwin," 447.
2. Sandra Herbert, *Charles Darwin, Geologist,* 357.

genealogical relation, rather than the simple claim that atolls' foundations must have subsided. Could Ladd really have been so eager to proclaim Darwin right?

Fautin's source for the story of Ladd's plaque was personal communication with Joshua Tracey, whose carefully preserved papers at the Smithsonian Institution Archives provide remarkable insight into history of coral reef science in the twentieth century. While taking my first look at the Tracey collection in 2005, I came across a photograph showing a picture of a cased borehole and a small handmade sign showing a three-part diagram of the transformation of a fringing reef into an atoll and containing the words "Darwin was right."[3] But this sign was not made by Harry Ladd. The photo is labeled on the back "E-1, Parry I., Enewetak 12 December, 1976. Photo by John Wells" and according to a letter written by Wells to Ladd in January 1977, but presently located in a separate part of the Tracey collection, the plaque was drawn by Brian Rosen.[4] Rosen was a young coral specialist from the British Museum (Natural History) who eventually published a small reproduction of the same photograph in a 1982 article on Darwin's theory.[5] It is certainly possible that there were twin signs placed at the hole almost a quarter century apart. However, it seems at least equally plausible that the sign Tracey described to Fautin was the one made by Brian Rosen in 1976 and not one made by Ladd in 1952.[6] Even if the situation remains unclear, the story still highlights a couple of factors worth mentioning as I draw this dissertation to a close.

The possibility that Ladd placed a "Darwin was right" sign at the Eniwetok bore

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3. Tracey Papers, box 4, folder "T.W.E. David Research, 1951-1996."
4. John [Wells] to Harry Ladd, 13 January 1977. Tracey papers, box 1, folder "Correspondence - Enewetak Samples, 1972-1977.
5. Brian Roy Rosen, "Darwin, Coral Reefs, and Global Geology," *BioScience* 32 (June 1982): 519.
6. In addition to noting the fact that Tracey had a photograph of the Rosen sign, it is worth reiterating that Tracey was not at Eniwetok for the 1952 drilling. I met Rosen, who is still at the Natural History Museum, in 2006. He was not aware of the story about Ladd's sign until I asked him about it, and therefore said that he had not drawn his sign in homage to an earlier sign. Tracey and Ladd are both deceased and I have not yet asked Fautin about this story.

hole rests easily with the historical literature on reef science as a whole, in which much more labor has been devoted to establishing Darwin's perceptiveness than to understanding why his theory was brought to a test on that particular atoll in 1952. One of the main objectives of my dissertation has been to bridge the nearly-empty space in the secondary material between Funafuti and the story of Eniwetok, and thereby to try to understand the Eniwetok study from Ladd's perspective and not just Darwin's.[7] By following the constantly active practice of reef study, this dissertation uncovers reasons why anyone was still interested in whether Darwin was right in 1952, and goes a long way toward explaining why anyone thought drilling an atoll would make a good component of a weapons test. To sum up these arguments from chapter five, there were scientists interested in whether Darwin was right in 1952 because the theory had never ceased to be one of the conceptual resources employed by the many individuals who carried on researching and debating the formation of reefs. The drilling at Bikini was stimulated by the fact that Ladd already believed (like most of his colleagues) that a Pacific atoll should be drilled, and the operations headed by Revelle at Bikini provided a venue and a patron for such an expensive undertaking. Ladd drilled Eniwetok, in turn, because his intellectual motivation to do so remained strong and because the comparison of seismic and magnetic data with deep bore samples had proved to be so useful at Bikini that there was good reason for the Armed Forces Special Weapons Project to fund the boring for its own purposes.

Brian Rosen's hole-side placard, meanwhile, is significant emblem of how the

7. Fautin's article shows that she herself has taken some interest in filling this void, although not for the reason I have expressed. Her paper argues that the many types of field studies undertaken at coral reefs in the twentieth century have helped to turn reef science into an "identifiable scholarly endeavor." She means identifiable in the sense that in recent years some scientists have identified themselves as reef scientists, and scholarly in contrast to nineteenth century studies "on the formation of atolls [...] based on inference rather than on the extensive empirical research that has come to characterize reef science." Fautin, "Beyond Darwin," 449, 446.

“coral reef problem” was resolved. This sign exemplified a different view of Darwin’s theory from the one held by Vaughan and Ladd. For those who had been in the fray of the old debates over the “coral reef problem,” the fact that Eniwetok’s foundation had subsided did not by any means prove that Darwin's global theory of coral reef formation was generally correct. Eniwetok did not suggest a new explanation for the origin of Vaughan's Florida reef tract, but it did suggest that studying the Florida reef tract would no longer be a plausible avenue, as it had been for Vaughan, to understanding the formation of reefs like Eniwetok’s. The boring helped to prove once and for all to the coral reef theorists, then, that no single theory would explain the origin of all reefs. What Rosen’s sign exemplified, on the other hand, was what emerged as the textbook explanation of atoll formation. The question of how shallow-water organisms might build a ring-shaped island in the deepest ocean evidently remained a compelling, almost compulsory question for textbook writers in geology, biology, and oceanography. The Eniwetok boring had proved that shallow water organisms built at least one annular reef atop a foundation that had done a lot of sinking. Darwin’s theory *had* been the theory to suggest such a thing, regardless of what else it said, and his suggestion had long been represented (much to the annoyance of R.A. Daly) by the iconic sectional views of fringing reefs, barrier reefs, and atolls. By this form of consensus, Eniwetok had proved that “Darwin was right,” even though Rosen’s “textbook” diagram showed depicted exactly the developmental sequence that Ladd would forever identify with Darwin’s theory, and forever deny had been proved at Eniwetok.[8] The Eniwetok drilling certainly did not put a stop to research into coral reefs’ origins. It also did not generate universal agreement about the accuracy of Darwin’s theory. And, because it showed that not all reefs could be veneers on an antecedent platform but had long and evidently very com-

8. See, e.g., Harry S. Ladd, “Bikini and Eniwetok Atolls, Marshall Islands,” in *Biology and Geology of Coral Reefs*, eds O. A. Jones and R. Endean (New York,: Academic Press, 1973), vol. 1, 93–112.

plicated histories dating well before the Pleistocene, Eniwetok was the burying ground for the two-hundred year old precept that a generally-applicable theory would be the likely outcome of research into coral reef formation.

