Title

Dredging Evolutionary Theory : the emergence of the deep sea as a transatlantic site for evolution, 1853-1876

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Dredging Evolutionary Theory:
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in

History (Science Studies)

by

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2014

The dissertation of Rodolfo John Alaniz is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Co-Chair

Chair

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2013

DEDICATION

This dissertation is dedicated to the late Philip F. Rehbock.

We never met in person. I started my research after your passing.

However, my colleagues speak of you with great fondness and admiration.

Your writing has inspired me

and convinced me that our words touch the lives of others

long after we have passed on.

EPIGRAPH

Organic life beneath the shoreless waves
was born and nurs'd in ocean's pearly caves
First forms minute, unseen by spheric glass
move on the mud, or pierce the watery mass
As these successive generations bloom,
new powers acquire, and larger limbs assume;
whence countless groups of vegetation spring,
and breathing realms of fin, and feet, and wing

- Erasmus Darwin, The *Temple of Nature* 1803[1]

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[1] Erasmus Darwin, *The Temple of Nature* (1803; reprint, Menston, Yorkshire: The Scholar Press Limited, 1973), 26-27. The original footnote reads, "*Beneath the shoreless waves*, l. 295. The earth was originally covered with water, as appears from some of its highest mountains, consisting of shells cemented together by a solution of part of them... It must be therefore concluded, that animal life began beneath the sea."

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LIST OF ABBREVIATIONS

BAAS	British Association for the Advancement of Science
BLMS	British Library Manuscripts
BLSC	Bodleian Library Special Collections
CDCR	Circumnavigation Dredging Committee of the Royal Society of London
CULA	Cambridge University Library Archives
DARC	Darwin Correspondence
MCZA	Museum of Comparative Zoology Archives
NABA	National Archives Building Annex
SIOA	Scripps Institution of Oceanography Archives

ACKNOWLEDGEMENTS

Writing this dissertation has been a labor of love. I have learned much from the process. That being said, the experience would have been worthwhile even if I learned only a portion of the eloquence needed to thank those who made it all possible. I stand deeply indebted to a great number of friends and colleagues. Each of these people is responsible for the lessons I gained and the joy I felt while writing this dissertation.

There is no better place to start my thanks than at the beginning. The truth is that this project started as a friendly jab at a colleague who I respect a great deal, Phil Clements. As member of my graduate cohort at the University of California, Phil began a study on the “geographies of science.” His historical actors researched science at the highest point on Earth, Mount Everest. At the beginning, I was deeply skeptical of his chosen historical lens. Yet, Phil continually trounced my objections with clarity and narrative flare. Like any self-respecting doctoral student, I couldn't let a friend get away with such a clean thesis. I could think no better way to express my skepticism than to counter his study with one on the lowest point on Earth, the deep-sea floor.

I use this story as a way to show my appreciation of my fellow graduate students at the University of California, San Diego, especially Phil Clements, Amanda Bevers, and Nick Saenz. Phil unwittingly introduced me to my dissertation topic. Amanda lent me the smile – and hot cocoa – I needed to weather difficult times. In Nick I found an patient editor, a political co-conspirator, and a lifelong friend. I came to the University of California because I loved the intellectual community I found there. I know I made the right choice because of them.

My next thanks goes to those people I have met in my travels. My trek has been made enjoyable and enlightening by a number of people, though I cannot name here all that have helped me along the way. To the archivists at these locations, thank you for helping me and – in some cases – listening to me rant about deep-sea creatures. I am especially grateful to the University of Leeds for hosting my “British invasion.” I owe Graeme Gooday for his hospitality and delightful witticism. Jon Topham and Jon Hodge gave me valuable insights into the most difficult portions of this dissertation. It is not easy to show such brilliant men your weakest work, but they proved to be paragons of collegiality and helpfulness. Claire Jones insisted – to my great benefit – that my stay in England be fun as well as informative. Thank y'all for making England my home.

I also found a delightful community within the history of oceanography. I am especially grateful to have met Helen Rozwadowski and the other Halifax conference-goers. It has come to my attention that not many people study the history of deep-sea biology. I am ecstatic that Helen, as a fellow in that subject, is such an encouraging and friendly colleague. The other conference attendees also shared their knowledge and resources freely. Two of them even sang sea-chanteys with me in front of a room filled with admirable scholars. Thank you both for not running away when the music started.

This dissertation is partially about patronage and institutional indebtedness. With this in mind, I am keenly aware of how important their support is for scholars like myself. I would like to thank the Science Studies Program and the History Department at the University of California, San Diego, for their generous financial and organizational support. The University of California has also given me an added honor in the form of a UC President's Dissertation Fellowship. The Parents and Friends of Gays and Lesbians has also supported my passion for curriculum development through the Mary Wagner Memorial Scholarship.

Two historians of evolution stand out more than any others as influences in my scholarship. I met Mark Ulett at a San Diego history and philosophy of biology conference. He has been a fast friend and colleague ever since. In addition, we both had the same two ideas for dissertation topics when we began writing. He has done me the added service of choosing the exciting dissertation topic that I would have otherwise studied! My mind would have dwelt continually upon the subject if he had not pursued that worthy project.

The other historian of evolution I mentioned is Lynn Nyhart. In this instance, my desire to express my thanks incalculably exceeds my ability as a writer. She is responsible for my first belief that I could be a graduate student, let alone a historian of science. She took a scrappy geneticist and introduced him to his greatest intellectual joy. Thank you for being the type of mentor who walks through gardens and has philosophical conversations with me. Honestly, none of this dissertation would have been possible without her guidance.

I also owe thanks to the members of my doctoral committee. I chose them all because they are the type of scholars – and people – I wanted to be like when I finished. I cannot claim to have achieved that yet, but I will continue with the wisdom given to me by each of them.

I am particularly indebted to my co-advisors, Cathy Gere and Tal Golan. Cathy has been my patient, generous, and unswerving beacon throughout this process. She has alternatively lent a kind smile and – on rare occasion – tough love, which is exactly the “blend of method” I needed to become the scholar I am today. I have learned much from her astounding abilities as a writer and scholar. I attribute any bright and intriguing parts of my dissertation to her.

Tal has been a constant source of feedback and advocacy since I arrived at the university. He poured hours upon hours of his time turning someone who could not string words together into a competent historian. I owe my writing ability, and therefore my capacity to share my ideas with others, to his tutelage. Therefore, I attribute any confusing or ill-written parts of my dissertation to him. I jest, of course. I can attribute only the opposite.

My final thanks goes to my family: my mother, brother, dad, and grandparents. All of you gave up so much so I could have the educational opportunities I have enjoyed. I will always remember what you have given me. Last, but not least, I must thank the man in my life, Kyle, for his love and support. Thank you for being my muse.

CURRICULUM VITA

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Ph.D., History (Science Studies), University of California, San Diego 2014.
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M.A., History, University of California, San Diego, 2012.

B.S., Genetics, University of Wisconsin, Madison, 2006.

FELLOWSHIPS AND GRANTS

University of California President's Dissertation Year Fellowship, Univ of California, 2013

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Center for the Humanities Interdisciplinary Grant, Univ of California, San Diego 2013

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TEACHING EXPERIENCE

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Teaching Assistant, University of California, San Diego, History Department, 2009-2010.

Program Assistant, University of Wisconsin – Madison, Wisconsin Program for Scientific Teaching, 2007-2008.

HONORS AND TEACHING AWARDS

The College Classroom Scholar, Center for Teaching Development, University of California, San Diego

Undergraduate History Journal Editor for Phi Alpha Theta History Honors Society at the University of California, San Diego, 2013

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Head Teaching Assistant for History Department at the University of California, San Diego, 2012.

Lead Teaching Assistant for the Culture, Art, and Technology Writing Program at the University of California, San Diego, 2011.

National Academies Fellow in Science Education for research and administration of a science education Summer Institute for Scientific Teaching, 2008.

Howard Hughes Medical Institute Fellow in Classroom Teaching for outstanding assistant teaching in a graduate-level science education pedagogical course, 2008.

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PUBLICATIONS

Alaniz, Rodolfo John. “Diversity in the History of Science Profession: Recent doctoral recipient statistics.” *Newsletter of the History of Science Society* 43, January 2014.

Alaniz, Rodolfo John. “All the Fish in the Sea.” Review of *All the Fish in the Sea: Maximum Sustainable Yield and the Failure of Fisheries Management*, by Carmel Finley. *Journal for the History of Biology* 46 (2013): 323-325*.*

WORKSHOPS AND INVITED PRESENTATIONS

“Marine Dredging, Deep Sea Crinoids, and Wyville Thomson's Late-Nineteenth-Century Rejection of Darwinian Natural Selection.” Paper presented at the History of Science Society annual meeting, Boston, Massachusetts, November 21-24, 2013.

“Evolution and the Rise of Mass Culture.” Invited lecture at Alamosa State University, Alamosa, Colorado, October 17, 2013.

“Deep Sea Crinoids and Primordial Oozes: Remapping the nineteenth-century evolutionary debates at the bottom of the sea.” Paper presented at the Science Studies Colloquium Series, University of California, San Diego, October 7, 2013.

“The Charles Darwin-Wyville Thomson Debate: deep sea crinoids, scientific evidence, and the adjudication of Darwinian natural selection.” Paper presented at the International Society for the History, Philosophy, and Social Studies of Science annual meeting, Montpellier, France, July 7, 2013.

“Backward Design: Preparing purposeful discussion sections.” Pedagogical workshop, Sixth College, University of California, San Diego, February 28, 2013.

“The Shape of the United States: The Coast Survey and the Federal Government.” Paper presented at a public lecture of the History of Science Society annual meeting, The Blue Marble: A History of Oceans and Climate, Scripps Institution of Oceanography, San Diego, California, November 15, 2012.

“Mentoring and Retaining Diverse Graduate Students.” Workshop series, Office of Graduate Studies, University of California, San Diego, August 30 & September 6, 2012.

“Dredging Evolutionary Theory.” Paper presented at the History and Philosophy of Biology in the Desert Conference, Phoenix, Arizona, February 4, 2011.

“Science and Photography: the shift from the naturalist's eye to photographic realism in the late-nineteenth century.” Invited lecture, University of California, San Diego Sixth College, San Diego, California, November 23, 2011.

“Genetic Identity and the Genetic Information Discrimination Act.” Paper presented at the Biology by the Sea Conference, San Diego, California, January 24, 2010.

“The Rise of the Genetic Self: myths and legends of the Human Genome Project.” Invited lecture, University of California, San Diego Department of History, San Diego, California, November 18, 2009.

SERVICE

History of Science Society Strategic Planning Committee Member, advised the long-term strategic goals for the international society for the history of science, 2013.

University of California President's LGBT Task Force, participated in a University of California system-wide diversity policy development group that reported directly to the University of California president, 2012.

University of California, San Diego Chancellor Search Advisory Committee, graduate student appointment to a fifteen-person committee charged with the selection of the university chancellor, 2011.

President, UCSD Graduate Student Association, chief executive of the graduate student body and chair of ten vice-presidents, two staff, and three appointed officers. Also served as interim External Affairs Officer, a position that oversees student-driven, state-wide legislative affairs for higher education in California, 2010.

Graduate Council, UCSD Academic Senate, member of the administrative body for graduate education, departmental reviews, and block grant funding at the University of California, San Diego, 2010.

Vice President for Internal Affairs, UCSD Graduate Student Association, primary vice president of the student body responsible for overseeing graduate student organizing at the University of California, San Diego, 2009.

Student Fees Advisory Committee, University of California, San Diego, member of the administrative body responsible for allocation of student fee funds and university programmatic budget cuts, 2009.

ABSTRACT OF THE DISSERTATION

Dredging Evolutionary Theory:
the emergence of the deep sea as a transatlantic site for evolution, 1853-1876

by

Rodolfo John Alaniz

Doctor of Philosophy in History (Science Studies)
University of California, San Diego, 2014
Professor Cathy Gere, Chair
Professor Tal Golan, Co-chair

Marine invertebrate specimens from the ocean floor played a large role in the formation of evolutionary theory and they continued to help men of science adjudicate natural selection later into the nineteenth century. By 1880, the deep ocean floor had become “Darwin's laboratory,” a place to test the “direct action of external conditions on organisms.” According to dominant Victorian marine biology, the deep sea was an eternal, unchanging biogeographical space. There, and only there, could naturalists investigate how organisms evolved without the influence of changing environmental factors. The ocean floor was also a politically-charged geographical location, as colonial trade networks relied upon accurate mapping of the sea floor to ensure the safety of merchant and naval fleets. This dissertation explores the emergence of the deep-sea floor as a contested space where science, practice, and politics became inextricably linked. One result of that entanglement was a challenge to Darwinian natural selection prompted by marine invertebrate specimens. Governmental and non-governmental organizations from Britain and America joined the battle over natural selection. This story illuminates ways in which the geographical location of an investigation can have long-lasting consequences on international policies, scientific discourse, and biological theories.

INTRODUCTION

This dissertation provides a history of how deep-sea creatures helped naturalists adjudicate evolutionary theory in the nineteenth century. Many men of science believed that marine invertebrates would help them solve the great mysteries of life, such as the origins of life and new species. Abyssal creatures were also captivating, alien forms. They had simple, seemingly ancient structures that made them valuable specimens. Most importantly, they were situated upon the deep-sea floor, a supposedly primordial and unchanging environment. However, deep-sea creatures were also rare and difficult to obtain; naturalists faced complex problems when both retrieving and interpreting their prized specimens. Deep-sea biology was a new field fraught with technological difficulties and, as a consequence, men of science engaged with this field continually negotiated their scientific practices. The disagreements and exchanges related to deep-sea practices melded with debates over evolution later in the nineteenth century. Naturalists used the conjunction of these two sciences to simultaneously decide what evidence was necessary to establish a natural law and, as a result, whether Darwinian natural selection was anything more than just an intriguing hypothesis.

This story focuses on the men of science who called themselves the 'philosophical naturalists.' These naturalists shared the goal of deriving universal, scientific laws from the natural world. Physicists and astronomers had successfully elucidated a number of physical laws in the previous centuries. The philosophical naturalists desired to do the same for other fields, such as geology, hydrography, and biology. They also increasingly used the term "philosophical" to describe the nomothetic – or law-discovering – activities in which they engaged; for example, they might compliment a naturalist by claiming that he studied “philosophical geology” rather than simply studying “geology.”

This dissertation expands upon previous scholarship on these new men of science. Philip Rehbock's work on early nineteenth-century philosophical naturalists offered an excellent starting place for this study.[2] Rehbock identifies a number of naturalists at the University of Edinburgh who began to search for lawful patterns in the biological world. Many of them shared an affinity for Continental – especially German – Idealist, transcendental philosophies. Rehbock argued that the philosophical naturalists shared an intellectual lineage with these Idealist philosophers; they valued speculative reasoning over empirical, minute observations of the world's imperfect specimens. They believed that nature's mysteries would be won by brilliant insights into the lawful patterns of the world, not mere fact collecting. Nonetheless, the philosophical naturalists demonstrated a diversity of ways in which they interpreted these Idealist philosophies.

Rehbock ended his analysis by placing Darwin into this milieu of biological transcendentalism. Ultimately, Darwin rejected transcendental philosophies. Yet, he also paradoxically incorporated many philosophical naturalist ideas, such as the biogeographical distribution of species, into his theory.[3] For Rehbock, the era of “philosophical natural history” ended with Darwin. The decline of transcendental scientific philosophy in Britain, and therefore the philosophical naturalists, occurred after “thirty years of highest respectability,” according to Rehbock.[4]

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[2] Philip F. Rehbock, *The Philosophical Naturalists: Themes in Early Nineteenth-Century British Biology* (Madison: The University of Wisconsin Press, 1983), 3-12.

[3] Rehbock, *Philosophical Naturalists*, 194-195.

[4] Rehbock, *Philosophical Naturalists*, 195.

Other historians have identified major changes in British scientific philosophy within another locus, Cambridge University. Laura Snyder, for example, has provided a history of four individuals who sought to reform science in the early-nineteen century: William Whewell, John Herschel, Charles Babbage, and Richard Jones.[5] These naturalists shared frequent breakfasts together as Cambridge undergraduates. They discussed their plans to change the practice of science, to make it conform to the law-seeking, inductive principles laid out by Sir Francis Bacon, the jurist, statesman, and philosopher of scientific method, who was a graduate of Trinity College, Cambridge. Over the nineteenth century, these four individuals fought non-inductive science and reformed British science whenever possible. Snyder also positioned Darwin within the context of this Cambridge scientific reform.[6]

Similar to Rehbock's narrative, Snyder's scientific network members did not always share the same scientific practice even when they professed the same philosophy. The members of the Cambridge circle did not always agree upon what it meant to conduct “inductive science.” These disagreements over inductivism grew more explicit when outside the Cambridge clique. Other notable figures also explicitly argued with Whewell over scientific method; the argument between John Stuart Mill and William Whewell over proper inductive method remains one of the most important philosophical debates of the nineteenth century.[7]

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[5] Laura J. Snyder, *The Philosophical Breakfast Club* (New York: Broadway Paperbacks, 2011), 1-7.

[6] While Snyder does not use “philosophical naturalist” to describe her historical subjects, these men of science were as engaged in the larger project of uncovering natural laws as those found at the University of Edinburgh. For example, William Herschel stands out as an individual who appears in both Rehbock’s study of the philosophical naturalists and Snyder’s analysis.

[7] Laura Snyder's earlier book, *Reforming Philosophy: The Victorian Debate on Science and Society* (Chicago: Chicago University Press, 2006) , focuses on the inductive debates between William Whewell and John Stuart Mill.

A juxtaposition of Rehbock and Snyder's historical narratives illustrates that scientific practices were far from homogeneous in the nineteenth century, even when naturalists professed the same philosophy. Rehbock and Snyder each identified a community of law-seeking naturalists that emerged during Darwin's intellectual maturation. However, each scholar emphasized only one network of these philosophical naturalists. By doing so, their narratives of scientific reform centered on the scientific philosophies most common to their chosen groups. A wider, more comprehensive survey of these debates over scientific practice is necessary to fully capture the influence they had on Darwin's theories.

This study acknowledges the diversity of naturalists who sought to uncover natural laws throughout the nineteenth century. The term “philosophical naturalist, as employed in this dissertation, has not changed significantly from its use in Rehbock’s *The Philosophical Naturalists*, and I have extended it to cover the other networks investigated here. The term was used at the time to identify men of science who pursued universal truth about the living world. All my protagonists sought to subsume their observations under the rule of higher laws, but the expanded geographical focus of this dissertation reveals the diversity of ways in which they did so.

Consequently, this dissertation focuses on material and conceptual scientific practices. There tended to be a gulf between a naturalist's professed philosophy and what he actually did. This shift to a practice-based analysis allows for greater specificity in describing the conflicts and confluences of scientific methodology. It also demonstrates the great diversity of scientific practices present in the nineteenth century while allowing the historian to group and classify them through their similarities. While naturalists sought to uncover universal laws of the world, they attempted to arrive at that truth by using very different means. These differences caused conflict. Yet, their shared goal of global knowledge also gave the naturalists immense impetus to resolve their differences in the pursuit of natural law.

This analysis of local knowledge and its tension with aspirations towards global truth is indebted to David Livingstone's concept of “geographies of science.”[8] This historical lens examines how knowledge, even supposedly universal knowledge, is dependent upon its place of origin. Livingstone also identifies how the textual interpretation of evolutionary theories, including those of Charles Darwin, depended on various “geographies of reading,” which were spread in geographical space.[9] Other historians have expanded upon Livingstone's geographies of science. They have included a study of scientific practices and performance. These scholars have argued that the reception of scientific ideas was dependent upon the social and cultural context of the environment to which it was introduced. They have also argued that scientific trials occurred in geographical space, and that that space guided the outcome of scientific experiments.[10]

However, what happens to the analysis of geographies of science when it is applied to a space that contained no inhabitants and could be neither seen nor directly accessed by its explorers? Previous work on the geographies of science has relied upon the context of science's origin or reception. The deep sea has neither inhabitants to provide social context for the production of knowledge nor a readership to receive scientific ideas. I argue that the production of scientific knowledge was still guided by the

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[8] David Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago and London: The University of Chicago Press, 2003), 1-16.

[9] Livingstone, *Putting Science in Its Place*, 116-123.

[10] See Livingstone and Wither's edited volume, *Geographies of Nineteenth-Century Science* (Chicago and London: University of Chicago Press, 2011), especially Part Two, “Practices and Performances,” regarding these excellent reception and trial studies.

geographies of its production and interpretation. The physical conditions facing naturalists who explored the deep sea shaped their practices and conclusions. Even the perception of the deep sea's geographical traits, such as its primordial nature, influenced the scientific trials they conducted there. Geographical context also marked the production of scientific practices as much as the production of knowledge. The deep sea's inaccessibility highlights the ways in which marine science practices were determined by the institutional and geographical milieu of their production.

Ultimately, the geographies of science hinge upon the negotiation of scientific practice, especially regarding local ways of deriving knowledge from objects or specimens; the knowledge gained from an object may change radically when scientific practices differ. Such was the case for evolutionary theory in the nineteenth century. Naturalists repeatedly observed the same specimen or phenomenon and came to opposite conclusions about the natural world. The most important of these disagreements occurred over the discovery of living stalked crinoids, a class of marine invertebrates, in the deep sea. Multiple naturalists independently retrieved stalked crinoids from the sea floor during the 1860s. While some believed that the stalked crinoids offered proof of Darwin's theory of evolution, others interpreted the specimens in the opposite light, arguing that the stalked crinoids demonstrated evolution outside the bounds of natural selection and that natural selection could not account for their form and their distribution.

Despite the crinoids' importance to the late nineteenth-century evolutionary debates, their historical significance has been noted primarily by historians of oceanography and not by historians of evolutionary theories. The stalked crinoid's prominence in oceanographic histories may be explained by their role in prompting a famous scientific voyage, the *Challenger* expedition. For this reason, the crinoid's significance for evolutionary theory appears in Margaret Deacon's seminal work on the history of physical oceanography.[11] Deacon provides an excellent, sweeping narrative of physical marine science from 1650 to 1900 and, while not focused on biological theories, she explains the fundamental aspects of the mid nineteenth-century biotic debate – the debate over the existence of life in the deep sea – and the later discovery of the deep-sea fauna. She poses the discovery of the abyssal fauna as a reemergence of interest in ocean science.[12] This may be true for physical oceanography. As my research shows, biological interest in the deep-sea fauna remained and grew steadily throughout the entire nineteenth century.

Other historians of oceanography, such as Susan Schlee, have commented upon the early American contributions to the biotic debate.[13] The United States Coast Survey, America's first scientific institution, guided both American statecraft and scientific culture since the beginning of the nineteenth century.[14] Since Deacon's *Scientists and the Sea*, American participation in the biotic debate and the discovery of the stalked crinoids by a Norwegian naturalist, Michael Sars, have become canonical events in the history of marine science. However, early British contributors to the biotic debate and the discovery of stalked crinoids by American deep-sea dredgers remain underexplored aspects of transatlantic marine biology.

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[11] Margaret Deacon, *Scientists and the Sea, 1650-1900: a study of marine science*, second ed., (Aldershot and Brookfield: Ashgate Publishing, 1997), 306.

[12] Deacon, *Scientists and the Sea*, 276.

[13] Susan Schlee, *A History of Oceanography: The Edge of an Unfamiliar World* (London: Robert Hale & Company, 1973), 23-79.

[14] See Schlee, *A History of Oceanography*, and Hugh R. Slotten, *Patronage, Practice, and the Culture of American Science: Alexander Dallas Bache and the U.S. Coast Survey*, (Cambridge: Cambridge University Press, 1994), 42-60.

Recent studies of the stalked crinoid's influence have led to a larger scholarship on the history of deep-sea biology. For example, Eric Mills reopened the case of deep-sea biology as a historical site of scientific and historiographical tension in his 1983 article, "Problems of Deep-Sea Biology: An Historical Perspective." This study attempts to synthesize the pervious historical "eras" in deep-sea science into one narrative of how the science changed over time. Mills acknowledges the episodic nature of marine science since institutional funding often determines what – and when – science may be conducted on the oceans. He also takes up the issue of oceanographic practices and "subdisciplines" as a subject for historical analysis, stating that economic factors affect practice as much as the periodization present in the history of oceanography.[15] Harold Burstyn has also emphasized the role of institutions and patronage by depicting the *Challenger* expedition and its report as a precursor to "big science." This scientific endeavor was characterized, as Burstyn points out, by large, interdisciplinary teams and large funding.[16]

More specific to the deep-sea science, Helen Rozwadowski has provided a cultural history of nineteenth-century deep-sea exploration in Great Britain and the United States.[17] She demonstrates how naval and scientific technologies, along with naval personnel and men of science, all contributed to the obsession with and, later, the scientific discovery of the ocean depths. Her sources differ from the previously mentioned scholarly work in her attention to cultural breadth. Her research showed the reciprocal relationship between cultural fascination in a scientific subject and the

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[15] Eric L. Mills, "Problems of Deep-Sea Biology: An Historical Perspective," in *The Sea, vol. 8: Deep-Sea Biology*, ed. Gilbert T. Rowe (New York: Wiley-Interscience, 1983), 3-4.

[16] Harold Burstyn, "'Big science' in Victorian Britain: The *Challenger* Expedition (1872-6) and its Report (1881-95)," in *Understanding the Oceans: A Century of Oceanic Exploration*, eds. Margaret Deacon, Tony Rice, and Colin Summerhayes (Abingdon: UCL Press, 2001).

[17] Helen Rozwadowski, *Fathoming the Ocean: The Discovery and Exploration of the Deep Sea* (Cambridge and London: Harvard University Press, 2005), 3-35.

research conducted. Her narrative also addressed the importance of the stalked crinoid for evolutionary theory and the *Challenger* expedition.

From these excellent beginnings, a deeper narrative about the role of marine biology in the evolutionary debates builds a further bridge between the newer history of oceanography and the well-developed history of evolutionary theories. The “Darwin industry” has developed a rich narrative of the development, negotiations, philosophies, and controversies surrounding the various theories of evolution. Many scholars have contributed to the vast historical and philosophical literature related to Darwin's theory of natural selection. A smaller number of these historians have explored the role of marine biology in the history of evolution. Most of these maritime accounts focus on Darwin's early career – through his mentor Robert Grant – or his relationship the German biologist Ernst Haeckel.

Phillip Sloan has contributed an analysis of Darwin's research as part of a larger “invertebrate program.”[18] He has identified a tension between narratives in Darwin's thought, where Darwin's first research consisted of marine invertebrate biology, followed by the formation of his transformist ideas. Darwin's development of his evolutionary claims seems to have been, then, interrupted by a study of another marine invertebrate, the barnacle. Sloan connects these two seemingly inexplicable periods of Darwin's life by placing his research on transmutationism into a larger project on marine invertebrate zoology. Darwin's introduction to the study of simple seabed invertebrates, or the “zoophyte problem,” originated with his early studies under the tutelage of Robert Grant, a recently graduated zoologist at the University of Edinburgh. Sloan's study ends with

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[18] Phillip R. Sloan, “Darwin's Invertebrate Program, 1826-1836: Preconditions for Transmutationism,” in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), 71-120.

Darwin's voyage of the *Beagle* and, therefore, the beginnings of Darwin's theory of evolution. A larger chronological scope, as taken by this dissertation, shows that Sloan's assertion is equally true at the end of the century as it was during Darwin's undergraduate education.

Some biographies of Darwin have also begun to recognize his work on marine invertebrates. For example, Adrian Desmond and James Moore's biography describe Darwin's early marine invertebrate research under Grant in detail.[19] Desmond and Moore's account is intended to place Darwin's life into the cultural and political context of Victorian society. While marine invertebrates played a large role in Darwin's development, crinoids did not affect the larger fabric of Victorian culture and, as a consequence, they do not feature prominently in the later half of their narrative. Darwin's interaction with the *Challenger*'s director, a conflict noted by many historians of oceanography, is relegated to a single paragraph describing a letter he wrote to *Nature* magazine.[20]

Despite the absence of late nineteenth-century marine invertebrate zoology in the historiography of Darwinism, my research shows that marine biology did not wane during the century. Quite the contrary, most of the major figures in the history of evolution were also major contributors to the transatlantic biotic debate. Their colleagues taught them a number of these new marine science practices, such as the use of the naturalist's dredge. Networks of naturalists formed around their shared experiences with certain instruments and practices. Scientific practices also yielded similar specimens and evidence. A group of dredgers, for instance, all retrieved marine invertebrate specimens

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[19] Adrian Desmond and James Moore, *Darwin: The Life of a Tormented Evolutionist* (New York and London: WW Norton & Company, 1991), 33-44. Also see Janet Borwne’s definitive biography, *Charles Dawin: A Biography* (Princeton: Princeton University Press, 1996).

[20] Desmond and Moore, *Darwin*, 646.

from the sea floor. Therefore, each network of naturalists was also bounded by common specimens and the shared questions about the natural world that the specimens would be used to explain.

I have used the prevalence of shared practices to trace a number of scientific networks in this study. This historical methodology is a pragmatic technique for handling large groups of individual historical actors, each with their own unique biographical contexts. Many of these actors shared common ways of examining the natural world that is sometimes difficult to define with great precision.[21] Naturalists often joined these networks when they were trained in the use of a scientific technique. For example, an influential professor at the University of Edinburgh might have encouraged his students to take up the naturalist's dredge as part of their scientific training. The number of dredgers would grow at the University and, even if a student did not take this professor's class, an initiate could learn from any number of dredgers in residence there. This network could then use common scientific specimens – those produced by the naturalist's dredge – to debate scientific questions.

I call the historical object under study in this dissertation "evidentiary practice." This designation facilitates the examination of conflict between differing scientific networks as they attempted to research the same scientific question or phenomenon. "Evidentiary practice" can be defined by three questions:

1. What specimens did the naturalist decide to collect or observe and why did he or

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21 Ludwik Fleck has also identified groups of naturalists who all share common ways of thinking, what he calls a "thought collective." These esoteric groups share a similar scientific ways of viewing an object. For example, a group of painters may see a crack in a wall and see an aesthetic blemish while a group of carpenters may observe a crack in the wall and see that the wall needed to be rebuilt. This study uses and expands on Fleck's examination of scientific groups. Ludwik Fleck, *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1979), 38-51.

she believe they were scientifically valuable?

2. How did the naturalist physically collect the specimen and what did he or she do to it once it was retrieved?
3. In what way did the naturalist then derive evidence for natural law from those observations?

Naturalists who studied under the same instructor often shared identical answers to these three questions. Conflict between differing scientific networks can often be explained by a differing answer to one or more of these questions. But these evidentiary practices also moved around. As one network expanded, its practices become more commonplace, available for adoption by other naturalists from a different network or institutional background.

This dissertation pays especial attention to the way in which these networks were not static, following closely how they shifted location, spread, and broke apart. For example, many dredgers were trained at the University of Edinburgh. However, the practice spread to the British Association for the Advancement of Science because one Edinburgh-trained scholar began a very popular dredging committee within that organization. There he taught new naturalists to use the dredge, thereby expanding the use of that practice and the specimens gained from that practice. While the scientific practices continued to bear the marks of the place from where they originated, they often changed geographical location as the network expanded. In this narrative, three different scientific networks came to examine specimens from the same geographical location, the deep seabed, during the nineteenth century. Each group had a distinct set of evidentiary practices. I argue that the Darwinian evolutionary debates were started – and later resolved – by a blending of these different practices. My argument consists of five parts.

Chapter one establishes that three different scientific networks – each originating at a different institution – began independent studies of the historical seabed during the early nineteenth century. These institutions were the University of Edinburgh, Cambridge University, and the United States Coast Survey.[22] Each of these networks initially employed different evidentiary practices to examine the sea floor. These practices were determined, for the most part, by the geographical context that produced them, ranging from local geological structures to regional institutional politics. I then demonstrate how two of these different scientific networks conflicted over pre-Darwinian evolutionary ideas and show how this conflict was partially based on their differing evidentiary practices.

Chapter two uses Charles Darwin as a case study to demonstrate a blending of "Cambridge" and "Edinburgh" evidentiary practices. Darwin attended both the University of Edinburgh, where he was taught zoogeology and dredging, and Cambridge University, where he was introduced to various methods of deploying evidence in search of natural laws. Darwin employed both practices simultaneously while aboard the HMS *Beagle*. I then show how he used the sea floor, a subject of interest to both scientific networks, as a scientific geography to reason through his theory of natural selection. That line of blended reasoning led to a prediction that living intermediate forms would be found to substantiate his concept of natural selection by morphological divergence. Darwin also speculated that these intermediate forms would be found in the deep sea – if it was not, as some naturalists argued, devoid of life.

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[22] For simplicity, I have included the United States Naval Observatory into the United States Coast Survey as an institution in this introduction. The more appropriate institutional list would include the United States government, consisting of two rival institutions: the Naval Observatory and the Coast Survey. This dissertation treats both of these institutions as separate entities and explores each in detail. However, both of these institutions shared a relationship with the United States government and a geographical context at Washington, DC, which is the justification for neglecting to list the Naval Observatory at this time. Their relationship will be further explained in chapters one and three.

Chapter three explains how and why American naturalists developed a method of surveying the deep-sea floor. This method employed a device, called Brooke's sounding line, to overcome the technological problems related to deep-water surveying. This device also brought back surprising samples from the abyssal floor. Microscopic shells were found lining the Atlantic seabed. The presence of these tiny organisms reopened the debate as to whether or not life existed on the sea floor. The United States Civil War interrupted the American biotic debate and prompted the circulation of naturalists across the Atlantic. This diaspora of American naturalists, practices, and samples later sparked the biotic debate in England and Scotland. These new practices opened the use of deep-sea biological specimens in British scientific networks.

Chapter four shows how Darwin's closest associates – the major figures in the history of nineteenth-century evolutionary theory – were also the earliest British contributors to the biotic debate. During the late 1860s, the period following the United States Civil War, British naturalists came to the forefront of the debate over the presence or absence of life in the deep sea. British marine biologists, as colleagues and friends of Charles Darwin, used specimens from the deep-sea floor as evidence for evolution by natural selection. These naturalists believed that deep-sea creatures, especially the stalked crinoids, were the intermediate forms that Darwin predicted would be found if his theory was correct. These developments prompted naturalists to organize the *Challenger* expedition to solve the biotic debate and test Darwin's theory of natural selection.

Chapter five, the final chapter, outlines the biological research done aboard the *Challenger*. This research program included a test devised to either prove or disprove Darwin's theory of evolution by modification. This test was of utmost importance to naturalists who had already accepted evolution, but awaited evidence for natural selection as the mechanism by which evolution operated. The failure of the *Challenger* expedition to produce that evidence caused many elite naturalists to either abandon the theory of natural selection or devise new strategies for proving Darwin's hypothesis. In essence, the *Challenger* expedition, along with other factors, initiated the period commonly known as the “eclipse of Darwinism,” when natural selection – as an explanation for the origin of species – waned in favor of a number of other competing evolutionary mechanisms.

The evidentiary practices employed to test Darwin's concept of natural selection were blended from scientific networks across the Atlantic. Thomson's test of evolution employed one element of evidentiary practice from three different institutions. Like Darwin's original hypothesis, this test combined the specimens valued by zoogeologists, originally from the University of Edinburgh, and the predictive methods used by astronomers to uncover natural laws, originally from Cambridge University. Unlike Darwin, Thomson also included a practice introduced to British naturalists since the publication of *On the Origin of Species;* the test also included a variation of Brooke's sounding line to procure the deep-sea specimens, which was originally from United States maritime institutions. The combination of evidentiary practice from each of these networks allowed naturalists to finally resolve the question of natural law that they pursued as philosophical naturalists. I conclude by showing how, as an outcome of this historical trajectory, Darwinism survived into the twentieth century as a type of evidentiary practice, or *method,* rather than an *explanation* for evolution.

CHAPTER ONE: A Geography of Philosophical Naturalists

An anonymous article, “Observations on the Nature and Importance of Geology,” appeared in the 1826 volume of the *Edinburgh New Philosophical Journal*. The article touted the potential of geological studies for the daring man of science. The author claimed that geology gave naturalists a window into the workings of the organic world. The study of rocks and fossilized remains provided a record of past organic forms and, therefore, a means of examining the progress of life over time: “Between the dead and the living there yawns a chasm, indeed, which we can never overleap; but if any thing can lift the veil that hangs over the origin and progress of the organic world, it must be those remains of it, for the knowledge of which we are indebted to geology.”[23] The anonymous author called for the use of geology to answer the grand questions of the natural world. The grandest of those questions was the origin of species, “...the scale of gradation, according to which he arranges the animal kingdom, is... the history of their origin; and the discovery of this truly natural method, the most important problem of the natural philosopher.”[24] This article is famous for being one of the early British texts to blatantly support a theory of evolution put forth by the French transmutationist Jean-Baptiste Lamarck.[25] As well as marking that important watershed, “Observations on the

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[23] Anonymous, "Observations on the Nature and Importance of Geology," *Edinburgh New Phil. J.*, 1 (1826), 295. The author was later identified by historians as Robert Jameson, who is discussed in this chapter. See James Secord, “The Edinburgh Lamarckians: Robert Jameson and Robert Grant,” *Journal of the History of Biology* 24 (1991): 1-18.

[24] Anonymous, “Observations,” 297.

[25] Other British naturalists had reviewed Lamarck's book *Philosophie Zoologique,* the text that advocated for his theory of evolution, before 1826. While the text was most likely available across the English Channel shortly after its publication in 1809, the first British reviews did not appear until 1811. The reviews were mostly mild or admiring. The 1826 article mentioned above remains the first in-depth appearance of Lamarck's views in the scientific press, though there is mention of medical case studies in other journals. For an exploratory examination of French scientific texts into Britain during this period, see Jonathan R. Topham, “Science, Print, and Crossing Borders: Importing French Science Books into Britain, 1789-1815,” in *Geographies of*

Nature and Importance of Geology" also raises other interesting questions. Who were these new natural philosophers and what did the author mean by stating that the discovery of a method was their most important problem?

Over the middle decades of the nineteenth century, these philosophically minded naturalists sought to craft a new method for exploring the natural world. This chapter examines three distinct groups of their kind. The three groups examined here were situated within the University of Edinburgh, Scotland, Cambridge University, England, and the US Coast Survey, the United States of America. Naturalists from each of these groups were engaged in the philosophical study of the sea floor over time, meaning that they examined the contemporary and historical seabed with an aim to discover new natural laws. While natural philosophers across the world were unified in a desire to explain the great mysteries of nature, each subgroup of this community had its own approach to uncovering nature's laws. Those approaches were initially rooted in the geographical locations where they lived and the institutions where they worked, a set of conditions encompassing everything from basic morphology of rock and water to the idiosyncrasies of institutional organization.

These men called themselves "philosophical naturalists." As a whole, they engaged in a number of emerging disciplines calibrated for a new purpose, the new discovery of fundamental natural laws. However, they shared a common identity related to their shared pursuit.[26] It should be noted that the focus in this chapter on philosophical naturalists studying the seabed is a category created through the use of historical hindsight. The men examined in this chapter would not have called themselves "seabed

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Nineteenth-Century Science, eds. David Livingstone *et al*. (Chicago: The University of Chicago Press, 2011), 311.

[26] Rehbock, *The Philosophical Naturalists*, 4.

naturalists."[27] They would have claimed a number of topics as their scientific domain: tidology, hydrography, zoogeology, astronomy, and natural theology. In one sense, the selection of the smaller seabed-studying members of the philosophical naturalists offers a way to see how they negotiated matters of method and scientific practice. Hindsight also informs us that these naturalists would later play a key role in the history of evolutionary theories. These early seabed naturalists and their students constituted the core, elite group of men of science that debated Darwin's theory of natural selection.

The later debates over natural selection were partly an argument over what scientific practices would best reveal the universal laws of nature. For this reason, practices and uses of evidence play a key role in the chapter's organization. The characterization of the three seabed naturalist communities is not meant to be a generalization of each institution, but rather a sharp focus on scientific practices embedded within each institution's geographical context.

This new focus may unsettle those historians of evolution who are accustomed to discussing the intellectual and philosophical lineages of their historical subjects. Indeed, the intellectual history of nineteenth-century biology is a well-researched and informative topic. An analysis of scientific practices, as distinct from scientific ideas, provides new insight into the nineteenth-century evolution debates and the institutional scientific differences that fueled those debates. In some sense, practices are passed down a scientific lineage with greater fidelity than philosophical leanings.[28] A student would often

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27 It should be noted that the name used by these individuals, "philosophical naturalist," is not the same as the modern term "philosophical naturalism" used by philosophers of science.

28 Other historians have also used local institutional practices to describe the development of biological practices. Soraya de Chadarevian describes how biophysics practices, centered on a Cambridge institution, would later spread into what is now known as molecular biology. Her study focuses on the local contingencies experienced related to the Cambridge group. She states that her study, "...points to the multiple arenas and the complex web of negotiations involved in

travel to a scientific institution to study under one of the scientific elite. That naturalist would then pass along his or her techniques to a cadre of students. It is difficult to use a scientific practice that one has never been exposed to, especially in the case of new technological methods or practices that require supervision to master. Even though these new naturalists traveled the world and spread to new locations, they shared a common exposure to the practices learned at their original institution. The individuals at the same institution will also have greater access to the practices of their colleagues through sheer proximity. Practices that became invisible to the individual naturalist sometimes created confusion or disagreement over evidence when confronted by the wildly different methods used in other institutions and communities. Such was the case for evolution and natural selection during the mid-nineteenth century. But before delving into the three communities of philosophical naturalists in Edinburgh, Cambridge, and the US, this chapter examines some of the background to the questions that united them and the debates that divided them. The source for much of their drive to seek the laws of life is to be found in post-revolutionary France, where political, philosophical, and scientific turbulence acted together to produce new questions, new answers, and new controversies about the order of nature.

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building molecular biology after [World War II]. More local studies will enrich the picture. But any new picture which emerges will form in the intersections of these local accounts, not by moving beyond them.” I agree with de Chadarevian's general premise that local – and even institutional – contingencies are vital to understanding the development of emerging scientific areas of study. However, by incorporating multiple networks and their practices, the historians maintains a focus on local contingencies, yet may still uncovers new aspects of scientific conflict that are beyond the capacity of individual, local studies to reveal. See Soraya de Chadarevian, *Designs for Life: Molecular Biology after World War II,* (Cambridge: Cambridge University Press, 2011), 12.

“Everything is in Flames:” *Biologie* and Natural History

The early-nineteenth century was a period of both intellectual and social upheaval in science. Intellectually, naturalists had little reason to challenge the long-standing Enlightenment traditions for studying the living world. Spreading European colonialism and American imperial expansionism continued to bring a number of exotic specimens into the hands of excited naturalists. The description and classification of God's creation was noble enough work to last many lifetimes, and European naturalists developed increasingly sophisticated methods for description and classification. Most famously, Carl Linnaeus established his system of taxonomy in 1735 through his publication of *Systema Naturae*.[29] Classification according to Linnaean taxonomy and the discovery of new species consumed the efforts of many European naturalists during the period. These Enlightenment naturalists, in turn, attempted to fit their classification into dominant intellectual frameworks inherited from their Renaissance predecessors. Linnaeus, for example, was concerned with how his classification scheme would fit into – or change – the older understanding of the *scala naturae*, the cosmic order of increasing perfection, ranging from rocks to animals to God.[30] Starting in the eighteenth century, however, naturalists began to challenge these theological assumptions about the hierarchies of the natural world, which led to new questions about the organization of living creatures in relation to each other. The new questions facing nineteenth-century naturalists required new methods.

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[29] Carolus Linnaeus, *Systema naturae per regna tria naturae :secundum classes, ordines, genera, species, cum characteribus, differentiis, synonymis, locis*, 10th ed., (Stockholm: Laurentius Salvius, 1758).

[30] The *scala naturae* is also known as the great chain of being. See Arthur O. Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge: Harvard University Press, 1961), for a discussion of the *scala naturae* as an influential intellectual framework. Linnaeus is mentioned specifically only on page 234 of Lovejoy's text, however.

Jean-Baptiste Lamarck, the naturalist lauded in the anonymous article mentioned at the beginning of this chapter, was one of the first scholars explicitly to call for a new science of the living world. Lamarck had joined the Parisian scientific community with the support of Georges-Louis Buffon, one of the leading French Enlightenment naturalists. Buffon was impressed by Lamarck's anti-traditional, non-Linnaean approach to botanical scholarship. Buffon died in 1788, leaving Lamarck without his powerful patron. Nonetheless, Lamarck gained one of the chairs available at the Museum of Natural History and took on an unusual professorship, “the zoology of the insects, worms, and microscopic animals." While potentially a less prestigious position than he previously held, his study of “invertebrates” during the 1790s helped him to challenge contemporary methods of science.[31]

His response was to convert to transmutationist thought, a belief that new species emerged from changes in older species. Lamarck's peculiar specialty at the Parisian Museum of Natural History linked the emerging study of life's organization with invertebrate zoology. By 1800, the year he began giving public lectures on transmutation, Lamarck began to call himself a philosophical naturalist (*naturaliste philosophe*). These new philosophical naturalists, as the historian Richard Burkhardt has explained, occupied a role uniquely suited to Lamarck's aim of meditating upon the natural world in order to give it a rational foundation.[32] Lamarck published *Zoological Philosophy* in 1809, which made a call for an entire zoological study within this new philosophical vein. He opened *Zoological Philosophy* with a challenge to traditional natural historians, those primarily interested in classification:

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[31] Richard W. Burkhardt, “Lamarck, Evolution, and the Politics of Science,” *Journal of the History of Biology*, 3 (1970): 280.

[32] Burkhardt, “Lamarck,” 285.

> Experience in teaching has made me feel how useful a philosophical zoology would be at the present time. By this I mean a body of rules and principles, relative to the study of animals, and applicable even to the other divisions of the natural sciences; for our knowledge of zoological facts has made considerable progress during the last thirty years[...]
>
> […] I was therefore led to embark upon successive inquiries of the greatest interest to science, and to examine the most difficult of zoological questions.
>
> How, indeed, could I understand that singular degradation which is found in the organization of animals as we pass along the series of them from the most perfect to the most imperfect...? How could I avoid the conclusion that nature had successively produced the different bodies endowed with life, from the simplest worm upwards?[33]

Lamarck pulled upon his unique position as professor of invertebrates to show how even the simplest organisms were imbued with the same power of life as humans. The only difference was that these invertebrates were less complex. As organisms became more perfect, they acquired greater complexity. This revelation – the relationship between complexity and organization – originated from his study of invertebrates. Consequently, invertebrates formed the cornerstone of *Zoological Philosophy*. Invertebrates allowed the philosophical naturalist to study organismal complexity and the organization of living forms exactly because of their simplicity:

> The study of *invertebrate animals* must, in fact, be of special interest to the naturalist for four reasons: -(1) The number of the species of these animals in nature is much greater than that of the vertebrate animals. (2) Since they are more numerous, they are necessarily more varied. (3) The variations in their organisation are much greater, more sharply defined and more remarkable. (4) The order observed by nature in the successive formation of the different organs of animals is much better expressed in the mutations which these organs undergo in invertebrate animals. Moreover, their study is more fertile in helping us to understand the origin of organisation, with its complexity and its developments, than could possibly be the case in more perfect animals such as vertebrates.[34]

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[33] Jean-Baptiste Lamarck, *Zoological Philosophy: An exposition with regards to the natural history of animals*, trans. Hugh Elliot (New York and London: Hafner Publishing, 1963), 1.

[34] Lamarck, *Zoological Philosophy*, 13. original emphasis.

Not only was the study of invertebrates important because of their abundance, but they could help the philosophically-inclined to unlock the secrets of sensation, organization, spontaneous generation, and other aspects of living creatures. Lamarck called his new, philosophical zoology "*biologie*."[35]

Other naturalists took up Lamarck's interest in *biologie* and the philosophical questions it raised. The intellectual divide between these new philosophical naturalists and other scholars – some engaged in their own scientific reforms – precipitated an intense, social rift between the two groups. As one example of this conflict, Buffon's other protégé Etienne Geoffroy Saint-Hilaire shared Lamarck's "philosophical" outlook regarding contemporary science. Geoffroy began to publicly disagree with another influential naturalist in Paris, Georges Cuvier, regarding the methodology of natural history. Cuvier saw the classification and ordering of living forms as the primary duty of the naturalist. Geoffroy, by contrast, pursued the dangerous questions raised by *biologie*, those that overturned strongly held philosophical assumption, according to Cuvier.[36] Primarily, the debate was a fight over the types of questions that naturalists should ask.[37]

Intellectual conflicts, such as that between the two eminent French naturalists, Geoffroy Saint-Hilaire, a philosophical anatomist, and Cuvier, a comparative anatomist,

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[35] Lamarck, *Zoological Philosophy*, 6.

[36] Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin*, (Oxford: Oxford University Press, 1987), 4-8.

[37] Geoffroy had helped Cuvier come to Paris and even lodged with him for some time at the Museum. The intimate friendship between these two naturalists has been the subject of much speculation by historians, especially considering that their careers ended with such bitter rivalry. There were other elements that fueled the Cuvier-Geoffroy dispute, such as a basic conflict of personalities. However, the conflict that arose out of such close friendship also illustrates the division between the philosophical methodology and the comparative methodology in science. Appel, *Cuivier-Geoffroy Debate*, 32.

could generate intense social discord within the scientific community. The two methods of observing nature came to a head at the Parisian *Académie des Sciences*. In a story recounted by historian Tony Appel, Johann Wolfgang von Goethe stormed into a friend's home alarmed and speaking about the “great event” of 1830. France had just concluded a revolution and Charles X was overthrown. Europe was once again riven with unrest and revolutionary turmoil. Goethe asked his friend, “What do you think of this great event? The volcano has come to an eruption; everything is in flames, and we no longer have a transaction behind closed doors!” When his interlocutor responded that he was not surprised given the current ministry and royal family, Goethe halted him, “I am not speaking of those people at all, but of something entirely different. I am speaking of the contest, of the highest importance for science, between Cuvier and Geoffroy Saint-Hilaire, which has come to an open rupture in the Academy.” For some, the open division between the philosophical naturalists and the comparative anatomists transcended contemporary politics and petty European squabbles. The debate between Cuvier and Geoffroy – between the traditional natural history and the new philosophical school – attracted international attention.[38]

The social unrest among scientists was not limited to France. Britain and the United States also felt the social tensions created by the emergence of philosophical naturalists. In some sense, the fight between Cuvier and Geoffroy was mirrored at the major scientific institutions of the age. Each conflict was colored by local politics. The

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[38] Appel, *Cuvier-Geoffroy*, 1-6. It should be noted that Cuvier was also interested in pushing forward the boundaries of natural history, but in a different way than Geoffroy. Appel's distinction between the forward momentum of these two prominent naturalists also gives distinction to the dissatisfaction with traditional natural history at the time. Appel's work, when paired with Rehbock's, form an insightful look into the natural philosophy- natural history movement and debate. Appel focuses on the unique French context of the Cuvier-Geoffroy debate while Rehbock examines the British response to the new methodology.

tension between traditional study of nature and the philosophical schools was also accompanied by institutional dissent in England and scientific reform the United States.

The Context of Scientific Reform in Britain and the United States

At the beginning of the nineteenth century, Joseph Banks, according to British science reformers, had been president of the Royal Society for far too long. The time had come for change. The English botanist had been elected president of the Royal Society in 1778 after returning from the famous Cook voyages to the Pacific. Banks exercised a tyrannical grip upon English natural history from his position. He not only stifled dissent among English naturalists, thereby stopping attempts to criticize more established natural history techniques, but also forcefully opposed the establishment of any other scientific societies. He was keen to keep the Royal Society as the major social organization for science in England and, thereby, maintain his position as one of the most influential figures in British natural history. By 1820, almost half a century after becoming the long-reigning president, Banks announced his retirement and the chemist Sir Humphrey Davy took the reins of the Society, followed by Davies Gilbert. Reform sentiment had been fomenting ever since Banks' retirement as Society president, so some naturalists awaited the upcoming election as a sign that reform in science could dawn on the English nation.[39]

The English reform of science was part of a larger upwelling of scientific society in Great Britain. The Royal Society of Scotland was also intent on reforming scientific society across Britain, so they weighed in on the English Royal Society election from

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[39] Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford: Clarendon Press, 1982), 37.

afar. Two major contenders for the English Royal Society presidency emerged. Each aspiring president represented a different avenue for the continuance of science in England. On one hand, the suggested successor for president of the Royal Society could be of aristocratic blood, represented by the Duke of Sussex. The Duke was a Whig in politics, but, for science, he represented a conservative past, when scientific institutions were led by noble birthright and not scientific accomplishment. Many reformers wanted to separate themselves from the aristocratic roots of the Royal Society. They wanted to see a first-rate man of science at the helm of their republic of scientific letters, not a patron whose only qualification was noble blood, liberal or not. Reformers were set against the continuance of aristocratic, scientifically inexperienced control over their scientific society.

If reform was to happen in the Royal Society of London, the organization would need a brilliant man of science at its helm. The next president would represent a new era for science in the nineteenth century. Many reformers believed that John Herschel was that man. Herschel was a member of the Cambridge scientific elite and a personal friend of Charles Babbage and William Whewell, both also Cambridge men and notable figures in scientific society. John Herschel represented a strong vision for the future of modern science and had the background to gain support from the scientific reformers. Reform was a common topic for both science and politics during that time. Factions of the scientific community desired security for their future, but nobody knew whether that security would come in the form of noble blood (representing a conservative step into British scientific past) or a genius man of science (representing a step toward the reform of scientific society). There was only one problem for the reformers: John Herschel was not willing to publicly announce his desire to be president of the Royal Society.

Whether feigned or sincere, Hershel's modesty – in addition to a miscommunication between reformist voters – crippled the early scientific reform movement. After a hasty campaign for Hershel, the Duke of Sussex won the presidency of the Royal Society.[40] The science reformers were understandably upset when the Royal Society chose a scientific patron with aristocratic blood rather than a sterling intellectual reputation. Rumors about a new association for naturalists around Britain began to spread.

Established naturalists that studied or taught at powerful institutions were not enthusiastic about forming a new scientific association at first. Unlike their provincial counterparts, these titans of British science were already well integrated into established scientific society. While the lesser naturalists looked to these paragons for scientific leadership in their new society, the established naturalists had few reasons to spend time and resources on a society that they considered to be redundant. When asked for assistance or input in constructing the new association, later called the British Association for the Advancement of Science, many prominent naturalists declined the invitation. However, a number responded enthusiastically to the call for a new scientific organization. In one sense, these enthusiastic members of the scientific elite continued their call for new leadership from their previous campaign in the Royal Society; if they were not able to vote a reputable naturalist into the presidency, they would rebel and form a new union of scientific minds. William Vernon Harcourt, later founder of the British Association for the Advancement of Science, described the break from the Royal Society in revolutionary language: “colony after colony disevers itself from the declining empire.”[41] The reform and rebellion language was, in one sense, a reference to the

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[40] Morrell and Thackray, *Gentlemen of Science*, 56.
[41] Morrell and Thackray, *Gentlemen of Science*, 46.

British preoccupation with imperial power.[42] The scientific elite that participated in the formulation of the new association had revolution on their minds.

A different vision for science would come with a revolution in British scientific society. A new association of science represented a reformulation for scientific knowledge and the perfect opportunity for these elite reformers to change British science. The famous members of the new British Association for the Advancement of Science had strong views regarding how science should be conducted. They struggled to impress those visions onto the new organization. Many different visions for science competed during the British Association's first years, but two major groups emerged with radically different ideas about how science should proceed beyond traditional natural history. The Royal Society had come to represent aimless collecting and categorizing. The reformers believed that a new organization could move beyond the Royal Society and allow science to achieve its full potential.

Harcourt may have likened the break from the Royal Society to the American Revolution, but in the United States itself a parallel rupture was breaking open, albeit in a much more pragmatic vein than in England or on continental Europe. For the small group of American philosophical naturalists, the most important divide between different scientific pursuits and methods was tied to the establishment of the first governmental scientific institutions. The leading figures of these scientific institutions actively reformed American science and education. They spread their vision of excellence in scientific practice by fighting against perceived charlatanism. They also advocated a grander,

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[42] I believe the reference to rebellion and lost “colonies” of science is a passing reference to the United States Revolutionary War and the rise of American science in the early-nineteenth century. The comment might specifically refer to the closure of the British Board of Longitude as the US Coast Survey began operations. However, beyond a passing reference, the comment certainly links British science to imperial power.

more philosophical science for the country. That philosophical vision for science stood in sharp contrast to the applied science practiced in the United States before the nineteenth century.

American science still labored under its history of colonial rule. With few notable exceptions, the British colonies had not been known as centers of enlightened knowledge. The New World had certainly provided valuable and rare specimens for examination. Colonial agents and indigenous peoples also acted as reservoirs of natural knowledge about these objects, but the great centers of learning at Paris and London outshone anything established in the new United States of America. The talents of American scientists seemed to lie in practical channels, and American inventors were quick to apply their knowledge to solving problems. If American science were to claim its place as equal to that of European civilizations, it would need to promote better education, establish scientific institutions, and support a number of new, elite naturalists. A self-proclaimed group of naturalists emerged with such reformist goals for American science in the mid-nineteenth century.

This cadre of elite naturalists called itself the “scientific Lazzaroni,” after a class of poor Italian beggars that lived by chance work and mob political action.[43] The key members of this group supported each other's scientific and political work. Together they represented the most powerful institutions of science in the young nation, including the Harvard Museum of Comparative Zoology, the Smithsonian Institution, and the US Coast Survey. The growing network of reformist naturalists was centered on the Coast Survey, the first of these nationally supported institutions.

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[43] Lillian B. Miller, *The Lazzaroni: Science and Scientists in Mid-nineteenth Century America* (Washington: Smithsonian Institution Press, 1972), 4-5.

The zeal to improve American science caused conflict between these emergent centers of scientific power. The Coast Survey, in its fierce conflict with the United States Navy, provides an excellent example of the distinctive character of scientific unrest in the United States, in which science was one element in the tumultuous process of forging a nation.

By the mid-nineteenth century, two agencies had conducted scientific surveying for the United States, the Navy and the Coast Survey. Naval officers had been in charge of all surveying until the United States Congress authorized a survey of the United States coast and a civilian naturalist was placed in charge of the operation. Ferdinand Hassler, the first superintendent of the Coast Survey, brought advanced European techniques to the task of mapping the American shoreline. For Hassler, surveying was a practice in exactitude, and he trained his assistants with the highest standards of science. The practices used in civilian surveying, through the Coast Survey, aligned well with academic science in the United States. However, some naval officers and politicians believed that the process of exact surveying took too long for their tastes. In addition, the Coast Survey took scientific authority away from the Navy.

On multiple occasions, the Navy attempted to wrest control of the Coast Survey away from its civilian leaders. In the spring of 1818, they succeeded. The United States Congress modified the Coast Survey to expel civilian employees on the grounds of expense and slow progress. Hassler, a civilian naturalist himself, demanded precision and precision took time. Hassler had also moved away from the Washington DC area in order to be closer to his surveying sites. A naval chaplain was able to convince Congress to give him – and the Navy – control of the Coast Survey, in part because none of the civilians were close enough to argue against him in Washington. The Coast

Survey remained under naval control until 1832. Despite promises of greater efficiency and productivity, little work was done in the intervening fourteen years. Eventually Congress reinstated Hassler's civilian, “pure science” practices into the Coast Survey by making him superintendent again. Nonetheless, the threats from the US Army and Navy never quite subsided during Hassler's years.[44]

The legacy of scientific precision did not dissipate with the subsequent change in Coast Survey leadership. Alexander Dallas Bache, the great-grandson of Benjamin Franklin, assumed the superintendency of the Coast Survey in 1843. Bache, like Hassler, would push the premier institution of science in the United States to practice exacting, high-caliber scientific investigations. Such a leader would support the efforts of the philosophical naturalists, as Bache himself belonged to the group of naturalists interested in finding the greater laws of nature.[45] For Bache and the Coast Survey assistants, this pursuit of higher knowledge would also be inseparable from the practical applications of science to problems, such as the production of naval charts.

The rivalries between the Coast Survey and naval institutions did not subside with Bache's appointment, either. Bache was the center of the cliquish Lazzaroni.[46] This group of eminent naturalists exercised considerable influence over people with political power, including members of Congress. A rivalry between Bache and the superintendent of the Naval Observatory grew; the conflict began primarily from a competition for scientific resources and status. The elite Lazzaroni also viewed Maury's scientific practices – as well as his conclusions – with doubt and suspicion.[47] As conflict

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[44] See Slotten, *Patronage*, 49-53.

[45] Slotten, *Patronage*, 63.

[46] Miller, *Lazzaroni,* 4.

[47] See chapter three of this dissertation for an in-depth discussion of Maury's scientific practices.

worsened, the Lazzaroni took steps to keep Maury from his political objectives. Maury – like other American naturalists at the time – had to consider whether his methods would be acceptable for other men of science while working within the fierce milieu of American institutional politics and statecraft.

Bache's desire to assimilate the deep-sea sounding practices developed by Maury's subordinates complicated the strained relationship between the Coast Survey and the Naval Observatory. The Naval Observatory emerged as a cutting-edge site for the acquisition of abyssal sediment samples. This technological development made Maury and his associates influential in discussions over the existence of life in the deep sea.[48] The acquisition of these scientific practices – often done without Maury's knowledge or permission – played into the heated rivalry shared between these two government-sponsored institutions.

Revolutionary and reformist scientific attitudes flourished across the Atlantic. These new naturalists sought to break away from traditional natural history and seek out new laws of nature. They would labor through their local institutions for the change they wished to effect in science. However, their concerns were rooted in the broad desire to push the boundaries of science that permeated many scientific institutions during the nineteenth century.

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[48] The importance of the Naval Observatory in scientific debates is discussed in detail later in this dissertation. Specifically, the Naval Observatory developed technologies to retrieve sediment samples from the deep ocean floor. These samples sparked a debate over the existence of life in the deep sea, known as the biotic debate. Chapter three discusses the development of these technologies, their spread across the Atlantic, and their eventual assimilation into Coast Survey network.

Unity and Diversity: Philosophical Naturalists Across the Atlantic

Naturalists across the globe shared Lamarck's desire to break free from traditional natural history. The historian Philip Rehbock has pointed out two very different uses of the term “philosophic” by naturalists during the mid-nineteenth century. Darwin reflected upon his experiences on the *Beagle* in his 1839 *Journal of Researches* that skeletons of extinct species were often morphologically similar to nearby, living species. Darwin posed this observation as a law of nature, specifically “the law of the succession of types.” This relationship between living forms and extinct forms captured the early-nineteenth century desire to discover natural laws for the living world. As Darwin put it, “The law of the succession of types... must possess the highest interest to every philosophical naturalist.”[49] That same year, the Edinburgh anatomist Robert Knox also used the term “philosophic” to describe the study of skeletons. His translation of a French anatomical lecture used the term “signification of the skeleton.” Because his readers might be unfamiliar with the term, Knox noted, “the term 'signification of the skeleton' belongs peculiarly to the philosophic or transcendental anatomy.”[50]

Darwin and Knox used the term “philosophic” in different ways, despite the texts’ shared subject (skeletal forms) and publication year (1839). The authors' dissimilar usage of “philosophical” suggests that naturalists varied in their exact meaning of the moniker; Darwin used the term to denote a scientific interest in the distribution of species

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[49] Charles Darwin, *Journal of Researches into the Geology and Natural History of the various countries visited by HMS Beagle*... (London: Henry Colburn, 1839; facsimile reprint, New york: Hafner Publishing Co., 1952), 210. Referenced in Philip F. Rehbock, *The Philosophical Naturalists: Themes in Early Nineteenth-Century British Biology*, (Madison: University of Wisconsin Press, 1983), 3. Later editions of Darwin's work omitted the term “philosophical naturalist.”

[50] Henri Marie DeBlainville, “The Comparative Osteography of the Skeleton and Dentar System, in the Five Classes of Vertebral Animals, Recent and Fossil,” ed. and trans. Robert Knox, *Lancet* October (1839): 139. Also in Rehbock, *The Philosophical Naturalists*, 3.

while Knox used it as an alternative word for “transcendental philosophy.” These different usages reflected variations in regional scientific communities, values, and practices; a French philosophical naturalist would be different from a Scottish philosophical naturalist. Nonetheless, one trend unified the people who used the term “philosophical” to describe a man of science, the desire to uncover an underlying pattern in nature. As Rehbock defines the term used by both authors, “For both men... a 'philosophical' worker was one interested in discovering the *laws* of the living world, one who was not satisfied with the mere description of individual beings.”[51]

This search for universal truths of the living world connected a global community of scholars together in their pursuit. However, the search for universal biological laws was not an easy task, and many philosophers of science emerged during the late eighteenth and early nineteenth centuries who addressed this question. Philosophical naturalists from different nations engaged with these notable literary and philosophical figures. The most prominent philosophers in the nineteenth century resided at French and German centers of learning. Rehbock has noted the way in which British philosophical naturalists engaged with Continental philosophies during the early nineteenth century. Specifically, he has argued that early philosophical naturalist pursuits emerged as a result of Idealist philosophies entering Britain from mainland Europe. For example, both Knox and his students shared a philosophical reliance on European transcendental idealism.[52] Transcendental idealism lent itself well to the pursuit of universal laws because it posited that great, “transcending” principles were more important than imperfect, physical observations. This form of “speculative

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[51] Rehbock, *The Philosophical Naturalists*, 4.

[52] Rehbock, *The Philosophical Naturalists*, 17. Transcendental Idealism was most notably espoused by Immanuel Kant in *Critique of Pure Reason* (1781) and *Metaphysical Foundations of Natural Science* (1786).

reasoning" asserted that lesser physical laws should be derived from universal patterns and truths. While transcendental philosophies were interpreted differently by individual philosophical naturalists, they remained powerful suggestions for the pursuit of the "laws of life."[53]

Other historians have embraced the view that Continental philosophies played a central role in the new biological theorizing. Philip Sloan has suggested that "synthetic theorizing," or the combining of philosophical techniques with new scientific questions, was of special interest to Charles Darwin and other self-proclaimed philosophical naturalists.[54] The synthesis mentioned in many of the examples is the pairing of Continental philosophies and British scientific questions.[55] Other historians have kept the search for the laws of nature as part of a Newtonian – and therefore British – heritage. Jon Hodge, for example, draws an analogy between laws of biological change and the laws of Newtonian celestial mechanics. Darwin, as a philosophical naturalist, sought to climb the hierarchy of Newtonian knowledge. The accumulation of minute planetary observations existed at the bottom of this hierarchy. General patterns and laws of planetary motion, such as those discovered by Kepler, stood above base observations. At the top of the Newtonian hierarchy stood the universal laws of attraction and

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[53] Rehbock, *The Philosophical Naturalists*, 17-19.

[54] Phillip R. Sloan "The making of a philosophical naturalist" in *The Cambridge Companion to Darwin,* eds. Jonathan Hodge and Gregory Radick (Cambridge: Cambridge University Press, 2003), 29-33.

[55] Sloan, "The making of a philosophical naturalist," 17 and 33-36. Sloan implicitly takes up Rehbock's assumption that Continental Idealist philosophies were essential to the formation of a philosophical naturalist. More specifically, Sloan attributes much of Darwin's development as a philosophical naturalist to Darwin's scientific hero Alexander von Humboldt, a German naturalist and philosopher.

gravitation. Hodge poses Darwin as an inheritor of the Newtonian impulse to discover universal laws, but within the biological world.[56]

Neither of these interpretations regarding the origin of the philosophical naturalists is incorrect. Some philosophical naturalists wished to follow the tradition of law-seeking laid down by Newton and his English contemporaries. Others found their law-making tools within the Idealist philosophies that dominated nineteenth-century France and Prussia. However, the search for universal knowledge was conducted by philosophical naturalists across the globe despite their disagreement over the best tools for the job or the correct philosophical framework. And over time, new philosophical naturalists blended and reshaped those tools to best uncover the lawful patterns of life. Seen within the greater transatlantic context of scientific unrest in the early nineteenth century, the only thing that unified these diverse and distant philosophical naturalists was their unwavering faith that universal laws of nature could be found. These natural laws were to be prized above the accumulation of base observations.

Rehbock's analysis of the philosophical naturalist community primarily concentrates on naturalists from one institution, the University of Edinburgh. Naturalists from other institutions appear in his analysis, but they are not the subjects of close scrutiny. Consequently, Rehbock's in-depth description of a philosophical naturalist is colored by his subjects. His analysis relies heavily on the influence of German Idealism on his British historical subjects, Edward Forbes and Robert Knox. A broadening of geographical area shows that philosophical naturalists differed greatly in their philosophical dispositions and allegiances. Each community of philosophical naturalists

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[56] Jonathan Hodge, “The notebook programmes and projects of Darwin's London years” in *The Cambridge Companion to Darwin* eds. Jonathan Hodge and Gregory Radick (Cambridge: Cambridge University Press), 50.

varied depending on its geographical position. Indeed, this analysis will demonstrate that these intellectual and practical differences often caused members of the various philosophical naturalist communities to come into conflict.

