TOP-DOWN TECTONICS

THE ROLE OF OCEANUS AND GAIA

BY JON THOREAU SCOTT

DON RITTNER, EDITOR

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Front cover:

Pangaea map courtesy Massimo Pietrobon. Background image view of the Alps from Space by Samantha Cristoforetti, courtesy of NASA. Back cover courtesy of NASA.

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Dedication

This book is dedicated to Reid A. Bryson and Robert A. Ragotzkie who convinced me that scientists should look at a problem from many angles and to consider research ideas that may seem far from the normal views on a problem. They are my former mentors when I was a student at the University of Wisconsin, Madison.

"Scientists still do not appear to understand sufficiently that all earth sciences must contribute evidence towards unveiling the state of our planet in earlier times, and that the truth of the matter can only be reached by combining all this evidence."
--Alfred Wegener, 1929, *The Origin of Continents and Oceans.*

"There is a growing recognition of the inadequacy of the separated disciplinary approach for the solution of planetary-scale problems. To understand even the atmosphere, which is the simplest of the planetary compartments, knowledge of geophysics is not enough; chemistry and biology are also needed. It might seem that research teams that included experts in each different discipline would resolve the problem, but anyone who has attended gatherings of experts knows that each expert speaks but does not or cannot listen. What might help would be a broader-based general science that provided an environment within which the separate disciplines could interact."
--James E. Lovelock, *Geophysiology-The Science of Gaia (*In *Scientists on Gaia,* 1993).

"The questions of origin, composition and evolution of the Earth require input from astronomy, cosmochemistry, meteoritics, planetology, geology, petrology, minerology, crystallography, fluid dynamics, materials, science and seismology at a minimum. To a student of the Earth, these are artificial divisions, however necessary they are to make progress on a given front. New ways of looking at things, new sciences, keep things lively. Advances in materials science, statistics, chaos theory, far-from-equilibrium thermodynamics, geochemistry and tomography make this an appropriate time to update our theory of the Earth."
--Don L. Anderson, 2007 *New Theory of the Earth.*

To Kathy and Beth in consideration of their patience.

May they both live long enough to see that there may be
a little bit of wisdom in the contents of this book.

Foreword

This book has two major themes. The first is the re-introduction of a mechanism that might explain how plate tectonics works. I call it the Expansion-Contraction (EC) mechanism of plate motion and I have been thinking about this idea for some sixty years since working on ice-covered lakes in Wisconsin. The mechanism has some very good features in that it explains such things as how ocean ridges between continental plates move away from the continents to which they were originally attached. I call this "ridge migration", but other authors describe it as a "decoupling" of the ridges from the convection currents that are thought of by many as the driving force that moves the plates. Ridge migration is required in the EC mechanism and thus no assumptions are needed. The EC mechanism also can possibly explain the variation in spreading rate at mid-ocean ridges, ridge height, triple points of ridges, compression within plates and such things that are not easily explained by arguments of other mechanisms of plate motion. As can be gathered from the title of this book I also attempt to show that the living world, or James Lovelock's Gaia, can be an important cause of plate tectonics if the EC mechanism actually works. I propose that it is the magnification of climate signals due to biologically driven positive feedbacks that result in the large changes in atmospheric temperature that eventually causes the variation of temperature at the top of the ocean ridges.

The second major theme of this book is that of top-down tectonics, which is in agreement with Don L. Anderson's ideas. He posits that it is the plates themselves that produce tectonics while the mantle convection is a result, not a cause, of plate motion. His statement is: "The tectonic plates can be viewed as an open, far-from-equilibrium, dissipative and self-organizing system that takes matter and energy from the mantle and converts it to mechanical forces (ridge push, slab pull) which drive the plates." The EC concept, while not the same as other top-down ideas, can be considered to be a top-down mechanism because the driving force is a result of seafloor spreading at ocean ridges.

Jon Thoreau Scott

Acknowledgements

The reasoning behind this book originated in the logic of the philosophy of science as much, or more, than in the scientific input of my proposal that the atmosphere, the oceans and ultimately life causes, or at least participates in the cause of the movement of the Earth's tectonic plates. Although many of my professors and peers have influenced my science it is the discussions with those who gave me the ideas that led to the, somewhat unusual, emphasis on how science works, that I would like to acknowledge here.

I begin with the influence of my father John G. Scott with whom I spent many hours in gardening, beekeeping, cutting wood, and the like, for our homes in Stelton, NJ and Taghkanic, NY in the 1940s and early 1950s. John was a natural scientist, an organic farmer and beekeeper for most of his life and often talked to me about the nature of science. The only "fertilizer" that he used in farming was limestone and he used no pesticides or herbicides. He grew up in Indiana and became interested in growing vegetables and honey for sale in local villages. Under his influence the family moved to the foothills of the Ozarks in Missouri, south of St. Louis, when he was twenty. Over five years he kept a journal on the phenology (timing of natural events) of the region and accumulated the largest bird egg collection in the state of Missouri which he later donated to the Museum of Natural History in St. Louis.

John G. Scott obtained a Bachelor of Sciences degree from the University of Missouri, Columbia, in three years with honors and was elected to the Phi Beta Kappa Society. A few years later he was dismissed from a teaching position in Pasadena High School in California for discussing the concept of evolution in one of his classes. After working on the Southern Pacific Railroad he became a union organizer, and joined the IWW (International Workers of the World) as a "wobbly." He then obtained a Masters of Science degree in Education and Economics at the University of Missouri and became an Associate Professor at Kansas State Teachers College in Pittsburgh, Kansas. He was fired from that job too, again for discussing evolution in his senior/graduate level course in civics. His class was planning a mock Scopes trial at the court house in downtown Pittsburgh in 1925 ten days after the real Scopes trial ended. In his journal he mentioned that he read Darwin's "Origin of Species" at least twice.

