

Hey all --

I am assigning the Preface, and the first part of the Introduction to this book, along with about six pages from Chapter 6, which is the long chapter on animal communication and whale song; the material most relevant to Professor Steingo's work.

-DGB

D. GRAHAM BURNETT

THE SOUNDING OF  
THE WHALE

SCIENCE & CETACEANS IN THE  
TWENTIETH CENTURY

THE UNIVERSITY OF CHICAGO PRESS  
CHICAGO & LONDON

UNCORRECTED PROOFS - NOT FOR CIRCULATION

D. GRAHAM BURNETT  
is professor of history and history of science at Princeton University.

The University of Chicago Press, Chicago 60637

The University of Chicago Press, Ltd., London

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Printed in the United States of America

21 20 19 18 17 16 15 14 13 12 1 2 3 4 5

ISBN-13: 978-0-226-08130-4 (cloth)

ISBN-10: 0-226-08130-3 (cloth)

Library of Congress Cataloging-in-Publication Data

Burnett, D. Graham.

The sounding of the whale: science & cetaceans in the twentieth century/

D. Graham Burnett.

p. cm.

Includes bibliographical references and index.

ISBN-13: 978-0-226-08130-4 (cloth: alkaline paper)

ISBN-10: 0-226-08130-3 (cloth: alkaline paper)

1. Whales—Research—History—20th century.
2. Whales—Research—Great Britain—History—20th century.
3. Whales—Research—United States—History—20th century.
4. Whaling—History—20th century.
5. Whaling—Law and legislation—History—20th century.
6. International Whaling Commission—History. I. Title.

QL737.C4B86 2012

599.5072—dc23 2011030694

© This paper meets the requirements of ANSI/NISO Z39.48-1992  
(Permanence of Paper)

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## PREFACE

For in the mere act of penning my thoughts of this Leviathan,  
they weary me, and make me faint with  
their out-reaching comprehensiveness of sweep,  
as if to include the whole circle of the sciences . . .

Herman Melville, *Moby-Dick*, 1851

### FRIENDS, HOLD MY ARMS

I've eaten a bit of whale. Not a lot, but a bit. Both smoked (I thought it quite like prosciutto of boar) and as a fresh steak, rare (indistinguishable from elk, in my view). This was in Norway, in the company of a particularly bloodthirsty spokesman for the industry who tried, as we chewed, to sell me on the idea that every species that has any sense kills other species by way of enlarging the ambit of its own vitality. He seemed, pressing this point, to deem whale conservation a kind of race suicide, which was a disorienting theory to be offered by a heavily accented German wielding a steak knife. But he was such a companionable fellow, gregarious and enormously likeable in other respects—not to mention abundantly knowledgeable about whale matters. So, thinking of Ishmael (“Not ignoring what is good, I am quick to perceive a horror, and could still be sociable with it—would they let me—since it is but well to be on friendly terms with all the inmates of the place one lodges in”), I raised my glass to human fellowship, and we drank a long draught in the beery twinkle of an endless Scandinavian summer's eve.

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By contrast, I have also wept in the presence of a living whale. This was in Baja California, in the Sea of Cortez, when our sputtering panga suddenly found itself in the middle of a boiling cauldron of crazed skipjack and terrified pilchards. Interesting enough. We cut the engine to watch. And then, some forty feet from the boat, without warning, up rose the towering bulk of a healthy young Bryde's whale, which launched itself skyward, mouth gulping in a colossal uprushing swallow. It seemed to continue rising for a count of ten before falling back into the cold blue, now pin-drop silent. In those suspended moments I had seen clearly the loose folds of striated skin that made up the expandable gape of that giant mouth, and their jowly openness had been touched with the faintest fresh pink. And seen from that side, suspended at the apex of its bolting leap, the creature reminded me of nothing so much as some fantastic and gargantuan frog, puffing its huge belly to the sky in a mad frolic of power and joy. In "Converse at Night in Copenhagen," Isak Dinesen writes of three kinds of perfect happiness, and the first is "to feel in oneself an excess of strength." There was some of that in this apparition, and that may be why I cried. I cannot say. The whole thing was simply too much to bear.

More whale moments? Most of them are in Mexico. I have the clearest memory of picking my way over a rocky stretch of island shore to investigate the extremely rotten carcass of a bull sperm whale, whose slow blasting under a tropical sun had left a slick of stench half a mile out to sea. The slightest shift in the wind meant strangled gags; sea lice in obscene hordes swarmed the strand, and the blowflies tormented the bold vultures that picked at strips of leathery yellow fat. Even broken by surf and decay, the animal's head was thicker than I am tall. Lying on its side, it towered over me.

And then, of course, there was that silent and moonless night in a small kayak, paddling about in terrified awe as, somewhere impossibly near, one of those giants—a fin, presumably—sucked up sudden, room-sized breaths and expelled them in deep and plosive gusts. I felt (alone in the inky dark, bobbing far from land) something of the basic, unmitigated, almost suicidal fear that one does well to recall while waxing eloquent about the beauties of untrammelled nature.

The field station in Bahia where I was staying had been used by generations of itinerant naturalists and students of the things that live in the sea. Some years back, on the occasion of the stranding of a small fin whale in the bay, a group of them had taken on the daunting task of recovering, preparing, and articulating the 35-foot skeleton. In the end, the project

took years, but the fruit of the labor still stood when I visited, bleached to a crumbly lightness, strung out on a rusted armature of pipe, the beast's nose pointing due east out over the sea, to where the sun rose every morning. Awakening at dawn on a cot perched below this looming scaffold of bone, it was impossible not to think of the "Bower in the Arascides," the temple-skeleton of a whale Ishmael writes of having explored on the island of Tranque, and which affords him so rare an access to the measure of his prey: diligently he had its dimensions tattooed on the skin of his right arm in order to preserve these data for the world of learning, though he elected to omit the odd inches in order to save space on his flesh for a poem upon which he was then at work. A good idea, that. One must not let whale knowledge take over everything.