The nature of this scientific conflict arose from tensions between the assumed universality of natural laws and the particularity of local scientific practices. The philosophical naturalist community, as a whole, shared a belief that universal laws of nature could be discovered. Yet, each community attempted to derive these natural laws in different ways. In one sense, this conflict may not have made sense – or may have been invisible – to contemporary naturalists. Any local observations, if applied to the discovery of universal natural laws, should be comparable with any other observation. This conflict is best illuminated by an analogy to Newtonian celestial mechanics. Certain stars and planetary movements could only be observed from some points of the globe. Similarly, some species can only be found in particular areas of the Earth. Any universal law of attraction should be able to describe the motion of all the planets, no matter where the astronomer might be located. Whether the biologist was a Prussian Idealist or an English Inductivist, any observations regarding the living world would also be accountable to a universal law. For the philosophical naturalist, differences in local scientific practices – if done with fidelity – should not have interfered with the production of global knowledge.

And yet, those local, institutionally-based practices did interfere with the philosophical naturalists' shared ambition. In turn, these differing practices caused philosophical naturalists to deploy and interpret evidence differently. Often, naturalists would observe the exact same natural phenomenon or scientific specimen and come to opposite conclusions regarding its implication. On the other hand, naturalists attempted to resolve these conflicts as the nineteenth century advanced. They debated what methods were best suited to discover natural laws. And they blended the practices from differing locations as they did so.

A firmer understanding of the local institutions and their practices is necessary before it is possible to show how these practices changed over time. The following sections will examine three institutions, the University of Edinburgh, Cambridge University, and the US Coast Survey, and the philosophical naturalists who lived and worked there. While these three institutions contained philosophical naturalists that observed the same geography, the seabed, each studied it within its own institutional milieu.

The remained of this chapter will focus on these three communities and their scientific examination of the seabed. Each section will address how these networks produced evidence for the elucidation of natural law by examining what scientific objects each group found valuable, what practices they performed on these specimens, and how they subsequently derived knowledge from their observations.

The Zoogeologists of the University of Edinburgh

The University of Edinburgh was an exciting place for naturalists at the beginning of the nineteenth century. The major center of the Scottish Enlightenment during the eighteenth century, Edinburgh drew brilliant naturalists to all its institutions of higher learning.[57] A number of new disciplines emerged at the University of Edinburgh during

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[57] Jack Morrell, “The University of Edinburgh in the Late Eighteenth Century: Its Scientific Eminence and Academic Structure” *Isis* 62 (1971): 158-171.

the early nineteenth century as a result. These new areas of research, as well as Edinburgh's reputation as a first-rate medical school, also attracted students to the institution to study natural history under the scientific elite.

Geographically, the University of Edinburgh was located only one hour's carriage-ride away from the Firth of Forth, a natural marine estuary. The University's location next to the firth influenced the naturalists studying and teaching there. For example, the firth became a popular area for collecting the marine creatures that inhabited the rocky shoreline. Pairs of students and instructors often walked the shore and examined the marine invertebrates in the area. Famously, Charles Darwin first began his zoological studies at the Firth of Forth seaside as a young medical student at Edinburgh.[58] This love of collecting marine creatures from the Scottish seaside was also common among the most elite naturalists at the institution, those that held the Regius Professorship of Natural History.

Robert Jameson held the esteemed Regius Professorship at the University of Edinburgh during the early nineteenth century. His ascent to that position shows the origin of marine zoology as an influential, institutional discipline. Jameson's scientific career was guided by a life-long love of marine organisms. His study of marine life guided the geological theories he developed later in life. As a child, Jameson was so struck by “collecting the mollusca and zoophytes on the sea-beach” that he decided to become a mariner. Though his father objected, he did nothing to stop his son from exploring his life's passion. A family friend suggested that he could more fully appreciate the marine creatures he encountered around the world as a student of medicine, whereby many naturalists learned the skills and knowledge needed to study the natural

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[58] Desmond and Moore, *Darwin*, 31-44.

world. He accepted the advice and swiftly became a celebrated pupil of John Walker, the then Professor of Natural History at Edinburgh. Jameson “accompanied by the Doctor himself, went on several dredging expeditions down the Firth of Fourth and often was very successful in obtaining valuable zoological treasures.”[59]

Dredging and marine life were formative aspects of Jameson's medical career. In many ways, dredging was an extension of the naturalist's ability to pick up marine creatures from the seaside. Once a naturalist had gathered creatures from the shallower waters, he would take to a small boat. There, he would lower something resembling an oyster dredge into the water. He would drag the dredge along the sea floor to collect the creatures beyond his arm's reach. Once he was satisfied with his catch, the naturalist would pull the dredge back into the boat to sort through his specimens. Jameson showed considerable skill in gathering these “zoological treasures” from the sea floor. He built his growing reputation as a naturalist on his study of marine creatures.

By 1801, Jameson had traveled to the Bergakademie in Freiberg, Saxony to study geology under the esteemed German geologist Abraham Gottlob Werner, founder of “Wernerian” Neptunist geology. Wernerian theory claimed that most geological formations had been produced by a ubiquitous, global ocean that had receded long ago. Consequently, the Neptunists posited that the study of fossilized marine creatures and the formations of the ancient sea floor were the primary tools for the science of geology. Upon his return, he gave a paper to the Royal Medical Society of Edinburgh on Wernerian geology.[60] Jameson's paper was an important event in his long fascination

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[59] Lawrence Jameson, “Biographical Memoir of the late Professor Jameson,” *Edinburgh New Philosophical Journal* 57 (1854), 6-7.

[60] Jessie M. Sweet, “Introduction” in *The Wernerian Theory of the Neptunian Origin of Rocks: A Facsimile Reprint of Elements of Geognosy, 1808 by Robert Jameson,* ed. George W. White (New York: Hafner Press, 1976), xv.

with the ocean and marine creatures. He later expanded his scientific interests into botany and other topics, but geology and marine zoology continued to be cherished aspects of his scholarship.

In 1804, Robert Jameson assumed the Regius Professorship of Natural History and, by doing so, became a formidable figure in British scientific circles. The position was left in a terrible state after Walker's death. Many of the specimens had been removed from the university's museum. Jameson was also asked to teach the entirety of natural history, which was a larger subject than he had lectured on before. Tasked with rebuilding the museum of natural history and carrying an expansive teaching responsibility, he forged a network of collectors and set out writing textbooks for his lectures.

Four years after assuming the Regius Professorship, Jameson published an influential series on Werner's theory, *System of Mineralogy,* which attracted a scientific and public audience beyond his students. The three-volume series, ending with *Elements of Geognosy*, captured the imagination of many British naturalists, including a good friend Professor Steffens, who crystallized the early nineteenth-century fascination with geology and the sea floor into poetry, “*A Midnight Scene on the Ocean*.- 'Once more,' says Steffens, 'let us rock our imaginations on the bosom of the deep, before we go back to the world of men and things.'”[61] Indeed, Steffens' passionate call for contemplation regarding the ocean and its depths was a reference to Jameson's work. Jameson concluded his first chapter of *Elements of Geognosy* with a discussion on the sea floor:

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[61] Jameson, “Biographical Memoir,” 13.

> Having in the preceding sections described the various inequalities [of geological heights and formations] observable on that portion of the surface of the globe which is elevated above the level of the sea, we may now give a short description of the inequalities discoverable on the *bottom of the sea*, or that part of the globe which is still covered with water.
>
> From the observations of mariners we learn, that the bottom of the sea has very considerable inequalities, and that these correspond, in many respects, to those observed on the surface of the land. Indeed, this must be the case, when we consider that the present dry land was formerly the bottom of the sea.[62]

Jameson's continued fascination with the ocean floor and marine life was an integral part of his geology. Most importantly, according to his theory, all geological features were once the deep-sea floor. Oceanic petrifactions, or marine fossils, offered a grand support for the complete submersion of the Earth's surface.[63] How else would simple marine organisms end up fossilized at the top of mountains? Similarly, layers of rocks and sediments generally expressed the age of the geological formation. Older formations – and their fossils – were usually buried under newer layers of rock. The explanatory power of Wernerian theory drew a variety of naturalists to Jameson's philosophical approach to the study of geology and life.

Jameson and a handful of other naturalists formed the Wernerian Natural History Society in Edinburgh the same year that *Elements of Geognosy* was published. The Wernerian Society was a foundational organization for many prominent and upcoming naturalists, such as Charles Darwin, who attended the Society meetings in late 1826. While the Society discussed a wide variety of subjects, members were more interested in “philosophical” questions pertaining to the natural world, such as the origin of rocks or

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[62] Robert Jameson, *The Wernerian Theory of the Neptunian Origin of Rocks: A Facsimile Reprint of Elements of Geognosy, 1808*, ed. George White (New York: Hafner Press, 1976), 24. Original emphasis.

[63] Jameson, *The Wernerian Theory*, 80.

living forms. Many of these naturalists studied marine life, but even those who studied surface phenomena were tied to the sea floor by the organization's namesake, Neptunist theory. They would also be influenced by Jameson's curricular influence at the University of Edinburgh.

Jameson's three-month course on natural history included the standard fare of mineralogy, paleontology, zoology, and botany. However, a substantial portion was also devoted to other subjects, such as meteorology, hydrography, and zoological philosophy. The fourteen lectures on zoological philosophy covered such topics as the "origin of the Species of Animals," "Distribution of Animals, Both physical and geographical, over the surface of the Earth, in the waters of the ocean...," "the connection of the Animal with the Vegetable Kingdom," and "Lastly, The mutual relations that exist amongst all the objects of nature, and those general laws that appear to be common to the whole."[64] Jameson's lectures on zoological philosophy institutionalized a search for the lawful patterns of life at the University of Edinburgh. These lectures were accompanied by instruction on dredging for his students, which would later be shared among the students themselves, effectively articulating the practices of dredging with the desire to uncover the natural laws of life.

Other prominent naturalists, especially Jameson's students, adopted the focus on marine life and the deep sea promoted by Jameson's Wernerian Society. Edward Forbes, Jameson's protégé and eventual successor to the Regius Professorship, connected the Edinburgh fascination with dredging and marine biology to Parisian

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64 University of Edinburgh, *Commissioners for the Universities of Scotland, 1826 and 1830. Evidence, vol. I*. (London: 1837), appendix, 115-118. A complete list of the zoological philosophy lectures given by Jameson may also be found in Philip F. Rehbock, "Organisms in space and time : Edward Forbes (1815-1854) and new directions for early Victorian natural history" (PhD diss., Johns Hopkins University, 1975), 118.

"philosophy of natural history." Like Jameson, Forbes had a deep love of nature from an early age that led him to study medicine at the University of Edinburgh. When Forbes took Jameson's course during the summer and spring of 1832 he was captured by a fascination with marine invertebrates and oceanic dredging that would change his life.[65]

By 1836 Edward Forbes had traveled to Paris to study under the master "philosophical anatomist," Etienne Geoffroy Saint-Hilaire. There Forbes incorporated philosophical studies of nature directly from the Parisian anatomists and brought it back to the Edinburgh community. A lecture series given by Forbes at Edinburgh titled "On Zoo-geology and Psycho-zoology" was intended to promote "more philosophical, and therein more intelligible, ideas than are commonly prevalent, of the great principles and central facts of natural sciences."[66] This lecture on Zoo-geology tied the new *biologie* to geology much like Jameson had done through *Elements of Geognosy* thirty-two years earlier.

Forbes' Zoo-geology lecture occurred only a year after a turning point in his career. Sometime around 1839 Forbes met with a sea floor dredger James Smith and accompanied him on a dredging expedition to the Clyde District of Ireland. Smith postulated that marine fossils found in the area resembled currently living specimens further north. These extinct fossils were very similar to cold-climate organisms found in the colder, northern waters, suggesting that the now-extinct species may have migrated due to environmental pressures. Forbes was deeply impressed with Smith's proposal that the changing environment of the sea floor geography could affect living forms within

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[65] Rehbock, *The Philosophical Naturalists*, 68.
[66] Rehbock, *The Philosophical Naturalists*, 69.

the region.[67] This new zoogeology had a profound impact on the Edinburgh enclave and by 1842, Forbes' notoriety had earned him the professorship of botany at King's College, London. While the professorship was technically in botany, Forbes continued to be an active dredger and marine zoogeologist. One biographer noted that:

> Although so long Professor of Botany, Edward Forbes, after he came to London, did almost nothing in the way of strictly botanical work. He read a few minor botanical papers at the British Association. But his most important contribution to botany arose not from the botanical but from the geological side.
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> It was chiefly in the domain of zoo-geology, to use his own phrase, that he passed his public life; and it is mainly as a zoo-geologist or palaeontologist that he will take rank in the annals of science. His palaeontological work, however, did not consist of mere descriptions of fossil species. These he regarded only as part of the preliminary ground-work of palaeontology, and he ever strove to rise above them to the broader scientific questions to which they led.[68]

Forbes continued to link geology to the new *biologie* well into his career at King's College. Specifically, as his biographer was careful to note, Forbes' zoogeology was part of an effort to address more philosophical questions than “mere descriptions.” His research remained focused on geographical features of the sea floor and marine fossils, much like the Edinburgh naturalists that taught him. The connection between the historical marine floor and the distribution of oceanic life carried the same weight that it did for his mentor. When Robert Jameson died in 1854, Edward Forbes was elected to the Regius Professorship at Edinburgh. His ascension speaks to the deep institutionalization of – and prestige associated with – the studies of geology, dredging, and marine organisms at the University of Edinburgh.

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[67] Philip F. Rehbock, “The Early Dredgers: 'Naturalizing in British Seas, 1830-1850,'” *Journal of the History of Biology* 12 (1979): 315.

[68] George Wilson, *Memoir of Edward Forbes, FRS.: Late Regius Professor of Natural History at the University of Edinburgh* (Edinburgh: Edmonston and Douglas, 1861), 537.

The zoogeological practices of Forbes and his fellow Edinburgh philosophical naturalists spread throughout the British Isles through the British Association for the Advancement of Science. When the BAAS met in Edinburgh in 1834, the naturalists there wondered why the Association paid little emphasis to zoological studies.[69] Edinburgh, being the center for natural philosophy and sea floor dredging, began to reshape the BAAS in order to coordinate and fund dredging activities for its members. A section for zoology and botany (Section D of the BAAS) had already been included in the structure of the organization; however, no funding had been given to this section yet and it had limited activity. By 1836, Edward Forbes and his dredging associates filled the British Association's zoological vacuum. They used Section D as a way to gather the many dredgers around Britain, teach new naturalists to collect from the ocean floor, and share the knowledge gained from their research. In 1839, Section D finally received formal funding through the formation of the Dredging Committee of the BAAS. The formalization of the Committee was prompted by a report given by Forbes to Section D. The very first act of the Committee was to print blank forms for dredging naturalists to specify the biogeographical context of their samples. For his work, Forbes remained a central member of the Dredging Committee and was elected President of the Geological Society in 1853.[70]

The Dredging Committee, under Forbes, focused on adding zoogeological context to the creatures they pulled off the sea floor. Section D's promise to print biogeographical forms for dredgers coincided with the turning point of Forbes' career, when he dredged off the Clyde district of Ireland with James Smith. Not only did Forbes

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[69] Rozwadowski, *Fathoming*, 109. The British Association for the Advancement of Science was only two years old at that point.

[70] Alison Rice and J Wilson, “The British Association Dredging Committee: a brief history,” In *Oceanography: the past*, ed. Sears (New York: Springer-Verlag, 1980), 376.

use the BAAS to expand the number of dredgers in Europe, he also used the network of BAAS Dredging Committee members to explore the temporal and environmental contexts of simple marine invertebrates.[71] Through dredging, Edinburgh zoogeology spread across Britain and into the oceans. Far from an isolated practice, whole communities, including Jameson's students and Forbes' Dredging Committee, worked to promote techniques to explore the ocean floor and, through it, the Edinburgh version of *biologie*.

Evidentiary Practices at the University of Edinburgh

Some themes emerge when examining the network of seabed naturalists at Edinburgh. These philosophical naturalists shared an entry point into biology through Jameson, his lectures, and the organizations he led – primarily the Wernerian Society. A study of Forbes shows the continuity of practices between the succeeding Regius Professors of Natural History at Edinburgh. Chapter two will continue this analysis to two other of Jameson's students: Robert Grant and Charles Darwin. Each of these naturalists, like the other Edinburgh zoogeologists, shared a number of evidentiary practices, such as an interest in simple marine invertebrates, seafloor dredging, and a loose Continental Idealist method for deriving patterns from their specimens.

Many Edinburgh zoogeologists used marine invertebrates in their scientific research. Marine creatures had interested both Walker and Jameson, key members of the Edinburgh naturalist community. The University's proximity to the Firth of Forth facilitated the spread of Jameson's scientific enthusiasm for marine life. The institutional

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[71] Rehbock, “The Early Dredgers,” 293-368.

effect of that fascination can be explored in the scientific efforts of his students, such as Forbes. Zoophytes, or “animal plants,” played a large role in zoogeological studies. These seabed creatures, such as corals and sea anemones, would grow much like marine plants, but functioned biologically like animals. These simple creatures helped zoogeologists to explore the boundaries between the plant and animal kingdoms.

Other simple marine invertebrates helped the zoogeologists to reason through patterns in the natural world. Rehbock has noted Forbes' study of British fossil echinoderms done through the Geological Survey. Specifically, Forbes used crinoids and cystoids to demonstrate the natural order of species. The simpler crinoids were situated below the more-complex cystoids. By extension, the cystoids represented the middle of a branching hierarchy between other complex echinoderms. However, the cystoids were believed to be completely extinct in the modern era. Forbes believed that the echinoderms demonstrated polarity between plant-like organization, as illustrated by the crinoids, and animal-like complexity, as illustrated by the cystoids and the extant echinoderms.[72]

Forbes believed the same polarity could describe the appearance of new fossil species in the geological record. A great number of new species appeared in very old sedimentary rocks and in recent formations. Middle portions of the geological record showed relatively few new species. Forbes speculated that there existed a polarity between older, simple forms and newer, complex forms. Some naturalists hailed Forbes'

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[72] Rehbock, *The Philosophical Naturalists*, 103.

concept of polarity as a brilliant uncovering of natural law. Most remained unconvinced by Forbes' conclusion, but were nonetheless impressed by his speculative prowess.[73]

The ordering of crinoids and the other echinoderms also challenged the traditional *scala naturae* espoused by natural history. The *scala naturae* ordered natural objects from the least perfect to the most perfect, where the most complex plants would be similar to the simplest animals. Forbes' study of crinoids and the zoophytes challenged that traditional understanding, “Thus, instead of finding, as we might expect *a priori*, the most perfectly developed vegetable bearing the closest resemblance to the lowest animal form, we find, on the contrary, that it is at the lowest points of both systems (the sponges, &c, in the one, and marine fuci [algal seaweed] in the other) that the closest resemblance exists.”[74] Marine invertebrates, especially the crinoids, gained a special status as a boundary between the plant and animal kingdoms, allowing them to be deployed in the future as evidence to explain the order and organization of species. For example, the crinoids so prominent within Forbes' zoogeology would play an essential role three decades later during the adjudication of natural selection.[75]

The combining of zoology with geology necessitated a blending of scientific practices as well. The study of geology continued for the Edinburgh philosophical naturalists. Wernerian geology dominated the group at first. Even after the slow receding of Wernerian geology during the nineteenth century, geological practices remained linked to marine zoology. The geologist's hammer remained as important for the

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[73] Rehbock, *The Philosophical Naturalists*, 104. Darwin remained unconvinced of Forbes' idea of polarity, believing that it was “unintelligible.”

[74] Edward Forbes, “On some Important Analogies Between the Animal and Vegetable Kingdoms,” *Athenaeum*, 1845, 199. This quote can be found in Rehbock, *Philosophical Naturalists*, 72.

[75] See chapters four and five of this dissertation about the crinoids' role in the Darwinian debates.

zoogeologists as the naturalist's dredge. The Edinburgh group would continue to gather fossilized remains of marine creatures by plundering the terrestrial rocks and the sea floor sediments. Both scientific methods of gathering specimens remained entrenched within the University of Edinburgh network.[76]

The use of the naturalist's dredge informed the development of Forbes' theories as much as his blend of Continental transcendental philosophies. Rehbock has noted that Forbes was influenced by Idealist philosophers who saw underlying patterns as more important than the details of the physical specimens themselves.[77] However, Forbes did not practice a pure form of German or French Idealist philosophy. His scientific practices, such as dredging, merged with his scientific philosophy to produce a novel, Edinburgh style of science. Forbes' azoic zone hypothesis illustrates this blending of practice and philosophy into an institutional evidentiary practice.

The azoic zone hypothesis was the belief that no life existed below 300 fathoms underwater. Forbes had joined an expedition to the Aegean Sea aboard the HMS *Beacon*. The crew took over 100 dredges from the Aegean from shallow water up to 230 fathoms, though the majority were under 60 fathoms deep. Forbes recorded the type of organism and its distribution at each instance. Key among his findings was that life diminished with depth. Forbes reported his finding before the 1843 British Association for the Advancement of Science meeting:

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[76] The building of common scientific practices served to bring the Edinburgh network together and signify individual status and prestige. See Thomas Gieryn's concept of “boundary work” for instances of how scientists navigate internal authority and prestige. While Gieryn's historical examples are mainly geared towards modern, institutional science, he does cover a general principle that helps the early-nineteenth-century historians think about scientific prestige and power. Thomas F. Gieryn, "Boundary-work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists," *American Sociological Review* 48 (1983), 781–795.

[77] Rehbock, *The Philosophical Naturalists*, chapters one and three.

> ...we have seen that the diminution in the number of species and of individuals as we descend in this lowest region pointed to a not far distant zero; therefore the greater part of this immense under-deposit will in all probability be altogether void of organic remains...[78]

Few naturalists had the opportunity to dredge so routinely in the open sea. Therefore, Forbes' speculation was difficult to challenge. In addition, his reputation as an excellent naturalist and well-connected individual helped to circulate the idea that no life existed below the 300 fathom line. And while the claim was intended as a preliminary observation, Forbes' death in 1854, not long after accepting the Regius Professorship, prevented him from further investigations of the azoic zone. The hypothesis quickly acquired the status of an unchallenged fact.[79]

Forbes' “discovery” of the azoic zone illustrates the blending of entrenched dredging practices in zoogeology with Continental Idealist philosophies. Actual dredging below 300 fathoms was not required to establish the existence of an azoic zone. Rather, the diminishing of life with increasing depth was deployed as evidence enough to convince people of the logical endpoint of this natural trend. Many British and American naturalists were comfortable enough with this line of reasoning to accept Forbes' hypothesis as a theory. Even naturalists who would not have been considered zoogeologists were swayed by Forbes' scientific conclusion. Such a theory would have circulated even faster among Forbes' scientific network, whose members shared experience dredging and an appreciation for marine invertebrates as evidentiary objects.

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[78] Forbes, Edward. “Report on the Mollusca and Radiata of the Aegean Sea, and on their distribution, considered as bearing on Geology.” in *Report of the Thirteenth Meeting of the British Association for the Advancement of Science, held at Cork in August 1843* (London: John Murray, Albemarle Street, 1844), 130-193.

[79] See Rozwadowski, *Fathoming*, 137-142 for a discussion of early British detractors from the azoic zone hypothesis. These skeptics were generally not taken seriously until the 1860s and 1870s.

Despite the distinct practices shared by the Edinburgh zoogeologists, Forbes acknowledged that their philosophical naturalist brethren could be found spread throughout the Atlantic. And through their shared impulse to find the laws of nature, they would by necessity act in unison – if not uniformity – throughout the nineteenth century. Forbes made this point clear in his 1 November 1854 introductory lecture in Natural History, the lecture post that he inherited from Jameson:

> The eminent men who have gone before me held that the student who aims at being a naturalist, in the proper sense of the word, must combine biological with geological knowledge. For the same view I most strenuously contend. It was the doctrine held and practiced by Linnaeus, by Cuvier, by Blainville, by Brongniart; and at the present day by such men as Owen, Darwin, and Falconer, all formerly Edinburgh students; by Agassiz, Loven, Phillips, and Dana. A philosophy of natural history can only spring out of this combination, and can never be evolved from the exclusive study of isolated sections.[80]

Such a strong statement as to the "proper" method for uncovering natural laws would become a battle cry against other networks of naturalists at the time. Most notably, an influential group from Cambridge University would oppose this "speculative" brand of philosophical naturalist methodology. And despite their shared desire to uncover the universal laws of nature, the Cambridge group would resist the methodologies of the Edinburgh network, and develop their own ways of studying the seabed.

The Astronomers and Tidologists of Cambridge University

Cambridge had a very different reputation from its Scottish counterpart. The University was established in during the High Middle Ages and it retained the dignity that

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[80] Edward Forbes, "Introductory Lecture Delivered at the Opening of the Natural History Class in the University of Edinburgh, on Wednesday, 1st November 1854," *Edin. New Phil. Journ., n.s.* I (1855), 156-157. Also cited in Rehbock, "Organisms," 245-246.

its history commanded. Cambridge had educated the sons of aristocracy and the English gentry for generations, and it remained a bastion of learning for civil servants and the clergy as it had the centuries before. It had attracted great scientific minds, such as those of Sir Isaac Newton and Sir Francis Bacon. In the eighteenth century, however, its reputation declined. Afterward, Cambridge took a long time to catch up with less venerable universities in the search for philosophical truths about the living world. As historian A. Rupert Hall has remarked, it was not until the foundation of the Cambridge Philosophical Society in 1819 that, "the first positive step [was] taken in modern times towards the emergence of Cambridge University as a great centre for teaching and research in science. It was the first move from almost a century of indolence and dullness during which, if Newton's name had been revered, his own example of relentless intellectual activity had rarely been followed."[81]

The scholars of Cambridge University concerned themselves with the "eternal sciences" of mathematics, astronomy, and theology.[82] This intellectual context aligned well to the pursuit of natural laws. Isaac Newton, Lucasian Professor of Mathematics at Cambridge University a century and a half before, had astounded the scientific world with his discovery of the law of gravitation, which explained the movement of all celestial bodies. Many naturalists sought to follow Newton's example and discover other universal laws. Newton's legacy also carried an affinity for the use of mathematics and minute computations. Such attention to mathematical detail would yield great dividends for astronomical research during the early-nineteenth century.

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[81] A. Rupert Hall, *The Cambridge Philosophical Society: A History* 1819-1969, (Cambridge: Cambridge Philosophical Society, 1969), 1.

[82] Sadly, I cannot claim the term "eternal sciences" as my own creation. The term is borrowed from a personal conversation had with historian Jon Hodge. His term perfectly captures the intellectual context found at Cambridge University at the beginning of the nineteenth century.

The early-nineteenth century saw a group of Cambridge scholars emerge in this milieu of mathematical precision and astronomical calculation. The astronomer and musician William Herschel had expanded the known solar system in 1781 with the discovery of Uranus, the seventh planet. Uranus' distance from the sun causes it to complete its solar orbit considerably more slowly than the Earth. Observations of Uranus proceeded piecemeal because of its slow orbit, but, by the nineteenth century, discrepancies began to emerge between the calculated position of the planet and its actual position.[83] By 1845, two separate astronomers took interest in these orbital discrepancies. Such a discrepancy would challenge Newtonian celestial mechanics or prove that there might be other, previously unknown planets in the solar system. John Couch Adams, a recently graduated senior wrangler from Cambridge, was one of the astronomers who investigated Uranus' strange orbit.[84] Adams was convinced that an undiscovered planet was responsible for disturbing Uranus' predicted path around the sun. Adams began the difficult task of calculating the position of this mysterious planet.[85] His Cambridge education – steeped in a history of mathematics and astronomy – served him well in his calculated search for the unseen celestial body.

Adams labored over the Uranus calculations. He requested observations from James Challis, the director of the Cambridge Observatory, as part of this work. Challis, in turn, forwarded the request to George Biddell, the Astronomer Royal at the Greenwich Royal Observatory. In September of 1845, Adams shared a portion of his work with

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[83] Modern astronomers believe that Uranus takes nearly 85 years to complete its orbit around the sun. That would place its full orbit around to the location it was first discovered around 1865.

[84] See Tom Standage, *The Neptune File: A Story of Astronomical Rivalry and the Pioneers of Planet Hunting* (New York: Walker & Co., 2000), for an excellent, readable account of Adams' search for the undiscovered planet.

[85] Robert W. Smith, “The Cambridge Network in Action: The Discovery of Neptune,” *Isis* 80 (1989), 395-422.

Challis, who recognized the effort put into the calculations, but doubted Adams' methods. The search for another astronomical body would require intensive observation. Challis was not convinced that Adams' calculations would yield a discovery.[86] Not long after this interaction, Urbain Le Verrier, a French mathematician, independently noticed the deviance of Uranus' theoretical orbit from its observed positions. In June of 1846, Le Verrier announced the theoretical position of a new planet based on his calculations. The new planet, Neptune, was exactly where he predicted.[87] Cambridge had missed its opportunity claim the astronomical discovery for itself.

The English naturalist community would remain sensitive about the Neptune event for almost a century. Cambridge was affected most by the oversight. Their reaction demonstrates the pride that Cambridge naturalists had in its tradition of minute astronomical calculations combined with grand Newtonian law. It also reflects the type of evidence that the Cambridge network would expect to yield discovery. The discovery of Neptune proved to be a great success for mathematical prediction. It also demonstrated the power of conclusions based on the accumulation of quantitative data, even if the success had not served Cambridge's reputation. And going into the later half of the century, the Cambridge network would labor to ensure that they did not miss such an opportunity again.

The Cambridge emphasis on astronomical calculation also drew a number of scholars together. Other naturalists recognized the network's shared legacy of celestial mechanics. The indebtedness of the Cambridge network to their institutional milieu came out in various correspondences, sometimes quite inadvertently. As one example, in

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[86] Smith, “The Cambridge Network in Action,” 401-402.

[87] Le Verrier's calculations were only off by one degree of Neptune's predicted location. The observations based on Le Verrier's calculations were done at Berlin.

1831, during the formation of the British Association for the Advancement of Science, Edinburgh naturalists recognized that they would have to share control over the organization. One of these naturalists wrote in fear that – should the Association's formation be further tied to Edinburgh naturalists – the action would deter the Cambridge naturalists from participation, or “might nearly... be the downfall of the association, if the whole constellation of talent at Trinity [one of Cambridge's colleges] were thus to be withheld from fostering so infantile a project.”[88] The Edinburgh naturalist recognized the Cambridge network as a loose “constellation of talent,” the wording associating them – perhaps subconsciously – with the study of the stars.[89] This core group of Trinity men would also be the same network that acted as a central authority for seabed science a few years later.

Trinity College had a unique identity within Cambridge's rich history of physics and astronomy. This college had an intimate, historical relationship to Sir Francis Bacon, the seventeenth-century Lord Chancellor of England; the young Bacon attended Trinity College in the late sixteenth century and, later, became an esteemed figure for Cambridge scholars. In his 1620 treatise *Novum Organum Scientiarum*, Bacon had laid out a vision for a new scientific methodology: inductivism. The *Novum Organum* critiqued Aristotelian syllogism and advocated for an alternative logical tool, the minute observation of natural phenomena and logical discovery of phenomenological causes. As a vision for science, Bacon's teachings offered a British historical figure that guided science away from *a priori* assumptions and focused on measurement, observation, and

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[88] University of St. Andrew's Archives. James David Forbes to John Phillips, 3 August 1831. The quote may be found in Morrell and Thackray, *Gentlemen*, 72.

[89] I have adopted this witty turn of phrase to describe the Cambridge network centered around William Whewell. It refers to the astronomically minded scholars at Trinity and their associates. The phrase also situates the Cambridge clique within the rich history of astronomical study describes later in this section.

the careful exclusion of potential phenomenological causes. Trinity naturalists saw themselves as the inheritors of the Baconian tradition and inductivist thought. For example, the polymath William Whewell became master of Trinity College in 1841. Four years later, a marble statue of Francis Bacon, crafted by one of the most successful sculptors of the nineteenth century, was erected in the Trinity Chapel. The early-nineteenth century was a period of renewed identification with Baconian scientific reform for the Trinity scholars.

The Cambridge Philosophical Society was one institutional mechanism by which faculty expressed their desire to transform science at their institution. The early members of the Society came from a diversity of scientific backgrounds and interests, from botany to astronomy. The divisions between the sciences were less pronounced in many cases and some naturalists had broad interests. Despite their differing fields, they shared the historical legacy of Cambridge and a desire to redirect the course of scientific investigation starting with their own university.[90] The combination of reformist intent and historical context surrounding the Cambridge group created a pronounced institutional milieu. Much like the University of Edinburgh's proximity to the Firth of Forth, Cambridge University's context as a historical site for celestial calculation and Baconian inductivism influenced the naturalists residing there. This influence was quite noticeable even in seemingly unrelated subjects, such as the study of the seabed.

Whewell and his students duplicated the practices that yielded earlier Cambridge scholars so much success in physics and celestial mechanics in their study of the historical seabed. They entered into this study through a predictive study of the tides. The movement of the tides had been an unresolved problem for naturalists. Before

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[90] Hall, *Cambridge Philosophical Society*, 10.

Newton, the surge of water upon the British Isles had seemed inexplicable. Newton and Edmond Halley managed to bring the tides into the realm of rationality through the laws of gravitation. The sun and the moon could draw water towards themselves even at a distance. Newtonian mechanics explained the cause of the tides, but it utterly failed at predicting the actual high tides at British ports. Newton's law of attraction did little for naval captains and dockyard administrators. Even the Thames, the tidal river that led to London ports, still regularly wrecked mighty ships with its fickle tides. Enterprising publishers printed tide tables in their almanacs for captains and dockworkers. However, the tide measurements conducted by these associate laborers, the everyday practitioners of tidal knowledge such as tide table calculators and colonial dockworkers, seemed to contradict the natural laws set out by Newton himself.[91] The problem of the tides combined celestial attraction, Newtonian law, minute measurements, and scientific prediction – all the rightful territory of the Cambridge elite. And in 1828, when the Society for the Diffusion of Useful Knowledge was accused of publishing faulty, unscientific tide tables, the organization turned to a Cambridge mathematician to bring the light of science to the London tides.[92]

The man they approached was John Lubbock, a graduate of Trinity College and a former student of Whewell.[93] Through his Trinity education, Lubbock had been directly exposed to the French analytical mathematics promoted by Whewell and Herschel. Such training was useful for astronomical calculations necessary to predict the pull of the

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[91] Michael S. Reidy, *Tides of History: Ocean Science and Her Majesty's Navy* (Chicago: University of Chicago Press, 2008), 20.

[92] Reidy, Tides of History, 62.

[93] There are a number of “Sir John Lubbocks” in British history. I refer specifically to Sir John William Lubbock, 3rd Baronet who was born on 26 March 1803 and died 21 June 1865. He was the son of Sir John William Lubbock and also the father of Sir John Lubbock. The Sir John Lubbock mentioned in this chapter is the same who bought a residence near Charles Darwin later in life.

moon on tidal waters, the crucial step needed to introduce science into a study of the tides. Lubbock's training in mathematics had practical uses as well. Lubbock would later find employment in finance. In addition, he also had financial interest in the London docks through an investment, providing extra incentive for him to agree to the Society's request.

Lubbock could have examined the tides and the littoral as a geological question first.[94] Instead, he defaulted to the practices that he had learned from Whewell at Trinity. In part, Lubbock's starting point was influenced by the previous methods of producing tide tables. Instead, Lubbock tied astronomical calculation to a prediction of the tidal high waters. After, he used observations of when high tide actually occurred to correct his mathematical predictions. Such a technique is practically similar to Cambridge astronomers' later prediction of Neptune's position. Through Lubbock, the nineteenth-century study of the tides reemerged as an astronomical problem to be solved by prediction, observation, correction, and intensive calculation.

Lubbock instructed his hired calculator to create a table that compared high tide to the position of the moon. This table was then used to correct the predictions of high water in London harbor. The table itself demonstrates the direct combination of astronomical calculation with hydrographical prediction; the two subjects literally existed next to each other on the same page. These calculations became the foundation of

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[94] There may have been many different ways to begin a study of the shifting tidal zone. During the 1830s, geologists such as Charles Lyell even disputed whether the littoral sea floor – or the sea floor covered by tidal waters – remained constant over time. The frontispiece of Lyell's 1830-33 treatise, *Principles of Geology*, depicted the Temple of Serapis in Italy. The columns displayed evidence of having been submerged for long periods of time. The series of columns showed bands of erosion from tidal action. These bands were far above the current high tide. To Lyell, the temple pillars implied that the tides were not constant over long periods of time because the sea floor elevation was not constant. Lyell provides just one alternative method for studying the littoral sea floor.

Lubbock's future tidological method. He reported his results to the Royal Society in June of 1831. His report, “On the Tides in the Port of London,” claimed that while the calculations had been done by his hired assistant, Lubbock alone was responsible for the “arrangement of the Tables, and the methods employed...”[95]

Lubbock continued to publish his tidological research in the Royal Society's *Philosophical Transactions* from 1831 to 1832. His methods also appeared in a *Companion to the Almanac* published through the Society for the Diffusion of Useful Knowledge.[96] The study was of obvious utility and scientific interest to the members of the Royal Society, which contained a large number of people with naval training at the time. The Society awarded Lubbock the Gold Medal for his contribution to science.[97]

However, not everyone was satisfied with Lubbock's intensive focus on London harbor and the Thames. Lubbock had drawn his mentor Whewell into the study of the tides during the early 1830s.[98] Whewell encouraged Lubbock to broaden his research and produce a full treatise on the subject. To date, there had been no sustained work on tidal theory. If Lubbock expanded his work, he would be able to make claims about the natural law of the global tides, not simply predict the tides in London. Whewell saw the perfect opportunity to apply his scientific practices to uncover natural laws. When his former student failed to widen the scope of his research, Whewell began a sustained,

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[95] John W. Lubbock, “On the Tides in the Port of London,” *Philosophical Transactions of the Royal Society of London*, 121 (1831): 379-415.

[96] Reidy, *Tides*, 81, 98.

[97] Reidy, *Tides*, 100.

[98] In 1833, Lubbock asked Whewell for help in securing funds for his hired calculator Joseph Foss Dessiou. The issue of funding the calculation of the tides caused a minor conflict between Lubbock and William Stratford, the new Superintendent of the *Nautical Almanac,* who refused to pay for Dessiou despite Lubbock's insistence. Whewell also provided what information he had about tides to Lubbock to assist Lubbock's research. The two continued a sustained correspondence of the subject of tides and measurement during the early 1830s. See Reidy, *Tides*, 110-111.

twenty-year project to uncover how the tides worked.[99] Unlike Lubbock, if Whewell was to make larger claims about natural law and the littoral zone, he would have to deal with other scientific practices used to research the same area, such as research as to whether the sea floor rose or subsided over time.

Evidentiary Practices at Cambridge University

Contemporary naturalists acknowledged the existence of a Cambridge network, already referred to here as the "Trinity Constellation." Whewell and his close associates were included in that group. Whewell acted as a central figure in that network; he was physically located at Trinity, giving him constant contact with the Cambridge Philosophical Society membership. He also maintained close ties to prominent naturalists, such as John Herschel and Charles Babbage, with whom he had regularly shared breakfast while an undergraduate student.[100] Whewell also taught at Trinity and attempted to maintain an active professorship at Cambridge. His educational activities brought in a network of students, including Lubbock, to whom he directly taught the French mathematical techniques that Whewell promoted at Cambridge. In the case of tidology, the student was the first to explore the subject, while the teacher followed a few years later. However, both teacher and student employed the scientific practices promoted at Cambridge – by Whewell himself – despite the differing scope of their projects. Whewell applied the techniques to the discovery of natural law while Lubbock constrained his study to the tides of one location, London harbor.

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[99] Reidy, *Tides*, 126.

[100] Snyder, *Philosophical Breakfast Club*, 19-43.

Lubbock's study of the tides may make a poor example of seabed science. He was interested in abstracting the tidal observations he collected into general mathematical principles. That aim, while constrained, was still within the scope of the goals of a philosophical naturalist. Yet, the study of the influx and recession of tidal water does not examine the seabed within an extended historical context. The tidal tables examined by Lubbock only extended back a few decades at most. Perhaps the contemporaneousness of Lubbock's early tidology is noteworthy in itself; he assumed that coastline and sea floor would be constant, even though he was aware that the tides changed over time.[101] His desire to produce accurate tidal predictions for the Society for the Diffusion of Useful Knowledge may have guided that early assumption. Whewell was not constrained by the same need for immediate results. A study of the tides could demonstrate the usefulness of science and support the funding of science by the English government. Whewell was not under pressure to produce tide tables for publication like his student. He was free to abstract tidal theory to whatever extent he felt was beneficial.

Because Whewell attempted to uncover more global laws of the tides, he was forced to consider the sea floor's history to a greater extent than Lubbock. One clear example of Whewell's engagement in seabed science is his request for funding from the British Association for the Advancement of Science. In 1833, a subcommittee consisting of Whewell, Lubbock, the Cambridge geology professor Adam Sedgwick, and others was granted, “A sum not exceeding 100£... to the procuring of satisfactory data & measurements towards the determination of the question of the permanence or change

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[101] The building of London Bridge had even changed the levels of the tides. And, as Reidy explains in *Tides of History*, the Thames in London had changed considerably over the eighteenth and nineteenth centuries. Each of the changes to the littoral zone had changed how the local tides operated.

of the relative level of sea and land on the coasts of Great Britain and Ireland..."[102] The proposed study used sustained tidal measurements to determine whether the sea floor rose out of the water – or global sea levels receded – over time. The calculations would not be as complicated as predicting future high tides; only the mean high and low tides would be used in the comparison. Nonetheless, the same practice of precise, sustained measurements, tabulation, and calculation used in creating the tide tables would be applied to study the historical sea floor.

Whewell outlined his research plan clearly for the British Association members, giving historians access to the exact practices he intended to employ for studying the sea floor:

> ...for the purpose of deciding the question whether any change is at present going on in the relative level of the land and the sea valuable and important observations might be made at Lighthouses situated on rocks or at steep cliffs & not much elevated above the water... For the purpose it would be necessary to obtain a series of observations of the height of the sea at high & low water every day for a considerable period as for instance two or three years, the height of the water being measured with reference to a fixed point on the land.[103]

The daily tide observations were to be conducted by the people stationed at lighthouses around England and Ireland. The installation of tidal registers was something that already interested Whewell. The regular recording of high and low tide around England would help his ongoing studies of tidology. The distribution of tidal observers would also add to his eventual network of observation stations that were necessary for a global study of tidal dynamics. Whewell fell back on his experience in tidology to answer the

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[102] BODL MS DEP BAAS 60, 4. This investigation was likely prompted by the recent 1830-1833 publication of Charles Lyell's *Principles of Geology*.

[103] BODL MS DEP BAAS 60, 7.

central question facing other seabed naturalists during the early nineteenth century, “does the sea floor move and, if so, in what way?”

Whewell suggested that the study of the sea floor would be facilitated by the installation of tidal registers at select coastal locations. The device would measure the effect of tidal action, or the pressure and height of waves upon the shore. Whewell left a detailed description of the device in his proposed study of the sea floor and how it was to be employed:

> ...This apparatus would consist of a measuring rod or line which being connected with a float as the surface of the water should mark the ascent of a moving point above or its descent below a fixed index... This index being at a given figure of the measuring rod or line the vertical distance of the fixed index from the surface of the sea must be determined; by this means the vertical direction of the index from the surface of the sea for any other figure of the measuring line will be known. Also the vertical distance of the fixed index from a given mark in the rock or land must be determined. The comparison of these measures will enable us to refer all the heights of the water to the mark on the land & therefore to refer the mean of all the heights to the same mark... This may... determine whether the mean heights of the water changes with reference to the mark on the land.[104]

The device would be used to generate the precise measurements of the relationship between the sea floor to the sea surface. With it, his affiliated workers would tabulate the position of the high tide to the land and send this information to a “suitable authority.”

From a modern perspective, Whewell's proposal might seem perfectly natural. However, the practice used to study the sea floor was far from undisputed. Whewell's proposal stands in stark contrast to a proposal put forward by the Oxford geologist John Phillips, a naturalist mentioned in Robert Jameson's list of prominent zoogeologists.

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[104] BODL MS DEP BAAS 60, 8.

Phillips also appealed to the British Association in 1840 to conduct a study of ancient sea margins, which he entitled “Ancient Sea-Margins as Memorials of Change in the Relative Level of Sea and Land.”[105] While these naturalists dealt with different phenomena, they both wished to determine the positioning of the sea floor over time.

Unlike Whewell's use of the tidal measurement, Phillips suggested the use of geological features to determine the ocean's past position. Specifically, the use of organic sediment left from tidal action, or detritus, could be used to uncover where ancient beaches and shallow bays had been in the past:

> The existence of marine detritus of various kinds at great elevations is recognised as proof of a deep immersion of the land under the sea at a comparatively recent period. Such detritus, united with marine shells of the present sea, is described by Mr. Trimmer as presented on the top of Moel Tryfane in Wales, at the height of 1500 feet...
>
> Points of lower elevation become, however, of considerable importance when we find there, - whether accompanied by marine detritus or not, - linear markings indicative of pauses or rests in the process by which the relative level of land and sea was changed. Ancient beaches, as these markings are called, - that is to say, plains, terraces, and shelves or beaches of land, produced by the [tidal] action of the sea at its margin in remote times, - are recognized as indicating such rests, but among British geologists, they have hitherto attracted little attention.[106]

Phillips privileged the use of organic remains and their distribution to determine the history of the sea floor. While Phillips directly mentioned tides as one source of the ancient detritus, he employed distinctly zoogeological methods rather than Whewell's tidal measurement and calculation. Such a zoogeological study might even have yielded more immediate conclusions about the movement of the sea floor; Whewell's study

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[105] BODL MS DEP BAAS 62, 81.

[106] BODL MS DEP BAAS 62 81.

could have taken decades or even centuries.[107] Both Whewell and Phillips understood that the study of the sea floor was a site for methodological dispute. Whewell was steeped in the context of his Cambridge tidological study which predisposed him to seek out different evidence for the same phenomenon.[108]

Whewell's study of the tides took advantage of more than Cambridge's history of astronomical observation and calculation. The study of the tides held a key place for past, prominent Cambridge naturalists. As mentioned previously in this chapter, Sir Isaac Newton had addressed the study of the tides in the seventeenth century. The tides remained a subject explored by Newtonian physicists, such as the French mathematician Pierre-Simon Laplace. The study of the tides was also shared by Sir Francis Bacon. Whewell acknowledged Bacon as an influential figure in his philosophy of science.[109] And Bacon – like Whewell – believed that the tides were integrally tied to a central element of his philosophy, inductivism.

As Cambridge students, Whewell, Hershel, and Babbage met for breakfast to discuss the future of scientific enterprise. Proper scientific methodology became influential topics for the young students.[110] Over the decades, the three Cambridge men labored over the promotion of Baconian scientific ideologies. According to them,

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107 A study of the tides could potentially confirm Phillips' conclusions. However, should Whewell's study contradict Phillips' zoological study, the privileging of some evidence and scientific practices would become an important part of the dispute's adjudication.

108 Snyder, *Reforming Science*, 13.

109 Whewell's vision for the history of science was embedded within the Baconian tradition and made plain in the preface to the first edition of the *History of the Inductive Sciences*, “The Novum Organon of Bacon was suitably ushered into the world by his Advancement of Learning; and any attempt to continue and extend his Reform of the Methods and Philosophy of Science may be... founded upon, a comprehensive Survey of the existing state of human knowledge. The wish to contribute something... to such a Reform, gave rise to that study of the History of Science of which the present Work [Whewell's treatise] is the fruit.” See William Whewell, *History of the Inductive Sciences: From the Earliest to the Present Times* (London: John W. Parker, 1837), xviii.

110 Snyder, *Philosophical Breakfast Club*, 36-43. Snyder provides a compelling narrative about the friendship and sometimes rivalry between the Cambridge Inductivists.

scientific knowledge should be built up from minute observations of the natural world without an appeal to grand generalizations or *a priori* speculation. These early conversations framed how the members of this network derived natural laws from evidence.

Whewell and Herschel based their evidentiary practices on the examples provided in Bacon's philosophical work. In the *Novum Organum*, Bacon demonstrated how inductive method might work through a study of tides. While the tides were mentioned multiple times in the *Novum Organum*, one of the clearest theoretical applications for the inductive method was presented through a proposed research plan to uncover the nature of “the ebb and flow of the sea.”[111] Bacon used the tides to reason through the possible causes for specific natural phenomena, thereby demonstrating how an inductivist might create a sound research plan. Whewell's interest rested in Baconian global observations of the tides, as laid out by Bacon himself in the *Novum Organum*, rather than precise mathematical modeling, as envisioned by Lubbock.[112] Whewell enlisted this network of Cambridge inductivists to observe tidal trends across the globe, including Herschel, his college breakfast companion then residing at the southern tip of Africa. Whewell began publishing on the problem of the tides by 1832. His number of articles on the subject expanded quickly over the next two decades.[113]

Whewell's work on tidology also coincided with his reflection on the history and philosophy of the sciences. Other historians have argued that the 1830s and 1840s were formative years for Whewell and that the subjects of tidology and inductivism mutually

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[111] Francis Bacon, “Novum Organum Scientarium” in *The Works of Francis Bacon* vol. 2 (London: M. Jones, 1815), 125.

[112] Deacon, *Scientists and the Sea*, 257.

[113] Whewell's bibliography on the tides is quite extensive. Among many shorter essays, he published fourteen more-extensive articles on the subject during the 1830s and 1840s.

reinforced and informed the other.[114] While the cyclical receding of the ocean and the movement of astronomical bodies could be calculated in a manner befitting an inductive science, one factor remained a major complication for Whewell's inductive observations, the sea floor itself: “And thus the [tidal] effects thus produced will depend upon the depth of the ocean, the form of its shores, and other causes, of which it is impossible to estimate the result *à priori*.”[115] Whewell's scientific tack of tidal measurement, tabulation, and calculation reflected the Cambridge milieu in which he was situated. And his practices of collecting these precise measurements through his network of university fellows would be shaped by Cambridge's own landlocked geography.

Across the Atlantic, in the United States of America, other naturalists examined the tides. Unlike Whewell and Lubbock's England, the United States gave one government agency the central authority to study the tides. That early tie between government and marine science would shape the study of the sea floor in its own unique way.

The Coastal Surveyors of the United States Government

In the United States, the study of the coastal zone – and later the deep-sea floor – was officially split between two different organizations, the Naval Observatory and the United States Coast Survey. Other American organizations shared an interest in the sea floor. For example, the Harvard Museum of Comparative Zoology had special interest in marine zoology through its director, the renowned Swiss naturalist and anti-

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[114] See especially Reidy, *Tides*, 132, for more on this historical discussion.

[115] William Whewell, “On the empirical laws of the tide in the Port of London; with some reflections on the theory,” *Phil. Trans*. 124 (1834): 43.

transmutationist Louis Agassiz. These organizations vied for influence; they cooperated in some instances and fought with each other in others. They were also joined by a common question in statecraft, “what authority did a democratic state have in the funding of scientific research?” This context of interorganizational competition and statecraft shaped who naturalists worked with and, through that division, the scientific practices used to research the seabed.

The number of scientific organizations presented in this section poses a small narrative challenge. Naturalists would sometimes shift between the various American organizations. Naval officers stationed at the Naval Observatory made excellent candidates for service on Coast Survey ships. Naturalists trained by Agassiz also made excellent civilian scientific staff on Coast Survey projects. The Coast Survey Superintendent Alexander Dallas Bache acted as a central figure in the numerous American scientific networks. In a gross – but perhaps not unfair – generalization, most of the scientific elite in nineteenth-century America gravitated towards one of two groups, those who followed Bache and those who did not. However, this section will not focus entirely on the surveying practices used by the Coast Survey. Practices sometimes flowed across these organizational boundaries. The United States naturalists also shared certain contexts worth exploring in greater detail.

These various organizations – and the naturalists that led them – shared strong contextual similarities. The United States was a comparatively young nation; as of 1807, when the Coast Survey was founded, the nation had been independent for only 31 years. The scientific institutions present in the United States still shared a colonial and Atlantic orientation. At one level, the United States was intricately connected to the oceans through commerce. And commerce was one reason that Congress authorized the Coast Survey. Like other Atlantic colonies, the British holdings relied upon a complex network of trade along the seaboard. The mainland colonies had produced tobacco for Europe, distilled rum cane from Jamaica, and shipped cod to feed the slaves of Barbados. The American Revolution did not suddenly change the people's centuries-long reliance upon the oceans for trade and transportation. A strong merchant fleet was the lifeline of the new nation. Accurate coastal charts ensured safer and more vigorous trade, not to mention protection from maritime invasion. In this way, America's first scientific institution had a practical purpose. Nonetheless, financial considerations by themselves were not enough to convince United States legislators to establish federal scientific institutes.[116]

The coastal survey was ultimately a manifestation of the United States' post-colonial apprehensions and aspirations. Even America's commercial orientation towards the Atlantic was the result of its colonial history. Additionally, England's knowledge of the Atlantic had facilitated its rule over the colonists. Even after the United States achieved independence, the nation still had to rely upon foreign naval charts to map out its defense and commerce. The apprehensions resulting from colonial dependency, especially for men of science, were exacerbated by the critique of scholars from other nations.

One such critique came from Alexis de Tocqueville's tour of the United States. His 1830s publication, *Democracy in America*, resonated with many citizens.[117] De

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116 The United States Congress had received a substantial gift for the establishment of a national university by James Smithson around this time. Despite the availability of funds, many members of Congress argued that the establishment of a national university overstepped the boundaries of federal power. By comparison, the United States Congress not only authorized the Coast Survey, they also funded this national scientific project.

117 Sally Kohlstedt. *The Formation of the American Scientific Community* (Chicago: University of Illinois Press, 1976), 4-5. Consider De Tocqueville's tenth chapter on American

Tocqueville implied that America still relied upon European literature and scientific theories. The United States citizens continued to import scientific theory much like they had imported the refined commodities that they were forced to buy from their former imperial masters. Once in possession of those theories, Americans brilliantly applied the philosophies to practical problems. They prided themselves on their ingenuity and technical inventiveness. However, de Tocqueville observed, men of science in the United States failed to discover novel, fundamental truths about nature. That failure was a harsh reminder of their tenuous political step into the unknown, especially for citizens conducting – as Thomas Jefferson framed it – the great American experiment. Their general lack of "proper" philosophers and federal institutes of scientific research was a continued badge of colonialism and dependence.[118] In essence, American statecraft became intertwined with its production of scientific knowledge. When legislators suggested the establishment of an institution for marine research, Congress passed the resolution with little debate.

During the early nineteenth century, the United States began to slowly transition from a coastal colony to a rapidly expanding nation state. National scientific organizations adapted to these changes. For example, the United States acquired over 820,000 square miles of territory in 1803 through the Louisiana Purchase. President Thomas Jefferson authorized the Corps of Discovery Expedition, also known as the Louis and Clarke Expedition, to explore the scientific and economic potential of the new

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science, "Equality begets in man the desire of judging of everything for himself: it gives him, in all things, a taste for the tangible and the real, a contempt for tradition and for forms... In America the purely practical part of science is admirably understood, and careful attention is paid to the theoretical portion which is immediately requisite to application. On this head the Americans always display a clear, free, original, and inventive power of mind. But hardly anyone in the United States devotes himself to the essentially theoretical and abstract portion of human knowledge."

[118] Kohlstedt, *American Scientific Community,* 4.

territory shortly after the purchase. The acquisition of this immense geological area gave access to new specimens that could be pulled from the rocks. Geological expeditions covering great distances could be conducted well within the borders of the United States and its new territories. The new territory also provided a former seabed geology rich in crinoid fossils, which provided an exciting area of study for American naturalists.

Gerard Troost, an influential naturalist who guided many early American scientific institutions, was an expert on geology and crinoids. Troost had impressive scientific credentials for an American naturalist. Troost was born in the Netherlands, received his medical degree at the University of Leyden and, by 1807, had swiftly gained the Dutch royal patronage needed to study in Paris as a natural philosopher. He trained under one of the most famous European geologists: René Just Haüy, the father of crystallography.[119] Like many other philosophical naturalists, Troost was introduced to many of the leading scientific figures of his time while in Paris. He became personal friends with his mentor, Haüy, not to mention the famous naturalist-explorer Alexander von Humboldt. Troost counted Robert Jameson's mentor, the Neptunist Abraham Gottlob Werner, among his friends as well.[120]

By the latter months of 1809, Troost began his scientific travels, but each one was beset with trouble. The King of Holland appointed him to an expedition to Java, but an English blockade made travel difficult. He was captured by an English privateer and held captive and eventually returned to Paris.[121] Finally free, Troost boarded an American ship via a German port and intended to sail to the East Indies in an American

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[119] William Jay Youmans, *Pioneers of Science in America: Sketches of their Lives and Scientific Work* (New York: Appleton & Co., 1896), 119.

[120] Elvira Wood and Gerard Troost, *Preface to A critical summary of Troosts̓ unpublished manuscript on the crinoids of Tennessee* (Washington: Government Printing Office, 1909), 10.

[121] Youmans, *Pioneers*, 120.

vessel, thereby avoiding any Anglo-Dutch hostilities. Instead, the American ship was boarded by a French privateer and Troost was taken back to Dunkirk as a prisoner. Luckily, his scientific reputation was enough to get him released and sent to Paris again.

Back in Paris, Troost was elected a corresponding member of the French Museum of Natural History and given leave to travel to the United States. In March of 1810, he was allowed to start his long journey to Java. His first port of call was the city of Philadelphia. Troost, finally allowed to travel without fear of being taken prisoner by pirates, was troubled by larger global political events. By July 1810, Napoleon Bonaparte had annexed Holland and incorporated it into the French empire, so Java still remained a possible destination for a young naturalist, especially a member of the French national Museum of Natural History. One year later, while Troost was still in America, Java was ceded to British forces. By that time, he had enough. He simply decided to stay where he was.[122]

Notwithstanding all his troubles, Troost found a small community of naturalists who were grateful to have such an eminent scientific figure in their city. He was accepted as a member of the American Philosophical Society and in 1812 he became one of the seven original founding members of the Academy of Natural Sciences of Philadelphia. He was quickly nominated as its president, a post that he accepted and built up for the first five years of the Academy's existence. Troost's prestige and direct connection to the Parisian philosophical naturalist community became an asset to the organization. During his presidency, he gave a formal lecture series on geology and mineralogy before the Academy, which attracted considerable attention. Troost continued to promote

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[122] L. C. Glenn, "Gerard Troost," *The American Geologist* 21 (1905): 72-94.

geological interests in the Philadelphia region until 1825 when he moved to Indiana to live in a utopian scientific colony.

Once again, travel proved to be troublesome. Disappointed with the colony's “peculiar social arrangements,” Troost was recruited to the University of Nashville, in Tennessee, where he began to conduct one of the greatest studies of crinoids, a class of marine invertebrates, ever done at that time.[123] His zoogeological *magnum opus* was completed in his tenth report to the State of Tennessee, but his governmental patrons refused to publish the work. By 1849, Troost's work on crinoids had captured Agassiz' attention. He presented “A List of the Fossil Crinoids of Tennessee. By Professor G. Troost, of Tenn.,” before the American Association for the Advancement of Science and mentioned the great value of crinoid studies before the Association.[124]

Shortly after Troost finished his work on crinoids, Agassiz read the paper to the Association. Troost also fell deathly ill not long after he completed his initial analysis of the crinoid fossils; he sent his manuscript and collection to the Smithsonian where he hoped that Agassiz would edit and publish his work. He died on 14 August 1850, two weeks after the monograph was completed. The manuscript remained in the custody of Professor James Hall, another American geologist and invertebrate paleontologist, though the volume was nearly forgotten by the Smithsonian staff for many years.[125] While the monograph remained unpublished for over forty years, Troost's collection and specimen descriptions remained valued contributions to science; Hall published the information he could in the course of his research, quoting Troost in the text. These rare

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[123] Glenn, “Gerard Troost,” 75.

[124] Wood, *Critical Summary*, 1.

[125] Wood, *A Critical Summary*, 2-3.

crinoid specimens exploited the rich fossil fields opened up by United States territorial expansion in the nineteenth century.

The United States' acquisition of the mid-continent expanded the geographical area and specimens accessible to geologists. This expansion also added new coastline to the nation. As a British colony, America was already oriented toward the Atlantic Ocean. This new coastline added more impetus for coastal surveying and the exploration of the United States' seaboard territory. The context of expansion and commercial interest of the American seaboard shaped the Coast Survey from a temporary project to the first sustained scientific agency in United States history. By the mid-nineteenth century, that agency would wield considerable power.

The Coast Survey was established during the Jeffersonian presidency.[126] Thomas Jefferson was given authority “to cause a survey to be taken of the coasts of the United States” by act of Congress on the 10 February 1807. Illustrious candidates applied for the position of superintendent, including future U.S. President James Madison.[127] Upon the recommendation of the American Philosophical Society, Jefferson employed Ferdinand Hassler, a supremely competent, but abrasive, Swiss immigrant to lead the project. For Hassler, surveying was an exercise in extreme exactitude and no amount political critique would dissuade him from his scientific mission. Hassler expected his elite European science education to yield social privilege; he expected to remain free from the political accountability that characterized the new republic. He was

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[126] See A. Hunter Dupree's *Science in the Federal Government* for the seminal overview of Jefferson's involvement in the Lewis and Clarke Expedition and the Coast Survey. A. Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities* (Baltimore and London: Johns Hopkins University Press, 1986), 24-33.

[127] Elliott B. Roberts. “United States Coast and Geodetic Survey, 1807-1957” reprinted from the *U.S. Naval Institute Proceedings* (Washington DC: Smithsonian Institution, 1957), 222.

famous for constantly aggravating government officials, especially those who would question the value of his scientific work.

Almost a century later, Hassler's conflict with leading figures in government was still legendary. The well-read *Harper's Monthly Magazine* related a number of these tales. In one instance, the United States Congress sent a team of legislators to ask Hassler about his work. The committee had been charged with assessing the progress made in the survey. Hassler exploded at the committee's inquiries and sent the congressmen back on the grounds that they were completely incapable of comprehending his great scientific enterprise.[128]

The Coast Survey faced difficulties other than Hassler's irascible disposition during its early years. The United States declared war on England in 1812 while Hassler was in London to procure surveying instruments, thereby stranding him across the sea to fend for himself. The U.S. also did not have the standardized weights and measures needed for exact scientific measurements. Hassler performed the duties of a national central weights and measures bureau for most of the nineteenth century.[129] For a number of years, the U.S. Navy also gained administrative control of the Coast Survey. Hassler was forced to defend his civilian vision for the institution before Congress in 1832 after mismanagement by the Navy.

The early relationship between scientific institution and the federal government was not an easy one. However, Hassler's work was first-rate and he understood the opportunity that his scientific enterprise represented to the United States, both in terms of freeing America from its colonial past and as an awesome form of statecraft. Science

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[128] Joseph A. Wraight and Elliott B. Roberts, *The Coast and Geodetic Survey, 1807-1957: 150 Years of History* (Washington: U.S. Coast and Geodetic Survey, 1957), 13.

[129] Slotten, *Patronage*, 43, 50.

was the great human enterprise, the sign of a truly civilized nation. Science represented exactly what America hoped to become, something greater. Hassler inspired a new relationship between science and national pride. That vision for science and statecraft was captured for posterity in a speech given in defense of the Coast Survey:

> During the times of the greatest turmoils of the French Revolution, a work was executed by the mathematicians of that country, the greatest in its kind... and therefore will ever stand in the history as a monument of the high state of civilization of its public... this work is the measurement of the Meridian of Paris...
>
> [Men of science in European governments have since executed similar projects that have] .. united them with the French works... and in spite of all disparities of political views and opinions ... the mathematicians... have since linked all European countries, and even Nations, by their works and... they have joined to elevate the science itself, as well as its usefulness in application to the most valuable wants of present state of civilized Society...[130]

Science represented a civilizing statecraft that could raise the United States from its colonial past. For Hassler, the advancement of science transcended petty state politics. The mathematical surveying of the oceans linked all humanity together in a great enterprise. Hassler's reflections on scientific cosmopolitanism in this Coast Survey document demonstrates how science – above all a science of the sea – was seen to be of obvious utility and benefit for America. The new nation was already oriented towards the Atlantic, so the advancement of marine science was a perfect marriage between "Yankee ingenuity" and the great scientific project of all civilized nations.

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[130] NABA "Records of the Office of the Secretary... relating to the Coast Survey. Records Relating to Personnel of the Coast Survey, compiled 1860 – 1901." n.d. Record Group 23 (Box 1): Records of the Coast and Geodetic Survey, 1806 – 1981. National Archives Building Annex, Washington, DC. Document has been edited for spelling. No author is mentioned, paper was transcribed and bound with other of Hassler's documents along with early Coast Survey papers. While this could be Bache's work, it is probable that Hassler wrote it considering that his contribution to survey practice in America had been to base his triangulations on a prime meridian.

After Hassler's vision for science and the Coast Survey spread, the United States legislature gave him a wider berth. He worked tirelessly until illness struck him in 1843. By the time that Hassler lay on his deathbed, his exact triangulations reached east to Rhode Island and south to the head of Chesapeake Bay; in total, he and his small team had surveyed over 1,600 miles of coastline. His exacting measurements became the foundation for modern geodetic and coastal surveys. With the coveted federal scientific position now open, the American Philosophical Society wasted no time in promoting a man of astounding lineage for the newly-vacant position, none other than Alexander Dallas Bache, the great-grandson of Benjamin Franklin himself.[131]

Bache and Hassler were alike in only two ways. Both of them were dedicated to conducting superb science. Hassler had his European scientific reputation at stake and Bache was the descendant of America's greatest man of science. Bache earned constant comparisons between himself and this great grandfather, even with those who knew both relatives personally. He graduated first in his class at the U.S. Military Academy at West Point, then the greatest location of scientific learning on American soil. He also shared Hassler's vision for America's most important marine science project. But that is where the comparison ends. Whereas Hassler would explode when asked when the survey of the coast would be finished, “Bache would” according to one account, “smilingly ask, 'when will you gentlemen stop annexing new territory?'”[132] Bache was a glib, charming, and outrageously intelligent man. The U.S. Coast Survey flourished under Bache's expert leadership. Marine science, through the Coast Survey, became America's most influential scientific project, whether federal or non-governmental. A surprising number of scientific organizations, including the American Association for the

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[131] See Slotten, *Patronage*, for the most complete account of Bache and the Coast Survey's connection to federal interests.

[132] Wraight and Roberts, *Coast Survey*, 15.

Advancement of Science and the Smithsonian Institution, aligned themselves to Bache's marine – and therefore Atlantic – interests. As one of the most influential men of American science, Bache governed each organization as president or regent at one point or another. He was a central, organizing figure for most of the new scientific institutes. They all sprouted and developed in the long shadow cast by the Coast Survey's success in U.S. government. The benefit was mutual. The support of a well-networked scientific community in the United States gave unprecedented protection and influence to the Coast Survey.

For example, in 1849, certain members of Congress once again threatened to place the Coast Survey under the leadership of the U.S. Navy, just as it had once done to Hassler. Bache and his supporters struck back with congressional power supported by his network of scientific organizations. Jefferson Davis, then senator, gave a fiery speech that swayed the final votes allowing Bache to retain civilian leadership of the Survey.[133] Davis did not realize that, decades later, the Coast Survey would use its scientific expertise to help the Union win the crucial victories that ended the Confederate States of America, the rebellious government which had elected Davis as its one and only president. The Coast Survey's influence on American statecraft was never more explicit than when it held the Union together.

Evidentiary Practices in the US Coast Survey

The American Philosophical Society served as an early site of American scientific endeavors. And there were precious few elite naturalists in the United States to run large

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[133] Slotten, *Patronage*, 92.

scientific projects, such as a thorough survey of the coast. The Philosophical Society played a key role in the selection of Hassler as Superintendent to the Coast Survey. Philadelphia remained an older, more established cultural center than the new capital, Washington D.C., during the early nineteenth century. However, Hassler's appointment defined an enduring new milieu for American science through the Coast Survey. Hassler often found Washington politics to be distasteful. Nonetheless, the Coast Survey's political ties to Washington would draw him back when neglected; the Survey suffered the constant threat of being assigned to the US Navy without his intervention.

