

The Rejection of Continental Drift

THEORY AND METHOD IN AMERICAN EARTH SCIENCE



Naomi Oreskes

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Theory and Method in
American Earth Science

NAOMI ORESKES

New York Oxford
Oxford University Press
1999

Oxford University Press

Oxford New York
Athens Auckland Bangkok Bogotá Buenos Aires Calcutta
Cape Town Chennai Dar es Salaam Delhi Florence Hong Kong Istanbul
Karachi Kuala Lumpur Madrid Melbourne Mexico City Mumbai
Nairobi Paris São Paulo Singapore Taipei Tokyo Toronto Warsaw
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Published by Oxford University Press, Inc.
198 Madison Avenue, New York, New York 10016

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Library of Congress Cataloging-in-Publication Data
Oreskes, Naomi.

The rejection of continental drift : theory and method
in American earth science / Naomi Oreskes.
p. cm.

Includes bibliographical references and index.

ISBN 0-19-511732-8; ISBN 0-19-511733-6 (pbk)

1. Continental drift.
2. Geology—United States—History—20th century.
 - I. Title.

QE511.5.074 1999
551.1'36—dc21 98-4161

1 3 5 7 9 8 6 4 2

Printed in the United States of America
on acid-free paper

To Shlomo Flötzgebirge, my faithful companion,
and to K. B., who sees connections that no one else notices
and always keeps me on track.

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PREFACE

This book began in 1978 when I first studied geology at Imperial College in London. I had completed two years as a geology major at a leading U.S. university and counted myself lucky to have chosen a field of science heady in the wake of revolutionary upheaval: geologists around the globe were reinterpreting old data and long-standing problems in the new light of plate tectonics. It seemed a good time to be an aspiring young earth scientist. Imagine my surprise—and dismay—to discover in England that the radically new idea of plate tectonics had been proposed more than half a century before by a German geophysicist, Alfred Wegener, and widely promoted in the United Kingdom by the leading British geologist of his era, Arthur Holmes. The revolution that had been described by my professors in the United States as the radical revelation of a dramatically new vision of the earth was viewed by many of my professors in England as the pleasing confirmation of a long-suspected notion. Whereas my textbooks in the United States had proclaimed the explanatory power of the new ruling theory, Dorothy Rayner, the doyenne of British Stratigraphy, dryly instructed in her text, *Stratigraphy of the British Isles*, “our stratigraphy and history has certainly been illuminated by the current hypothesis, but so far the light shed is somewhat uneven.” And although I had only just learned of these ideas two years before, my English flatmate could pull out the dog-eared copy of Arthur Holmes’s 1945 textbook she had read in elementary school.

The seeds of an intellectual inquiry were sown. For some years they lay dormant while I lived the life of a field geologist, although the ground in which they lay was being heavily fertilized. Working as a professional geologist in Australia, I learned—often from mildly indignant colleagues—not only that many Australian geologists knew about and believed in the idea of continental drift in the 1940s and 1950s but also that in several instances they were ridiculed at international meetings or on visits to the United States by rude and arrogant Americans. I also learned that other theories of

crustal mobility, including the expanding earth hypothesis, had been advocated and in some cases were still being advocated by Australian geologists. One Australian who was receptive to the idea of an expanding earth was my employer, the director of exploration of Australia's third-largest mining company, who periodically circulated papers on this topic among his scientific staff. It was evident that the recent history of earth science was much more complex, much more nationalistic, and much more *interesting* than my professors and textbooks—or my readings in the philosophy of science—had ever suggested.

In the early 1980s, I returned to the United States to pursue graduate studies in geology and again encountered a conundrum. My English training and Australian experience had inculcated in me an inductive methodology, in which scientific problems originated in the observation of geological phenomena in the field, but many of my American professors disdained inductive science and what they pejoratively dismissed as “outcrop” geology. They encouraged me to pursue a deductive strategy and to rely primarily on the tools of laboratory analysis. This was particularly true of younger professors and those who had achieved a high level of professional recognition. The issue was not one of theoretical belief but of methodological commitment. My American and British professors promoted contradictory and ultimately incompatible views about the right way to generate scientific knowledge. Two strands began to merge: divergent visions of the recent history of earth science and divergent methodological commitments. Was there some relation between the two? So began the active portion of the inquiry represented by this book.