. . . . .

I am moved to note, here at the outset, that most of the work on this book looked nothing like these scenes. Most of it involved sitting perfectly still in a chair, sometimes reading and sometimes writing. Sometimes I would lie down. In this respect, I tend to think that the making of this book has amounted to a kind of extended spiritual exercise: a project of self-denial and self-abnegation; a minor-key rendition of the ascetic ideal. No sun, no waves, no tattoos. Wanderings of the mind from the austerities of the task at hand were often fruitful, but the better for being brief, and stolen. The best part of the process, I think, was the extremely strange way that everything could look at the end of a workday as one went outside, say, or saw another person, or wondered what life looked like to those who had spent the day in full career with the actual world, as opposed to bookish resignation from its affairs. I associate the most memorable of such moments with feelings approaching hysterical glee, and thus it is probably all to the good that these shivers of addled euphoria were generally fleeting. One mustn't have too much fun writing whale books. Or reading them either. But that is probably easier.

TIME, STRENGTH, CASH, AND PATIENCE

Solitary as the task of making whale books can be, they do not happen absent various emoluments and sociabilities. Princeton University afforded me both the time (in the form of a pair of generous leaves) and the cash (including research funds from the dean of the faculty, the History Department, and other internal sources) to realize this project. I am very grate-

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ful. In addition, portions of this work were supported by a grant from the National Endowment for the Humanities (FA-37754-03) and a Howard Foundation Fellowship. Over the years I have presented sections of this material to a number of helpfully critical audiences, including the 2005 HMAP “Oceans Past” conference at the Syddansk Universitet, Kolding; the Rutgers Center for Historical Analysis “The Sea in Global History” conference in 2006; “The Decimation of Whales,” an international symposium at the Hvalfangstmuseet, Sandefjord, in 2007; the “Knowing Global Environments” celebration conference at the University of Pennsylvania in the same year; the American Cetacean Society’s 2008 conference; and a variety of history of science and environmental history workshops, including gatherings at the annual meetings of the History of Science Society, York University (Toronto), University of California (Berkeley), Harvard, Yale, and Princeton. Teaching, too, has been important to the development of this project as a whole: several classes of students who participated in the Stanford Summer Session at the Vermilion Sea Field Station, Baja California, heard and commented on chapters 2 and 6; graduate students at Princeton helped me familiarize myself with relevant literatures in a pair of seminars (“Science Across the Seas,” in 2002, and “Humans and Animals,” in 2005); and the freshmen in my “Beast in the Sea” seminar in 2008 soldiered through chapters 4 and 5 and gave me valuable feedback. My colleagues in the Program in History of Science—Angela Creager, Michael Gordin, Helen Tilley, Keith Wailoo, and, of course, the late Michael Mahoney (who gave me my first training in the field)—offered collegial advice and generous readings. Along the way, other colleagues took the time to read and respond to portions of the material that appears in this book. The following deserve special mention for making such time: Dan Rogers, John Krige, James Schulz, and Lorraine Daston. In the endgame, I received the benefit of two close analyses of the whole manuscript by expert readers for the University of Chicago Press, Kurk Dorsey and Gary Kroll, both of whom delivered me generous comments, specific corrections, and helpful amplifications. At about the same time, Henry Cowles went through the text line by line, pressing me on secondary literature and catching a number of errors. Finally, Bill Perrin afforded the grace of a technical reading, pen in hand, by one of the most distinguished marine mammal biologists living. To all of them, my sincere thanks.

A study like this one requires a great deal of assistance from archivists, librarians, practicing scientists, research assistants, friends, and others who pitch in with references, recollections, leads, or sources. I am sure I am

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omitting many of those who afforded me such aid, but here is at least a partial list: John Bannister, Jeff Breiwick, the late Sidney G. Brown, Robert Bruesewitz, Anne Datta, Deborah Day, Jeff Dolven, Greg Donovan, Michael Dyer, Richard Ellis, Stuart Frank, Ray Gambell, Anthony Grafton, Catherine Hansen, Judy Hanson, Robert Headland, Aaron E. Hirsh, Paula Jenkins, Henrik Stissing Jensen, Christine Kim, Sonja Kromann, Richard Laws, Steven Mandeville-Gamble, Debbie Macy, Rosalind Marsden, Scott McVay, James Mead, Ed Mitchell, Domingo Monet, the late Lara Moore, Jac Mullen, Joe Nardello, Naomi Oreskes, Dmitri Petrov, Joanna Rae, Randy Reeves, Norman Reid, Sam Ridgway, Pauline Simpson, Tim D. Smith, Janani Sreenivasan, William Tavolga, Roberto Trujillo, Polly Tucker, the late David Van Keuren, Veronica Volny, the late William Watkins, and Emma Woodason, along with the whole staff of Article Express, Interlibrary Services, and Printing and Mailing at Princeton University, without whom this work would have been impossible.

Finally, whale books do not happen without at least an even measure of succor. Yes, it is true that my mother told me whale stories as a small boy, and yes, she and my father took me out on campus at Indiana University in the mid-1970s to meet an earnest, bearded grad student who gave me a copy of the 1975 *Audubon* issue on whales. These things, I presume, stuck. I have a sister who was for several years my scuba-diving partner (we went our separate ways underwater when she started cage diving with great whites), and much of my sense of the sea was shaped in her company. By the end of all this, I myself had a few children, including a daughter who could ask me, clear as a bell, at the dinner table, “Y dada, ¿qué hiciste en tu oficina hoy?” To which the ritual answer was, “Hoy escribí mi libro de ballenas,” a predictable reply always greeted with a patronizingly theatrical “¡Oh, qué bien!” I took great courage from these reliable little parleys.

As I did from the remarkable support of a beloved wife, Christina, who laid her hands on this manuscript and on its maker—making each, in its season, whole in its way. *Gracias*.