I remember very clearly when, in about 1950 or so, my father pointed out that the continents were at one time together as demonstrated by the jig-saw fit on either side of the Atlantic. I don't remember that he mentioned the name of Alfred Wegener, but he was no doubt familiar with the idea of continental drift. He was an expert on the stars of the night sky, the study of nature and ornithology. He taught me to recognize all of the bird songs and mentored me on the ecology of our woodlands and the names and benefits of the trees native to our region in Columbia County, NY. He and my mother Jo Ann Wheeler Scott were followers of Henry David Thoreau and considered themselves to be "Thoreauvian Anarchists." Needless to say I read "Walden" several times and several of Thoreau's many journals.

As a boy I attended the anarchist organized Ferrer Modern School, a school that emphasized the ideal of "Freedom in Education" where both of my parents were teachers. The school allowed children to plan their own education and there was no "curriculum" for the students to follow. This may help to explain the non-traditional approach of this book.

As stated in the Dedication I must also acknowledge my former mentors, Professors Reid A. Bryson and Robert A. Ragotzkie of the University of Wisconsin. Both Bryson and Ragotzkie were

involved in research that involved many scientific, and some non-scientific viewpoints, and their multidisciplinary approach was very much akin to my thinking. Bryson started out as a Geologist and then obtained his Doctorate in Meteorology at the University of Chicago. Ragotzkie was originally a Zoologist who obtained his PhD degree in a combination of Limnology and Meteorology at Wisconsin. Because these two scientists convinced me that a multidisciplinary approach is an important way to do science, I have opened this book with quotes from three persons who also believe that one must look at a problem from many ways of thinking.

In the fall of 1958 I was just starting a graduate program in the Department of Meteorology at the University of Wisconsin, Madison. My first adviser, Reid Bryson, started me off with a one credit reading course. The topic he chose was ice on lakes in cold climates, along with energy balance of ice on lakes, glaciers, and the like. It was the best course I have ever taken. He began the reading course on the properties of lake ice that he and William Bunge had published in three reports to the University of Wisconsin Lake Investigations Committee (1956) which represented three years of research. Later Bryson started me on a research program repeating some of Bunge's energy balance experiments that led, eventually, to my doctoral thesis. In 1961, Bryson turned me over to Robert A. Ragotzkie as a thesis advisor who led me through many of the problems dealing with my thesis on the energy balance of ice-covered lakes. Bob has been a lifelong friend.

One of the reports by Bunge and Bryson (1956 #2) was a discussion of the pressure ridges and ramparts that occur on the ice of fairly large lakes in cold climates and they showed aerial photos of these ridges on many lakes in the Madison, Wisconsin region. For several years, during my lake ice research, I was able to observe this ice expansion phenomenon that leads to the formation of these ridges, some of which can be at least five meters high on very large lakes like Lake Winnebago in Wisconsin. The understanding of these ice processes combined with the teaching of various oceanography courses involving the origin of oceans at the University at Albany led to the idea that is discussed in this book that climate variation can possibly lead to the movement of the Earth's tectonic plates by changing ocean bottom temperatures.

In addition to the two professors at the University of Wisconsin just mentioned, two others come to mind regarding thoughts on the philosophy of science. They are Aaron Ihde for whom I was a teaching assistant in his course "The Physical Universe" and Lyle Horn for whom I assisted in his course in the "Earth Sciences." Both of these courses were in a special program called "Integrated Liberal Studies" where the courses combined knowledge from several disciplines; philosophical ideas on science were foremost in the discussions.

When I assumed a position in the Department of Earth and Atmospheric Sciences (DEAS) at the University at Albany of the State University of New York in 1963, Eugene McLaren, then Dean of the Division of Science and Mathematics of the College of Arts and Sciences, asked me to teach a course on the history of science that was very similar to the courses in which I was involved at Wisconsin. We discussed how to present the material that covered science from the astronomy of the Greeks to modern views on the various sciences of physics, chemistry and biology as they existed in the 1960s. I learned much from these discussions with Gene. He and I still discuss many topics on the history of science.

A discussion I remember very well was with John M. Bird ("Jack" as we knew him), then a member of the DEAS. I had finished summarizing the ideas on continental drift in a lecture to my senior/graduate level class in General Oceanography in 1964 and had the arguments in favor and against the concept on the blackboard. Jack came in after class to chastise me that, as an atmospheric scientist, I shouldn't be teaching geology and "any way (to the effect) all that stuff about continental drift was a bunch of bull roar etc." I explained that I was discussing the origin of the current ocean basins that were being formed as the continents move and discussed with him the pros and cons of the continental drift hypothesis. I don't think I convinced him at the time, but several years later Jack and John Dewey published a seminal paper on the origin of mountains on either side of the Atlantic. Jack was a "continental" geologist and was not that familiar with the elegant research that the marine geologists were undertaking with respect to the ocean floors in the 1950s and 1960s. Most continental geologists rejected the continental drift hypothesis at the time. I met Jack in 1989 at the American Geophysical Union meeting in Washington and at dinner he told me about the aggravation from his peers that he went through when he converted to the idea that the Earth's plates moved.

I attended two of Vincent Schaefer's scientific field trips, one to Yellowstone Park in the winter and another to Glen Canyon in Arizona and Utah. These trips brought together scientists and some persons in governmental agencies and it was the discussion of various scientific phenomena and ideas that made them so fruitful. I particularly remember my discussions with Frits Went, who was a participant at the Yellowstone trip I attended (I think in about 1965). We discussed his experiments on the conditions of growing plants. I was very familiar with this work because I had given a presentation on Went's research in a graduate seminar course at the University of Wisconsin.