The enterprise of drilling Eniwetok was not qualitatively different from many other undertakings into the study of coral reefs. Ever since the 1830s, if not earlier, one of the chief features of research into reef formation has been the direct test of a theory. From Beaufort's instruction to FitzRoy to evaluate the "modern and very plausible [crater-rim] theory" by sounding for the maximum depth of coral growth, to Mayor's experiments on solution of limestone, to the several boring attempts, theories did not merely guide the accumulation of knowledge. They often dictated what counted as the accumulation of knowledge. Alongside the observation that particular theories had particular implications for how to carry out surveys of coral islands (and which islands to survey), this suggests that theories were not just explanations, they were also tools for deciding what to do. This practice not just of theory-laden seeing, but of theory-laden choices about what to try to see, was nowhere more explicit than when it was written down as official doctrine in Herschel's (and Beaufort's) Admiralty *Manual of Scientific Inquiry*. There Darwin and his fellow scientific specialists instructed navy men how to go about studying nature with advice like "On the shores of every kind of reef, especially of atolls and of land encircled by barrier reefs, evidence of the slow sinking of the land should be particularly sought for."[9]

The role of scientific disciplines like geology and biology in this history, and of coral reef study in the history of individual disciplines, are matters too complex for me to wrap up in a neat way. It might seem that I have uncovered an utterly predictable story of change over time: the sciences of natural philosophy and natural history were

9. Reprinted in Paul H. Barrett, *Collected Papers*, vol. 1, 246–47.

divided and subdivided into modern disciplines; as knowledge of coral reefs increased, individuals were forced to become specialists. But this does not explain all the features of the coral reef debate. It does not, for example, help us to understand many individuals' choice of the coral reef problem as a research topic. To my mind, one of the most counterintuitive features of the tradition of inquiry that I have studied in this dissertation is the near invisibility of a mechanism by which new participants were consistently initiated. Yet the tradition remained so vigorous for so many consecutive generations of scholars that it would be tempting to call this a genealogical study if it weren't so difficult to identify any discernable parentage or lineage from participants of one generation to those of the next.[10] This may help to explain why disciplines were often invoked to express preferences in temporal and material subject matters moreso than philosophies of science. Geologists studied the past and rocks. Zoologists studied living corals. In this respect references to discipline were actually often claims about the way reefs were made, not about research methods per se.

10. In this respect, what I have called the "tradition of inquiry" into coral reef formation resembles what Lynn Nyhart calls an "orientation," which "encompass[es] an area of study—primarily related to research but not excluding issues of teaching—the group of pople engaged in that area, and the philosophical attitudes accompanying the cluster of problems they are working on. It is similar to 'school' used loosely; the problem with the word 'school' is that it is also used narrowly, to mean a teacher and his students. [...] An orientation can be generated by a school but also includes people who take up a given set of problems and attitudes whether or not they had the same teacher." Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900*, in *Science and Its Conceptual Foundations* (Chicago: University of Chicago Press, 1995), 4.

MANUSCRIPT COLLECTIONS CONSULTED

BL	British Library (microfilm).
CIW	Carnegie Institution of Washington.
CUL	Cambridge University Library. DAR Darwin papers.
DH	Down House, Kent.
EUL	Edinburgh University Library.
GEFCO	Geo. E. Failing Supply Company. Company records sent by mail.
GS	Geological Society of London.
HU-GH	Gray Herbarium, Harvard University.
HU-H	Houghton Library, Harvard University.
HU-P	Pusey Library, Harvard University.
MCZ	Museum of Comparative Zoology, Harvard University.
NARA II	National Archives and Records Administration, College Park, MD.
NHM	Natural History Museum, London.
OUL	Oxford University Bodleian Library.
OUM	Oxford University Museum.
PUL	Princeton University.
RS	Royal Society. HS Herschel papers.
SIA	Smithsonian Institution Archive.
SIO	Scripps Institution of Oceanography (photocopies and online).
SMB	Science Museum, Boston (Boston Society of Natural History).
SU	Syracuse University.
UCL	University College London.
UKHO	UK Hydrographic Office, Taunton.
UT	University of Texas.

HRC Harry Ransom Humanities Research Center.

WHOI Woods Hole Institution of Oceanography.

YUL Yale University Library (microfilm).

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447 Figure 6.1: Brian Rosen's sign at Eniwetok Atoll. J.I. Tracey, Jr. papers. Accession 02-021, box 1, folder "T.W.E. David Research, 1951-1996." SIA.