ONE

## INTRODUCTION

Like the boy on the burning deck the little Herr Professor (as he came to be called) stood on the flensing stage. . . . Between his boots and the planking there existed a layer of viscous yellowish grease: whence, doubtless, the apprehension betrayed at his bearded lips, the awkward stiffness of his bodily attitude. But his eyes, under beaded brows, were brightly alert, for the spirit was gaining mastery over the flesh, as it so often does when Science is goddess.

J. J. Bell, *The Whalers*, 1914

During these months at sea, I have watched the sperm whales, looking for keys to an understanding. I have found it impossible to function simply as an impassive machine, turning the actions of the whales into scientific truths. . . . I lower the hydrophone, and hear the whales: “Click . . . click . . . click . . .”

Hal Whitehead, *Voyage to the Whales*, 1990

### SCIENCE AND THE WHALES

This is a book about whales, but there are relatively few whales in it. Indeed, let’s start with a basic truth: there is not a single cetacean of any sort in these pages. You knew that, of course, since even the smallest dolphin needs much more room than the largest trim size of the most voluminous scholarly tome. And though they breathe air, cetaceans basically like being in the water, while books are mostly written on paper, a substance that fares poorly when submerged. In this sense books and whales are, in an

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important way, immiscible. I tried to keep this in mind as I wrote, and it will be good to keep it in mind as you read.

So let me start again: this is a book about knowledge of whales. And to be still more precise, it is a book about the knowledge of whales garnered and mobilized by experts over the course of the twentieth century. Experts like the two men who appear in the epigraphs for this introduction, two whale scientists (a tribe sometimes known by the Melvillean moniker “cetologist,” sometimes by the more sedate professional designation “marine mammal biologist”) whose labors—one slogging through the gruesome residue of a whaling station with knife and notebook, the other bronzing himself on the bow of a hydrophone-equipped sailboat in the Indian Ocean—mark out the chronological (and perhaps also the spiritual) endpoints of this book as a whole. Two whale scientists pursuing knowledge of whales in different ways, at different times, for different purposes. Their work and its effects—this is my subject.

Knowledge is a funny thing. It is hard to explain what it is, hard to explain how we get it, hard to explain how it works in the world. It is characteristic of knowledge that it takes different forms than the thing known, and this means that the known thing is consistently absent from knowledge of it. One feels this, sometimes, even painfully. This book is interested in all these problems, and it frets about them, even as it recapitulates and reenacts them. In this sense, at least, the writing of whale books and the doing of whale science are more alike than different. Both go into the world absent their whales. If it is the whale you want, you will have to go to sea, where, because of the events I recount in this book, you are likely to have a considerable wait. Bring a book. You might bring this book, since it is long.

Like knowledge, whales are also funny, and a little hard to pin down. It would be difficult to pick a set of creatures that have been subjected to a more dramatic reimagining over the course of the last century: once seen as monstrous dwellers in the abysmal depths, shelled with explosives, melted for industrial commodities, and gunned as target practice by gleeful flyboys, these peculiar beasts eventually came to be understood by many as soulful, musical friends of humanity, symbols of ecological holism, bellwethers of environmental welfare, and even totems of a movement to transform the world and our attitude toward it. How did this happen? This book offers an answer to that question, and in sifting out that answer, it traces almost a hundred years of human efforts to understand these fugitive and mysterious animals. At the beginning of the chronology of this

book, the most significant scientific publication in the world, *Nature*, could prominently and grossly misidentify the species of a whale depicted in its pages—and go uncorrected. Such was the extent of general scientific ignorance of these animals.<sup>1</sup> By the end of the period surveyed below, there was hardly a schoolchild in North America who had not been obliged to write up a whale report for science class. Because these superlatives of organic organization have taken up a great deal of space in the collective imagination, and because of the remarkable trajectory of their reconception since 1900 (a process in which the sciences played a significant role), I contend that a history of whale science can shed considerable light on the changing understanding of nature in the twentieth century. That is my claim, and the pages that follow represent my best effort to deliver thereupon.

I have various (imagined) readers in mind for this work, which is situated at the intersection of several different disciplinary literatures. For starters, my primary approach is that of the history of science. It is—after all, and for better or worse—the scientists’ techniques for producing knowledge of nature that have proved most robust and authoritative in the modern world. How do those techniques work? How do they develop? And how do the findings of the scientists help make the world in which we live? These are, I think, the central questions that concern any historian of science, and they are questions that motivate and organize this study. I am, therefore, preoccupied throughout with showing what it meant to have scientific knowledge of cetaceans at different moments in the twentieth century, and I work to demonstrate who succeeded in making such claims, how they did so, and what larger consequences followed on their efforts. The range of different kinds of “cetology”—from sloppy slaughterhouse anatomy conducted under macabre and trying conditions to fiddly bioacoustics work performed by tidy military scientists wearing headphones (or stoned hippies playing synthesizers)—proves surprising, and the conflicts between these different sorts of whale science ended up playing a significant role

1. See Roy Chapman Andrews, “What Shore Whaling Is Doing for Science,” *Nature* 88, no. 2200 (28 December 1911): 280–82, at p. 281, where the caption on figure 3 reads “‘Cutting in’: a Right Whale,” but the animal in the photograph is clearly a rorqual. A year later, Theodore Salvesen, lecturing on the explosive growth of the modern whaling industry at the Royal Society of Arts, in London, could be met by a pressing question from the chairman, Lord Sanderson, “whether a whale was really a fish?” Salvesen, “The Whaling Industry of Today,” *Journal of the Royal Society of Arts* 60, no. 3097 (29 March 1912): 515–23, at p. 523.

both in the history of whaling, and in the history of whale conservation, which was in turn an important component of the rise of the modern environmental movement.