I also directed two of Vince Schaefer's summer programs for the Natural Science Institutes designed for senior high school and college undergraduate students. The method of the program was to allow students to tackle problems of their own design without a great amount of direction from an advisor. That was a very productive undertaking. Vince remembered working with Noble Laureate Irving Langmuir at the General Electric Research Center for twenty years. The General Electric (GE) Research Center's director "Doc" Whitney believed in the concept of freedom in science, where scientists at the lab were free to work on their own ideas. That was in agreement with the philosophy of my early education at the Ferrer Modern School. At GE, Vince discovered the seeding of clouds with dry ice in a serendipitous manner. See Vince's autobiography "Serendipity in Science: Twenty Years at Langmuir University." (2013).

Some of the most productive discussions on how science works came to me after Vince Schaefer and Bernard Vonnegut organized lunches for retired professors, or researchers, in Atmospheric Science, held every Monday, appropriately at Grandma's Restaurant. I participated in many of those discussions on aspects of science and research and these discussions later became known as "the Curmudgeon Lunches." Long-time participants were Duncan Blanchard, Eugene McLaren and Raymond Falconer. Vonnegut discovered cloud seeding with silver iodide much used today, when at the GE Research Center, and was an expert on atmospheric electricity. Blanchard wrote several profound papers on serendipity in science, a biography of "the Snowflake Man" Wilson Bentley and a popular book "From Raindrops to Volcanoes." He is a cloud physicist and studied air-sea interaction and especially the formation of things like condensation nuclei from bubbles at the sea surface. Eugene McLaren, as Dean of the Division of Sciences and Mathematics in the late 1950s and early 1960s, was responsible for organizing programs in Atmospheric Sciences at the University at Albany. Falconer was a very popular weather forecaster in the Albany region. Recently Chris Walcek has attended the lunches and has given me some very thoughtful comments on how the EC mechanism might, or might not, work. Others who attended the luncheons were Roger Cheng, Ulrich Czapski, Ken Demerjian, Phil Falconer, Dave Fitzgarrald, Carl Howard, Gar Lala, Volker Mohnen, Jim Schwab, Jim Schaefer, Ronald Stewart, and Geoffrey Williams.

I especially owe much gratitude to my friend and colleague Ulrich Czapski with whom I have discussed the rationality of many scientific ideas over dinners after several glasses of wine.

Still coming regularly to the Monday lunches in 2014 are Douglas and Eugene McLaren with whom I discuss aspects of science, politics, sports and the like. In particular, Gene has helped me greatly in my writing of a paper and this book on the proposed mechanism of plate tectonics. He coerced me into clarifying the heat exchange mechanism at the top of the ocean ridges and how the heat from below participates in this process. The diagram (Figure11) with respect to this process can be accredited to his tenacity. I propose that this heat exchange produces the expansion and contraction of the top of the ocean crust. Gene also read several versions of this manuscript and came up with some crucial suggestions on how to improve it and I thank him for his efforts. McLaren was Chair of the Division of Science and Mathematics in 1963 when he hired me to join Department of Earth and Atmospheric Science of the State University of New York at Albany (now called the "University at Albany – State University of New York"). He asked me to teach a general education course that covered the history of science and we have had many discussions on the philosophy and methods of science. Some of the arguments from those discussions appear in this book.

One thing that came out of the "Curmudgeon Lunches" was an informal group on the subject of how science works. One day in 1986, or so, Bernie Vonnegut stopped me in the hall outside his office in an excited state. He had just finished reading Thomas Kuhn's book *The Structure of Scientific Revolutions* and said that it changed his life. He was in his late seventies at the time! He had been working for years on the idea that lightning was formed by a convective mechanism rather than by the conventional wisdom which was that charge separation in clouds produced lightning bolts. He was fascinated in the idea of Kuhn that anomalies were needed to change the course of an idea in science. He gave me a copy of Kuhn's book and I discussed it with him a few days later. We agreed to hold an informal seminar on "How Science Works" and several faculty and graduate students attended our meetings for several years. Bernie gave us his list of anomalies to the current lightning theory, and ultimately there were many. He finally had his paper published in the *Bulletin of the American Meteorological Society* on the subject after several attempts. In "How Science Works" we discussed the scientific method, scientific revolutions and anomalies to paradigms, serendipity in science, Project Cirrus (a GE research study), raindrops, the Gaia hypothesis, global warming, the "Big Bang theory," continental drift, the plate tectonics revolution and many other topics. Participants were Duncan Blanchard, David Fitzgarrald, Robert Keesee and a few guest discussants from various departments at the University at Albany. Some of the students who participated in these discussions were Anantha Aiyer, Eyad Atollah, Jeffrey Freedman, Gregory Hakim, Olga Sharoichenko (now Hogrefe), Anton Seimon, and David Schultz, but I may have forgotten a few.

Recently I have corresponded with Michael Fischer who also questions the concept of convection currents as the driving force of plate tectonics. He introduced me to ideas of Imre Lakatos who modified the concept of the paradigm of Thomas Kuhn to the idea that he terms "research programmes." Michael also corrected me on my ideas of the formation of the Helderberg Escarpment (see the Preface below) pointing out that most of the uplifting of such mountains as the Helderbergs, Catskills and Adirondacks has been relatively recent, not much earlier than about 100 million years ago. I was of the impression that the uplifting had occurred before that time, but was wrong. I have incorporated some of Michael's ideas into to the text of this book and I look forward to more discussions with him.

I thank those students who met every other Saturday from 1987 to 1989 to discuss the possibility that the Expansion-Contraction mechanism could actually produce, or participate in the motion of the Earth's plates. They are Gregory Hakim, Mary C. Memrick Hawkes, Tony Mainolfi, Nan Elliott Rosenbach and Andrea Rutherford. I will refer to their work later on.