The Naval Observatory, the organization that would later become the Coast Survey's major rival, also found its home at the nation's capital. The connection to Washington statecraft and its organizational rivalries would help define what practices the men and women of the Survey employed. Obviously, surveying the coastline was a major pursuit of the Survey. Hassler was a very competent mathematician and led a large-scale, geodetic survey of the United States coastal areas that took the curvature of the Earth into account. Such a detailed and mathematically-intensive program exceeded what was necessary for the creation of useful maps for maritime navigation. Hassler insisted on the scientific exactitude of his survey despite criticism from naval officers and congressmen alike. He passed that practice of exactitude to his assistants, the title given to his highest-ranking subordinates.

While exact, the measurements taken by the Coast Survey differed from the type taken by the Cambridge astronomers. The Cambridge Constellation used their measurements for celestial calculations and predictions. They were capable of predicting the position of existing – and even previously unknown – celestial bodies. All the astronomer had to do to provide evidence of their scientific conclusion was to point their telescope into the sky and see if the object was where they predicted it. The Survey assistants also used precision instruments to measure positions; the theolodite could be used to triangulate the position of geographical features.[134] However, the surveyor knew that no measurement was perfect. Surveyors would take multiple measurements of the same geological feature and often get slightly different measurements, even with exact instruments and careful operation. These measurements would be taken multiple times and adjusted or averaged together.[135] Absent the great unifying theory provided by Newtonian celestial mechanics, the product of the assistants was a map, not a predictive position for celestial bodies. Constant triangulation and knowledge of the terrain was valuable for the Coast Survey geodists. The focus on mapping and measurement would change the way that the Coast Survey studied the sea floor as well.

The members of the Coast Survey did not limit their activities to a simple mapping of America's coastline. As the assistants moved across the national seaboard, they conducted many studies related to the area to which they were assigned. Bache listed these activities in his 1844 *Report of the Superintendent of the United States Coast Survey Showing the Progress of the Work*, the first report written under his superintendency. He listed the five primary operations of the Coast Survey in each region as: primary triangulations, the astronomical and geographical observations that go with mapping the specified areas; secondary triangulations, relative positions of points upon the coast; mapping the topography of the area; hydrographical observations of the bays and harbors; and the processing of these observations into a uniform system for drafting, publication, and distribution. The Coast Survey's hydrographical activities included soundings from the local bays and waterways. These soundings were

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134 The Coast Survey conducted astronomical observations as well as map the coastline.
135 The Coast Survey used multiple, averaged measurements to determine heights of markers and depths of the sea.

particularly important to the creation of sailing charts. Navigators would use the Coast Survey charts and maps to avoid dangerous shoals that could sink their ships. However, Bache's vision for the hydrographical studies differed from Hassler's; he was not content to simply map the region. Bache desired a thorough, scientific understanding of the coastline and sea floor. He explained his expanded program in terms of this vision and its utility:

> The hydrography, which includes the determination of the depth of water off the coast, and in the bays, harbors, and other navigable waters connected with the ocean, the existence of shoals, rocks, &c., and the direction and velocity of currents.
>
> The results of these operations, when requiring calculation, are reduced by the parties making the observations, and checked by others. They go to form the maps and charts, which are the ultimate objects of the work; to give minute knowledge of our coast, in a high degree important to our commercial and national marine, and in conexion with defence.[136]

Bache envisioned the ultimate goal of the Coast Survey's work as a “minute knowledge” of the national coastline. He had full faith that a more scientific approach to understanding the oceans would yield practical results. Such as statement would be persuasive to the congressmen funding his scientific research. He was also drawing upon his own legacy as the great-grandson of Benjamin Franklin, who had conducted a scientific study of the Gulf Stream. Bache's interest in the sea floor was both practical and a point of scientific pride.

Bache and his assistants would have surveyed the sea floor as an expected feature of the maps and navigation charts he produced. He was keen to mention when he found dangerous underwater rock formations that were previously unrecorded and,

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[136] Alexander D. Bache, *Report of the Superintendent of the United States Coast Survey Showing the Progress of the Work* (Washington DC: Government Office, 1844), 2-3.

therefore, lacked buoys to warn ships.[137] Bache's vision for a scientific understanding of the national coast had unexpected consequences. The deeper sea floor became an object of interest as a geographical extension of terrestrial topography. The Coast Survey was already conducting shallow water soundings, so the added deep water soundings were not a great imposition. The same sounding techniques could be used, where a heavy lead attached to rope would be lowered into the water. The length of rope extended into the ocean would then be read to see the number of fathoms that plunged into the depths.[138] When the lead hit bottom, the final distance of rope extended would be recorded as the depth of the sea floor in that area. Bache's scientific interest in the sea floor also led him to require his assistants to collect sedimentary samples of the sea floor. These samples would be gathered by applying tallow or grease to the bottom of the sounding lead.[139] The grease would then pick up a small sediment sample, which could be preserved and examined by microscope at a later date.

Bache began this program of deep water sounding the first year of his superintendency. The first deep-sea soundings were taken off the coasts of Connecticut, New York, and New Jersey. The depths recorded ran from 107 fathoms to approximately 200 fathoms in depth.[140] Over the years, Bache was able to compile a general map of the ocean floor along the American seaboard.

The interest in the deep ocean floor, including the growing collection of sediment samples, would later prompt Bache to recruit naturalists proficient in geology and zoology to examine the composition of the sea floor. Bache recruited one such naturalist

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[137] Bache, 1844 *Report*, 6.
[138] A fathom is a length equal to 6 feet (1.8 meters), the approximate armspan of a sailor. Distances of under one fathom were measured at low tide and usually estimated in terms of feet.
[139] Refer to chapter three for a detailed account of sounding practices in the United States.
[140] Bache, 1844 *Report*, 8.

through his friend Louis Agassiz to measure the tides and conduct a zoogeological study of the sediment samples. This naturalist, Louis Françoise de Pourtalès, and his practices, are described in detail in chapter three of this dissertation. The combination of tidal observations and sedimentary studies conducted by Pourtalès stand in contrast to the tidal calculation done by the Cambridge group, which did not include an interest in organic remains.

Bache and Pourtalès lacked the ability to sound greater depths in the early nineteenth century. Sediment samples from depth greater than 1,000 fathoms could not be collected until the 1850s, when a naval officer under the command of Matthew Fontaine Maury, Bache's rival, developed a deep-sea sounding lead. When this new technique was developed in the mid-nineteenth century, Bache understood that he could employ this new deep-sea sounding practice to extend his existing sedimentary studies. And, as explored in chapter three, Bache secured this new practice from his rival through political maneuvering in Washington. The Coast Survey's geographical and political connections to Washington shaped what practices it employed. Statecraft and politics would also determine the Coast Survey's access to new scientific methods of studying the sea floor.

Conclusion: The Different Seabed Sciences and Pre-Darwinian Conflict

This chapter has examined the origins and development of three different studies of the sea floor during the early nineteenth century. These differing practices led to scientific disagreements over a range of scientific subjects, including pre-Darwinian evolutionary ideas. Naturalists disagreed over the proper way to establish natural law as they sought to break away from the traditional practices of natural history. Anglo-American interest in the seabed arose in a context of scientific reform; the philosophical naturalists desired to investigate nature rather than simply collect and label its vast inventory. These new naturalists sought to uncover the fundamental laws of nature in geology and zoology the same way that had proven successful in physics and astronomy during the previous centuries. These law-seeking philosophical naturalists turned to the sea floor to understand how the Earth and its creatures changed over geological time.

Yet, despite their shared interest in the same geographical region, three distinctive types of seabed science emerged during the early-nineteenth century. The differences between the seabed sciences depended upon the practices employed by the naturalists investigating the sea floor and its history. Groups of naturalists emerge that studied the sea floor in similar ways. These groups had common names for those practices. “Zoogeology,” used organic remains as evidence, such as fossils and various sedimentary rocks. The zoogeologists gathered specimens with their geologist's hammers and naturalist's dredges. Those fossils or sediments were analyzed to determine their geographical distributions, and then compared to past distributions to see how their placements changed over time. These practices differed radically from the more inductive measurement of the tides or the geodetic mapping of sea floor topography. Each of these seabed science practices required specialized training. These practices also provide insight into the various networks of naturalists that wrestled over scientific methodology and the “proper” way to establish global claims about the natural world.

What caused these differing scientific practices to emerge in the forms they did? Nineteenth-century naturalists might have imagined that laws of the natural world, when explored through the same subject, would not be affected by the route taken to discover them.[141] This was not the case. Not only did the philosophical naturalists argue over the practices employed by their distant brethren, their practices produced different types of evidence and privileged differing specimens. Ultimately, these differing scientific practices originated from the institutional milieu in which each naturalist was situated.

The University of Edinburgh served as a central locus for the training of new zoogeologists. Robert Jameson was already disposed to the study of marine invertebrates from an early age. He found a mentor in the form of John Walker, the Regius Professor of natural history at the University. The University's proximity to the Firth of Forth facilitated Jameson's learning to use the naturalist's dredge, a skill he passed along to his future students.[142] These students would also inhabit key positions in the Wernerian Society and its undergraduate counterpart, the Plinian Society. This early network privileged the evidence procured from the naturalist's dredge, making Edinburgh a hotbed of marine invertebrate research. And generation after generation of naturalists to inhabit the Regius Chair at the University of Edinburgh would be the most celebrated zoogeologists and dredgers in the United Kingdom, each trained in the legacy of Walker and Jameson.[143]

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[141] Gertrude Lenzer, “Introduction” in *The Essential Writings of Auguste Comte and Positivism* (Piscataway: Transaction Publishers, 2009), xi-1.

[142] Jameson passed this skill to a number of his students, though the most important students, for the purpose of this dissertation, are Forbes, Grant, and Carpenter. These students are discussed at length elsewhere in this dissertation

[143] A sequence of the regius chairs secured the Edinburgh natural history position by being renowned dredgers, from Jameson, Forbes, and later Wyville Thomson. Thomson is discussed at length in chapters four and five. This leaves Allman, who temporarily inhabited the natural history position after Forbes. I am uncertain whether Allman dredged. However, He was the consultant on a submarine marine cable pulled up from great depths.

Cambridge University did not have the same geographical or institutional context as the University of Edinburgh. Being landlocked, the naturalists trained there did not have easy access to marine areas where they could dredge. Instead, William Whewell and his student John Lubbock resorted to a measurement of tides, which could be transported by paper to a central location.[144] This reliance upon minute calculations and tabulation also pulled upon a long history of astronomical computation at Cambridge that stretched back to Newton. Whewell deployed Cambridge's legacy of Newtonian and Baconian science in his examination of the sea floor over time, which stood in stark contrast to the zoogeologist's method of studying the same phenomenon.

Across the Atlantic, the United States Coast Survey developed in a context of government patronage and interorganizational competition. The United States’ recent colonial history already oriented its commerce and security toward the Atlantic seaboard. Rapid westward expansion also opened up vast areas rich in marine fossils. The abundance of these marine fossils, combined with the pragmatic necessity of producing topographical maps for navigation, created a unique blend of geology and seabed mapping at the Coast Survey.[145] Routine soundings and surveys were expanded to include deeper and deeper samplings of the seabed. Alexander Dallas Bache also recruited members to analyze these organic sediments and their location along the sea floor. The centralization of hydrographical studies, geodetic mapping, tidology, and

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[144] Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Harvard: Harvard University Press, 1987), 215.

[145] The newer territories had been previously submerged and, therefore, were once shallow seas. Parts of Tennessee and all of Texas had once been underwater and provided rich areas for marine fossil research. Naturalists collected these rare fossils and sent them to American institutions, such as Troost’s collection and the Smithsonian. These fossils could be compared to extant creatures. Also, Bache had considerable influence on these American institutions and, therefore, had access to both marine creatures off the coast and fossils from the new United States territories.

sedimentary zoology into one government-supported institution gave the Coast Survey an unprecedented ability to adapt to new seabed practices.

Each locus of seabed science perpetuated its own practices for the study of the sea floor. Practices were passed along from teacher to student or superintendent to subordinate. However, not only the practical methodologies of science were promoted in these institutions. The privileging of certain specimens and methods of extracting truth claims from those specimens were also passed along from generation to generation. Jameson's students not only learned to use the naturalist's dredge, they also gained an appreciation for marine invertebrate specimens and a predilection for Romantic, speculative science based on biogeographical analysis. The same was true for the other institutions and their naturalists.

These three aspects of scientific lineage provide the historian of science with a way to track the members of a scientific network. One may ask three questions to gain a perspective on these evidentiary practices: “What did the naturalists physically do to their specimens?” “Why did the naturalist value that specimen over other potential ways of observing natural phenomena?” and “How did the naturalist derive truth claims from the specimen?” Similar answers to these three questions reveal the presence of a network with shared evidentiary practices. This pragmatic method for studying and defining groups of naturalists helps the historian to reach beyond what methods their historical actors profess to use in order to study their practices and uses of evidence. Of course, there are many ways to slice a scientific network. The use of evidentiary practices is one of those methods that illuminates a practical relationship between scientific groups.

This method of studying scientific networks is not only a theoretical approach; it had explicit significance for the historical actors in this study. Seabed naturalists defended their own evidentiary practices against rival methodologies during the nineteenth century. Conflicts emerged along these lines frequently. One example of these conflicts was a 1833 collision between a small group of speculative naturalists and the Trinity Constellation.[146] At its core, the argument centered on differing practices. The naturalists argued over who could use the word “philosophical” to describe his work; the group which had the proper method for deriving natural law was fit to use the title of a “philosopher” naturalist.

The disagreement came to a head during the third meeting of the British Association for the Advancement of Science, held at Cambridge. William Whewell himself was the proud host of the Association meeting.[147] The meeting locations had profound influence upon the debates over scientific methodology during the 1830s.[148] The rival philosophical naturalists in attendance were hardly passive while seated at this geographical hub of Cambridge influence. Samuel Taylor Coleridge, the Romantic poet and philosopher of scientific method, rose to his feet and actively denied Whewell the title of “philosopher” at the meeting.[149] Whewell agreed that the appellation was no longer suitable as a common identity for the differing philosophical naturalist

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[146] See Snyder, *Philosophical Breakfast Club*, 2.

[147] Snyder, *Philosophical Breakfast Club*, 2.

[148] Charles W. J. Withers, “Scale and the Geographies of Civil Science: Practice and Experience in the Meetings of the British Association for the Advancement of Science in Britain and Ireland, c. 1845-1900.” In *Geographies of Nineteenth-Century Science*, eds. David N. Livingstone and Charles W. J. Withers. (Chicago: University of Chicago Press, 2011), 99-101. As the historian Charles Withers points out, among others, the choice of meeting locations was a central concern for the British Association members. Withers focuses on the effect that the Association had on British appreciation for scientific pursuits, showing how each town responded differently to the peripatetic organization.

[149] Sydney Ross, “Scientist: The story of a word.” *Annals of Science* 18 (1962): 67. See also Snyder, *Philosophical Breakfast Club.*

communities. He proposed a new word for people that shared his hands-on vision of scientific practice. He called these men “scientists.”

The term scientist, as proposed by Whewell, was not an immediate success, suggesting widespread discomfort at a vision of science removed from the “philosophy” of the “philosophical naturalist.”[150] The neologism was a bold move and a clear statement on Whewell's part. The “philosophical” schism continued into the next year. The next meeting of the British Association was hosted at the University of Edinburgh, the geographical stronghold of the zoogeological philosophical naturalists and Idealist scientific practices.

Evidentiary differences may have started conflict, but evidentiary similarities played as much of a historical role in bridging some groups. An excellent example of a shared use of specimens can be found in a comparison between the Scottish and the American naturalists. The evidentiary parallels between Robert Jameson and Gerard Troost are made more explicit by a historical focus on the sea floor. Both gentlemen were first-rate geologists who learned Wernerian theory through Werner himself. Jameson taught geology and natural history at the University of Edinburgh while Troost helped to establish American science by founding, leading, and teaching geology at the Academy of Natural Sciences of Philadelphia.

Troost's expertise in simple marine invertebrates was echoed by the crinoid studies of Jameson's students. Both geologists used crinoids to gain a sense of organismal complexity during early geological periods. While Jameson's students were

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[150] Many British naturalists also communicated a distaste for the barbaric smashing together of Latin and Greek words used to create the word “scientist.” Many believed that the word sounded too much like an American creation. Many did not realize that Whewell, one of the most prominent naturalists in the United Kingdom, was the creator of the word.

Troost's juniors, all of these geologists had studied in Paris, the great center for the new *biologie*. Indeed, it hardly seemed as though Gerard Troost could leave the French scientific center at all! Both Troost and Forbes were directly exposed to Lamarck's legacy of *biologie* and its connection to simple marine invertebrates. Whether by dredge or by rock hammer, both naturalists studied the same, unassuming denizens of the deep in order to gain a window into the past. And as the dredge and hammer became symbols of the biological philosophers, these naturalists crafted a new way to collect and observe nature itself. These crinoids would later play a large role in the adjudication of Darwinian evolutionary theory.

The philosophical naturalists' studies grew in popularity as the century progressed. The development of evolutionary theory, before and during Darwin's publications, must be viewed in the context of this battle over scientific methodology; differing scientific practices contributed to conflicts over evolutionary theory even before Darwin introduced his concept of natural selection. For example, in 1844, the anonymously written *Vestiges of the Natural History of Creation,* an ambitious “evolutionary epic that ranged from the formation of the solar system to reflections on the destiny of the human race,” brought pre-Darwinian evolutionary ideas to the forefront of Victorian society. The book was a sensation, read by over “a hundred thousand other men and women across the spectrum of Victorian society,” including Queen Victoria, elite naturalists, handloom weavers, and militant freethinkers.[151]

The author was later identified as Robert Chambers, an Edinburgh printer and member of the Edinburgh Royal Society since 1840. His membership in the Edinburgh

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[151] James Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation (Chicago and London: University of Chicago Press, 2000), 1-2.

Royal Society put him into contact with members of the Edinburgh philosophical naturalists, especially Edward Forbes.[152] The ocean floor and simple marine invertebrates played a critical role in Chambers' evolutionary reasoning. Chambers used the word "crinoids" more often than the word "Creator" in his first edition of *Vestiges*.[153] The sea floor also occupied more chapters than both outer space and heaven combined. Beginning in the second chapter, each of the Earth's epochs was explained as geological stratum and the deep ocean process that created it. By doing so, Chambers linked the origins of simple life to a geographical location and geological time:

> The hypothesis of the connexion of the first limestone beds with the commencement of organic life upon our planet is supported by the fact, that in these beds we find the first remains of the bodies of animated creatures... the deposition of these limestone beds was coeval with the existence of the earliest, or all but the earliest, living creatures upon earth.
>
> And what were those creatures? ...behold, the interrogation only brings before us the unpretending forms of various zoophytes and polypes... all of them creatures of the sea. It is rather surprising to find these before any vegetable forms... but it is probable that there were sea plants, and also some simpler forms of animal life...
>
> ...Zoophyta, polyparia, crinoidea, conchifera, and crustacea, are the orders of the animal kingdom thus found in the earliest of earth's sepulchres.[154]

Here, as in Forbes' philosophical reasoning, zoophytes and crinoids from the ocean floor link geological time to the origin of species and the orderliness of nature. The reasoning used to address transmutationism and generation was directly in line with zoogeological "principles of philosophical investigation."[155]

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152 William Chambers, *Memoir of Robert Chambers,; With Autobiographical Reminisces of William Chambers* (New York: Scribner, Armstrong & Co., 1872), 254.

153 Robert Chambers, *Vestiges of the Natural History of Creation* (London: John Churchill, 1844). The word "crinoid" is mentioned nine times, while "creator" is mentioned five times.

154 Robert Chambers, *Vestiges,* 58-60.

155 Chambers, *Vestiges*, 177.

Vestiges' sweeping, evolutionary narrative was both a literary and scientific success, though some eminent men of science were greatly troubled by the method employed by the anonymous author. Edward Forbes and William Benjamin Carpenter, both members of the Edinburgh philosophical naturalist circle, wrote favorable reviews of Chambers' evolutionary book. Forbes' review was significantly shorter and summarized his appreciation for the treatise:

> This is a very remarkable book, calculated to make men think. For some time back we have been so immersed in the *facts* of science, that to read a volume of speculations is like a breath of fresh air... Throughout the book, the technicalities of natural history are misunderstood... Nevertheless, it is worth reading, and will be read, in spite of its defects, for it is written in earnest, and with good faith, though *very* imperfect knowledge.[156]

Forbes' review of *Vestiges* communicated what practices he valued for the creation of scientific ideas; small errors in detail could be overlooked in a work of such refreshing scientific speculations.

Carpenter's review was much longer, though of the same sentiment as Forbes'. Carpenter engaged much more closely with the grand intent of *Vestiges* and its implications for general truths about natural law and divine creation. However, most notable about Carpenter's review is his rationale for recommending the work despite its flaws regarding physiological facts:

> It is in his reasoning upon the general question, that we recognize the mind of the true philosopher. The sublime inductions of astronomy, and the revelations of geological history, have never, perhaps, been so well interpreted with reference to the attributes of the Creator. But when the author comes to analyse the difficult problems of physiology, we see the

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[156] Edward Forbes, “Review: Vestiges of the Natural History of Creation,” *The Lancet* ed. Thomas Wakely (London: John Churchhill, 1844), 265.

> workings of a mind better fitted (like that of the immortal Bacon) to deal with the general than the particular, — with reasoning, than with facts.[157]

It was the philosophical reasoning, not the conclusions, that made the book a worthy contribution to scholarship, according to Carpenter. The commentary also outlined a tension that would need to be solved. Philosophical reasoning would have to become compatible with Baconian method at some point during the future. Carpenter also took a different interpretation of Bacon's legacy; while Whewell claimed his legacy through strict adherence to mathematical principles and the accumulation of minute observations, Carpenter saw Bacon as a man of reason and not one who conducted his own observations.

There were many criticisms of *Vestiges*, and the disagreements over whether it was an acceptable scientific book split along differences in evidentiary practice. William Whewell wasted little time before attacking *Vestiges* and the entire transmutationist argument based in morphology and geology that it espoused.[158] In 1845, Whewell refuted, point-by-point, its assertions. More importantly, the exercise was an explicit attempt to reinforce his own evidentiary practices in light of the geological evidence for transmutation and the origin of species. The anti-*Vestiges* tract, titled *Indications of the Creator*, was a methodological argument set in the context of natural theology. Whewell made his position clear: the disciplined, inductive mind could ascertain the final cause and intentions of the Creator through a collection of minute observations; philosophical naturalists should reject inadequately supported speculations about final cause and the

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[157] William B. Carpenter, “Review of Vestiges of the Natural History of Creation,” *The British and Foreign Medical Review* ed. John Forbes (London: John Churchill, 1845), 168. 155-181.

[158] William Whewell, *Indications of the Creator* (Philadelphia: Carey and Hart, 1845), xvii.

mind of God.[159] Whewell was appealing to public religious sentiment regarding the orderliness of nature in the same way that *Vestiges* appealed to the dramatic increase of Victorian readership and desire for epic scientific literature.[160] At its base, Whewell's attack was drawn from his *History and Philosophy of the Inductive Sciences,* a touchstone of self-proclaimed Baconian inductive practices.

It was in this fierce competition between the differing philosophical naturalist communities that Charles Darwin would be trained to study the natural world. He would be given an eye to uncovering the underlying laws of nature at both the University of Edinburgh and, later, at Cambridge University. His training at both institutions provided him entry into both philosophical naturalist communities. It also changed his evidentiary practices; Darwin had been given two radically different tools to research the sea floor over time. For example – to return to the event with which I opened this chapter – the 1826 anonymous article "Observations on the Nature and Importance of Geology" was published while Darwin was a student at the University of Edinburgh. The article promised so much from the study of the fossil records, geology, and biological organization. A zoogeological study of the sea floor could even potentially unlock the secret of the origin of species. When Charles Darwin boarded the HMS *Beagle*, he was armed with the questions raised by the philosophical naturalist community and their accompanying techniques. And those were exactly the answers he found on his journey.

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159 Whewell, *Indications*, xi-xii.
160 Secord, *Victorian Sensation*, 220-221.

CHAPTER TWO: Living Fossils and Darwinian Evidence

This chapter examines the case of Charles Darwin as an exemplar of the evidentiary practices discussed in chapter one. Darwin was the proud inheritor of two different scientific legacies; he learned natural history at two elite British institutions, the University of Edinburgh and Cambridge University. He would gain an appreciation for marine invertebrate zoology at the University of Edinburgh. His Edinburgh instructors introduced him to cutting-edge scientific practices, such as the use of the naturalist's dredge and distribution studies. At Cambridge, Darwin also gained respect for the techniques previously used by astronomers to derive universal truths from minute observations. In addition, his Cambridge connections provided an opportunity to circumnavigate the globe on a British survey ship as a gentleman naturalist. Darwin's early educational experiences, as well as his voyage aboard the survey ship *Beagle*, sparked his lifelong study on the origin of species. However, Darwin did not choose to examine what he encountered around the world using the practices he learned at only one institution; the interest in the sea floor shared by both institutions allowed him to create a novel method for uncovering natural laws. This new method yielded remarkable insights into the workings of the natural world, but it also created a tension in the evidence he produced. He predicted that evidence for his theory of the origin of species would come in the form of intermediate forms he called “living fossils,” creatures that displayed morphological traits shared by two divergent species. Ultimately, there was one problem with Darwin's argument; he was unable to provide this evidence.

Darwin expounded upon this paucity of evidence with the same blended evidentiary practices derived from his dual training by naturalists at the University of Edinburgh and Cambridge University. The combination of those two types of evidentiary practice merged during Darwin's years aboard the *Beagle* and formed the cornerstone of his proof for natural selection. The evidentiary practices that Darwin encountered at Edinburgh and Cambridge were espoused not by any single individual, but by institution-based networks of naturalists. Dov Ospovat, in his canonical *The Development of Darwin's Theory*, issued a challenge for historians to view the emergence of Darwin's theory in the context of his naturalist contemporaries.[161]

Much excellent work has been conducted to show how these other scholars contributed to natural selection, but there is a tendency to present this collection of “peripheral” Darwinian contributors as constituting one homogenous network of scholarship. I have argued in chapter one of this dissertation that scientific practice varied significantly from one institution to the other. While far from being homogenous, local individuals within a network shared practices to pursue their studies of nature. The proximity of these naturalists ensured that they could pass down their scientific techniques to students and other local enthusiasts. This chapter will disentangle the different threads of Darwin’s methodological debts to the mentors and contemporaries from whom he learned seabed science.

These scientific practices varied greatly among institutions, such as Edinburgh University, Cambridge University, and the United States Coast Survey, even in relation to a similar subject. For example, naturalists associated with each of these institutions actively studied the seabed throughout the nineteenth century. Yet, Edinburgh scholarship relied upon its own set of practices, stories of scientific triumph, and assumptions about the pursuit of natural laws. Dredging became common practice for

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[161] Dov Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859* (Cambridge: Cambridge University Press, 1981), 1-5. See Ospovat's “Introduction” for the general call for seeing Darwin in relation to his contemporaries.

the zoogeologists of Edinburgh due to its close proximity to the Firth of Forth, a marine bay. Naturalist networks also engaged with similar philosophies of science, such as the unique Idealist science featured in Edinburgh journals and publications. While there are many ways to demarcate networks of naturalists, this chapter will focus on local evidentiary practices. Charles Darwin serves as a case study of merging scientific networks because he was trained at two major locations for the study of the seabed, Edinburgh and Cambridge.

Evidentiary practices are inevitably a type of methodology, or how a naturalist pursued the discovery of universal natural laws. The question of methodology becomes difficult to analyze historically when following large numbers of people and their interactions. An analysis of evidentiary practices narrows a historian's focus to a network's choice of informative scientific specimens, what they did to those specimens, and how they derived universal scientific laws from those objects. That focus allows the historian to manage otherwise unwieldy networks of correspondents and international collaborators as they collided over scientific method. In the case of Charles Darwin, the collision over evidentiary practice created an entirely new way to study the history of life itself.

The ability of Charles Darwin to move between two distinct and geographically separated scientific networks was partially related to his economic and social position in life. He was a wealthy son of the English professional class.[162] He had initially wished to follow in his father's footsteps as a brilliant physician, but his medical education at the

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[162] It is not my intention to add to the already well-established biographical literature on Charles Darwin. Rather, I would like to explore how his movement from Edinburgh to Cambridge scientific networks changed his scientific practice and use of evidence in natural selection. For further biographical information, I defer to Janet Browne's *Charles Darwin* and Adrian Desmond and James Moore's *Darwin: The Life of a Tormented Evolutionist* for biographical narratives.

University of Edinburgh was cut short due to an inability to tolerate the sight of surgery.[163] Instead, Darwin spent his time studying natural history with some success, but he left for home in 1827 after two years of study. Frustrated at his second son's perceived professional aimlessness, Darwin's father sent young Charles to Cambridge to prepare for a career in the Church of England, an acceptable vocation for a younger son of his middle-class station and reduced ambitions.[164] At Cambridge, Darwin did not excel at ecclesiastical pursuits, instead finding a growing passion for geology. After achieving a Cambridge BA and filled with wanderlust, Darwin was offered the chance to sail around the world to practice natural history exactly because of his gentlemanly status and connections to Cambridge.

Such a voyage offered Darwin the chance to indulge both his desire for adventure and his growing interests in natural history. During the nearly five year journey, he would encounter a natural world beyond the books, specimens, and landscape of the British Isles. Darwin's global experience aboard the *Beagle* not only gave him the authority to grapple with the question of scientific practice, but also forced him to navigate the methods of his two previous educational institutions in order to make sense of his scientific observations. Darwin's blending of the Edinburgh and Cambridge evidentiary practices later formed a fundamental part of his argument on natural selection. When Darwin published *On the Origin of Species* in 1859, he used seabed geology as a cornerstone of his synthetic evidence and reasoning.

As discussed in chapter one, Darwin's two universities were at tension regarding evidentiary practices during the early-nineteenth century. These two networks began to

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[163] Janet Browne, *Charles Darwin: A Biography, vol. I - Voyaging* (Princeton: Princeton University Press, 1995), 62-63.

[164] Francis Darwin, *The Life and Letters of Charles Darwin vol. I*. (London: John Murray, 1969) reprint by Johnson Reprint, 45.

collide over ocean floor science, with Edinburgh naturalists interested in zoogeological distribution of seafloor organisms and Cambridge’s using tidal prediction as part of their defense of their proud Baconian heritage. These two geographical networks were steeped in their own local institutions and natural environments. For example, the University of Edinburgh was only one hour's carriage ride away from an ideal marine environment for dredging and beach combing, and Darwin's diary notes his frequent trips to the Leith seaside.[165] This chapter traces how Darwin traveled between these institutions and, by merging their intellectual heritages together, used seabed science as proof for natural selection.

Dredging and Distribution as Practice: Darwin at the University of Edinburgh

In 1825, Darwin left Shrewsbury, England to study medicine at the University of Edinburgh after spending the summer apprenticed to his father. Although his education was standard fare for a young medical student at the time, the chemistry courses were exceptional and there were ample opportunities to learn natural history.[166] While, like many other students, he did enroll in Robert Jameson's natural history lecture, Darwin's first active participation in a professional scientific network occurred through the Edinburgh Natural History Society under the mentorship of Robert Grant, Jameson's pupil and a recent medical graduate from the University.

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[165] The Leith docks are situated upon the Firth of Forth shore. The area is now a district of Edinburgh, located north of the University.

[166] Darwin had practiced chemistry while young at his home before going to Edinburgh. He also maintained a lifelong passion for collecting beetles. Edinburgh gave him the first time to explore natural history in a systematic way and in an institutional setting.

Like many medical students, Grant took advantage of medicine's close affiliation with natural history.[167] After acquiring his medical degree at Edinburgh in 1814, Grant left to study in France, Italy, Germany, Switzerland, and Austria. Before leaving, he already admired the concept of transformism, the idea that new organisms had developed from previously existing types of living organisms. That inclination toward evolutionary thought was reinforced while abroad when he encountered French natural philosophy. Grant returned to Edinburgh in 1820 an expert in marine invertebrates, a Lamarckian naturalist, a radical reformer, and a dedicated transmutationist.[168] Back in Scotland, the Edinburgh professors welcomed the return of their pupil with open arms. Grant studied under John Barclay, the professor of comparative anatomy, even going so far as to teach the invertebrate section of his course in 1824.[169] By the time Darwin arrived at Edinburgh the next year, Grant had built a reputation for natural history based on his studies of sponges and other marine invertebrates, having contributed to the *Edinburgh Philosophical Journal* and later the *Edinburgh New Philosophical Journal* on the subject.

Grant also became a prominent member of Jameson's Wernerian Natural History Society and its congruent undergraduate student association, the Plinian Society.[170] Grant's reputation for exciting scientific conversation, his connections at the Plinian and Wernerian, and his zeal for marine biology drew a number of students who desired to

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[167] The study of natural history was often pursued through a degree in medicine.

[168] See Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago: University of Chicago Press, 1989), 52-92 for some of Grant's political background.

[169] Sarah Parker, *Robert Edmond Grant (1793-1874) and His Museum of Zoology and Comparative Anatomy (*University College London: Grant Museum of Zoology, 2006), 12. The anatomist was Dr. John Barclay of Surgeon's Square.

[170] As discussed in chapter one, the Wernerian Society was named after Abraham Gottlob Werner, the famous geologist. He was best known as the founder of the Neptunist, or Wernerian, geology that posited all geological land formations were the result of the slow recession of a universal ocean. The Wernerian Society was mostly a general natural history society, but Werner's theory remained a strong influence on the Society's thinking.

learn the techniques of philosophical naturalists. Grant would often take his students along on his dredging excursions. Darwin most likely met Grant through the undergraduate Plinian Society, where students gathered to discuss matters they encountered in their coursework. Much of the time, the topics included philosophical questions of animal classification, marine creatures, and sea plants. They also discussed their dredging excursions to the coast, “[Darwin] went scouring the Firth of Forth shoreline with his Plinian friends and accompanying the trawlers dredging the ocean bottom.”[171] There is a good chance that these student dredging excursions were prompted by Grant in his capacity as the 1825-1826 Secretary of the Society, whose enthusiasm for collecting at the Firth of Forth was burnished by his access to the latest Continental knowledge regarding marine invertebrates.[172]. Darwin quickly absorbed Grant's knowledge of dredging, zoophytes, and philosophical biology, becoming a personal associate of his mentor. Previous historians have examined Grant's intellectual relationship to Darwin in great detail.[173] In 1827, Darwin was just another eighteen year-old boy who aspired to be a famous physician like his father and grandfather. He was also an avid collector. He dabbled in stuffing birds, collecting beetles, and beachcombing with his brother Erasmus.[174]

Grant discussed Lamarck’s evolutionary theories with Charles, as well as to *Zoonomia,* the transmutationist treatise written by Darwin's grandfather, after learning of

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171 Desmond and Moore, *Darwin*, 33.

172 Sloan, “Darwin's Invertebrate Program,” 74-75.

173 See Sloan (1985), Hodge (1985), and Desmond (1991) for close examinations of Grant and Darwin's relationship. Also see James Endersby, “Escaping Darwin's Shadow,” *Journal of the History of Biology* 36(2003): 385-403 about examining evolutionary history with a bias towards Charles Darwin's future publications.

174 Charles' brother Erasmus, or Eras, was a fellow medical student at Edinburgh.

Darwin's familial relations.[175] His grandfather's work left Charles in admiration, but he failed to see the deeper implications of the treatise, which speculated on the development of new organisms from existing forms. Charles lacked the practical scientific experience that he needed to understand the questions that *Zoonomia* raised and Grant set out to instruct him in the practices and techniques, mostly through the study of sea floor fauna, that he would need in order to see his grandfather's transmutationist concepts reflected in the world around him.

Grant's expertise in marine zoophytes, not to mention the prevailing Wernerian context of the Edinburgh scientific community, shaped Darwin's abilities as a young philosophical naturalist. Darwin's first forays into publication relied upon his ability to use the naturalist's dredge. Grant had bought a house by the Leith seaside that allowed him to raise zoophyte colonies for observation. He published a frenetic burst of papers in Edinburgh journals on zoophyte biology while mentoring Darwin.[176] Darwin was greatly impressed by Grant and quickly set out to follow in his mentor's footsteps by making his own observations on zoophytes, the sea-mat *Flustra*, under Grant's tutelage.

In the latter half of 1826, Darwin began to accompany Grant to Wernerian Society meetings. Grant's sponsorship was a special treat since students were only allowed to attend as the guest of an official member. By March of 1827, Darwin was celebrated for his observations on the “peppercorn-like bodies found inside oyster shells, and thought by fishermen to be seaweed spores,” which turned out to be leech eggs. Darwin then gave his first scientific presentation before the Plinian Society on his observations on marine invertebrates. Darwin's scientific upbringing emphasized a

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[175] Browne, *Darwin*, 83. Browne argues that Darwin was already versed in evolutionary theories. However, I believe it is likely that Charles Darwin lacked the practical, hands-on experience needed to fully appreciate these theories until he was trained by Grant.

[176] Desmond and Moore, *Darwin*, 36.

knowledge of sea floor marine invertebrates as an extension of Grant's research. In turn, Grant's scholarship was a continuation of Lamarck's evolutionary idea that simple invertebrates could explain the organizational origin of both plants and animals.[177] Darwin was supposed to commit to a career as a physician while enrolled at Edinburgh, but his attention had instead been riveted to Grant's research.

Grant's connection to the Edinburgh naturalists later earned him a Professorship of Comparative Anatomy. He continued to teach marine dredging to his students when he moved to the University of London. Another of Grant's notable students, William Benjamin Carpenter, attributed his lifelong fascination with dredging to Grant's Comparative Anatomy class at London. Having returned from the West Indies in 1833, the twenty-one year-old Carpenter enrolled at the University of London a year later, just a Grant was establishing his courses on the subject.[178] The first scientific articles that Carpenter published acknowledged a great debt to Grant's lectures and expertise on marine invertebrates. Carpenter, continuing Grant's research, used marine organisms to explore the natural world, much like Darwin did a few years before. Carpenter, after exploring some geology, set out to Edinburgh, his mentor's intellectual home, to complete his medical studies in October of 1835.[179] Four years later, Carpenter had fully integrated his medical studies with the study of marine invertebrates by publishing an award-winning dissertation, "...on the physiological inferences to be deduced from the structure of the nervous system in the invertebrated classes of animals...," among other written works and lectures.[180] Carpenter became an expert on marine invertebrates, dredging, and comparative neurology. The combination of those subjects drew heavily

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[177] Desmond and Moore, *Darwin*, 37-42.

[178] Estlin J. Carpenter, "Memorial Sketch," in *Nature and Man, essays scientific and philosophical*, ed. Estlin J. Carpenter, (London: Kegan Paul, Trench & Co.,1888), 10.

[179] Carpenter, "Memorial Sketch," 13.

[180] Carpenter, "Memorial Sketch," 13.

not only from Robert Grant's mentorship, but also from the Edinburgh enclave's pervasive sea floor evidentiary practices. Dredging spread quickly as an identifying skill for the established philosophical naturalist.

Other scholars have noted the persistent effect that Darwin's marine invertebrate studies had upon his biological thought. Most notably, Darwin engaged in an “invertebrate program” of research before and during his voyage aboard the *Beagle*. Darwin's early Edinburgh education, under Grant's tutelage, shaped his biological thinking even years later when he encountered new specimens of zoophytes as a gentleman naturalist.[181] Both Darwin and Grant were engaged in the study of colonial sea floor organisms, called zoophytes. Zoophytes constituted a focal point for Edinburgh naturalists who investigated the organizational relationship between plants and animals.[182] Despite their shared interests, Darwin differed from Grant regarding their studies of these marine invertebrates in a number of ways, including whether or not zoophytes represented a link between simple animal and plant life. While Grant believed that the boundary between the plant and animal worlds blurred as the organisms became simpler, specifically as in simple marine invertebrates, Darwin maintained a definite difference between these two types of organisms.

Darwin's early interest and affinity for chemistry was probably the source of his rejection of Grant's blending of animal and plant organization at the simple zoophyte level. Darwin had already been introduced to advanced chemistry while living at home through his older brother, Erasmus.[183] The fascination continued into his medical

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[181] Sloan, “Darwin's Invertebrate Program,” 71-85.

[182] See also the sections on Forbes, Carpenter, Chambers, and Wyville Thomson in this dissertation.

[183] Silvan Schweber, “The Wider British Context in Darwin's Theorizing,” in *The Darwinian Heritage,* ed. David Kohn (Princeton: Princeton University Press, 1985), 35-69.

education with his attendance in Thomas C. Hope's intensive chemistry lectures during the 1825-1826 term, most likely before he was introduced to Grant.[184] Hope taught that there was a definite division between the plant and animal kingdoms due to the addition of “[the element] Nitrogene... in the Animal Constitution.”[185] As a young student interested in chemistry, Darwin felt much more uncomfortable bridging such an obvious difference between the animal and vegetable worlds than his mentor, Grant.

Many Edinburgh naturalists researched or commented upon invertebrates and what they explained about the order of living forms, even chemists. Scholars have portrayed Darwin's invertebrate program as “Grantian,” thereby placing Darwin's interest in sea floor biology in relation to one person. However, as asserted in chapter one, many others in the Edinburgh scientific network also explored seabed biology and shared a number of evidentiary practices for exploring the relationships between different marine invertebrates over geological time. Many people in this Edinburgh group, including the influential Edward Forbes, believed that the relationship between various sea floor invertebrates could explain the inner workings of life's organization. For some, simple marine invertebrates provided a comprehensive way to study more complex physiologies. For Forbes and Grant, the simplest invertebrates could help naturalists adjudicate the ordering of life's organisms by demonstrating which organisms appeared first in chronological order. Forbes explored that chronological order in relation to the geographical origin and the frequency at which new species appeared. Alternatively, Grant's interest in marine invertebrates explored how simple marine creatures represented a past physiological and morphological divergence between the plant and animal kingdoms. The evolutionary implications of Grant's research program, and

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[184] Sloan, “Darwin's Invertebrate Program,” 85.

[185] Sloan “Darwin's Invertebrate Program,” 85. Original from T.C. Hope MSS, UEL, Gen. 268, Box 1, Item 14.

therefore Darwin's early research experience, rested firmly within a larger Edinburgh scientific interest in the historical sea floor.

Darwin also learned a number of evidentiary practices from this Edinburgh network. Like Grant, Darwin utilized the naturalist's dredge to collect sea floor invertebrate specimens. For example, in 1831, Darwin contacted members of his Edinburgh scientific network, including the 1824 and 1825 president of the Plinain Society John Coldstream, to inquire about the collection of marine animals in preparation for his *Beagle* voyage. Coldstream replied,

> As I have paid very little attention to Natural History of late, I feel myself but ill prepared to give you any information which might be of service to you:—but with regard to the collecting of marine animals, I may state, that I think a common oyster-trawl, of *the ordinary* size, would prove very serviceable. This you may readily procure in any of the fishing villages at the mouth of the Thames, (if not in London)—but, as you wish it, I shall sketch a figure of the dredge or trawl usually employed in the Firth of Forth.
>
> The frame is made of iron, and measures... about 3 feet;—the bar which scrapes the ground has a blunt edge in front; the lower surface of the bag is formed of iron rings, the upper of strong netting...
>
> You might supply yourself also with a few lobster traps of various constructions. Many of the rarest of our Mollusca and Zoophytes are found adhering to the deep sea fishing lines; (such as are set for cod and haddock, and allowed to remain at sea for many hours together undisturbed). When at anchor, you might 'shoot' some such lines, with small pieces of worm:eaten wood, or small baskets &c, as well as hooks, attached to them: by leaving these in the water over night, sunk to a considerable depth, you might obtain a rich supply in the morning."[186]

Coldstream's reply made obvious the importance of dredging practices – among other methods – in the collection of marine animals. However, the letter also implies the role that dredging played for the Edinburgh network. Despite eschewing most natural history

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[186] DARC, Frederick Burkhardt and Sydney Smith, eds, *The Correspondence of Charles Darwin* (Cambridge: Cambridge University Press, 1985), v1: 151-152. John Coldstream to Charles Darwin, 13 September 1831.

in order to fulfill his duties as a medical professional, Coldstream retained an intimate knowledge of dredging techniques.

Through Grant's tutelage, Darwin also learned to use the microscope to conduct morphological observations of marine invertebrates. In addition to morphological description, he also described the generative processes of simple, sea floor organisms. For example, Darwin's Edinburgh Notebook is filled with morphological descriptions and speculations as to their reproductive or developmental functions. His 1827 description of the common lump fish captures his descriptive interests and his desire to use morphological descriptions to position marine animals within their differing ocean environments:

> March 16th 1827.
>
> Procured from the black rocks at Leith a large Cyclopterus Lumpus (common lump fish). Length from snout to tail 23 ½ inches, girth 19 ½. It had evidently come to the rocks to spawn & was there left stranded by the tide; its ovaria contained a great mass of spawn of a rose colour. Dissected it with Dr Grant. - Eyes small. - Hence probably does not inhabit deep seas?[187]

While the lump fish is a vertebrate animal, the notebook is also interspersed with microscopical sketches of marine invertebrates. These include the morphological description of the sea mat *Flustra*, which Darwin later presented to the Edinburgh Wernerian Society.

> Observed ova in the Flustra Foliacea & Truncata, the former of which were in motion.-
>
> 28th

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[187] Charles Darwin, "On the Ova of Flustra, or, Early Notebook, Containing Observations Made by C.D. When He Was at Edinburgh, March 1827," in *The collected papers of Charles Darwin. 2 vols.* ed. P. Barrett (Chicago: University of Chicago Press, 1977), 285-91.

> Adhering to the Fuci one frequently finds whitish circular masses of Ova, of an extremely viscid consistence,—
>
> ...when magnified however, it appears to be a mass [of] capsules P containing animals d, united together by a transparent gelatinous matter. In this species I believe I was the first to observe both the animal d & its ciliae, x, in most rapid movement. By the aid of these ciliae it could revolve in its capsule & when freed from it moved so quickly, as to be discernible to the naked eye at some distance. — To what animal these ova belong. I am ignorant? —[188]

Darwin later remembered the research projects as successful, though he also recalled the limitations of many of his other attempts at the Edinburgh evidentiary practice, “But from not having had any regular practice in dissection, and from possessing only a wretched microscope, my attempts [at studying marine zoology] were very poor.”[189] Darwin learned dredging and microscopical techniques to obtain samples that the Edinburgh network found valuable. By extension, the physical samples represented an assumption about what objects could lead to the production of scientific information. The samples and the philosophical process of deriving biological laws from them were all part of the Edinburgh evidentiary practice.[190]

While modest about his first forays into marine zoology at Edinburgh, his connection to the marine zoological community persisted into his time at Cambridge and beyond. His marine zoologist friend John Coldstream – who by this time had turned from natural history to become a physician – later wrote Darwin and asked specifically about natural history at his new educational institution, “Be so good as write me again soon, and tell me something of the present state of Natural History in Cambridge. Have you

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[188] Darwin, “Early Notebook,” 288.

[189] Darwin, *Life and Letters,* 39.

[190] As discussed in chapter one, the Edinburgh network combined both French and German biological philosophies together into a unique scientific Idealism. Grant and Forbes labored under an influential assumption at Edinburgh, that their samples were imperfect reflections of universal biological truths. Speculation and deductive law-building were valued more than focusing too much upon one sample or measurement.

had any opportunity of studying marine Zoology since you left this?"[191] Indeed, the educational emphasis that Darwin encountered at the English university was much different than that of Edinburgh.

Measurement as Practice: Darwin at Cambridge University

Darwin arrived at his new university on 26 January 1828, with the academic year already in full swing.[192] As opposed to the marine invertebrates, medicine, and chemistry of Edinburgh; he encountered the "eternal sciences" of Cambridge, mathematics, the Classics, theology, and moral philosophy. Those subjects prepared Cambridge bachelor of arts students for further theology and divinity studies, something Darwin would need for an ecclesiastical career. The difference in emphasis between Edinburgh and Cambridge was easily recalled by Darwin even later in life. He had to work especially hard to reclaim his knowledge of Classics and mathematics. In his autobiography, Darwin recounted his struggles with the shift back to Cambridge subjects, "...as I had never opened a classical book since leaving school, I found to my dismay, that the two intervening years [before Edinburgh] I had actually forgotten... almost everything which I had learnt, even to some few of the Greek letters... I attempted mathematics, and even went during the summer of 1828 with a private tutor to Barmouth, but I got on very slowly."[193] As explored in chapter one, the dominant Cambridge evidentiary practices involved extensive mathematical computation and non-speculative reasoning by the

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[191] DARC v1: 78, John Coldstream to Charles Darwin, 28 February 1829, specifically about the prevalence of marine zoology at his different institution, Cambridge

[192] John Van Wyhe, "Charles Darwin's Cambridge Life 1828-1831," *Journal of Cambridge Studies* 4 (2009): 2.

[193] Darwin, *Life and Letters*, 46.

gathering of minute measurements. Darwin had some trouble adjusting to these radically different practices and institutional values.

Despite the new emphasis, Darwin never attained a graceful facility with the mathematics so valued by his Cambridge network. He wrote to his cousin and confidant William Darwin Fox one day about his difficulties with the subject and wondering why he had not received correspondence from him yet, "...I hope it arises from your being 10 fathoms deep in the Mathematics, & if you are God help you, for so am I, only with this difference I stick fast in the mud at the bottom & there I shall remain in statu quo.—"[194] Interestingly, Darwin's language reverted to the practices of dredging, "fathoms" and the muddy sea "bottom," that he had learned at Edinburgh. Darwin's correspondence from this period also illustrates the shift in evidentiary practices he experienced when he moved to Cambridge. Even though he was immersed in a new, mathematically focused institution, Darwin still wrote about the sea floor he had become familiar with while at Edinburgh. Other Edinburgh friends, such as Coldstream, inquired about marine biology in turn. Darwin maintained his Edinburgh contacts while at his new institution and corresponded about scientific practices. Those two systems of evidentiary practice created tension within Darwin's thinking since the Cambridge network relied heavily upon measurement, computation, and mathematical predictions.[195]

Darwin completed the requirements for his Cambridge degree, but had to stay at the university to fulfill his residence requirement. A few significant things happened during that last year at Cambridge. First, Darwin recalled reading two books that

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[194] DARC v1: 62, Charles Darwin to William Darwin Fox, 29 July 1828.

[195] Darwin's lack of mathematical ability does not suggest that he did not use mathematical reasoning in his work. Much the opposite, I show later how the Cambridge mathematical method of reasoning is used in *On the Origin of Species*. Janet Browne, in "Darwin's Botanical Arithmetic, 1854-1858," shows how Darwin used mathematical techniques to explore botanical distribution.

changed his perspectives regarding science, Alexander Von Humboldt's *Personal Narrative* and Sir John Herschel's *Introduction to the Study of Natural Philosophy* [*Preliminary Discourse*].[196] Of course, one must be cautious at accepting a historical subject's autobiographical reflections as absolute truth, but in this case Darwin's personal correspondence agrees with his biographical account of the effect Herschel's treatise on scientific method had upon his thinking. On 15 February 1831, Darwin urged his cousin Fox to share in his experience reading *Preliminary Discourse*; “If you have not read Herschel... read it directly.”[197] Out of all the Cambridge philosophical scholars, it is probable that Sir John Herschel and his introduction to natural philosophy was the most influential for the young Darwin.

Preliminary Discourse primarily concerns itself with the disciplining of scientific evidence. Herschel, like many other naturalists of his time, believed that science's ultimate endeavor was the discovery of universal laws of nature. *Preliminary Discourse* was essentially a manual on which philosophical and physical practices would lead to that higher purpose of science. Herschel's book argued that observations should be conducted with enough precision to be useful in the discovery of those universal, natural laws. The overwhelmingly prevalent practice advocated by Herschel was characteristic of his participation in the Cambridge scientific network, “Indeed, it is a character of all the higher laws of nature to assume the form of precise *quantitative* statement.”[198] Precise, quantitative measurement was the secret to the discovery of natural laws and the main message of the treatise.

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[196] Darwin, *Life and Letters,* 55. The juxtaposition of the Cambridge inductivist book *Preliminary Discourse* and the German Romantic *Personal Narrative* has always struck me as a microcosm of the tension between Edinburgh Romantic Idealism and Cambridge Inductivism that Darwin constantly navigated.

[197] DARC v1: 118, Charles Darwin to William Darwin Fox, 15 Feb 1831, post script.

[198] John Herschel, *Preliminary Discourse on the Study of Natural Philosophy* (London: A. Spottiwood, 1831), 123-124, emphasis original.

Preliminary Discourse is mostly known to philosophers of science for its hypothetico-deductive methodology. However, the book predominantly addresses the philosophical practices of a naturalist. For example, there were two important issues that Herschel connected to that essential collection of quantitative measurement: instrumentation and prediction. Instrumentation represented the turning point that allowed naturalists to produce quantitative measurements, “What an important influence may be exercised over the progress of a single branch of science by the invention of a ready and convenient mode of executing a definite measurement, and the construction and common introduction of an instrument adapted for it...”[199] Such commonly-used, quantitative instruments would have two effects upon the philosophical naturalist: they would standardize the efforts of a scientific community while also focusing that community on the acquisition of numerical data. Herschel's advocacy of Cambridge evidentiary practices took a typical form, the inductive collection of numerical data that could be effectively combined with other naturalists' measurements and observations. Indeed, according to Herschel, the ideal example of instrumental data collection – when applied to geology – was the same project to determine the permanence of the sea floor proposed by William Whewell that I discussed in chapter one of this dissertation.

Herschel also shared a second philosophical similarity with his Cambridge network: an adherence to the predictive value of correct scientific knowledge. The accumulation of precise mathematical measurements was essential for the verification of scientific laws, the end goal of the entire scientific endeavor for Herschel. The connection between measurement and prediction that Darwin was exposed to is best summarized in Herschel's chapter on the formation and verification of theories:

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[199] Herschel, *Preliminary Discourse*, 354.

> The importance of obtaining exact physical data can scarcely be too much insisted on, for without them the most elaborate theories are little better than the mere inapplicable forms of words... we need no more [than calculated measurements] to enable us to predict all the movements of [the sun and planets'] several parts, and the changes that will happen in it for thousands of years to come...

> The proof, too, that our data *are* correctly assumed, is involved in the general verification of the whole theory, of which, when once assumed, they form a part; and the same comparison with observation which enables us to decide on the truth of the abstract principle, enables us at the same time, to ascertain whether we have fixed the values of our data in accordance with the actual state of nature... Thus it happens, that as theories approach to their perfection, a more and more exact determination of data becomes requisite.[200]

Herschel recognized that instrumentation and the collection of exact, mathematical data was necessary to predict natural phenomena in the distant future or past. That absolute predictability was needed to ensure the veracity of a universal natural law, and even to calculate unseen aspects of the natural world out of discrepancies in their predictions, such as the unknown planet Neptune discovered by Herschel's father. Cambridge measurement was the practice by which Herschel provided evidence for grand laws of nature. Predictions based on those measurements provided the soundest verification of that law. Darwin, already intrigued by scientific practice, took explicit note of Herschel's treatise on evidentiary practice to the point that he could recall its influence upon his thinking in his later years.

Other scholars have recognized Darwin's indebtedness to Herschel's *Preliminary Discourse*. Specifically, Darwin seemed to assimilate Herschel's philosophical method of deliberating between competing scientific explanations for a phenomenon, the search for explanations that can be independently verified. Herschel called these explanations

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[200] Herschel, *Preliminary Discourse*, 212-213.

verae causae.[201] Herschel invoked Isaac Newton's definition of *verae causae* as "causes recognized as having a real existence in nature, and not being mere hypotheses or figments of the mind."[202] In essence, causal evidence should be supported by observable, if not measurable, instances in nature. That privileging of physical phenomena as evidence for larger laws of nature directly conflicted with the Continental, Idealist evidentiary philosophies of the Edinburgh group. However, the example that Herschel used is most instructive.

Like many other nineteenth-century British treatises on natural philosophy, the *Preliminary Discourse* invoked the sea floor – or, in this case, its fossil remains – as an example of how to properly construct natural knowledge from observations. For Herschel, the puzzle of "The phenomenon of shells found in rocks, at a great height above the sea..." served as a perfect demonstration of how philosophically minded naturalist should choose among multiple potential explanations:

> By some [the cause of fossil shells on top of mountains] has been ascribed to a plastic virtue in the soil; by some, to a fermentation; by some to the influence of the celestial bodies; by some, to the casual passage of pilgrims with their scallops; by some, to birds feeding on shell-fish; and by all modern geologists, with one consent, to the life and death of real mollusca at the bottom of the sea, and a subsequent alteration of the relative level of land and sea. Of these, the plastic virtue and celestial influence belong to the class of figments of fancy. Casual transport by

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[201] See Michael Ruse, "Darwin's Debt to Philosophy: An Examination of the influence of the Philosophical Ideas of John FW Herschel and William Whewell on the Development of Charles Darwin's theory of Evolution," *Studies in History and Philosophy of Science Part A* 6 (1975): 159-181, Ruse, *Darwinian Revolution*, Michael Ruse, "Darwin and Herschel," *Studies in History and Philosophy of Science Part A,* 9 (1978): 324, and Johnathan Hodge, "Darwin's Argument in the Origin" *Philosophy of Science* 59 (1992): 461-464.

[202] Herschel, *Preliminary Discourse*, 144.The language used later in *Discourse*, 209, reads, "[The causal agents in any theory] must be *verae causae*, in short, which we can not only show to exist and to act, but the laws of whose action we can derive independently, by direct induction, from experiments purposefully instituted; or at least make such suppositions respecting them as shall not be contrary to our experience, and which will remain to be verified by the conclusions which we shall deduce from them, with facts." The differing ways in which naturalists interpreted Herschel's meaning of *vera causa* becomes a subject of consideration in chapter five of this dissertation.

> pilgrims is a real cause, and might account for a few shells here and there dropped on frequent passes, but is not extensive enough for the purpose of explanation. Fermentation, generally, is a real cause, so far as that there *is such a thing*; but it is not a real cause of the production of a shell in a rock, since no such thing was ever witnessed as one of its effects, and rocks and stones do not ferment. On the other hand, for a shell-fish dying at the bottom of the sea to leave his shell in the mud, where it becomes silted over and imbedded, happens daily; and the elevation of the bottom of the sea to become dry land has really been witnessed so often, and on such a scale, as to qualify it for vera causa available in sound philosophy."[203]

It should be noted that Herschel did not solely reference the new works of Charles Lyell, the geologist who was famous for influencing Darwin later while aboard the *Beagle*. Rather, Herschel claimed that "all modern geologists" held that the above-water world was once the sea floor and that geological formations were subjected to submarine activity. Herschel's statement must be seen in the larger context of sea floor studies during the nineteenth century. The seabed provided a subject of intense curiosity to many naturalists during this time, from many different regions and disciplines, not just Lyell. And there is perhaps no greater proof that the sea floor provided a mechanism for philosophical naturalists to argue over methodologies than when Sir John Herschel, possibly the most influential British naturalist of the century, used the sea floor to explain his method of weighing scientific evidence.

Darwin's introduction to Herschel's work took place in the context of his becoming acquainted with the larger concerns of the Cambridge network. He took the occasional walk with the well-known Cambridge inductivist William Whewell, discussed in the last chapter, and the two men maintained a correspondence for a number of years

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[203] Herschel, *Preliminary Discourse*, 144-145. It should be said that Herschel used many more cases in his treatise than are mentioned here. However, I am certain that Herschel chose the sea floor example to illustrate what he meant by *vera causa* because of its prevalence and importance to the 19th century methodology debates.

after Darwin’s Cambridge days.[204] And Darwin remained indebted to both Whewell and Herschel for his philosophical foundations.[205] The ubiquity of seabed science swept Darwin into a methodological conflict that was already brewing. Darwin's later career as gentleman-naturalist aboard the HMS *Beagle* allowed him to apply the seabed evidentiary practices he had already learned, thereby – probably unwittingly – creating a blend between the Cambridge and Edinburgh versions of philosophical naturalist methods.

Darwin's Cambridge affiliation also afforded him some opportunities that were not readily available to other young naturalists. For example, John Stevens Henslow, the Cambridge professor of botany, became an influential mentor for the young Darwin. Henslow encouraged his studies and interests in natural history, becoming Darwin's friend in later years. Henslow's botany courses, which Darwin attended from 1829 to 1831, were the only formal scientific training that he received while at Cambridge.[206] And while Henslow taught botany at Cambridge in his official capacity, he remained interested in mineralogy and geology, much like Adam Sedgwick, the Woodwardian Professor of Geology at Cambridge. Both professors taught Darwin the practical aspects of geological practice. Henslow acclimated Darwin to the clinometer, an instrument to measure angles, and trigonometric calculations for the study of geological inclinations and formations. Sedgwick took Darwin on a scientific excursion to Northern Wales in the summer of 1831. This field excursion, like the marine invertebrate studies of Edinburgh,

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[204] Darwin, *Life and Letters*, 54.

[205] Michael Ruse, in “Darwin and Herschel,” suggests this indebtedness, which I believe to be correct and verified through Darwin's correspondence. See also Ruse’s *The Darwinian Revolution: Science Red in Tooth and Claw* (London: University of Chicago Press, 1979) and David Hull’s *Darwin and His Critics*, viii, for more on this subject.

[206] Browne, *Darwin*, 118. See her chapter “The Professors” for an excellent summary of the influence of both Henslow and Sedgwick upon the young Darwin while at Cambridge.

allowed Darwin to practice the techniques he had learned from his scientific network.[207] Darwin's connection to his Cambridge network continued long after he returned from his expedition to Wales and graduated with his baccalaureate from Cambridge University. In August of 1831, Darwin received a letter from Henslow relaying another opportunity made possible by his integration into the Cambridge scientific network of patronage and mutual assistance.

Blending Practices: Darwin Aboard the HMS *Beagle*

The position forwarded by Darwin’s Cambridge mentor was an offer to be gentleman companion to Robert FitzRoy, the captain of the survey ship *Beagle*. The *Beagle* had been ordered on a two-year voyage to observe and measure the hydrographical conditions around Britain's South American commercial and colonial interests in Terra del Fuego. The ship's mission was a scientific one, mostly measuring coastlines and sounding for dangerous shoals along the South American continental shelf. FitzRoy had a dangerous and solitary voyage ahead of him. That same hydrographical surveying mission had worn upon the *Beagle*'s previous captain Pringle Stokes enough that he shot himself in the head. A little over a decade earlier, FitzRoy's uncle, the third Marquis of Londonderry, had also slit his own throat after a particularly bitter political career.[208] Himself a passionate man, FitzRoy feared that his fate would be similar to his uncle and predecessor as captain. A scientific companion would help alleviate the burden of solitary command. FitzRoy turned to Pringle Stokes to find him a suitable gentleman companion and the opportunity traveled the Cambridge network until

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[207] Browne, *Darwin*, 136-143.
[208] Browne, *Darwin*, 146.

it reached Darwin. Darwin inquired further into the matter, receiving a letter from Henslow's associate and fellow Cambridge graduate George Peacock in response:

> I received Henslow's letter last night too late to forward it to you by the post, a circumstance which I do not regret, as it has given me an opportunity of seeing Captain Beaufort at the admiralty (the Hydrographer) & of stating to him the offer which I have to make to you: he entirely approves of it & you may consider the situation as at your absolute disposal: I trust that you will accept it as it is an opportunity which should not be lost & I look forward with great interest to the benefit which our collections of natural history may receive from your labours.
>
> The circumstances are these
>
> Captain FitzRoy (a nephew of the Duke of Graftons) sails at the end of September in a ship to survey in the first instance the S. Coast of Terra del Fuego, afterwards to visit the South Sea Islands & to return by the Indian Archipelago to England: The expedition is entirely for scientific purposes & the ship will generally wait your leisure for researches in natural history &c...
>
> The ship sails about the end of September & you must lose no time in making known your acceptance to Captain Beaufort, Admiralty hydr I have had a good deal of correspondence about this matter, who feels in common with myself the greatest anxiety that you should go. I hope that no other arrangements are likely to interfere with it.
>
> Captain will give you the rendezvous & all requisite information: I should recommend you to come up to London, in order to see him & to complete your arrangements I shall leave London on Monday: perhaps you will have the goodness to write to me... to say that you will go
>
> The Admiralty are not disposed to give a salary, though they will furnish you with an official appointment & every accommodation: if a salary should be required however I am inclined to think that it would be granted
>
> Believe me My dear Sir Very truly yours Geo Peacock
>
> If you are with Sedgwick I hope you will give my kind regards to him[209]

The letter to Darwin regarding the position illustrates the circumstances of the offer; there was an intimate network of Cambridge naturalists involved in the appointment. In part, Darwin was able to accept this opportunity not only because of his social standing

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[209] DARC v1: 129-130, George Peacock to Charles Darwin, c. 26 August 1831.

as a gentleman and the wealth available to him, but also because he had an interest in and facility with up-to-date geology and natural history. The *Beagle* opportunity would allow Darwin to travel around the world and earn a reputation as a practicing naturalist.[210]

Young Charles began his frantic preparations for the journey and made immediate use of both his Cambridge and Edinburgh networks in the process. Under great stress to prepare for a journey unlike any he had undertaken before, Darwin turned to familiar people for assistance. The most obvious place to start was with Henslow and his connections within Cambridge itself. Henslow gave Darwin letters of introduction to aid his preparations. The first obstacle that Darwin would face would be the selection of equipment. He could not bring many items with him, so each piece had to reflect Darwin's scientific interests and knowledge.

In the choice of equipment, Darwin's ideas about evidence played a crucial role, affecting the nature of the scientific work he would do during the voyage. Darwin gathered his geological compass and other equipment he had become acquainted with under Henslow and Sedgwick's tutelage.[211] Guns and Spanish language books made it on Darwin's short list of things to bring. Sedgwick introduced him to a number of geological texts, even ones "filled with Wernerian nonsense," the geological theories of his Edinburgh rivals.[212] Beyond these basics, Darwin also brought his microscope and, as already noted, he sought advice on dredging equipment from his old Edinburgh colleague John Coldstream. Aboard the *Beagle*, the mixture of these gathered instruments – and the evidentiary practices they represented – left Darwin little choice

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[210] Browne, *Darwin*, 145.

[211] DARC v1: 143-144, Charles Darwin to Susan Darwin, 6 September 1831.

[212] DARC v1: 157-158, Adam Sedgwick to Charles Darwin, 18 September 1831.

but to blend his Edinburgh training with that of Cambridge. And his observations would gravitate to the sea floor, the crucial geography that both scientific networks had trained him to observe.

Darwin's Beagle voyage has been well researched by a number of scholars, along with a number of wonderful biographical and autobiographical accounts of the work done while he was in South America and the Pacific.[213] What is missing from this scholarship is an appreciation of the fact that a major portion of that voyage was spent engaged in seabed science. This Cambridge and Edinburgh training combined in novel ways. Later, those melding practices dominated the evidence he used in *On the Origin of Species*.

The primary objective of the voyage was to survey South American harbors and marine passages. The voyage also had a number of subsidiary objectives, including the investigation of coral reef formation. This undertaking required frequent sounding the sea floor to determine how shallow the waterways became near the shore. Darwin took a keen interest in the soundings and measurements used by the *Beagle* crew.[214] Darwin's cabin-mate was John Lort Stokes, the ship's Assistant Surveyor, so between Stokes and FitzRoy, Darwin was well informed of the voyage's surveying efforts. He began to regularly record and tabulate of the ocean depths on the reverse side of his

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[213] See Frank Sulloway, “Darwin’s Early Intellectual Development: An Overview of the Beagle Voyage (1831-1836),” in *Darwinian Heritage* (Princeton: Princeton University Press, 1985) and selected articles from MacLeod and Rehbock's *Darwin's Laboratory.* For more-recent additions to *Beagle* scholarship, see Pearn, Alison M. ed., *Charles Darwin and the Beagle collections in the University of Cambridge* (Cambridge. Cambridge University Press, 2009) and Sponsel (2009) as discussed later in this chapter. Sponsel, especially, contributes to scholarship regarding the *Beagle* experience and Darwin’s marine science methodology.

[214] Alistar Sponsel, “Coral Reef Formation and the Sciences of Earth, Life, and Sea, *c*. 1770-1952” (PhD diss., Princeton University, 2009). Sponsel argues that Darwin's coral reef theory was a direct product of the maritime practices employed by the *Beagle* crew on their mission. See especially chapter two of his dissertation for this argument.

zoogeological notes.[215] Being steeped in seabed surveying and geologizing, a considerable portion of Darwin's thought – and consequently his Beagle notebook – was dedicated to thoughts on seabed geology and dynamics. The minute tabulation of ocean depths and water levels ultimately combined with his notes on morphological forms and their distribution along the sea floor.