It is the fraught history of modern whaling (of which more later in this introduction) that gives the story of whale science much of its significance, not to mention its poignancy. The bulk of chapter 2, for instance, deals with the emergence of an extensive and well-funded program of biological research on the large whales of the Southern Hemisphere in the early part of the century—work that aimed to lay the foundations for the “rational regulation” of the whaling industry, which was then rapidly expanding into new waters in the Antarctic. The failure of this initial scientific-cum-regulatory undertaking—and it was a complicated sort of failure, as I show in some detail—had lasting repercussions, I argue, for the later history of efforts to control the commercial exploitation of the world’s whales. And for the scientists who were charged to do “biology”—the science of *life*—in the stygian swamps where their subject organism underwent Brobdingnagian dismemberment and rendering, field research came to mean a demanding acculturation to industrial-scale killing, grinding, and cooking. It is my hope that this aspect of my investigation—a portrait of a life science at work in the maw of death, a set of scientific investigations inextricably entangled with a highly remunerative and destructive activity—will hold the attention of traditional historians of biology as well as historians who work on the field sciences, natural history, agricultural research, and science in commercial settings. The changing relationship between science and industry is a significant theme in this study.

Because much of the early research into the life histories, migration patterns, and basic biology of the large whales was conducted by Great Britain as part of a major multi-vessel scientific initiative (known as the “Discovery Investigations”), chapter 2 also engages the larger history of oceanography in the first half of the twentieth century. And because Britain had designs on the ice, islands, and waters of the Southern Hemisphere (where the vast majority of the whaling in this period was conducted), I have also gestured, if passingly, at issues of science and imperialism in writing about Discovery and the ways that whale research served to advance various geopolitical strategies in the period before World War II.

The question of what it would mean to be “rational” about the fantastically lucrative circum-Antarctic killing fields dogged the work of whale scientists and the policy makers who hoped to make use of their findings. This issue is central to chapters 3, 4, and 5, where I am concerned

to unfold the changing relationship between science and regulation from 1930 to 1965. It is my hope that this material will be of interest not only to historians of science but also to political scientists, environmental activists, and others concerned to understand how expert knowledge functions in the complex arena of collective decision making.<sup>2</sup> Because whales were a unique, open-ocean commercial quarry, they raised from early on unprecedented problems for regulators, diplomats, and international lawyers, and these challenges eventually led to the formation of the first formal international body dedicated to the management of a biological resource, the International Whaling Commission (IWC), founded shortly after World War II. This organization was explicitly committed to building a mechanism whereby “scientific findings” about whales would serve as the basis for sound regulatory policies that could be implemented on a global scale. A Panglossian techno-scientific optimism spangled the early years of the IWC, a touch of which can be found in this paean to whaling “factories” (the big blue-water whale-processing vessels that roamed the oceans digesting large cetaceans into commercial fats, waxes, and fertilizers) offered by a leading member of the IWC’s “Scientific Committee” in 1952:

In the course of time, the floating factory has become more and more of a technical marvel. It is an oil-plant and a meat-meal factory. It is also a canning factory. It is a very well-equipped chemical works, with a most ingenious and varied routine. It is in fact a scientific institute of the first rank.<sup>3</sup>

Chapters 4 and 5 take up the fate of this dream in some detail. In doing so they not only lay out a revised history of one of the great debacles of

2. For helpful introductions to this literature, consider the following studies (which intersect with this one at several points): Stephen Bocking, *Nature's Experts: Science, Politics, and the Environment* (New Brunswick, NJ: Rutgers University Press, 2004); Edward Christie, *Finding Solutions for Environmental Conflicts: Power and Negotiation* (Cheltenham, UK: Edward Elgar, 2008); Radoslav S. Dimitrov, *Science and International Policy: Regimes and Nonregimes in Global Governance* (Lanham, MD: Rowman & Littlefield, 2006); and Robert F. Durant, Daniel J. Fiorino, and Rosemary O'Leary, eds., *Environmental Governance Reconsidered: Challenges, Choices, and Opportunities* (Cambridge, MA: MIT Press, 2004).

3. This was Birger Bergersen, in a speech preserved in his private papers: Birger Bergersen Papers, Hvalfangstmuseet, Sandefjord, box 3, folder “taler.” Note that he subsequently revised this to read, “It can in fact be used as a scientific laboratory of the first rank.”

twentieth-century natural resource management, but also suggest a way of approaching the larger problem of telling suitably nuanced stories about the intersection of science and politics in a regulatory setting. In chapter 4, for instance, I trace out the evolution of the scientific advising system in the IWC, paying particular attention to the ways that scientists themselves functioned as savvy political actors sensitive to the need for careful “boundary work” between the questions that would be defined as “scientific” and those that would be defined as “political.” An analytic focus on the elaboration of these boundaries leads to some larger conclusions about what it meant to “do science” in a new and challenging environment: the committee rooms of the post–World War II international organizations for global governance, geopolitical diplomacy, and international regulation.<sup>4</sup>

There were new sciences in play as well. Chapter 5 examines the mobilization of mathematical models of population dynamics in the regulatory arena in an effort to show how these models were made into powerful tools for forcing consensus among conflicting actors. This section of the book may be of interest to those historians concerned to understand the ways in which numbers, calculations, and computational systems have come to affect public life. And if there is a chapter of this book that I think could be profitably read by a student of politics, I think this would be it. Though, to be fair, it would have to be a more than ordinarily patient student of the discipline, since my treatment of this episode cannot easily be reduced to the sort of “finding” that one could readily mobilize in a think tank working group: there is a narrative here, there are characters, and there are some mathematical models too. It is the (tacit) contention of the chapter that one cannot really understand what happened without rolling up one’s sleeves and working to make sense of the math, the people, and the specific sequence of historical events. What is the take-home point once one has subjected oneself to this exercise? Well, the most important lesson may simply be that one must do this actual work; that without this work one cannot really understand what happened. In that sense, while I would like this material to be read by political scientists (particularly those with an interest in science, society, and environmental problems), I am aware that

4. See Joseph E. Taylor III, “Boundary Terminology,” *Environmental History* 13, no. 3 (July 2008): 454–81. My interest in boundaries is different from Taylor’s, but his concern with terminological specificity in the move to “global” and “international” historical framings is right on point. It is an open question whether I have been careful enough, but I have certainly been preoccupied with this problem.

some of them may find its historical (and scientific?) detail tedious, even rebarbative.