I would like to acknowledge my daughter, Elizabeth Scott who helped me greatly on many aspects of the web site mentioned above and on some of the Figures. I also am indebted to my nephew in law Bjőrn Grőnnesby of Trondheim, Norway, an expert on computers, who took the website I had produced and put it into a format that could be edited. I would be lost to try to do that! I thank Carol Coogan who turned some of my fuzzy figures into diagrams that are much clearer to read. Last, but not least, I am grateful to Don Rittner for accepting my offer to edit this work, for clearing up much of my confusion, for help in structuring the book and for getting it printed.

Special thanks to Massimo Pietrobon for permission to reproduce his Pangaea illustration on the cover and to NASA for the tectonic plate illustration on the back cover.

Preface

"Our life is frittered away by detail...Simplify, simplify."
--From *Walden* by Henry David Thoreau

As I sit on the deck of my passive solar house in Altamont, New York, I look directly south to a range of hills culminating to their east by the Helderberg Escarpment. The escarpment is seen from our house as a series of three visually pleasant bluffs that fall sharply from the Helderberg Highlands to the low lying Hudson River and Mohawk Valleys to the east and north near Albany New York. Perhaps the Helderbergs are not strictly a mountain range, but they do rise some 600 meters (about 1950 feet) above the lowlands of the Hudson River Valley and extend far westward into New York State. The Helderberg Escarpment is well-known as a viewing spot that looks toward the east to Albany and the Taconic mountain range that lie in Eastern New York and Western Massachusetts. Thousands of people visit the John Boyd Thatcher Park each year to gaze on these spectacular views.

For the past forty years, or so, I have envisioned this escarpment to be a remnant geological feature, now severely eroded, that was formed when North America was moving to the east and collided with the European and African continents about 300 million years ago (Mya) to form the supercontinent Pangaea. This turned out to be incorrect as pointed out to me by Michael Fisher (personal communication, 2014) who read an earlier version of this manuscript. So I read the New York State Museum documents *Guide to the Geology of John Boyd Thatcher Park* (1997) originally written in 1933 by Winifred Golding with an update by Ed Landing and John Skiba and *The Geology of New York* by Yngvar W. Isachsen *et al* (2000).

From these works I find that the Helderbergs were formed in a large sedimentary basin during the Devonian Period about 400 Mya (million years ago). There were two orogenies (mountain buildings) that led to the deposition of the sediments that form the Helderbergs. The first, known as the Taconian orogeny, was due to the collision of Eastern North America with a volcanic island arc that led to the very high Taconic Mountains of Eastern New York and Western New England. This took place about 450 Mya. Bird and Dewey (1971) suggest that the Taconics and the ridge-valley system of the region from Northern New England to the Carolinas were formed from this collision. The erosion of these mountains filled a sedimentary basin of Eastern North America during a time when North America was near the Equator.

The second mountain building episode, called the Acadian orogeny, took place around 380 Mya when Eastern North America collided with a small continent known as Avalon. Mountains from this event also eroded to fill the basin in Eastern North America that was still near the Equator at that time. These sediments were probably formed in coral reefs and this produced the several layers of resistant limestone that form the Helderberg bluffs with the Coeymans and Onondaga Limestones being two of them. These form the eastern bluffs of the Helderberg Escarpment. A third major event was the formation of Pangaea when North America and Africa collided. Isachsen *et al* state that the lifting of the Helderberg region occurred when a new Atlantic Ocean was being formed and North America and Africa were moving apart and they place the date of this in the Mesozoic very approximately at 200 Mya. The lifting mechanism is unknown, but I suspect that it is related to plate tectonics; the way it happened may not be known even to geologists.

After the lifting of the sedimentary basin the Escarpment was formed by erosion of the resulting peneplain by the Hudson and Mohawk Rivers and their tributaries. The bluffs that are seen from my house are formed of the limestones that were more resistant to erosion than the sedimentary layers below. The top of the Helderbergs contains a fairly deep layer of the Marcellus formation that is the subject of the "fracking" program to release natural gas in Northern Pennsylvania and proposed for the Alleghany region of Southern New York. What is interesting in the Helderberg story to the theme of this book is that plates have moved thousands of kilometers so that land that was formed under equatorial oceans is now seen in the eroded hills of the mid-latitudes of North America.

This book is not meant to describe the role of plate tectonics in the formation of mountains and other geological features (except as in the above and in a general sense), but it is directed toward those interested laypersons who may find it fruitful to ferret out the plate tectonics mechanism through ideas from the philosophy of science and how science works. My emphasis is that a viewpoint from a variety of approaches might solve the problem whereas the use of science relying on a particular discipline may fail. In particular, this book is directed to those interested laypersons who may find it fruitful to view the plate tectonics mechanism through ideas that are relatively easy to understand. I also direct this book to some professional scientists including those Earth Scientists who have followed Lovelock's ideas (see Lovelock, *The Ages of Gaia*, 1988). The serious reader may find some substantive ideas proposed herein by reading the Appendices and some of the literature. Especially, I suggest reading the ideas of Don Anderson on top-down tectonics as a self organized system discussed in his newest book *New Theory of the Earth* (2007) and in some of his papers that I quote in the Literature.

There is a long history behind this book. As mentioned in the Acknowledgements I started thinking about an alternative to the existing mechanisms of plate tectonics when at the University of Wisconsin, Madison in the early 1960s. There I spent three years doing research on ice-covered lakes. The ice research, coupled with ideas that I had been reading about Alfred Wegener's concept of continental drift, proposed five decades earlier, led me to thinking about forces that cause mountains on the Earth. Wegener's ideas were discussed by Reid Bryson in a graduate course in Paleoclimatology and that caused me to read Wegener's 1929 book on the subject. I was convinced by the paleoclimatic, fossil, geological and other evidence that Wegener presented that the continents must have moved and so was Bryson. Much later I read the book by Vladimir Kőppen and Wegener, "The Climates of the Past" (1963), and that convinced me of the logic of the climate evidence for the continental drift hypothesis.