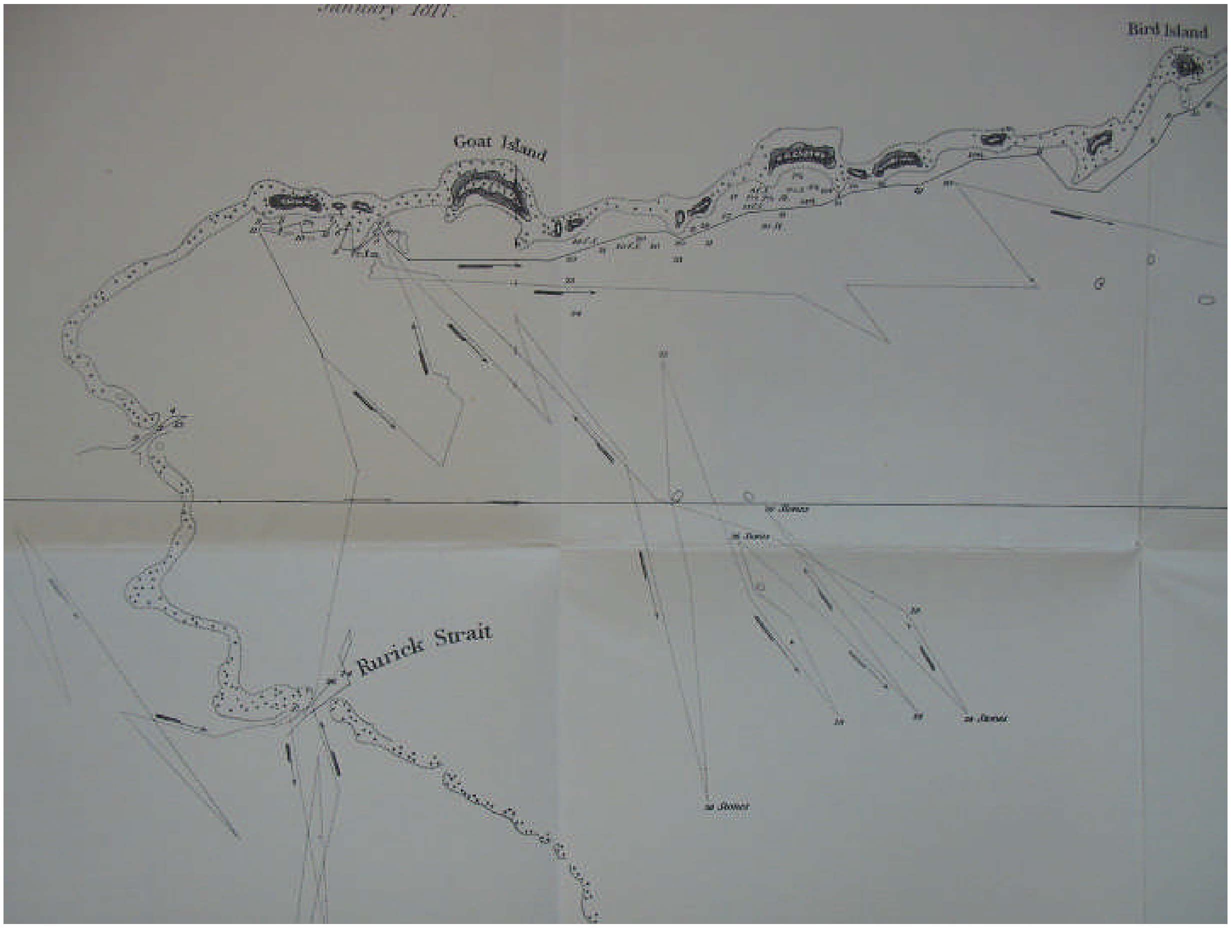

Figure 1.1 and 1.2 (next page): Kotzebue's track at Otdia (detail). The systematic northward, eastward and then southeastward movement helps to indicate why Kotzebue and company only gradually realized that the reef was annular.

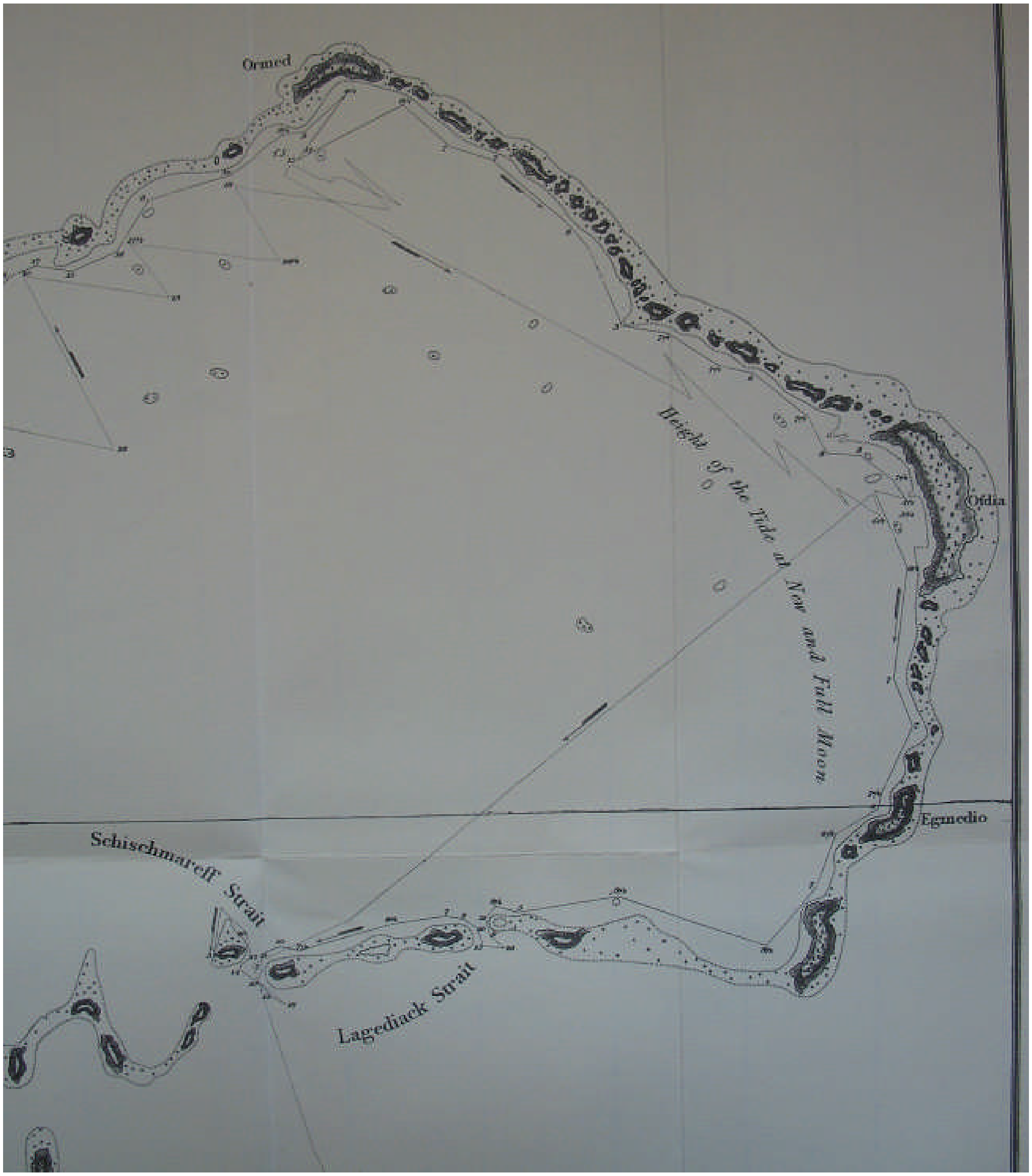
Ormed
Height of the Tide at New and Full Moon
Otdia
Egmedio
Schischmareff Strait
Lagediack Strait