And that points to a larger fact about this book and its approach: this is an archival history of a somewhat demanding variety. It has been written out of reams of published and manuscript material—personal letters, scientific notebooks, technical reports, diplomatic correspondence—from dozens of archival collections in half a dozen countries. It is not unreasonable to ask some hard questions about the ultimate value of such studies, which are difficult to research and compose and often by no means especially pleasant to read. I am, as I give this volume to a world increasingly concerned with Twitter-scale texts, acutely conscious of these sorts of questions and feel them with great force—particularly when, say, I glance from the walls of my office (crammed with unwieldy binders and an unholy proliferation of old books) to the screen of my iPhone (which quietly insists that the relevant world can stream bright and clean through a glassy lozenge of responsive obsidian). This, however, is not the place to mount a full-scale defense of the culture of the book, or, for that matter, a plea for the future of the bricklike academic monograph. Suffice it to say that the satisfactions of the latter are an acquired taste, and I, having tasted, would happily share my morsel with any comer.

Including environmental historians. Chapter 6, which attempts to explain—by reference to changing scientific ideas and practices—much of the extraordinary shift in attitudes toward whales and dolphins that occurred across the 1960s and 1970s, is at least a contribution to the history of environmentalism in Europe and North America in the period associated with the Vietnam conflict and the rise of the counterculture. If I am right, this story is a remarkable instance of crossing lines of biology, linguistics, information theory, and acoustics, all of which get tangled up in an unlikely hot tub churning Cold War bioscience, ocean theme park entertainment, sexual liberation, and mind-altering drugs. The story of learning to love the whales is an adult swim, as it turns out, and I very much hope that this chapter makes the case for pushing the links between the history of science and the history of environmental ideas and movements.<sup>5</sup> Is it, or are any of the other parts of this book, really engaged with environmental history?

5. The story I tell in chapter 6 can be usefully read in conjunction with Adam Rome's work on the relationship between the environmental movement and the culture of the 1960s: Adam Rome, "Give Earth a Chance': The Environmental Movement and the Sixties," *Journal of American History* 90, no. 2 (September 2003): 525–54.

I would like to think so. In important ways, for instance, I have accepted the arguments of a set of pioneering scholars over the last two decades who have insisted that animals and our relations with them constitute a crucial subject for historical investigation. This study seeks to contribute to a robust literature on human-animal relations and the historical construction of the human-animal boundary. By rearranging a history of several quite disparate modes of scientific research in the twentieth century (reproductive physiology, psychology, biological oceanography, population dynamic modeling, acoustics) around a specific taxon, I aim to show the value of thinking with animals. Some would argue, I think, that this historiographic move (which I am by no means the first to make) does not really bring us into the heartland of environmental history. But there is more to my story than that: the tapping of the ocean resources of the Antarctic Convergence in the first half of the twentieth century, for example, certainly represents an instance of human-driven environmental change that can vie with the most salient and historically significant episodes of such phenomena, and here we would seem to be very squarely on the environmental historian's terrain. Though of course we are not on terra firma at all, but out upon the oceans, which have to date proved somewhat recalcitrant historical subjects. There is reason to think this is changing: American historians recently heard a clarion call for new work in the environmental history of the oceans, and I would be delighted if this book found readers intending to make new contributions in this area—not least because I have benefited from my exposure to this scholarship and have presented much of this work to colleagues in this field over the last several years.<sup>6</sup>

But here too I am aware of the challenges. There are, for instance, some fundamental differences in approach that militate against easy synthesis of history of science and environmental history, despite their shared terrain. A slightly caricatured account of the problem would run something like this: environmental historians are inclined to deploy as historical *explanans* some of the very findings that historians of science consider the *explanandum*. This tends to frustrate the historian of science. At the same time, the arguably exaggerated preoccupation with treating “nature” as endlessly and ineluctably constituted by human discourse or practices can

6. I am thinking of W. Jeffrey Bolster, “Opportunities in Marine Environmental History,” *Environmental History* 11, no. 3 (July 2006): 567–97, and Bolster, “Putting the Ocean in Atlantic History: Maritime Communities and Marine Ecology in the Northwest Atlantic, 1500–1800,” *American Historical Review* 113 (February 2008): 19–47.

(not wholly unreasonably) strike the practicing environmental historian as either sophomoric, paranoiac, quixotic, or downright nuts—or, I suppose, as some combination of all of the above.<sup>7</sup> I must say that I am inclined to think this problem basically insurmountable. At any rate, I have not surmounted it. But I have reconnoitered the escarpment, thrown a grapple or two, hollered over the ridge. I hope the environmental historians who make their way through this book will discern evidence of my attempts, and that they can find things of use herein. There remain, though, a number of environmental-historical approaches I have not even attempted. Just one example: Do I give the whales “agency” in this book? Not really. There aren’t any whales in this book, remember? Only words about whales. Though many of those words, particularly in chapter 6, are exactly about the agency of the cetaceans—about their inner lives, their minds, their efforts to “tell us what they are thinking.” But it is the emergence of much talk on this subject that I am trying to *explain* in this chapter. What were the whales saying? I have no idea. Do I give too much agency to (human) words? Maybe. It is ever thus with bookish folk. If it is whales you want, you have to go to sea.

And with that, let me turn to a brief history of those who did just that: the whalers. The scientists would follow in their wake.

7. There is a considerable literature that goes after these problems patiently and seriously. For a useful recent position paper, consider Sverker Sörlin and Paul Warde, “The Problem of the Problem of Environmental History: A Re-reading of the Field,” *Environmental History* 12, no. 1 (January 2007): 107–30.