After I left Madison I took a position in the Department of Earth and Atmospheric Science at what is now called the University at Albany – State University of New York. In 1964, I taught a course called "General Oceanography" for students who were seniors or graduate students in the Earth Sciences. Many were teachers in local schools who needed graduate Earth Science credits. I used several *Scientific American* articles on the subject of continental drift, particularly one by J. Tuzo Wilson (1963) entitled "Continental Drift" and on the concept of seafloor spreading to point out how the ocean basins are formed and are changing in size and shape.

It seemed to me that the arguments for mantle convection that had been proposed by Arthur Holmes in 1933 and in his 1944 book, and others earlier, were not sufficient to produce the forces needed to cause continental collisions and uplift to form mountains like the Alps. The convection arguments were ignored by most geologists as a mechanism of continental movement until the concept of seafloor spreading proved the idea that the continents do, in fact, move. But despite its long rejection by most geologists the concept of deep mantle convection became the leading idea on the forces that cause plate tectonics. My reasoning to doubt this was that compression forces such as those that produced ice-ramparts and pressure ridges on ice-covered lakes were needed to explain seafloor spreading and continental movement. My General Oceanography course, mentioned above, required a term research report and I suggested that one of the topics could be to find out if any geologist had proposed a mechanism of seafloor spreading similar to the way ice ramparts and pressure ridges were formed on ice-covered lakes, that is, by expansion and contraction of the Earth's crust at the bottom of the oceans. Understandably, there were no takers on the topic.

After many years of teaching a General Education (first year level) course called "The Oceans" I lectured on the possibility of what I called the "Expansion-Contraction Mechanism of Plate Tectonics" and how it could cause the movement of the plates in the newer idea that became known as the concept of plate tectonics. In the mid 1980s, after reading all of Lovelock's books and many papers on the Gaia hypothesis, I changed the name of the course to "Oceanus and Gaia" and offered the Expansion-Contraction topic as a term report each year, again with no takers. In 1987, I then offered students the opportunity to volunteer to attend a Saturday morning seminar on the topic for which they were to do library research in exchange for not having to take the three exams that were part of the course. About twenty students attended along with some former students who were Atmospheric or Earth Science majors. We made no progress in determining that geological research had been done on the Expansion-Contraction idea.

The Saturday group of students was whittled down to those who were quite interested in the topic and we continued to meet every other Saturday for two years. We wrote a paper entitled "Crustal Expansion and Contraction in Response to Climatic Change as a Mechanism for Driving the Earth's Tectonic Plates." Hereafter this mechanism is referred to as "EC." The students of the final discussion group were Gregory J. Hakim, Mary C. Memrick Hawkes, Anthony Mainolfi, Nan Elliott Rosenbach and Andrea Rutherford and I also dedicate this book to their efforts. In that study we suggest that the conventional wisdom of mantle convection is suspect due to anomalies related to how the ridges associated with continental plates migrate (*i.e.* away from Africa, the Americas and Antarctica) and because it is difficult to explain many of the observations of the ocean bottom crust in the convection model. We sent an abstract to the 1989 meeting of the American Geophysical Union and it was accepted only as a poster session. Also in 1989, we attempted to publish the 1989 paper in the journal "*Global and Planetary Change*" but it was rejected as discussed later on.

More recently I have become familiar with a large number of papers by Donald L. Anderson and his recent book (2007). He and several of his colleagues pose that it is the plates themselves that drive plate tectonics and cause forced convection in the mantle rather than the other way around. He states that plate tectonics is "a far-from-equilibrium, dissipative and self-organizing system that takes matter and energy from the mantle and converts it to mechanical forces that drives the plates." The idea that it is the plates that drive tectonics is quite similar to what our group came up with in 1989 in that our mechanism also uses the concept of top-down tectonics (see Anderson, 2001) although we did not come up with that term. Our forces are different from those suggested by Anderson, because we base our conclusion on what we term the EC mechanism of plate tectonics, that in addition to gravity forces, those driven by temperature changes at the bottom of the ocean can produce plate movement.

As mentioned earlier the emphasis in this book is on the logic of the philosophy of science. To keep the discussion on the simplest concepts it is based on the characteristics of a good theory (therefore hypothesis) and I discuss my ideas on how science works in Chapter 3. If the characteristics of a good theory are followed, and the discussion is kept simple, complex arguments using many assumptions and complicated scientific terminology on how plate tectonics works can often be ignored. Some ideas that may be rather easy to understand and have fewer complicated assumptions than much of the science devoted to the concept of plate tectonics may root out the cause of plate motion.

Introduction

In Part I of his 2007 book "New Theory of the Earth" Don Anderson states:

"We now know that plate tectonics is unique to the Earth, perhaps because of its size or water content… Any theory of plate tectonics must explain why the other terrestrial planets do not behave like the Earth."

In the quote above Anderson was, of course, referring to the terrestrial planets of our solar system (Mercury, Venus, Earth and Mars). Recently I have read in the popular literature that there may be plate tectonics on ice-covered Europa, a moon of Jupiter. Because Europa probably has water beneath the ice cover can this be related to the EC mechanism described in this book?

