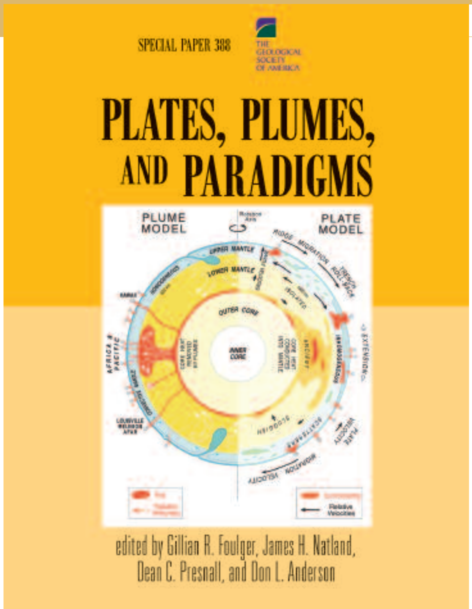
Plates, Plumes, and Paradigms[1]



Plates, Plumes, and Paradigms (PPP) is an outgrowth of a Penrose Conference ("Plumes IV: Beyond the Plume Hypothesis," 2003) held to develop alternatives to the currently dominant mantle plume model to explain the origin of melting anomalies such as Iceland and Hawai'i. The main objective of PPP is cogently stated in the volume's preface: "The fundamental question is whether plumes exist, and if they do, are they a common phenomenon in the mantle, or are they rare occurrences beneath particular localities?" With 861 pages of data, experiments, hypotheses, critiques, and historical-scientific reviews, PPP makes a powerful anti-plume case, which may be enough to shake the certainty of many who take the hypothesis for granted. As a perpetual scientific agnostic, I must admit that it is a pleasure to see so many assumptions questioned by so many and to have the alternatives to current dogma spelled out so clearly and comprehensively (PPP is weighty enough to be used for hand-to-hand combat if the debate should get too fierce). I have a vivid memory of the reaction I would typically get from my co-students (during the Early Anthropocene, 1986) whenever I questioned whether Iceland was actually the result of a plume. "What else could it be?" would be the inevitable reply. This book goes a long way towards spelling out what else Iceland could be. I could have used this book then; and I will certainly use it now.

Among the major problems with the plume hypothesis is the fact that many of the volcanic suites that are thought to be plume-derived lack the high $^3He/^4He$, large swells, large buoyancy fluxes, time-progressive volcanic chains, anomalously hot volcanism, high heat flow, or seismic signatures predicted by the hypothesis. Given these apparent inconsistencies with the model, many of which are documented in some detail in PPP, it is clear that the classical plume hypothesis can be retained only if many of its core axioms (immobility, deep enriched source) and predictions are abandoned. While the introduction of such extensive modifications to the plume hypothesis can sometimes reconcile data with theory, the adulterated hypothesis thus becomes essentially untestable, and plumes thereby become indefinable.

Many of the papers in PPP address the issues of whether or not "hotspots" are anomalously hot in contrast to normal MORB and of the relative importance of excess temperature versus fertile source composition as an explanation for the larger melt volumes of "hotspots" (or "notspots" as some would state it). For the Hawai'ian case (as for many other examples), there seems to be no compelling argument for hotter temperatures, and many geochemists have already proposed the participation of non-peridotitic source components to explain the larger magmatic fluxes and particular geochemical and mineralogical characteristics of these magmas. For Iceland, there is an ongoing debate about the temperature issue, which seems to hinge on how the composition of the "primary" melt is calculated, a matter about which there is no consensus as yet. The excess-volume argument for Iceland is addressed creatively in PPP by proposing that a large slab of Caledonian-age oceanic crust trapped in an orogenic suture zone is being recycled (i.e. the dominant Iceland source is not peridotite). That there is much discussion on this topic is no surprise, since the problem of hotspot magma genesis is extremely intractable, with the nature of the calculated melt and residue compositions being critically dependent on assumptions about source composition and melting processes. For that matter, there is little consensus on how MORB or arc magmas form either.