As an example of the meticulous seabed studies that Darwin conducted, here are his notes on his finds between the Falkland Islands and St. Cruz:

> April 1834.
>
> (The lead brings up every thing in a circle, diameter of which 2 & 1/2 inches. -)
>
> The First soundings, obtained after entirely leaving the Falkland group were at noon in Lat. 50°.2'. Long. 63°.25'. Distance from nearest part of coast of Pat: 195 miles. - depth 85.
>
> Fathoms:
>
> The bottom was apparently a mottled sand, but really was composed of very minute. 1/80th to 1/100th of inch rounded fragments of black & reddish rocks & transparent quartz. it appeared to part in bed of sand. - These some 40 miles to the Westward examined where they were rather larger. - & will be described
>
> With the lead there came up a fragment of Echinus (perhaps allied to Cnidaris. Large pointed striated spines & smaller ones of dark red color. - I should not be surprised if it should eventually be proved this tube (Mem. Echina & Echinus in 57 + 50 Fathoms coast of Patagonia) was commonly inhabitants of great depths. -
>
> In the noon of the next day (11th)...
>
> 40 miles NE of C. Virgins in 50 Fathoms piece of an Echinus was brought up. -
>
> Abreast of Gallegos, out at sea. 57 fathoms fragment of Pecten & Ophiusmis
>
> I observe sounding from 40 to 60 fathoms on coast of S. Patagonia the bottom chiefly consists of small pieces of Balanidae.

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[215] Sponsel, “Coral Reef,” 88.

> Between Staten land & Falklands in 50-70 fathoms consisted of small stony corallines. minute fragments of shells. Spirotis. -
>
> Porph. Pebbles Falkland Land off Staten lond
> Dec. 17th. Lat 43°.30' S. the water seems in shade perpendicularly stay very pale blue, was of a remarkable colour: "Venditer blue" with tinge of green & milk. - Depth 55 fathom sandy bottom...[216]

While Darwin did not spend the majority of his time at sea, a significant portion of his ocean-bound time consisted of sustained observation of the seabed, especially using the soundings of the *Beagle* survey to determine the constitution of the ocean floor. One might also observe that these notes were an essential part of the coralline study that Darwin embarked upon, which historian Philip Sloan asserts was crucial to Darwin's questioning of the organizational relationship between simple marine invertebrates.[217] These observations then led to two more linked studies, one of the tidal action and the other of seabed formation over time. Darwin noted the resulting tidal action upon shells along the coast later in his diary:

> The tides are on this coast very powerful the rise being about 40 feet all along the coast... tide runs N & S. I was much surprised at finding at the distance only of 15 miles pebbles only about 0.4 of inch large. - And these form so great a bed of shingle as the Patagonian one. - Very minute ones are as we have seen present at 195 miles. - From the same cause I was surprised at not meeting with fragments of littoral shells, as muscles & limpets which so abound in the coasts.
>
> The following fact would seem to prove that even at trifling depths the water has little power over even small pebbles. - Outside of the bay & completely exposed & about 3 miles from shore in 10 fathoms water when tide rises & falls 40 feet pebbles of various series, some not with greater diameter than 1/2 of inch, were encrusted with species of Flustraceae, which were living & producing eggs: one [of] these had the cells provided with most delicate stony setae, barely visible to the naked eye: the whole surface of the bottom was closely striated with thin pebbles (there were very many soundings taken). now it is impossible to suppose these are

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[216] Charles Darwin, *Geological diary: Observations on the bottom of the sea between the Falkland Islands & St. Cruz*. (4.1834-1.1835) CUL-DAR34.87-92 Transcribed by Kees Rookmaaker, ed. John van Wyhe (Darwin Online, http://darwin-online.org.uk/): 114.

[217] Sloan, “Darwin's Invertebrate Program,” 103-109.

> ever agitated one against the other, else the Corallines encrusting convex surfaces, could not retain their setae. - These pebbles were not protected in hollows or behind large blocks, for the lead would have showed either of these cases.- [218]

The same subject of tidal action was also noted in Darwin's red notebook, which was used more for theorizing than actual observation.[219] Here, Darwin began to speculate upon the tides, a Cambridge fascination and its relationship to the continuity of the sea floor and surface geology, an Edinburgh assumption passed down from Robert Jameson in his lectures and alluded to in Herschel's *Preliminary Discourse*.

Darwin also blended Cambridge calculation with Edinburgh interest in seabed constitution. His geological notes from the *Beagle* voyage contain a curious, two-page chart titled "Attempt to find general inclination of the bottom of the sea off the coast of Patagonia."[220] Here, Darwin explicitly combined the incline calculations taught to him by Henslow with Jameson's interest in oceanic sediment accumulation and marine subsidence. Slowly, though such measurements and investigations, Darwin began to see how Lyell's gradualism could provide a plausible explanation for modern geological formations. Darwin would have also been deeply impressed by the concurrence between Herschel's example for *vera causa*, the appearance of fossil shells high in the mountains, and Lyell's theory. Darwin may have been unaware that Lyell was at least one contributor to Herschel's philosophical thinking.

The first half of Darwin's red notebook, which he filled while still skeptical of Lyell's gradualism, mentions the formation of the ocean floor quite often. Seabed

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[218] Darwin, *Geological diary, 89*

[219] For a very brief example, consider Darwin's "General reflections on the geology of the world," p. 77 of Sandra Herbert's *The Red Notebook of Charles Darwin*, where he notes that the "action of sea on coast" is a fundamental aspect of global geology. Sandra Herbert ed., *The Red Notebook of Charles Darwin* (Ithaca and London: Cornell University Press, 1980).

[220] CULA MSS DAR 34: 112.

drawings constitute the majority of his sketches in the red notebook. Starting on page 10, Darwin discussed volcanic formations as originating at the bottom of the ocean,

> The view of the Volcanos of the chain of the Cordilleras as arising from... faults or fissures, produced by the elevations of those mountains on the continent of S. America is inadmissible... The volcanos originated in the bottom of the ocean & the present Volcanos have been said to be merely accidental apertures still open... That axis was produced, from a fissure in a deep & therefore weak part of the ocean's bottom.[221]

Darwin later speculated upon the lack of fish in the deep sea in his notebook and how combined sedimentation and tidal action forms the sea floor and also determines its underwater inclination. However, halfway through the notebook, Darwin's seabed observations shifted to speculations of how seabed action might affect terrestrial formations and the organisms inhabiting the land. The event responsible for this shift in thinking was also one of his most salient memories from the South American portion of the voyage.

On 19 January 1835, Darwin witnessed the eruption of Orsono, a Chilean volcano. In his published traveling narrative of the event, he recalled the bright red glare on the midnight sky. Many other volcanoes in the region erupted, spewing molten earth into the sky. This explosion would trigger a series of events, both within the geological region as well as within Darwin's thinking, that would forever change his theories about the terrestrial globe. The entire region had become geologically active, starting with the Chilean eruptions. Simultaneously, Darwin began researching the formation of new land and its relationship to the bottom of the ocean. Undersea volcanoes that protruded above water afforded one explanation of new, terrestrial geological formations. The

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[221] Herbert, *Red Notebook*, 34.

Orsono eruption was a colorful and dramatic reminder of the old debate over seabed emergence that Darwin jotted down in his notebook.

On 20 February 1835, one month after Darwin's observation of the volcanic eruption, a severe earthquake laid many of the local coastal towns in ruins. This earthquake was worse than any experienced by the living inhabitants of the area he was in, Valdivia. The earth rippled as he stood on the ground, much like waves would undulate beneath the *Beagle*. Darwin noted the earthquake's intellectual effect as well, “A bad earthquake at once destroys our oldest associations: the earth, the very emblem of solidity, has moved beneath our feet like a thin crust over a fluid;- one second of time has created in the mind a strange idea of insecurity, which hours of reflection would not have produced.”[222] Cities came tumbling down. The tides surged. Darwin marveled at the wanton destruction that such a movement would bring to Cambridge or London. The entire English nation – people, buildings, and economy – would collapse in a single instant. The geology of England could not reflect the true nature of the violent, wild world outside of the park-like British Isles. Thus moved, Darwin was ready to experience what the geological earth could teach him about its own origin.

The ship landed at Concepcion, Chile, twelve days after the great earthquake. The mayor relayed the news, “That not a house in Concepcion or Talcahuano (the port) was standing; that seventy villages were destroyed; and that a great wave had almost washed away the ruins of [the port city].” Darwin looked for proof of this tragedy along the shore, “The storehouses at Talcahuano had been burst open, and great bags... [of] valuable merchandise were scattered on the shore. During my walk round the island, I

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[222] Charles Darwin, *The Voyage of the Beagle: Journal of Researches into the Natural History and Geology of the Countries Visited During the Voyage of H.M.S.* Beagle *Round the World* (New York: The Modern Library, 2001), reprint of 1909 edition, original publication 1839, 270.

observed that numerous fragments of rock, which, from the marine productions adhering to them, must recently have been lying in deep water, had been cast up high on the beach; one of these was six feet long, three broad, and two thick."[223] The measurements in Darwin's description served more than a flair for the dramatic; they also betrayed his desire to describe his observations quantitatively, just as Herschel had instructed. His minute observations of the marine invertebrates on the rocks betrayed the ocean depth from which the rocks had originated.

However, the destruction of property and physical violence were not the most remarkable aspects of the earthquake. Instead, as Darwin reported in his travel narrative,

> The most remarkable effect of this earthquake was the permanent elevation of the land; it would probably be far more correct to speak of it as the cause. There can be no doubt that the land round the Bay of Concepcion was upraised two or three feet; but it deserves notice, that owing to the wave having obliterated the old lines of tidal action on the sloping sandy shores, I could discover no evidence of this fact, except in the united testimony of the inhabitants, that one little rocky shoal, now exposed, was formerly covered with water. At the island of S. Maria (about thirty miles distant) the elevation was greater; on one part, Captain Fitz Roy founds [sic] beds of putrid mussel-shells *still adhering to the rocks*, ten feet above high-water mark: the inhabitants had formerly dived at lower-water spring-tides for these shells. The elevation of this province is particularly interesting, from having been the theatre of several other violent earthquakes, and from the vast numbers of sea-shells scattered over the land, up to a height of certainly 600, and I believe, of 1000 feet.[224]

In other words, the most remarkable part of the entire earth-shaking experience was that he encountered an event in which the sea floor, marine invertebrates still gasping and clinging to it, rose ten feet out of the water and stayed there. Darwin collected marine

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[223] Darwin, *Voyage of the Beagle,* 271.

[224] Darwin, *Voyage of the Beagle*, 277-278.

invertebrate specimens from the curious South American coastal rocks. Specifically, one jar filled with an unidentified barnacle made it into his stores. Little did Darwin know that this barnacle was a rare type and that classifying it would occupy eight years of his life, the decade he mulled over his origin of species problem.

When Darwin returned, he made a name for himself as a geologist. His natural history collections were classified, sorted, and displayed by his now-growing network of British naturalists. Almost all of these specimens were processed by other scholars. Curiously, Darwin kept the barnacles to classify himself.[225] Now back in England, he wrote up his adventures as a naturalist in South America into a popular narrative, got married, settled down, and began to publish on a variety of geological topics. Darwin's time aboard the *Beagle* was at an end. He dedicated himself to drawing conclusions from his scientific observations.

During the years between the *Beagle*'s return and his publication of *On the Origin of Species,* just over twenty years, Darwin immersed himself in the study of two marine invertebrates, corals and barnacles. The coral problem related directly to his geological interests. He hypothesized that reef islands, those with lagoons, were formed by the emergence of an oceanic volcano that slowly receded back into the water. Over time, such volcanoes would become ringed by networks of corals, even when the volcano disappeared beneath the waves, the corals would still be capable of growing near the surface of the water upon the older corals beneath them. Darwin's publication on the subject was well received, though not without criticism from James Dwight Dana, an

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[225] Rebecca Stott, *Darwin and the Barnacle: The Story of One Tiny Creature and History's Most Spectacular Scientific Breakthrough* (New York and London: WW Norton & Co., 2003), 68-91.

American coral expert and fellow naval expedition naturalist.[226] The coral studies also combined Darwin's volcanic observations with his intimate knowledge of sea floor marine invertebrates.

The second marine invertebrate to capture Darwin's attention was the barnacle found along the Chilean coast, but this interest did not peak until he was speculating on the origin of species. During this time, Darwin had read the moral and economic philosophy of Thomas Malthus, cleric and author of *An Essay on the Principle of Population*. In 1798, Malthus had posed a mathematical relationship between food production and population growth. He argued that populations grow exponentially while the food supply grew at a slower, steady rate. At some point, Malthus posited, a proportion of the population would starve.[227] Malthus' view of a harsh, unforgiving world resonated with Darwin and his experiences abroad. Darwin began to apply the Malthusian population problem to the biological world: if populations were left with little food, then those varieties best-suited to gathering resources would be the only ones to survive. These initial thoughts would lead Darwin to a new concept of the origin of species.

Evolution Steeped in Seabed Science: Charles Lyell and Edward Forbes

Many factors led to Darwin's theory of evolution. Recent scholarship has shied away from the account that Darwin conceived of natural selection in a flash of

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[226] See David 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 MacLeod and Philip Rehbock (Honolulu: University of Hawai'i Press, 1994), for more on this debate. Also, see Sponsel (2009).

[227] Thomas Malthus, *An Essay On The Principle Of Population, as it Affects the Future Improvement of Society*. (London: Johnson,1798).

inspiration, and most scholars agree that Darwin's thoughts on evolution began to take shape – step by step – between the *Beagle* voyage and the mid 1840s. Darwin sketched out his species idea in two subsequent drafts; the second draft was completed in 1844 and stored in his foyer closet. He left explicit orders regarding who should inherit the draft in the event of his premature death. The first person on the list was Charles Lyell, the uniformitarian geologist who had become his friend and associate. The second was Edward Forbes, the rising zoogeologist, dredger and soon-to-be Chair of Natural History at Edinburgh. Lyell and Forbes shared a few things in common, including friendly relationships with Darwin and reputations as prominent scholars. Both of these naturalists also engaged in an extensive intellectual exchange with Darwin regarding seabed dynamics as he developed his theory of evolution. The centrality of the sea floor in Darwin's theorizing was strong enough that he chose these two individuals, as opposed to all other naturalists with whom he had become familiar, to interpret and continue his work should he die.

Scholars have developed an extensive literature to recount the development of Darwin's theories.[228] This section contributes an examination of seabed science as a pervasive context for Darwin's theorizing, as demonstrated by his correspondence with Lyell and Forbes. Darwin conducted correspondence with both individuals. He also followed their research closely, citing it often in his later work. Both individuals were well-respected members of the British naturalist community. Both Lyell and Forbes shared a deep intellectual connection to Darwin's work, although Lyell's relationship with Darwin

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[228] Dov Ospovat's *The Development of Darwin's Theory* remains a canonical account of how Darwin came to his theory of evolution. See Sulloway's “Darwin's Early Intellectual Development” and Hodge and Kohn's “The Immediate Origins of Natural Selection” in *The Darwinian Heritage* and also Ruse's “Origin of the Origin,” in *The Cambridge Companion to the “Origin of Species”* for other narratives. I intend to neither challenge, subvert, nor reformulate any of these accounts. Rather, I only hope to provide one more context – that of seabed science – to the many contexts that affected Darwin's thinking at the time.

began earlier and was unquestionably more influential on Darwin's scientific thought. Other historians have produced excellent scholarship regarding Lyell's relationship to Darwin, so this section does not go into great detail on that subject other than to establish their shared interest in the sea floor.[229] His lesser-known relationship with Edward Forbes receives somewhat more detailed treatment.

As the historian Sandra Herbert has noted, Lyell's *Principles of Geology* had been an influential part of Darwin's training since the *Beagle* voyage. Darwin's primary introduction to the geologist was through his publications; he had received a first volume of the book from FitzRoy, the *Beagle*'s captain.[230] While the treatise summarized much contemporary knowledge regarding geology, Lyell also constructed an argument regarding methodology. *Principles of Geology* explained the history of geological formations by using only processes that could be observed in present times. This was not a new method. However, Lyell differed from his contemporaries in that he believed that any explanation of geological phenomena that did not appeal to strictly observable mechanisms did not yield valid scientific evidence.[231] He also challenged Wernerian Neptunism and its use of speculative geology. He lamented that such a brilliant geologist as Werner "had never travelled to distant countries. He had merely explored a small portion of Germany, and conceived, and persuaded others to believe, that the whole surface of our planet... [was] made after the model of his own province."[232] Ultimately,

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[229] Sandra Herbert, *Charles Darwin, Geologist* (Ithaca and London: Cornell University Press, 2005), 63. Darwin continually acknowledges Lyell's influence on his thought, both in *On the Origin of Species* as well as his autobiography. See Martin Rudwick's "Introduction," in *Principles of Geology*, first edition, Chicago and London: University of Chicago Press, 1990. This Introduction is an updated version of "The Strategy of Lyell's *Principles of Geology*," in *Isis* 61, 1970, 5-33. See also Leonard Wilson, *Charles Lyell; The years to 1841: The revolution in geology* (New Haven and London: Yale University Press, 1972) for a biographical account of Lyell.

[230] Herbert, *Charles Darwin, Geologist*, 63.

[231] Rudwick, "Introduction," ix.

[232] Lyell, *Principles*, 57.

Lyell argued that no universal ocean or unseen catastrophes were needed to explain the creation of geological formations; the geologist needed only search for the gradual changes to the landscape seen year-to-year. Lyell's rigor in this regard was the most celebrated example of the prevailing scientific concept of *vera causa*.[233] Darwin would later use a similar reasoning to explain the appearance of new species in the geological record.

The result of Lyell's reasoning led him to believe that the sea floor rose and fell gradually over time. The Earth's uplift and subsidence was ultimately the product of natural, constant processes acting with the same intensity seen in the modern era. That vision for geological formation required long periods of time if it was to be feasible. Lyell elaborated on the imponderable length of geological eras through a description of aqueous processes, including the effects of tidal action upon rocks and the slow process of marine sedimentation. Nearly one-third of his first volume – chapters ten through eighteen in a twenty-five chapter book – were dedicated to aquatic geological processes or those associated with oceanic activity, such as volcanoes. Almost all of these processes involved the sea floor or marine areas.[234]

Lyell also shared his ideas by letter once Darwin and he were better acquainted. Darwin's correspondence with Lyell was quite voluminous and it addressed a variety of topics. Nonetheless, the historical movement of the sea floor remained a frequent subject of communication between the two naturalists. During the 1840s, Darwin entered into a lengthy correspondence with Lyell on the topic of seabed subsidence, especially in relation to the formation of coral atolls. Darwin's explanation of thick coral reef formation required very slow seabed subsidence to make any sense. One letter captures this

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[233] Rudwick, “Introduction,” ix.

[234] Recall Darwin’s association of volcanoes with seabed activity and uplift.

exchange and the role that calculation and speculation of sea-floor dynamics played in Darwin's use of evidence:

> Considering the probability of subsidence in the middle of the great oceans being very slow... considering that reefs not very rarely perish (as I cannot doubt) on part or round the whole of some encircled islands & atolls... I admit as very improbable that the polypifers should continue living on & above the same reef, during a subsidence of very many thousand feet; & therefore that they should form masses of enormous thickness, say at most above 5000 feet...
>
> There are... many considerable islands & groups of islands... all of which a subsidence between 4000 & 5000 ft would entirely submerge or wd leave only one or two summits above water; & hence that they would produce either groups of nothing but atolls... I am far from wishing to say that the islands of the great Oceans have not subsided, or may not continue to subside any number of feet, but... the reefs wd perish [over time] & if the subsidence continued they wd be carried down; & if the group consisted only of atolls only open ocean wd be left; if it consisted partly or wholly of encircled islands, these would be left naked & reefless; but should the area again become favourable for growth of reefs, new barrier-reefs might be formed round them. As an illustration, of this notion of a certain average duration of reefs on the same spot compared with the average rate of subsidence... [such as Tahiti,] an island of 7000 ft high; now here the present barrier-reefs would never be continued upwards into an atoll...
>
> Who will say what this rate & what this duration is; but till both are known, we cannot... tell whether we ought to look out for upraised coral-formations... above the unknown limit, say between 3000 & 5000 feet, necessary to submerge groups of common islands. How wretchedly involved do these speculations become![235]

This letter demonstrates a reliance upon seabed speculation and calculation. Darwin hypothesized that low islands could easily be submerged underwater and become the sea floor. That new seabed could then support thick coral structures. However, Darwin would need to compare the rate of coral growth to the rate of seabed subsidence to investigate island geology for this phenomenon. He frequently contemplated seabed

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[235] DARC v2: 329-330, Charles Darwin to Charles Lyell, undated, c. Sept–Dec 1842.

science on similar matters with Lyell at the same time that he formulated his sketch of natural selection.

Edward Forbes, the man second in line to inherit his species sketch, had also contributed to Darwin's geological studies. Darwin consulted Forbes on the distribution of marine shells for his book *Geological Observations of South America*. The research for this book took four years, beginning in 1842 and ending with its publication in 1846. These years were the crucial period during which Darwin worked through his evolutionary abstracts. Forbes had presented a “Report on the Mollusca and Radiata of the Aegean Sea” before the British Association for the Advancement of Science in 1843. The report laid out the general distribution of marine creatures on the sea floor; he found that the ocean contained a number of zoological zones that corresponded to the depth at which the fauna was found. As depth increased, the number of creatures living upon the sea floor also seemed to decrease. Forbes speculated that no fauna would exist below 300 fathoms because of this correlation between depth and the diminution of marine life.[236] Forbes' report was well received and Darwin sought his expertise regarding the South American shells he used in his geological work.

Darwin wrote to Forbes in 1845 asking for help. Forbes was unable to answer right away, but managed to send a letter back on 9 May 1845. His letter contained a list of probable depths from which his fossil shell specimens would have originated while living on the sea floor. He requested that Darwin send two more pieces of information that would help narrow the specimens' potential depths, “the average size of the specimens... found in each locality... [and] The comparative abundance of specimens of

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[236] See chapter one of this dissertation for a deeper description of Forbes' marine research.

each species..."[237] Forbes believed that he could infer the depth of a particular specimen by its size in relation to other members of its species since organisms seemed to become smaller as depth increased. If compared to the general range of the species found in the neighboring seas, he could determine the depth with greater precision by using the species' pattern of morphological distribution.

Darwin replied that he would be unable to supply this added information, though he would try once time permitted.[238] The estimates remained the same, so it can be presumed that Darwin was unable to furnish this information to Forbes. Nonetheless, Darwin used the estimates to determine the rate of seabed subsidence along the historical South American coast:

> It is well worthy of remark that these shells... must have been covered up, on the *least* computation, by 4,000 feet of strata: now we know from Professor Forbes's researches, that the sea at greater depths than 600 feet becomes exceedingly barren of organic beings, – a result quite in accordance with what little I have seen of deep-sea soundings. Hence, after this limestone with its shells was deposited, the bottom of the sea where the main line of the Cordillera now stands, must have subsided some thousand feet to allow of the deposition of the superincumbent submarine strata... I may add that in Professor Forbes's opinion, the above enumerated species of mollusca probably did not live at a much greater depth than twenty fathoms, that is only 120 feet.[239]

Darwin's reasoning relied upon a number of Forbes' evidentiary practices in this passage. He chose fossil marine invertebrate specimens because of their ability to describe the historical depth of the sea floor. Darwin was also willing to accept Forbes' biogeographical reasoning regarding the depth at which the specimens had lived. No direct observation of the neighboring sea life was necessary for Darwin or Forbes to

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[237] DARC v.3: 189-190, Edward Forbes to Charles Darwin, 9 May 1845.

[238] DARC v.3: 190-191, Charles Darwin to Edward Forbes, 13 May 1845.

[239] Charles Darwin, *Geological Observations on South America*. London: Smith, Elder and Co., 1846, p. 193. I offer thanks to the Darwin Correspondence Project staff for noting Darwin's use of Forbes' shell identification.

establish the species' range; previous observations could be extrapolated upon in the same manner that Forbes had used to establish the lower limit of marine life.

Yet, Darwin employed the marine invertebrates in a novel manner; he used them to tabulate and calculate data on marine sediment deposition. That calculation yielded an implicit prediction, a rate of marine subsidence and an accompanying rate of sedimentary accumulation. He was also careful to verify his practice with the use of a sounding lead, an instrument of measurement. Darwin's geological research continued to blend the evidentiary practices he had encountered at his earlier educational institutions.

Darwin would have encountered two more of Forbes' seabed theories through his publications. Like Lyell, Forbes advocated that the origin of a species could be traced to its appearance in one geographical location. Naturalists could not explain the appearance of new species and varieties using only principles of ancestry or environment; new morphological forms seemed to appear in only one geographical place and disperse from there. Forbes called the appearance of new species in one geological space and time the "theory of specific centers." He elaborated this view in *The Natural History of the European Seas*, which was published in 1859, but had been an object of research for many years.[240] The general idea, however, had been growing in popularity since Lyell noted it in *Principles of Geology*.[241] While Darwin cited Lyell's use

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[240] See chapter one for more of Edward Forbes and *The Natural History of the European Seas*. There is some evidence that Forbes came to his conclusion regarding specific centers in 1832, when he was only 17 years old. George Wilson, the author of Forbes' memoir, notes that his 1832 Notes on the Geology of the [Manx] Island demonstrate – and are organized by – this theory of specific centers.

[241] See Janet Browne's *The Secular Ark: Studies in the Histories of Biogeography* (New Haven and London: Yale University Press, 1983), for the growing concepts of biogeography in the late-eighteenth and early-nineteenth centuries. She also covers more of Forbes' conception of biogeographical distribution.

later in *On the Origin of Species*, he also acknowledged Forbes' unpublished work. After 1859, most naturalists cited or attributed Forbes' work on specific centers rather than Lyell's; the theory was widely accepted by that point and Forbes was recognized as its major advocate.

Darwin would have also been exposed to Forbes' study of crinoids while formulating his theory of evolution. In 1841, Forbes published a *History of British Starfishes*, which gave a full account of the crinoids and other starfish echinoderms surrounding the British Isles. Darwin was steadily inundated with the use of sea floor methods and specimens between the voyage of the *Beagle* and the mid 1840s and – while under that mass of literature – his theory of natural selection began to solidify.

After publishing *Geological Observations of South America,* five years after Forbes' crinoid studies and one year after their correspondence*,* Darwin returned to study his own sessile marine invertebrates; he exhumed the *Beagle* barnacle specimens he had stored away for classification. However, one specimen was more difficult to place than expected. At the time, barnacles were classified by their hard shells, and his variety had none. Instead, it burrowed within the shells of other creatures. He gathered other samples of barnacles from fellow colleagues, but nothing seemed to help him set the organism, now named “Mr. Anthrobalanus,” in its proper place. After much study, he rewrote the classification of the barnacles, or *Cirripedia*. The barnacle research consumed eight years of his life, from 1846 to 1854. Darwin used this small marine invertebrate to investigate the details of his species idea, eventually picking up his earlier drafts with a greater interest in the relationship between species.[242]

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[242] See Marsha Richmond’s “Darwin and the Barnacle” in *The Cambridge Encyclopedia of Darwin and Evolutionary Thought*, ed. Michael Ruse (Cambridge: Cambridge University Press,

As other members of the Edinburgh network had done, Darwin speculated on the origin of new species before 1846, but this time he spent years conducting minute microscopical observations and measurements in his search for evidence. At long last, armed with a novel scientific method and steeped in seabed science, he was ready to finish his work on the great species question.

Theorizing from Blended Practice: Darwin and the Origin of New Species

During June of 1858, many years into his species research, Darwin famously picked up a package sent to him from one Alfred Russel Wallace, a naturalist and collector then working in the Malay Archipelago. He was utterly shocked when he opened the parcel. Wallace had sent an essay on a theory of new species.[243] That theory was almost identical to Darwin's idea of competition, extinction, and divergence from previous forms, “I never saw a more striking coincidence. If Wallace had my MS sketch written out in 1842 he could not have made a better short abstract!”[244]

Upon the advice of his closest scientific confidants, Darwin announced his species idea jointly with Wallace's, even though Wallace had not been present nor had he consented to having his idea announced. There was little stir from the audience, but this maneuver bought Darwin some time to finish his species book. A little over a year

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2013), for a review of the literature surrounding Darwin’s barnacle studies. Also see Stott, *Barnacle,* for a narrative of Darwin's barnacle studies.

[243] The codiscovery of natural selection by Wallace makes for a potentially-informative parallel case study for a naturalist's movement through different networks. Here we have two naturalists that came to the same scientific conclusion, but through exposure to very different networks. While this comparison is beyond the scope of this chapter, it is interesting to note that both Darwin and Wallace disagreed on the use of evidence for natural selection throughout their careers. I would also venture to claim that had Wallace published on natural selection first, it would not have received the methodological attention that Darwin's work attracted.

[244] DARC v7: 107, Charles Darwin to Charles Lyell, 18 June 1858.

later, in November of 1859, Darwin published *On the Origin of Species*. Within its pages was the culmination of his Edinburgh, Cambridge, and *Beagle* experiences expressed in one grand argument about where and how new species appeared. For the purposes of this chapter, his use of evidence to prove natural selection is most illustrative of Darwin's blending of Cambridge and Edinburgh evidentiary practices.

Starting from the first edition of *On the Origin of Species,* Darwin built his descent through modification argument by speculating on the existence of “intermediate forms.” These intermediate forms are the ancestral “missing links” so familiar to modern readers of Darwin's work. According to Darwin, every species interacts with both its environment and other organisms around it, or – in his words – the species' “conditions of life.” As some varieties of the species become more successful, nature selects those extreme characteristics for increased reproduction. Over time, that species might diverge into two different species by a succession of minute variations over a vast span of time, leaving creatures with intermediate morphologies behind. Darwin needed his speculations on the existence of intermediate forms to be borne out by evidence, in order to prove his assertion on natural selection. The problem was that he could not actually provide that evidence directly.

Darwin laid out his argument, writing, “...an interminable number of intermediate forms must have existed, linking together all the species in each group by gradations... it must be asked, why do we not see these linking forms all around us?”[245] While the problem he posed is covered in many chapters of his book, he dedicated almost the entirety of “Difficulties on Theory” and some of “On the Imperfection of the Geological Record” to the lack of direct evidence for intermediate forms. According to Darwin, the

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[245] Charles Darwin, *On the Origin of Species* (London: John Murray, 1859), 462.

lack of immediate and substantial fossils was related to the nature of fossilization and sea floor dynamics. Fossils were mostly formed when the sea floor sank at the same rate that marine sediment piled up. If sediment piled too quickly, then the seabed would rise above sea level and expose the fossils to destruction by the tides. If the sea floor sank faster than the sediment accumulated, then the organic remains would be subjected to the harsh pressure and conditions of the abyss. Darwin concluded that fossils formed during slow marine floor subsidence. However, new species were most often formed during periods of upheaval, when species had a new environment in which to expand and proliferate. This unfortunate aspect of fossilization meant that the fossil record would be necessarily incomplete and rarely preserve intermediate forms.

Sea floor mechanics were responsible for the elusiveness of the evidence that Darwin needed to prove natural selection. And while animal and plant breeding provided an analogy to natural selection and proof of the rules of variation, intermediate forms – Darwin's crucial missing evidence for selection – were the subject of littoral and seabed subsidence.[246] The deep sea floor also provided a sticky point for Darwin's reasoning. He admitted that neither sediment nor animals could accumulate in the deep ocean without complicating his logic. The existence of either phenomenon would continually preserve intermediate forms in the greater depths and Darwin would be forced to show his proof of their existence. These fossil forms would persist especially because the sea floor remained unchanged and eternal according to contemporary beliefs.[247] However, Darwin explained how preservation of deep-sea creatures was not possible for two

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[246] Peter Gildenhuys, “Darwin, Herschel, and the role of analogyin Darwin’s origin” *Stud. Hist. Phil. Biol. & Biomed. Sci.* 35 (2004): 593–611.

[247] Darwin specifically says regarding the timelessness of the deep sea floor, “The many cases on record of a formation conformably covered, after an enormous interval of time, by another and later formation, without the underlying bed having suffered in the interval any wear and tear, seem explicable only on the view of the bottom of the sea not rarely lying for ages in an unaltered condition,” Darwin, *On the Origin of Species*, 288.

reasons: first, sediment did not fall upon the deep ocean floor and, second, animal life was incapable of existing below the lower limit of marine life.

This assertion does *not* imply that Darwin lacked fossil evidence of intermediate forms. On the contrary, the most substantial proof he offered was not in the form of the stone fossils that many of us think of when we hear the word, but rather in what he called “living fossils.” Darwin offered two specific examples of the living intermediate forms, the platypus (Ornithorhynchus) and the lungfish (Lepidosiren), “which, like fossils, connect... orders now widely separated in the natural scale. These anomalous forms... have endured to the present day, from having inhabited a confined area, and from having thus been exposed to less severe competition.”[248] Darwin used the platypus as his primary example. Australia's smaller land area had caused there to be less competition than there would be on a larger land mass. The protected geographical space allowed the platypus to endure as an intermediate form between the egg-laying birds and the furred mammals.

Darwin uses the platypus and lungfish as definitive examples of intermediate species throughout *On the Origin of Species*. He mentions them again when describing his metaphor of species descendants as a tree with many branches. Many of these branches have gone died, representing “whole orders, families, and genera” which have gone extinct and have no living specimens to observe. These extinct species are now only known through the stone fossils they left behind. However, the platypus and lungfish remain living examples of this branching between two different types of organisms: “…so we occasionally see an animal like the Ornithorhynchus or Lepidosiren, which in some small degree connects by its affinities two large branches of

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[248] Darwin, *Origin of Species*, 107.

life…"[249] The platypus appears again as a way for Darwin to demonstrate the affinities between various organic beings. In this case, he uses the platypus' morphological characteristics to elaborate his way of describing how a philosophical naturalist would associate descent through divergence: "If the Ornithorhynchus had been covered with feathers instead of hair, this external and trifling character would… have been considered by naturalists as important an aid in determining the degree of affinity of this strange creature to birds and reptiles, as an approach in structure in any one internal and important organ."[250] Finally, Darwin mentions both the platypus and lungfish later as an example of aberrant forms of fossil specimens, which are actually intermediate forms.[251] In each case, the platypus and lungfish were deployed as evidence for morphological intermediacy by modification.

It is important to note that the existence of an unaltered form – by itself – is not what Darwin meant by a "living fossil." Darwin noted other instances of organisms unchanged by the ravages of time and competition, such as the Silurian *Lingula* which showed little to no modification over time. Since living fossils were, by definition, intermediate forms, *unchanged* species that did not demonstrate intermediacy did not qualify as proof of natural selection.[252] The platypus, though unchanged, demonstrated a link between birds and mammals because it had morphological characteristics of both, while the *Lingula* had simply not changed over time and, therefore, might even offer proof against the mutability of species.

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[249] Darwin, *Origin of Species*, 130.

[250] Darwin, *Origin of Species*, 416-417.

[251] Darwin, *Origin of Species*, 429.

[252] The platypus and lungfish occur as Darwin's staple examples of intermediate forms. These – in Darwin's words – living fossils show traits common to two branching types of organic forms. Darwin excludes the *Lingula* from these examples, deploying it, instead, as a creature showing no change over time. Darwin also represents these creatures differently in his graph, though less explicitly, depicting the *Lingula* as "species F," a straight line, instead of a branching one, in *On the Origin of Species*.

Nonetheless, Darwin's first evidence for descent through extreme variation incorporated living fossils. The fossil record, whether living or preserved in stone, must show intermediate forms in order for natural selection to be a valid theory. Darwin's assertion rested upon the rare, but provable, preservation of living intermediate forms. Darwin argued for natural selection through other ways, such as his analogy with artificial selection. However, much of his argument related to the fossil record. Natural selection needed preserved intermediate forms as evidence, but also required a seabed geology that often destroyed the fossils or prevented their creation altogether.

In terms of the fossil evidence in *On the Origin of Species*, the argument regarding the seabed movements can be broken into two sections: the first consists of measurements, tabulation, and quantitative predictions of tidal effects on coastal geology, while the second is descriptive and reasons through the distribution of organisms over geographical space and time. His ninth chapter, “On the Imperfection of the Geological Record” begins by calculating the thickness of geological formations and the rates at which tidal action would wear upon the rocky shores of England. He continues later in that chapter and into the next to outline the distribution of organic beings and their remains. This chapter provides a clear blending of both Edinburgh evidentiary practices and those of Cambridge, now melded into one, novel theory. The longer and more public argument that Darwin provided in *On the Origin of Species* allowed naturalists to engage with Darwin's species theory. And that publication couched the formation of new species, and the evidence needed to prove natural selection as a natural law, in the language of sea floor science. As explored in later chapters of this dissertation, many naturalists believed that Darwin needed only to wait until the gaps in the fossil record were filled by intermediate forms, and then he would have the evidence he needed to prove natural selection once and for all.

The evidentiary practices Darwin encountered in the move from the University of Edinburgh to Cambridge University helped to produce the novel seabed science that he used in *On the Origin of Species*. According to modern standards, Darwin's journey between Edinburgh and Cambridge would not be geographically far. However, this shift in practice occurred in the context of greater, faster circulation of naturalists' ideas. These institutions would have seemed vastly more distant before the widespread construction of railroads that accelerated during the middle nineteenth century. This geographical isolation would have allowed for the circulation of papers and letter, but not the regular, casual face-to-face meetings that could occur later because of the growing British railroad system. Darwin's financial and social resources also allowed him to conduct biological research without being tied to one institution and its practices. Such gentlemen naturalists were not uncommon at the time. Such unassociated, financially secure individuals like Darwin may have been more able to blend elements from differing scientific networks because they were unmoored to institutional politics.

Darwin as a case study in the convergence of evidentiary practices shows the power that these geographical locations could have upon the practice of science. Darwin's trajectory as a naturalist was far from smooth. His scientific career led him to two of the major centers of seabed science, Edinburgh and Cambridge. There he learned from two vastly different networks. At Edinburgh, Darwin learned to dredge the sea floor for marine invertebrates from Robert Grant and, by extension, Grant's mentor Robert Jameson. Grant walked Darwin along the Leith seaside collecting specimens and taking them back for analysis. The microscope provided a means for the minute observations that would yield the organisms' environmental context, such as how deep the organism lived beneath the waves, and the organizational relationships between plants, animals, and simple marine invertebrates. Yet, speculation and deduction led the way for Edinburgh naturalists, and morphological observations served to refine larger theories of the living world.

The network of Cambridge was very different from that of Edinburgh, as Darwin himself recalled. The Cambridge dons with whom Darwin associated taught him to measure and quantify his observations before applying them to larger theories. Their self-identified inductive method valued the accumulation of vast charts and tables that could be used to distill out predictions and theories, much as the discovery of Neptune had been calculated by minute orbital differences. Yet, both institutions, each with their own practices and tales of scientific triumph, studied the sea floor.

When Darwin was offered the opportunity to accompany the HMS *Beagle,* he needed to act swiftly to gather instruments and advice before the vessel set sail. Under the pressure of time, he sought out individuals from both Edinburgh and Cambridge networks, bringing both dredge and clinometer with him. And as the Beagle sounded the seabed for its rocky shoals and clear passages, Darwin saw the sea floor itself move onto dry land during the earthquake of Concepcion. That event impressed upon him the importance of the sea floor for understanding the history of species. Darwin brought both sets of evidentiary practices into his explanation of the gradual change of zoology and geology over time. In essence, Darwin based his new theory's evidence on ocean floor dynamics exactly because of the prominence that seabed science played across his two different evidentiary communities.

While not all of Darwin’s evidence was based in seabed science, a large portion of his argument for natural selection relied on sea floor geology for its evidence. As shown in later chapters of this dissertation, this seabed evidence became the subject of great debate for elite naturalists in subsequent decades. Naturalists would later respond to Darwin's theory by questioning his methodological blending of Cambridge prediction with Edinburgh zoogeographical evidence from marine fossils. For the first time, these disparate scientific networks encountered the others' evidentiary practices in a way that was legible to them and important to their own seabed science. Later, that new, developing scientific method would turn its gaze back to the deep sea floor in order to find evidence for Darwin's intermediate forms. It would also pose one of the greatest challenges that Darwinian natural selection faced in the nineteenth century. The challenge posed to natural selection would begin with the introduction of another scientific network's evidentiary practice, that of the United States Coast Survey. The deep-sea sounding techniques that the US Coast Survey employed allowed dredging to be done deeper than ever attempted before. That opening of the deep-sea floor as a site for research would later provide a supposedly unchanging environment that would be an ideal geography to search for Darwin's living fossils. That proof would later determine whether naturalists validated or rejected his theory.

CHAPTER THREE: Civil War and the Discovery of the Deep-Sea Fauna

The last chapter explored how Darwin invoked deep-sea organisms as potential proof for his theory of evolution. It also explained the how the vicissitudes of seabed geology might destroy the evidence: the movement of the sea floor determined when fossils would be created or destroyed. New variations would proliferate when the sea floor rose and expanded the geographical area available for competition. However, that same rising of the sea floor would also destroy the stone fossils by subjecting them to tidal action. Seabed dynamics was both the creator and destroyer of Darwin's evidence for evolution by natural selection. The deep sea floor was technologically inaccessible to British naturalists until the middle 1850s, when their American cousins developed the technology to gain samples from the Atlantic abyss. When those American practices confronted British marine science in the midst of the natural selection debates, naturalists from both sides of the Atlantic would finally turn their attention to the deep sea to find proof for Darwinian evolution.

Already in the 1850s, as Darwin was working out the details of his theory, the United States Coast Survey, the nation's premier scientific institution, verged on a major international discovery regarding the sea floor fauna. Unfortunately, war intervened. On 4 March 1861 several American states seceded from the United States and formed their own government, the Confederate States of America. A bloody, four-year war ensued that split the young nation and pitted countryman against countryman. The Civil War also split the scientific allegiances and institutions of the US. Networks of naturalists halted many of their research projects and adapted to the national war. Scientific resources, such as survey ships, were also redeployed to the naval fronts, making it difficult to conduct routine scientific investigation of the sea floor. The very alignment to federal statecraft that characterized American seabed science during the nineteenth century changed the institutional production of knowledge as the country mobilized for conflict. And while the war delayed the Coast Survey's continuing deep-sea fauna research for a number of years, the discovery of life in the deep ocean eventually opened a unique geography that naturalists would investigate to test Darwin's theory of natural selection. This chapter recounts the emergence of deep-sea biology, the development of American deep-sea sounding practices, and their circulation of those practices around the Atlantic. Specimens also play a role in this narrative: during the early 1850s, only a handful of people were capable of retrieving sediment from the deep-sea floor.

Brooke's Sounding Line and American Deep Ocean Technologies

While no single technological development opened the deep-sea floor for exploration, the most influential instrument in seabed science during the mid-nineteenth century was a new type of sounding line. This simple, but ingenious device solved two major problems for deep ocean studies: naturalists had no way to verify whether their deep ocean soundings actually reach the ocean bottom and they had no account of how accurate those measurements were if the surveying lead successfully reached the sea floor. This inability to verify deep-sea measurements was widespread, even among elite naturalists. For example, one common misconception about the sea was that water density would keep objects from sinking below a certain depth depending on their weight.[253] One famous naturalist acknowledged that he believed the “density myth” even

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[253] This misconception may have been partially due to debates about the changing beliefs about the temperature of deep water. Some believed that the density of water would reach that of iron at some point and leads would travel no deeper than that layer of water. The misconception

until later in life, leading to visions of sunken ships floating deep under the water and gold coins slowly spinning from the wreckage.[254] Many other men of science undoubtedly believed the density myth as well. While the belief would not have interfered with shallow water zoogeology, such as that practiced by Edinburgh naturalists, it prevented an extension of British biological studies into the deep ocean.

Those naturalists who were certain that their leads reached down into the deep ocean floor still faced a number of other problems. The "fathom" as a unit of measurement was originally defined as the armspan of one sailor – about six feet. This was precise enough for distances of about 10 to 20 fathoms, but at great distances of 1,000 to 2,000 fathoms, the slight differences of "the armspan of one sailor" aggregated to a significant variation. Nobody knew if their fathom was exactly the same as others' fathoms, which posed problems for precise surveying practices. When that uncertain length of line finally pulled slightly, shipboard technicians had to judge whether a that sudden tug upon the line was due to deep water currents, extreme drag due to the movement of the ship, or the lead hitting bottom. And once the line operator determined that the line had hit bottom, he still had to account for any angle and drift of the line. If the ship had moved at all, then the angle of the extended line also had to be taken into consideration when determining depth. To further complicate the procedure, the weight of the water would often snap the line when it was pulled back to the surface. Shallow water sounding and surveying techniques did not prepare naturalists for deep ocean studies and precious few people were specifically trained for deep water surveying.

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likely twisted as it spread through different populations and merged with older beliefs about the deep sea.

[254] Wyville Thomson, *The Depths of the Sea: An Account of the General Results of the Dredging Cruises of HMSS 'Porcupine' and 'Lightning' during the Summers of 1868, 1869, and 1870*, second edition (London: Macmillian & Co., 1874), 32.

The new sounding line solved one crucial aspect of naturalists' instrumental uncertainty, the verification of the lead touching the deep-sea floor. The sounding device was invented by Lieutenant John Mercer Brooke of the United States Navy to help with deep water surveying. It was in use by 1853, when the North Pacific Exploring Expedition set sail. The line operated much like other sounding apparatuses. The lead weight would drag the line down into the deep. Once the line hit bottom, the lead weight would push a tube into the sea floor sediment. The upward pressure from the tube would disengage the lead weight, which would remain on the seabed. The disengagement of the lead would prove that the line had hit bottom. Brooke's sounding line would also bring up a sediment sample, thereby verifying the lead had hit the sea floor. Brooke developed these sounding practices to solve the technical problems associated with deep water surveying. He adopted a sequence of smaller leads and replaced hemp line with silk or twine. He also sounded from boats deployed from large naval ships, even though dredges were still deployed from the larger ship's deck. These developments reduced the instrumental uncertainty and made deep-sea sounding programs much more feasible. American deep-sea surveying practices also produced the first deep water sediment samples.

The first specimens from the sea floor were controlled by the individuals with the technological capacity to produce them. In this case, Brooke and his close network of associates at the Naval Observatory had the ability to produce those deep water samples because of the development of Brooke's sounding line. Brooke's connection to the government-supported program of deep water research allowed him to develop and conduct American sounding practices. Getting to important sampling locations was difficult in itself. Areas with a depth over two miles are often located far from shore. For example, the first sediments retrieved came from the so-called ‘Telegraph Plateau,’ far offshore in the Atlantic Ocean.[255] Those deep water geographies were outside the sailing range of even wealthy, independent naturalists. Deep water sounding and dredging also required teams of trained hands, unlike dredging in coastal waters, which could be done by a single naturalist in a rowboat or even untrained oyster fishermen. The combination of technological novelty and access to government scientific sponsorship gave Brooke's deep-sea sediment samples their immense scientific value.

Matthew Fontaine Maury: The First Deep-Sea Sediments as Celestial Observation

The circulation of the first scientific specimens from the deep ocean determined who could participate in the resulting debates that they raised. While Brooke was the inventor and first proficient operator of his sounding line, his immediate superior at the Naval Observatory, Matthew Fountaine Maury, circulated many of the first specimens. For example, in 1853, the first sediment samples were carefully preserved by the crew of the USS *Dolphin* and sent back to Maury, who split them and forwarded them to two well-known microscopists. The first sample portion was sent to a fellow American at West Point, Professor Jacob Whitman Bailey. The second half of the sample was sent overseas to Professor Christian Gottfried Ehrenberg of the University of Berlin. Bailey's proximity allowed him to answer in November of 1853, a week after receiving the sample:

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[255] Matthew Fontaine Maury, subject of the next section, named the “Telegraph Plateau” to suggest a use for the underwater geographical area that had been surveyed. From the new soundings, Maury extrapolated that the “Plateau” was most completely flat and deep, but not too deep to keep a telegraph cable from sinking all the way to the sea floor. Besides his sounding samples as evidence, Maury also had financial interests in the laying of the transatlantic telegraph cable and used the name to influence the locations chosen and perceived feasibility of the cable.

> I am greatly obliged to you for the deep soundings you sent me last week, and I have looked at them with great interest. They are exactly what I have wanted to get hold of. The bottom of the ocean at the depth of *more than two miles* I hardly hoped ever to have a chance examining; yet, thanks to Brooke's contrivance, we have it clean and free from grease, so that it can at once be put under the microscope. I was greatly delighted to find that *all* these deep soundings are filled with microscopic shells; not a particle of sand or gravel exists in them. They are chiefly made up of perfect little calcareous shells (Foraminifera), and contain, also, a number of silicious shells (Diatomaceae). It is not probable that these animals lived at the depths where these shells are found, but rather think that they inhabit the waters near the surface; and when they die, their shells settle to the bottom. With reference to this point, I shall be very glad to examine bottles of water from various depths which were brought home by the Dolphin, and any similar materials, either 'bottom,' or water from other localities. I shall study them carefully... The results already obtained are of very great interest, and have many important bearings on geology and zoology...[256]

In 1855, Bailey also examined the samples brought back by Brooke during the North Pacific Exploring Expedition.[257] Bailey was able to compare these two samples and drew some preliminary conclusions.

The delicate shells of the Telegraph Plateau were intact and beautiful to behold while the more recent sample consisted of mixed shell fragments. The North Pacific Exploring Expedition sediments also contained representatives of all organismal groups found in sea floor deposits, such as sponge spicules and diatomaceaous shells. Such an array of groups was a little curious, but it suggested that the deep-sea floor acted as a repository for things carried by the ocean currents from all over the globe. Maury reasoned that the North Atlantic bed of perfect, preserved shells could not be subjected to deep-sea currents or abrasions, making it a perfect place to lay the telegraph cable.

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[256] Matthew Fontaine Maury, *The Physical Geography of the Sea: And Its Meteorology* ed. John Leighly (Cambridge: Harvard University Press, 1963) reprint of the eighth American edition 1861, 291.

[257] See the description of the North Pacific Exploring Expedition later in this chapter.

He also reasoned from Bailey's letters that the deep seabed could not sustain life; the small shells had probably fallen from the surface onto the sea floor. The absence of currents and living organisms from the Telegraph Plateau would remove any known threats to an underwater cable, according to Maury's understanding of the deep ocean.

Maury recorded his correspondence with Bailey in a popular 1855 treatise, *The Physical Geography of the Sea,* which was reviewed with skepticism by elite scientific circles, but heartily embraced by the lay public. The text was not known for being the highest caliber of science and elite naturalists could easily point to incorrect information and unproven speculations within the book.[258] However, Maury's treatise was read widely in America and abroad. The book's wide appeal certainly helped to circulate Maury's ideas about the deep sea.[259] Nonetheless, well-known naturalists frequently commented on *Physical Geography of the Sea*'s scientific inaccuracies. Luminaries such as John Herschel, the British astronomer and tidologist, responded to Maury's conclusions. Maury had advocated what is now known as “thermohaline circulation” to explain the Gulf Stream. Herschel rejected Maury's interpretation in favor of explaining the eastward flow as due to prevalent trade winds across the Atlantic. Despite the frequent criticisms, Maury revised relatively few of his positions through the various editions of *The Physical Geography of the Sea.* However, he had no hesitation defending himself against Herschel regarding the Gulf Stream – something Maury felt

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[258] See John Herschel, “Physical Geography,” in *Encyclopaedia Britannica, 8th ed., vol. 17* (1859): 615, for Herschel’s response. James Espy also responded to Maury’s ideas in “Fourth Meterological Report,” Washington: 34th Congress 65 (1857): 159. One critique that yielded a beneficial counter-theory, William Ferrel’s “An Essay on the Winds and Currents of the Ocean,” *Nashville Journal of Medicine and Surgery* vol 11 (1856). I find the reaction to *The Physical Geography of the Sea* from elite versus popular readerships to be somewhat similar to the anonymous publication of *The Vestiges of the Natural History of Creation* around the same time.

[259] John Leighly, “Introduction,” in *The Physical Geography of the Sea: And Its Meteorology* ed. John Leighly (Cambridge: Harvard University Press, 1963) reprint of the eighth American edition 1861, xvii.

was his area of expertise.[260] Whether for convenience or strategic reasoning, Maury selected what conversations to update in his revisions. As a comparison, thorough and damning critiques lobbed against his theory of atmospheric circulation – which made up a large portion of his text – were almost completely ignored in later editions of the treatise.[261]

While Maury lacked the most updated, elite scientific information in his revisions, his charismatic style appealed to a wide readership. His sweeping and general claims about the workings of atmospheric and oceanic circulation captivated lay readers. One particularly successful strategy Maury employed to lend his arguments gravity was to compare the study of the sea floor to the study of the heavens. Maury posed two comparisons in particular to get his reader to connect the deep sea with the great celestial mysteries. First, he related the action of plumbing the depths of the watery world to astronomical stargazing. For example, he posed the surveying of the deep sea as being scientifically similar to the measuring and weighing of distant planets; if an astronomer could weigh a celestial body, then it should be possible for a naturalist to fathom the ocean. His second comparison linked the study of the sea floor to theology. His most explicit deployment of this theological perspective was his invocation of biblical text, specifically the creation of the heavens, as scientific evidence, which is covered in the next section of this chapter. To Maury, pondering the great mysteries of heaven and

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260 Leighly, “Introduction,” xix.

261 The critique is mentioned in Leighly's “Introduction.” MJ Bourgois gave a complete and thorough refutation of Maury's speculations on atmospheric circulation in Simeon Bourgois, “Réfutation du système des vents de M. Maury” (Paris, 1863); repr. from *Revue Maritime et Coloniale* (Paris: Berger-Levrault, 1883). It is very possible that Maury's involvement in the United States Civil War contributed to his skipping the finer points of elite science during this post 1861 period. However, Maury continued to keep up with certain scientific conversations during this period, such as discoveries in deep sea sediment composition.

the abyss were bound to fill the naturalist with the same sense of religious wonder. That sense of wonderment connected the astronomical and theological comparisons:

> Therefore the contemplative mariner, as in midocean he looked down upon its gentle bosom, continued to experience sentiments akin to those which fill the mind of the devout astronomer when, in the stillness of the night, he looks out upon the stars, and wonders. Nevertheless, the depths of the sea still remained as fathomless and as mysterious as the firmament above. Indeed, telescopes of huge proportions and of vast space-penetrating powers had been erected here and there by the munificence of individuals, and attempts made with them to gauge the heavens and sound out the regions of space.[262]

This passage explicitly references astronomical practices by crafting an analogy between sounding the depths of the sea and surveying the night sky. Instrumentality and measurement became Maury's link between the celestial and the American seabed sciences through his strategy. Maury not only lent his studies grandeur with his comparison to celestial observation, he also made it legible for the renowned Cambridge astronomers with whom he wished to identify.

Maury's direct invocation of biblical authority in *The Physical Geography of the Sea* appealed to a general readership, but had lost popularity with elite naturalists by the mid-nineteenth century. His biblical references were far from subtle. At the end of a discourse on the unchanging nature of the sea floor despite extreme undersea currents and pressures, Maury portrayed the quietude of the ocean depths as a window into the divine will itself:

> *Compass was set upon the face of the deep; because its waters were measured in the hollow of the Almighty hand; because bars and doors were set to stay its proud waves; and because when He gave to the sea His desire that its waters should not pass His command, He laid the*

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[262] Maury, *Physical Geography*, 283.

> *foundations of the world so fast that they should not be removed forever*.[263]

Maury invoked the first act of biblical creation to frame the scientific plumbing of the deep sea. By doing so, he linked the study of the deepest waters of *Genesis* to the study of its highest heavens.[264]

Deriving Knowledge from Sediments: The Biotic Debate

Biblical references not only framed the study of the deep, but also factored into Maury's evidentiary calculus. The *Genesis* creation narrative helped Maury to weigh new scientific specimens and their implications for scientific conclusions. The question of whether the first deep-sea sediment samples indicated life at the bottom of the sea was framed by biblical truth for Maury. The comparison between the study of the heavens and the study of the sea floor went beyond appeals to a sense of wonderment and into scientific reasoning for Maury. His public audience responded to his practices with an enthusiasm that the scientific elite did not immediately share.

Maury had agreed with Bailey's analysis that the minute shells in the sediments samples fell from surface water onto the ocean floor. When combined with the fact that, according to biblical narrative, the sun was created before God commanded life to spring forth in the oceans, Maury concluded that life could not exist in the ocean depths. In Maury's reasoning, *Genesis* implied that the sun was needed to sustain life in the ocean. No life existed in the deep sea because no sunlight reached the great depths, otherwise

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[263] Maury, *Physical Geography*, 295-296. Emphasis in the original.

[264] Genesis 1:7 “And God made the firmament [heavens], and divided the waters which *were* under the firmament from the waters which *were* above the firmament: and it was so.”

God would have created life earlier in the creation narrative.[265] Biblical chronology allowed Maury to dismiss the shells as being proof of life on the deep-sea floor. The shells, instead, acted only as proof of the gentle, lifeless, and unchanging nature of the Telegraph Plateau environment. If the sea floor had been a harsh environment, then the delicate shells would be shattered and destroyed. And if such delicate shells could survive, then so could an underwater cable.

Microscopists divided over whether the sediment samples indicated that the deep-sea floor was as lifeless as Maury asserted. Not everybody agreed with Maury's reasoning about the lifelessness of the deep, including Ehrenberg, the other recipient of Brooke's sample. Both Ehrenberg and Bailey had observed that the sediment sample's delicate, foraminiferous shells contained small amounts of organic pulp inside them. While Bailey saw only preserved remains in his microscope, Ehrenberg came to the opposite conclusion from examining a portion of the same specimen taken from the North Atlantic. Ehrenberg began to explore whether or not other evidence could support life on the deep-sea floor. He found that evidence; the samples sent to him contained new microscopic forms that had not been observed in other geographical regions of the ocean. These forms had a number of morphologies that were not found in the surface or intermediate waters. In addition, the number of previously unseen forms seemed to increase with the depth where the sample had been taken. When combined with the well-preserved pulp in the shells, Ehrenberg amassed enough evidence to convince him of “stationary life at the bottom of the sea.”[266]

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[265] Maury, *Physical Geography*, 300.

[266] Maury, *Physical Geography*, 298. Excerpt from an October 1857 letter from CG Ehrenberg to Maury.

While Maury ignored many scientific critiques in the subsequent editions of *The Physical Geography of the Sea*, as evidenced by his not revising his work after definitive critiques against his theory of atmospheric circulation, he did revise his treatise at great length regarding the deep-sea sediments and their implications on the biotic debate, or the question as to whether life existed at great ocean depths. Maury summarized Ehrenberg's conclusion on the pulpy matter found in the deep-sea formainiferous shells and the reasoning behind the “biotic” position. Maury also outlined Bailey's “anti-biotic” argument which refuted the possibility of life at great depth. Maury first enlisted Edward Forbes' azoic zone, a zoogeological theory which suggested that the number of living creatures decreased with oceanic depth, as support of the anti-biotic position.[267] According to the prevalent interpretation of Forbes' theory, life would be highly improbable at the depths where the sediment samples originated. However, Forbes' theory did not account for the pulpy matter in the shells nor the number of new morphological forms in the sediment samples, as observed by Ehrenberg.

Maury countered the biotic arguments with two forms of reasoning. The first explanation that Maury gave was derived from lived naval practices. The other was derived from a knowledge of the tides, an influential type of seabed science in the nineteenth century. Maury drew an analogy between the flesh found in the foraminiferous shells and preserved meat. “Corned” meat was created aboard ships by subjecting it to the “antiseptic properties of sea water.” Mariners, especially those on transatlantic voyages, would preserve fresh meat by sinking it into deep water. The

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[267] See chapter one of this dissertation for an explanation of Edward Forbes' azoic zone theory. Originally, Forbes, the new Regius Chair of Natural History at Edinburgh, observed that fewer life forms were dredged up in greater depths. By calculating the rate at which life decreased, he speculated that all life would disappear at 300 fathoms under the ocean surface. Forbes' reputation raised the authority of this speculation to a full-fledged, mostly-uncontested theory after his death.

process of preservation was attributed to “the pressure and the affinity which not only forces the water among the fibres of the meat, but which also induces the salt to leave the water and take to the meat...” Maury speculated that the same antiseptic properties of deep ocean water that preserved meat would also preserve the flesh of the Foraminifera, “...by the fact that they [the shells] are brought up in the middle of the ocean, and remained on board the vessel exposed to the air for months before they reached the hands of the microscopist...” The tiny pulp matter in the sediment shells, just like preserved meats, were spared from decay until they reach the microscopic plates of naval officers.[268]

Yet, the antiseptic properties of sea water only accounted for the fleshy pulp found in the deep-sea shells. The anti-biotics still had to explain the numerous, unknown morphological forms found in the sediment sample. The tides, according to Maury's argument, could have swept unknown forms from the shallow waters around the world into the open ocean, where they would fall into the quiet abyss. The deep ocean was portrayed as a still and dark repository for the microscopic organisms, a type of cemetery of gentle repose where the shells would slowly accumulate into deep-sea ooze. And while Maury recognized that the biotic debate was still active, he recounted Erhenberg's finding of morphologies found in Switzerland that had probably floated down and were deposited in the deep Mediterranean sea floor. The combination of tidological thinking and analogy with naval practices caused Maury to support of the anti-biotic conception of the sea floor.

Maury's detailed attention to the deep-sea sediment debates in *The Physical Geography of the Sea* outlines a curious absence: the work of other American deep-sea

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[268] Maury, *Physical Geography*, 299.

experts from the United States Coast Survey network is not mentioned as part of the biotic debate even though they were active and prolific contributors. Two notable deep-sea naturalists, William Stimpson, the dredger who accompanied Brooke on the North Pacific Exploring Expedition, and Louis François de Pourtalès, a member of the United States Coast Survey and pupil of Louis Agassiz, were completely absent from Maury's account of the biotic debate. Both Stimpson and Pourtalès had personally examined freshly acquired sediment firsthand while part of American scientific expeditions, making them the first individuals to see scientific samples from the ocean depths from Brooke's sounding line. These were the first samples that were free from preservatives, such as alcohol, and not spoiled by the grease used on traditional sounding leads.

Stimpson had traveled with Brooke during the 1853 North Pacific Exploring Expedition and, therefore, was one of the first people to examine fresh, unadulterated deep-sea sediment. Pourtalès, who would have had access to his own sediment samples soon thereafter, began publishing his findings while Brooke and Stimpson were at sea.[269] The scientific activities of both Pourtalès and Stimpson provide an essential counterpoint to Maury's famous narrative of American marine science. This science, called “thalassography” by Pourtalès' associates, featured prominent American naturalists who were later erased – whether intentionally or thoughtlessly – by Maury's account of the biotic debate.

Louis François de Pourtalès, the most prolific member of the mid-nineteenth century biotic debate, was born in Neuchâtel on 4 March 1824. Neuchâtel is located in modern Switzerland, but was also under Prussian administration at the time of Pourtalès' birth. The Pourtalès family name undoubtedly opened many doors for the young man;

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[269] Rozwadowski, *Fathoming*, 54.

Pourtalès would not only inherit his father's comital title in the future, but also the family's fortune. Additionally, he was renowned as a brilliant, humble, and extraordinarily likeable individual, something that many remarked upon both while he was alive and after his death.[270] Pourtalès originally trained as an engineer and, therefore, had the perfect education required for triangulations and surveying. Natural history managed to capture his interest while still at the young age of seventeen, when he became the favored student of Louis Agassiz, the Professor of Natural History at the Academy of Neuchâtel.

Pourtalès' apprenticeship to Agassiz illustrates the movement of European naturalists across the Atlantic during the nineteenth century. The lifelong relationship between these two notable members of United States science also showed the prominence of marine zoology practices in American scientific networks. Agassiz had been appointed Professor at Neuchâtel in 1832, following an expedition to Brazil.[271] The expedition's main naturalist died before he could classify the valuable freshwater Amazonian fish and Agassiz was given the opportunity to succeed the project. Upon his return, Agassiz immediately continued his work on marine vertebrates, but this time through a study of the fossil fish. When Pourtalès began his relationship as a lifelong protégé of Agassiz, the elder mentor was at the end of publishing his five volume *Recherches sur les Poissons Fossiles,* the ichthyological work that formed the basis of

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[270] One obituary in the *Popular Science Monthly* (Volume 18, February 1881) recounts, "[LF Pourtales] had not an enemy, and could not have had one; for, although firm and persevering in temper, he possessed the gentleness of a child and a woman's kindness. His modesty amounted almost to a fault; and people wondered why a man who was master of three languages should talk so little. But with intimate friends he would speak freely, and never without giving information and amusement. His range of learning was very wide, and his command of it perfect; nor was it confined to mathematics, physics, and zoölogy. He did not scorn novels and light poetry, and was knowing in family anecdotes and local history. Indeed, it was a saying in the Museum [of Comparative Zoology] that, if Count Pourtales did not know a thing, it was useless to ask any one else."

[271] Edward Lurie, *Louis Agassiz: A Life in Science* (Chicago: University of Chicago Press, 1960), 70.

Agassiz' later fame.[272] Agassiz had also formed strong ties to the British Association for the Advancement of Science and the Royal Society during his professorship at Neuchâtel, gaining an interest in both marine invertebrates and historical glaciation in the years prior to his introduction to his young protégé.[273] Pourtalès accompanied the adventurous Agassiz on a glacial expedition to the Alps in 1840 and remained a friend and student for the rest of his life.[274]

In fall of 1846, Agassiz had gained enough fame to receive an invitation to cross the Atlantic and deliver a series of lectures in Boston. Agassiz' lectures drew attention and enthusiasm to the study of natural history at Harvard. A philanthropist donated money to establish a school of science after listening to Agassiz' lectures. He convinced the great naturalist to settle in the United States as the first director of the newly established Lawrence Scientific School at Harvard University. Pourtalès joined his friend and mentor in the United States in 1847, settling in East Boston and later in the Cambridge area. There, Pourtalès would be introduced to a small community of seabed naturalists, including William Stimpson, another student of Agassiz who started dredging in September of 1849, and Alexander Dallas Bache, the Superintendent of the United States Coast Survey. This Harvard group would form Pourtalès' scientific network, which grew based on his mentor's earlier marine zoology research.

Pourtalès entered into the service of the Coast Survey in 1848. Bache was impressed with Pourtalès' keen mind, industriousness, curiosity towards seabed science,

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[272] Lurie, *Agassiz,* 79.

[273] The British Association and Lord Francis Egerton both assisted Agassiz in the publication of his icthyological research. Egerton presented Agassiz' work to the Geological Society of London, where Agassiz was awarded the Wollaston Medal, the Society's highest honor, in 1836 for his work on fossil ichthyology. By 1838, the Royal Society had elected Agassiz as foreign member. See also Lurie, *Agassiz*, 90.

[274] Alexander Agassiz, *Biographical Memoir of L. F. de Pourtales* (Washington: National Academy of Sciences, 1881).

and his connections to Agassiz. Both Agassiz' and Bache's scientific circles had an interest in seabed science, Agassiz though the marine zoology studies that made him famous and Bache through an interest in deep-sea sediments.[275] Pourtalès was an excellent addition to the Coast Survey and was given two duties, tidal observations and deep-sea sediment research.

Pourtalès' early notebook entries regarding deep-sea surveying start with observations done in 1844, three years before he joined his mentor at Harvard. Most likely, Pourtalès joined a continuing study of sounding and surveying the deep-sea floor. Bache had joined the Coast Survey in 1843 and immediately began an investigation into the constitution of the deep-sea environment. Pourtalès was the perfect person to assist Bache in his study. The charismatic Bache almost certainly recruited Pourtalès with his scientific questions about the deep sea. The Coast Survey's deep-sea sediment studies preceded Pourtalès' interest in them, though Pourtalès was a fundamental actor in the research and shaped its progress from its first years. Pourtalès primarily employed the sounding lead in his sea floor surveying, but also incorporated the results from other dredging naturalists into his conclusions. Later, especially in the Gulf Stream survey, Pourtalès would employ the dredge often. Pourtalès wrote in his notes:

> ...On our coasts, particularly south of New England, little the dredge has been as yet very little used [sic]. The character & constituents of the bottom are however pretty well known, thanks to the care of the late Superintendent of the Coast Survey, Prof. Bache, who since the beginning of his administration of that work, required the hydrographical parties to preserve the specimens brought up by the lead. From eight to nine thousand specimens have there been accumulated at the Coast Survey Office, from a region between the coast & the outer edge of the Gulf Stream & reaching nearly to 1500 fathoms. But of course, aside from the Foraminifera & Diotomacea, for the study of which this material has proved of high interest, not much can be expected for the knowledge of

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[275] Slotten, *Patronage*, 87.