In this book I attempt to explain Anderson's observation. I suggest that one must look at ideas that come from a variety of disciplines as suggested at the outset above by Alfred Wegener, James Lovelock and Donald Anderson. By looking across ideas that come from many of the Earth Sciences, Biology and other sciences it seems to me that *Gaia* and *Oceanus* may play a role in the cause of plate motion. I suggest that Geophysiology, Lovelock's idea on the workings of the Goddess *Gaia*, is a primary cause of variation of the climate over long periods of time and that through her son *Oceanus* she changes the ocean water temperature to produce seafloor spreading at the mid-ocean ridges by the expansion and contraction of the ocean crust (the EC Mechanism). The mechanism is explained more completely in Chapter 1.

Wegener's approach to the concept of continental drift was that there was a large body of information that showed many anomalies to the assumption that forces due to the shrinking of the Earth and continental fixity (continents were always where they are now) were suspect. He did very little research of his own on these topics, but used observations from many sciences such as the "jig-saw" fit of continents on either side of the Atlantic Ocean, climates of the past, glacial evidence, fossil evidence, similarities of mountain ranges and geological features on either side of the Atlantic Ocean, and the like, to hypothesize that the continents were together in a "super-continent" called Pangaea that broke apart and the continents then moved to their present positions after about 200 million years. Together with his father in law, Bioclimatologist Vladimir Kőppen, he co-authored a book entitled "Climates of the Geological Past" that showed that climatic evidence ruled out the idea that the continents never move. The idea of continental drift was verified by the discovery of seafloor spreading by marine geologists in the 1960s. This led to the current model of plate tectonics in the late 1960s including the current conventional wisdom of mantle convection as the driving force of plate motion.

James Lovelock has written several thought provoking books and many papers on how life is the most important regulator of the environment of planet Earth. He posits that once life gets established on a planet it has mechanisms that regulate such things as atmospheric chemistry, climate and other geophysical properties. He even proposes that life can produce the forces of plate tectonics. The emphasis in this book is that Lovelock's Gaia, or Geophysiology, plays a most important role in why the Earth's tectonic plates move and I will come back to that idea later on.

Chapter 1
The Expansion-Contraction (EC) Mechanism

"It is well-known that a level cannot be used on the ice. At one rod from the shore its greatest fluctuation, when observed by means of a level on land directed toward a graduated staff on the ice, was three quarters of an inch, though the ice appeared firmly attached to the shore… Who knows but if our instruments were delicate enough we might detect an undulation in the crust of the earth."
-- From "Walden" by Henry David Thoreau.

The quote from "Walden" is from Chapter XVI "The Pond in Winter" and it is my opinion that the deviation of the level of ice was caused by the well-known expansion and contraction of the ice due to temperature changes. On my small pond (one-third of an acre) at times I measured deviations of at least an inch when there was little snow on the ice and when it was about a foot thick. These height differences persisted through most of the winter.

This book and the website (tectonicsdrivenbyclimvariation.com) describe a mechanism that might produce, or might combine with other proposed mechanisms, to produce the driving forces of plate tectonics. I decided to bring back the EC hypothesis after 24 years, in this form, after some discussion with Greg Hakim, now at the University of Washington, because there have been no other similar proposals related to the concept of the EC mechanism. Also, after a recent search of the literature there does not seem to be a consensus on what forces are required that move the Earth's tectonic plates. As mentioned above, the current conventional wisdom found in most textbooks is that large-scale convection in the mantle drives the Earth's plates, but as Anderson states in the quote at the beginning of this book, we may need to take a new view of how the Earth works.

According to the EC mechanism the expansion and contraction at the mid-ocean ridges are caused by the ocean temperature changes resulting from the variations in the Earth's climate. These variations cause changes in the temperature or mixing rate of the ocean water that flows over the mid-ocean ridges. Heating or cooling of the upper crust caused by a warming or cooling of the ocean water that moves over the ridges produces thermal expansion and contraction, and is the proposed cause of sea-floor spreading at ocean ridges. The heat source comes both from downward heat flow in the ocean and upward heat flow in the crust. The ocean water above the crust does not have to be warmer than the top of the crust it merely needs to change in temperature or in the mixing rate from turbulence in the ocean water above the crust.

Many of the observations of the ocean bottom features are explained, without frills and extra assumptions, by the EC mechanism and these are, in some ways, more natural than explanations using some of the conventional hypotheses on the driving forces of plate tectonics. I call this a parsimonious explanation of observed phenomena, the well-known concept of "Occam's razor" in which hypotheses with extra assumptions are excluded and those with the simplest explanations are assumed to be best and are, at least temporarily, accepted.

Ridges and Ramparts on Ice-Covered Lakes

In this chapter I describe the EC mechanism as it works on lakes and how it might work at the mid-ocean ridges on Earth to cause sea-floor spreading. Figure 1 illustrates the mechanism that causes the area of lake ice to grow on lakes in cold climates due to the expansion and contraction caused by day-night changes in the air temperature above the ice.

In Figure 1(a) the situation for a cold night is depicted. Note that temperature increases to the right in the diagram. The lines D_t. and D_b in Figures 1(a) and 1(b) represent the relative lengths of ice perpendicular to a given crack. Note that the length at the top (D_t) during a cold night is less than that at the bottom (D_b) where the temperature is near the freezing point of water. Thus, when the air temperature is much below the freezing point of water, as shown in 1(a), the top of the ice contracts to form a crack into which water up-wells (D_t is less than D_b). This water freezes, forming new ice. When the air temperature or solar radiation warms the ice the column of ice reaches zero degrees C and so the top of the ice must now expand as shown in Figure 1(b) and $D_t = D_b$. This causes the ice area to increase in directions perpendicular to the crack, because of the new ice. This increase in area produces "ice ramparts" that push sheets of ice onto the lakeshore and also causes it to buckle upwards across the lake in the form of "pressure ridges." Because ice cracks are in random directions it is actually the area of ice that increases. More information on the mechanism is found in Hobbs (1911), Scott (1926) and Zumberge and Wilson (1953).