The basic data documenting the trends and age progressions of putative volcanic plume tracks are also considered in much detail in PPP. This phenomenological critique of the assumptions underlying the plume hypothesis is extremely effective. Data from several ocean basins and some continents show that most hotspot-type volcanism does not define coherent age-progressive tracks (contrary to popular belief), commonly exploits older linear structures (in many cases repeatedly), may occur synchronously at widely dispersed sites (right across Africa, e.g.), with age-spikes that can be correlated to documented plate-boundary reorganizations or collisional events. In a similar vein, the evidence for radiating dike swarms associated with plume-driven continental breakup is critically reexamined. Many apparent radiating patterns turn out to be composite structures, with directions controlled by an older tectonic grain or with several generations of intrusions constituting the pseudo-radial pattern.

Alternative plate tectonic explanations have been suggested for many volcanic features attributed to "hotspots," including crack propagation, reactivated and incipient plate boundaries, membrane and extensional stresses, gravitational anchors, reheated slabs, decompression melting of heterogeneous mantle, leaky transform faults, etc. Many of these models are discussed in PPP and a variety of plausible alternative explanations for the data are provided. The diversity of proposed mechanisms suggests that there is more than one way to induce mantle melting, which may thus produce a concomitant diversity in the types of hotspots seen on Earth.

One anomaly in PPP is the near-absence of debate about how plume intensity may have varied with time. Archaean komatiites are widely considered to have been hotter than modern MORB and are generally interpreted to have formed from plumes, with a minority view assigning some types of komatiite to a subduction environment. Here, too, there is a vigorous debate in the literature, which is only barely touched on in PPP. Another anomaly is the near-absence of dissenting voices. Although the pro-plume viewpoint is abundantly represented in the literature, it would have been nice to see a couple of summary papers by the most devoted defenders of the hypothesis. Despite the strong anti-plume case that is made in PPP, I can't help but feel that somewhere in the bathwater there might be a viable baby. Perhaps plumes are not as ubiquitous as is commonly assumed, but given our profound ignorance of the deep mantle it seems premature to categorically reject the possible existence of active plume-like instabilities. On the other hand, uncritical acceptance of the plume hypothesis to explain linear volcanic chains will never again be possible for anyone who takes the trouble to read PPP. Plume advocates now need to provide counter-arguments to the serious objections that are set out in PPP. Are we on the cusp of a paradigm shift? Will the students of 2056 look back on the Great Plume Debate as the moment when "modern" mantle dynamics originated? Whichever viewpoint ultimately prevails, PPP will remain an important milestone in the evolution of thought on mantle plumes by focusing and triggering debate on this important issue.

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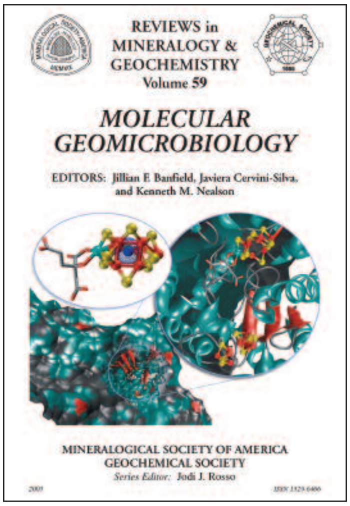
1 Foulger GR, Natland JH, Presnall DC, Anderson DL, editors (2006) Plates, Plumes, and Paradigms. Geological Society of America Special Paper 388, 861 pp, ISBN 978-0-8137-2388-4, US$180

Molecular Geomicrobiology[2]



The chapters in *Molecular Geomicrobiology* reflect the great advances made in the last few years in our understanding of microbial roles in mineral formation, mineral diagenesis and degradation. They reinforce expectations of further rapid progress in the field. They also clearly demonstrate how important the extensive collaboration of environmental microbiologists, molecular microbiologists, geochemists, mineralogists, organic and inorganic chemists, as well as specialists in each of these fields, has become to geomicrobiology. Such collaboration provides the complementary expertise in different fields and the special instrumentation in the laboratories of these experts that are needed to attain a desired research goal.

The volume was prepared for a short course entitled Molecular Geomicrobiology, sponsored by the Mineralogical Society of America, the Geochemical Society of America, the US Department of Energy, and NASA Astrobiological Institute, and held at the University of California Berkeley, December 3–4, 2005 prior to the fall AGU meeting in San Francisco.

Depending on their scientific background, different readers of this volume may find some chapters of greater interest than others, but that should in no way discourage them from perusing the other chapters. All chapters include a wealth of references, with emphasis on the most recent literature pertinent to the topic of the chapter. All authors make clear that much remains to be discovered and that molecular geomicrobiology is a work in progress.