> the higher classes, the instrument used being only adapted to procure a small quantity of sand or mud.[276]

Pourtalès recorded the type of samples taken in his early sediment studies, small quantities of sand or mud, most likely gained from Brooke's sounding line after its development in 1853. Samples from before 1853 were likely gained from adding grease to the standard surveying line so that sediment would adhere to the heavy lead as it touched bottom. By 1853, when Maury sent off his deep-sea sediment samples to members of his scientific network, Bache and the Coast Survey staff had been studying the sea floor for almost a decade, though not with the benefit of Brooke's sounding line.

While William Stimpson accompanied John Mercer Brooke on the 1853 North Pacific Exploring Expedition, thereby joining the activities of the Harvard and Naval Observatory networks, Louis Francois Pourtalès stayed with the Coast Survey and conducted his own studies aboard the wooden steamer built for the Survey. The survey ships represented the interdependence between the United States government and science that characterized American seabed studies during the nineteenth century.[277]

Pourtalès was familiar with Edward Forbes' azoic zone theory. Forbes' theory was well accepted within the scientific community. Few had the opportunity to experience deep-sea dredging, let alone gather evidence against such an established scientific assertion. The survey ships, such as the USS *Corwin*, offered Pourtalès a civilian-controlled vessel for deep-sea research. While it is true that the ship had its own naval captain and the relationship between the civilian Coast Survey and the United

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[276] MCZA. Louis F. Pourtalès, “Notes on soundings.” MCZ 628. The strange wording in the first sentence is due to an incorrect pencil strike through in Pourtalès' notes.

[277] See chapter one of this dissertation for a discussion of the union between science and statecraft which benefited the United States Coast Survey's seabed studies and surveying science.

States Navy could be tense at times, the civilian naturalists of the Coast Survey dictated the use of the *Corwin*.[278] Pourtalès himself noted the unique situation available for him to explore the limits of Forbes' theory:

> The study of the constitution of the inhabitants of the bottom of the sea is a field of research which has attracted the attention of naturalists in comparatively recent times. What Humboldt did with regard to the distribution of life at different heights in the atmosphere, was done by Edward Forbes for the different depths of the ocean. The former's diagrams of the zones of vegetation on the slopes of the Andes are considered indispensable in every Atlas of Physical Geography…
>
> It is particularly in the greater depths, in the abyssal region as Forbes has designated it, that our knowledge is deficient. This is easily understood, since on most coasts it is situated at considerable distances from land, and its exploration requires an outfit beyond the means of but few private individuals. Government expeditions are generally fitted out for other duties & can rarely devote their time to a dredging occasion.[279]

Pourtalès recorded the challenges to early British and American deep-sea dredging practice: ships under naval control were not able to conduct the thorough dredging exercises needed to explore underwater zoogeology and the biogeographical distribution of marine creatures. Dredgings were sometimes taken by naturalists aboard naval-sponsored expeditions, such as the studies Charles Darwin conducted while aboard the *Beagle*. However, even the American North Pacific Exploring Expedition, a voyage specifically for the purpose of sounding and dredging the ocean, did not always afford enough time to deep water dredging as could be conducted. The tensions between American naval officers and civilian naturalists often limited the progress that could be done in deep water dredging.

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[278] See chapter one for a description of the tension between naval and civilian American naturalists.

[279] MCZA. Louis F. Pourtalès, “Notes on soundings.” MCZ 628.

Pourtalès' affiliation with the Coast Survey gave him the time and resources he needed to investigate the biotic debate. From an early point, Pourtalès expressed doubt regarding Forbes' azoic zone theory. His notes indicated that he was well acquainted with Forbes' work. However, his 1853 voyage brought him to doubt the veracity of Forbes' extrapolation regarding life at great depths. Pourtalès recognized that the practices developed in the American biotic debate complicated many of the theories based on zoogeology. He recorded this collision between American surveying practices used in the biotic debate and Forbes' azoic zone theory, including the evidence that convinced him against the azoic zone.

> A few interesting remarks suggest themselves from the above examinations, but before passing over to them the following question has to be addressed: are these small animals actually living in the location from which they were obtained, or have they been gradually washed down from the reefs near which the current has formed? I feel inclined to answer that they were living found from the fact that the greater number of the individuals are perfect notwithstanding the great delicacy of their shells. The delicate pink color of the Globulina... would scarcely be preserved if in specimens transported from a distance. The best argument in favor of this opinion is perhaps the fact of that the same species are found in a perfect state as far North as the Coast of New Jersey & New York. This is very singular however that the same species should be found living on the shores of Cuba & of some of the other West India Islands under exceedingly different circumstances of light & temperature.
>
> If we admit their living in the great Depths where we have found them in such abundance we are enabled to extend the limits of animal life to a much greater depth than is usually admitted. Prof E Forbes in his report on the distribution of Mollusca and Radiata in the Aegean Sea (Rep. Brit. Assoc. 1843) supposed that in depths beyond 300 faths animal life does not exist. In a former report on the subject (Proc. Am. Ass. Charleston meeting) I remarked that the Globigerina rubra seemed to increase in abundance with the depth. I have then seen specimens from Depths not exceeding 267 fath. Its greatest abundance did not exceed about 50 p.c. of the mass. We have now found a maximum of its occurrence at a depth of 1050 fath. Where this & allied forms constitute the entire bottom. It is but reasonable to suppose that it still Deeper exploration would show a gradual decrease for a considerable depth before it should cease to

> appear, as was shown for other species living in more shallow water in the report alluded to.[280]

Here, Pourtalès notes that the delicate pink shells used to support the biotic argument could also be deployed to reason through Forbes' theories of species distribution. These undersea organisms seemed to defy the basic logic used to support the azoic zone, since the delicate creatures increased with depth instead of slowly disappearing.

Pourtalès' notes from early voyages reveal a number of important evidentiary practices that differentiated the Coast Survey studies from other sediment-studying naturalists. Various naturalists involved in the biotic debate noticed the delicacy of the microscopic shells from the deep sea floor. These delicate shells were consequently regarded as objects of high scientific value. However, Maury, Ehrenberg, and Pourtalès each interpreted the intact nature of these deep seabed shells differently. Ehrenberg noticed the morphological similarity of the shells to other species at different geographical locations and depths. Maury saw the microscopical deep sediment through a tidological lens colored by his knowledge of naval shipboard practices. Pourtalès began to receive the sediment samples pouring in from various locations, thereby centralizing many of the sediment samples at the Coast Survey.[281] It was Pourtalès,

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[280] MCZA "Notes on the specimens of bottom obtained in the Gulf Stream by... steamer Corwin in 1853" June 8th 1853, pages 59 and 60. one sample from this expedition reached down to 1050 fathoms.

[281] Bache, 1858 *Report*, 249. The Boston Society of Natural History received Bailey's specimens after his death, which were subsequently loaned to Pourtalès. Later, various agents would send their specimens directly to Pourtalès instead of Bailey. Pourtalès used a Spencer microscope loaned to him by Professor Henry of the Smithsonian Institution. Pourtalès did have to catch up on European microscopical research at first, but his ready access to samples and institutional support (from both the his mentor at the Museum of Comparative Zoology and the Coast Survey) allowed him to begin announcing new discoveries quickly. All the latest European microscopical research could be reviewed at the Harvard libraries, where Agassiz resided. Perhaps a little romantically, Bailey lived long enough to conduct the first deep sea sediment microscopy thanks to Brooke's sounding line, and thereby starting the biotic debate, even though Pourtalès was the most active member of the debate after.

however, who offered the most authoritative voice in the American biotic debate and carried it forward in the late-nineteenth century. And while Pourtalès' gentle manner kept him from claiming his place in Maury's treatise, his analysis of the sediments carried significant weight for elite naturalists later into the century.

Pourtalès disagreed with Bailey's assessment that the microscopical specimens did not challenge the azoic zone theory. The morphological characteristics of the delicate shells led him to believe that the sediment was local, and not washed in from distant tides. Pourtalès would have been keenly aware of the Gulf Stream current and its ability to move marine creatures due to his role at the Coast Survey. If such delicate shells were to be preserved in such a tumultuous flow of water, they could not have settled far from where they died. Such a tiny, pink shell could not be preserved with its delicate arms and color intact if it had been dragged along the coast and into deep water through the Gulf Stream. That observation not only showed an awareness of marine hydrology, a perfect union of his engineering training and his post as Coast Survey tidologist, but also an awareness of marine biogeographical distribution. Specifically, Pourtalès began to compare his observations from other soundings up and down the Atlantic. Deep water *Globerina* were found still intact off the cold coast of New Jersey, beyond the Gulf Stream waters. Yet, the same species of shell was also found in the shallower coastal waters off Cuba. This line of reasoning showed the first signs of challenging not only the azoic zone theory, but also the established relationship between species and the geographical environment into which they fit. If *Globerina*'s biogeographical range could stretch from the coast of Havana down to 1000 fathoms to the cold sea floor, what else might be capable of living past Forbes' azoic line?

The result of Pourtalès' biogeographical reasoning, when combined with his ability to survey multiple sites along the American seaboad via the *Corwin*, would have a profound influence upon later evidences brought to bear on natural selection. Specifically, Pourtalès ended his analysis with a recognition that life in the deep would necessitate a reevaluation of zoogeology and theories of sedimentary fossil formation:

> In concluding I would remark how importantly a knowledge of the habitat & distribution of the Foraminifera is for geologists since of all classes of the animal kingdom none has contributed so large a share to the formation of rocks, at least in the Cretaceous & Tertiary formations.[282]

The discovery of deep-sea sedimentation could change naturalists' understanding of ancient geological formations. By extension, a belief that life could exist within the deep sea could change a zoogeologists way of examining the microscopic – or possibly even macroscopic – fossils found within the earth.

Pourtalès continued his sediment research dutifully until 1860 and produced five major papers.[283] His last paper, “On the Genera Orbulina and Globigerina” was published in 1858 in the *American Journal of Science and Arts*. In this last, rushed

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282 MCZA “Notes on the specimens of bottom obtained in the Gulf Stream by... steamer Corwin in 1853” June 8th 1853, pages 59 and 60.

283 The following is a list of Pourtales' major American publications before 1861, including the sediment studies. “On the Distribution of Foraminifera on the Coast of New Jersey, as shown by the Off-shore Soundings of the Coast Survey.” Proc. Amer. Assoc. for Adv. Of Sc, Charleston meeting, 1850. “On the Order of Succession of Parts in Foraminifera.” Proc. Amer. Journ. Of Sc. And Arts, 2d series, Vol. II, 1851. “On the Holothuridae of the Atlantic Coast of the United States.” Proc. Amer. Assoc. for Adv. Of Sc., 1851, p.8. A paper read in 1847 at meeting of Assoc. of Amer. Geol. And Nat. at Boston. “On the Gephyrea of the Atlantic Coast of the United States.” Proc. Amer. Assoc. for Adv. Of Sc., 1851, p. 89. “Extracts from Letters of L.F. Pourtales, Assistant in the Coast Survey, to the Superintendent upon the Examination of Specimens of Bottom obtained in the Eploration of the Gulf Stream by Lieutenants-Commanding T.A.M. Craven and J.N. Maffit, USN,” Coast Survey Report for 1853, and Proc. Amer. Assoc. for Adv. Of Sc., Cleveland meeting 1853. “Tidal Reports,” 1854. “Report of Assistant L.F. Pourtales on the Progress made in the Microscopical Examination of Specimens of Bottom from Deep-Sea Soundings,” Coast Survey Report for 1858. “On the Genera Orbulina and Globigerina,” Amer. Journ. Of Sc. And Arts, 2nd series, Vol. XXXVI, 1858. The next publications by Pourtales would not occur until 1867.

article, Pourtalès challenged the classification of two marine microorganisms established by Ehrenberg. Pourtalès believed that these two creatures, originally thought to be different genera, were actually different developmental stages of the same organism. His researches were not completed, yet he chose to publish his results while he could, perhaps anticipating an interruption in his research. And indeed, starting in 1861, larger issues began to concern the United States Coast Survey naturalists and the nation that they served. The biotic debate, the emerging American science, would have to wait.

Scientific Diaspora: American Seabed Naturalists During the Civil War

Conflict had been brewing in the United States for more than a decade over the acquisition of new territories. The land area claimed by the United States had quadrupled within two generations. As the United States stretched westward, politicians began to covet Mexico's ports and harbors along the Pacific. By 1845, the Republic of Texas joined the Union as a new state. The boundaries of Texas were still disputed. Only five years later, the United States acquired the California coastline. The Coast Survey was charged with surveying from the northern Oregon Territory to San Diego, California, by 1850. The question of whether new states would be slave-owning or free still lingered. A congressional compromise stalemated the northern and southern states for a decade. With such rapid expansion, the identity of the new states would either ensure the cultural future of the country as either northern or southern. Serious political conflict would erupt if the United States could not find a workable solution to their differences.

That conflict came to a head in November 1860 when Abraham Lincoln, the abolitionist Republican candidate, was elected president of the United States. This election precipitated decades of anxiety over the North-South political divide in America. Radical southerners successfully deployed the complex emotional interlude between the election and Lincoln's taking of the presidential office to turn southern fear and pride into political action. By February 1861, the Southern secession was in full swing. For some people, such as those from the South serving in naval institutions, the fracturing of America caused divided loyalties between Southern honor and national allegiance. The scientific networks of American seabed naturalists were no different. The United States Civil War reorganized the naturalist networks of the nation and temporarily spread its members across the Atlantic.

Matthew Fontaine Maury offers a prime example of this mid-century movement of naturalists across the ocean. He had already cultivated an international scientific influence, but his presence in during the Civil War is especially revealing. Specifically, his story illustrates that naturalists were deployed as international agents because of their scientific connections. Maury had been born in Spotsylvania County, Virginia, an area within the mid-Atlantic South.[284] When Maury was still young, his family had moved to Franklin, Tennessee, an area as split as Virginia in its allegiance to both the northern union and the southern secession. Like both of his childhood homes, Maury encountered tension in his dual loyalties. Maury's first duty was to his nation as a naval officer. He opposed secession and had worked to alleviate the brewing conflict over slavery.[285]

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[284] See Frances L. Williams, *Matthew Fontaine Maury, scientist of the sea* (Piscataway: Rutgers University Press, 1963), 1 and 14 for the number of kinsmen in the area and the importance of having them around to Matthew's parents.

[285] Maury had called an inquiry and received a promotion to Commander in January of 1858. This long process had validated his long years as Lieutenant while working for the Naval

Maury was also dedicated to his home state and his family residing there. Should he decide to support of his home, he would be forced to relinquish his hard-won commission with the US Navy in order to side with the Confederacy. Maury was in England to attend the British Association for the Advancement of Science annual meeting when he heard of Lincoln's election. Upon return, he wrote letters to various statesmen in a somewhat naïve attempt to coordinate a peaceful solution to the North-South conflict. He hoped that his home state of Virginia would remain faithful to the Union. Early surveys showed a 2 to 1 vote against secession in early April. However, everything changed with the provisioning of Fort Sumter, a federal fort off the South Carolina coast.

The military presence of the fort was a sensitive political issue for the state. When Lincoln provisioned the fort instead of evacuating it, the South Carolina forces fired upon the soldiers there. While no lives were lost, many of the upper Southern states saw the act as a breach of trust from the Lincoln administration. The middle states shifted radically in favor of secession. While he had urged Virginia to stay united with the North, he would follow his home state if she decided to leave the Union. On 18 April, he received word that Virginia had left the Union. April 19th would be Maury's last night as an officer of the Naval Observatory, the institution that he had grown and that had acted as his scientific center for many years. He crossed the Potomac the next day and made his way to Richmond, where he would join the Confederate forces.[286]

Observatory. Given the symbolism of that promotion, his handing it over during the Civil War was a significant gesture in Maury's eyes. See Williams, *Maury*, chapters 15, 18.

[286] See Williams, *Maury*, 365-398, for a narrative of Maury’s activities at the outbreak of the Civil War. See also Charles Lewis, *Matthew Fontain Maury: The Pathfinder of the Seas* (Annapolis: United States Naval Institute, 1927), 143-167.

Maury's experience with naval practices made him valuable for the Confederacy. Working in the Naval Observatory gave him a familiarity with the resources, both human and inanimate, available to the United States Navy. His defection to the Confederacy was not received well by his former commanding officers. He was labeled a traitor and accused of sabotage. The Boston *Evening Traveller* reported a bounty of $3,000 “for the Head of the Traitor, Lieut. Maury,” as much as offered for General Beauregard and over half of that offered for Jefferson Davis, the newly appointed president of the rebellious states.[287] Maury found opposition within the Confederacy as well. Davis had helped Alexander Dallas Bache to retain civilian leadership of the United States Coast Survey and, by doing so, aided Maury's main scientific competitor. Davis helped to transfer purview of deep-sea soundings to the Coast Survey.[288] The presence of people who had previously worked against his interests in the Confederate and United States Navy was in tension with his desire to use naval knowledge and be helpful in the war. Maury employed his technical skills by developing torpedoes and designing miniature gunboats for naval combat against the North. However, his Atlantic connections as an internationally renowned hydrographer made him invaluable to the Confederacy.

Naturalists and politicians from other nations esteemed Maury's scientific contributions. On 28 October, Maury received a plea to continue his scientific work from the Grand Duke Constantine of Russia. Maury's international fame gave him opportunities, and that first opportunity was to escape the war-torn state of his birth:

My Dear Captain Maury,

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287 Williams, *Maury*, 370.

288 See Brooke, *Brooke*, 142-152, for a narrative of the transfer of Lt. Berryman of the Dolphin to Bache’s control and the subsequent dispute over souding practices. Also see Williams, *Maury*, 236-247, for a similar, in-depth narrative which includes the participation of Senator Davis.

> The news of your having left a service which is so much indebted to your great and successful labours, has made a very painful impression on me and my companions-in-arms. Your indefatigable researches have unveiled the great laws which rule the winds and currents of the ocean, and have placed your name amongst those which will ever be mentioned with feelings of gratitude and respect, not only by professional men, but by all those who pride themselves in the great and noble attainments of the human race.
>
> That Your name is well known in Russia I need scarcely add, and though "barbarians" as we are still sometimes called, we have been taught to honour in your person disinterested and eminent services to science and mankind. Sincerely deploring the inactivity into which the present political whirlpool in your country has plunged you, I deem myself called upon to invite you to take up your residence in this country, where you may in peace continue your favourite and useful occupations.
>
> Your position here will be a perfectly independent one; you will be bound by no conditions or engagements, and you will always be at liberty to steer home across the ocean in the event of your not preferring to cast anchor in our remote corner of the Baltic.
>
> As regards your material welfare, I beg to assure you that everything will be done by me to make your new home comfortable and agreeable; whilst at the same time, the necessary means will be offered you to enable you to continue your scientific pursuits in the way you have been accustomed to.
>
> I shall now be awaiting your reply, hoping to have the pleasure of seeing here so distinguished an officer, whose personal acquaintance it has always been my desire to make, and whom Russia will be proud to welcome on her soil.
>
> Believe me, my dear Captain Maury,
>
> Your sincere well-wisher,
>
> Constantine,
>
> Grand Admiral of Russia[289]

The letter given to Maury illustrates the value of his interpersonal connections and the respect that the international community of philosophical naturalists had for him. Maury's

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[289] See Reverend J. William Jones, ed., "A High Courtesy from Across the Waters," in *Southern Historical Society Papers*, v. 2 (Richmond: Virginia Historical Society, 1876), 49, for the publication of this letter and other references to Maury, including his reply to the offer. Note that this letter was published earlier in the Richmond *Enquirer*, Nov 21, 1861. See also Williams, *Maury,* 384.

ability to elucidate natural laws for the atmosphere and oceans secured him an invitation to Russia. The letter also recognized that the Civil War was enough reason to desire an escape from the United States during the 1860s. Not only would Maury's researches be halted because of the fighting, but his very life was in danger. The United States considered him a traitor. War conflict could claim his life. Even the harsh wartime living conditions could endanger him, his family, and his research. Maury consulted his family about the offer, but they ultimately decided to continue fighting for the honor of Virginia.

The Grand Admiral of Russia would not be the only person to offer Maury a new start based on his connections as a famed naturalist. In April of 1862, the French minister to the United States Henri Mercier handed Maury a letter from Emperor Napoleon III. The letter was an invitation to live in France. Napoleon's invitation offered Maury a secure location to continue his scientific works. Maury declined. However, a meeting with the French diplomat would not be complete without a discussion of the *Trent* affair, a political gaffe that had threatened rupture between England and the United States, which had transpired the previous November.[290] United States politicians were keen to keep European countries from recognizing the Confederate States as a legitimate government, even though many had granted Confederate ships the same rights as Union ships in their foreign ports. Captain Charles Wilkes, the irascible United States captain who had commanded the first American Exploring Expedition, had stopped two new Confederate diplomats aboard a British mail carrier headed to England. The captain fired two warning shots at the ship, declared the two diplomats “contraband,” and attempted to take the two into custody by force. The diplomats turned themselves over to Wilkes instead of endangering the British crew. Wilkes and his first mate, however, were still not pleased with the situation and stopped just short of seizing

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290 See Williams, *Maury*, 360, 390.

the *Trent* and sailing her to the nearest port as a contraband prize. Needless to say, Her Majesty's government was not pleased by the violation of the HMS *Trent*.[291] The United States could not afford a two-front fight and the incident almost caused England to declare war against the torn nation. A declaration of war against the United States would likely have forced England to recognize the Confederacy as a sovereign nation.

While originally thrilled at the capture of the two diplomats, Lincoln quickly realized the reality of needing to avoid war with England. He released the prisoners and disavowed the actions taken by Captain Wilkes. While an Anglo-Northern war was averted, the *Trent* affair had repercussions for travel across the Atlantic. The incident drew attention to Confederate diplomacy. The interrupted diplomatic mission was not successful, which could have been partly due to political sensitivity after nearly going to war over the *Trent* capture. The Confederates needed to send agents who would be seen by British and French statesmen, but who would not draw as much attention. The dispersal of dignitaries from the North and South was also more politically protected after the gaffe. The United States could not risk another incident and the Confederacy would not risk losing favor after England had nearly gone to war over the capture of their diplomats. England, in turn, would be much more ready to protect her ships. Of course, while the movement of Confederate agents was somewhat protected, they still had to cross the United States blockades of the major Southern ports.

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[291] The Anglo-American War of 1812 was fought over British ships stopping American vessels and taking people off board by force. The British were understandably incensed by the Americans doing the same thing that they went to war over only decades before.

In the summer of 1862, Maury received orders that he was to run the blockades and proceed to Europe as a spy and Confederate agent.[292] His main mission was to purchase a cruiser in England and equip it. However, he also had the opportunity to gather information and influence decisions while abroad. Maury felt as though he was being sent away because of his enemies within the Confederate government. His well-known tendency to criticize and antagonize his colleagues would certainly lend credence to that conclusion. However, Maury's position within a scientific network that stretched across Europe gave him an entrée to influential people to which diplomats would not normally have access. As just one example, Maury had been in contact with such naval figures as Vice-Admiral Robert FitzRoy, the captain made famous for his South American survey aboard the *Beagle*.[293]

On 9 October 1862, Maury quickly transferred to the CSS *Herald* from the heavier *Hero* under cover of night. He had waited patiently for an opportunity to bypass the Northern blockade for almost a month. A Northern sloop-of-war, however, saw the *Herald* as it attempted to slip into the open sea when the moon came out from behind the clouds. The sloop opened fire and Maury barely made it back to harbor. Three nights later, the tiny ship managed to squeeze by the patrols and headed, in need of repairs from wind and rain, towards Bermuda, where Maury would catch a British ship and proceed to England. During the trip, Maury's ability to navigate by celestial observation

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[292] See Williams*,* *Maury*, 399-420, for Maury’s dealings with the British in the Civil War. See also Lewis, *Maury*, 168-185, for a more explicit narrative of Maury’s political actions in Britain and Europe.

[293] Williams, *Maury,* 396. Both individuals had an interest in current and meteorology.

and his reception as a great scientific dignitary awed the crew.[294] After two weeks as an honored guest in Bermuda, Maury boarded the Royal Mail Steamer *Delta*.

Maury had made it safely out of American waters, but his danger was not over. The USS *Mohican* and the USS *San Jacinto*, Captain Wilke's ship, were waiting for him. Rumors were that Wilkes would attempt to capture Maury the same way he had captured past Confederate diplomats aboard the *Trent*. With a price on his head nearly as high as Confederate President Davis', Maury was understandably worried as he sailed from Bermuda. However, as the *San Jacinto* closed in, Wilkes was met by two British ships, the HMS *Immortality* and the HMS *Desperate*. Wilkes fell behind and Maury was allowed to continue safely to Halifax, Nova Scotia, where the Confederate flag was flown above Maury's hotel in honor of their distinguished guest. From Nova Scotia, Maury made his way to England aboard the *Arabia*.

Maury arrived at Liverpool on 23 November, 1862. He met with Confederate agents with orders to buy and outfit ships. He was also authorized to conduct other, subtler services on behalf of the Confederacy. After a short stay, he moved to London,

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294 A story from James Morgan, *Recollection of a Rebel Reefer* (Boston and New York: Houghton Mifflin Co., 1917), 100, recounts the Hero and Herald's attempts to run the harbor blockades. After repairs, the captain had asked a schooner for the current latitude and longitude. Such ships and their crews were not used to being so far out to sea, so the captain easily became lost. After the six days his maps told him the journey should take, Bermuda was nowhere to be found. Finally, the captain admitted to Maury, possibly the most world-famous navigator at the time, the he had lost his way from Charleston harbor to Bermuda. Maury told the captain that he could do nothing until nightfall, at which point he emerged from his cabin and, “At ten o'clock, the great scientist and geographer went on deck and took observations, at times lying flat on his back, sextant in hand, as he made measurements of the stars. When he had finished his calculations, he gave the captain a course and told him that by steering it at a certain speed he would sight the light at Port Hamilton by two o'clock in the morning. No one turned into his bunk that night except the commodore [Maury] and his little son; the rest of us [the crew] were too anxious. Four bells struck and no light was in sight. Five minutes more passed and still not a sign of it; then grumbling commenced, and the passengers generally agreed with the man who expressed the opinion that there was too much d—d science on board and that we should all be on our way to Fort Lafayette in New York Harbor as soon as day broke. At ten minutes past two the masthead lookout sang out 'Light ho!' -and the learned old commodore's reputation as a navigator was saved.” See Williams, *Maury,* 400-401 for a recount of the voyage experience.

where dignitaries frequently came to pay their respects to the eminent naturalist. Maury drafted a long letter for popular publication in support of the Confederate cause, which appeared in the London *Times*.[295] He made numerous efforts to gain sympathy for the South while in England. He also gathered cultural and political information and sent it back to his Confederate colleagues. Northern agents in England watched Maury, mostly in secret. However, some Northern attempts to thwart Maury's actions had to be carried out more explicitly.

While Maury helped to purchase and equip a new confederate ships with cotton bonds, he also continued to attend scientific gatherings. Maury attended both the 1863 Newcastle-on-Tyne and the 1864 Bath meetings of the British Association for the Advancement of Science. There, he would have access to influential members of British society. The visit to Newcastle-on-Tyne would not only serve scientific purposes for Maury, it would also be an opportunity to exchange his international fame for Southern sympathies and state secrets.[296] The potential effect of Maury's attendance at the 1863 British Association meeting did not go without response by United States agents. The British Association signature book for foreign dignitaries attending the meeting includes a curious entry for 1863. Joseph Henry McChesney had been assigned as American Consul at Newcastle-on-Tyne and was in attendance at the meeting with a delegation. McChesney was a professor of natural history in Illinois and had campaigned for Abraham Lincoln during his election. McChesney and Lincoln had bonded somewhat

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[295] Matthew Fontaine Maury, “Letter to the Editor” in *Official Records of the Union and Confederate Navies in the War of the Rebellion* (Harrisburg: Broadfoot Publishing, 1987) reprint of *London Times,* December 22, 1862.

[296] Take, for example, the combination of scientific knowledge and war secrets in Maury’s research on electric torpedoes and mines. See Lewis, *Maury*, 178-179.

because of a mutual love of geology.[297] The American Consul's signature is bold and unusually formal in the British Association book. Maury signed his name in the book as well, with small, discreet lettering. In place of an address where he could be found, “M.F. Maury” listed that he was “staying with the mayor” and gave no national affiliation. The 1863 British Association meeting must have been tense for the American delegates, both northern and southern. The 1864 meeting had no known American scientific delegation. Maury signed his name proudly, “Matt F. Maury – Confederate States.”[298]

Maury continued his campaign for Confederate sympathy until the end of the war. He began an extensive correspondence with Emperor Maximilian, the newly appointed French ruler of Mexico, who had written to congratulate Maury on his scientific achievements.[299] Upon the war's end, he would join Maximilian in Mexico for a short time. And while Maury's efforts on behalf of Virginia and the South would gain him great scientific notoriety abroad, it eroded any legitimacy he had in the United States. For example, Maury's ties to the British network of physical naturalists earned him a doctorate of law from the University of Cambridge in 1868.[300] Maury's longstanding comparison of deep-sea studies and celestial observation had finally yielded scientific recognition from the Cambridge naturalists. However, the sales of Maury's *Physical Geography of the Sea* had dropped significantly in the United States since the start of the war, leaving him with little money. His status as a high-ranking Confederate agent also excluded him from Lincoln’s amnesty. Maury was not only unable to return to the United States Naval Observatory, the center of his former institutional power, he was unable to return to America altogether. His Atlantic connections started his journey as a

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[297] James Lander, “Herndon’s 'Auction List' and Lincoln’s Interest in Science” *Journal of the Abraham Lincoln Association* 32 (2011): 16-49.

[298] BLSP DEP BAAS 135.

[299] Williams, Maury, 414.

[300] Williams, Maury, 450-451.

naturalist abroad, made him valuable to the Confederacy, and then forced him to continue moving across the Atlantic for many years after the war ended.[301]

Maury had the ability to spread American scientific practices while abroad. As a member of John Mercer Brooke's close network, he had access to the newest developments in deep-sea surveying; Brooke had invented the scientific instrument needed to retrieve deep-sea floor samples. Brooke's pre-1861 developments remained the newest practices because Brooke was also caught up in the events of the United States Civil War.

Like Maury, Brooke had ties to old Virginia. He joined the United States navy when he was only fourteen, where his curiosity and mechanical inclinations served him well. Brooke had actually worked for the Coast Survey by compiling data beginning during the 1849-1850 winter and later aboard the brig *Washington*, so he was aware of the Survey's activities and methods.[302] He was also integrated into the Washington DC community, where he lived with his newlywed wife. Indeed, after intermittent cruises along the African Coast to suppress slave trading, Brooke began to focus on scientific work in order to gain prestige and more time with his wife. On 15 October 1851, Brooke reported for duty at the Naval Observatory and, therefore, gained Maury as a direct supervisor.[303] After six months at the Naval Observatory, Brooke offered his first solution to sounding depths greater than 1000 fathoms. While the first invention was impractical, his second resulted in the sounding line that was used in later survey missions.[304] The

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[301] Maury eventually returned to the United States to accept a professorship at the Virginia Military Institute in 1868. He was not arrested -as he feared- when he reentered the country.

[302] George Brooke, *John M. Brooke: Naval Scientist and Educator* (Charlottesville: University of Virginia Press, 1980).

[303] Brooke, *Brooke*, 51.

[304] A minor dispute arose over who invented the sounding line, Brooke or William Greble, the instrument-maker. Greble had suggested using two arms that would disengage the sounding lead

sounding line brought up deep water samples for the first time, prompting more sounding devices to be made and distributed. Brooke's line was not without its complications. The new technology, while easier to use than previous models, still required some finesse to master. Even its inventor had to try a few trials before the technique became reliable.

The usefulness of Brooke's sounding line for the biotic debate became immediately apparent on its first expedition. When assigned to the North Pacific Exploring Expedition, Brooke and the crew were intensely interested in the changes in surface coloration of the ocean. The expedition flagship *Vincennes* would pass from clear, dark blue water to markedly green water very suddenly. Brooke and Stimpson, the ship's naturalist and member of the Harvard seabed network, both speculated that the color change was due to diatoms in the water's surface. The presence of diatoms on the water's surface was a major topic in the biotic debate, as some naturalists speculated that the diatoms would die at the surface and drift down to the lifeless sea floor below. Commander Ringgold took great scientific interest in this phenomenon, “We often suddenly passed from the ordinary clear blue water into green, of very decided colour, indicating Soundings – but although various casts were obtained at different depths, our efforts to reach bottom were unavailing.”[305] Brooke was crushed at the inability of his new sounding line to obtain deep-sea samples from the green areas. Such a sample might have shed light upon the biotic debate, a substantial aspect of the voyage's mission. Later soundings from the expedition were more successful, which cheered up Brooke's dampened spirits.

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when it hit bottom. Brooke had originally proposed one arm. Brooke won the disagreement, saying that the crucial invention was the arm itself, not the number of arms on the device. Either way, the one-arm version of the sounding line became dominant over time. See Brooke, *Brooke*, 56-57.

[305] Brooke, *Brooke*, 84-85. Original quote taken from Brooke's Abstract Log of the Vincennes, 22 and 25 July 1853, RG 45, National Archives.

Brooke did get the opportunity to observe organisms from the deep before the Civil War because of his apparatus. As he continued to make improvements to his device, he gained more sediment samples for analysis. From observing these samples, he became convinced that life existed in the deep: “As soon as we recovered our specimen, a greenish sediment it was put under the microscope and we saw, I believe we are the first, living animals from that great depth [1,700 fathoms]. To be certain that they came not from the upper waters we cut a quill in two and from the middle portion of the sediment which was firmly packed we selected a specimen and in it we found the animals, infusoria [microscopic organisms] abundant.”[306] Brooke and other naturalists in the biotic debate were well-poised to reach a few conclusions about the existence of microscopic life on the deep-sea floor when the Expedition returned. That debate, like the unfinished surveying charts and yet-to-be-classified specimens from both the North Pacific Exploring Expedition and Brooke's subsequent expedition aboard the *Fenimoor Cooper*, would have to wait to be completed until after the Civil War.[307]

The Civil War severely impeded Brooke's ability to develop deep-sea instruments, practices, and research. That cessation of research was abrupt. To highlight the interruption that the Civil War posed to Brooke, consider that in February of 1861 Brooke was sending sea floor specimens to England for the examination of the Microscopical Section of the Manchester Literary and Philosophical Society.[308] Two

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306 Brooke's Vincennes Journal 26 July 1855, BP. The quote can be found in Brooke, *Brooke*, 1980, 122.

307 Brooke commanded the Fenimoor Cooper and its expedition. The ship was to finish the aspects left undone by the North Pacific Exploring Expedition, like surveying the sailing routes from the United States to East Asia. The Fenimoor Cooper was lost in the process. Brooke, however, commanded the first Japanese diplomatic voyage from Japan to the United States. While only a consulting navigator at first, his naval command experience was so valuable that the crew basically gave complete control of the ship to Brooke. See Brooke, *Brooke*.

308 Brooke, *Brooke*, 218. Personal correspondence, Brooke to Maury, 21 Feb. 1861, Letters received, Naval Observatory, RG 78, NA.

months later, when Brooke resigned from the United States Navy, the international network of seabed naturalists lost a major contributor. Brooke had not only invented the deep-sea sounding line, he was also the most highly trained naturalist in deep water sounding practices. The gathering of specimens was difficult and not everybody was successful in the sounding line's operation. The sounding samples were sought after as objects of great scientific curiosity. Brooke's practices represented the first, reliable method of obtaining those samples. And those samples reached across the Atlantic before the Civil War, therefore reinitiating a major debate in England over the existence of life at great depths of the sea.

However, Brooke did not participate during the first years of the debate that emerged from his invention and practices. He resigned his commission with the Navy on the same day as Maury, 20 April 1861, after Virginia's provisional vote to secede from the United States. Brooke's technical expertise was in high demand. The Confederates had no navy and their ports were being blockaded by United States ships. When Brooke joined the Confederate forces in Virginia, he was given the task of solving the Union blockade with no fleet. Brooke's solution was the creation of a vessel unseen in the United States before that point. When a group of Confederates managed to capture the USS *Merrimack* by surprise, Brooke enacted his plan to create an armored warship, known as an ironclad. Brooke sketched the plans for the ironclad conversion, adding a number of innovations to his design as he went. The *Merrimack* was relaunched as the renowned CSS *Virginia*. However, the refitting had left out a few of Brooke's design elements, including a gun that would have pierced armored plating on other ships. The ironclad *Virginia* sank two Northern ships before she encountered an enemy ironclad that the North had built in response to Brooke's ingenuity. The battle resulted in a stalemate. Had the builder included the rest of Brooke's design elements, including the heavier guns, it is very likely that the *Virginia* would have sunk the North's ironclad, the USS *Monitor,* and turned the tide of the Civil War. The blockade played a large role in limiting the power of the Confederacy and an unchecked Southern ironclad would have broken the naval stranglehold upon the South. As it stood, the *Virginia* still tied up Federal ships and resources. It also struck fear into politicians and citizens living along the Atlantic seaboard; they envisioned the *Virginia* laying waste to Boston harbor and other vulnerable seaports.

Brooke continued to be a valued member of the Confederate naval force. However, his participation in the war understandably halted all engagement with the biotic debate. He could not produce more sediment samples for distribution. This lull in activity alleviated foreign pressure to keep up with the debate. Maury also faced a difficult situation at the end of the Civil War. While many of his close friends remained loyal to Brooke even though they had fought on opposite side of the war, Brooke's rank in the Confederacy excluded him from the general amnesty offered to the members of the Confederate armed forces. He would have to apply personally to President Lincoln for a pardon. In addition, Brooke's ingenuity had caused immense distress to the Northern forces over the course of the entire conflict. For example, the CSS *Virginia*'s ability to break blockades caused more than enough frustration for the North. The Union forces had to scramble to match Brooke's ironclad. Such successes were bound to generate animosity, and he encountered opposition when he sought amnesty once the war ended. It took the intervention of Brooke's old commander from the Coast Survey Admiral S. P. Lee to gain the president's pardon. And in August of 1866, Brooke received his pardon thanks to Lee's efforts.[309]

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[309] Brooke, *Brooke*, 291.

Northern naturalists also moved across the Atlantic and shared information during the Civil War. William Stimpson was also a member of Brooke's scientific network and, therefore, had intimate knowledge of his deep water surveying practices. He had also developed a number of his own deep-sea dredging practices while naturalist for the North Pacific Exploring Expedition. Stimpson provides another point of entry for American deep-sea scientific practices during the Civil War. He traveled to England during the later half of the war as the guest of the British Association for the Advancement of Science dredging committee. There he waited out several months of the war abroad, dredging and sharing American techniques developed on the North Pacific Exploring Expedition.[310] Stimpson, like Maury, was a naturalist who had worked directly with Brooke as he developed his deep-sea sounding technique and, so, was well-qualified to spread the practice while abroad. However, the majority of Stimpson's research was mishandled after the North Pacific Exploring Expedition returned to the United States. The mismanagement was exacerbated by the onset of the war. And upon the war's conclusion, most of his specimens were destroyed in the Great Chicago Fire of 1871.[311] The specimens that survived were those already in circulation to fellow naturalists at the time. Like his contemporaries, Maury and Brooke, the end of the Civil War was a particularly difficult time for Stimpson. He died shortly thereafter, not so much from the fire as from the loss of his life's work.

The US Civil War disrupted American seabed science during the mid-nineteenth century. Americans had dominated the deep-sea sediment debate and the biotic debate up until 1860 because of their ability to produce the first samples from great depths. Brooke's sounding line provided the instrument required to explore beyond Forbe's azoic

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[310] Rozwadowski, *Fathoming*, 56.
[311] Rozwadowski, *Fathoming*, 55.

zone and pull back samples of the abyssal seabed. The outbreak of the Civil War occupied John Mercer Brooke and he was unable to continue his sounding activities. However, the two naturalists that worked closest to him found themselves in England for extended periods of time during the conflict. Both Stimpson and Maury had extensive contact with the British Association for the Advancement of Science during the 1860s. Maury had strengthened his connections to the Cambridge group through his astronomical work and hydrography. Stimpson associated with the Edinburgh groups through Forbes' old crowd, the BAAS dredging committee.[312] Other naturalists also traveled to England during this period, but their influence is beyond the scope of this dissertation. What is known is that American deep-sea dredging and sounding practices diffused into the British seabed science networks during the 1860s.[313] These practices would be modified in unique ways by each of the philosophical naturalist networks, especially since some of the most revered American seabed naturalists were rebuilding their lives after the Civil War instead of participating in the biotic debate.

Pourtalès and Seabed Science in the Post-Civil War Era

By 1867, the war was over and the Coast Survey could focus on scientific pursuits with more support from the federal government than ever. Resources that had been diverted to the war were given back to the Coast Survey. For example, the USS *Corwin* was placed back under the Survey's control. Pourtalès was assigned to the *Corwin* and given permission to expand his biological research, though a bout of yellow

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[312] See chapter one of this dissertation for more discussion about the Edinburgh and Cambridge networks of seabed science.

[313] See SIOA, “Matkin Papers,” MS 2, San Diego, California, and also see chapter five for a discussion of this topic.

fever delayed much of his early progress. Several years had passed since the Coast Survey members had engaged in the biotic debate. However, upon getting back to scientific work, the biotic debate research became a priority. Even when Alexander Dallas Bache, the Survey superintendent, died in 1867, the next superintendent Benjamin Pierce continued to support Pourtalès' biological studies.

While previously only able to conduct microscopic analyses of the sea floor, Pourtalès was ready to develop his deep-sea dredging techniques and search for macroscopic life in the abyss. Pre-1860 studies had convinced him that Edward Forbes' azoic zone was incorrect. Microscopic organisms were capable of living in the deep. If small organisms could escape the harsh and unforgiving environment of the abyss, it was possible that larger creatures could do the same. Pourtalès recorded how he rejoined the biotic debate after the Civil War. As the Coast Survey's studies stretched down to the Gulf Stream again, so did Pourtalès' soundings and dredgings:

> The investigations of the character of the bottom of the sea & of its inhabitants was one of the points to which the late Superintendent of the Coast Survey, Prof. A. D. Bache had given particular attention since the early part of his administration of that work. For this purpose a larger number of specimens of bottom, brought up by the lead, were collected, which have yielded results of high interest when examined microscopically.
>
> Among those results none were perhaps more surprising than those connected with the exploration of the Gulf Stream. The entire bottom at depths previously supposed to be entirely uninhabited by living beings, was found to be entirely covered & indeed composed of minute animals & their debris. Unfortunately, the late war caused a long interruption in those researches, and it is only last year that the present Superintendent B. Pierce, has been enabled to extend his field of operation again over the Gulf Stream. It is his intention henceforth to continue & complete the work so successfully initiated in that last part of the ocean by his predecessor, and besides observations of the depth, velocity, direction of the stream, temperature & density of the water at different depths, it will include researches into the Fauna of the surface of the bottom & of the

> intervening depths. Not only is it expected to obtain in this way an insight into a world scarcely known heretofore, but that knowledge will have a direct bearing on a better understanding of many of the phenomena of that great current. Thus a new light may be thrown on its powers of transportation from shallow to deeper water or along its bed, its action in forming deposits in particular localities, or its possible influence on the growth of coral reefs on its shores.[314]

Pourtalès' skepticism of Forbes' azoic zone is well documented in his sounding notes. He was especially interested in finding living creatures in the abyss. His biological research strategy was linked integrally to the Coast Survey's hydrographical research; the two projects were indistinguishable and inseparable. The Gulf Stream represented a powerful, physical force for moving sediments and living organisms across the sea floor, thereby affecting their zoogeological distribution in future sedimentary formations. Pourtalès' sounding and dredging research seamlessly combined both physical hydrography of the Cambridge variety with Edinburgh-inspired zoogeology.

Part of his dredging technique included constant microscopical sediment observations. At each location where he applied the dredge, Pourtalès recorded what type of sediment was found. The distribution of marine creatures could be identified by the deep-sea sediment found in the geographical region. Naturalists could identify the fossils by examining the microscopical creatures that could be found where the creature once lived, died, and was preserved as sedimentary fossil. Unlike the Edinburgh zoogeologists, Pourtalès made no immediate morphological observations of his dredge specimens. His primary concern was to see whether life existed in deep water and, if so, how they might be preserved in stone. The morphological observations remained

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[314] MCZA. Louis F. Pourtalès, “Notes on soundings.” MCZ 628.

incomplete until he surveyed what organisms lived in the deep Gulf waters, when the identification of his dredge specimens became necessary.[315]

The expedition's mission of surveying the seabed between “Key West and Havana, incidentally with the purpose of sounding out the line for the telegraph cable shortly afterwards laid between these two points” facilitated Pourtalès' constant dredgings and soundings.[316] Knowing the depth between Key West and Havana was of obvious utility for the laying of a cable between the two cities, as companies needed to know how much cable to manufacture. The microorganisms located on the sea floor would also indicate the physical conditions the cable would face while resting deep below the surface. In addition, Pourtalès was also interested in any creatures he might find, both for information about protective casing for the cable, but also for zoogeographical reasons. Pourtalès research was in no way simply commercial in nature, the results of his studies stood to challenge previous zoogeological theories. If larger creatures did exist in the depths, then they would also be preserved in the sedimentary rocks of the Cretaceous and Tertiary periods. An understanding of sedimentation could afford a window into the living creatures and physical conditions of Earth's distant past.

The Americans still led the biotic debate, even after the Civil War, though more people had access to deep-sea sediment samples than before. While many of the

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[315] His sounding notebook records the changing consistency of the bottom sample and what creatures are found in this area. He observed when the sediment consists of “Globulina,” which is a microscopic organism. See page 8, row 95 for just one example. This record indicates regular microscopic examination and sediment observation. He recorded other creatures found in the immediate area, such as minute bivalves. No morphological recordings are included in his early field notes. However, he does describe creature morphologies later, once larger organisms were dredged from great depths.

[316] See Louis F. de Pourtales, “Contributions to the Fauna of the Gulf Stream at Great Depths,” *Bulletin of the Museum of Comparative Zoology* 6 and 7 (1867), 103.

American naturalists were out of commission due to their previous defection to the Confederacy, the remaining individuals were still the most highly trained deep-sea dredgers in the world. British naturalists excelled at the shallow water dredging practiced by Edward Forbes earlier in the century. The only other active and proficient deep water dredgers known in the international biotic debate were Scandinavians. While the details of Scandinavian dredging practices are beyond the scope of this chapter, both Pourtalès and British dredgers were aware of Scandinavian dredging activities. Pourtalès wrote in his journal about the differences between British, American, and Scandinavian dredging practices, “170 faths [sic] the greatest depth dredged in the British Seas. Greater depths obtained on Scandinavian Coasts.”[317] The comment on the maximum depth dredged by the British members of the biotic debate is in reference to his notes on a standard 270 fathom dredge.

The dredgings along the Gulf Stream waters regularly pulled up complex organisms that lived deeper than the azoic zone. Brooke's sounding line also validated that the dredge picked up the creatures from a depth around or greater than 300 fathoms. American waters from Florida to Cuba are relatively shallow, especially when compared to Atlantic and Pacific depths. However, a depth of almost 300 fathoms, where Forbes had postulated would contain no life, was enough to offer significant proof of organisms living in the deep. Pourtalès was able to dredge up larger, complex creatures, too. For example, the 270 fathom dredge pulled up a brachiopod specimen. That species of brachiopod had been known as having fossils ranging all the way back to the Devonian period. Pourtalès took particular interest in complex creatures found in great depths. His notes describe the find in both morphological and biogeographical terms:

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[317] MCZA. Louis F. Pourtalès, “Notes on soundings.” MCZ 628.

> Terebratula, n sp. Shell globose their light Lorncolined, transparent, obscurely pentagonal, mouth is showing faintly the lines of growth; hinge teeth strong; the interior margin of the transverse portion of the loop denticulated, showing rounded three indentations & two sharp angles, differing in respect from T. vitrea in which this part is entire; otherwise the shell resembles the latter very closely. [?] rather small, largest shell 1 1/10 inch long, 9/10 inch broad, 7/10 inch high, several specimens found in 270 faths off Havana. It may prove to be identical with an undescribed Terebratula from recent formation in Guadaloupe, mentioned in Bulletin Soc. Geol. De France, t.XX, 1863.[318]

Most interestingly, Pourtalès related his living creature to ancient fossils reported in a French publication. Here was a potential living creature only previously known to have been preserved within ancient geological formations. The *Terebratula* would be one of the early examples of these ancient creatures found at depths around or below 300 fathoms.

The greatest discovery of the expedition was a *Rhizocrinus* that seemed to be distributed from the Florida Straits all the way to Norway. A few years earlier, the Norwegians had found similar creatures below 300 fathoms that were known only in the fossil record.[319] The *Rhizocrinus* linked American deep-sea dredging with Norwegian accounts. It also linked the bathymetric distribution of these species to the Gulf Stream, where the underwater current was supposed to carry the creatures from Florida waters to the Lofoten Islands. If the dredging of creatures from an area that was supposedly devoid of life was not enough to elicit curiosity, the discovery that these creatures were forms thought to be long since extinct would draw considerable attention. These strange, deep-sea creatures were dredged with increasing regularity on subsequent voyages. Pourtalès was able to persuade the aging Louis Agassiz to join him on a third cruise in

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[318] MCZA. Louis F. Pourtalès, “Notes on soundings.” MCZ 628.

[319] See chapter four of this dissertation for more on the discovery of the *Rhizocrinus* by the Norwegian naturalists.

1869, this time on the *Bibb*. By the 1870s, international research on the biotic debate had changed the way naturalists perceived the deep sea. It had gone from a lifeless zone to a subject of intense scientific curiosity.

Pourtalès continued to publish on marine invertebrates for the remained of his life. His first publication on the fauna of the Gulf Stream appeared in 1867, and he continued to publish on the subject until 1869. In 1871, he published what was possibly his most well-known and comprehensive study of deep-sea marine invertebrates, *Deep-Sea Corals*. He saved the crinoids, echini, and corals dredged from his expeditions for his own study and publications, which continued until 1880, having published fifteen post-war publications in all. His later years yielded more time for research. The death of his father left him a considerable inheritance and the title of “Count Pourtalès.”[320] He quit his position at the Coast Survey and returned to Harvard to assist Louis Agassiz. He also mentored the young Alexander Agassiz, Louis Agassiz's son. Upon Louis Agassiz's death in 1873, Pourtalès became the director of the Museum of Comparative Zoology, where he continued his work on the biotic debate. By that point, there was little doubt that Forbes' azoic zone theory was not completely correct. However, it was not known whether Forbes had underestimated the depth of the azoic zone or if life existed even at the greatest depths. The question of life in the deep, the extension of the biotic debate into the later nineteenth century, would be addressed by British naturalists. And that incorporation of American deep-sea dredging practices would change other theories as well. American deep-sea sediment studies would later pose a serious challenge to the theory of evolution by natural selection.

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[320] Many of the letters that Pourtalès received affectionately and respectfully address him as “Count Pourtalès.” MCZA, also see the obituary footnote from earlier in this chapter.

CHAPTER FOUR: The Deep-Sea Floor as Darwin's Laboratory

In *On the Origin of Species* Charles Darwin portrayed the deep ocean as a lifeless environment. An azoic deep sea allowed Darwin to explain why there existed no evidence for evolution in the form of deep-sea marine fossils: “the imperfection of the record being chiefly due to organic beings not inhabiting profound depths of the sea.”[321] According to his reasoning, if neither creatures nor sedimentation existed within the deep sea, then no fossilized remains would be preserved from the deep ocean environment. Without creatures, there would be no fossils. And anyway, the deep sea's lack of sedimentation would prevent rock-forming material from burying any organic remains even if they did exist. Unfortunately for Darwin’s argument, knowledge of deep-sea biology changed rapidly during the years that followed. American naturalists began to question whether or not life existed on the deep ocean floor as a result of new sounding practices. This debate, known as the “biotic debate,” spread from the United States to the British Isles at the same time that Darwin's theory of natural selection made its debut. Darwin's closest scientific colleagues were prominent British contributors to the biotic debate. As the British biotic debate gained momentum, these naturalists began to link their deep-sea biological research to their efforts to support or test evolutionary theory. By the summers of 1868 and 1869, when British naturalists organized a series of three deep-sea dredging expeditions to search for life in the abyss, the British biotic debate had become inextricably entwined with the search for evidence of natural selection.

A number of historians, especially historians of oceanography, have examined the events that led to the British deep-sea dredging voyages: the HMS *Lightning*, the

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[321] Darwin, *Origin of Species*, 172-173.

HMS *Porcupine*, and the HMS *Challenger*. These expeditions, especially the voyage of the *Challenger*, are canonical events in the history of oceanography. The historian Margaret Deacon has pointed out the importance of the expeditions' evolutionary aims.[322] However, Deacon's study was primarily a history of physical oceanography and, therefore, the voyages' importance for nineteenth-century biology remains less explored. These three voyages were pivotal moments in the history of evolutionary theory. For example, the discovery of two new deep-sea creatures, *Bathybius* and *Rhizocrinus*, prompted the voyages of the *Lightning* and the *Porcupine*. Eminent naturalists, such as Edward Forbes, had valued crinoids, such as the *Rhizocrinus*, as sources of biological information throughout the nineteenth century. The new crinoid generated much excitement within elite scientific circles. The abyssal invertebrates sought by the British deep-sea dredging expeditions held historical importance for the study of organismal complexity.[323] This longstanding tradition of using marine invertebrates to prove zoogeological theories combined with Darwin's own claim that proof for his theory could be found within the deep ocean. Once naturalists determined that both life and sedimentation occurred on the deep-sea floor, naturalists gravitated towards that geographical location to determine the validity of natural selection.

As explored in other chapters of this dissertation, the practices used to retrieve evidence from the deep-sea floor also influenced their interpretation. British naturalists had pulled specimens from the deep seabed as early as 1818. Sir John Ross, a naturalist famous for his exploration of the arctic regions, had sounded a depth with a "deep sea clamm," a device invented to retrieve samples from the deep ocean. The device was deployed around Baffin Bay and Ross believed it brought up invertebrates

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[322] Deacon, *Scientists and the Sea*, 306-307 and Schlee, *History of Oceanography,* 90-97.
[323] See chapter one.

from more than 1,000 fathoms deep.[324] These specimens might have challenged Edward Forbes' azoic zone theory when it was developed. However, they remained largely ignored until American deep-sea practices spread to British scientific networks. Ross' contemporaries were less than impressed with his interest in deep-sea mud; Edward Sabine, one supernumerary stationed on the voyage wrote back to his brother "half ashamed of [him]self for laughing at [Ross'] stupidity in collecting mud, and packing it in pickle jars, and in glass tubes hermetically sealed, and in conceiving that he [was] doing Sir Joseph Banks great service in supplying him with it."[325] Naturalists did not employ techniques that did not yield, in their opinion, valuable scientific evidence.

One of Ross' midshipman was nonetheless significantly moved by the deep-sea research done during the voyage; James Clark Ross, Sir John's nephew, had accompanied the arctic expedition and conceived an interest in sampling the deep sea upon seeing the retrieval of samples from the deep ocean. James Ross would later urge a group of naturalists under his lead to dredge for deep-sea life when in command of his own expedition, as explored later in this chapter. However, other naturalists largely ignored the specimens from James Ross' voyage, much as they had his uncle's deep-sea research.

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[324] See Rehbock, "Early Dredgers," 352, Deacon, *Scientists and the Sea*, 281, and Schlee, *History of Oceanography*, 85. Primary source language describing Sir John Ross' deep sea works can be found in A Rice, "The oceanography of John Ross' Arctic Expedition of 1818; a reappraisal," *Journal of the Society for the Bibliography of Natural History* 7 (1975): 303. The shorter quote is as follows, "Soundings were obtained correctly in one thousand fathoms, consisting of soft mud, in which there were worms; and, entangled on the sounding line, at the depth of eight hundred fathoms, was found a beautiful *caput* medusae [a starfish]; these were carefully preserved, and will be found described in the Appendix." Rice argues that the line had obviously been drawn along the sea floor and covered in mud, calling into question the accuracy of the Ross' sounding measurements. Combined with a "carelessness" in reporting his data, this caused naturalists to dismiss his supposed discovery. I, for the most part, agree; the issue would also not become of great interest until Brooke's sounding line solved the practical problem of verifying sounding depths and the samples retrieved from deep sea devices (see chapter three).

[325] Rice, "John Ross' Arctic Expedition," 296.

The production of specimens was not enough to challenge the scientific and cultural belief that the deep sea was a primordial, lifeless “desert under the sea.”[326] The British half of the Anglo-American biotic debate must be seen within the context of shifting and competing scientific practices. On one hand, naturalists doubted the practices used to gather deep-sea specimens. The technological insecurity faced by deep-sea sounding, such as not knowing if the lead had actually touched bottom, transferred to the collection of living specimens. The only way to secure these specimens was through scientific instruments that disappeared under the waves and returned with mysterious creatures. On the other hand, the production of deep-sea sediment samples was not seen as particularly valuable by many British naturalists despite their interest in the historical sea floor. Scientific networks had differing values for specimens, including deep-sea “mud.” The value of these sediment specimens would change for the British naturalists as the century progressed and the biotic debate spread across the Atlantic.

In 1853, American naturalists offered the first serious challenge to Forbes' conviction that no life existed below 300 fathoms, using the new sounding line invented by naval midshipman John Mercer Brooke. The outbreak of the United States Civil War only aided the spread of American surveying practices to England, making it possible for British naturalists to conduct their own studies of the deep-sea floor without competition from the war-weary American naturalists. As American naturalists traveled across the Atlantic in the 1860s, they circulated Brooke's sediment specimens along with his method of obtaining deep-sea samples. The first specimens retrieved came from the North Atlantic seabed. They contained tiny microscopic shells. Brooke's discovery

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[326] See Alain Corbin, *The Lure of the Sea: The discovery of the seaside* 1750-1840 tran. Jocelyn Phelps (London: Penguin Books, 1994), 1-10 for an excellent account of the cultural context that led naturalists to view the deep sea as primordial.

challenged the belief that there was no sedimentation in the deep sea since these microscopic organisms could have fallen from the ocean surface onto the sea floor. Other naturalists wondered whether the microorganisms might live on the sea floor itself, as the shells seemed too delicate to survive the long descent into the abyss. The dispute over whether these shells lived on the bottom of the sea was enough to throw doubt onto Forbes' azoic zone theory, and along with it, the question of fossil evidence for Darwin’s theory of evolution.

Louis Francois de Pourtalès, a naturalist working for the United States Coast Survey, began a sustained study of the deep-sea sediments procured from his agency's surveying practices. He inherited his specimens from the Coast Survey's early sounding efforts. These samples were from relatively shallow areas. However, the routine sounding required for surveying the United States coastline amassed a sizable collection of sediment samples. Pourtalès was also able to dedicate professional time to the study of these samples. Upon the death of Jacob Bailey, one of the leading microscopists at the time, many American naturalists began to send their sediment samples to Pourtalès instead. Pourtalès was understandably interested when Brooke's device managed to bring up sediment samples from the deep sea.[327]

Pourtalès followed the biotic debate with great interest and became one of its most prolific authors. His own examination of deep-sea sediment samples led him to believe that life did exist in the abyssal regions. He began to publish his conclusions in American journals. His position within the Coast Survey and his extended study of the marine sediments gave his publications some evidentiary authority. Many naturalists did not have access to marine sediment samples at all. Deep-sea sediment specimens were

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[327] See chapter three.

even rarer as only a select few American naturalists were able to procure them during the mid-nineteenth century. However, the United States Civil War interrupted American contributions to the biotic debate in 1861.

The story of British involvement in the biotic debate before 1861 demonstrates the effect of deep-sea practices upon the way scientific samples were examined; naturalists began to interpret seabed evidence differently as American practices spread. British naturalists did not confine their use of these practices to the biotic debate. Rather, they used these practices to produce evidence for evolution, thereby combining the biotic and evolutionary debates.

Darwin's Scientific Circle and Seabed Biology

Many of Darwin's scientific associates were well-versed in the biotic debate from a very early stage. This section places Darwin's emerging scientific circles in the context of the biotic debate and demonstrates that Darwin's associates were some of the most proficient deep-sea naturalists in Great Britain in the middle decades of the century. To these ends, this study will focus on Darwin's two closest scientific confidants, Joseph Hooker and Thomas Henry Huxley.

Joseph Hooker was born into a family of distinguished botanists, including William Jackson Hooker, the director of the Kew Botanical Gardens. James Clark Ross had promised the young Joseph a post aboard an expedition as a favor to William Hooker. Joseph, recently appointed as the young surgeon's assistant, had asked to be elevated to ship's naturalist above Robert McCormick, the expedition's senior medical officer.[328] Such a request might have been particularly sensitive to McCormick, who had his position informally challenged only a few years before while aboard the *Beagle*.[329] However, Ross and McCormick responded diplomatically and gave Hooker the tentative title of expedition “botanist.” McCormick also graciously introduced Hooker to Charles Darwin while strolling in Leicester.[330] Hooker's father had lent him a draft of Darwin's *Voyages of the Beagle* a few years before. Hooker admired the scientific adventures had by Darwin while traveling around the world. He desired to follow a similar path to a scientific career by accompanying the Ross expedition as naturalist.

Hooker's scientific development was accompanied by his introduction to Darwin's seabed science. He carried a copy of Darwin's *Beagle* narrative on his voyage aboard the HMS *Erebus*. While at sea, the HMS *Erebus* and its companion ship, the HMS *Terror,* visited St. Jago, a site Darwin had mentioned in *Voyages*. Hooker took the time to notice “the ancient sea beach of shell sand, resting upon one layer of lava and covered by another” described by Mr. Darwin.[331] In some sense, Hooker followed in Darwin's footsteps while on his own expedition; this would not be the only time that Hooker would visit the sites mentioned in Darwin's travel narrative. The ancient sea floor

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[328] See Iain McCalman, *Darwin's Armada: Four Voyages and the Battle for the Theory of Evolution* (New York and London: WW Norton & Co., 2009), 85-105, “The Puppet of Natural Selection,” for a readable account of Joseph Hooker and the Ross Expedition.

[329] Robert McCormick trained at a hospital, but also took classes at the University of Edinburgh. Despite this shared institutional link, Darwin noted the methodological differences between his and McCormick's scientific observations. I also find it fascinating that McCormick did not properly preserve and examine the valuable dredging specimens taken while on voyage. These two facts clearly demonstrate that simply attending the University of Edinburgh to study natural history did not confer evidentiary practices enjoyed by the elite naturalists such as Darwin, Jameson, and Grant (chapter 1 and 2). Joseph Dalton Hooker was the son of the Regius Chair of Botany at Glasgow University. He read proofs of Darwin's *Beagle* voyage while aboard the *Erebus*. After being denied a professorship at the University of Edinburgh, he assumed the directorship of the Geological Survey of Great Britain and then the directorship of the Kew Gardens in succession to his father.

[330] McCalman, *Armada*, 102.

[331] Joseph Dalton Hooker, Antarctic Journal, 18 May 1839-28 March 1843, typescript copy JDH/1/1, archives, Royal Botanical Gardens, Kew, 26. See also McCalman, *Armada*, 106.

risen above sea level was noteworthy, especially because Hooker would later observe the dredging of the sea floor along the expedition. Hooker's initiation to seabed science, marine invertebrate zoology, and deep-sea sedimentation was learned in the context of Darwin's 1831-1836 *Beagle* explorations.

As mentioned, Captain James Clark Ross had investigated marine zoology, especially that of the sea floor. This interest had grown since his uncle Sir John Ross supposedly pulled up creatures from the sea floor using a deep-sea device. James Ross urged his officers to dredge the deep-sea floor while commanding the expeditions of the *Erebus* and *Terror* in Antarctica. Ross took the young Hooker under his tutelage and spent many hours with him on the subject. The long weeks at sea around Antarctica afforded little time for botanical practice once his specimens had been sketched and preserved. Seventy years after the voyage, Hooker recalled his marine zoological activities and the unique opportunity afforded to him by serving under Ross: "...I was the sole worker of the tow-net, bringing the captures daily to Ross, and helping him with their preservation, as well as drawing a great number of them for him."[332] He also noted in a letter to his father:

> No other vessel or collector can ever enjoy the opportunities of constant sounding and dredging and the use of the Towing-net that we do, nor is it probable that any future collector will have a Captain so devoted to the cause of Marine zoology, and so constantly on the alert to snatch the most trifling opportunities of adding to the collection, and lastly, it is my only means of improving the expedition much to my own advantage (as far as fame goes) or to the public, for whom I am bound to use my best endeavours.[333]

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[332] Leonard Huxley, *Life and Letters of Sir Joseph Dalton Hooker* (London: John Murray, 1918), 47.

[333] Huxley, *Life and Letters Hooker*, 113.

Hooker's marine zoology research would have to rise to the scientific challenge presented by other nations on the subject, most notably their rival American naturalists. Hooker would also recall in a letter to his father that other naturalists were pleased with his marine zoology drawings, “of which many are entirely new; I must, however, redouble my efforts on that head, little as I care about them, as I hear that the Americans have done much during their voyage to them, and that... is the only thing they have done.”[334] Hooker had the opportunity to add to the voyage's fame by discovering new marine invertebrates hitherto unknown to the American expeditions. British mastery over the seas was at stake since American marine zoological studies had become more advanced under the patronage of the United States government.[335]

Hooker was also well-positioned to contribute to the early biotic debate on his voyage.[336] His voyage was made more exciting by his discovery that a strange oceanic glow, attributed to electricity, was actually the product of microscopic animals at the water's surface, *Entomostraca crustacea*. He also found microscopic life at 400 fathoms depth and assisted Ross in his routine deep-sea soundings and marine zoological studies.[337] Historian James Endersby has noted that Hooker's marine zoology work aboard the *Erebus* functioned as a way to hone his drawing techniques.[338] Hooker mentions as much in a letter to his father in 1840:

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[334] Huxley, Life and Letters Hooker, 122.

[335] See chapters one and three of this dissertation.

[336] McCalman, *Armada*, 113-114, original quote from Huxley, *Life and Letters Hooker*, 116-117. Joseph's family, especially his father, had encouraged an interest in botany. However, much of Joseph's early expedition time was spent studying marine zoology. While some narratives have treated this time as a distraction, the importance of marine zoology and the biotic debate cannot be stressed enough as the context for Hooker's interest.

[337] Among other things, Ross' own account of the Antarctic pack includes a reference to Ehrenberg's microscopic examination of geological formations. See p. 146 of Ross' expedition report. Also see Huxley, *Life and Letters Hooker*, 55.

[338] Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago and London: University of Chicago Press, 2008), 130.

> Since leaving St. Helena, my time has been employed exactly as before; the net constantly overboard, and catching enough to keep me three-quarters of the day employed drawing; the dissections of the little marine animals generally take some time, as they are almost universally microscopic. Though I never intend to make anything but Botany a study, I do not think I can do better than I am doing; it gives me a facility in drawing which I feel comes much much [sic] easier to me; it pleases the Captain beyond anything to see me work, and further, it is a new field which none but an artist can prosecute at sea..."[339]

As seen from this passage, Hooker presented his acquisition of greater skills in drawing as something of an excuse to his father to justify the enormous amount of time he spent learning marine biology. Hooker honed many scientific practices through his interest in marine invertebrate zoology.

Hooker's microscopical studies also led him to study deep-sea sedimentation. He recorded the study in his journal in February of 1841: "Much young ice was seen to-day of a light brown colour; when dissolved in water it deposited a very fine sediment, composed of exceedingly minute, transparent, flat quadrangular flakes..." Although Hooker would attribute this to ash from Mount Erebus in publication, he noted, "I recognised them as diatoms, &c., at the time. J.D. Hooker.," in his own copy of the voyage.[340] Hooker discovered that the Antarctic sediment consisted of diatoms, a marked discovery in the biotic debate.

Hooker also tied the pursuit of the philosophical naturalist to seabed science. It was aboard the *Erebus* that Hooker also began his first attempts at "philosophical botany." As noted by historian Iain McCalman, Hooker began to move beyond taxonomic concerns to explore "larger, law-like patterns, such as those governed the origins,

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[339] Huxley, *Life and Letters Hooker*, 57.
[340] Huxley, *Life and Letters Hooker*, 58.

geographical dispersals and adaptations of plant types."[341] This philosophical botany was also entwined with the study of marine invertebrates learned upon the *Erebus* under Ross' tutelage. The two dredged the deep-sea floor in search of more organisms, finding complex creatures such as a "deep sea *Pycnogon* which [they] dredged up in the *Erebus*, especially the *Amnothea comunis*, which astonished the crew. It is much desired that zoologists would follow the example of most botanists in giving the geographical range of the species they deal with."[342] Hooker's philosophical researches employed marine invertebrates and fossil evidence to determine past patterns of living forms, much as Darwin had done. This evidence would be used to determine the history of ancient sea floors and their elevations over time.