Figure 1.2

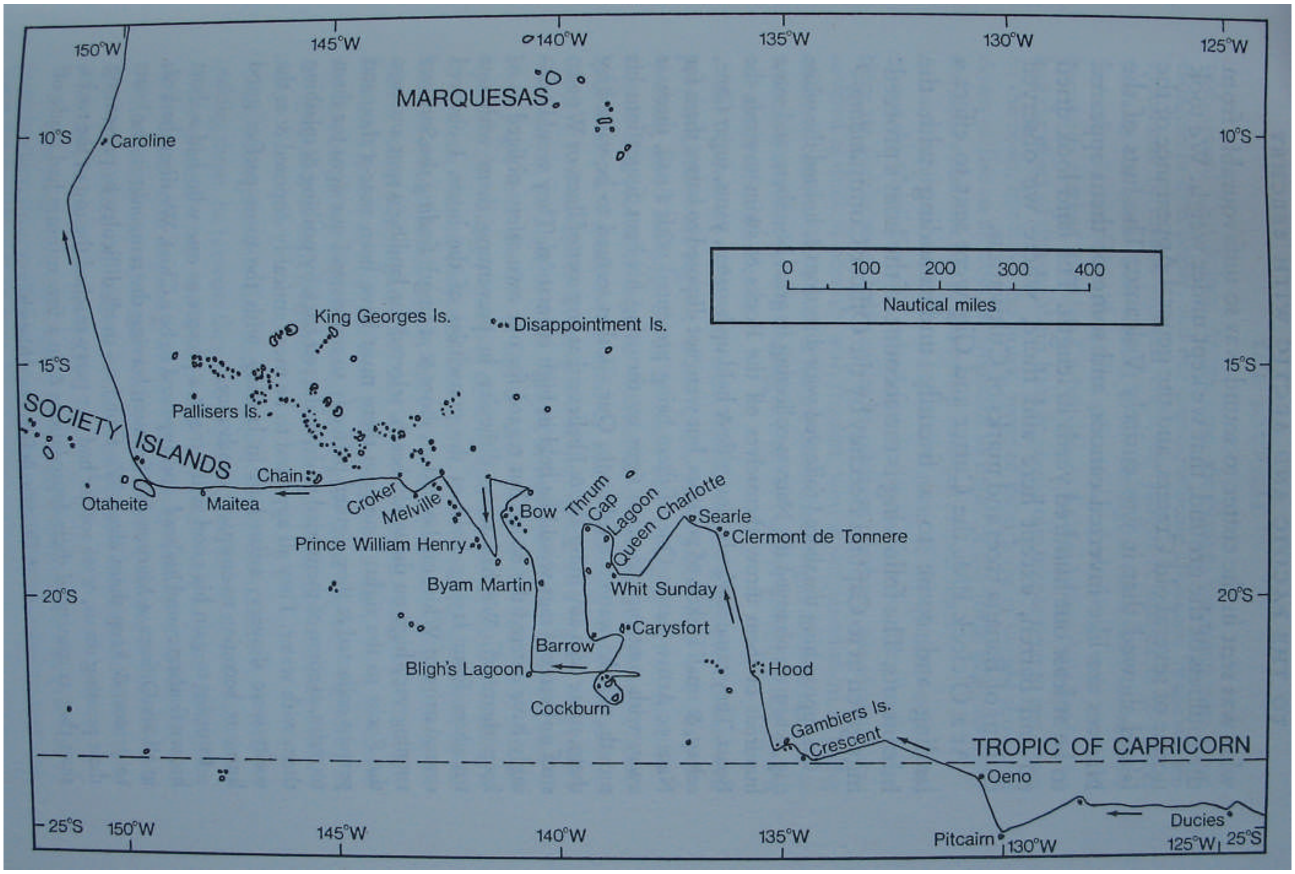

Figure 1.3: Modern map of Beechey's track in the *Blossom* showing his visits to the islands of the Low Archipelago.

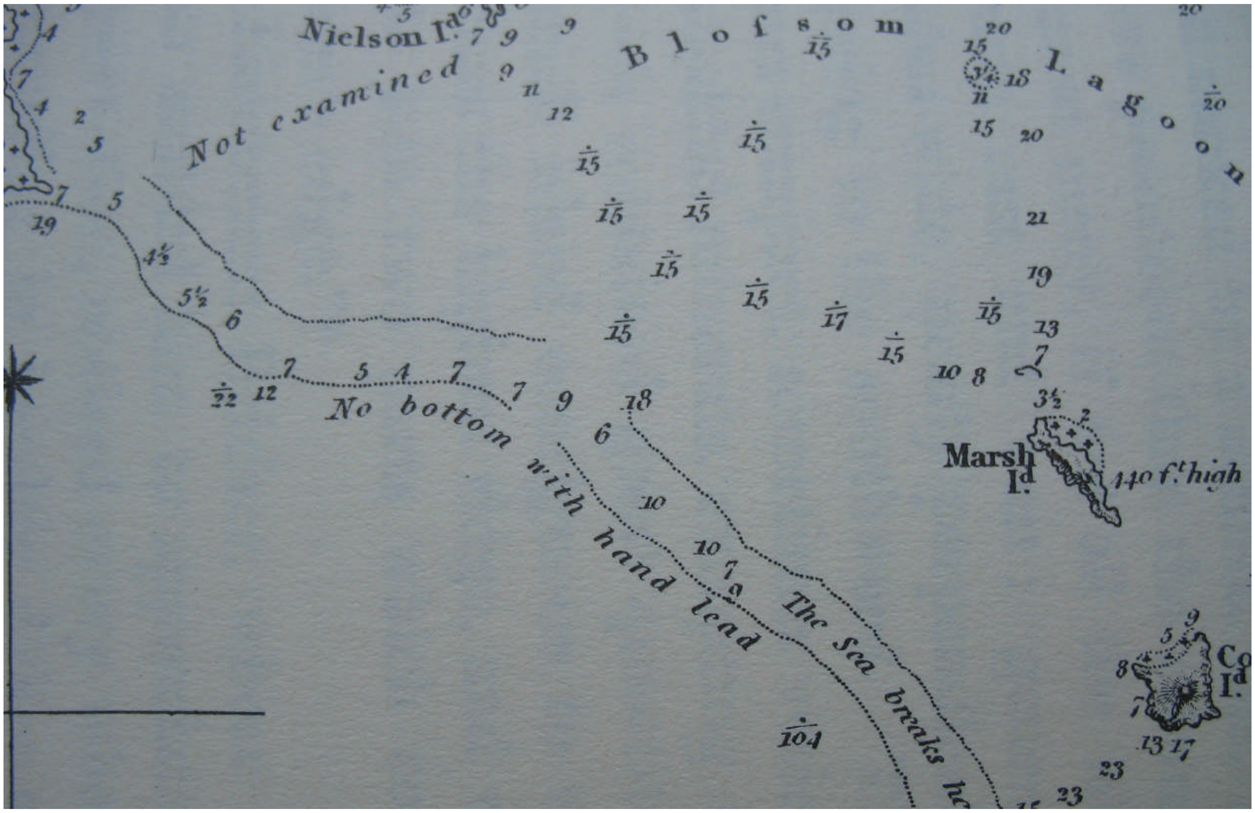

Figure 1.4: Beechey's survey of the Gambier Islands indicating the work close in to the reefs (detail).