Among the topics treated in this volume are the following: a consideration of how genetics can be applied to geomicrobiology and geobiology in general; electron transport phenomena in microbial redox interaction with certain metals of mineralogical interest in the environment; siderophore interaction with iron-bearing minerals in marine environments; iron cycling in a geomicrobial context; nanoscale mineral particles, some of their properties, and aspects of microbial formation of and interaction with such particles; the role of the organic–mineral interface in the formation of biominerals by prokaryotic and eukaryotic microbes; speculation about the transition from abiotic to biotic catalysis in the origin of life; the evolution of carbon and nitrogen cycling in the Precambrian and beyond; advances in the use of biomarkers to construct the tree of life; the use of biomolecular techniques in studying population dynamics in extreme environments; and examples of the use of genomics and proteomics in geomicrobiology.

For instructional purposes, this volume would have benefited from a glossary of special terms and acronyms used in various chapters, and an index.

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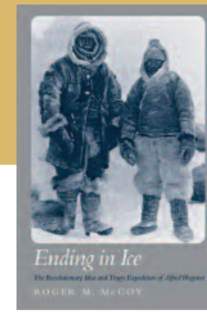
2 Banfield JF, Cervini-Silva J, Nealson KM, editors (2005) Molecular Geomicrobiology. Reviews in Mineralogy & Geochemistry 59, Mineralogical Society of America and Geochemical Society, Chantilly, VA, ISBN: 093995071–5, US$40.00

Ending in Ice
– The Revolutionary Idea and Tragic Expedition of Alfred Wegener[3]



In *Ending in Ice*, Roger M. McCoy, emeritus professor of physical geography at the University of Utah, recounts the life and accomplishments of Alfred Wegener. Of course, the name is familiar to Earth scientists and intertwined with the term “continental drift” (we learn that “drift” was not the best translation of Wegener's term *Verschiebung*: “displacement” would have been a better term). But how did an astronomer/meteorologist with no background in geology and geophysics come to publish on what was at the time a revolutionary idea in Earth sciences? As a climatologist, Wegener became interested in past climates and started examining fossil evidence worldwide with his father-in-law, Wladimir Köppen (who published a classification of climates still in use today). He became intrigued by the similarities of nonmarine Paleozoic fossils in Africa, Brazil, and India and started digging for more evidence.

Others before him had postulated that continents might once have been joined into a single supercontinent and had drifted into their present position, but Wegener was the first to compile extensive geological, geodetic, and paleontological data in support of the idea. He first presented his concept in 1912 at a meeting of the Geological Association of Frankfurt. His book *The Origin of Continents and Oceans* was published in German in 1915. The subsequent three editions were extensively revised and new data added. The third edition (1922) was translated into French, Spanish, and English in 1924. Once the English version became available, the hypothesis became more widely known. In 1926, the Royal Society and the Association of Petroleum Geologists convened special sessions to discuss the theory. For lack of a plausible mechanism to move the continents, the idea was heavily criticized and abandoned. It would not be until the 1960s that new evidence would lead to the birth of plate tectonics.

Born in Berlin in 1880, Wegener first worked as an astronomer, then as a meteorologist. In 1906, he was invited to join a Danish expedition to Greenland. This expedition initiated his other lifelong scientific interest: the climate of Greenland. Why was there so much interest in Greenland's climate at that time? Air travel was expected to become commonplace in the near future, and many routes would pass over Greenland. McCoy provides a gripping account of the harsh conditions and complex logistics of the 1929–1930 expedition led by Wegener. This expedition was the first to establish a station in the middle of Greenland at Eismitte with the purpose of collecting climatic data in winter. Establishing that station was the undoing of Wegener. He died tragically of exhaustion returning to the coast from Eismitte.

In the final chapters, McCoy summarizes the advances that eventually led to the acceptance of plate tectonics. As such, it provides an excellent summary of how all the pieces of the puzzle came together in the 1960s. Such historical accounts are invaluable for putting progress and changes in paradigms in perspective. Wegener's story illustrates what holds true for many scientific breakthroughs: people deny it is true at first; then they deny it is important; then they credit the wrong person. So before shooting down the next new idea that comes along, think twice.

Many archival photographs and an index add to the enjoyment and usefulness of the book, which reads like a novel.

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3 Roger M. McCoy (2006) Ending in Ice – The Revolutionary Idea and Tragic Expedition of Alfred Wegener. Oxford University Press, 194 pp, ISBN 0-19-518857-8, US$29.95