## This begins the excerpt from Chapter 6

Born in 1915, Lilly, from a well-to-do family in Saint Paul, Minnesota, took a bachelor of science degree from the California Institute of Technology in 1938 and studied at Dartmouth Medical School for two years before moving to the University of Pennsylvania, where he completed his MD in 1942 and remained on the faculty. There, under the influence of Britton Chance and Detlev Bronk, Lilly pursued research in biophysics, including applied investigations into real-time physiological monitoring—work linked to wartime service in military aviation, where techniques for assaying the respiration of airmen were needed.<sup>64</sup> Lilly had contact through his family with the neurosurgeon Wilder Penfield in the later 1940s and developed an interest in neuroanatomy and the electrophysiology of the brain. By 1953 he had been appointed to the neurophysiology laboratory of the National Institute of Mental Health, where he worked under Wade Marshall as part of a joint research program with the National Institute of Neurological Diseases and Blindness.

By the mid-1950s Lilly's lab in Bethesda, Maryland, was performing *in vivo* electrical stimulation of the brains of macaques—work aimed at cortical mapping by means of correlating point applications of currents at varying thresholds with specific behaviors and reactions in subject animals.<sup>65</sup>

64. This work was done through the E. R. Johnson Foundation for Medical Physics, which was run by Bronk, and which had contracts with the army and navy air forces through the Committee on Medical Research of the Office of Scientific Research and Development. Interestingly, in light of Lilly's later work on underwater breathing masks at NIMH, these gas-monitoring technologies were applied, among other things, to detect mask leakage. For a discussion of the nitrogen meter Lilly apparently helped to develop, see "Curriculum Vitae, John Cunningham Lilly, M.D. 1968," p. 2, Stanford University Library, Lilly Papers, box 3C2[D1], file "CRI personnel." See also Lilly and Thomas F. Anderson, "The Nitrogen Meter: An Instrument for Continuously Recording the Concentration of Nitrogen in Gas Mixtures," Report 299, 28 February 1944, National Research Council, Division of Medical Science, Acting for the Committee on Medical Research of the Office of Scientific Research and Development, Committee on Aviation Medicine. This device used photoelectric monitoring.

65. It is important to emphasize the rapid growth of this sort of work in the period. It was in 1954 that Olds and Milner demonstrated that a rat could learn to stimulate its own brain, and later investigations by Delgado and others demonstrated similar behavior

Reporting on some of these investigations at a conference on the reticular formation of the brain, held in Detroit in 1957, Lilly would explain,

The neurophysiologist has been given a powerful investigative tool: the whole animal can be trained to give behavioral signs of what goes on inside. . . . We are in the position of being able to guess with less margin of error what a man might feel and experience if he were stimulated in these regions.<sup>66</sup>

This was, in many ways, unpleasant business, Lilly acknowledged, pointing out that he had “spent a very large fraction of my working time for the last eight years with unanesthetized monkeys with implanted electrodes.” In addressing the nebulous region where neurology, psychology, and animal behavior overlapped, Lilly permitted himself some observations on the affective universe of his scientific subjects:

When an intact monkey grimaces, shrieks, and obviously tries to escape, one *knows* it is fearful or in pain or both. When one lives day in and day out with one of these monkeys, hurting it and feeding it and caring for it, its experience of pain or fear is so obvious that it is hardly worth mentioning.<sup>67</sup>

It would not be the last time that Lilly would reflect on the inner lives of his experimental animals with considerable confidence. But his experimental animal was about to change. Like a number of American psychology researchers in the mid-1950s—including the echolocation researcher Winthrop Kellogg—Lilly was in the process of leaving monkeys behind for the bottlenose dolphin, *Tursiops truncatus*.

His first brush with cetology came in 1949 when, during a visit to a neurosurgeon friend on Cape Cod, Lilly learned that a recent storm had beached a whale on the coast of southern Maine. A plan took shape for an

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in cats, as well as the reverse—namely, learning to turn off a current that apparently caused pain/fear/discomfort.

66. John C. Lilly, “Learning Motivated by Subcortical Stimulation: The Start and Stop Patterns of Behavior,” in *Reticular Formation of the Brain*, ed. Herbert H. Jasper et al. (Boston: Little Brown, 1958), 705–21, at p. 705.

67. *Ibid.*, p. 719.

impromptu expedition north, with a view toward collecting a novel brain.<sup>68</sup> As it happened, Lilly was acquainted from his days at the University of Pennsylvania with the Swedish-Norwegian physiologist and oceanographer Per F. “Pete” Scholander, who had also worked with Detlev Bronk in aviation physiology during World War II and had then moved to WHOI.<sup>69</sup> Scholander—something of a daredevil, and fascinated by the physiology of extreme environments—had published research on dive physiology and decompression, and while still living in Scandinavia he had conducted a number of pioneering studies on the deep-diving capabilities of marine mammals, particularly whales.<sup>70</sup> Lilly looked up Scholander and recruited him for the trip, and the three men suited up for a drive to Maine. Shortly after reaching the carcass (a large pilot whale), exposing the skull, and beginning to chip away toward the brain, they were joined by two other researchers who had independently made the drive up from Woods Hole: William Schevill and his wife and collaborator Barbara Lawrence. They were, reportedly, somewhat miffed to discover that they had been beaten

68. Lilly recounts this story in *Man and Dolphin* (Garden City, NY: Doubleday, 1961), 40–47.

69. Much can be learned about Scholander’s work from his autobiography, *Enjoying a Life in Science: The Autobiography of P. F. Scholander* (Fairbanks: University of Alaska Press, 1990), and a shorter memoir published earlier, “Rhapsody in Science,” *Annual Review of Physiology* 40 (1978): 1–17. Scholander attended the First International Symposium on Cetacean Research. His papers are held at Scripps. I have consulted these holdings (5 boxes), which include some interesting material on his work with whales and dolphins, including a set of photographs depicting his visit to Brødrene Saebjørnsen’s whaling station in Steinshamn, Norway, in the 1930s. These papers also contain a folder of his notes on the hydrodynamics of dolphin bow riding, work that resulted in a pair of articles in *Science* in 1959: “Wave-Riding Dolphins: How Do They Do It?” *Science* 129, no. 3356 (24 April 1959): 1085–87, and, with Wallace D. Hayes, “Wave-Riding Dolphins,” *Science* 130, no. 3389 (11 December 1959): 1657–58.