My debts are thus spread over several continents. My research advisors at Stanford University, Peter Galison and Marco T. Einaudi, encouraged me to pursue the questions raised in this book while still engaging in scientific research. Neither of these men has ever allowed his thinking to be constrained by the historically contingent boundaries of academic disciplines, and for this I am deeply grateful. Among the Stanford faculty, past and present, I am also indebted to Nancy Cartwright, who profoundly influenced my thinking; to Dennis Bird, John Bredehoeft, John Dupree, Jane Maienschein, James O'Neil, Tjeerd van Andel, and Norton Wise; and to fellow graduate students David Magnus, Carey Peabody, Barbara Bekken, Lisa Echevarria Benatar, Peter Mitchell, Hilary and Jon Olson, Nicolas Rose, and Allan Rubin. In Australia I am indebted to Roy Woodall, formerly Director of Exploration of Western Mining Company, and to colleagues at Western Mining and BP Minerals who shared historical anecdotes; at Imperial College, London, to John Knill, Paul Garrod, Angus Moore, Mike Rosenbaum, Ernie Rutter, Richard Sibson, and the late Janet Watson, all of whom inspired me in important ways; and at Dartmouth College to Claudia Henrion, Richard Kremer, and the late, great Chuck Drake, who I dearly wish had lived to read this book.

Throughout this project I have benefited from the intellectual generosity and moral support of many colleagues: Duncan Agnew, Ron Doel, Robert Dott Jr., Mott Greene, David Kaiser, Homer LeGrand, Phil Pauley, Ron Rainger, Martin Rudwick, and Kenneth Taylor commented on the manuscript; Allan Allwardt, Richard Creath, Henry Frankel, Eli Gerson, Carl-Henry Geschwind, Bruce Hevly, Rachel Laudan, Leo Laporte, Chandra Mukerji, Robert Smith, and Don White provided feedback and information; Helen Wright Greuter and Finley Wright gave me access to their father's letters; Michele Aldrich sent historical tips. Among librarians and archivists, I am indebted to Deborah Day of the Scripps Institution of Oceanography, Charlotte Dirksen and Henry

Lowood at Stanford, Barbara DeFelice and the late Susan George at Dartmouth College, Ronald Brashear at the Huntington Library, Susan Vasquez and Shaun Hardy at the Carnegie Institution of Washington, and the staff of the Yale University Libraries. In Ireland, I thank Patrick Wyse Jackson and the staff of the Trinity College Archives; in South Africa, Leonie Twentyman-Jones and Jill Gribble at the University of Cape Town, the family of Alexander du Toit in Johannesburg, and Mr. Gerry Levin of the Geological Society of South Africa, all of whom provided extremely generous help. Thanks are also due to my research assistants Mary Ann Marcinciewicz, Drew Tenenholz, Jennifer Wehner, Virginia Terry, and Laurie Norris; to Karen Endicott, who proofread the entire text; to the staff of the Dartmouth College Day Care Center and the University Plaza Nursery School; and to Joyce Berry and Cynthia Garver at Oxford University Press.

I have had generous financial support from the National Science Foundation (SBE-9222597 and EAR-9357888), the National Endowment for the Humanities (Fellowships for University Teachers), the Mellon Foundation New Directions Fellowship (Stanford University), the William and Mary Ritter Memorial Fellowship of Scripps Institution of Oceanography, and the Burke Research Initiation Award Program at Dartmouth College. For continuous intellectual support and stimulation, I am grateful to my parents, Susan and Irwin Oreskes; my siblings Michael, Daniel, and Rebecca Oreskes; my husband, Kenneth Belitz, and, above all, my children, Hannah and Clara Belitz, who I hope will never stop asking “why?”