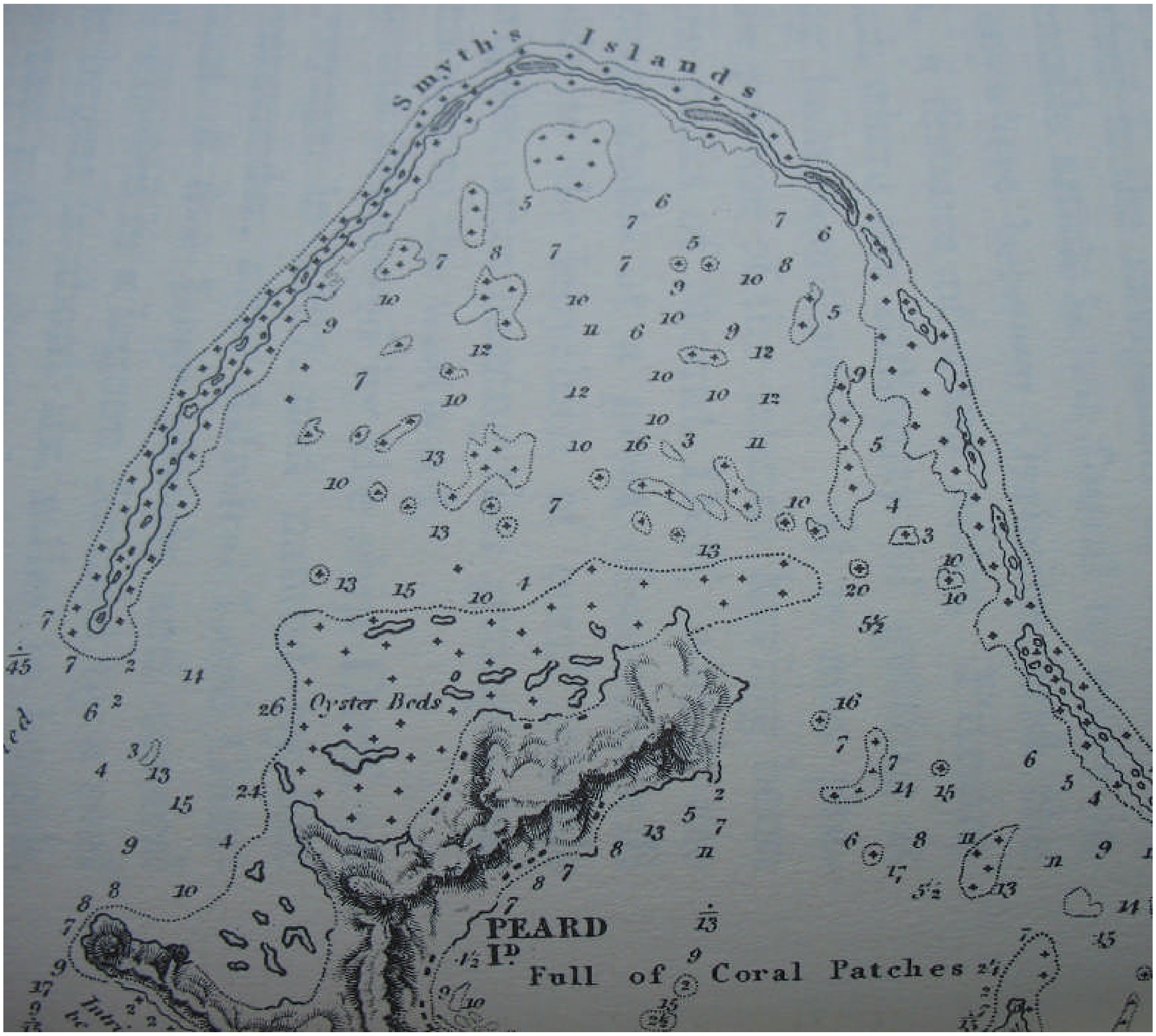

Figure 1.5: Beechey's survey of the Gambier Islands (detail).

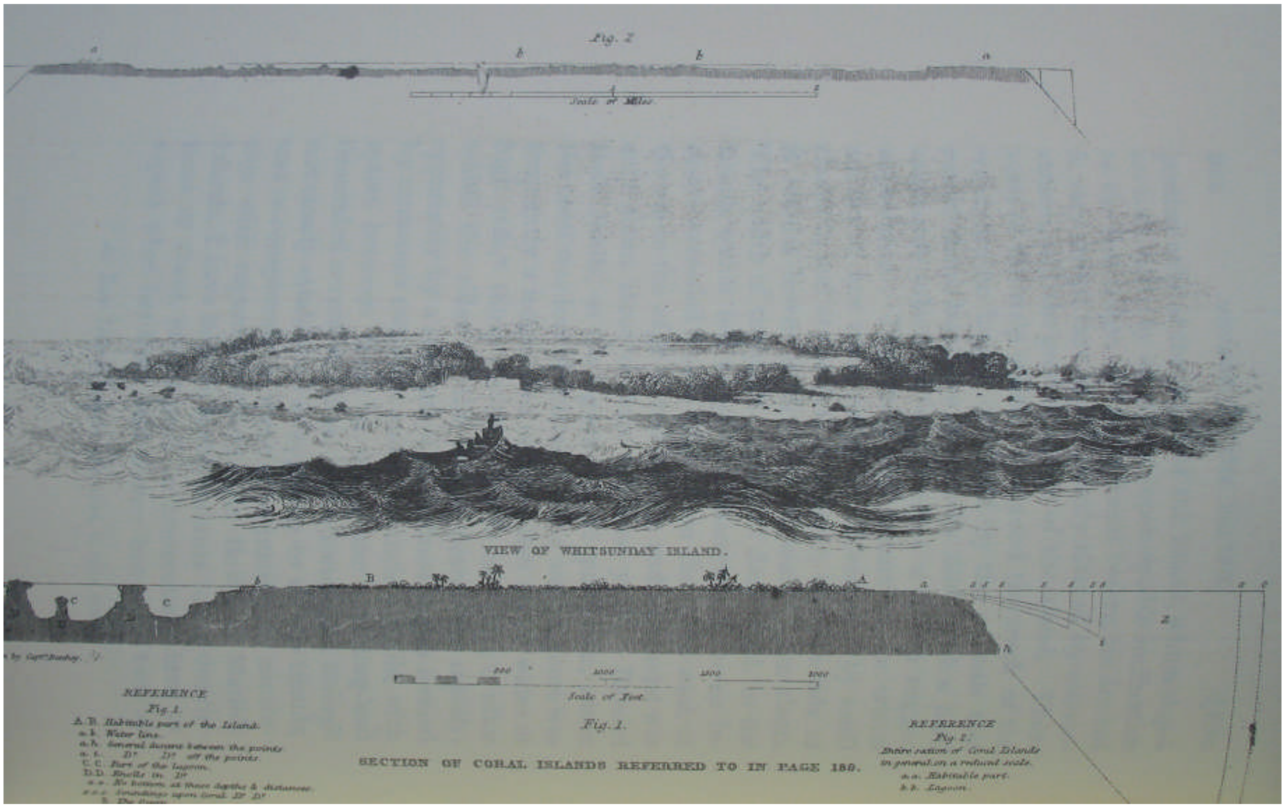

Figure 1.6: Beechey's profile and view of Whitsunday Island, which was incorporated (with modifications) into publications by Lyell and Darwin.