70. The most substantial early piece of this work was the monograph published in 1940 in *Hvalrådets Skrifter*: P. F. Scholander, “Experimental Investigations on the Respiratory Function in Diving Mammals and Birds,” *Hvalrådets Skrifter* 22 (1940): 5–131. I write about this work in “Self-Recording Seas,” in *Oceanomania: Souvenirs of Mysterious Seas*, ed. Mark Dion and Sarina Basta (London: Michael Mack, 2011). A valuable discussion of Scholander’s research in this area, along with a full bibliography, can be found in John W. Kanwisher and Gunnar Sundnes, eds., *Essays in Marine Physiology, Presented to P. F. Scholander in Honor of His Sixtieth Birthday*, *Hvalrådets Skrifter* 48 (Oslo: Universitetsforlaget, 1965). Some of the early experiments involved the use of pressure gauges affixed to whaling harpoons.

to the punch and particularly concerned that the hacksaw dissection might have damaged the airways of the upper head, which they had come to examine. In the end, however, the cadaver would be theirs, since Lilly and his partners found that the brain had largely been dissolved through autolysis; the smell alone overpowered them.

Though he headed home with little to show for the trip, Lilly had brushed the shores of cetology, and his curiosity did not dissipate. At a meeting of the International Physiological Congress four years later, in 1953, Lilly and Scholander again crossed paths, and Scholander suggested that Lilly get in touch with Forrest G. Wood at Marine Studios. Lilly did, and as a result, he was one of eight investigators to participate in what came to be known informally as the “Johns Hopkins expedition” in the autumn of 1955. It was, in a way, 1928 all over again: a mixed crew of physiologists and medical men gearing up to vivisect some bottlenose, only this time it would be in the carnival environs of a Florida ocean theme park, rather than a remote fishing village on a barrier island.<sup>71</sup>

In preparation for this 1955 trip, Lilly spent the summer in correspondence not only with Wood (securing access to a set of dolphins for experimental work), but also with Schevill at Woods Hole (concerning the anatomy of the airways of the common dolphin)<sup>72</sup> and with Scholander (concerning restraint techniques and the respiratory characteristics of the odontocetes).<sup>73</sup> Using this information, and reaching back to Orthello Langworthy’s work (discussed in chapter 3), Lilly worked up a dolphin respirator that would, it was hoped, permit the surgeons and neuroscientists of the party to expose the brain of an anesthetized animal in order to begin the work of cortical mapping by neurophysiological techniques.

The Johns Hopkins expedition of 1955, like its counterpart in 1928, was at best a qualified success. Lilly and the other investigators were unsuccessful with their anesthetics and their respirator, and in the end they euthanized, without dexterity, five dolphins, apparently alienating a number of the Ma-

71. The investigators, in addition to Lilly, were J. Rose, V. Mountcastle, and L. Kruger from Johns Hopkins Medical School; C. Woolsey and J. Hind, University of Wisconsin; Karl Pribam, Institute for Living, Hartford, CT; and Leonard Malis, Mount Sinai Hospital. The full records of this work can be found in Stanford University Library, Lilly Papers, box 6A1–B1.

72. Ibid.

73. As early as 1940 Scholander had done respiratory analysis on several restrained and submerged *Phocoena*.

rine Studios personnel in the process.<sup>74</sup> The most significant result of the work was the securing of a set of particularly good (fresh) brain specimens—perfused with preservatives before decomposition could begin—on which Lilly’s expedition colleague Lawrence Kruger (who would soon move to UCLA’s Brain Research Institute) would later conduct neuroanatomical research, some of which would be presented in Washington in 1963. By that time Lilly was (privately) accusing Kruger of sitting on the specimens and thus inhibiting competing interpretations of the neuron density and other features of the dolphin brain. By 1963, of course, with his name in lights across the idea of dolphin intelligence, Lilly resented Kruger’s relentlessly deflationary assessments of the cortical tissue of *Tursiops truncatus* (Kruger himself thought of them as merely dispassionate).<sup>75</sup> Nor was this the only controversy spawned during the Hopkins trip. The visit—and two shorter ones by Lilly that followed—would be a bone of contention between Wood and Lilly for years, finally coming to a head in Washington at the 1963 conference, with, as I will show below, significant repercussions.

If the 1955 investigations were not a triumph, they did deepen Lilly’s continuing interest in the cetacean brain.<sup>76</sup> Having heard a set of Wood’s recordings of bottlenose at Marine Studios, Lilly was much struck—like a considerable number of others at this time, as we have seen—by the range and apparent complexity of dolphin phonation. In October 1957 and again in 1958—after a visit with Schevill and Lawrence in Massachusetts, where they were conducting work on the auditory range and echolocatory capabilities of a bottlenose dolphin in a facility near Woods Hole—Lilly returned to Marine Studios. This time he was equipped to undertake investigations of the dolphin brain and behavior using techniques like those he had deployed and refined with macaques at NIMH; namely, percutaneous electrodes, driven by stereotaxis, that could probe the brain tissue of an

74. Stanford University Library, Lilly Papers, box 6A1–B1. These records include minute-by-minute logs of each operation and phonograph disks recording the interactions of the scientists during each intervention. Given the broad disagreements that erupted later over this work, closer attention to these materials might prove interesting.

75. For “dispassion,” see *Whales, Dolphins, and Porpoises*, ed. Kenneth Stafford Norris (Berkeley: University of California Press, 1966), 237.