New York
March 1998

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The Rejection of
CONTINENTAL DRIFT

Evidence is hard to come by, it is largely
circumstantial, and there is never enough of it.
—*J. Hoover Mackin*

We only know causes by their effects.
—*William Whewell*

Introduction

The Instability of Scientific Truth

Scientists are interested in truth. They want to know how the world really is, and they want to use that knowledge to do things in the world. In the earth sciences, this has meant developing methods of observation to determine the shape, structure, and history of the earth and designing instruments to measure, record, predict, and interpret the earth's physical and chemical processes and properties. The resulting knowledge may be used to find mineral deposits, energy resources, or underground water; to delineate areas of earthquake and volcanic hazard; to isolate radioactive and toxic wastes; or to make inferences and predictions about the earth's past and future climate. The past century has produced a prodigious amount of factual knowledge about the earth, and prodigious demands are now being placed on that knowledge.

The history of science demonstrates, however, that the scientific truths of yesterday are often viewed as misconceptions, and, conversely, that ideas rejected in the past may now be considered true. History is littered with the discarded beliefs of yesterday, and the present is populated by epistemic resurrections. This realization leads to the central problem of the history and philosophy of science: How are we to evaluate contemporary science's claims to truth given the perishability of past scientific knowledge? This question is of considerable philosophic interest and of practical import as well. If the truths of today are the falsehoods of tomorrow, what does this say about the nature of scientific truth? And if our knowledge is perishable and incomplete, how can we warrant its use in sensitive social and political decision-making?¹

For many, the success of science is its own best defense. From jet flight to the smallpox vaccine, from CD players to desktop scanners, contemporary life is permeated by technology enabled by scientific insight. We benefit daily from the liberating effects of petroleum found with the aid of geological knowledge, microchips manufactured with the aid of physical knowledge, materials synthesized with the aid of chemical knowledge. Our view of life—and death—is conditioned by the

results of scientific research and the capabilities of technological control. Our moral and political judgments are colored by what science tells us is natural. Nearly every aspect of our lives has been affected by scientific knowledge or technical innovation facilitated by that knowledge. But science and technology have also brought us to the edge of an environmental abyss with its familiar litany of silent crises: global deforestation, ozone depletion, greenhouse effects, groundwater contamination, nuclear annihilation. Most of us stand as mute witnesses to this leviathan: we know surprisingly little about the process of scientific research that has brought us this confusing largesse.

Why do we know so little about how scientific knowledge is generated? Throughout the first half of this century, philosophers and historians of science were concerned primarily with the progression of scientific theories and rarely with the processes by which those theories came to be.² The mechanics of scientific research—the choice of questions and methods, the construction and application of instruments, the resolution of contradictions between overlapping data sets—traditionally have received scant attention. The reason was deliberate: analytic philosophers were interested in the logic of scientific theories and their demonstration, not in their genealogy. Viewing science as a rational enterprise, philosophers such as Hans Reichenbach explicitly denied the relevance of the methods by which scientific ideas came to fruition. According to Reichenbach, the philosopher of science was “not much interested in the . . . processes which lead to scientific discoveries. . . . He is interested not in the context of discovery, but in the context of justification.”³

Reichenbach’s ideal has been radically challenged in the last several decades. Most contemporary historians and sociologists of science now believe that the social context of science—the context of discovery—is *more* important than the context of justification.⁴ Any theory can be rationally reconstructed to sound logical, even inevitable, in retrospect, but this tells us little about how scientific knowledge actually develops.⁵ Scientists are not free agents, historians and sociologists have argued, and the social context of their work not only delimits their options but may even determine the content of their knowledge.⁶ And if all knowledge is socially constructed, then objectivity is a chimera. This radical claim strikes at the heart of scientists’ beliefs about their enterprise.

Not surprisingly, scientists (and many others) resist such a view and argue that these historians have missed the point. Scientists are interested in truth, and since scientific knowledge works, it must be more or less correct. Human factors sometimes “get in the way” of objective knowledge, but the point of science is to resist such influences. If scientists permit social pressures to distort their perceptions of the physical world, then they are failing to live up to the scientific ideal. Of course, scientists do fail, because they are human, but in most cases it scarcely matters because bad science gets weeded out by the collective scrutiny of the scientific community and—imperfect or not—the surviving knowledge *works*.⁷ To attack science simply because it exists within human culture is pettiness, at best. At worst, some believe, it is to attack the very foundations of rationalism.