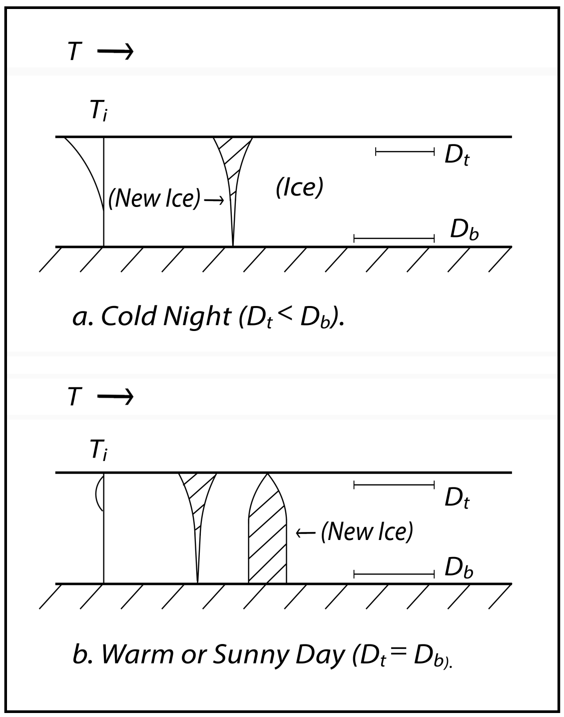

Figure 1: Expansion, contraction and growth of lake ice area.

Shown in Figure 2 is an aerial photo of a pressure ridge on Lake George, NY. This ridge occurs in the same location on Lake George every winter. I suggest that the mid-ocean ridges are much like the pressure ridges on ice-covered lakes in cold climates and that the ocean ridges are elevated by compression forces much like the pressure ridges on lakes. A map of pressure ridges is shown in Figure 3 made from an aerial photograph by William Bunge in 1955. The ridges often cross the lake from Picnic Point, the narrow peninsula near the west shore to Governor's Island on the north shore or Maple Bluff on the east. In Figure 3 the main ridge goes from Second Point to Governor's Island and shows a "triple point" to the northeast of Second Point. Note that this is much like the triple points that take place on many ridges on Earth. A triple point frequently forms in the middle of Lake Mendota (on which the University of Wisconsin, Madison is located). The ridge often starts out at Picnic Point and divides at mid-lake with one branch going to Governor's Island to the Northeast and another toward Maple Bluff to the east.

Figure 2: A pressure ridge on Lake George, NY.

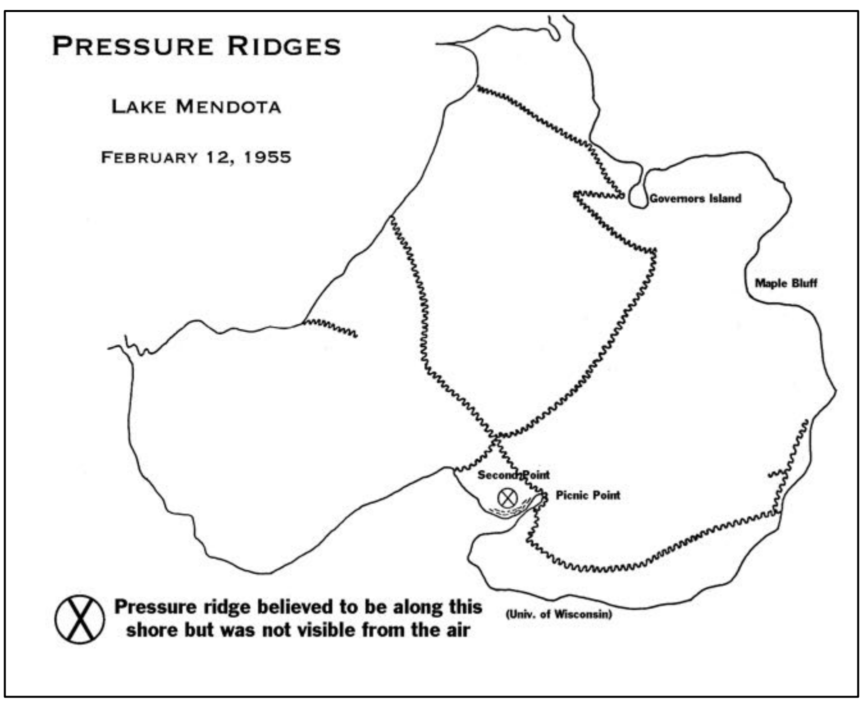

Figure 3: Location of pressure ridges on Lake Mendota, WI in 1955.

The EC Mechanism As Applied To The Earth's Plates

The suggestion of how the EC mechanism might work on the planet Earth is shown schematically in five diagrams (Figures 4 through 8). In the first one (Figure 4) the process is started at a cold time of a climatic variation (often periodic). Note that the temperature at the top of the crust is colder than the mean temperature of the crust. The temperature gradient in the crust, (straight line designated "T_{mean}") averages about 25^{o}C per km near ridges. Also note that the lithosphere grows thicker quite rapidly as it moves away from the ridge, by the square root of time (essentially the same as distance from the ridge center, because the spreading is nearly constant in a given region). This increase in the lithosphere thickness has been shown in many sources in the literature and is not to scale in Figures 4, 5, 7 and 8. This change in thickness may prevent cracks from forming at short distances away from ridges. The crust itself is from 5 to 8 km thick near the ridges. In the diagram I have shown that there has been no change in seafloor spreading, because I have started, arbitrarily, at a time when a given period of climate variation is at its start (time t = 0).