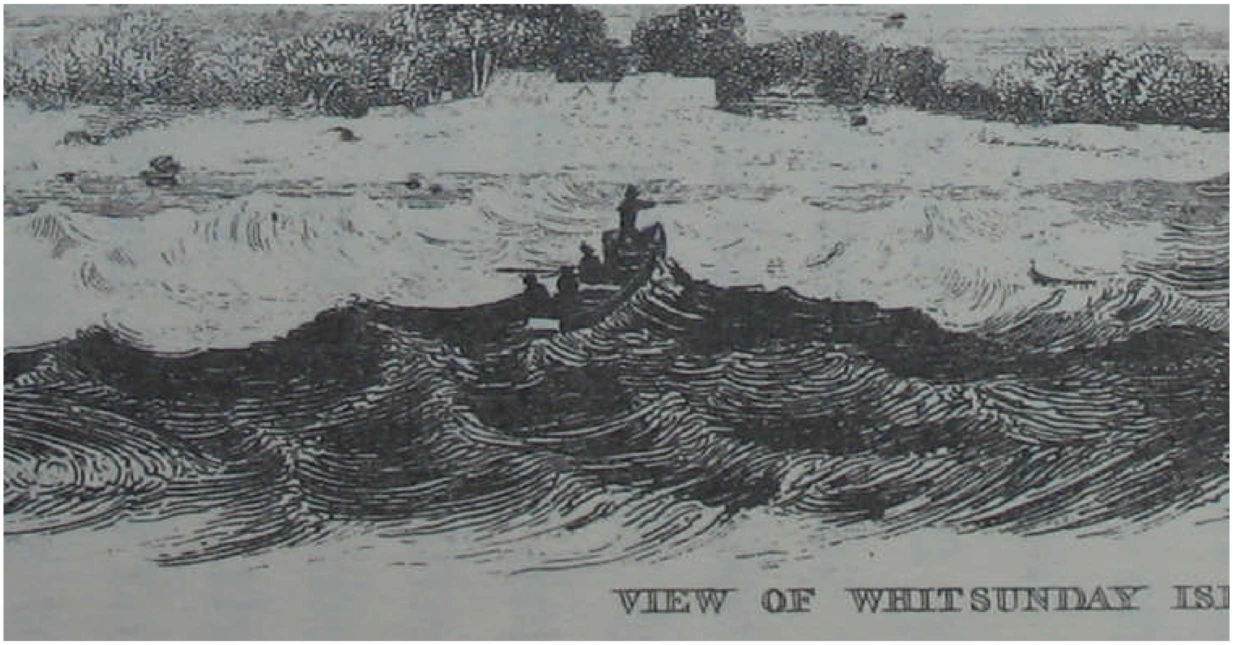

Figure 1.7: How the survey was made. Sounding with the hand lead outside the reef. Detail of Beechey's profile of Whitsunday Island (detail of Figure 1.6).

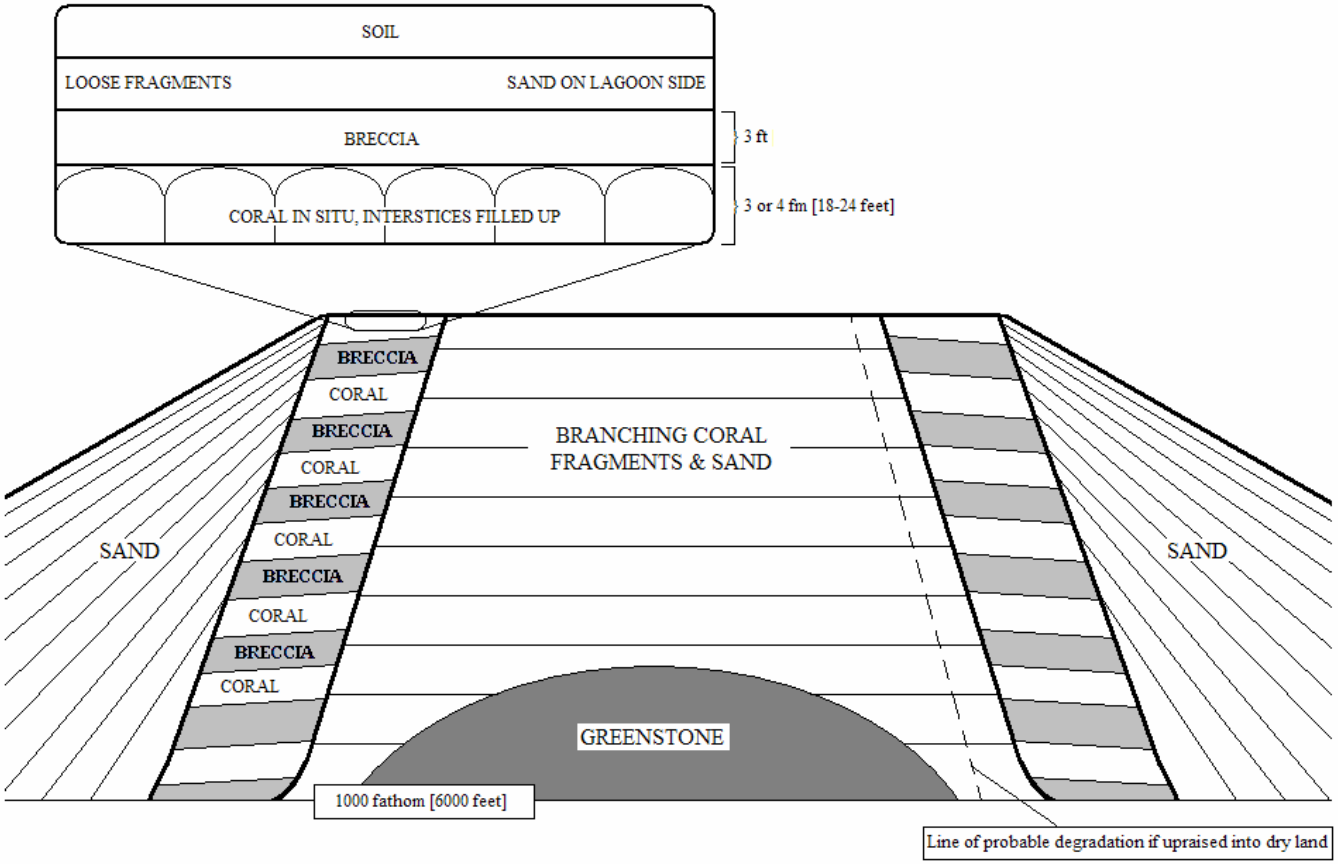

Figure 2.1: Diagram combining Darwin's section of the surface structure of Keeling Atoll (boxed) and his conjectured section of the submarine structure.