Historians would argue that it is scientists who have missed the point. *All* science is socially structured, both the good and the bad, and so is the peer review system that adjudicates between them. But can historians prove this point? Historical case studies can illustrate how the development of a particular idea—including our best science—

reflects the constraints of historical situations, and in recent years historians have produced many such studies.⁸ But in many such contextualized histories of science, social context is a kind of miasma that pervades scientific thinking in an intangible and ultimately inexplicable manner.⁹ The evidence for the role of social forces in the production of scientific knowledge is almost always circumstantial. If scientists are consciously struggling to generate knowledge independent of time and place and historians are claiming that this is an illusory or even meretricious goal, then either scientists are self-deluded or historians are disingenuous.¹⁰ Can these positions be reconciled? Can knowledge be both contingent and transcendent?

Part of the answer may lie in the realm of scientific methods and the process of scientific research. Until recently, few historians, philosophers, or scientists focused their concern on the epistemic status of scientific process.¹¹ The scientific method, always in the singular, has been taken as monolithic and unproblematic—a textbook cliché, the one sure thing we all know about science. But is there such a thing as *the* scientific method? The answer is clearly no. From the past two decades of historical scholarship, one insight has emerged unequivocally: the methods of science are complex, variegated, and often local. Throughout history scientists have drawn on a wide variety of epistemic commitments and beliefs, linguistic and conceptual metaphors, and material and cognitive resources, all of which have changed with time and varied in space. At different times or in different social contexts, scientists have preferred either inductive or deductive modes of reasoning and argumentation, experimental or theoretical approaches to problem-solving, laboratory- or field-based methodologies. Various attempts have been made to prove the superiority and permanently establish the hegemony of particular methodological approaches, but most of these attempts have failed. The diversity of scientific methodology persists. And whatever methods scientists have chosen, it is through these choices that representations of the natural world have been forged from the infinite sea of sensory perceptions. Scientific practices are the tools with which scientists link the phenomenological world and their representations of that world. Perhaps they provide the link between science and the human world as well.

The Problem of Continental Drift

The story of continental drift illustrates how choices about methods constrained possibilities for scientific truth. In the early part of the twentieth century, a number of scientists—most notably Alfred Wegener—proposed that the relative positions of the continents of the earth were not fixed. Among the leading North American scientists of the 1920s and 1930s—members of the U.S. National Academy of Sciences, presidents of scientific societies, professors at distinguished universities, and men for whom we have medals named today—continental drift was widely discussed and almost uniformly rejected, not merely as unproved, but as wrong, incorrect, physically impossible, even pernicious. American scientists were much more hostile to the idea than their European counterparts; some even labeled the theory *unscientific*.¹² Yet forty years later, the basic central idea of continental drift—that the continents are not fixed but move horizontally over the face of the earth—was established as scientific fact. Why did distinguished scientists adamantly reject as false a claim now universally accepted as true?¹³

The standard answer, to be found in most geology textbooks and many works on the history of science, is that continental drift was rejected for lack of an adequate causal mechanism. Because scientists could not explain *why* continents moved, they concluded that they *could not move*. But this is an example of a rational reconstruction in the history of science. Because moving continents are now accepted as fact, scientists have tended to assume that if the idea was rejected by earlier scientists, then those scientists must have had a good reason to reject it. Perhaps there was not enough evidence. Perhaps the arguments were inadequately articulated. Perhaps the people who proposed the ideas were not well-known, or published their ideas in obscure places. When scientists and historians in the 1960s and 1970s looked back at Alfred Wegener's work, they found an obvious deficit in that the mechanism he proposed was not the same as that accepted today. Indeed, it was patently implausible by today's standards. The implausibility of Wegener's mechanism was taken as the obvious explanation of why his theory was rejected.