Ross noted Hooker's discovery of life in areas below 300 fathoms in his voyage narrative. Hooker also contributed an introduction to the voyage's 1844-1875 publication, the *Zoology of the Erebus and Terror*, but failed to mention his marine zoology discoveries.[343] Instead, he sent his diatom samples to Professor Ehrenberg of Berlin for examination and publication. The expedition's other marine zoological samples were not well-preserved and did not survive the voyage. Naturalists also questioned the techniques used to retrieve both sets of specimens.[344] Forbes' azoic zone theory remained in place despite Ross' deep-sea specimens. Forbes' practices and authority regarding the distribution of marine life dominated the observations made by Ross. This would not be the only time that British deep-sea specimens were dismissed during the biotic debate due to differing scientific practices when offered against the azoic zone theory.

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[341] McCalman, *Armada*, 123.

[342] Huxley, *Life and Letters Hooker*, 57.

[343] John Richardson, *The Zoology of the Voyage of the HMS* Erebus *and* Terror (London: EW Janson, 1844).

[344] Schlee, *History of Oceanography*, 86.

The connection between marine zoology, seabed science, and the biotic debate created a context of shared marine biology experiences between Hooker and Darwin. The two naturalists also shared a common history of sailing around the world to practice philosophical biology. When Hooker returned to England in September of 1843, he was met by a number of botanical specimens that naturalists desired to have identified and described. Darwin's own South American botanical samples were among them. Darwin would seek out Hooker in person a month later and their meeting would begin a lifelong friendship and scientific collaboration.[345]

Four years later, Lieutenant Joseph Dayman, a crew member previously assigned to the *Erebus*, would see another young assistant surgeon deploy the tow net from his ship's deck.[346] This time the ship was the HMS *Rattlesnake* and the aspiring naturalist was Thomas Henry Huxley, the man who would later be known as Darwin's most vocal supporter. Huxley was born in the outskirts of London to a struggling instructor at a local private school.[347] Unlike Darwin, who was born into the wealthy professional class, or Hooker, who could rely upon his family's scientific reputation, Huxley had little social advantage when making his name as a naturalist. He did not have the early education enjoyed by his fellow, aspiring naturalists who ventured out to sea to build their scientific reputations.

The shrewd Huxley was aware of his disadvantages. Luckily, he also had a number of intellectual assets. He was a keen observer and had an engineer's eye for understanding how things operated. This skill made him a deft physiologist. When he joined the navy as assistant surgeon, Huxley also recognized that he had an opportunity.

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[345] McCalman, *Armada*, 148.
[346] McCalman, *Armada*, 163.
[347] McCalman, *Armada*, 155.

He began his notebook with the following observation, "what I can do and they cannot is: I can observe 1. the "habits" of living bodies, 2. their mode of development and generation, 3. their anatomy by dissection of fresh specimens, 4. their histology by microscopic observation."[348] Like his predecessors, he would observe the delicate bodies of marine invertebrates, but he would have access to them as living specimens while aboard the *Rattlesnake*.

Huxley's use of the dredge and tow net was reminiscent of the practices employed by Hooker and Darwin on earlier voyages. Yet, Huxley did not yet have access to a mentor, the way that Darwin had Grant and Hooker had Ross.[349] Captain Stanley of the *Rattlesnake* had scientific inclinations and professed support for natural history. However, he was no Ross. The best Stanley offered was a series of introductions to the scientific elite. These introductions were of incalculable value to a naturalist as yet unconnected to prominent scientific networks. Huxley was introduced to Richard Owen, the famous comparative anatomist, John Edward Gray, a worker at the British Museum who was analyzing the *Erebus* zoological collections; and – most importantly – to Edward Forbes, the Edinburgh student of marine invertebrates and director of the British Geological Survey.[350]

Huxley immediately took to the charismatic and slightly irreverent Forbes. He would also be taught the scientific practices most important to Forbes' network, dredging and the tow-net. Forbes demonstrated the finer points of dredging before Huxley left

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[348] McCalman, *Armada*, 160.

[349] See chapter two of this dissertation for the relationship between Grant and Darwin at the University of Edinburgh.

[350] Adrian Desmond, *Huxley: From Devil's Disciple to Evolution's High Priest* (Reading: Addison-Wesley, 1997), 42. See chapter one of this dissertation regarding Edward Forbes and the University of Edinburgh scientific network.

aboard the *Rattlesnake*.[351] These devices would draw the fresh marine invertebrates that Huxley desired from the ocean. Forbes would even give him his first mystery, an enigmatic *Amphioxus*, a burrowing seafloor invertebrate that had a vertebra-like nerve chord running along its back, but no heart. Forbes introduced his young apprentice to the techniques and specimens needed to examine the world as a philosophical naturalist. Huxley later visited Forbes' center of dredging activity, the British Association for the Advancement of Science, during a stay at port while as the *Rattlesnake* was being refitted.[352]

Forbes' introduction to marine zoology led Huxley to his first scientific discoveries. The dredge and tow-net provided him with marine creatures to study while at sea. Those specimens launched Huxley's reputation as an upcoming naturalist. Huxley would study his fresh prizes each day.[353] Of special interest were worms, sea-squirts, and the jellyfish so esteemed by Forbes. Huxley caught his first *Physalia*, the stinging, poisonous man-of-war, while still in the Atlantic. The organism was beginning to liquefy before morning, so he had to be quick-witted and focused in his observations. Huxley managed to analyze the creature quickly and, therefore, correct previous, “horridly superficial” observations made of the man-of-war.

The simple marine invertebrates that Huxley examined raised questions about organismal complexity, in the vein of Forbes' zoogeological thinking. He began to wonder at the creatures under his microscope. Were the small parts of the organism their own individual creatures come together in one larger form, or were they nearly-discrete parts of one organismal whole? Huxley found that some organisms changed

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[351] Desmond, *Huxley*, 59.

[352] Desmond,, *Huxley*, 45.

[353] Desmond, *Huxley*, 56-57.

over their reproductive lives from one form into another, as well. Were these the same creatures? Each “stage” of the organism did not even share morphological similarity to the other stages. Huxley's marine organisms began to unlock novel thoughts about complexity, morphology, and physiology.[354] He sent off his *Physalia* paper to England in hopes of it making its debut in the Linnean Society.

Huxley's series of *Rattlesnake* papers drew attention and praise back in England. Forbes acted as a friend and mentor for the budding philosophical naturalist. Huxley kept a correspondence with Forbes while at sea and he kept the older naturalist apprised of his thoughts on the affinities between marine invertebrate species.[355] Forbes, in turn, returned news of the reception of Huxley's papers back in England.[356] As Huxley's voyage progressed, the news from Forbes became more and more encouraging.[357] Forbes paved the way for Huxley to become a man of science.[358]

Just as he had with Joseph Hooker, Lieutenant Dayman watched the young assistant surgeon Thomas Henry Huxley forge a name in science while he sounded and charted the waters. However, though both Hooker and Huxley would both sift regularly through the tow net spoils, Huxley focused almost exclusively on the marine invertebrates he found on the voyage. When his naturalist companions collected birds and sprigs, Huxley labored over his microscope and marine zoology.[359] In a strange way, Hooker and Huxley's interests and experiences complemented each other; the two

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[354] Desmond, *Huxley*, 60, 83, 126.
[355] Desmond, *Huxley*, 69, 73.
[356] Desmond, *Huxley*, 81.
[357] Desmond, Huxley, 123.
[358] Desmond, *Huxley*, 152-154.
[359] Desmond, *Huxley*, 98. Huxley's early dedication to marine invertebrate science and dredging was quite noteworthy; consider that Huxley would even dredge with his beloved wife while on their honeymoon.

even became close friends upon Huxley's return.[360] However, Huxley remained dedicated to deep-sea studies as the years passed.

In 1857, when Dayman was reassigned to the *Cyclops*, a frigate sent to explore the routes for the transatlantic telegraph cable, it was Huxley who instructed the lieutenant on sea floor dredging.[361] Dayman's post and Huxley's tutelage yielded its own unique scientific opportunity. While exploring the deep-sea floor on his expedition, Dayman pulled up his own sample of sediment from far beneath the waves. As explored in chapter three, such sediment samples were relatively rare and, therefore, scientifically valuable. Dayman modified Brooke's deep-sea sounding device, giving it a conical head. He then used this modified version of the sounding line to retrieve a deep-sea sediment sample for Huxley, who had advised him to preserve "the freshly brought up soundings in a tolerably strong alcoholic mixture, so that the presence or absence of soft parts in them might be determined at any future time, and under more convenient circumstances."[362] Dayman did exactly as Huxley instructed and sent a sample of the deep sea sediment preserved in "spirit of wine [alcohol]."

Huxley wrote an appendix to Dayman's report regarding the sediment samples. The analysis was short and preliminary. Like Hooker, Pourtalès, and Ehrenberg before him, he had observed a large number of foraminifera, but was unable to determine whether they resided upon the sea floor or had floated down from the ocean surface. He was, nonetheless, inclined to believe that the microscopic animals were bottom-dwellers.[363] Huxley shelved the samples for examination at a later date; Darwin had

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[360] Desmond, *Huxley*, 167.

[361] Desmond, *Huxley*, 237.

[362] Deacon, *Scientists and the Sea*, 296. The quote can be found in Philip F. Rehbock, "Huxley, Haeckel, and the Oceanographers: The Case of *Bathybius haeckelii*" *Isis* 66 (1975): 511.

[363] Rehbock, "*Bathybius*," 511-512.

called a number of his scientific circle together to discuss his thoughts on natural selection in 1856, which consumed Huxley's efforts.[364] Nonetheless, the biotic debate was fresh in Huxley's mind – as well as Hooker's – during the formative years when they assisted Darwin with his theory on the origin of species.

Darwin and the Biotic Debate

It was in this context of debate over deep-sea sedimentation that Darwin published his first edition of *On the Origin of Species*. Darwin had incorporated seabed science into his explanation for evolution.[365] Darwin also contributed to discussions regarding the biotic debate as well. While the ultimate goal of Darwin's "species book" was not to weigh in on the biotic debate, he did express his beliefs on the subject. His biotic debate arguments incorporated both zoological and fossil evidence. Darwin had used some of that same evidence when reasoning through the lack evidence for divergence in the fossil record. He wrote in 1859, regarding the lack of fossilized organisms showing slow transition from one species into another;

> But, as by this theory [descent with modification] innumerable transitional forms must have existed, why do we not find them embedded in countless numbers in the crust of the earth?.. I will here only state that I believe the answer mainly lies in the record being incomparably less perfect than is generally supposed; the imperfection of the record being chiefly due to organic beings not inhabiting profound depths of the sea, and to their remains being embedded and preserved to a future age only in masses of sediment sufficiently thick and extensive to withstand an enormous amount of future degradation...[366]

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[364] Huxley sent a letter to Darwin about Dayman's discovery on 14 April 1860.

[365] See chapter two of this dissertation.

[366] Darwin, *Origin of Species*, 172.

In essence, Darwin argued that fossilized remains of organisms would be created only at rare moments of gradual seabed rise.[367] The infrequency of gentle, shallow seabed upheaval caused the fossil record to be incomplete enough to demonstrate intermediate forms. The shallow seabed would stop rising at some point and new fossils would cease to be created. Naturalists would not have access to the remains of those forms that came before the new species, nor those that came after the period of seabed upheaval. Therefore, few transitional forms would be observed in the geological record. Only a selection of the species would be observed, giving the appearance of morphological distinction rather than indefinite transitional forms of the slowly diverging species. Darwin dedicated an entire chapter, "On the Imperfection of the Geological Record," to this problem in *On the Origin of Species*.

Darwin took up the issue of life in the deep sea and sedimentation again in that chapter. Here he also made his thoughts known on the emerging biotic debate. He deployed the biotic debate to prove his lack of paleontological evidence of intermediate forms. His perspective was couched in the zoogeographical research of his fellow Edinburgh naturalist and correspondent, Edward Forbes. Darwin invoked Forbes' study of seabed dredging to explain the lack of fossil evidence for intermediate forms:

> That our palaeontological collections are very imperfect, is admitted by every one. The remark of that admirable palaeontologist, the late Edward Forbes, should not be forgotten, namely, that numbers of our fossil species are known and named from single and often broken specimens, or from a few specimens collected on some on spot.[368]

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[367] See chapter two for a more complete description of Darwin's argument in relation to transitional forms.

[368] Darwin, *Origin of Species*, 287.

Darwin also deployed new zoogeological evidence in service of the biotic debate. His arguments about life in the deep sea followed Forbes' own speculations. In addition to supporting Forbes' conclusion that no life existed within the depths of the ocean, he also adopted prevalent beliefs about sedimentation on the deep-sea floor derived from Forbes' work:

> Shells and bone will decay and disappear when left on the bottom of the sea, where sediment is not accumulating. I believe we are continually taking a most erroneous view, when we tacitly admit to ourselves that sediment is being deposited over nearly the whole bed of the sea, at a rate sufficiently quick to embed and preserve fossil remains. Throughout an enormously large proportion of the ocean, the bright blue tint of the water bespeaks its purity.[369]

Darwin deployed his experience aboard the HMS *Beagle* to claim personal observation of the sea surface's relationship to the sea floor, namely that sedimentation did not happen to a great extent from the sea surface. The purity of the water at the sea surface was intended to demonstrate that sediment-forming microorganisms could not be present in great numbers at the sea surface to fall upon the deep seabed. Sedimentation would occur close to shore and in select places in the deep ocean, but the tiny shells needed to create significant sedimentation – enough to preserve fossil forms – were not present in the open ocean.[370]

Darwin's knowledge of deep-sea biology, through Forbes' publications, worked in his favor in *On the Origin of Species*. Most explicitly, his familiarity with the subject allowed him to circumvent the need for fossil evidence for his theory within the

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[369] Darwin, *Origin of Species*, 288.

[370] It is also possible that Darwin was implicitly suggesting that sedimentary creatures could live upon the sea floor instead of falling from the clear ocean surface, thereby aligning himself with Hooker and Huxley. However, his conclusion rested firmly upon a belief that sediment did not accumulate uniformly across the entire sea floor.

geological record. He capitalized on Edinburgh zoogeological practices to derive his claim. Darwin understood one of Forbes' primary principles on the origin of species, that the appearance of a species could be tracked to one – and only one – point in geographical space and time. A species was “created” only once in the geological record. A species would then radiate outward from the location of its first appearance. Forbes called this the principle of “specific centres.”[371]

Forbes had been unable to comment on the emerging sedimentation debate because he died in 1854, only two years after naturalists first examined sediment procured from Brooke's sounding line. Darwin extrapolated from Forbes' work in light of the international deep-sea sedimentation debate in *On the Origin of Species.* If Forbes' azoic zone theory was correct, then it would be perfectly understandable for naturalists to not see fossilized forms of deep-sea creatures in the geological record. Darwin reasoned that fossil evidence from intermediate depths, where Forbes' claimed marine life existed, could inform naturalists about seabed upheaval and subsidence. Sedimentation would not capture the same species consistently because fossilization occurred intermittently, only during periods of gradual sea floor subsidence. When a fossil was created, it captured an organism somewhere along its radiation away from its specific center. Using that logic, an lineage of organisms would not appear to change gradually because sedimentation would capture its ancestors at different times:

> But the imperfection in the geological record mainly results... from the several formations being separated from each other by wide intervals of time. When we see the formations tabulated in written works, or when we follow them in nature, it is difficult to avoid believing that they are closely consecutive.[372]

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[371] See chapter two for more on specific centers.

[372] Darwin, *Origin of Species*, 289.

Gradual morphological change would make it seem as though marine organisms were distinct, thereby erasing any proof of transitional forms. Darwin used the same evidence to explain his views on the biotic debate that he had used to explain his lack of evidence for natural selection; his speculations were equally as important for the biotic debate as they were for biology.

At a deeper level, probably completely unknown to Darwin at the time, he also used Edinburgh evidentiary practices to circumvent a strict use of “inductive” logic. His familiarity with the naturalists' dredge allowed him to explain why a close observation of the fossil record would not yield adequate truth claims.[373] As Forbes himself had said, naturalists were usually only able to dredge up broken specimens and, even when complete, the fossils were often collected from one geographical location. Darwin's zoogeological methods yielded explanatory power completely beyond the calculations, minute observations, and tabulation common to Cambridge seabed practices. The fossil evidence was not complete because of the way that marine fossils were retrieved by the dredge. Darwin deployed his zoogeological evidence alongside calculations to demonstrate his proficiency with both methods.[374] He applied the same blended methods to the biotic debate as he had to divergence.

Darwin relied upon Forbes' conception of the deep-sea floor when reasoning that the geological record did not yield evidence in support of natural selection. Two

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[373] According to rules of scientific evidence that rejected *a priori* assumption, Darwin's claims about sedimentation and biology might seem like circular reasoning. His evidence against sedimentation in the deep sea relied upon an absence of zoological fossils. In turn, his argument regarding the lack of zoological fossils in the deep sea relied, in part, on a lack of sedimentation. Darwin relied on his argument against sedimentation more when Forbes' azoic line was pushed deeper into the ocean. However, this combination of zoological and geological thought was quite common in Edinburgh zoogeology; the same evidence for the biotic debate would have direct bearing on zoological reasoning.

[374] Chapter two of this dissertation.

geographical locations continually emerged as sites for the formation of evidentiary marine fossils, the deep sea and the intermediate depths:

> Such thick and extensive accumulations of sediment [as to resist the destructive power of tidal action] may be formed in two ways; either, in profound depths of the sea, in which case, judging from the researches of E. Forbes, we may conclude that the bottom will be inhabited by extremely few animals, and the mass when upraised will give a most imperfect record of the forms of life which then existed; or, sediment may be accumulated to any thickness and extent over a shallow bottom, if it continue slowly to subside. In this latter case, as long as the rate of subsidence and supply of sediment nearly balance each other, the sea will remain shallow and favourable for life, and thus a fossiliferous formation thick enough, when upraised, to resist any amount of degradation, may be formed.[375]

Darwin's use of the intermediate depths as a site for evidence is covered in chapter two of this dissertation. The second site Darwin identified as being critical for his production of evidence, the deep sea, also used Forbes' zoogeological reasoning. When combined with the rarity of deep-sea sedimentation, the azoic zone theory would remove all fossil evidence – which could prove both divergence and deep-sea sedimentation – from the geological record.

An incomplete fossil record, as advocated by other naturalists, would only give isolated glimpses at the progression of a species over time. Darwin acknowledged his indebtedness for this new way of seeing the fossil evidence in *On the Origin of Species:* “Thus the geological record will almost necessarily be rendered intermittent. I feel much confidence in the truth of these views, for they are in strict accordance with the general principles inculcated by Sir C. Lyell; and E. Forbes independently arrived at a similar

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[375] Darwin, *Origin of Species*, 290-291.

conclusion."[376] The two esteemed naturalists mentioned by Darwin here, Lyell and Forbes, were the same two naturalists to whom Darwin was most willing to leave his sketch of natural selection in the instance of his unexpected death.[377] Darwin's use of the ancient seabed and marine invertebrate fossils tapped into a decades-long debate on the history of life by using its most prevalent evidentiary objects. This argument would prove compelling for many naturalists so long as Forbes' azoic zone theory remained unchallenged.

However, as demonstrated in chapter three, American deep-sea sediment studies challenged the azoic zone theory shortly before the onset of the United States Civil War. Maury, now the former superintendent of the US Naval Observatory, defected to the Confederacy and was sent to England. He was a well-respected, international naturalist, which is one reason he was so valuable to the Confederacy as a foreign agent. His prolonged and active stay in England would have given opportunity for his scientific views to spread, including his views on the biotic debate, a subject of special interest for him. Maury had prompted the first deep-sea sounding expeditions that deployed Brooke's sounding line. As a consequence of the expedition, Maury firmly believed that the sediment brought up by Brooke's sounding device was the result of microorganisms living at the ocean surface that drifted to the deep-sea floor when they died. He maintained that the deep-sea environment was devoid of life.

Brooke's deep-sea specimens provided evidence that sedimentation occurred. Such a view would challenge Darwin's claim that sedimentation did not occur to any great extent on the deep-sea floor, one of the two crucial sites that could produce

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[376] Darwin, *Origin of Species*, 292. Also note the blending of Forbes' zoogeological methods with the Cambridge consilience so important to William Whewell, the "independently arrived at" conclusions.

[377] See chapter two of this dissertation.

evidence of fossilized transitional forms. Darwin recognized that some sedimentation might occur in the deep sea in *On the Origin of Species*. But by his own admission, if two conditions were met he would be no longer able to explain why the deep sea did not provide evidence in support of natural selection: if sedimentation occured in the deep sea *and* life existed far below the azoic zone, then his explanation for gaps in the fossil record would collapse. The early biotic debate settled the first condition for naturalists. American naturalists remained split as to the second condition, the existence of life in the deep sea.

Without immediate access to American deep-sea scientific practices, British naturalists had no way to explore the question themselves. Some American naturalists, such as Pourtalès, disagreed with Maury's analysis of the deep-sea sediments. Pourtalès believed that the sediments indicated that microscopic life could exist in the deep sea and, therefore, larger organisms might be able to reside there as well. However, Darwin, like many others, relied upon the authority of Forbes' analysis of deep-sea biogeographical distribution. The azoic zone remained the dominant theory in the United Kingdom during the early 1860s despite some doubt on the part of a few British naturalists, such as Hooker and Huxley. And so long as the azoic zone theory remained relatively unchallenged, Darwin would see no reason for the deep sea to produce evidence of transitional forms. Even if sedimentation occurred, as Maury advocated, it was impossible to produce fossils without living creatures to trap within the forming rocks.

British naturalists would soon find evidence of organisms within the deep sea. At first, these findings would be dismissed. Naturalists would view the deep-sea specimens with skepticism based on the methods used to pull them from the abyss. Nonetheless, as naturalists began to suspect that life existed within the deep sea, they also began to search for proof of natural selection in the site identified by Darwin as a potential source of evidence.

Wallich and the Starfish

On 5 December 1860, Darwin opened his copy of *The Times* to see an article titled “The North Atlantic Telegraph Expeditions.” The newspaper claimed that the HMSS *Bulldog* and *Fox*, under Sir Leopold McClintock, had returned from a deep-sea surveying expedition along the North Atlantic Seabed. Their objective was to determine “the practicability of the proposed scheme for carrying a line of telegraph from Europe to America...” The *Bulldog* had left at the beginning of July to take a series of soundings along the sea floor. The newspaper then mentioned discoveries made by the expedition's crew. First, some depths on this new route were much shallower than anticipated, as evidenced by the discovery of “a depth of only 748 fathoms... where it was expected to find 2,000.” The reporter went on to claim that the expedition settled the biotic debate:

> The return soundings of Sir. F. L. McClintock were of a peculiarly interesting character in a scientific point of view, inasmuch as they set at rest the long disputed question of the existence of animal life at great depths in the ocean. Several starfish were brought up from the depth of 1,260 fathoms, which had become entangled with the lower portions of the line, which had lain upon the bottom.[378]

The article then casually explained how the instruments used to collect samples from the deep were basically useless in the hands of the crew, “The instruments supplied by the

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[378] “The North Atlantic Telegraph Expedition” *London Times*, December 5, 1860, 5.

Admiralty for the purpose of obtaining specimens of the bottom, according to all account, did not answer the expectations formed of their efficacy...” The crew was able, however, to create their own devices and practices for obtaining samples.

Besides the existence of life in the deep sea, the article also mentioned deep-sea sedimentation and the constitution of the sea floor. It seemed as though the expedition's naturalist made a claim that the surface water had little relationship to the top layer of sediment on the deep-sea floor, a radical departure from Maury's claim in *Physical Geography of the Sea*:

> By these contrivances [the crew's makeshift instruments] not only were the soundings rendered more certain, but very often the understratum of the bottom was found to be composed of entirely different material from that which lay upon the surface, and which is usually brought up by the rod or lead in ordinary deep sea soundings. Indeed, it is not too much to say that until the present soundings were taken by Sir F. L. McClintock the true nature of the bottom of the sea as considerable depths was hardly understood.

The rest of the article related a dramatic account of the voyage's perils and discoveries. The voyage seemed to be a success: according to the interviewed mariners a cable could successfully be placed across the route explored by the *Bulldog* to replace the one previously laid.

Not long before reading the *Times* article, Darwin had also received a parcel from the *Bulldog*'s naturalist, George Charles Wallich.[379] Huxley had trained Wallich on deep-sea marine zoology. Wallich, it seemed, had continued Huxley's research from the last transatlantic cable voyage.[380] The parcel contained a copy of Wallich's latest

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[379] DARC v. 8: 526, Charles Darwin to George Wallich, 12 December 1860.

[380] Huxley had been trained by Forbes, placing Wallich – Huxley's deep sea “student” – firmly within the Edinburgh dredging network. In addition, Wallich, like Darwin and Forbes, had attended the University of Edinburgh as a medical student and gained an interest in the sea floor.

publication. The pamphlet, titled *Notes on the Presence of Animal Life at Vast Depths of the Sea; with Observations on the Nature of the Sea Bed, as Bearing on Submarine Telegraphy*, directly referenced the transatlantic biotic debate; it discussed Forbes' azoic zone theory, the discoveries made by Maury, and Huxley's observations of the deep Atlantic sediment. The ever-present tiny foraminiferous shells that constitute the deep-sea sediment were the main subject of the paper. He restated popular scientific sentiment regarding the deep sea; that no life existed below 500 fathoms and that a universal sedimentation occurred across the ocean floor below 150 fathoms.[381] At its core, naturalists conceived of the deep-sea floor as a geography of “perfect repose,” a concept held by Maury, Darwin, and others before them.[382] It was this conception that Wallich challenged.

Most important for Darwin, Wallich's notes claimed that naturalists were “warranted in assuming that a large class of creatures inhabit the deeper recesses of the sea, and that... [their] knowledge of the conditions under which many fossil forms lived and perished may be materially augmented [in light of Wallich's discoveries].”[383] Wallich claimed that formanifera were able to live at great depths. He had also supposedly retrieved a number of complex organisms from the deep sea. If this was true, Darwin's theorizing about deep-sea fossils – or more appropriately, his lack of deep-sea fossils – to prove natural selection would need to be modified.

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381 George Wallich, *Notes on the Presence of Animal Life at Vast Depths in the Sea; with Observations on the Nature of the Sea Bed, as Bearing on Submarine Telegraphy* (London: Taylor and Francis, 1860), 7.

382 The phrase “perfect repose” is taken from Wallich's *Notes*, though it bears a striking similarity to language used by Maury. See chapter two of this dissertation for more on the deep sea as an eternal, unchanging environment, as espoused by Maury, but challenged by Pourtalès.

383 Wallich, *Notes*, 9.

Wallich's conclusion about the biotic debate rested upon his collection of 13 starfish – and a number of other complex organisms – from deep-sea soundings. He wrote in his report:

> On two occasions living specimens of *Serpula* [plume worms] were obtained. One at moderate depth, the other at 680 fathoms, and in conjunction with a living *Spirorbis* [a small, polychaete worm]... But by far the most interesting discovery remains to be noticed. In sounding... in 1260 fathoms, whilst the sounding apparatus brought up an ample specimen of course, gritty-looking matter, consisting of about 95 per cent. of clean *Globigerina*-shells, a number of Starfishes, belonging to the genus *Ophiocoma*, came up adherent to the lowest 50 fathoms of the deep-sea line employed. This quantity of line had been paid in excess of the depth, which was determined by a separate operation... One very perfect specimen, which had fixed itself close to the extreme end of the line, was still convulsively grasping it with its long spinous arms, was secured *in situ* on the rope, and consigned to immortality in a bottle of spirits.[384]

Most interestingly, he had found the tiny foraminifera in the stomachs of the starfish, leading him to conclude that the starfish and microscopic creatures both lived upon the sea floor. Wallich also addressed the potential reasons why he might have found starfish by error, such as ocean currents carrying the specimens into deep water or mechanical error when collecting the creature, in the rest of the analysis. There were a few other possible explanations for finding starfish tangled on his sounding line than the one he gave, but Wallich faced each potential challenge confidently. Ultimately, he believed that no complex deep-sea creatures had been found previously because nobody had been looking for them and, therefore, had not devised mechanical devices for their retrieval.

Wallich's analysis notably combined both American and British biotic argument; the American research was represented by Maury’s delpyment of Brooke’s sounding line while the British side was represented by Huxley's work on marine invertebrate

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[384] Wallich, *Notes*, 22.

zoogeology. *Notes on the Presence of Animal Life at Vast Depths of the Sea* proclaimed itself as a new starting point in the transatlantic biotic debate. Wallich genuinely believed that he had resolved the biotic debate once and for all.

Some naturalists doubted Wallich's claim. To his credit, Darwin responded to Wallich's letter with praise. He was particularly interested in Wallich's thoughts on the thickness of deep-sea sedimentation, an area important to his evidence for natural selection. In a letter, he probed Wallich for detailed information regarding his discoveries:

> If you would not think me very unreasonable, you would do me a great favour, if you would inform me on one point not noticed in your Notes. In the account given in the Times, it is stated that the Machine or Borer, either often or sometimes penetrated through the Foraminiferous deposit into different underlying matter. This would show that the Foraminiferous deposit was sometimes or often thin; and this is the point on which I am anxious for information. It bears on the decay of the Exuviæ of organisms at the bottom of the sea; & is important for me in relation to some few passages in my Book on the Origin of Species, of which I am now preparing a corrected Edition.[385]

If Wallich's research as reported in *The Times* was correct, then the foraminifera created very thin deposits over the whole sea floor. Darwin had anticipated that sedimentation did not occur with great thickness over the whole ocean. Leaving soft-bodied marine invertebrates exposed to the deep-sea environment, according to Darwin's reasoning, would destroy any morphological structures capable of creating fossils. Wallich, despite finding creatures in the deep sea, might have explained another reason why no fossil evidence of transitional forms was to be found in the deep-sea floor. Wallich replied that

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[385] DARC v.8: 526-527, Charles Darwin to George Wallich, 12 December 1860.

the *Times* had misrepresented him, though he did believe that sedimentation varied in thickness greatly and that the deepest areas seemed to be relatively bare.[386]

Darwin also wrote back positively regarding Wallich's discovery of the starfish. Indeed, subsequent editions of *On the Origin of Species* acknowledged that life probably existed on the deep-sea floor. However, Darwin noted that the number of creatures was still negligible in regards to the creation of fossil evidence:

> ...we may conclude that the bottom will be inhabited by few animals [based on Edward Forbes' research], but it will not be, as we at last know from the telegraphic soundings, barren of life; consequently the mass when upraised will give a most imperfect record of the forms of life which existed during the period of deposition.[387]

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386 DARC v.8: 529, Geogre Wallich to Charles Darwin, 14 December 1860. Specifically, Wallich replies, "I had pointed out whilst at sea that the *surface layer* of the Foraminiferous deposit *sometimes* differed in certain respects from the substratum. The difference being due to the fact that whilst live Globigerinæ occurred in the former, retaining the brownish-yellow color of their Sarcodic contents in the latter they only occurred dead the color of the sarcode being thus rendered dusky at the same time that a larger proportion of decayed animal & vegetable matter contributed to the difference— This statement was twisted into a declaration that 'by these contrivances not only were soundings rendered more certain but *very often* the understratum of the bottom was found to be composed of *entirely* different *material* from that which lay upon the surface, & which was ordinarily brought up by the rod or lead in ordinary deep sea soundings'!... I have no doubt that the Foraminferous as well as all other deep sea deposits vary greatly in thickness- Until soundings & surveys are conducted in a much more systematic and searching manner, it is almost impossible to arrive at anything like positive results... But there is I think quite sufficient evidence to shew that in many portions of the deep sea bed, the deposits are of no great thickness. At times we come across deposits of great thickness (several inches, for that is the limit to which any form of sounding apparatus hitherto employed has been able to penetrate) but almost entirely deficient in either minute calcareous or silicious organic remains. On the other hand we meet with deposits in which the entire mass is composed of nearly pure Globigerina shells, alive & dead, as in the cases I just referred to... As regards the occurrence of bare rock I would observe that at 682 Fathoms between the Farœ Islands & Iceland I obtained a living Serpula, the stout calcareous tube of which was broken - together with several pieces of stone & but a faint trace of mud- From this I cannot help inferring that the bottom was comparatively bare of deposit... I cannot believe that large tracts are bare - but strange to say the sounding in which the Serpula & stones occurred with so little admixture of mud is the deepest we encountered between the points named - the last place in short at which we should a priori have inferred the existence of bare rock or gravel! The interval between this sounding and those on either side of it was very great- Alas I had no voice in determining where to sound."

387 Darwin, *Origin of Species*, 1861 edition, 312.

Darwin remained steadfast that the deep sea would not yield evidence of transitional forms, despite Wallich's discovery. The beginning of Darwin's “Difficulties on Theory” chapter still summarized the deep sea as being nearly devoid of life. Historian Margeret Deacon has pointed to the conservatism of Wallich's claim, which is the most likely reason Darwin hesitated to change his fundamental argument. Wallich upheld that life would still slowly decrease as depth increased, despite having reason to claim that Forbes' azoic line might not exist at all.[388] Wallich concluded his report with the claim that “Although animal life has been detected... at depths far exceeding those hitherto assumed as its boundary..., we are justified in taking for granted,... as we descend from moderate to great depths, that the number of living creatures diminishes, and that a point may be reached at which organic life ceases.”[389] While Wallich challenged the azoic zone theory, he did not dispute the fundamental practices Forbes used to reach his conclusion. He simply pushed the azoic zone deeper into the ocean depths.[390] Pushing Forbes' line deeper did not ultimately challenge Darwin's reasoning about the formation of marine fossils in the deep sea, so his claim remained undisputed.

Wallich and Darwin shared fundamental evidentiary practices regarding deep-sea creatures that caused them to interpret the deep-sea starfish in a similar manner. Wallich's discovery, as other deep-sea research at the time, must be seen as modifications to Forbes' theory and not direct challenges. The eventual disproving of the azoic zone might tempt a historian to view Wallich's discovery with this eventual outcome in mind. However, neither Wallich nor his contemporaries viewed their deep-

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[388] Deacon, *Scientists and the Sea*, 298.

[389] Wallich, *Notes*, 34-35.

[390] Previous dredgings by P.C. Absjørnsen, a Norwegian naturalist, had dredged starfish from somewhere around 200 fathoms and speculated that the azoic line was lower than Forbes had suggested. See Schlee, *History of Oceanography*, 88, for more on these early challenges to Forbes' 300 fathom line.

sea evidence in this light. The resilience of the azoic zone was, instead, a direct result of Forbes' influence among the transatlantic dredging community. It is possible that the likeable Forbes exerted scientific influence even after his death. However, it is most probable that Wallich produced evidence that spoke directly to his Edinburgh network colleagues. And each of them interpreted the evidence with Edinburgh network assumptions about the deep sea in mind.

Darwin, Huxley, and Wallich had all been trained in the same Edinburgh dredging network shared by Forbes. When shown the same evidence, all three Edinburgh network naturalists interpreted the starfish as a modification to Forbes' theory. The historian Susan Schlee noted that Wallich's starfish discovery was largely ignored. Some historians have noted a potential criticism of Wallich’s instrument and practice: that the starfish had been picked up as the line was retrieved in intermediate depths.[391] Wallich was also a difficult personality, which may have increased the skepticism that some naturalists used to examine his evidence. That stated, Wallich's discovery was not universally ignored; his deep-sea starfish certainly influenced naturalists directly connected to his scientific network, such as Darwin and Huxley. As already shown, Darwin refined his argument about the production of deep-sea fossils as a result of Wallich's discovery.

Wallich's report also caused Huxley to reexamine Dayman's deep-sea sediment sample. That action would lead Huxley to an exciting scientific discovery. That same

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[391] Schlee, *History of Oceanography*, 89. I would also add that Wallich changed some fundamental dredging and sounding practices, which he felt were inadequate for deep sea zoology. While potentially yielding novel discoveries, the instrumental changes probably made people nervous. There was already many uncertainties when it came to deep sea sounding and dredging. Completely new instruments would have caused naturalists to challenge the validity of these methods even more. I would further speculate that those naturalists farther removed from the Edinburgh network – and therefore less familiar with the training Wallich received – would have been more nervous by the sudden changes to established methods.

discovery, however, would eventually become the most humiliating moment in Huxley's scientific career.

Huxley and *Bathybius*, the Primordial Ooze

At the beginning of the 1860s, Huxley and his close associates had already been at the forefront of the British biotic debate. Issues of deep-sea sedimentation and the existence of life in the deep sea had already shaped Darwin's theorizing about the origin of species. Both Huxley and Hooker had also been privileged to receive the first deep-sea sediment samples produced by British naturalists. The 1860s were also the crucial years when this tightly-knit group worked tirelessly to promote natural selection. The two subjects of seabed science and natural selection were never far removed from each other. In 1867, Huxley inextricably entwined the British biotic debate with the theory of natural selection.

Wallich's discovery had thrown some doubt as to the inability of creatures to live in the deep-sea environment. Huxley may have doubted his student's findings, but another piece of evidence would be discovered the same year that Wallich presented his deep-sea starfish. A telegraph cable running across the Mediterranean was retrieved for repairs late in 1860. The cable rested over 1,000 fathoms below the sea's surface in some areas.[392] Fleeming Jenkin, a telegraph engineer, found that creatures had encrusted themselves on some sections the deep-sea cable. The cable sections were sent back to the University of Edinburgh, to Professor George Allman, the residing Regius Professor of Natural History, and Alphonse Milne-Edwards, a Parisian deep-sea

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[392] Schlee, *History of Oceanography*, 89.

researcher.[393] Professor Allman claimed that the creatures were residents of the sea floor from over 1,000 fathoms deep. The recovery of creatures encrusted upon the telegraph cable had resolved the final instrumental uncertainty remaining from Brooke's sounding line.

Some naturalists doubted whether their sounding lines had picked up deep-sea zoological samples from the depths they measured.[394] Charles Wyville Thomson, the Chair of Natural History at Belfast, had viewed Wallich's starfish with some skepticism. It was not until the retrieval of the Mediterranean cable that he was able to say that Forbes' azoic line was been pushed into the great depths for creatures more complex than foraminifera:

> Before laying a submarine telegraphic cable its course is carefully surveyed, and no margin of doubt is left as to the real depth. Fishing the cable up is a delicate and difficult operation, and during its progress the depth is checked again and again. The cable lies on the ground throughout its whole length. The animal forms upon which our conclusions are based are not sticking loosely to the cable, under circumstances which might be accounted for by their having been entangled upon it during its passage through the water, but they are moulded upon its outer surface or cemented to it by calcareous or horny excretions, and some of them, such as the corals and bryozoa, from what we know of their history and mode of life, must have become attached to it as minute germs, and have grown to maturity in the position in which they were found. I must therefore regard this observation of Mr. Fleeming Jenkin as having afforded the first absolute proof of the existence of highly-organized animals living at depths of upwards of 1,000 fathoms.[395]

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[393] Deacon, *Scientists and the Sea*, 306. I do not address Milne-Edwards' discoveries in great detail in this chapter. However, his claim against Forbes' azoic zone theory can be found at A. Milne-Edwards, “Observations sur l'Existence de divers Mollusques et Zoophytes a de tres grandes profondeurs dans la Mer Mediterranee.” *Annales des Sciences Naturelles*; quatrieme serie — Zoologie. Tome xv. Paris (1861): 149. It should be noted that British naturalists distrusted the list of creatures that Milne-Edwards said had been found at extreme depths at the time.

[394] Thomson, *Depths of the Sea*, 26-30.

[395] Thomson, *Depths of the Sea*, 29-30.

He claimed the removal of this instrumental uncertainty finally allowed a naturalist to produce valid evidence for life in the deep sea.

Huxley exhumed his old sample of deep-sea sediment in 1867, after Wallich's and Jenkin's discoveries of complex life on the deep-sea floor. When he placed it under a more powerful microscope, he noticed something that had not been visible before. The “lumps of transparent, gelatinous substance,” once examined under great magnification, “exhibits – imbedded in a transparent, colourless, and structureless matrix – granules, coccoliths [disk-shaped bodies found in chalk], and foreign bodies.”[396] He examined the tiny structures in minute detail, noting that the jelly-like substance between the small disks possessed a “granular aspect... such as a layer of protoplasm might assume.”[397]

The majority of the paper consisted of precise microscopical measurements of the Atlantic ooze, and Huxley saved his speculative reasoning until the very end of the report, saying that he intended to keep his statement of facts and his interpretation apart. His conclusion was bold. Huxley believed that he had found nothing less than the primordial ooze, the origin of living “protoplasm” that separated living matter from non-living matter, living at the bottom of the sea.

The discovery of an “Urschleim” had been anticipated by Ernst Haeckel, a German professor of zoology and disciple of Darwin, in an 1868 book titled, *Monographie der Moneren*. The discovery of this primordial ooze fit into current protoplasmic and cellular theories of the time.[398] It also cleverly supported abiogenesis, or the spontaneous generation of living matter from non-living substances, though

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[396] TH Huxley, “On Some Organisms Living at Great Depths in the North Atlantic Ocean” *Quarterly Journal of Microscopical Science* 8 (1868): 205.

[397] Huxley, “Great Depths,” 208.

[398] Rehbock “*Bathybius*” places the discovery of the urschleim into the context of nineteenth-century biological theories.

Huxley stopped just short of making a hard, materialist claim regarding the origin of life.[399] However, Huxley was not shy about connecting the oozes discovery with Haeckel and, therefore, spontaneous generation. He declared the substance as a new living species, the simplest to be found, and named it *Bathybius haeckelii,* in honor of the esteemed German naturalist. That same year, Huxley gave a famous lecture titled "The Physical Basis of Life," where he supported protoplasmic theory and, by association, spontaneous generation.

Huxley's discovery of the primordial ooze generated a frenzy of research on the new organism. *Bathybius* was entered into the *Zoological Record* as a species in 1869, shortly after Huxley's announcement.[400] Henry Bastian, Huxley's protégé and an ardent advocate of spontaneous generation, published a book and a series of supportive papers in the *British Medical Journal*.[401] The German geologist C. W. von Gümbel even validated Huxley's finding by discovering *Bathybius* in his own benthic samples.[402]

By the beginning of the 1870s, many deep-sea zoologists envisioned swaths of *Bathybius* blanketing the deep ocean. The existence of primordial ooze along the deep-sea floor began to change theories related to the creation of marine sediment and, therefore, geology. William Benjamin Carpenter, then vice-president of the Royal Society, found traces of *Bathybius* in the geological record.[403] Naturalists across the Atlantic reinterpreted their geological findings in light of Huxley's primordial ooze. In 1858, a complex layering of limestone and serpentine had been discovered in an ancient

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[399] James Strick, *Sparks of Life: Darwinism and the Victorian Debates over Spontaneous Generation* (Cambridge and London: Harvard University Press), 22, 79.

[400] Donald J. McGraw, "Bye-Bye *Bathybius*: The Rise and Fall of a Marine Myth." *Bios* 45 (1974): 164-171.

[401] Strick, *Sparks*, 68-69.

[402] See Nicolaas A. Rupke. "*Bathybius Haeckelii* and the Psychology of Scientific Discovery," *Stud. Hist. Phil. Sci.* 7 (1976): 53-62 for a list of research that validated Huxley's findings.

[403] Rupke, "*Bathybius*," 54.

Canadian geological formation, the Laurentian limestones. The formation would be reexamined in 1864 by John William Dawson, palaeontologist and the civic leader of Montreal. He contacted Carpenter to explore whether the formation, which dated back to the Precambrian Era, had organic origins. Carpenter confirmed that, indeed, the geological formation was the product of an ancient “dawn animal of Canada,” or *Eozoön canadense*. Other naturalists fell behind the discovery of the *Eozoön* in North America.[404] Darwin was careful to note the *Eozoön* and give his support, stating that "it is impossible to feel any doubt regarding its organic nature," in the fourth edition of *On the Origin of Species*.[405]

Huxley agreed that the *Eozoön* had a basic foraminiferous origin.[406] Given the similarity between the three organisms, the *Foraminifera* living at the bottom of the sea, the *Bathybius* slime currently blanketing the deep-sea floor, and the *Eozoön* layer found in the Precambrian geological past, naturalists quickly pieced together an evolutionary history of the primordial ooze. The *Eozoön* seemed to be the ancient ancestor of Huxley's *Bathybius*. And the *Foraminifera* seemed to have developed from *Bathybius*. Just as Darwin had predicted, evidence for evolution had been found within the deep sea. *Bathybius*' geological record may have not demonstrated the principle of morphological divergence, but these were ancient, oozing creatures and, therefore, not in possession of complex morphological features. And it was a start. Given the primordial nature of the deep-sea floor, who knew what other evidence for Darwin's theory might be found there?

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[404] Rehbock, “*Bathybius*,” 513. See also Charles O’Brien, “Eozoon canadense: ‘The Dawn Animal of Canada,’” *Isis* 61 (1970): 206–223.

[405] Charles Darwin, *The Origin of Species by Charles Darwin: A Variorum Text*, ed. Morse Peckham (Philadelphia: University of Pennsylvania Press, 1959), p. 515.

[406] Rehbock, “*Bathybius*,” 513-514 covers Eozoon and the international debate, including detractors.

The discovery of *Bathybius* was the product of nineteenth-century British and American seabed science; while American technologies opened the deep-sea for investigations, Huxley had interpreted the specimens from his own zoogeological perspective. Darwin had been taught, while a student at the University of Edinburgh, that the ancient sea floor was the place where the naturalist could “lift the veil that hangs over the origin and progress of the organic world.”[407] The practices he was taught at Edinburgh linked him to a network of other naturalists who also believed that the sea floor yielded truth about the origins of life and new species, the philosophical naturalist's greatest question. Darwin drew upon his Edinburgh evidentiary practices when he speculated that the deep-sea floor would be a potential site for evolutionary evidence. When Huxley deployed those same practices, he pulled up the evidence that Darwin had predicted. And when other naturalists looked to the ancient seabed, they found the first geological confirmation that Darwin's zoology was correct. Despite Huxley's conservative claims about *Bathybius*, a large number of elite naturalists felt as though the scientific “veil of the past” was ready to be pulled back. Darwin verged on the grandest of discoveries, a law of the natural world.

New findings later generated even more excitement over deep sea biology. One creature in particular, made the connection between the biotic debate and evolution even closer. That specimen was independently discovered by an American – Pourtalès - and the Norwegian naturalist Michael Sars. These two naturalists discovered a “living fossil” in the deep sea. The philosophical naturalist community had long been awaiting confirmation of Darwin's great law when this discovery was announced.

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[407] See chapter one of this dissertation. The quote is from the Anonymous, “Observations on the Nature and Importance of Geology,” 293, described in chapter one.

The Stalked Crinoid as Darwin's Living Fossil

Charles Wyville Thomson, the professor of natural history at Queen's University Belfast, was almost convinced that creatures lived in the deep sea. As already mentioned, he had heard of the creatures encrusted on the Mediterranean cable. That was fairly convincing evidence, in Thomson's opinion. However, he wanted confirmation. Like Forbes and Darwin, he had trained at the University of Edinburgh as a medical student, but turned his attentions to marine zoology and dredging. He researched a number of marine invertebrates, though he was particularly interested in crinoids, marine invertebrates also valued by other zoogeologists.[408] Forbes, the late Regius Professor of Natural History at Edinburgh, had used crinoid fossils to explore the morphological relationships between ancient extinct and extant organisms.[409] In 1868, Thomson was offered a professorship at the Royal College of Science, Dublin. He accepted the position and used the opportunity to visit a fellow naturalist in Norway regarding an interesting crinoid specimen.

Thomson traveled to Norway to visit Michael Sars, a theologian and distinguished professor of zoology at the University of Christiania [Oslo] who was one of the leading Norwegian marine biologists. Norwegian dredgers had already made some strange discoveries, such as when the naturalist Peter Christen Absjørnsen recovered a new starfish, the *Brisinga*, from a depth of around 200 fathoms.[410] It seemed as though Sars' son had made another discovery from a greater depth, which might confirm

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[408] AA Manten, "C. Wyville Thomson, J. Murray, and the 'Challenger' Expedition," *Earth-Science Reviews* 8 (1972): 255.

[409] See chapter one of this dissertation.

[410] See Thore Lie, "The Introduction, Interpretation and Dissemination of Darwinism in Norway during the period 1860-1890," in *The Reception of Charles Darwin in Europe* eds. Eve-Marie Engels, Thomas F. Glick (London and New York: Bloomsbury Publishing, 2008), 158-163 for more on Absjørnsen and Darwinism.

Jenkin's challenge to the azoic zone theory.[411] Perhaps Sars' find could determine whether the azoic line was simply lower than previously established.

When Thomson arrived, he found more than another challenge to the azoic zone; he found potential proof for Darwin's natural selection.[412] Sars had dredged a crinoid from the deep Norwegian fjords from a couple hundred fathoms. The presumed depth of the crinoid's habitat was believed to be much less than that from where the transmediterranean cable had been pulled in 1860. Even Wallich's starfish had supposedly come from a much deeper region, though Thomson maintained some doubt as to whether the starfish had actually come from such a great depth. What interested him about the new crinoid was not its depth, but rather the fact that the stalked crinoid wasn't supposed to be alive; it was only known as an ancient, extinct morphological form. The crinoid had a slender stalk by which it tethered itself to the deep sea floor. Such a morphological trait had only been seen in very old fossils. The discovery of a stalked crinoid fossil was a rare and celebrated event among naturalists. British and European naturalists traded valuable specimens of their own to receive a stalked crinoid fossil from the Smithsonian collections or an American naturalist in return.[413] Yet, Thomson held a freshly-preserved stalked crinoid specimen in his hands.

The specimen was a variety of *Rhizocrinus*. Sars named it *Rhizocrinus lofotensis* after the Norwegian area it was found. Thomson asked Sars to send a stalked crinoid specimen sent to him in Belfast. Professor Sars died on 22 October 1869, not long after

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[411] See Rozwadowski, *Fathoming*, 148.

[412] See Thomson, *Depth of the Sea*, which is discussed at great length in chapter five of this dissertation.

[413] See chapter one regarding the great availability of crinoid fossils in the United States. Records from the Smithsonian also show a number of naturalists, such a the North Pacific Exploring Expedition's William Stimpson, trading crinoid fossils, especially *Pentacrinus* fossils, for valuable specimens or goodwill.

the visit. However, his son Georg Ossian Sars continued his father's work. Michael Sars' discovery captured Thomson's imagination. He contemplated the implications of the literal “living fossil” found in the deep sea. Naturalists might finally be able to solve Forbes' question: how do the crinoids fit between the extinct, stalked cystoids – which were also only found as fossils – and the extant echinoderms? Here was the perfect morphological form that showed the stalk of a cystoid, but also the feathered arms of a crinoid.[414] The stalked crinoid was not only a living example of an ancient form , it was also an example of morphological intermediacy between two types of organisms. The *Rhizocrinus* was the crucial evidence for natural selection – the “living fossil” – that Darwin had predicted would be found in *On the Origin of Species*.[415]

Another stalked crinoid was found by Pourtalès while dredging off the Florida coast. Pourtalès immediately identified it as belonging to the genus *Bourguetticrinus*. Specifically, Pourtalès believed that the specimen might be a *B. hotessieri*, which had been found within a geological formation on Guadeloupe. The fossil was fragmentary, but pieces of the stem remained. The mystery of the fossil was solved by Pourtalès' discovery of “half-a-dozen [live] specimens” obtained “between 230 and 300 fathoms, unfortunately... injured by the dredge.”[416] Thomson did not find out about the American discovery right away. However, it was clear that the Swedish and American naturalists were on the threshold of an amazing zoogeological discovery. That point would only be emphasized when Thomson heard that Pourtalès' had captured his own stalked crinoid.[417] He was convinced that both American *Bougetticrinus* specimens were actually

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[414] Thomson, *Depths of the Sea*, 452.

[415] See chapter two of this dissertation for Darwin's prediction that “living fossils” would provide proof for natural selection.

[416] Pourtales, “Deep-Sea Dredgings in the Region of the Gulf Stream,” *American Journal of Science and Arts* 46 (1868), 415. See also Thomson, *Depths of the Sea*, 278.

[417] Thomson, *Depths of the Sea*, 278-279.

Rhizocrinus; the British were the only ones to not have procured their own specimen. He knew that the British had to catch up, not as much for national pride, but so that British naturalists could remain at the forefront of scientific discovery. The biotic debate had taught British naturalists how important it was to be able to produce their own specimens from the deep sea.

For Thomson, the abyss became a veritable treasure trove of new scientific wonders. Would he find other living fossils in the abyss? How deep was Forbes' azoic zone? He considered the mounting evidence from the deep sea and began to consider new possibilities. Did the azoic zone exist at all? He arranged a dredging trip with his friend, Carpenter. He knew that Carpenter had been defending the *Eozoön* in Canada and, by extension, Huxley's *Bathybius* discovery as well. Carpenter was as interested in deep-sea zoogeology as himself.

While dredging off the British coast, Thomson proposed a deep-sea dredging expedition, much like the ones conducted in the United States.[418] Routine deep-sea sounding and dredging was beyond the capabilities of private enterprise. Such an expedition would require larger ships and steam-powered machinery to bring up the heavy nets, such as those given to the United States Coast Survey. Thomson and Carpenter could request the use of a naval ship explicitly for the purposes of dredging and sounding the deep sea. Even with a ship at their disposal, the two naturalists would still require specialized equipment for the voyage.

Luckily, a number of Darwin's closest circle had great influence over British scientific societies at the time. This nine-person dining group, called the X Club, met once a month to socialize and promote Darwin's theory of evolution. Huxley and Hooker

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[418] Thomson, *Depths of the Sea*, 3.

were both members of the X Club, as well as John Lubbock, son of the tidologist Sir John Lubbock.[419] These three individuals had been present when Darwin had revealed his new theory of natural selection; they would be interested in the evolutionary evidence found by Sars in the deep sea. Hooker and Huxley were also prominent naturalists with previous connections to the biotic debate.[420]

In 1868, Carpenter contacted the Royal Society endorsing Thomson and requesting assistance. The cover letter was addressed to the president of the Royal Society, Edward Sabine, the same naval officer who had scoffed at Ross' attempt to seal deep sea mud in jars so many years ago. Thomson outlined the purpose of the expedition. The *Lightning*, a cranky surveying ship lent to Thomson and Carpenter for their investigations, left port on 11 August 1868. The weather was stormy, which often halted dredgings. During the six weeks at sea, “only ten days were available for dredging,” and few of those were in very deep water.[421] Nonetheless, the voyage was a success. The Admiralty dedicated two more ships, the *Porcupine* and the *Shearwater*, to Thomson's research on the summers of 1869, 1870, and 1871.

For Thomson, the voyages of the *Lightning* and the *Porcupine* were opportunities to explore deep-sea biology. He had been deeply affected by Professor Sars' discovery of the stalked crinoid. He had hoped to settle the questions of deep-sea faunal distribution, the azoic zone, and Darwin's proof for natural selection. Carpenter had recently engaged in a debate about oceanic circulation, leading from his previous

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[419] See Ruth Barton’s “’An Influential Set of Chaps’: The X-Club and Royal Society Politics 1864-85,” *British Journal for the History of Science* 23 (1990), 53-81 and *The X Club: Power and Authority in Victorian Science* (Aldershot: AshgatePublishing, 2001) for more on the X Club.

[420] Joseph Hooker would later become president of the Society in 1873, to be followed by William H Spottiswoode in 1878 and Huxley in 1883, all members of the X Club.

[421] Thomson, *Depths of the Sea*, 79. See also Schlee, *History of Oceanography*, 103.

research in deep-sea sedimentation. Thomson, as a marine zoologist, communicated his own goals for the expeditions very clearly:

> I pretend to no special knowledge of physics, and I should have greatly preferred confining myself to the domain of Biology, my own proper province; but certain physical questions raised our late explorations have so great importance in relation to the distribution of living beings, and have of late been brought into so great prominence by Dr. Carpenter...[422]

The discovery of ocean circulation made during the *Lightning* voyage had raised physical questions related to the distribution of marine invertebrates. The deep sea was found to not be of a constant temperature. Oceanic circulation also facilitated the movement of species in the deep sea. However, Thomson made clear that their investigations of physical oceanography, while interesting, were ultimately subsidiary to their study of marine zoology.

Of the zoological questions raised by Thomson, the origin of species took central priority. Thomson situated Darwin's hypothesis in relation to other dominant theories regarding the origin of species, such as the concept of specific centers of creation. Forbes had used marine invertebrate fossils to establish that new species appeared only once in the geological record and were initially bound to one geographical location. The species in question would radiate outward from that point of creation. If the species radiated outward to a different environment, the species might adapt over time to thrive in the new location. If the location where the species first appeared became inhospitable, the species might disappear in that location, or even go extinct altogether.[423]

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[422] Thomson, *Depths of the Sea*, viii-ix.

[423] Thomson, *Depths of the Sea*, 2.

To Thomson, Darwin's reliance upon variation as *vera causa* for “converting one species into... a different species” made sense under certain conditions.[424] The deep-sea environment obviously affected not only species distribution, but also the dominant variety of the species in the area, known as the “representative” type. However, variation had its limitations:

> The individuals compromising a species have a definite range of variation strictly limited by the circumstances under which the group of individuals is placed. Except in man, and in domesticated animals in which it is artificially increased, this individual variation is usually so slight as to be inappreciable except to the practised eye; but in any extreme variation which passes the natural [environmental] limit in any direction clashes in some way with surrounding circumstances, and is dangerous to the life of the individual.[425]

A species would naturally balance the tension between natural variation and environmental extremes. Individuals within the species would spread as far as their physiology was capable of withstanding. Variation might slowly increase that range. However, the process would be limited and would take time. For example, if an organism fell too far to withstand the pressure of the deep sea, it would pass beyond the species' geographical “line of safety,” and perish. That line of safety could change position, too; if the location where the species' resided became colder or deeper, then many of the individuals would find themselves suddenly outside their environmental limits.[426] Species variation and distribution were tied together in the natural world.

Recent discoveries in deep-sea biology challenged some aspects of Forbes' theory of specific centers. If species could range indefinitely into the deep, then the effect of other environmental factors besides pressure and the lack of sunlight played an

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[424] Thomson, *Depths of the Sea*, 10.
[425] Thomson, *Depths of the Sea*, 10.
[426] Thomson, *Depths of the Sea*, 10.

important role in geological formation. And if species could change indefinitely, as Darwin asserted, then the species and its progeny could spread indefinitely through the entire deep sea. Such a claim might complicate Forbes' theory that species were limited to the geographical range attached to its specific center:

> Now, although the admission of a doctrine of evolution must affect greatly our conception of the origin and *rationale* of so-called specific centres, it does not practically affect the question of their existence, or of the law regulating distribution of species from their centres by migration, by transport, by ocean currents, by elevations or depressions of the land, or by any other causes at work under existing circumstances. So far as practical naturalists are concerned, species are permanent within their narrow limits of variation, and it would introduce an element of infinite confusion and error if we were to regard them in any other light. The origin of species by descent with modification is as yet a hypothesis.[427]

Thomson was willing to challenge prevailing theories on the origin of species, the same way he was willing to challenge Forbes' azoic zone. However, if he was going to accept Darwin's theory of natural selection, he wanted more proof of morphological intermediacy or to see the production of a new species from an existing one. He intended to use the distribution of marine creatures to test Darwin's natural selection "hypothesis."[428]

However, Darwin's hypothesis of natural selection was only one potential explanation of *how* evolution occurred. Thomson was willing to bet that there was a completely unknown deep-sea fauna. Sars and Pourtalès had already found one living fossil, the *Rhizocrinus*. More creatures that demonstrated morphological intermediacy

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[427] Thomson, *Depths of the Sea,* 11.

[428] At this point in the dissertation, I will use the term "hypothesis" to describe Darwin's theory of natural selection. Up until this point, it would have been proper to describe natural selection as a hypothesis, but I have avoided that term for ease of reading. This distinction becomes important later when naturalists accept evolution as a theory, but begin to test Darwin's proposed mechanism, natural selection, as a hypothesis. See chapter five of this dissertation for more on this subject.

emerged, such as the heart urchin, *Poutalesia jeffreysi*, “discovered by M. de Pourtales in the Gulf-Stream explorations of the American coast, and second by Mr. Gwyn Jeffreys near Rockall.” This urchin had a “disjunct ambulacra” that was only known to occur in a fossil species, the *Dysasteridae*, then believed to be extinct. Morphological intermediacy could also be found in the newly discovered *Echinothuria* and a collection of *Ethusa granulata* described by Reverend A. Merle Norman.[429]

Ethusa granulata demonstrated how a marine invertebrate species could vary morphologically in response to different submarine environments. The same species exhibited “most extraordinary modification of structure” in different geographical areas. In 110 to 370 fathoms of water, the creature had a long spinose rostrum [a spiny, nose-like beak] and two extended eye-stalks. However, the animal was blind. Farther north, the same species was found in 542 to 705 fathoms of water. Norman found that the true rostrum had been absorbed completely into the animal's carapace in this deeper variety. Instead, the eye-stalks had hardened into immobile spikes and the creature used them instead of a rostrum. The deeper *Ethusa granulata* was originally thought to be a monstrosity. However, there were so many examples of this deeper form that Thomson believed the creature provided morphological evidence “of modification of structure under altered conditions of life.”[430] This showed that environment could change a species' morphology, but could it produce an entirely new species by minute, unending variation?

More dredging was needed to answer these remaining questions. The living deep-sea fauna had the capacity to answer questions about the ancient past; creatures that had only been known as fossils could now be observed while alive. They

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[429] Thomson, *Depths of the Sea*, 163-164.

[430] Thomson, *Depths of the Sea*, 176.

demonstrated that the deep-sea floor could explain the history of life with living creatures and not just geological specimens.[431] Thomson began to plan a larger zoological, deep-sea expedition once the *Lightning* and the *Porcupine* voyages concluded.

This new expedition would research a number of questions related to deep-sea zoology. First, Thomson would search for more living crinoid specimens. Of all the possible specimens that he could gather, crinoids would be most able to reveal the organization and complexity of living creatures:

> Both on account of their beauty and extreme rarity, and of the important part they have borne in the fauna of some of the past periods of the Earth's history, the first order of the Echinoderms, the Crinoidea, has always had a special interest to naturalists; and, on the watch as we were for missing links which might connect present with the past, we eagerly welcomed any indication of their presence...

The crinoids, especially the stalked crinoids, had been used to explore zoogeological questions since Forbes' work on polarity.[432] The crinoids also held a special place as the first “living fossil” evidence for natural selection found by a member of his scientific network.

Thomson proposed a test of Darwin's theory. Because the deep-sea environment changed over geographical space, the higher species would change morphologically depending upon where they were found within the deep sea over time. For example, the Gulf Stream of the Atlantic seemed to heat the deep sea while the polar regions spread super-cold water along the ocean bottom. These temperature variations, as the sea floor elevated or sank, would change the environment experienced by deep-sea fauna:

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[431] Thomson, *Depths of the Sea*, 80.

[432] See chapter one of this dissertation.

> Accepting, as I believe we are now bound to do so in some form, the gradual alteration of species through natural causes, we must be prepared to expect a total absence of forms identical with those found in the old chalk, belonging to groups in which there is sufficient structural differentiation to require or to admit of marked variation under altering circumstances. The utmost which can be expected is the persistence of some of the old generic types, and such a resemblance between the two faunae as to justify the opinion that, making due allowance for emigration, immigration, and extermination, the later fauna bears to the earlier the relation of descent with extreme modification.[433]

In other words, Thomson was prepared to find living varieties of crinoids – and other echinoderms – that were completely different than those creatures he found in the ancient fossil record.[434] Crinoids were ancient creatures, after all, and they would vary a lot over such a long history. Obviously, there would be similarities between the ancient fossil crinoids and the current crinoids since they shared common history; the current crinoids were the product of the ancient fossil specimens. Deep-sea creatures were extremely old and they seemed to adapt very slowly. Ultimately, modern crinoids should show many forms of intermediacy when compared to fossil specimens. It should be difficult to tell where a new species of crinoid began and older species ended, if Darwin's theory of natural selection was correct. Darwin had insisted that living fossils would prove his theory – from the deep sea if creatures lived there – and Thomson was going to search for them. Thomson published a summary of his evolutionary test in *Nature* on 9 November 1871.[435]

Also working hard to produce evidence for or against Darwin's theory, Thomson and Carpenter had found an ooze in various states of morphological complexity while

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[433] Thomson, *Depths of the Sea*, 479-480.

[434] Thomson also explained how the other stalked crinoids, *pourtalesia* as an example, and echinoderms, such as *Echinothuridea*, could demonstrate Darwin's intermediacy once and for all. See Thomson, *Depths of the Sea*, 487-490.

[435] Wyville Thomson, “The Relation Between Zoology and Palaeontology,” *Nature* 5 (1871): 34. The article was a summary of Thomson's lecture at the University of Edinburgh.

dredging aboard the *Porcupine*. They sent the sample back to Huxley, noting that it showed some similarity to his description of *Bathybius, "*There was an appreciable quantity of diffused amorphous organic matter, which we were inclined to regard as connected, whether as processes, or 'mycelium,' or germs, with the various shelled and shell-less protozoa, mixed very likely with the apparently universally distributed moner of deep water, *Bathybius*."[436] Huxley confirmed Thomson and Carpenter's discovery: they had, indeed, pulled up another sample of the primordial ooze from the deep sea.

Interestingly, Thomson's *Bathybius* sample contained examples of "each of the invertebrate sub-kingdoms." The ooze even showed morphological similarity to the primitive and ancient *Rhizocrinus,* but with slight differences to the crinoid samples known at the time. Another dredging expedition could further explore how Huxley's *Bathybius* grew and differentiated. It might also reveal something about the origin of morphological characteristics.

Lastly, Thomson and Carpenter had successfully dredged down to 1,500 fathoms on their expedition. Their latest dredge confirmed Wallich's challenge to Forbes' azoic zone. Life existed as far down as 1,000 fathoms into the abyss, just as he had claimed. With such routine and profound dredgings, Thomson began to believe that the marine fauna might extend down to the deepest reaches of the ocean floor. Thomson believed that if they could dredge from 2,500 fathoms and bring up living creatures, then, "the general question would be virtually solved for all depths of the ocean, and any further investigation of its deeper abysses would be mere matter of curiosity and of detail."[437] Naturalists had continually pushed the azoic line into deeper and deeper water.

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[436] Thomson, *Depths of the Sea*, 96. A "moner" is a simple, single-celled organisms, one of the Monera.

[437] Thomson, *Depths of the Sea*, 93.

However, if Thomson could dredge at double the distance where life was known to exist, then perhaps naturalists could abandon the azoic zone theory altogether. Thomson was willing to finally lay Forbes' azoic zone to rest once and for all, but only if he found enough proof to do so.

The Expedition to Test Evolution by Natural Selection

By the end of 1869, the British biotic debate had become intertwined with Darwin's theory of natural selection. Huxley had married the biotic debate to Darwin's theory of evolution with the discovery of *Bathybius haeckelii*. The primordial ooze finally described the link between the tiny, foraminiferous shells found by Brooke on the North Atlantic deep-sea floor and the ancient fossil record. Recent discoveries indicated that the *Eozoon* was the fossil ancestor of *Bathybius*. In turn, the delicate foraminifera seemed to have evolved from *Bathybius*. Where American naturalists were generally content to ask whether the shells lived upon the sea floor or drifted from the ocean's surface, Huxley also connected the specimens to evolutionary questions. Huxley believed that he had found evidence for natural selection.

Thomson also found evidence for natural selection while exploring the biotic debate. He was impressed by the stalked crinoid found by Sars and Pourtalès. The creature had been dredged in an intermediate depth of water; naturalists already believed that creatures could live deeper than Forbes originally claimed. However, the species was a form found only in the fossil record before that point. It also demonstrated morphological intermediacy between the cystoids and other echinoderms. Darwin had anticipated such a living fossil might be found within the deep sea if creatures were capable of living at such a depth. The voyages of the *Lightning* and the *Porcupine* had found more living fossils, though Thomson desired final proof of natural selection by a morphological comparison of deep-sea creatures with their fossil counterparts.