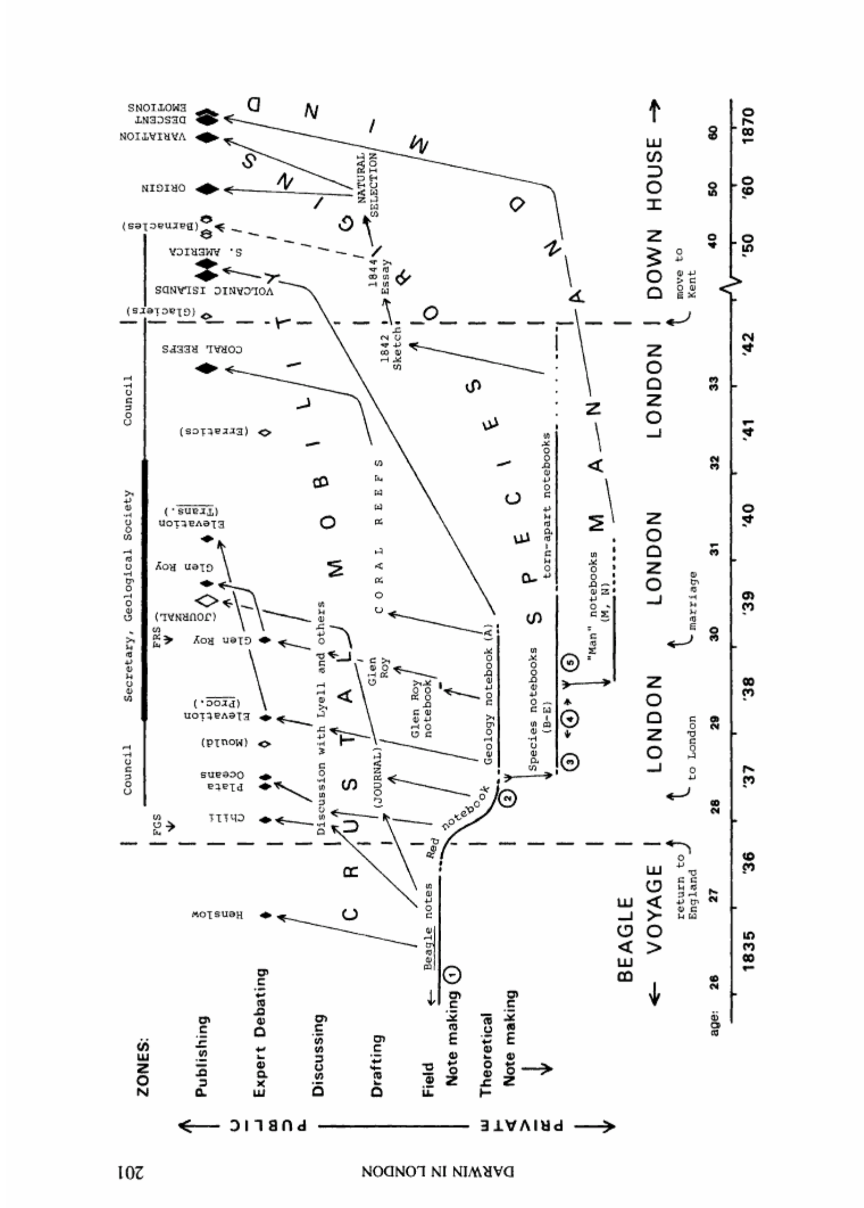

Figure 3.1: Rudwick's "Scale of Relative Privacy."

Rudwick's caption reads: The temporal development of Charles Darwin's three cognitive enterprises (crustal mobility, species origins, Man and Mind) and his London years, plotted against the scale of relative privacy. Notebooks are shown as thick lines (dashes are uncertainties of dating), papers as small black diamonds, books as larger black diamonds; circled points 1 to 5 refer to episodes of theorizing about species. Only the last part of Darwin's Beagle period is shown; for his years at Down House the time-scale is compressed and only a few publications are shown. Tailed arrows show filiation of theoretical notebooks; all other arrows indicate general direction of projects through the scale of relative privacy towards publication. Note that these arrows are schematic: they connect dated documents by the simplest lines and should not be taken to indicate dated transitions through specific zones en route, or that all projects passed through all zones. Abbreviations for drafts, articles, and books are identified in the footnotes; those not closely related to any of Darwin's major enterprises are marked by open diamonds and named within brackets.

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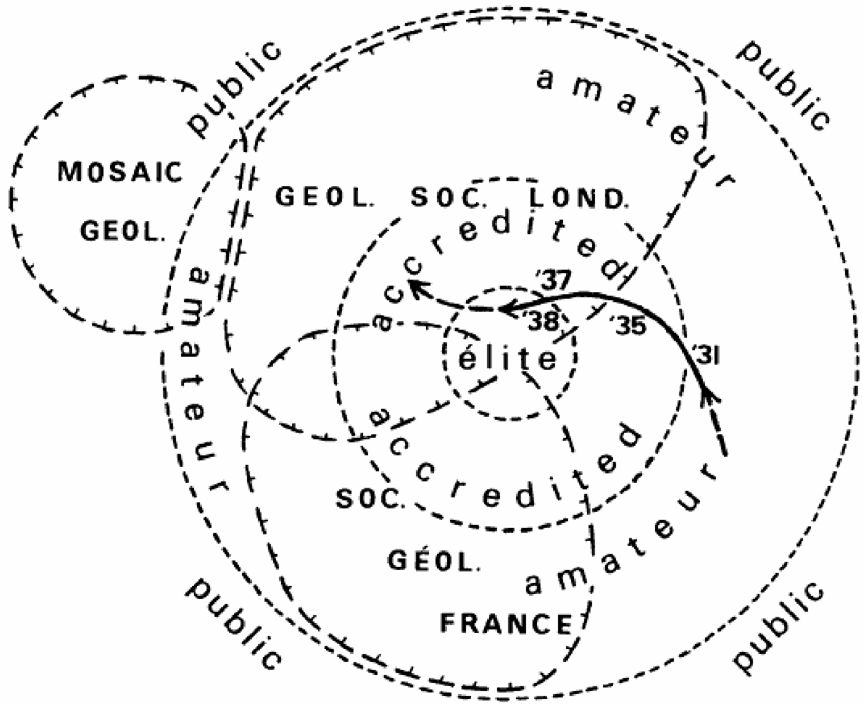
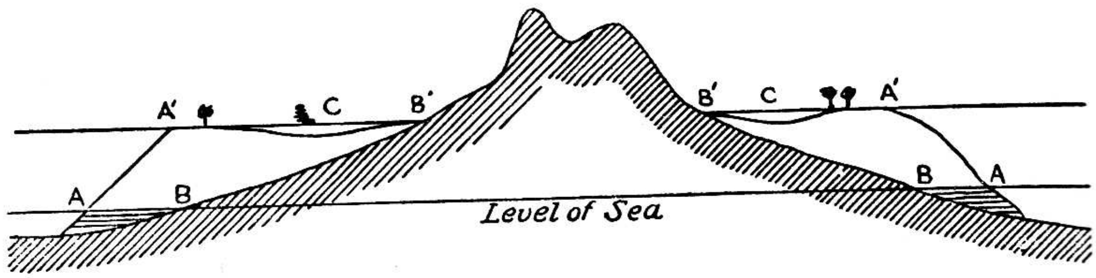