But this explanation is anachronistic. What matters in historical argument is not what seems plausible to us, looking backwards, but what was said and done at the time the events took place. What matters is what was plausible to *them*. Viewed this way, the standard account is demonstrably false. Part I of this book shows how Wegener's mechanism was not considered hopelessly implausible at the time it was first proposed; it drew on a large body of work in geodesy and physical geology that pointed to flow in a plastic substrate beneath the crust. Wegener argued that this flow, widely accepted in the 1920s as a demonstrated phenomenon, could help to account for continental drift. Broadly construed, Wegener's argument holds today. But more important, Wegener's proposed mechanism was just the opening round in a series of discussions and debates about the mechanism of continental drift. By the early 1930s, a number of mechanisms had been proposed for continental drift, including the one that is generally accepted as the driving force of plate tectonics: convection currents in the earth's asthenosphere.

So why was continental drift rejected? The thesis of this book is that American earth scientists rejected the theory of continental drift not because there was no evidence to support it (there was ample), nor because the scientists who supported it were cranks (they were not), but because the theory, as widely interpreted, violated deeply held methodological beliefs and valued forms of scientific practice. The idea of the motion of continents, the empirical evidence for it, and the mechanical explanation of it developed by Arthur Holmes have all been corroborated by contemporary earth science.¹⁴ But to accept these ideas in the 1920s or early 1930s would have forced American geologists to abandon many fundamental aspects of the *way they did science*. This they were not willing to do.

The conclusion of this book, therefore, is that *science is not about belief; it is about how belief gets formulated*. At any given moment, only a finite set of knowledge satisfies the reigning criteria for the formulation of scientific belief, and only *this* knowledge is eligible as truth. But the discriminating criteria are historically contingent; over time and across communities, they shift, they evolve, they are overthrown, they transmute. The changing criteria for the formulation of belief provide the pathways through which cultural context delimits the boundaries of scientific knowledge.

NOT THE MECHANISM

Joly and Holmes have a beautiful theory,
and I believe it will be epoch-making.

—*Chester Longwell, January 1926*

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Two Visions of the Earth

Plate tectonics is the unifying theory of modern geology. This theory, which holds that the major features of the earth's surface are created by horizontal motions of the continents, has been hailed as the geological equivalent of the "theory of the Bohr atom in its simplicity, its elegance, and its ability to explain a wide range of observation," in the words of A. Cox.¹ Developed in the mid-1960s, plate tectonics rapidly took hold, so that by 1971, Gass, Smith, and Wilson could say in their introductory textbook in geology:

During the last decade, there has been a revolution in earth sciences . . . which has led to the wide acceptance that continents drift about the face of the earth and that the sea-floor spreads, continually being created and destroyed. Finally in the last two to three years, it has culminated in an all-embracing theory known as "plate tectonics." The success of plate tectonics theory is not only that it explains the geophysical evidence, but that it also presents a framework within which geological data, painstakingly accumulated by land-bound geologists over the past two centuries, can be fitted. Furthermore, it has taken the earth sciences to the stage where they can not only explain what has happened in the past, and is happening at the present time, but can also predict what will happen in the future.²

Today moving continents are a scientific fact. But some forty years before the advent of the theory of plate tectonics, a very similar theory, initially known as the "displacement hypothesis," was proposed and rejected by the geological fraternity. In 1912, a German meteorologist and geophysicist, Alfred Wegener, proposed that the continents of the earth were mobile; in the decade that followed he developed this idea into a full-fledged theory of tectonics that was widely discussed and debated and came to be known as the theory of continental drift. To a modern geologist, raised in the school of plate tectonics, Wegener's book, *The Origin of Continents and Oceans*, appears an impressive and prescient document that contains many of the essential

features of plate tectonic theory.³ His ideas were substantiated by an impressive volume of data culled from diverse branches of earth sciences. Yet Wegener's theory was rejected by most of his contemporaries. In the British Commonwealth and in continental Europe, he gained a minority following; in the United States his ideas were resoundingly rejected and in some quarters ridiculed. Thirty years after the last edition of Wegener's book was published, however, a major revolution in earth sciences occurred that incorporated many of the essential features of Wegener's theory. In light of the considerable overlap between Wegener's concepts and a modern theory that commands virtually universal support, why was the theory of continental drift rejected?⁴ Why did American geologists in particular react so negatively to the idea of moving continents? To answer these questions, we must first ask where the theory of continental drift came from and what it sought to replace.