The deep-sea dredging voyages also made Wyville Thomson famous. He was knighted in 1870 and offered the esteemed Regius Professorship of Natural History at Edinburgh. Darwin himself wrote a letter of encouragement for Thomson and his potential appointment:

> My dear Sir
>
> I am very glad to hear that you are a candidate for the Chair of N. History in the U. of E. You have my sincere good wishes for your success, for to the best of my judgment, you have proved yourself well fitted for the Post by what you have done in Natural Science & by the course of your recent investigations. Should you succeed you will have an admirable opportunity for bringing forward & encouraging new recruits & as I believe you will do your utmost, I cordially wish for your success.[438]

Darwin viewed Thomson as a fellow evolutionary biologist and showed his support in a number of ways, including an appreciative letter published in *Nature* on 25 September 1873.[439] The letter described Thomson's investigation of male barnacles, a topic on which Darwin had done extensive research. He lauded Thomson for his competency. Along with Darwin's letter of support for the Regius Chair, the *Nature* letter demonstrates that Thomson followed Darwin's work closely and that Darwin was aware of Thomson's studies.

Thomson accepted the Regius Chair of Natural History at Edinburgh, thereby placing himself in a long lineage of dredging philosophical naturalists. He now had the

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[438] DARC v18: 261-262, Charles Darwin C. Wyville Thomson, undated, most likely sent after 7 Oct 1870.

[439] Charles Darwin, “On the Males and Complemental Males of certain Cirripedes, and on Rudimentary Structures.” *Nature* 8 (1873): 431.

institutional resources and connections to perform his test of Darwin's hypothesis. Carpenter and Thomson had already laid the groundwork for a grander dredging expedition. In November of 1871, Carpenter managed to assemble a committee of the Royal Society to discuss an expedition.[440] The Circumnavigation Dredging Committee met to discuss the expedition's scientific mission. They lobbied the Admiralty for the use of a ship to conduct this grand scientific voyage; the committee suggested that all manner of scientific research could be done while at sea, even physical hydrography. The Committee reconvened almost a year later, prompted by a letter sent from the Admiralty on 22 August 1872. The letter requested more information as to the scientific work that would be done on such a voyage; it appeared as though the Admiralty would support a dredging expedition after all. The Royal Society Council was not in session, so the Circumnavigation Committee answered the call for information.[441] The committee consisted primarily of those naturalists engaged in evolutionary research and the biotic debate: Carpenter, Jeffreys, Thomson, Huxley, and Hooker. The committee also added Alfred Russel Wallace, the co-discoverer of natural selection, to their number, to advise on the expedition's scientific mission.[442]

Discovering new marine fauna was mentioned first among the scientific objectives of the cruise. The meeting report suggested a number of locations where no marine biological work had been conducted, such as regions bordering the Antarctic Sea. The committee acknowledged the difficulty conducting marine investigations in this

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[440] Rozwadowski, *Fathoming*, 162.

[441] Circumnavigation Dredging Committee of the Royal Society of London, *Report of the Circumnavigation Committee of the Royal Society on the Request of the British Admiralty for Suggestions Relative to the Scientific work of the Proposed Expedition of Her Majesty's Ship 'Challenger' Round the World,* Bureau of Navigation, Navy Department. (Washington: Government Printing Office, 1872).

[442] Thomson, *Voyage of the Challenger: The Atlantic*, vol. I. (London: Macmillian & Co., 1877), 68.

region, “Probably, investigations in these latitudes may be difficult. It must be remembered, however, that the marine fauna of these regions is nearly unknown; that it must bear a most interesting relation to the fauna of the high northern latitudes...”[443] Government sponsorship would finally allow British naturalists access to regions far beyond their normal areas of investigation. A comparison between the arctic and antarctic marine fauna would yield information of the effect of environments on speciation. British naturalists had already surveyed the Arctic Sea. What remained was access to the distant and dangerous areas which could be accessed by experienced naval ships.

The other studies suggested by the committee blended biological with physical science questions. For example, research on the distribution of marine fauna took a primary role on the voyage. The crew was also charged with taking regular temperature soundings.[444] These temperature soundings would be tabulated and used to predict trends in oceanic circulation; temperature soundings could be taken at various depths, especially on differing sides of submarine formations, to determine the flow of water. Oceanic circulation, however, played a direct role in the distribution of marine life. Tidal observations were also urged by the committee, but not for the simple collection of information; the committee believed that, “No opportunity of making tidal observations should be lost...,” because these tidal measurements could finally answer, “The interesting question of the elevation or subsidence of land...”[445] The committee linked the voyage to almost all areas of seabed science conducted in the last century.

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[443] CDCR, *Report*, 3.

[444] CDCR, *Report*, 4.

[445] CDCR, *Report*, 7.

As for the zoology done on the voyage, the committee gave the expedition's leader Charles Wyville Thomson complete latitude to investigate his evolutionary questions. They phrased this charge tactfully and did not elaborate on the zoological research to be done on the voyage. Instead, they simply stated, “As the scientific director of the expedition is an accomplished zoologist, and has already had much experience in marine exploration, it will suffice to offer a few suggestions under this head.”[446] The Committee made a small number of suggestions, such as to explore the fauna found at “Wallace's line” in the Malay Archipelago. The Admiralty seemed to agree with these recommendations and promised the use of HMS *Challenger* for scientific investigation.

By 1872, the *Challenger* was ready to set sail. It would circumnavigate the globe with dredge in tow. As Thomson and members of the X Club had anticipated, the expedition would bring up conclusive evidence of *Bathybius* and Darwin's living fossils. However, that evidence would not be the type that Darwin's close associates expected. The *Challenger* set sail to gather information on deep-sea marine invertebrates, creatures long-esteemed for their ability to help naturalists adjudicate questions of organismal complexity. When Thomson returned with those creatures, as well as a strong denial of natural selection, naturalists began to question the validity of Darwin's theory of evolution. The intimate link between deep-sea biology and Darwin's *On the Origin of Species* transformed Thomson's deep-sea specimens into a devastating critique.

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[446] CDCR, *Report*, 14.

CHAPTER FIVE: The *Challenger* and the Eclipse of Darwinism

The *Challenger* expedition was disastrous for Darwinian natural selection. In 1880 Sir Charles Wyville Thomson, Britain's leading expert on deep-sea invertebrates, who had organized the celebrated deep-sea dredging expedition, reported that "the character of the abyssal fauna refuses to give the least support to... evolution... guided only by natural selection."[447] Darwin was quick to respond. His scathing letter accused Thomson, a man of considerable scientific achievement – including being the newly appointed Regius Chair of Natural History at Edinburgh, of not understanding the basic principles of evolution.[448] The letter accused Thomson of sounding more like a theologian than a proper naturalist, a uniquely acerbic condemnation from the otherwise reclusive Darwin. Thomson's report had the ability to sway naturalists against natural selection and Darwin knew it. Such a strongly worded response would be less surprising from Thomas Henry Huxley, Darwin's most vociferous advocate; Huxley was famous for his biting – though brilliant – public controversies. However, Huxley actually advised Darwin against being so harsh in his letter regarding Thomson. Huxley's temperance can be partially explained by his own recently resolved conflict. Only five years earlier he had been embroiled in – and spectacularly lost – his own argument about deep-sea organisms as a result of the *Challenger* expedition.

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[447] C. Wyville Thomson, "General introduction to the zoological series of reports," in *Report on the scientific results of the voyage of the H.M.S. Challenger during the years* 1873-76, Zoology, Vol. 1 (London: John Murray, 1880), 50.

[448] Many historians of evolution will be familiar with William Thomson, the famous nineteenth-century physicist, in relation to the history of natural selection. I refer to William Thomson as Lord Kelvin, his title, to avoid confusion in this chapter. Both Wyville Thomson and Lord Kelvin offered a challenge to natural selection in the late nineteenth century, but Wyville Thomson plays the larger role in this chapter as a biologist and researcher into the mechanisms of evolution. I would argue, because of Sir Wyville's status as part of Darwin's circle, that he is the more salient historical actor in this narrative and that Lord Kelvin is, consequently, "the other Thomson." This argument continues towards the later part of this chapter.

This chapter examines how the *Challenger* expedition affected the late nineteenth-century debate over natural selection. As described in the previous chapter, Thomson had promoted the expedition as a way to test Darwin's hypothesis. Darwinian evolution represented the potential discovery of a biological law, the ultimate goal of the philosophical naturalists. However, when Thomson finally returned with a survey of the global deep-sea fauna, naturalists still had to determine how to derive scientific knowledge from those specimens. The expedition collection offered a way for naturalists to negotiate and reconcile their methods for establishing natural law. When the expedition came back with two different lines of evidence against natural selection, many naturalists abandoned the hypothesis in favor of other possible evolutionary mechanisms.

The resulting *Challenger* debate signified a shift from seabed naturalists who were primarily defined by their local institutions to a more cohesive, transatlantic network. The spread of evidentiary practices related to the study of the historical sea floor during the nineteenth century made widespread interest in the debate possible. It also increased the importance of the samples retrived by the Challenger crew and drew attention to the methods naturalists used to interpret those specimens.

The value assigned to crinoid specimens drew its legacy from zoogeological practices. Even before the opening of the deep sea as a site of inquiry, naturalists relied upon shallower areas of the sea floor as a way to explore Earth's biological and geological past. Influential professors at the University of Edinburgh used marine invertebrate zoology to explore the history of life throughout the nineteenth century. Naturalists valued marine invertebrates for their morphological similarity to the simplest of plants and animals. The Edinburgh zoogeologists hoped to learn about the complexity of life through the study of its simplest creatures. The crinoid was one of the most ancient and, therefore, one of the most valuable of these invertebrate specimens.[449]

The method by which naturalists retrieved the deep-sea specimens was a product of sounding and dredging practices developed during the biotic debate. The deep sea was a relatively unexplored area for British and American naturalists at the beginning of the nineteenth century. However, American naturalists developed technologies to measure deep ocean depths and bring back samples in the mid-nineteenth century.[450] Those new sounding techniques opened the deep-sea environment for exploration, which heralded the laying of transatlantic cables and a fascination with abyssal creatures.[451]

The *Challenger* samples were novel, much-anticipated evidence in the evolutionary debates. Darwin’s scientific networks had also instilled into him an appreciation for these marine invertebrates and their biogeographical distribution. It also trained him to consider how the sea floor changed over time as a way to explore the geological and biological past. As a result, Darwin set up the deep sea – and the marine invertebrates found there – as a place to find evidence for his hypothesis. He incorporated the deep sea into his argument for natural selection. Other naturalists, including Thomson, continued to combine deep-sea biology with Darwin's hypothesis during the 1860s.

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[449] See chapter one of this dissertation.
[450] See chapter three of this dissertation.
[451] See chapter four of this dissertation.

Evidence and the Reception of Natural Selection

At stake in the debate about the *Challenger* evidence was the question of 'divergence,' a principle demonstrated by the existence of intermediate species. By itself, unequal selection did not explain the appearance of new species. Darwin used variation in domestic breeds to form an analogy between an animal breeder who promoted the reproduction of unique animal traits, called artificial selection, and the selection of animals with beneficial traits through competition, or natural selection. However, taken by itself, selection of one trait would only emphasize that trait in a population, causing the species to slowly change over time, but only within set boundaries. Darwin's crucial step was to turn natural selection into a mechanistic explanation for the evolution of species over time. Variation was the "raw material upon which selection can act."[452] Natural selection, in turn, caused species divergence, the gradual divergence of one species into two differing extremes. Divergence allowed for the appearance of new species from common ancestry. At its core, divergence was an ecological argument; diversification allowed species to make best use of the natural resources around them.[453] One species would become two predominant extreme varieties, through natural selection, leading progressively to two different species over vast amounts of time.[454] As historian David Kohn notes, "Divergence... is in fact [Darwin's] one and only explanation

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[452] Robert Olby, "Variation and Inheritance" in *The Cambridge Companion to the Origin of Species* eds. Michael Ruse and Robert Richards (Cambridge: Cambridge University Press, 2009), 31.

[453] David Kohn, "Darwin's Keystone," in *The Cambridge Companion to the Origin of Species* eds. Michael Ruse and Robert Richards (Cambridge: Cambridge University Press, 2009). 88. Kohn relates Darwin's ecological principle with a quote from *On the Origin of Species*: "...the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the polity of nature, and so be enabled to increase in numbers." See also Kohn's "Darwin's Principle of Divergence as Internal Dialogue," in *Darwinian Heritage*, 245-257.

[454] Kohn, "Darwin's Keystone," 95.

of how new species are made."[455] Such an essential aspect of Darwin's theory required evidence. Without divergence, there was no "origin of species." Yet, Darwin was – for the most part – unable to provide that evidence.

Darwin had acknowledged that if divergence did occur the fossil record should contain numerous intermediate, linking forms showing one species evolving into two extreme types, and he mobilized his training as a zoogeologist to explain the apparent lack of intermediate fossil forms. The one area that could possibly capture the entire geological record would be the deep sea, since sedimentation would slowly pile on the remains of organisms from generation to generation. This constant preservation would, theoretically, preserve the entire fossil record and the fossils would, therefore, show divergence. Darwin argued that since the deep sea had neither living creatures in it nor significant sedimentation, it would produce no record of transitional forms. Ultimately, Darwin's focus on the sea floor as a potential site for evidence would facilitate his hypothesis' demise.[456]

Nonetheless, Darwin was forced to provide some evidence of intermediacy and divergence for his hypothesis to convince other naturalists. While Darwin was unable to provide fossil evidence for divergent characteristics, he did offer examples of living species that displayed intermediacy. The platypus proved to be the most memorable example of species transition; it seemed to have distinct morphological characteristics present in two different animal groups. A platypus had a bill, which it presumably passed

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[455] Kohn, "Darwin's Keystone," 99.

[456] This chapter continues the use of "hypothesis" in relation to natural selection from chapter four of this dissertation. Wyville Thomson had explicitly labeled natural selection as a hypothesis. The word "hypothesis" to describe natural selection also helps to differentiate between the "theory of evolution" and the "hypothesis of natural selection," Darwin's proposed mechanism for evolution. Many naturalists, including Thomson, accepted evolution, but remained skeptical of natural selection as its mechanism.

to its avian descendants, and fur, which passed to its mammalian descendants. Following Darwin’s logic regarding divergence, natural selection favored the most extreme versions of these two traits, the platypus' bill and fur, causing two differing lines to branch off their common ancestry. One line of descendants benefited from having fur, so they retained their fur, but lost the beak. Their avian cousins, with better-adapted beaks, would out-compete the mammals for food gathered by their beaks. However, the avians would eventually lose their fur since the mammals had a more extreme version of that morphology. The platypus, having both a beak and fur, was presumed to be an intermediate form and a common ancestor of two morphological lineages. It had been allowed to survive as an intermediate form because Australia, being a small landmass, did not offer as much selective pressure as larger continents. Darwin predicted that naturalists would discover more intermediate forms, or “living fossils,” as they continued to explore special geographical locations, like Australia.

British naturalists generally accepted Darwin's concept of common descent. By 1869, a number of lingering doubters of evolution finally admitted that no significant opposition to evolution existed in the United Kingdom.[457] Darwin's choice to publish his theory as a book instead of a series of journal articles ensured that his ideas entered a wide readership.[458] Natural selection was also taken up by a number of scientific societies. For example, the Royal Society of London did not have an explicit discussion of Darwin's hypothesis during the first decade of *On the Origin of Species*' publication. However, as early as 1860, naturalists began to incorporate Darwin's hypothesis into their own research. William Benjamin Carpenter, the later co-organizer of the HMS

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[457] MJS Hodge, “England” in *The Comparative Reception of Darwinism* ed. Thomas Glick (Chicago and London: University of Chicago Press, 1988), 3.

[458] Frederick Burkhardt, “England and Scotland: The Learned Societies” in *The Comparative Reception of Darwinism* ed. Thomas Glick (Chicago and London: University of Chicago Press, 1988), 33.

Lightning and *Porcupine* expeditions, published a long report on foraminifera in the Society's *Transactions*. The *Formaninifera*, as a phylum, contained a great diversity of morphological forms. He concluded that the great number of forms might have descended from a small number of types by minute modifications.[459] Carpenter later supported natural selection more explicitly in two reviews, one in the *National Review* and the other in the *British and Foreign Medical and Chirurgical Review*.[460] Here, Carpenter agreed that Darwin had identified the *vera causa* of the emergence of new species through diversification. The Royal Society would later award Darwin the Copley Medal, its highest honor, in 1863. Darwin's friends played no small role in making sure that he was recognized for the honor. Other naturalists followed suit and incorporated their thoughts on evolution and natural selection into their work; numerous passing references to both evolution and natural selection appear in the Royal Society *Proceedings* and *Transactions*, despite the general lack of direct references to Darwin.[461]

At first, naturalists also debated the general lack of evidence for divergence. A number of naturalists later came forward with examples of intermediate species to confront this critique. For example, Sir John Lubbock read a paper before the Linnean Society in 1860 on marine crustaceans, “On Some Oceanic Entomostraca collected by Capt. Toynbee.”[462] This paper presented the specimens as examples of intermediacy between species. He went so far as to exclaim “How worthless then is the argument

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[459] William B. Carpenter, “Researches on Formainifera” *Philosophical Transactions of the Royal Society of London* 150 (1860): 569-570. See Burkhardt, “England and Scotland” as well as Hull, *Darwin and his Critics*, for a deeper discussion of Carpenter's incorporation of evolution into his foraminifera research.

[460] Burkhardt, “England and Scotland,” 34.

[461] Burkhardt, “England and Scotland,” 35-36.

[462] John Lubbock, “On Some Oceanic Entomostraca collected by Capt. Toynbee,” *Transactions of the Linnean Society* 23 (1860): 253-254.

against the mutability of species which depends upon the supposed absence of 'links!'"[463] From an early stage, naturalists debated a lack of evidence for divergent morphological characteristics. Supporters of natural selection also used marine invertebrate specimens to try and provide that evidence.

Naturalists also used fossil organisms to show intermediacy. In 1860, Adam Sedgwick, Darwin's Cambridge geology professor, complained that the morphological changes in the fossil record were too great to explain, given the absence of intermediate forms.[464] However, not all fossil evidence was connected to Darwinian questions as explicitly as Sedgwick's first criticisms.[465] In 1863, the paleontologist Sir Richard Owen, the influential superintendent of the British Museum of Natural History, read a paper before the Royal Society on an *Archeopteryx* fossil that showed both reptilian and avian morphological traits.[466] Five years later, Huxley gave a lecture in response to Owen's description of the fossil, also at the Royal Society. Neither naturalist levied the fossil as evidence for or against Darwin's hypothesis in their papers. However, a week after his Royal Society paper, Huxley used the *Archeopteryx* specimen in a Royal Institution lecture – aimed at a broader audience – to support divergent characteristics:

> ...the facts of Palaeontology, so far as Birds and Reptiles are concerned, are not opposed to the doctrine of Evolution, but on the contrary are quite such as that doctrine would lead us to expect; for they enable us to form a

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[463] Lubbock, “Entomostraca,” 174. The quote may be found in Burkhardt, “England and Scotland,” 50.

[464] See Darwin, *Life and Letters*, 247-250.

[465] Burkhardt, “England and Scotland,” 71.

[466] Richard Owen, “On the Archeopteryx of Von Meyer, with a Description of the Fossil Remains of a Long-Tailed Species, from the Lithographic Stone of Solenhofen,” *Philosophical Transactions of the Royal Society of London* 153 (1863): 33-47. See Burkhardt, “England and Scotland,” 37. Owen would, in 1863, establish his own test of natural selection and conclude that independent traits were, when taken in conjunction, too perfectly suited to their environment to have been selected for over long periods of time. He compared the traits to something designed by the human intellect, thereby attempted to reinforce the use of teleology in biological theorizing. See Burkhardt, “England and Scotland,” 56. See also Nicolaas Rupke, *Richard Owen: Victorian Naturalist* (New Haven: Yale University Press, 1993), 175.

> conception of the manner in which Birds may have been evolved from Reptiles, and thereby justify us in maintaining the superiority of the hypothesis, that birds have been so originated, to all hypotheses which are devoid of an equivalent basis of fact.[467]

Huxley's mobilization of the *Archeopteryx* fossil as evidence for intermediate forms demonstrates the interest in evidence for divergence. He posited the *Archeopteryx* as the ancestral link between birds and reptiles, much as the platypus had linked birds and mammals. It also illustrates how naturalists at elite venues, such as the Royal Society, may have presented evidence for or against natural selection without explicitly stating the theoretical implications of their research for Darwin's hypothesis. The desire for evolutionary evidence sometimes simmered below the surface of formal papers in elite scientific circles.

British response to natural selection differed in the United States, where the Civil War disrupted its scientific communities. However, the lack of transitional species was cited as a crucial argument against evolution even before the onset of the Civil War. The botanist Asa Gray, later a supporter of Darwin, had argued that the fossil record showed the appearance of ferns and leaf-bearing plants within the same geological period. If more complex organisms descended from simpler organisms, how did the two kinds – one simple and the other complex – appear within the same rock specimen? Gray deployed the lack of intermediate forms as a critique against an earlier evolutionary treatise, *Vestiges of the Natural History of Creation*.[468]

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[467] TH Huxley, *The Scientific Memoirs of Thomas Henry Huxley,* 4 volumes eds. Sir Michael Foster and E Ray Lankester. (London:Macmillan, 1901), III, 313. The quote may be found in Burkhardt, “England and Scotland,” 38.

[468] See chapter one of this dissertation for a discussion of *Vestiges*. On Gray, see Edward J Pfeifer, “United States” in *The Comparative Reception of Darwinism* ed. Thomas Glick (Chicago and London: University of Chicago Press, 1988), 171.

Despite the shared interest in intermediate forms as evidence, natural selection seems to have remained less popular in the United States. This perception is, to some degree, colored by a prominent American naturalist who staunchly defended the immutability of species, Louis Agassiz. Harvard University had attracted Agassiz to the United States in 1847, where he quickly took a prominent position in American science. When Darwin's hypothesis publicly reached the United States, Agassiz took a swift and decisive stand against the idea. His opposition was challenged by another eminent American naturalist William Barton Rogers. Their clash, which spanned four meetings of the Boston Society of Natural History, was both eloquent and enthralling[469] Most accounts record that Rogers won the debates.[470] Agassiz offered the Silurian *Lingula,* the same marine invertebrate that Darwin contrasted against the living fossils, as evidence against evolution; the *Lingula* had persisted through the geological epochs relatively unchanged.

Despite these criticisms, evolution – and to a lesser extent natural selection – continued to grow more accepted within elite Anglo-American scientific groups. In 1872, Darwin's sixth and final edition of *On the Origin of Species* dropped the word “On,” becoming *The Origin of Species*.[471] Through this action, Darwin and his supporters subtly claimed a transition from hypothesis to theory. The edition contained an entirely new chapter refuting recent critiques.[472] This retort, as well as the new title for Darwin's evolutionary treatise, was couched in an upwelling of Darwinism within the British

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[469] See Pfeifer, “United States,” 177.

[470] Pfeifer, “United States,” 176.

[471] Morse Peckham’s varorium text of the *Origin of Species* notes the absence of the word “On” from the sixth edition title, 23.

[472] The critiques mentioned here were given by St. George Jackson Mivart, a former student of Huxley. Mivart, a devout Catholic, eventually turned against Huxley and Darwin. He wrote *On the Genesis of Species* in 1871 to reconcile evolution and Catholicism, but was attacked by both communities for the attempt. See also Jacob Gruber’s *A Conscience in Conflict: The Life of St. George Mivart* (London: Oxford University Press, 1961).

Association and other scientific societies that continued throughout the 1870s. After a particularly encouraging meeting at Norwich in 1868, Huxley wrote to Darwin with news of the event. He jokingly threatened to switch sides because he was not having enough fun fighting Darwin's opponents, “The only fault was the terrible 'Darwinismus' which spread over the section and crept out where you least expected it... You will have the rare happiness to see your ideas triumphant during your life time... I am preparing to go into opposition – I can't stand it –.”[473] Supporters of natural selection continued to enjoy this triumph and happily searched for more evidence of intermediate species.

Searching for Evidence of Natural Selection

Darwin's explanations regarding the incomplete fossil record were more or less sufficient for his community of naturalists until the two events explored in the previous chapter, which provided the incentive for Thomson to search for evidence of natural selection in the deep ocean. The first event only complicated Darwin's assertion that life did not exist on the deep-sea floor. In 1860, a telegraph engineer pulled up a cable from far beneath the Mediterranean with living creatures encrusted to its outer casing. The second event also opened the deep sea as a site of evolutionary investigation. In 1868, a specimen showing ancient, morphological intermediacy – a stalked crinoid – surfaced from the deep-sea floor. This stalked crinoid had the features of extant crinoid echinoderms, but also possessed a stalk similar to the extinct, older cystoids.

These two developments were made more urgent by the discovery of a primordial ooze in the deep ocean. Huxley had also heard of creatures pulled from deep

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473 DARC v16 part II: 740, TH Huxley to Charles Darwin, 12 Sept 1868.

within the ocean. These discoveries caused him to reexamine a sediment sample from the Atlantic seabed. When viewed under high-powered magnification, the sediment consisted of a gelatinous mass similar to living protoplasm. Huxley stopped just short of claiming that the deep-sea ooze was the origin of all evolutionary life on Earth. If living creatures evolved from simpler creatures, even the most complex life could trace its lineage back to a very simple origin. Huxley believed that this ooze, which he called *Bathybius haeckelii*, was the simple, ancient organism from which life differentiated. It represented the boundary between the living world and the non-living. *Bathybius* entered into the zoological record not long after the announcement of his microscopical analysis of the deep-sea sediment.

After these exciting discoveries, many naturalists hoped that the sea floor would yield the intermediate forms that Darwin had anticipated. Thomson and William Benjamin Carpenter organized a series of deep-sea dredging expeditions in the late 1860s. Thomson published the results from these expeditions in a well-received treatise, *The Depths of the Sea*. He set out a test of Darwin's hypothesis for evolution within the book: he would search the deep sea for the living fossils mentioned by Darwin and compare them to more ancient forms. Thomson was in a position to provide the long-awaited evidence for natural selection that naturalist desired.

In the summer of 1872, the British government gave Thomson use of HMS *Challenger* to conduct his study of the deep sea. Sir William Herdman, Thomson's assistant, would later recall the expectations expressed by those naturalists as he entertained them on long walks through the Scottish countryside: “There were great and widespread hopes and expectations amongst scientific men that the 'Challenger' explorations would result in the discovery of... archaic connecting links comparable in morphological importance with such land or shallow-water forms as *Ornithorhynchus* [the platypus]..."[474] The *Challenger* was refitted for scientific purposes and a staff of civilian naturalists was selected for the voyage.

The Admiralty chose George Strong Nares to command the expedition. Captain Nares had worked with Carpenter on the *Shearwater* and showed an ability to work well with civilian naturalists while in command of a ship. Thomson would lead the scientific staff. The *Challenger* would investigate many aspects of the ocean, including oceanic circulation and marine chemistry, though Thomson's biological questions would take a majority of the staff's attentions. The Royal Society selected the other members: Henry Nottidge Moseley was chosen to examine marine invertebrates, John Murray was given the biological investigations at shallow and intermediate depths, John Young Buchanan became ship's chemist, John James Wild was the voyage's artist, and Rudolph von Willemöes-Suhm replaced the zoologist right before the expedition set sail.[475] After some final instrumental outfitting and a dinner, the *Challenger* set out from Portsmouth on 21 December 1872.[476] The crew quickly set into routine practices as their three-year circumnavigation began. Regular testing of sea currents and underwater temperatures became a less exciting part of the naturalists' day.

Dredging proved to be somewhat more exciting at first. Even their deeper dredges pulled up complex organisms:

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[474] William A. Herdman, *Founders of Oceanography and their Work: An Introduction to the Science of the Sea* (London: Edward Arnold & Co, 1923), 61-62.

[475] Deacon, *Scientists and the Sea*, 335-336.

[476] Deacon, *Scientists and the Sea*, 337. The ship had been outfitted with equipment for dredging at depths around 2,700 fathoms depth. Thomson had used hemp lines for nets and trawls. Piano wire for sounding, but the device collapsed when used. Deacon reports, "For sampling the sea floor they used for preference the modification of the Hydra sounding machine named after its inventor, Lieutenant C.W. Baillie."

> ...there is no depth limit to the distribution of any group of gill-bearing marine animals. Fishes, which from their structure and from what we know of the habits of their cogeners must certainly live on the bottom, have come up from all depths, and at all depths the whole of the marine invertebrate classes are more or less fully represented.[477]

However, these organisms were surprisingly distributed; they were nearly uniformly spread across the ocean. At each deep cast, similar species came up from the abyss. Nonetheless, the deep reaches of the oceans contained complex life.

The existence of foraminifera and sedimentation on the deep-sea floor, however, turned out to be much more complicated than originally supposed. Naturalists had debated whether foraminifera lived on the deep-sea floor – or died at the sea surface and floated to the bottom – since the 1850s. In 1853, American seabed surveyors had used Brooke's sounding line to pull up sediment from almost 2,000 fathoms. The Atlantic mud consisted completely of tiny calcareous shells. The delicate shells rested unbroken upon the sea floor until they were retrieved with Brooke's sounding line. Naturalists on both sides of the Atlantic split over whether or not the tiny creatures lived deep within the ocean. Thomson had suspected that he would find the same foraminifera spread throughout the entire sea floor. However, not long after the expedition set sail, the samples the *Challenger* crew brought up began to darken with each sounding and, eventually, the sediment disappeared. The tiny shells did not blanket the entire surface of the ocean.

On 26 February 1873, the *Challenger* crew found themselves in the middle of the Atlantic, 3,000 fathoms above the sea floor. Their soundings retrieved a “perfectly

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[477] Wyville Thomson, “Presidential Address for Section E – Geography” in *Report of the 1878 British Association for the Advancement of Science* (London: John Murray, 1879), 621.

smooth red clay, containing scarcely a trace of organic matter."[478] The gray, foraminiferous ooze returned as the *Challenger* continued across the Atlantic to the Dolphin Rise. Thomson supposed that the red clay was a local phenomenon. Then, as the ship pulled away from the shallower Dolphin Rise, the ooze disappeared again. The sea floor dropped to almost 3,000 fathoms for a second time. Thomson set the dredge overboard. Instead of gray slime, the dredge pulled up red clay – this time inhabited by tiny, tube-building worms. There in the middle of the Atlantic – at over three times the depth where the transmediterranean telegraph cable had been retrieved – Thomson found living marine invertebrates.

Thomson recorded the historic dredge. He had previously decided that he would abandon Forbes' azoic zone theory if creatures could be pulled from 2,500 fathoms depth or more. The red clay worms showed "conclusive proof that the conditions of the bottom of the sea to all depths are not only such as to admit of the existence of animal life, but are such as to allow of the unlimited extension of... animals high in the zoological series..."[479] Forbes' azoic zone was – at long last – extinguished.

The question of deep-sea sedimentation still needed to be solved. Thomson originally believed that South American rivers had dumped the red clay into the deep Atlantic. However, when they continued to find the same sediment north of St. Thomas, Thomson could no longer claim that the red clay was a local concern. Again, the depth reached in excess of 3,000 fathoms. A pattern emerged; the foraminiferous ooze

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[478] Thomson, *Voyage of the Challenger*, 182. The quote may also be found in Deacon, *Scientists and the Sea*, 338.

[479] Wyville Thomson, "Notes from the Challenger," *Nature* 8 (1873), 51-53. The quote may also be found in Deacon, *Scientists and the Sea*, 338.

seemed to yield to red clay when depths approached 3,000 fathoms. Nonetheless, Thomson began to search for an alternative explanation for the deep red clay. [480]

Murray was not convinced that the foraminifera lived at the sea floor, which might provide an explanation for why they did not coat the deeper regions of the sea. He threw the tow net overboard and found foraminifera in the surface waters above the gray ooze, as other naturalists had found in the 1860s. He continued to collect at depths between 0 and 150 fathoms.[481] He found that the foraminifera lived at the sea surface across almost all of the ocean. These living foraminifera came to the surface at night, but then sank deeper into the ocean at daylight. Living foraminifera had yet to be collected from the sea floor. Instead, they teemed above the ooze along the surface and middle depths. Thomson changed his opinion regarding whether the tiny creatures lived along the seabed; he became convinced that they lived at the sea surface.[482]

Murray's discovery complicated another scientific discussion. If the foraminifera lived at the ocean surface, what did the creatures living far beneath the waves eat? Wallich had posed the question in *Nature* in January of 1870. Jeffreys and a number of other naturalists responded to the question later that month. Thomson had believed that microorganisms capable of absorbing sustenance directly into their bodies had to support the higher organisms living at great depths.[483] Thomson had mentioned the Protista, which included Huxley's *Bathybius*, as the simple life at the bottom of the

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[480] Deacon, *Scientists and the Sea*, 339. However, as Deacon notes, the red coloring of this region was, in fact, due to South American sediment.

[481] Deacon, *Scientists and the Sea,* 342.

[482] Wyville Thomson, “The 'Challenger' in the South Atlantic,” *Nature* 10 (1874): 142-143.

[483] Wyville Thomson, “Letter to the Editor,” *Nature* 1 (1870): 315-316. See also Gwyn Jeffreys, “Food of Oceanic Animals” in the same issue and George Wallich, “Food of Oceanic Animals,” *Nature* 1 (1870): 241-242.

sea.[484] The inability for foraminifera to live at great depths might indicate that other microscopic life found it difficult to live there as well. And if simple life *did* form the chemical foundation for food at great depths, how did it achieve this process? *Bathybius* remained a potential consumer and producer of organic material for deep-sea life.

The red clay at depths reaching 3,000 fathoms also remained a mystery. The clay seemed to gain its distinctiveness from being absent of foraminifera. For some reason, the foraminifera at the ocean surface disappeared – or changed somehow – on their descent into the 3,000 fathom range. Curiously, the greater depths also seemed to be absent of shell-secreting creatures, too. This led the *Challenger* staff to conclude that, "the red clay [was] essentially the insoluble residue, the *ash*, as it were, of the calcareous organisms which form the Globigerina ooze after the calcareous matter has been by some means removed."[485] Buchanan suggested that the tiny creatures dissolved into carbonic acid from falling so far into the water.

Buchanan then turned his chemistry skills upon Huxley's primordial ooze. After departing Hong Kong, he tried to heat sea floor water in order to isolate the deep-sea protoplasm. To his surprise, he found no organic matter in the water sample. Buchanan returned to the samples themselves to solve the mystery. The crew had yet to find a live sample of *Bathybius* from their dredging. When analyzed, Buchanan found that the ooze was actually a calcium sulfate precipitate from the mixing of saltwater, floor sample, and alcohol. Huxley's evolutionary common ancestor was a human artifact, a mistake. The *Urshleim* samples became jars of a simple, chemical mixture.

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[484] Rhebock, "*Bathybius*," 508.

[485] Wyville Thomson, "The 'Challenger' Expedition II," *Nature* 11 (1874): 117.

Murray was shocked by the discovery, like many of his contemporaries. Murray had previously seen the samples take to life himself as he prepared them on board for Huxley. Thomson wrote their observations to Huxley, hoping to still find evidence of *Bathybius* in the deep. Huxley, on the other hand, was more willing to accept defeat. He publicly apologized for the mistake and distanced himself from the ooze.[486] His forward admission of the blunder was in line with Huxley's critical character, but nonetheless painful for a man so aimed at challenging others; he wrote to a personal correspondent regarding the incident not long after, "I shall eat my leek handsomely if any eating has to be done."[487] Some naturalists could not believe that Huxley had been wrong on such a fundamental evidence for evolution. However, without the organism's "discoverer" to defend it, *Bathybius* faded – for the most part – by the next century.

The *Challenger* crew had eliminated one piece of evidence for natural selection by removing *Bathybius* from the zoological record. However, naturalists still waited for news regarding the "living fossils" to be found in the deep ocean. Even the naval crew keeping the *Challenger* sailing was vaguely aware of the ship's evolutionary mission and its connection to Darwin's missing links. Thomson had given a lecture to the naval crew regarding the research that was to be done while aboard.[488] Using very simple terms, Thomson explained why naturalists believed that no life could exist below 300 fathoms and the practices that had changed that thought, "The first dredging in deep water with anything like success was obtained by the Americans. A clever lieutenant in their navy invented an instrument similar to the one we now use in sounding, whereby a very small

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[486] TH Huxley, "Notes from the 'Challenger'," *Nature* 11 (1875): 316.

[487] Leonard Huxley, *The Life and Letters of Thomas Henry Huxley* (New York: D. Appleton & Co., 1901), 480. The quote may also be found in Rehbock, "*Bathybius*," 529.

[488] See Philip F. Rehbock, ed., *At Sea with the Scientifics: The* Challenger *Letters of Joseph Matkin* (Honolulu: University of Hawaii Press, 1992) for an account of the expedition from the perspective of the naval crew.

portion of the bottom could be brought up." The crew then had the chance to see some of the strange creatures that had been captured by the *Challenger* "scientifics." The sailor who took notes on Thomson's lecture regretted that they did not continue. [489]

The *Challenger*'s evolutionary mission did make its way into sailor's rhyme. Thomson "forwarded" a few versus from "Jack Skylight," likely written by Thomson himself, regarding the expedition's objectives:

> Don't yer see these learned bosses have come to search the ocean,
> But for what, old son, 'twixt you and I, I'm blow'd if I've a notion...
> Of course you know they've got to find the link atween the species,
> Some say as there's a coon aboard as liks it all to pieces...
> They seems to me to make a deal and show a great surprise
> At things we've seen, Bill, many times, when first they meet their eyes.
> Perhaps it's 'cos the things's alive their fancies somewhat tickle,
> They only having seen them home screwed up in brine or pickle.
> …
> A scientifick genelman, our Genius on the cruise,
> Explained to us the hanimals, their habits, and their use;
> I don't tumble to it much; but, Bill, he spun a yarn
> About the objeck of the cruise which I was glad to larn.
> He said 'twas for the good of man to raise him summat higher,
> Since it was proved by some one that a monkey was his sire;
> I don't see how it follers – but he sed from wat he found
> There was fields of blazing sea weed below upon the ground;
> And every little blessed thing we druge out of the sea
> Was for the good of all mankind, including u and me...
> He told us that we'd all be dooks [dukes] when this 'ere cruise is done;
> I think he was mistaken, or he meant he would be one...[490]

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[489] SIOA, "Matikin Papers," Manuscripts 2. San Diego, California. To some extent, Thomson marshaled British naval pride in attributing the scientific success to the Americans. However, he also truthfully reported the exchange of seabed scientific practices from American surveyors during the 1850s and 1860s. The admission of the American origins of deep-sea practicesalso had the bonus effect of being an inspirational aspect of the story; a naval lieutenant, like anyone in the audience might have one day become, could change scientific history.

[490] Jack Skylight, "Notes from the 'Challenger,'" *Nature*, 9 (1874): 304-305. The rhyme was most likely not written by a crew member, though it is possible. The sentiments found in the longer version correlate to those found by Rehbock in his study of the Challenger naval crew towards the "scientifics." Thomson also translates one nautical word for the *Nature* readers, among other potential evidence of naval authorship. However, the reference to the lecture exceeds the material covered by Thomson according to historical records.

The rhyme includes phrasing on the “link atween the species” and a man that proved “a monkey was his sire,” which are both references to Darwin and his living fossils. The longer verse also references the topography of the sea floor and “fields of blazing sea weed below upon the ground,” the other two subjects of biological investigation given to the *Challenger* crew. While whimsical, the verse accurately captured the connection between natural selection and the *Challenger*'s activities.

Thomson did not share his analysis of the abyssal fauna and its relationship to natural selection immediately after the expedition returned. When the *Challenger* finally docked at Spithead on 24 May 1876, Thomson addressed a number of groups regarding the results of the voyage. For years, he did not comment on his conclusions regarding evolution. Even in 1878, when addressing Section E [Geography] of the BAAS as its president, Thomson carefully remarked:

> The abyssal fauna is of a somewhat special character, differing from the fauna of shallower water in the relative proportions in which the different invertebrate types are represented... It is a rich fauna, including many special genera, and an enormous number of special species, of which we, of course, know as yet only a fraction; but I do not think I am going too far in saying that from the results of the *Challenger* expedition alone the number of known species in certain classes will be doubled. The relations of the abyssal fauna to the faunae of the older Tertiary and the newer Mesozoic periods are much closer than are those of the faunae of shallow water; I must admit, however, that these relations are not so close as I expected them to be, - that hitherto we have found living only a very few representatives of groups which had been supposed to be extinct. I feel, however, that until the zoological results of several of these later voyages, and especially those of the *Challenger*, shall have been fully worked out, it would be premature to commit myself to any generalizations.[491]

Thomson's reticence to make immediate “generalizations” regarding natural selection can be attributed mostly to the circumstances he encountered upon his return. He was

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[491] Wyville Thomson, “Presidential Address,” 621.

inundated with organizing the *Challenger* Reports, controlling and distributing the specimens along his world-wide network, and answering requests for information. Even Alfred Russel Wallace, the co-discover of natural selection, was keenly interested in "the geographical distribution of deep water forms." Thomson had to ask him to wait for the *Challenger* Reports, like everybody else. [492]

Though Thomson did not comment on natural selection through generalizations, he did communicate his evolutionary findings through a different medium. He folded his evolutionary thoughts into minute observations of the *Challenger*'s stalked crinoid species. The crinoid specimens – and the questions that other philosophical naturalists associated with them – influenced Thomson's conclusions on natural selection. In turn, the politics surrounding the *Challenger* specimens later determined how he would communicate his findings.

Thomson and the Challenger Stalked Crinoids

Naturalists had commented upon the lack of evidence for intermediate forms before the *Challenger* expedition. This concern was not the only argument against natural selection, as discussed by many other historians.[493] However, the concern was fairly widespread among those naturalists that were willing to accept Darwin's argument to some extent. This group enjoyed a majority within the younger scientific elite.[494] Many accepted natural selection. However, some were convinced of evolution, but unwilling to

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[492] BLMS, "correspondence 11 May 1876 from Wyville Thomson to AR Wallace," ADD MS 46435, London.

[493] See David Hull, *Darwin and His Critics The Reception of Darwin's Theory of Evolution by the Scientific Community* (Cambridge: Harvard University Press, 1973).

[494] Consider my previous comment on how Huxley joked about the BAAS meeting being too full of Darwinists.

accept Darwin's mechanism for evolution without more proof. Thomson believed that Darwin and Wallace had created a hypothesis for evolution, not a complete theory.

This sentiment was captured in a number of contemporary scientific discussions. For example, one naturalist, replying to a discussion on Wallace's support for evolution, wrote to *Nature*;

> ...Natural Selection is insufficient to explain the “Origin of Species,” and that, rather, the origin of the variations of which Natural Selection is said to avail itself must be looked to for this purpose...
>
> One of the objects of Mr. Darwin has been to show that the existence of species as an absolute entity is a mere idea of our minds; that if we could at the same moment look around us in space, and also backwards in time, we should find the organic world together as one whole, one great mass of beings extremely closely applied to each other, and distinguishable only by an accumulation of small and perhaps scarcely appreciable differences. A second and closely-connected object has been to show that this great mass of beings has had a common origin from one primeval ancestor (or at most a few ancestors). These two points are the chief ones involved in the “origin of Species” question, *as it is ordinarily understood*; and if they be borne in mind, it will be seen that the doctrine of “Natural Selection...” deals with only a small portion of the numerous problems involved in this great question...
>
> [Yet we don't see innumerable gradations], one of the first questions suggested by it is, where are the connecting links? This first question has never yet been answered to any extent, or with anything like adequacy. The links produced are but few, and not sufficient to bear the great weight attached to them. For at no period of the geological record do we find any traces of the general and intimate connection of beings with one another that Mr. Darwin's view would lead us to look for.[495]

The correspondent concluded that a larger doctrine of evolution was needed to explain the differences among the species. Naturalists based the common conception of natural selection, “*as it* [was] *ordinarily understood*,” upon two scientific points that required proof. The first point was that species could be understood in the context of time *and*

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[495] D[avid] Sharp, “Letter to the Editor,” *Nature* 3 (1870): 67. Emphasis original. The letter was almost certainly written by David Sharp, the entomologist.

space.[496] Thomson envisioned the deep sea as an ideal geography in which to explore spatial distribution of species over time; it was vast and virtually timeless. The unique geography of the deep sea would allow creatures to spread over vast space and adapt to any underwater conditions that they found there. The relationship to the existing deep-sea fauna and the geological record would show Darwin's intermediate forms – if his hypothesis was correct.

The correspondent also mentioned the second proof for natural selection that naturalists expected: the existence of a common, primordial ancestor. While unknown when the author penned his letter to *Nature*, the *Challenger* expedition would fail to provide both these forms of evidence that naturalists needed to continue their support of natural selection. Naturalists were eventually denied the second evidence for natural selection when Buchanan disproved *Bathybius*. Thomson would explore – and eventually declare against – evidence for intermediate forms. He already associated the stalked crinoids with intermediate morphology during his visit to the Norwegian naturalist Michael Sars, when Sars showed him a living specimen of *Rhizocrinus*. And while Thomson neither supported nor denied Darwin's “living fossil” evidence, he portrayed the stalked crinoids as intermediate species long before the *Challenger* set sail.

Thomson had dwelt on the crinoids as evidence of intermediate forms and divergence even before the *Challenger* set sail. In April 1871, Thomson presented a paper before the Royal Society “On The Structure of the Palaeozoic Crinoids.” This paper examined – in great detail – the relationship between the stalked crinoids' morphological features and those of other crinoids. Thomson included both extinct and

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[496] Zoogeologists, especially Edward Forbes, had previously established that species existed in both time and space. That acknowledgment led Forbes to advocate for the theory of specific centers. See chapters one and four regarding the history of species in time and space and its relationship to the theory of specific centers.

extant crinoids in his analysis. He compared the *Pentacrinus* and *Antedon* genera first. He noted that the *Pentacrinus* and the *Antedon* resembled each other, except that the *Pentacrinus* attached itself to the sea floor by means of a long stalk; the *Antedon* had no such appendage.

One by one, Thomson showed how each crinoid type in the study shared structural similarities, or were morphologically "allied," to other crinoids. However, the crinoid types were allied only to one other specific group. The ancient crinoids shared features with the more recent crinoids, but modern crinoids did not necessarily share those features with each other. For example, the *Hyponome sarsii* shared a body structure similar to the *Antedon*, but did not resemble the Cystideans. Nonetheless, "It [the *Hyponome*] has... the same arrangement as to its internal radial vessels and mouth which we find in the older crinoids. It bears the same structural relation to *Antedon* which *Extracrinus* bears to *Pentacrinus*."[497] Without stating so explicitly, Thomson arranged each stalked crinoid into an ancient lineage of morphological intermediacy.

Thomson's analysis of the crinoids diverged significantly from previous zoogeologists and is noteworthy for its novel evidentiary practices. Like other zoogeologists, Thomson shared a belief that the crinoids could illuminate the order and relationship between the species. Their status as ancient and simple organisms gave them this attribute; Forbes had used the crinoids and cystoids in a similar manner earlier in the century. Thomson also used a similar practice to retrieve the crinoid specimens, although his dredge went much deeper than Forbes' did. Thomson was the benefactor of new deep-sea dredging practices, the same sounding practices that opened the deep-sea floor to scientific investigation. Regarding the crinoid specimens, however, Thomson

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[497] Wyville Thomson, "On the Structure of the Palaeozoic Crinoids," *Nature* 4 (1871): 496-497.

was unwilling to start from general principles. Instead, he used minute morphological features to build a map of relationship between each crinoid type. His preference for minute observations instead of extrapolations from general principles differed greatly from the zoogeologists that preceded him, such as Edward Forbes.

In November of 1871, five months after the Royal Society paper regarding the stalked crinoids, Thomson delivered the opening lecture in Natural History at the University of Edinburgh, titled “The Relationship Between Zoology and Palaeontology.” In his lecture, Thomson placed himself in the same lineage of Edinburgh zoogeology as Forbes. He lauded Forbes' scientific insight and method:

> ...although recent investigations with better appliances and more extended experience have invalidated many of his conclusions, to Forbes is due the credit of having been the first to treat these questions in a broad philosophical sense, and to point out that the only means of acquiring a true knowledge of the *rationale* of the distribution of our present fauna is to make ourselves acquainted with its history, to connect the present with the past. This is the direction which must be taken by future inquiry...[498]

Thomson then continued to summarize Forbes' theories on new species, primarily those of “specific centers” and “representation.”[499] These principles led Forbes to believe in the immutability of species over time, though variation did occur within the species itself.

The latter half of his lecture discussed evolution and natural selection. Thomson was clearly supportive of evolutionary theory, “I do not think that I am speaking too

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[498] Thomson, “Zoology and Paleontology,” 34.

[499] Thomson, “Zoology and Paleontology,” 35, Thomson outlines Forbes’ law of representation as “…far removed… where the conditions of life are similar, species and groups of species occur, which, though not identical, resemble one another very closely…” He goes on to say that the same was true of fossil species, too. These representative organisms, because they were removed by distance, could not be explained by descent if the law of specific centers was true. There is also the implication the representative species are related to the similar conditions in which they are found.

strongly when I say that there is now scarcely a single competent general naturalist who is not prepared to accept some form of the doctrine of evolution." He also recognized that natural selection as the primary mechanism for evolution was still hypothetical, "...and many are inclined to believe that some law, as yet undiscovered, other than the 'survival of the fittest' must regulate the existing marvellous system of extreme and yet harmonious modification."[500] While variation could act as *vera causa* of evolution, the circumstances of living in any environment would limit the extremes of variation. Ultimately, Thomson asked why variation should diverge into two species when environmental pressures would favor one extreme variation over another. The elucidation of that question would require the study of an ancient seabed, the Silurian rocks.

Thomson dedicated himself to the testing of Darwin's hypothesis, descent with modification. He was somewhat skeptical. He ended his lecture with the primary reason for this skepticism:

> During the whole period of recorded human observation, not one single instance of the change of one species into another has been detected, and, singular to say, in successive geological formations, although new species are constantly appearing, and there is abundant evidence of progressive change, no single case has as yet been observed of one species passing through a series of inappreciable modification into another.[501]

Fossil evidence obviously showed that new species appeared in the geological record. The crinoids that he observed also showed evidence of progressive change over time. However, the relationship between one crinoid and its allied type seemed to be in only one direction, not branching into two.

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[500] Thomson, "Zoology and Paleontology," 35.
[501] Thomson, "Zoology and Paleontology," 35.

It may seem contradictory for Thomson to accept the crinoids as changing progressively through time and yet remain skeptical of natural selection. If one type of stalked crinoid showed intermediacy with another stalked crinoid, was this not enough evidence to claim variation as *vera causa* for Darwin's hypothesis? In one sense, yes. Thomson accepted evolution. He also accepted variation as its potential *vera causa*. However, Thomson's continued skepticism must be seen in the context of changing evidentiary practices.[502] Thomson interpreted Herschel's concept of *vera causa* in a distinct manner. He demanded that the hypothesis of variation be confirmed as a plausible and observable mechanism. He predicted the effects of variation and divergence over the deep-sea landscape and insisted that he be able to test to see whether his observation matched that prediction. Though Thomson could accept variation as a *vera causa*, he was not willing to generalize a potential cause into a law of nature until he had seen more comparative, biogeographical evidence.

Thomson's attitude towards Edward Forbes also demonstrated the same style of scientific reasoning. He obviously valued Forbes appreciation for the investigation of species in space and time, going so far as to claim that investigation of the natural world should continue along that method in his lecture. However, Forbes' had fallen prey to premature generalization, especially in the case of his azoic zone theory. Forbes' willingness to extrapolate a few observations into a general law, such as the limit of life being 300 fathoms underwater, from only a few observations led to flaws in his conclusion. Forbes had not actually dredged below 300 fathoms. Indeed, the azoic zone theory was on the verge of being disproven by Thomson himself when he gave the

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[502] See chapter one of this dissertation. I argue that the elements Thomson blended were the specimens valued by the zoogeologists and the predictive method of deriving natural law from minute observations characteristic of astronomers and tidologists. Darwin also blended these two evidentiary practices together.

Edinburgh lecture. While increasingly harsh conditions of life under deeper water might be *verae causae* for the diminution of life, it did not entail that no life existed below 300 fathoms. Thomson wanted proof of species divergence. That proof should come in the form of Darwin's living fossils, but he needed that evidence to come from biogeographical specimens. He would dredge up a variety of intermediate species from the deep sea or he would reject natural selection by gradual modification.

In this way, Thomson abandoned Forbes' method of deriving universal truth claims from his marine invertebrate specimens. He, instead, adopted the methods more akin to the Cambridge scientific network. He would make a prediction instead of a generalization. His prediction would not be numerical, like an astronomical calculation, but rather it would be morphological and zoogeological. Cambridge astronomers had used a similar prediction when establishing the existence of Neptune; they calculated the difference from the observed position of Uranus and its theoretical position. The Cambridge network then used that comparison to find the exact location of a new planet, Neptune, in the night sky. Similarly, Thomson predicted what morphological features he would find in the geological record and compared that observation to the extant faunae of the sea floor. Like the Cambridge astronomers turning their telescopes to the sky to see if their calculations were correct, Thomson would release his dredge into the sea to either observe – or not observe – the insensible gradation of one species into another. He did exactly that while aboard the *Challenger* in 1872*,* one year after the lecture.

Thomson confirmed his criticism of Forbes' scientific reasoning in his next paper on crinoids, written while aboard the *Challenger*. Specifically, Forbes' lack of dredging below 300 fathoms, but speculating upon what lies at that depth, kept him from discovering the deep-sea fauna. In this case, the crinoid specimens illustrated Forbes' fault in scientific reasoning:

> As early as 1850... Dr. Michael Sars, had challenged Edward Forbes's conclusions respecting the bathymetrical terminus of animal life. He remarked, that at least in the Norwegian Seas, it appeared to extend much beyond the limit which the English naturalist had fixed for it. Forbes had not dredged below 230 fathoms, and at this depth he had only obtained two living Mollusca and a couple of Serpulæ; hence he was led to place the zero animal life at 300 fathoms. Sars, on the contrary, even at the early period just mentioned, had obtained from a depth of 300 fathoms, a number of animals, including a species of Coral, Molluscs, Polyzoa, &c.; and he sagaciously remarked that there was evidence of the existence of a *vigorous* animal life at this great depth...[503]

Sars' wisdom in dredging at 300 fathoms and below had revealed valuable specimens for science, including the *Rhizocrinus*.

Thomson reported some of the other prizes that the Sars family had pulled from the deep, such as the *Rhabdopleura*. The paper drew upon Sars' scientific notes, not his own, so he chose to not criticize interpretations of the evidence. However, he recognized that the specimens were very valuable to the investigation of evolution. What interested him about this curious creature, again, was its morphological affiliation with the other crinoids. It was similar to its allied species, but only in a few aspects. Thomson wrote to *Nature*:

> Without entering into minutiæ, I shall endeavour to describe briefly the characteristics which mark out the *Rhabdoplura* as unique, and invest it with so high an interest, not only for the student of the Polyzoa, but also for the philosophical biologist. In the first place, it may be stated broadly that we find in this form the Polyzoan type in a rudimentary and half-developed condition. It clearly represents a very early stage in its evolution, if evolution be the method of Nature. The points which separate it most strikingly from its congeners are not the equivalent of the ordinary

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[503] Wyville Thomson, "On Some Remarkable Forms of Animal Life from Great Deeps Off the Norwegian Coast," *Nature* 8 (1873): 189.

> differences that occur amongst the members of the same class; they might rather be regarded as surviving features of another and very different type, from which it has diverged, and are strictly transitional in character.[504]

Sars' dredging below the 300 fathom line yielded specimens valuable to “philosophical biology,” and therefore the search for natural law. Specifically, the trait that gave the *Rhabdopleura* its value was its transitional morphology. The creature seemed to be an early common ancestor to the Polyzoans and another class of creature, the Hydroids; “In a word, two types of structure seem to blend in this remarkable animal, one, as it were, fading away, and the other dawning.”[505] Thomson's description was, again, accompanied with the morphological minutia that he promised to spare the reader.

Even when the *Challenger* returned from its voyage, Thomson's immediate act was to deliver two papers on the *Challenger* dredgings; the *Challenger* docked on 24 May 1876 and Thomson delivered his crinoid paper before the Linnean Society on the first day of June. His research emphasized the crinoids and their relationship to other species. Thomson also framed the results of the *Challenger* expedition at the Glasgow meeting of the British Association for the Advancement of Science in September of the same year. Of the initial conclusions found from the voyage, “the third, which [was] perhaps the most interesting and curious, [was] the nature and distribution of the peculiar races of animals which are now found at the bottom of the sea.”[506] Thomson confirmed that he believed that fossil forms had been trapped continuously at certain points of the seabed since the creation of the oldest chalks.[507] That continual creation of fossil forms since the most ancient of geological areas would have created a complete

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[504] Thomson, “Norwegian Coast,”, 190.
[505] Thomson, “Norwegian Coast,” 189.
[506] Wyville Thomson, “The Challenger Expedition,” *Nature* 14 (1876): 493.
[507] Thomson, “The Challenger Expedition,” 495.

geological record needed to test Darwin's theory against extant fauna. Thomson sketched, very briefly, his impressions of this test:

> It was our impression that when we examined this fauna we should find it very analogous to that of the ancient chalk, for we believed, and we believe still, that the deposition of chalk has been going on continuously in various parts of the ocean, from the chalk period to the present time. In this expectation we were to a certain extent disappointed, for the species found in the modern beds are certainly in very few instances identical with those of the chalk or even with those of the older tertiaries. But although the species, as we usually regard species, are not identical, the general character of the assemblage of animals is much more nearly allied to the cretaceous than to any recent fauna.[508]

The deep-sea fauna was, generally, distributed universally throughout the globe. However, he believed that the deep sea had geographical spots where ancient fauna had become trapped due to various causes; the sea floor might have created a deep valley around the area or the surrounding environment might have changed, for example.

The distribution of abyssal species, according to Thomson, would unlock the question of the origin of new species. The widespread distribution of the same species across the globe offered a unique opportunity to study how species interacted with each other, their environments, and possibly evolved over space. Thomson's last sentence of his BAAS paper sketched the opportunity afforded to naturalists because of the unique geographical features of the deep sea:

> The fauna of the deep sea is wonderfully uniform throughout. No one who has once seen it can fail to recognise this general uniformity... and yet, although in different localities the species are evidently representatives, to a critical eye they are certainly not identical, and I believe that one of the most important lines of inquiry which have been opened up to us by these investigations is the range and amount of variation, or possibly the

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[508] Thomson, “The Challenger Expedition,” 495.

> passage of one apparent species into another over this vast area, remoteness in space being, when we consider the conditions of migration with the accompanying change in surrounding circumstances, equivalent to lapse of time.[509]

Here, Thomson called upon the zoogeological method of investigating species in both space and time. When he compared the ancient, geological fauna to the recent fauna, he found that the species had indeed changed over time. This evidence supported a general belief in evolution, though he did not explicitly state that the evidence still didn't support divergence; the fossils, when compared to living fauna, showed a slow change into other species, but not through divergence. A finer understanding of *how* evolution functioned could be learned through studying the way that species spread out across the vast surface of the sea floor. Thomson used the spatial aspect of Forbes' specific centers theory to see if species diverged over time due to natural selection. He would sort through the specimens that ranged across vast geographical space to see if they demonstrated continual intermediacy by extreme modification.

Thomson wanted to know if any of his deep-sea specimens showed intermediacy in three directions; a common ancestor, a more recent form, and – most importantly – a second form that had diverged through extreme modification. That crucial fourth specimen, the alternative form that sprung from its parent, would demonstrate that morphological variation did not have limits and that natural selection by modification occurred. These “fourth specimens” should be quite common over the vast geographical space of the ocean if natural selection was the major cause of evolution.[510] This was exactly the type of living fossil naturalists expected to find as a result of Darwin’s language in *On the Origin of Species*. However, despite Thomson's early publications on

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[509] Wyville Thomson, “The Challenger Expedition,” *Nature* 14 (1876): 495.

[510] Recall how Thomson had interpreted the *Rhabdopleura*.

the subject, he was unable to continue his work on morphological intermediacy right away. Political matters regarding the *Challenger* specimens would complicate his research.

The Politics and Burdens of Scientific Administration

Thomson's life became much busier – and more complicated – once he settled in England. Queen Victoria knighted him on 27 June of 1876 in recognition of his scientific leadership aboard the *Challenger*.[511] He returned to Edinburgh soon afterward to begin sorting, cataloging, and distributing the expedition samples for scientific analysis. The Admiralty had agreed that Thomson would keep the specimens until the expedition results had been published. After, the specimens would become property of the British Museum. Thomson had a five-year grant with which to publish the expedition report. He also had the esteem of naturalists all over Great Britain for a successful scientific circumnavigation. That excellent start would soon turn into a political mess.

Problems surrounding the *Challenger* specimens began when the British Museum demanded the *Challenger* materials be given to them immediately. Thomson managed to retain the marine specimens at Edinburgh, though the other specimens were surrendered to the Museum. He accomplished this partial victory through his alliance with Joseph Hooker, then President of the Royal Society, and TH Huxley, who had a large hand in arranging the expedition.[512] Thomson was free to begin distributing the marine specimens throughout the international network of experts in each field of

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[511] *The London Gazette*, June 30, 1876, 3731. Sir Wyville Thomson was knighted at Windsor Castle.

[512] Deacon, *Scientists and the Sea*, 366.

zoology. Most of the samples would stay in Great Britain. However, this network of experts stretched across the Atlantic and Thomson believed that the samples should go to the most qualified individual for analysis. This kind of distribution was common practice. Seabed science stretched across the Atlantic; its ideas – as well as the naturalists who produced those ideas – moved frequently across the ocean for the entire nineteenth century. He had also made an arrangement to work on the *Challenger* collections with his American colleagues. The specimens were packaged to be distributed to his network of marine biology experts. For example, Thomson intended to send the echinoderms to Alexander Agassiz at the Harvard Museum of Comparative Zoology, while the jellyfish would go to Ernst Haeckel in Germany. He would keep the crinoids for himself in Edinburgh.[513]

More political problems arose regarding the specimens. When news of the distribution crossed the Atlantic from the United States, a group of naturalists vehemently objected to the *Challenger* specimens leaving England. Thomson had used national pride to mobilize the *Challenger* expedition; he had continually stated that British naturalists had to keep up with American deep-sea science in his requests for resources. Now that the United Kingdom had its scientific prize, the deep-sea collection, this group was incensed that the specimens would be sent to non-British naturalists. P. Martin Duncan, President of the Geological Society, sent a letter to Thomson stating that others had charged him with securing the *Challenger* samples for British naturalists.[514]

Duncan could have been motivated by many reasons to keep the samples for British science. As already mentioned, Thomson had deployed national competition to gain support for the expedition; giving the specimens to competitors seemed ridiculous

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[513] Deacon, *Scientists and the Sea*, 367.
[514] Deacon, *Scientists and the Sea,* 367.

after such a political maneuver. Historian Margaret Deacon has also claimed that few people studied deep-sea marine zoology in England before the *Challenger* expedition and so when the expedition returned, the naturalists were eager to be a part of the growing field.[515] On the other side, a number of British scientific networks had obvious prior interest in marine invertebrates; the Edinburgh zoogeological network alone – now distributed across the United Kingdom through the British Association – had a number of naturalists who would be delighted to gain access to the *Challenger* collection. Thomson's campaign had convinced naturalists that deep-sea marine invertebrates could unlock the greatest of life's scientific mysteries, the mechanism for evolution and the origin of species. Why spread such valuable specimens to other naturalists when British science – and Britain's naturalists – could have the glory of discovering a great law of nature? Duncan was understandably upset when Thomson replied that he would not change his scientific plan for a number of unnamed discontents.

Thomson wanted to send the specimens to those most qualified to handle them. He knew that the larger network of seabed naturalists was distributed across the Atlantic and he was willing to fight Duncan on this issue.[516] He verged on an important scientific discovery; he wouldn't let petty national jealousy interfere with his scientific practice. Duncan would not accept this explanation. He accused Thomson of neglecting his distinguished British colleagues.[517]

The Darwinian network of seabed naturalists sprang into action to support Thomson's decision to distribute the specimens. A number of them – including Hooker, Huxley, Darwin, Carpenter, and other X Club members – submitted a letter defending

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[515] Deacon, *Scientists and the Sea,* 367.

[516] Much of this account has been reported by Deacon in *Scientists and the Sea*. I offer only a different context for the events that transpired between Duncan and Thomson.

[517] Duncan, *Scientists and the Sea,* 368.

Thomson to *Nature*. The letter claimed that Duncan was acting with inappropriate hostility for a man of science. The specimens should be given to those established naturalists who would examine them with specialization and thoroughness due valuable objects of science:

> Now everyone who has kept himself en rapport with recent zoological research, must know that the foreign zoologists, to whom Sir C. Wyville Thomson has intrusted these collections, stand before all others in the amount and thoroughness of their work in the special departments of zoology for which their aid is asked, and the narrowest nationalism cannot deny that it was the duty of the director to see that the specimens were placed in the hands of men most competent to secure for science the results which have been obtained at the cost of so much labour, skill, and public expenditure.[518]

The Darwinists, in one sense, defended someone they recognized as an influential researcher of evolutionary theory. Thomson was one of them, though he did not attend X Club meetings. Darwin, Huxley, and Carpenter all shared a direct connection to the Edinburgh seabed science network; and all of them – including Hooker – had been involved in the biotic debate from an early stage.[519] Thomson, like Huxley, had connected their interest in evolution with their expertise in deep-sea biology and the biotic debate. Thomson had already made several important discoveries on both fronts.

The letter writers also shared Thomson's conviction that their scientific network was international; members of their group could be found in America, England, Scotland, and Germany. It was not the individual's nationality that mattered most, but their integration in the established deep-sea biology network. Their argument against British nationalism followed their conviction:

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[518] Joseph Hooker, et al., “The Challenger Collections” *Nature* 14 (1877): 118.

[519] See chapter two for Darwin's connection to the Edinburgh seabed science network. See chapter four about Hooker and Huxley's connection to the British biotic debate.

> If this country [Great Britain] can be shown to enjoy the unique distinction of possessing in every department of zoological research men at least as good as can be met with elsewhere, the advocates of a national science may find an argument in favour of having the work absolutely confined to Englishmen; but if we cannot assume a position which no other nation in the world would think of claiming, it is plainly for the interests of science that we should supplement from abroad those departments of research in which foreign workers may excel us.[520]

British science could at best join the vast network of competent marine zoologists, like any other nation. The network of seabed science was, by nature, a transatlantic community.

The *Challenger* collections could easily have stayed only in the United Kingdom. Thomson met with resistance for his decision from the onset of his research; both the British Museum and Duncan, if successful, would have kept the samples within the British Isles. Thomson and the other marine biologists fought twice to circulate the specimens around the Atlantic. More revealingly, they were willing to concede on the terrestrial collection, which remained at the British Museum. Thomson's colleagues fought specifically and explicitly so the marine invertebrate specimens would *remain* transnational objects – objects that would be identified beyond national borders.

The Darwinists' strategy worked. The debate settled down over the next couple months, though the fight did not fade from memory. Individual naturalists remained sensitive about their national status for many years. For example, Alexander Agassiz, Louis Agassiz's son and associate of the Coast Survey, believed that Duncan criticized his work on the *Challenger* specimens based on the conflict.[521] Agassiz wrote to Thomson about one of Duncan's articles; he despised the "*tone* of superiority" shared by

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[520] Hooker, "The Challenger Collections," 118.

[521] Deacon, *Scientists and the Sea*, 368.

the British naturalists who had fought to keep the *Challenger* specimens local.[522] American naturalists had been at the forefront of seabed research for most of the nineteenth century. He was glad to see the subject grow in prestige across the Atlantic, but he disliked the condescension shown by some more recent English seabed naturalists.

American naturalists also engaged in their share of scientific nationalism regarding deep-sea biology. Richard Rathburn, an associate at the Smithsonian, wrote an 1884 article published in both *Science* and *Nature* titled, “The American Initiative in Methods of Deep-Sea Dredging.” The paper lamented the fact that British accounts of oceanography were better known despite America’s history of exceptional dredging and sounding research. Rathburn asserted that “American naturalists have not received the credit which is their due, either at home or abroad; and much of the honor that justly belongs to them has passed into other hands.”[523] Many British accounts of the history of oceanography following the *Challenger* began to omit the American origin of their deep-sea scientific practices. This historical erasure may have been political strategy resulting from the Anglo-American *Challenger* conflict. Nonetheless, the rapid increase of interest in the *Challenger*'s results strained the long-standing international relationship between naturalists across the Atlantic. The community of seabed biologists, however, remained committed to its international origins.[524]

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[522] Deacon, *Scentists and the Sea*, 368. Correspondence Alexander Agassiz to Wyville Thomson, 29 December 1879.

[523] Richard Rathburn, “The American Initiative in Methods of Deep-Sea Dredging,” *Science* 4 (1884): 54.

[524] Margaret Deacon, “Some Aspects of Anglo-American Co-operation in Marine Science, 1660-1914,” in *Oceanography: The Past* eds. M. Sears and D. Merriman (New York: Springer-Verlag, 1980), 101-113.