76. The 1955 investigations also set in motion the research that would lead, almost a decade later, to the first successful techniques for major surgery on the small whales. See E. L. Nagel, P. J. Morgane, and W. L. McFarland, “Anesthesia for the Bottlenose Dolphin, *Tursiops truncatus*,” *Science* 146, no. 3651 (18 December 1964): 1591–93.

unanesthetized, living animal.<sup>77</sup> Over the two visits, three more animals were sacrificed, and Lilly experienced a kind of scientific epiphany that would shape his scientific life, even as its reverberations eventually unmade his scientific reputation.<sup>78</sup>

Compressing a complicated encounter that took place over several days—and that continued to draw Lilly’s reflections and reconstructions for years—is not easy, but we can summarize Lilly’s sense of his findings this way: First, Lilly persuaded himself that, in comparison to his experience with monkeys, the dolphins appeared to learn very rapidly how to press a switch to stimulate a “positive” region in their brains (and to turn off stimulation to a region causing pain).<sup>79</sup> Second, he claimed to have been much struck by the sense that an injured experimental subject, when returned to the tank with other dolphins, “called” to them and received their ministrations, suggesting an intraspecies “language.”<sup>80</sup> Third, on reviewing the tapes made of these investigations, Lilly grew increasingly certain that

77. See John Cunningham Lilly, John R. Hughes, Ellsworth C. Alvord Jr., and Thelma W. Galkin, “Brief, Non-Injurious Electric Waveform for Stimulation of the Brain,” *Science* 121, no. 3144 (1 April 1955): 468–69, and Lilly, “Electrode and Cannulae Implantation in the Brain by a Simple Percutaneous Method,” *Science* 127, no. 3307 (16 May 1958): 1181–82. Note that Lilly alleged that Schevill and Lawrence were working in a navy facility; Watkins (interview by the author, 9 August 2003) insisted that the work was being done in a private pool on Nonamesset Island, owned by the Forbes family.

78. It was also on these trips that Lilly got interested, through Wood, in the apparent ability of these animals to control the direction of their sound. Using an early AMPEX stereo tape recorder (on loan), Lilly and Wood were able to hear clearly that the click-trains emitted by captive dolphins had directional specificity. Wood discusses this finding in *Marine Mammals and Man: The Navy’s Porpoises and Sea Lions* (Washington, DC: Robert B. Luce, 1973). See also Gregg Mitman, *Reel Nature: America’s Romance with Wildlife on Film* (Cambridge, MA: Harvard University Press, 1999), 248.

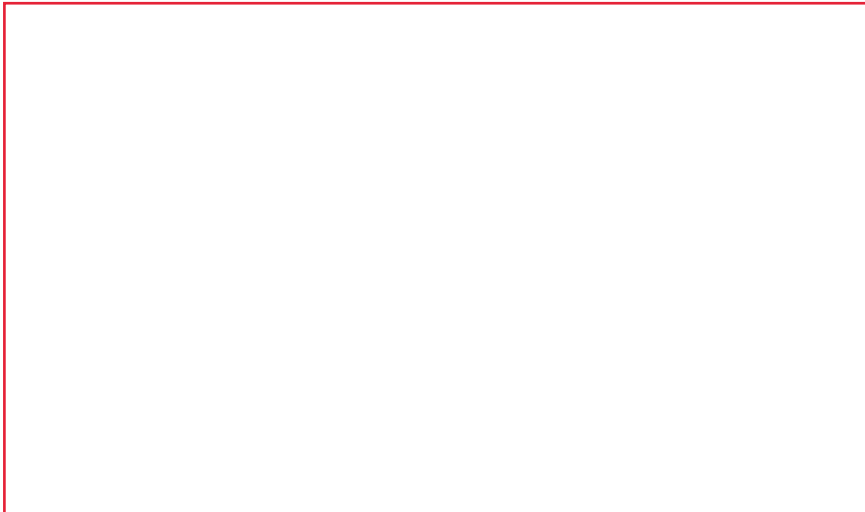
79. This was done by means of a switch, placed within reach of the animal’s beak. While I have never seen a reference to this problem, it must be asked whether contact with the switch could have been a product of convulsions and/or efforts by the animal to escape its constraints. Lilly’s repeated emphasis on the “purposive” could perhaps be read as special pleading.

80. This issue of the “distress call” was central to later disputes; trainers and animal handlers were well aware of “epimeletic,” or caregiving, behavior among these animals. Wood, and before him McBride and Hebb, had raised the subject of the “language” value—“language in the sense that a dog’s barking or growling is a language”—of these whistlings. See F. G. Wood, “Underwater Sound Production and Concurrent Behavior of Captive Porpoises, *Tursiops truncatus* and *Stenella plagiodon*,” *Bulletin of Marine Science of the Gulf and Caribbean* 3, no. 2 (March 1953): 120–33, at pp. 124–25.

his experimental subjects had been parroting his speech and other human sounds in the laboratory. These three elements—intelligence, an intraspecies language, and (perhaps most significantly) what he took to be fleeting glimpses of an attempt at interspecies communication—left Lilly with a feeling that he was on the cusp of something vast. Reflecting on the work of 1955, 1957, and 1958 in his Lasker Lecture in April of 1962, Lilly tried to explain:

We began to have feelings which I believe are best described by the word “weirdness.” The feeling was that we were up against the edge of a vast uncharted region in which we were about to embark with a good deal of mistrust concerning the appropriateness of our own equipment. The feeling of weirdness came on us as the sounds of this small whale seemed more and more to be forming words in our own language.<sup>81</sup>

After hammering his way into hundreds of mammalian brains, Lilly suddenly heard a voice.



81. Joan McIntyre, comp., *Mind in the Waters: A Book to Celebrate the Consciousness of Whales and Dolphins* (New York: Charles Scribner's Sons and Sierra Book Club, 1974), 71.