Tectonics in the Nineteenth Century

One of the outstanding problems of early-twentieth-century geology was the origin of mountains. Throughout the eighteenth and nineteenth centuries, geologists had focused interest on mountain ranges, for their economic importance as physiographic boundaries and sources of mineral resources, for their scientific importance as repositories of large-scale exposures of the earth's internal structure, and for the psychic impression made by their size and beauty. In the nineteenth century, causal theories of mountain-building became a primary focus of geological concern in both the United States and Europe.⁵ Theorists such as Eduard Suess in Austria and James Dana in the United States proposed that mountains formed through compressive stresses generated by a gradual thermal contraction of the whole earth. But these two men—and the colleagues they influenced—held very different views of contraction.

Suess's Collapsing Earth

Eduard Suess (1831–1914) was a field geologist and professor of geology at the University of Vienna (figure 1.1) who dedicated his career to unraveling the structure of the Swiss Alps. From 1883 to 1904, Suess published his magnum opus, a four-volume treatise entitled *Das Antlitz der Erde*, in which he articulated the theory of thermal contraction as the earth's mechanism of mountain-building. (These volumes were published in English between 1904 and 1909 as *The Face of the Earth*, translated by the English geologist Hertha Sollas.) Suess likened the process of terrestrial contraction to the wrinkling of the skin of a desiccating apple and suggested that mountains resulted from a wrinkling of the earth's crust to accommodate diminishing surface area. The earth's contraction, he proclaimed, allowed us to witness “the breaking up of the terrestrial globe.”⁶

According to Suess's theory, the features of the earth were explained in terms of vertical motions of its crust. Initially, the entire earth was covered by a continuous continental crust. As the earth cooled and began to shrink, portions of the outer crust collapsed and began to form the ocean floor, causing the earth to become differentiated into continents and oceans. With continued cooling, the remaining uplifted portions were undermined and became unstable, and they collapsed to form the next generation of ocean basins. What had formerly been ocean became dry land. With



Figure 1.1. Eduard Suess.
(From Zittel 1901, p. 513.)

each additional increment of cooling, what had been continent became ocean basin and what had been ocean became continent.

The continual transformation of oceans into continents and vice versa explained a number of puzzling geological phenomena, such as the presence of marine deposits on dry land and the numerous alternating episodes of marine and terrestrial conditions recorded in stratigraphic successions. Perhaps most important, the theory explained the well-known similarity of fossil assemblages in widely separate continents: animals and plants had migrated and been dispersed across now sunken continents. Darwin had explained the divergence of plant and animal species as the result of natural selection in isolated communities under contrasting environmental conditions, but paleontologists had found the *same* fossil forms in widely separated continents with radically different biological and climatic environments. Paleontologists concluded that these areas must once have been contiguous and must later have broken apart. Suess's world of gradually decreasing environmental continuity could account for gradually increasing faunal diversity.

This evidence from historical geology was a primary motivation for most paleontologists and stratigraphers to accept the concept of "Gondwanaland"—named after the Gondwana system of India—the term used by Suess to describe the giant supercontinent that had once united much or all of the globe.⁷ Sunken portions were referred to as "land bridges" across which ancient species had traveled (although the broad swatches of sunken continents were more like platforms than bridges). But Suess's theory also had consequences for structural geologists: it suggested that mountains would occur across the globe—on all continents and all parts of continents—a prediction borne out by the widespread occurrence of mountains throughout Europe and nearby parts of Asia and North Africa. The collapse of continental blocks was also linked to the origin of igneous intrusions and volcanism, as molten rock escaped from the earth's interior along radial cracks formed during periods of collapse.

Contraction theory was a unifying account of global progression that explained both the history of life and the history of the planet on which life evolved. Writing in

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