Despite the complications, the decision to circulate the *Challenger* specimens ensured that seabed science remained a transnational pursuit. Another round of exchange of specimens and naturalists began, this time originating from Edinburgh and radiating outward across the oceans. The collections also attracted naturalists from other countries to the University of Edinburgh as collegial visitors.[525] As these specimens circulated, naturalists negotiated and merged their local scientific practices in the pursuit of universal claims about the natural world. However, since the last major exchange directly preceding the United States Civil War, the samples now carried the potential for solving the question of natural selection. The burden of administering this network of exchange and negotiation had already caused political conflict for Thomson. The effort spent cataloging and shipping the physical specimens also took its toll on the expedition director.

Thomson's health failed not long after his return to Edinburgh; he was expected to manage the vast network of research and publication resulting from the expedition while also resuming his university duties. In June of 1879, Thomson suffered a stroke that left him temporarily paralyzed and from which he recovered very slowly.[526] Nonetheless, work on the *Challenger* Reports continued. Even in his weakened state, Thomson pushed onward with his ruminations on evolution and natural selection.

Dissension from Within Darwin's Ranks

On 10 July 1880, Huxley used the twenty-first anniversary of *On the Origin of Species* to contemplate evolution and natural selection. In a lecture to the Royal

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[525] See Deacon, *Scientists and the Sea*, 369.

[526] Deacon, *Scientists and the Sea*, 371.

Institution, he spoke of the conflict that Darwin's publication had caused. Yet the book had weathered its early years. The general tone of Huxley's article was triumphant; Darwin's ideas had survived the test of time and matured into a thing to be respected. *The Origin of Species* was not without its critics, however. Huxley acknowledged that the types of criticisms *The Origin of Species* had recently changed in the previous two years:

> ...during the second decade of the existence of the “Origin of Species,” opposition, though by no means dead, assumed a different aspect. On the part of all those who had any reason to respect themselves, it assumed a thoroughly respectful character. By this time the dullest began to perceive that the child was not likely to perish of any congenital weakness of infantile disorder, but was growing into a stalwart personage...[527]

Huxley recognized that evolution and natural selection had yet to persuade all naturalists. Nonetheless, the recent criticisms of evolution were of a more respectful and respectable nature. In a way, the theory of evolution and its critics matured together. Yet, while those criticisms remained, the theory had still come to dominate biological thought.

Huxley explained what caused the *On the Origin of Species* to be attacked when it was first published, but be widely accepted only twenty-one years later. Both of these explanations had to do with differing ways in which naturalists dealt with evidence, “Every belief is the product of two factors: the first is the state of mind to which the evidence in favor of that belief is presented; and the second is the logical cogency of the evidence itself. In both these respects the history of biological science during the last twenty years appears to me to afford an ample explanation of the change which has taken place...”[528] According to Huxley, the acceptance of uniformitarian geology, the

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[527] TH Huxley, “The Coming of Age of the Origin of Species,” *Science* 1 (1880): 15. This article was a print of a lecture given by Huxley at the Royal Institution on 19 March 1880.

[528] Huxley, “Coming of Age,” 16.

belief that present and observable processes caused geological formations, had caused the new “state of mind” which allowed naturalists to accept Darwin's theories. Uniformitarian geology changed the types of evidence that naturalists would expect in biology, “No physical geologist now dreams of seeking outside the ranges of known natural causes for the explanation of anything that happened millions of years ago... The effect of this change of opinion upon biological speculation is obvious... if no such interruptions of the ordinary course of nature have taken place in the organic, any more than in the inorganic world, what alternative is there to the admission of Evolution?”[529] Huxley reasoned that, if only observable causes could be used as evidence, then “descent with modification” was the obvious logical mechanism for these gradual changes.

Darwin's evidence of intermediate forms, or general lack thereof, did not escape Huxley's history of evolution. The fossil record, at times, did not show the slow change of one species into two extreme varieties. When this break in the fossil record occurred, “Mr. Darwin could account for them only by supposing that the intermediate forms which once existed had become extinct.”[530] This reasoning attracted critique until the *Archeopteryx* fossil had been found that linked the reptiles and the birds. Yet, not everybody accepted Huxley's support for natural selection. Despite this obvious evidence for intermediate forms, said Huxley, some naturalists still followed with “a tirade upon this terrible forsaking of the paths of 'Baconian induction.'”[531]

If the *Archeopteryx* fossil was too complex a creature to provide evidence for intermediate forms for his audience's tastes, Huxley could turn to the *Amphioxus* and

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[529] Huxley, “Coming of Age,” 16.
[530] Huxley, “Coming of Age,” 16.
[531] Huxley, “Coming of Age,” 16.

Tunicata, which straddled the boundary between marine vertebrates and invertebrate species. The *Amphioxus*, a simple marine creature that lacked a heart, yet possessed a spine, provided evidence that the vertebrate animals had arisen from the invertebrates, “though the full proof of the manner in which the transition was actually effected may still be lacking.”[532] Huxley was willing to extrapolate the mechanism for evolution from a small number of examples. Actually seeing proof of *Amphioxus* divergence was not necessary for Huxley.

The remainder of the article was spent surveying the evidence for transitional forms found to that point. Paleontological research had yielded a number of forms that showed transition from one species into another. These fossils had convinced most naturalists that Darwin's book explained the origin of species. Overall, Huxley believed that evolution and natural selection's victory was complete; he predicted that all other explanations other than Darwin's would go extinct within a few generations:

> History warns us, however, that it is the customary fate of new truths to begin as heresies and to end as superstitions; and, as matters now stand, it is hardly rash to anticipate that, in another twenty years, the new generation, educated under the influences of the present day, will be in danger of accepting the main doctrines of the Origin of Species with little reflection, and it may be with as little justification, as so many of our contemporaries, twenty years ago, rejected them.
>
> Against any such a consummation let us all devoutly pray...[533]

Huxley's prayer was soon answered.

The *Challenger* zoological reports began to appear not long after Huxley's “Coming of Age” lecture.[534] Thomson wrote the introduction of the much anticipated

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[532] Huxley, “Coming of Age,” 17.
[533] Huxley, “Coming of Age,” 15.

series. Naturalists around the world finally received Thomson's conclusions regarding evolution and natural selection. Thomson stated his rationale for the evolutionary test very clearly: he had searched the deep sea – as a geographical space – for Darwin's living fossils.

Thomson portrayed the deep-sea floor as an expanse that would not accelerate the evolutionary process for the creatures residing there. Darwin had claimed that new environmental conditions and pressures encouraged the divergence of species. Those new conditions were most often caused by the rising of new landmass; old species would rush into the new area, compete, and adapt to the conditions there. That was the primary reason why, according to Darwin, the fossil record was incomplete. Sea-floor sinking created fossils while a rising sea floor created new species. Thomson depicted the deep-sea floor as an ancient and stable geographical site, which made it valuable for the study of life over time:

> The discovery of the abyssal fauna, accordingly, seems to have given us an opportunity of studying a fauna of extreme antiquity, which has arrived at its present condition by a slow process of evolution from which all causes of rapid change have been eliminated. A careful study of such an assemblage of forms must in time do much to throw light upon many difficult problems of distribution.[535]

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[534] The introduction to the report on the Challenger's zoological research was published sometime between 20 June 1880, the date listed on the preface, and 4 November 1880, when the publication is mentioned in *Nature*. The publication of the report is most likely closer to November than June or July. The lecture had already been delivered at that point (19 March 1880). Huxley's "The Coming of Age" lecture was published in *Nature* on 10 July 1880. While the lecture had been given, it is possible that the lecture was published right after the report. This means that it is also possible that the lecture was *published* in response to Wyville Thomson's critique. See Martyn E. Y. Low and Neal L. Evenhuis, "Dates of publication of the Zoology parts of the Report of the Scientific Results of the Voyage of H.M.S. Challenger During the Years 1873–76" *Zootaxa* 3701 (4): 407. for the range of dates during which each of the *Challenger*'s zoological reports may have been published.

[535] Thomson, "General Introduction Zoological", 50.

The deep sea had slightly differing environments, but lacked the acute effects that could be found on land. The sea floor was a vast, continuous, and slowly changing environment for species to spread across. It was the perfect place to search for intermediate species.

As the recognized expert on the deep-sea fauna, especially of the crinoids, Thomson was in a unique position to sway people's thoughts on their distribution. He also had direct access to the prestigious *Challenger* collections. Thomson did not avoid a strong conclusion as he had over the past decade; he stated his position on natural selection very clearly:

> There is every reason to believe that the existing physical conditions of this area [the abyss] date from a very remote period... I believe that the abyssal fauna... brings... powerful support of the doctrine of Evolution. However... the character of the abyssal fauna refuses to give the least support to... the evolution of species to extreme variation guided only by natural selection. Species are just as distinctly marked in the abyssal fauna as elsewhere... If all the species living on the floor of the ocean were... in a state of instability, acted upon by external influences, and perpetually passing by insensible gradations into other species, it seems certain that [they would exhibit] indefiniteness and transition. This is not the case. Transition forms, linking species so closely as to cause a doubt as to their limit, are rarely met with. There is usually no difficulty in telling what a thing is.[536]

The marine creatures gave excellent evidence of evolution over time. However, Thomson did not find Darwin's promised evidence in the deep sea; Darwinian natural selection failed Thomson's test. And it had failed during an examination of one of the zoogeologists' oldest scientific objects, the crinoids.

Huxley was the first to respond to Thomson's conclusion in the *Challenger*'s zoological reports. He published a review of the Reports in *Nature* on 4 November

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[536] Thomson, “General Introduction Zoology,” 50.

1880. He appreciated the excellent work done by those analyzing the *Challenger* collections, though he admitted to not having read all of the series. He was more interested in Thomson's introduction, which he found very readable. He also believed that, perhaps, Thomson had come to the wrong conclusion regarding his interpretation of the *Challenger* specimens. His objection targeted Thomson's depiction of the deep-sea environment as well as his thoughts on natural selection:

> That the deep-sea fauna presents us with many forms which are the dried [direct] and but little modified descendants of Tertiary and Mesozoic species is a proposition which few who attend to the evidence will be disposed to deny. But I may venture to express some doubt, whether it may not be well to keep a conclusion of such gravity and so well founded, apart from views respecting the absence of "minor local oscillations of the crust of the earth" in the area of the present great ocean basins which Sir Wyville Thomson expresses more fully elsewhere.[537]

Huxley then stated that it was quite possible that the gentle slopes of the deep sea could have thrust themselves high above sea level and then sunk again at some point. Thomson's conclusions did not necessarily follow his evidence regarding sea floor dynamics.

Huxley continued to challenge Thomson's interpretation of the deep-sea fauna itself. In characteristic fashion, he deftly addressed Thomson's claims one-by-one, though this time the tone remained genial as well as critical. He first addressed Thomson's thoughts on the limits of variation found in deep-sea species:

> ...the grounds assigned [against natural selection] are hardly so cogent as might be desirable.

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537 TH Huxley, “The First Volume of the Publications of the 'Challenger,'” *Nature* 23 (1880): 2-3. Huxley later corrected the publication of the word “dried,” and blamed poor penmanship as the cause.

> Species are just as distinctly marked in the abyssal fauna as elsewhere, each species varying within its definite range as each species appears to have varied at all times past and present.
>
> Exactly so; the abyssal species are like species elsewhere. The difficulties in the way of the application of the evolution of species by variation and selection therefore in this case cannot be greater than elsewhere. In fact, from the sentences which end the "Introduction" it seems doubtful whether they are not less than in many other cases.[538]

Huxley disagreed that the deep-sea fauna would vary differently than terrestrial fauna. This disputation combined two assumptions about the deep-sea environment; he disagreed that the ability of a species to spread indefinitely would affect the way it varied over geographical space and that the geography of the deep sea offered a unique laboratory for species variation. These two differing interpretations of the deep-sea floor's effects, Huxley's and Thomson's, followed directly into a disagreement regarding Thomson's attack on natural selection.

Huxley was still willing to admit that the deep-sea floor offered valuable evidence for natural selection. However, he believed that the marine fauna supported natural selection rather than disproved it:

> Transition forms linking species so closely as to cause a doubt as to their limit are rarely met with. There is usually no difficulty in telling what a thing is.
>
> Hence it appears that the study of the abyssal fauna has satisfied Sir Wyville Thomson that transitional forms are sometimes met with; and that, sometimes, he has found a difficulty in "telling what a thing is." And this admission is all that the most ardent disciple of Mr. Darwin could desire.[539]

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[538] Huxley, “First Volume,”.3.

[539] Huxley, “First Volume,” 3.

Huxley was satisfied that the discovery of some transitional forms, especially from the deep sea, provided Darwin's evidence for natural selection. That discovery outweighed the fact that not all species exhibited intermediate morphologies.

The primary disagreement between Huxley and Thomson resided in their differing interpretations of the intermediate forms as specimens; they argued over whether the crinoids, as living fossils, actually demonstrated the divergence that evolution by extreme variation required. Thomson believed that *most* deep-sea creatures should display intermediacy if natural selection was the primary mechanism for evolution – because of the unique geographical qualities offered in the deep sea. From previous crinoid studies, he was also worried that most of his specimens showed evolutionary changes in *only one direction*, not the divergence into *two extreme types* espoused by Darwin. For example, a crinoid type seemed to have close morphological similarity to a more ancient type and another modern type. The creature showed transition, but only from one species into another. How could all modern species come from one common ancestor if each species seemed to produce only one other species before going extinct? Another evolutionary mechanism besides natural selection had to account for divergence, if it existed. Thomson was not willing to speculate to natural law beyond his available morphological evidence.

Huxley, on the other hand, believed that *only a few* cases needed to display intermediacy in order to prove natural selection. In fact, Thomson had already presented evidence of intermediacy in his earlier crinoid papers. Those few examples demonstrated that natural selection as a mechanism existed. Even one or two examples of divergence showed that two species could arise from one common ancestor. Taken to its inevitable conclusion, natural selection had to exist and could explain the origin of species. Huxley was willing to speculate from a solidly proven mechanism into general law even though few instances of the evidence had been found. Thomson had set out to see if the deep sea produced intermediate forms. It did. Was that not enough, given all of Darwin and Wallace's other evidence, to prove natural selection? Huxley was willing to establish natural law from a few, well-supported mechanisms. Huxley accepted variation and divergence as *verae causae* based on an analogy made from a few examples, such as the *Amphioxus*.

Darwin wrote a less cordial response to Thomson's conclusions. He shot a letter off to Huxley defending his theory and asking for Huxley for advice; he was angry and he knew that it came across in his retort. He asked Huxley, “If my manuscript appears too flat, too contemptuous, too spiteful, or too anything, I earnestly beseech you to throw it into the fire.”[540] Darwin's family would later recall the *Nature* response to Thomson as unique in its acidity; Francis Darwin, Charles Darwin's son, claimed that the letter was the only time that Darwin “wrote publicly with anything like severity.”[541] Darwin asked Huxley to simply edit the letter and publish it without consulting him. Huxley kept the critical language, but stuck mostly to a criticism of Thomson's uses of extreme variation:

> I AM sorry to find that Sir Wyville Thomson does not understand the principle of natural selection, as explained by Mr. Wallace and myself. If he had done so, he could not have written the following sentence in the Introduction to the Voyage of the *Challenger*: – "The character of the abyssal fauna refuses to give the least support to the theory which refers the evolution of species to extreme variation guided only by natural selection.” This is a standard of criticism not uncommonly reached by theologians and metaphysicians, when they write on scientific subjects, but is something new as coming from a naturalist. Prof. Huxley demurs to it in the last number of NATURE; but he does not touch on the expression of *extreme variation*, nor on that of evolution being guided only by natural

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[540] Francis Darwin, ed. *More Letters of Charles Darwin: A Record of His Work in a Series of Hitherto Unpublished Letters*, volume I (London: John Murray, 1903), 388.

[541] Francis Darwin, *Charles Darwin's Works: The Life and Letters of Charles Darwin* (New York: D. Appleton & Co., 1896), 418.

> selection. Can Sir Wyville Thomson name any one who has said that the evolution of species depends only on natural selection? As far as concerns myself, I believe that no one has brought forward so many observations on the effects of the use and disuse of parts, as I have done in my "Variation of Animals and Plants under Domestication"; and these observations were made for this special object. I have likewise there adduced a considerable body of facts, showing the direct action of external conditions on organisms; though no doubt since my books were published much has been learnt on this head. If Sir Wyville Thomson were to visit the yard of a breeder, and saw all his cattle or sheep almost absolutely true, that is closely similar, he would exclaim: "Sir, I see here no extreme variation; nor can I find any support to the belief that you have followed the principle of selection in the breeding your animals." From what I formerly saw of breeders, I had no doubt that the man thus rebuked would have smiled and said not a word. If he had afterwards told the story to other breeders, I greatly fear that they would have used emphatic but irreverent language about naturalists.[542]

Huxley left some of Darwin's stronger language on the editing room floor instead of allowing it into print. Naturalists never saw Darwin's last sentence about the man he had supported for the Regius Chair of Natural History at Edinburgh, “Perhaps it would have been wiser on my part to have remained quite silent, like the breeder; for, as Prof. Sedgwick remarked many years ago... a man who talks about what he does not in the least understand, is invulnerable."[543]

Darwin denied that the existence of other evolutionary mechanisms – besides divergence with extreme modification – offered evidence against his theory; his treatise on evolutionary mechanisms, “Variation of Animals and Plants Under Domestication,” had admitted that other types of evolution existed besides natural selection.[544] However, the debate was not over.

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[542] Charles Darwin, “Sir Wyville Thomson and Natural Selection,” *Nature* 23 (1880): 32-33.

[543] Darwin, More Letters, 389.

[544] Darwin's deployment from deep-sea creatures to farm animals also shifted focus from hard-to-acquire specimens to objects readily found to most English naturalists, agricultural animals. This appeal to common sense and the experiences shared by those who did not have access to deep-sea creatures may have swayed some naturalists.

Thomson had sent a friendly – but strong – reply to Huxley's review in the same issue of *Nature*. This letter explained why he would not entertain a theory of massive, acute seabed upheaval. The reply explicitly argued what limits he was willing to take regarding the use of evidence. He laid down the evidence he had gathered regarding seabed movement and then explained that he was not able to entertain such a drastic conclusion without evidence:

> The hypothesis of the elevation of a mass of land equal to Europe and as high as Mont Blanc in the middle of one of the great ocean basins could in our present state of knowledge be defensible only on the supposition that it was a phenomenon of the same order as the elevation of some portion of our existing continental land, and there is now to say the least, grave reason for doubting that any rock which is due to accumulations formed at depths over 2500 fathoms, the average depth of the basins to which Prof. Huxley refers, enters into the composition of any existing continent... Such a hypothesis therefore besides being without a single fact in its support, would be met by a strong adverse argument from analogy, and would be, so far, in a worse case than the hypothesis of the origin of species by natural selection.[545]

Here, Thomson claimed that he would only entertain scientific conclusions based on plausible evidence in geology, which was the same criticism he had regarding natural selection. The exchange between Huxley and Thomson made it clear that their methods for deriving universal laws of nature from the same evidence differed greatly.

If Thomson's retort to Huxley was collegial in response to Huxley's review, his reply to Darwin was as indignant as Darwin's letter was scathing. Thomson had spent the last twenty years of his life in the pursuit of universal laws related to Darwin's work. The attack elicited a response aimed directly at Darwin's use of evidence to reach universal truth and – in a sense – pushed back against the shift from deep-sea geography to analogies based on domesticated, farmyard animals:

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[545] Wyville Thomson, “Changes in Geological Level,” *Nature* 23 (1880): 33.

> I HAVE at least great reason to be thankful that my stupidity has not prevented me from thoroughly enjoying the teachings of Mr. Darwin and Mr. Wallace, which I confess to having regarded chiefly masterly and charming “studies in variation,” for the last twenty years.
>
> The title of the epoch-marking book which came of age last month was, however, “The Origin of Species by Means of Natural Selection.” Mr. Darwin, as I am well aware, has put forward this mode of the origin of species as a part only of a hypothesis which is universally looked upon as a supreme effort of genius.
>
> It seemed to me, rightfully or wrongly, that the fauna of the enormous area forming the abyssal region existed under conditions which held out the hope that it might throw some light upon a question which appears to underlie the whole matter, and which is still unanswered. Are physiological species the result of the gradual modification of pre-existing species by natural selection, or by any similar process; or are they due to the action of a law as yet utterly unknown, by which the long chain of organisms rolls off in a series of definite links?
>
> I fear I scarcely follow Mr. Darwin's illustration. If one were to pay his first visit to a breeder's, and be shown a flock of Leicesters, never having seen or heard of a sheep before, he would see nothing but a flock of sheep, and would certainly, without justly incurring the contumely of the breeder, be entitled to set them down merely as a group of animals of the same *species*... But give him an opportunity of comparing the results of breeding throughout a long period of time, or of observing the process of breeding over half the world, which comes to much the same thing; the breeder might then have cause to rail if he had not picked up the stages of the process.
>
> The close examination of the newer tertiaries and the careful analysis of the fauna of the deep sea seem to me fairly to represent these two methods [time and space]; both of these promise to yield a mass of information in regard to the course of evolution, but as to the *mode* of the origin of species both seem as yet equally silent.
>
> I will ask you in a week or two for a space for a short paper on “The Abyssal Fauna in Relation to the Origin of Species.”

Thomson modified Darwin's farmyard example to illustrate the limitations of observation in relation to universal claims. The naturalist would be required to observe either the difference between species over time or over space in order to see the effect of selection. Thomson adhered to Forbes' theory of specific centers and limited himself to available observations to build a theory. Darwin rejected the special geographical traits of the deep sea. Thomson disagreed with Darwin’s objection and, by doing so, he also disagreed with Darwin’s method of deriving scientific knowledge from specimens and observations.

Thomson's promised rebuttal never made it into *Nature*. He never fully recovered from his illness and, given the added stress, he buckled under the strain. He resigned his university appointment in October of 1881. He then died on 10 March 1882, one month before Darwin. The major figures in the deep-sea evolution debate were dead. Naturalists would be forced to adjudicate the evidence for – or against – natural selection by examining other scientific evidence.

Conclusion: The Eclipse of Darwinism

Julian Huxley, TH Huxley's grandson and a renowned biologist in his own right, later recalled the period immediately following this exchange as the “eclipse of Darwinism.” Starting in the 1880s, a number of competing theories emerged to challenge Darwinian natural selection.[546] The debate focused on the *mechanism* for evolution, the very item that Thomson had disputed during the last years of his life. For over a decade, Thomson had drawn scientific attention to the deep-sea fauna and its geography as a test of Darwin's theory; and there the Lilliputian fauna of the sea floor struck a nearly-mortal wound to Darwinian natural selection.

However, when Thomson died, few people could match his understanding of the deep-sea fauna. Thomson had stated his conclusions; Darwin had disputed that the

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[546] Peter Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore and London: Johns Hopkins University Press, 1983) for a great account of these competing theories. It is also Bowler's belief that the mechanism of natural selection was the major item up for debate during this period.

deep sea even offered any valuable perspective to his theory. However, the question of how to investigate – and how to prove – the mechanism of evolution remained. Naturalists began proposing new mechanisms of evolution, each originating from their own areas of expertise. The deep sea and marine invertebrates had focused the evolutionary debates since the beginning of the century. When this elite and influential group of seabed naturalists turned away from the deep sea floor as the site to prove their theory, they lost a long-standing, shared source of evidence. That loss of cohesiveness is one essential context for the proliferation of extra-Darwinian explanations for evolution during the late-nineteenth century.

The deep-sea evolution debate emerged in the midst of other disputes regarding evolution and evidence. Much historical attention has focused on the conflict between religion and evolution. Some of the greatest critics of evolution did so for religious reasons. Some older naturalists fought for a vision of the natural world where divine plan still had a place. Darwin had anticipated these theological criticisms; the thought of religious criticism kept him from publishing his theory for a number of years. However, even some of the most famous of these naturalists, such as Louis Agassiz, were surprised at evolution's success within elite scientific circles.[547] Despite a continual flow of criticism from religiously minded objectors, naturalists increasingly accepted evolution following the publication of *On the Origin of Species*.

Darwin's theory had also elicited concerns about scientific method and evidence. Naturalists had debated how to establish natural law since the early-nineteenth century. The historical sea floor provided a site over which naturalists debated method since Darwin's medical school days at Edinburgh. Philosophical naturalists at different

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[547] See John Hedley Brooke, *Science and Religion: Some Historical Perspectives* (Cambridge: Cambridge University Press, 1993), 275, for more on this example.

locations developed their own practices for crafting natural law. These practices collided, especially around shared scientific specimens and evidence. Sea floor sediments and fauna provided evidence that many philosophical naturalists found valuable. Darwin had incorporated this evidence into his hypothesis of natural selection. The general topic of evolution also generated wide scientific interest during the nineteenth century. Consequently, the debates over evolution offered a way for naturalists to negotiate and share their evidentiary practices.

As one example, historians have discussed the negotiation between inductivism and evolutionary theory.[548] Darwin recognized that inductivism was an influential method of reasoning throughout the nineteenth century. He had also been trained at one of the hotbeds of inductive logic, Cambridge University. Darwin labored for twenty years to gather evidence for his theory and arranged that evidence to convince the scientific minds he admired, Herschel, Whewell, Lyell, and Forbes, among many others.[549] However, some of the most powerful – and personally dismaying – criticism came not from unscientific and religious individuals, but rather from the very people had had set out to impress. As historian David Hull stated, "...he had not anticipated the vehemence with which even the most respected scientists and philosophers in his day would denounce his efforts as not being properly 'scientific.'"[550] When Darwin, perhaps unknowingly, blended the evidentiary practices of nineteenth-century inductivism with zoogeological speculation, he created an opportunity for these philosophers to apply their new philosophies of science. Some naturalists, such as Whewell and Herschel,

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[548] See especially Hull, *Darwin and His Critics.*See also Silvan Schweber, "Origin of the Origin Revistited," *Journal of the History of Biology* 10 (1977) and Schweber, "Herschel and Darwin."

[549] Hull, *Critics*, 6. Darwin grew a reputation for being an inductive naturalist, though his work of coral reefs and barnacles was theoretical.

[550] Hull, *Critics*, 3.

claimed that Darwin had not followed the inductive method and, therefore, could not make a proper claim for natural law. However, even the "inductivists" could not agree on how one would properly apply their philosophies correctly.

Whewell and Herschel had retained Francis Bacon's term "induction" for their method of deriving truth from nature, but had recognized that Bacon's method was not applicable in its original form. Consequently, each philosopher created a different method of science, but retained the same name for it, and by doing so claimed intellectual lineage with Bacon.[551] This proliferation of "inductive" philosophies generated heated debate about the proper way to derive truth from the natural world. John Stuart Mill, another philosopher of inductive science, famously debated Whewell over the details regarding what leads to inductive truth when Mill published his 1843 *System of Logic, Ratiocinative and Inductive, Being a Connected View of the Principles of Evidence, and the Methods of Scientific Investigation*.[552] Whewell replied in 1849 with *Of Induction, with Especial Reference to Mr. J. Stuart Mill's System of Logic*.[553] The concept of inductive method was far from stable or decided by the middle of the nineteenth century. How naturalists applied induction in the observation of actual scientific specimens was equally as unstable, if not more so.

Darwin's hypothesis, and the way he built his claim of natural law, offered philosophers a way to argue over scientific method. For example, Adam Sedgwick, the Cambridge professor of geology who had initially encouraged Darwin, criticized that his former pupil had "departed from the true inductive track" with his theory of natural

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[551] Hull, *Critics*, 5.

[552] John Stuart Mill, *A System of Logic: Ratiocinative and Inductive Being Connected View of the Principles of Edividence, and the Methods of Scientific Investigation*, eighth edition (New York: Harper & Brothers, 1882).

[553] William Whewell, *Of Induction: With Especial Reference to R. J. Stuart Mill's System of Logic* (London: John W. Parker, 1849).

selection.[554] Herschel called Darwin's hypothesis “the law of higgledy-piggledy.” Alternatively, Mill argued that Darwin had acted “in the most exact accordance with the strict principles of logic,” though Darwin still required proof if it was to move beyond the logic of discovery into substantiated natural law.[555] Nonetheless, Mill endorsed Darwin's philosophy as properly inductive. Even Huxley had recognized in his “Coming of Age” lecture that Darwin attempted to follow the rules of *vera causa* and induction, but was still ridiculed by the Baconians from time to time.

Nonetheless, Huxley and others also agreed that evolution and – to a slightly lesser extent – natural selection had become increasingly accepted within elite scientific circles from 1860 until 1880 despite criticism. Those criticisms drew attention to the use of evidence regarding natural selection; naturalists were apprehensive about the unresolved Darwin-Thomson debate after 1876. Thomson's search for deep-sea evidence for evolution forced naturalists to decide whether that evidence was enough to disprove natural selection or not. The expedition was also well publicized and people awaited its results with great anticipation. The *Challenger* expedition acted as a catalyst for the simmering doubt that natural selection was the primary mechanism for organic evolution. As an established evolutionist and member of this elite seabed network, Thomson's critique lent credibility and weight to this doubt.

The critique was never fully resolved. Had Darwin and Thomson lived – and been well enough to continue the fierce conflict – naturalists might have continued to use deep-sea objects to adjudicate natural selection. However, naturalists waited for

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[554] Hull, *Critics*, 6.
[555] Hull, *Critics*, 8.

Thomson's final retort on *how* one would use evidence to prove natural selection, yet that paper was never published.[556]

Thomson's disagreement regarding natural selection also joined other critiques from outside the community of biologists. William Thomson (no relation), known later as Lord Kelvin, had contributed to the deep-sea research by inventing a sounding cable which used piano wire instead of hemp. Like many other seabed naturalists, he was interested in the implications of evolution by natural selection. When Darwin's *On the Origin of Species* was published, he doubted that the Earth would have been habitable long enough to account for the vast time needed for natural selection to act upon species. Starting in 1869, he used physics to argue that the age of the Earth was not as ancient as Darwin had supposed.[557] Kelvin continued to work on his final calculation of the Earth's age until 1897; his argument regarding the age of the Earth extended from before the *Challenger* expedition almost into the next century. Despite Kelvin's critique, evolution and natural selection continued to gain credibility over the 1870s. It was not until the 1880s, when Thomson published the Challenger zoological reports, that the elite naturalists fractured and searched for alternative evolutionary mechanisms.

Naturalists eventually used the shortened age of the Earth to challenge natural selection later in the century. Historian Peter Bowler recounts a canonical instance in the eclipse of Darwinism, when Huxley found himself defending Darwin's theory of evolution against Robert Gascoyne-Cecil, third Marquess of Salisbury.[558] The Marquess of Salisbury, a past prime minister, had been elected president of the British Association for

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[556] Jeffreys did give a lecture on deep-sea dredging that dismissed the marine invertebrates as evidence for natural selection, but the discussion of evidence grew starting in 1880.

[557] William Thomson, “On Geological Dynamics,” *Transactions of the Geological Society of Glasgow* 3 (1869): 215-240, was written in response to Huxley's address to the Geological Society of London.

[558] Bowler, *Eclipse*, 3.

the Advancement of Science. He presided over the 1894 Oxford meeting, which marked a quarter of a century since Huxley had verbally battled Bishop Wilberforce over natural selection. Huxley was present at the 1894 meeting but it would be his last public appearance. The crowd of distinguished naturalists packed the Sheldonian theatre and awaited the Marquess' address.

Salisbury spoke about the unknowns of science. He delved into physics, chemistry, and astronomy; each of these sciences had problems yet to be solved. Finally, he arrived at the study of biology.[559] Salisbury dilated upon a few biological questions before choosing Darwin and evolution as a topic:

> Yet certainly the most conspicuous event in the scientific annals of the last half century has been the publication of Mr. Darwin's work on the Origin of Species, which appeared in 1859. In some respects, in the depth of the impression which it made on scientific thought, and even on the general opinion of the world, its momentous effect can hardly be overstated. But at this distance of time it is possible to see that some of its success has been due to adventitious circumstances. It has had the chance of enlisting among its champions some of the most powerful intellects of our time...[560]

One of the powerful intellects to whom Salisbury referred, Huxley, sat nearby and began furiously tapping his foot.[561] The tension in the room became palpable.

Salisbury lavished Darwin with praise, especially in regards to the revolution in scientific method he had effected:

> But by far the largest part of its accidental advantages was to be found in the remarkable character and qualifications of its author. The equity of judgment, the simple-minded love of truth and the patient devotion to the pursuit of it through

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[559] Huxley, *Life and Letters Huxley*, 399.

[560] Robert Cecil, “President's Address,” in *Report of the Sixty-Fourth Meeting of the British Association for the Advancement of Science Held at Oxford 1894* (London: John Murray, 1894), 11.

[561] Huxley, *Life and Letters Huxley*, 399.

> years of toil and of other conditions the most unpropitious – these things endeared to numbers of men everything that came from Charles Darwin, apart from its scientific merit or literary charm. And whatever final value may be assigned to his doctrine, nothing can ever detract from the luster shed upon it by the wealth of his knowledge and the infinite ingenuity of his resource. The intrinsic power of his theory is shown at least in this one respect, that in the department of knowledge with which it is concerned it has effected an entire revolution in the methods of research. Before his time the study of living Nature had a tendency to be merely statistical; since his time it has become predominantly historical. The consideration how any organic body came to be what it is occupies a far larger area in any inquiry now than the mere description of its actual condition; but this question was not predominant—it may almost be said to have been ignored—in the botanical and zoölogical study of sixty years ago. [562]

Salisbury's speech continued to affirm that the theory of evolution had triumphed; naturalists no longer took arguments on the immutability of species seriously.

Evolution remained unquestioned, according to Salisbury. The mechanism for evolution, however, remained the great unsolved mystery of biology. Specifically, naturalists remained unconvinced of universal common descent and could not agree on “the process by which it has come about.” These lingering questions prevented Darwinian theory from succeeding in “the conquest of scientific opinion; and still less [was] there any unanimity in the acceptance of natural selection as the sole or even the main agent of whatever modifications may have led up to the existing forms of life.”[563]

A few objections stood in the way of natural selection. The first of these was Lord Kelvin's “young Earth” argument. The second was more serious. Salisbury borrowed a comment recently published by an influential evolutionist as to the problem of natural selection, “We accept natural selection... not because we are able to demonstrate the process in detail, not even because we can with more or less ease imagine it, but simply

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[562] Robert Cecil, “President's Address, 12.

[563] Cecil, “President's Address,” 13.

because we must – because it is the only possible explanation that we can conceive." There was a problem with proving that natural selection was the mechanism of evolution, said Salisbury, "It is purely hypothetical. No man, as far as we know, has ever seen it at work."[564] Salisbury had a problem accepting that men of science should "accept as established a hypothetical process the truth of which he admits that he cannot demonstrate in detail, and the operation of which he can not even imagine," when searching for something as grand as a law of nature. He had a problem with Darwin's evidence.

The lecture against natural selection put Huxley in a difficult place. His task was to second a vote of thanks for the Marquess' address. Lord Kelvin began a vote of thanks. Huxley, seconding the vote, stood up and delivered a gripping and vigorous protest against Salisbury's attack on natural selection while thanking him at the same time. It was a memorable performance and a fitting end to Huxley's defense of Darwin and his theories. The aging Huxley avoided further sessions, then returned home from his last public appearance.[565]

Huxley had no response to Lord Kelvin's critique in his reply to Salisbury. The age of the Earth, as calculated by Kelvin, posed a challenge to natural selection. It must be remembered, however, that in Salisbury's address, the lack of evidence for divergence – not the age of the Earth – posed the greater threat to natural selection. Salisbury deployed Lord Kelvin's young Earth argument as support for Wyville Thomson's critique that the principle of divergence lacked evidence.

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[564] Cecil, "President's Address," 14. The evolutionist to which Salisbury refers is August Weismann, the influential German evolutionary biologist.

[565] Huxley, *Life and Letters Huxley*, 153-154.

The criticism that available evidence did not support evolution by extreme variation – as a natural law – began with the *Challenger* expedition specimens. Darwin recognized that Thomson's criticism was something new; he had become used to criticism from “theologians and metaphysicians,” but a critique from a fellow evolutionist posed a more serious challenge. Thomson's denial of natural selection also usurped Darwin's own blending of evidentiary seabed practices. In a sense, Thomson used Darwin's own method against him. Darwin had hoped that his scientific reputation could overwhelm Thomson's attack, but the evidence provided from the sea floor ultimately proved more persuasive than Darwin himself. Naturalists would search for the mechanism of evolution for years. They would not be fully reunited under one mechanism for evolution until the mid-twentieth century when natural selection was reborn in the form of the modern evolutionary synthesis, based on the mathematical analysis of populations.

CONCLUSION: Darwinism as a Method Shaped by Deep-Sea Biology

In 1897, George John Romanes, the youngest of Darwin's disciples, reflected upon Darwin's influence upon the scientific world. He authored a lecture on evolutionary theory and Darwin's legacy. The opening lines of his lecture recognized the key component of Darwin's historical influence: “Among the many and unprecedented changes that have been wrought by Mr. Darwin's work on the *Origin of Species*, there is one which, although second in importance to no other, has not received the attention which it deserves. I allude to the profound modification which that work has produced on the ideas of naturalists with regard to method.”[566] Romanes was not the only person to remember Darwin's role in the history of scientific methodology.

Julian Huxley, TH Huxley's grandson, also established a formidable career as a biologist and evolutionist; his 1942 book, *Evolution: The Modern Synthesis* summarized much of the evidence for evolution that had been missing during Darwin's lifetime. *The Modern Synthesis* recalled the role that Darwin played in the history of evolution. Darwin's contribution was not so much the discovery of a mechanism by which evolution operated, since “Even among professional zoologists the modern conception of natural selection and its mode of operation is quite different from Darwin's day...” Instead, Darwin had contributed a way of conducting science itself:

> Biology at the present time is embarking upon a phase of synthesis after a period in which new disciplines were taken up in turn and worked out in comparative isolation. Nowhere is this movement towards unification more likely to be valuable than in this many-sided topic of evolution; and already we are seeing the first-fruits in the reanimation of Darwinism.

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[566] George J. Romanes, *Darwin After Darwin: An Exposition of the Darwinian Theory and a Discussion of Post-Darwinian Questions*, fourth edition (Chicago: Open Court Publishing, 1910), 1.

> By Darwinism I imply the blend of induction and deduction which Darwin was the first to apply to the study of evolution. He was concerned both to establish the fact of evolution and to discover the mechanism by which it operated; and it was precisely because he attacked both aspects of the problem simultaneously, that he was so successful. On the other hand he amassed enormous quantities of facts from which inductions concerning the evolutionary process could be drawn; and on the other, starting from a few general principles, he deduced the further principle of natural selection.[567]

The term “Darwinism” here was not a mechanism or explanation for the naturalists who followed in Darwin's footsteps; Darwinism was a method of using evidence to uncover laws of the natural world.

Darwin's treatise, *On the Origin of Species*, was as much a methodological work as it was a claim about the natural world. His theories of evolution and natural selection provided nineteenth-century naturalists a way to negotiate the use of evidence in science. This contribution was neither explicit nor overtly philosophical in nature. Rather, contemporary naturalists fought over scientific practices. Darwin had learned a number of these practices early in his scientific career. For example, he had attended both the University of Edinburgh and Cambridge University during his undergraduate education. Both of these institutions were engaged in a process of studying the sea floor when Darwin attended them as an undergraduate, though they conducted their research in very different ways. When Darwin applied the training he was given while voyaging aboard the *Beagle* – and during the years that followed – he blended elements he had learned at both institutions. That blending, along with his focus on the sea floor, gave naturalists from both institutions a way to defend their unique evidentiary practices.

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[567] Julian Huxley, *Evolution: The Modern Synthesis* (New York and London: Harper & Brothers, 1942), 13.

Evidence for Evolution and the Development of Deep-Sea Biology

The search for natural law in the biological world had only started in earnest at the beginning of the nineteenth century. A transatlantic community of self-identified “philosophical naturalists” coalesced to find those laws. Discrete communities of philosophical naturalists, while joined in a search for natural laws, had not yet agreed on the proper way to establish those laws. On multiple occasions in the period covered in this dissertation, naturalists observed the same biological phenomenon or specimen, yet came to opposite conclusion about the natural world. These disagreements caused tension for the naturalists who were each searching for universal biological truth. To a large extent, these differences arose from the differing geographical and institutional milieus from where they originated. The naturalists at each institution were hardly sessile; the natural philosophers formed a community that stretched across the Atlantic and their members – as well as their specimens – moved frequently. As a result, their unique evidentiary practices circulated and collided during the nineteenth century.

While Darwin's evolutionary work would eventually offer a means for naturalists to negotiate their methods, their scientific practices originated as ways to conduct seabed science. For example, a long line of naturalist dredgers inhabited the Regius Chair of Natural History at Edinburgh. Proximity to the Firth of Forth, a marine bay, facilitated the passing down of dredging practices from mentor to student throughout the century. In turn, dredging became a way to engage with the scientific community at the University, as well as to follow the work of their prominent figures. These dredging philosophical naturalists called themselves zoogeologists.

Zoogeology, as an evidentiary practice, valued marine invertebrate specimens brought up from the ocean floor by the dredge. Naturalists going back to Edward Forbes had used these invertebrates, especially the crinoids, to investigate the relationship between different types of life. The crinoids seemed to share many similarities with plant life as well as animal life. Their simplicity made them easy to research. The dredge had also reinforced a type of speculative reasoning, whereby naturalists could extrapolate larger patterns in nature by intuiting them from a small number of observations. For example, Forbes had speculated that life ceased to exist below 300 fathoms underwater based on dredgings that did not actually reach to that depth; he believed that he could extend his findings to that depth by observing the diminution of life in shallower areas.

The seabed science that originated at Edinburgh was quite distinct from that practiced at Cambridge University. Cambridge's possessed a legacy of scientific success in astronomy and mathematics. Consequently, the influential professors there propagated the practices of great scientific minds from Cambridge's history, such as Sir Francis Bacon and Sir Isaac Newton. These naturalists were known by a number of names: the “Baconian Inductivists” and the “Trinity Constellation,” among others. Like their fellow philosophical naturalists, these Cambridge dons sought to uncover natural law; their methods differed in their reliance on minute calculations, identification of *verae causae*, and prediction.

In the early-nineteenth century, members of the Cambridge network turned their methods to the investigation of the historical sea floor. These studies ran concurrently with those conducted by the Edinburgh network. Cambridge scholars began a predictive study of the tides, starting in London Harbor and, eventually, reaching worldwide. The Cambridge tidologists collected facts and measurements; they claimed that their collections would reveal the inner workings of nature without resorting to *a priori* assumptions. Their methods, for the most part, were successful in predicting local high tides. They had faith that a sustained measurement of sea levels would even answer whether the seabed subsided or rose over time. And while the Cambridge tidological studies competed with the Edinburgh zoogeologists for resources from the same organization, the British Association for the Advancement of Science, the two programs remained relatively discrete.

Darwin trained at both of these seabed science institutions and, as a result, remained intimately aware of seabed science developments as he developed his theory on the origin of species. As a result, he invoked the historical sea floor in his evidence for natural selection. Specifically, he was unable to provide substantial proof of the divergence of species when he published *On the Origin of Species* in 1859. He admitted that, if his theory was true, naturalists would find intermediate forms that demonstrated divergence in the fossil record. Yet, he could not provide fossil evidence for divergence. Darwin argued that seabed subsidence and tidal action destroyed the evidence he needed. He could, however, point to extant creatures, such as the platypus, which showed morphological traits from two different classes of organisms. Darwin offered these “living fossils” as provisional evidence with a promise that more living and stone fossils would be found as naturalists searched for them. Darwin's use of seabed science caused disparate naturalists to clash over his novel blending of inductive and deductive methodology.

The American biotic debate eventually provided a common practice for the investigation of evolution. In 1852, an American naval midshipman invented a sounding device, Brooke's sounding line, which was capable of retrieving specimens of deep-sea sediment. These samples brought to the surface for observation consisted entirely of microscopic shells. Naturalists could not agree whether the shells lived upon the sea floor or died at the surface and drifted into the abyss. As a result, the new sounding line indirectly caused naturalists to argue over the existence of life in the deep ocean. The biotic debate, as well as the American deep-sea sounding practices, circulated to Great Britain, where they were interpreted through local scientific milieus. In the 1860s and 1870s, Darwin's closest associates used these deep-sea specimens as evidence in favor of natural selection, including TH Huxley, who believed that he had discovered the primordial ooze, *Bathybius*.

The discovery of a living, stalked crinoid specimen that showed intermediacy only deepened interest in deep-sea marine invertebrates as adjudicators of the evolution debate. In 1866, a Norwegian naturalist discovered a stalked crinoid from a region believed to be devoid of marine life. This discovery was followed closely by an American dredger independently capturing the creature in 1867. The stalked crinoid was an ancient form, thought to have gone extinct long ago; it had been found previously only as fossilized fragments. The creature demonstrated morphological intermediacy between two different groups, the stalk-bearing cystoids and the feathered echinoderms. Thomson used the living stalked crinoid to describe aspects of the creature that could not be revealed by fossilized specimens, such as its soft-body morphology. He then turned his attention to the stalked crinoid as a way to explore ancient morphologies as evidence for evolution.

Prompted in part by the stalked crinoid's discovery, Her Majesty's Navy gave Thomson the use of the HMS *Challenger* to conduct a worldwide study of the deep-sea fauna. While on the voyage, Thomson's crew solved the long-standing biotic debate. They also disproved the existence of Huxley's *Bathybius*, the supposed primordial common ancestor of all evolutionary life; it was nothing more than a chemical precipitate.

The eviction of *Bathybius* as proof for common ancestry was detrimental to the theory of evolution by natural selection, but it was not the greatest challenge presented by the dredging expedition. Thomson also announced that the deep-sea fauna did not show the proof of divergent forms that Darwin had promised in *On the Origin of Species*. Darwin and Huxley attempted to discount Thomson's analysis. However, Thomson remained victorious for a number of reasons, the greatest of those reasons being that he had used Darwin's own blended methodology to reason through his specimens.

The Deep Sea as a Scientific Geography

The unique geography of the deep sea itself affected the evolutionary science conducted there. This section discusses six geographical qualities that influenced the production of specimens from the deep sea and, consequently, the evidence used to adjudicate the late nineteenth-century evolution debates. These qualities ultimately forced naturalists to decide upon the scientific method that would determine the credibility or otherwise of natural selection.

First, the deep sea was an *invisible* space. Naturalists could not see the abyssal floor for themselves, so they relied upon technologies to procure the entirety of their evidence. Deep-sea geography posed a methodological problem for naturalists since the invention of Brooke's sounding line. The line was difficult to operate with the consistency needed for scientific investigation. Naturalists modified the sounding device in an attempt to fix these difficulties. The sounding line underwent as many minor modifications as there were naturalists who used the device. The diversification and evolution of Brooke's device accelerated as the device's use spread to new communities around the Atlantic. The deep-sea floor also remained accessible only through technological investigation; naturalists were unable to observe the sea floor for themselves and, therefore, were forced to rely upon other scientific practices to produce reliable evidence.

The invisibility of the deep-sea geography tied arguments over scientific practice to debates over the correct interpretation of evidence. In one instance, George Wallich retrieved several starfish from a depth of 1,000 fathoms. While naturalists had already begun to doubt Forbes' azoic line, they split over whether Wallich's use of the sounding line versus the naturalist's dredge was reliable for the collection of marine invertebrate specimens. Wyville Thomson dismissed Wallich's starfish based on his use of the sounding line instead of the dredge, for example. The unique geography of the sea floor forced naturalists to judge the fidelity of their scientific practices at the same time that they derived information for natural law from the specimens.

The process of natural selection and divergence that Darwin proposed was equally invisible. Naturalists could observe the competition between various terrestrial species. That everyday struggle for existence and its effect on variation satisfied the principle of *vera causa* by providing an observable and cogent explanation for his observations, yet the process could only be observed over great lengths of time. The long geological periods required to actually observe the divergence of one species into two different species made the process unobservable to the individual naturalist or even to generations of naturalists; he would be forced to derive his evidence from the fossil record. Darwin was unable to provide this evidence. He argued both that the deep sea did not sustain life, and that even if it did, the harsh environment would destroy his fossil evidence. Brooke's sounding line opened the deep sea for the search for living specimens not long after Darwin made this claim, so naturalists began scouring the abyss for Darwin's evidence as soon as they discovered that creatures lived in the deep sea. Naturalists searched an invisible space for evidence of an invisible evolutionary mechanism.

Second, the deep sea was an *inaccessible* space. It was also hard to gain access to it with sounding equipment. Naturalists found it difficult to survey the open ocean without institutional sponsorship. Ships often had to sail far from land in order to reach great depths. The distance required to reach areas for deep-sea dredging put the study outside the reach of amateur – and even most elite – naturalists. This made deep-sea specimens rare and, therefore, valuable. Only those in possession of samples could participate authoritatively in the debates. To gain those samples, naturalists would have to both learn the necessary deep-sea practices and gain the resources needed to dredge in the open ocean. The naturalist would need a ship, a crew, a sounding engine, and the freedom to leave on an expedition for a number of months. Alternatively, a naturalist could use his contacts to gain the specimen from another naturalist's collection. In either case, the naturalist relied upon a scientific network to gain his samples, either for training or for access to the collection. And samples certainly circulated through established marine biology networks.

These networks, in turn, were invariably connected to an institution which sponsored the expedition. The American seabed naturalists relied upon government resources to conduct their deep-sea practices. As shown in chapter three, American scientific networks were consequently immersed in the politics of their patron institutions, even going so far as to actively work against the interests of each other. Equally, Thomson's reliance upon Her Majesty's Navy to conduct the Challenger expedition generated a conflict over possession of the specimens. That conflict would have never happened if Thomson did not rely upon naval resources to dredge the deep sea. Institutional politics crept into seabed science as more than a context for the development of practices; the deployment of the practices was equally as immersed in institutional milieu.

Third, the deep sea was an *international* space. As already mentioned, philosophical naturalists resided around the Atlantic, from Boston to Edinburgh to the Cape of Good Hope. The Atlantic acted as a border to each of these locations. It also provided the primary means of transportation; ships frequently sailed to and from these maritime nations. The Atlantic facilitated naval and colonial interests as well. Seaboard safety relied upon charts of the coast and sea floor to ensure passage for warships, mail carriers, and commercial vessels. These philosophical naturalists, as a community, were connected together by their relationship to the sea. They sailed on survey vessels, circulated marine objects, and visited one another freely.

Naturalists conducted their practices in international waters, as well. While terrestrial exploration might be hampered by shifting territorial lines, the open ocean remained beyond national affiliation. In some ways, the struggle for possession of the open ocean transferred to scientific competition. Naturalists used national pride to gain resources for scientific discovery, a very successful tactic. Nonetheless, this space belonged to the seabed naturalists and they intentionally fought to maintain themselves as a community that was not defined primarily by its national affiliation. They were joined in the search for universal laws, no matter what coast from which they had set sail.

Fourth, the deep sea was an *unbounded* space. While landmasses end, the deep sea circles the globe continuously. This vast expanse constitutes the largest ecological area on the planet. Nineteenth-century naturalists recognized the biological value of an unbounded area for the study of biogeographical distribution. Since early in the century, zoogeologists argued that organisms should be analyzed in terms of both space and time. The large expanse available for organisms to spread out illuminated the effects of environmental factors, such as depth, and divergence. If a naturalist could not travel into the past to observe an organism's history, he could see how it changed over geographical space instead. And if Darwin had required a vast amount of time to observe the divergence of species, the unbounded deep sea could provide the necessary vast space.

Fifth, the deep sea was a *primordial* space. Naturalists viewed the deep-sea floor as an unchanging geographical location; it was beyond the swift changes that characterized the surface world. Many naturalists viewed the ocean depths as a realm of gentle repose, with the lifeless bodies of marine shells continually drifting down to the floor, where they remained eternally undisturbed. These naturalists viewed the ocean depths as lifeless, desolate, and unchanged since the beginning of time. Often, cultural and religious assumptions about the deep sea supported the azoic zone theory as much as scientific evidence.

Even naturalists who disputed the lifelessness of the deep sea equated their specimens with antiquity. Thomson recognized the stalked crinoid pulled from the depths as something known previously only as fossilized remains. In Thomson's opinion, the specimens that he pulled from the deep sea were ancient forms that gave naturalists a window into the geological and biological past. Huxley and Carpenter, in a similar manner, both imagined the primordial ooze swaying gently upon the sea floor.

Sixth, the deep sea was an *exotic* space. Its inaccessibility caused naturalists to imagine and wonder at what creatures might lie far beneath the waves. Maury likened the feeling of contemplating the deep-sea floor as a religious experience akin to staring up into the heavens. Once retrieved, the deep-sea creatures did not disappoint the naturalists. One Norwegian naturalist named a deep-sea starfish he collected *Brisinga*, after a jeweled torc worn by the Norse goddess Freya; according to myth, the torc had been torn from her and thrown into the abyss. The specimens' beauty, rarity, and otherworldliness made them fascinating objects to study. Their study yielded knowledge that had previously been literally unfathomable.

The deep sea undoubtedly possessed unique qualities as a nineteenth-century scientific geographical space. This study has shown how these qualities, at different times, have guided the evidence that naturalists produced for the evolution debates. In many cases, the extreme nature of these qualities makes their influence on scientific methodology more direct and, therefore, historically informative. The deep-sea floor's geography clarifies the relationship between scientific practice, specimens, and the creation of biological laws.

The nineteenth-century seabed science debates explored in this dissertation conclude with the acceptance of evolutionary theory and the rejection of Darwinian natural selection as evolution’s mechanism. Biologists, in turn, split into many independent groups as they searched for an explanation of species change over time. Yet, while the naturalists did not agree on a mechanism, they did agree upon a method for continuing that search. They also agreed the accumulation of data was not the ultimate goal of science, but rather the search for natural law by blending inductive and deductive methods of extracting knowledge from specimens. As explained by Romanes:

> For it was the *Origin of Species* which first clearly revealed to naturalists as a class, that it was the duty of their science to take as its motto, what is really the motto of natural science in general,
>
> Felix qui potuit rerum cognoscere causas.[568]
>
> Not facts, then, or phenomena, but causes or principles, are the ultimate objects of scientific quest.[569]

That new method for science, the *Darwinian* method, along with the abandonment of natural selection by extreme modification, provided the method by which population genetics and molecular evolution would be developed. It also provided the means for revolution against traditional natural history, which philosophical naturalists had sought since the beginning of the century. The philosophical naturalists, at long last, had their method for establishing universal truths about the natural world.

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[568] "Fortunate is he, who is able to know the hidden causes of things." This quote is from Virgil's 29BC "Georgics."

[569] Romanes, *Darwin after Darwin*, 5.

GLOSSARY OF PEOPLE MENTIONED

Abjornsen, Peter Christian. 1812 – 1885. Norwegian writer and marine biologist. Discovered the deep-sea starfish, *Brisinga*.

Adams, John Couch. 1819 – 1892. British mathematician and astronomer. Calculated the exact position of Neptune.

Agassiz, Alexander. 1835 – 1910. Son of Louis Agassiz. American marine zoologist.

Agassiz, Louis. 1807 – 1873. Well-known American marine zoologist. Diretor of the Museum of Comparative Zoology at Harvard. Born in Switzerland. Opposed Darwin's theory of evolution by denying the immutability of species. Father of Alexander Agassiz. Friend to Alexander Dallas Bache and Louis Pourtales.

Airy, George Biddell. 1801 – 1892. English Astronomer Royal from 1835 to 1881. Failed to assist John Couch Adams to discover Neptune.

Allman, George. 1812 – 1898. Irish ecologist and marine zoologist. Served as Regius Professor of Natural History at Edinburgh.

Babbage, Charles. 1791 – 1871. Mathematician, philosopher, and inventor of the first mechanical computer. Student at Cambridge with William Whewell.

Bache, Alexander Dallas. 1806 – 1867. Grandson of Benjamin Franklin. Second Superintendent of the United States Coast Survey. Leader of the Scientific Lazzaroni, an elite group of American naturalists. Political enemy of Matthey Maury.

Bacon, Francis. 1561 – 1626. English philosopher, naturalist, and Lord Chancellor of England. Advocated an inductive scientific method during the scientific revolution. Graduate of Trinity College. Cambridge. Lauded by nineteenth-century "Inductivists."

Bailey, Jacob Whitman. 1811–1857. American pioneer of microscopic research in the United States. Geology professor at West Point. Early contributor to the biotic debate.

Banks, Joseph. 1743 – 1820. Sailed as botanist aboard Captain James Cook's first voyage (1768–1771). Returned to immediate fame. President of the Royal Society for over 41 years. Helped found the Royal Botanic Gardens, Kew, and facilitated a large -network of botanical collection from around the world.

Bastion, Henry. 1837 – 1915. British advocate of spontaneous generation. Student of TH Huxley.

Beaufort, Francis. 1774 – 1857. was an Irish hydrographer and officer in Britain's Royal Navy.

Brooke, John Mercer. 1826 – 1906. United States naval officer and inventor of the Brooke’s sounding line, a deep-sea surveying instrument. Defected to the Confederacy during the Civil War.

Buchanan, john Young. Chemist aboard the *Challenger* expedition. Disproved *Bathybius*.

Buffon, Georges. 1707 – 1788. French Enlightenment naturalist, mathematician, cosmologist, and author of *Histoire Naturelle*.

Carpenter, William Benjamin. 1813 – 1885. English physician and invertebrate zoologist. Helped to organize the *Challenger* expedition with Wyville Thomson.

Challis, James. 1803 – 1882. English professor and astronomer. Director of the Cambridge Observatory. Failed to assist John Couch Adams in the discovery of Neptune.

Chambers, Robert. 1802 – 1871. Scottish publisher, evolutionist, and anonymous author of *Vestiges of the Natural History of Creation*, a pre-Darwinian evolutionary text.

Coldstream, John. 1806–1863. Scottish physician and graduate of the University of Edinburgh. Studied marine zoology with Charles Darwin.

Coleridge, Samuel Taylor. 1772 – 1834. English Romantic poet and science philosopher.

Cuvier, Georges. 1769 –1832. French naturalist, paleontologist, andcomparative anatomist. Came into conflict with Etinne Geoffroy.

Darwin, Charles. 1809 – 1882. English biologist and geologist. Author of the well-known treatise on evolutionary theory, On the Origin of Species, which argued natural selection as the mechanism for species change over time. Student at Edinburgh and Cambridge. Sailed aboard the *Beagle*.

Davis, Jefferson. 1808 – 1889. United States Senator and friend of Alexander Dallas Bache. First President of the Confederate States of America.

Davy, Humphrey. 1778 – 1829. Cornish chemist and inventor.

Dayman, Joseph. Unknown. Mate aboard the *Erebus* with Joseph Hooker. Also served as Lieutenant aboard the *Rattlesnake* with TH Huxley. Gathered deep-sea sediment sample for Huxley.

Ehrenberg, Christian. 1795 – 1876. German microscopist and geologist. Early contributor to the biotic debate.

FitzRoy, Robert. 1805 – 1865. British naval officer and hydrographer. Captain of the survey ship Beagle while Charles Darwn was on board. Opposed the theory of evolution.

Forbes, Edward. 1815 – 1854. Manx marine zoogeologist. Dredger and Regius Professor of Natural History at Edinburgh. Discovered the azoic zone, a theory of marine animal distribution which was later disproven.

Gascoyne-Cecil, Robert Arthur Talbot. 3rd Marquess of Salisbury. 1830 – 1903. British

Prime Minister and Chancellor at Oxford.

Geoffroy Saint-Hilaire, Étienne. 1772 – 1844. French naturalist who defended Lamarck’s evolutionary theories. Transcendentalist. Came into conflict with Georges Cuvier.

Goethe, Johann. 1749 – 1832. German writer, statesman, and naturalist.

Grant, Robert. 1793–1874. Medical student at Edinburgh turned zoogeologist and dredger. Well-known early nineteenth-century biologist at Edinburgh and first Professor of Comparative Anatomy at University College London. Mentor to the young Charles Darwin. Taught him dredging and marine zoology.

Haeckel, Ernst. 1834 – 1919. German marine biologist, evolutionist, and philosopher. Popularized Darwin’s work in Germany.

Hall, James. 1811 – 1898. American geologist and paleontologist.

Halley, Edmond. 1656 – 1742. English astronomer, mathematician, and physicist. Known for calculating the orbit of “Halley's Comet.” Second Astronomer Royal in Britain.

Hasslar, Ferdinand. 1770 –1843. American mathematician. First Superintendent of the United States Coast Survey and head of the Bureau of Weights and Measures. Born in Switzerland.

Haüy, René Just. 1743 – 1822. French mineralogist.

Henslow, John Stevens. 1796 – 1861. was an English botanist and geologist. Professor at Cambridge. Friend and mentor to his pupil Charles Darwin.

Herschel, Frederick William. 1738 – 1822. British astronomer and composer. Born in Germany. Discovered Uranus. Father of John Herschel.

Herschel, John. 1792 – 1871. English mathematician, astronomer, chemist, and inventorof photography. Student at Cambridge with William Whewell.

Hooker, Joseph Dalton. 1817 – 1911. Well-known British botanist. Sailed aboard the

Erebus. Director of the Kew Gardens. Close friend and confidante of Charles Darwin.

von Humboldt, Friedrich Wilhelm Heinrich Alexander. 1769 – 1859. Well-known Prussian geographer, naturalist, and explorer. Known for his work on biogeographical distribution.

Huxley, Julian. 1887 – 1975. English evolutionary biologist. Grandson of TH Huxley. A key architect in the evolutionary synthesis.

Huxley, Thomas Henry. 1825 – 1895. Well-known British marine zoologist. Known as Darwin's fiercest supporter. Sailed aboard the *Rattlesnake*. Discovered *Bathybius*, the primordial ooze, which was later disproven.

Jameson, Robert. 1774 – 1854. Scottish naturalist and mineralogist. Studied under Abraham Werner, the Neptunist. Regius Professor of Natural History at Edinburgh. Darwin's Natural History instructor.

Jefferson, Thomas. 1743 – 1826. Third President of the United States (1801–1809). Author of the Declaration of Independence. Advocated for and practiced natural philosophy.

Jeffreys, John Gwyn. 1809 – 1885. British marine zoologist. Conducted dredging expeditions with Edward Forbes and Wyville Thomson. Participated in the cruise of the *Porcupine*.

Jenkin, Fleeming. 1833 –1885. British engineer and physicst. Repaired submarine telegraph cables. Regius Professor of Engineering at Edinburgh. Argued against Darwin's theory of natural selection, stating that variation would be dominated by prevalent traits in a population.

Kelvin, Lord. See Thomson, William.

Kepler, Johannes. 1571 – 1630. German mathematician and astronomer. A key figure in the scientific revolution. Best known for his laws of planetary motion,

Knox, Robert. 1791 – 1862. Scottish surgeon and zoologist. Transcendentalist.

Lamarck, Jean-Baptiste. 1744 – 1829. French naturalist and evolutionary thinker. Author of the pre-Darwinian treatise *Zoological Philosophy*, which started “biology.”

Laplace, Pierre-Simon. 1749 – 1827. French mathematician and astronomer. Solved a problem in Newtonian mechanics through a study of the tides.

LeVerrier, Urbain. 1811 – 1877. French mathematician. Discovered Neptune through calculation.

Linnaeus, Carolus. 1707 – 1778. Swedish botanist best known for his system of species classification.

Lubbock, John. 3rd baronet. 1803 – 1865. English banker, astronomer, and tidologist. Student of William Whewell at Trinity College, Cambridge. Father of Sir John Lubbock, 4th baronet.

Lubbock, John. 4th Baronet. 1834 – 1913. English banker, biologists, and archeologist. Grew up in Downe, near Charles Darwin. Became one of Darwin’s younger friends. X Club member. Son of Sir John Lubbock, 3rd baronet.

Lyell, Charles. 1797 – 22 February 1875) Well-known British geologist. Author of *Principles of Geology*, a treatise which argued for uniform geological change over vast amounts of time. Friend and confidante of Charles Darwin.

Maury, Matthew Fontaine. 1806 – 1873. United States naval officer. Superintendent of the Naval Observatory. Defected to the Confederacy during the Civil War. International figure in martime science, especially hydrography. Enemy of Alexander Bache.

McCormick, Robert. 1800 – 1890. Surgeon and ship’s naturalist aboard the Beagle with Charles Darwin and the Erebus with Joseph Hooker. Famous for leaving the

Beagle when Darwin began to take naturalist duties.

Mill, John Stuart. 1806 – 1873. was a British philosopher, political economist, and inductivist. Debated William Whewell over the proper means of induction. Home schooled.

Milne-Edwards, Alphonse. 1835 – 1900. French carniologist. Developed zoological ideas which Darwin used in his reasoning. Director the Muséum National d'Histoire Naturelle.

Moseley, Henry Nottidge. 1844 – 1891. British marine naturalist who sailed aboard the *Challenger* expedition.

Murray, John. 1841 – 1914. Scottish oceanographer, marine biologist. Sailed with the *Challenger* expedition. Became Director of the expedition publications when Charles Wyville Thomson died.

Newton, Isaac. 1642 – 1726. English mathematician, astronomer, and physicist. Key figure in the scientific revolution. Author of *Principia Mathematica*, which began classical mechanics. Discovered gravity. Lucasian Professor of Mathematics at Cambridge.

Owen, Richard. 1804 – 1892. English comparative anatomist and paleontologist. Outspoken opponent of Darwin’s theory of natural selection.

Peacock, George. 1791 – 1858. Scottish mathematician. Professor at Cambridge.

Phillips, John. 1800 – 1874. English geologist and professor at Oxford.

de Pourtales, Louis Francois. 1824–1880. American naturalist. Born in Switzerland. Student and lifelong friend of Louis Agassiz. Naturalist at the Coast Survey. Director of the Museum of Comparative zoology after Agassiz’ death.

Romanes, George. 1848 – 1894. English evolutionary biologist and physiologist. Youngest of Darwin’s academic friends. Coined the term “Neo-Darwinian.” Born in Canada.

Ross, James Clark. 1800 – 1862. British naval officer and explorer. Sailed with his uncle, John Ross, on an arctic expedition. Later captain of the *Erebus,* in command of Joseph Hooker, while on an expedition to Antarctica. Had an interest in marine zoology.

Ross, John. 1777 – 1856. British arctic explorer. Uncle of James Clark Ross. Retrieved samples from the deep-sea floor before Edward Forbes established the azoic zone theory.

Sabine, Edward. 1788 – 1883. Irish astronomer and explorer. Sailed with Captain John Ross to the arctic. Advocated magnetic research as President of the Royal Society.

Sars, Michael. 1805 – 1869. Norwegian theologian and marine zoologist. Discovered the first stalked crinoid. Father of Georg Ossian Sars.

Sedgwick, Adam. 1785 – 1873. English geologist. Professor at Cambridge. Taught geology to Charles Darwin. Later opposed the theory of natural selection.

Stimpson, William. 1832 – 1872. American naturalist and marine biologist. Most famous dredger in the United States at the time. Accompanied the North Pacific Exploring Expedition. Researcher at the Smithsonian Institution and later, director of the Chicago Academy of Sciences.

de Tocqueville, Alexis. 1805 – 1859. French historian and political philosopher. Known for his works *Democracy in America*.

Thomson, Charles Wyville. 1830–1882. Scottish natural historian and marine zoologist. Regius Chair of Natural History at Edinburgh. He served as the chief scientist on the *Challenger* expedition. Disputed Darwin’s theory of natural selection.

Thomson, William. 1st Baron Kelvin. 1824 – 1907. Well-known British mathematician, physicist, and engineer. Discovered the First Law of Thermodynamics. Calculated the age of the Earth.

Troost, Gerard. 1776 – 1850. American geologist and paleontologist. Founder and first president of the Philadelphia Academy of Natural Sciences. Studied crinoid fossils.

Walker, John. 1731–1803. Scottish natural historian. Regius Professor of Natural History at Edinburgh.

Wallace, Alfred Russel. 1823 – 1913. British naturalist, explorer, and evolutionist. Famous for co-discovering natural selection with Darwin.

Wallich, George. 1815-1899. British physician and marine biologist. Contributor to the late biotic debate. Collected starfish from the deep-sea floor.

Werner, Abraham. 1749 – 1817. German geologist and Neptunist.

Whewell, William. 1794 – 1866. English naturalist, philosopher, theologian, and historian of science. He was Master of Trinity College, Cambridge. Conducted extensive studies into global tidology.

Wilkes, Charles. 1798 – 1877. American naval officer and explorer. He led the United States Exploring Expedition. Captured the British ship *Trent*, leading to an international diplomatic affair with England.

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