Figure 1. *The social and cognitive topography of geology in the 1830s (drawn as a Venn diagram), showing the three weak-boundaried concentric zones of ascribed competence (elite, accredited, amateur) surrounded by the general public; the two (strong-boundaried) major geological societies with overlapping membership, and the marginal Mosaic geologists; and Charles Darwin's trajectory (thick line) from amateur to elite status during the 1830s and his later reversion to the accredited zone. The relative areas of the various envelopes give a rough qualitative impression of the relative numbers of individuals populating those areas.*

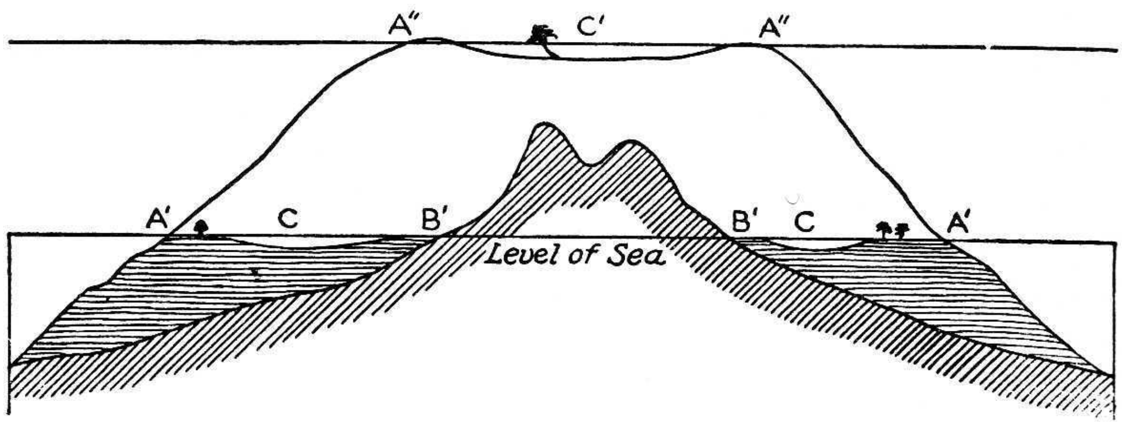
Figure 3.2: Rudwick's topographic map of British geology.



AA. Outer edges of the fringing-reef, at the level of the sea. BB. The shores of the fringed island.

A′A′. Outer edges of the reef, after its upward growth during a period of subsidence, now converted into a barrier, with islets on it B′B′. The shores of the now encircled island. CC. Lagoon-channel.

N. B.—In this and the following woodcut, the subsidence of the land could be represented only by an apparent rise in the level of the sea.



A′A′. Outer edges of the barrier-reef at the level of the sea, with islets on it. B′B′. The shores of the included island. CC. The lagoon-channel.

A″A″. Outer edges of the reef, now converted into an atoll. C′. The lagoon of the new atoll.

N. B.—According to the true scale, the depths of the lagoon-channel and lagoon are much exaggerated.

Figures 3.3 and 3.4: Sectional diagram showing the proposed transition from fringing reef to barrier reef by the action of coral growth during subsidence (above) and from barrier reef to atoll (below). In each case the diagrammed barrier reef stage drawn from a true survey of Bolabola. From Darwin's *Structure and Distribution of Coral Reefs* (1842).

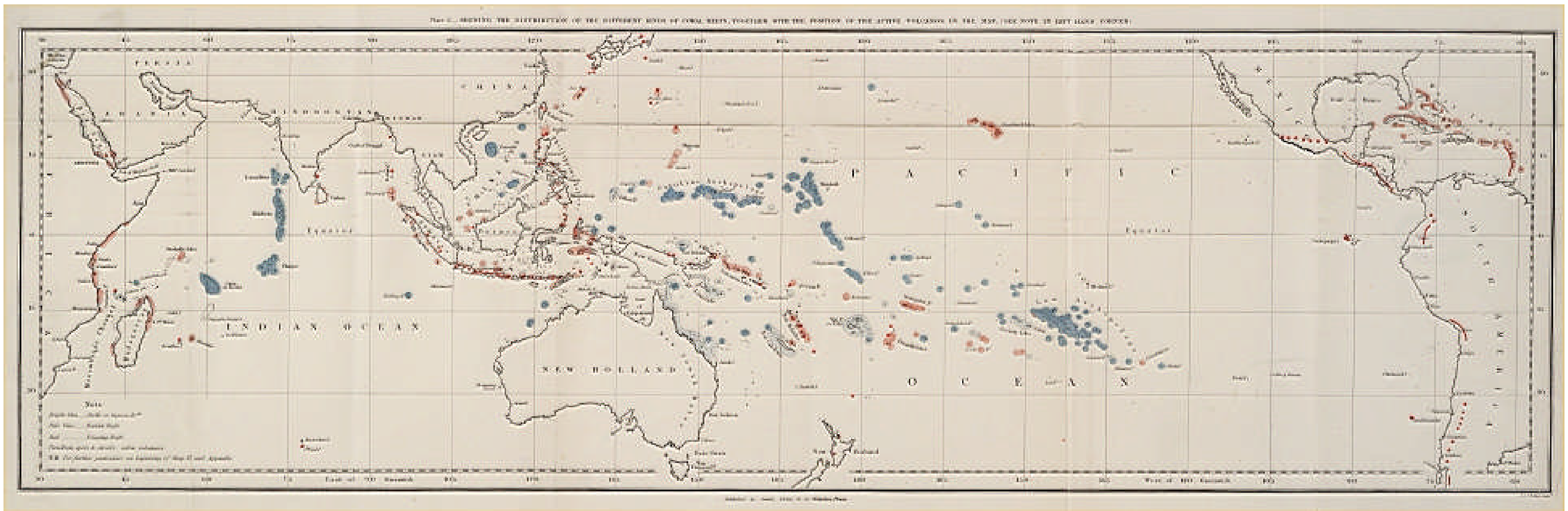


Figure 3.5: Chart showing distribution and classification of coral reefs and distribution of active volcanoes. From Darwin's *Structure and Distribution of Coral Reefs* (1842).

Figure 5.1: The Failing 1500 Holemaster drill rig at Bikini Atoll, 1947. L.P. Schultz papers, RU 7222, box 26, folder 4. SIA.

Figure 5.2: Roger Revelle (left) and Harry Ladd (holding sieve) at Bikini Atoll, 1947. L.P. Schultz papers, RU 7222, box 26, folder 4. SIA.

Figure 6.1: Brian Rosen's sign at Eniwetok Atoll. J.I. Tracey, Jr. papers. Accession 02-021, box 1, folder "T.W.E. David Research, 1951-1996." SIA.