In the second schematic the warmest time of a climate period is shown in Figure 5 (half a period of climate variation later than the cold time of Figure 4). Heat from below and from the increase in ocean water temperature is stored near the top of the crust causing it to be warmer than average and thus to expand. The oceanic crust on either side of the ridge expands in all directions and is forced to move away from the ridge due to compression; this causes seafloor spreading. If the crust is between continental plates the continents are forced to move as the ocean crust on either side of a ridge increases.

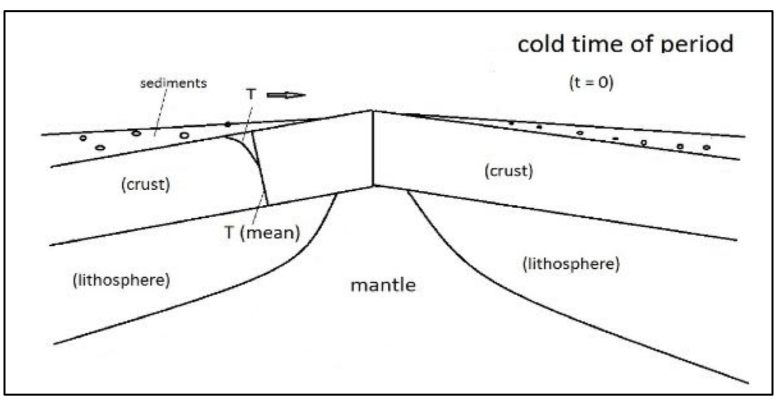

Figure 4: Schematic of the EC mechanism for the Earth starting at a time of cold ocean water (t=0).

In Figure 6, the solidus curves are shown for two minerals redrawn from Anderson (2007). At temperatures and pressures above the curves these minerals melt possibly to form the fertile material that rises into the ridge where it eventually solidifies to form new crust. I posit that it is the reduced pressure, from up-warping (due to compression) that causes the minerals to pass across their solidus to the liquid state where they then can rise to form the new material at the ridge.

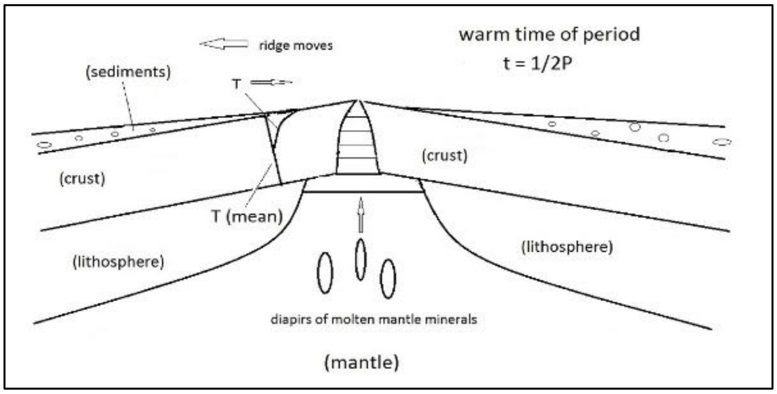

Figure 5: Schematic of the EC mechanism for a time at one half of a period from t=0 (a time of warm ocean bottom temperature).

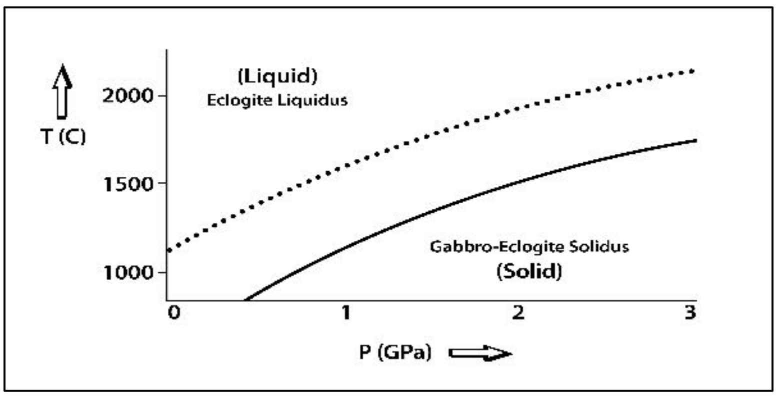

Figure 6: Solidus plots for Gabbro and Eclogite (redrawn from Anderson, 2007).

In the third schematic a time for 3/4 of a climatic period later than t=0 is shown in Figure 7 and the temperature profile (T) is nearly the same as the "T_{mean}." It is proposed that magma that has flowed into the gap at the ridge center solidifies over time to form new crust at the spreading center (hatched area).

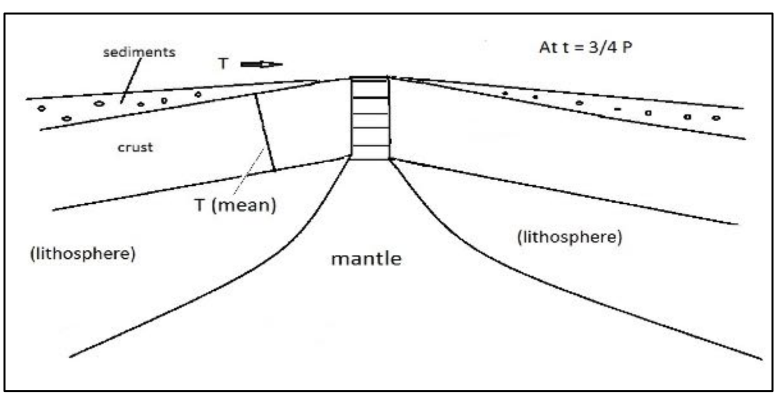

Figure 7: The EC mechanism for a time of ¾ of a period after t=0.

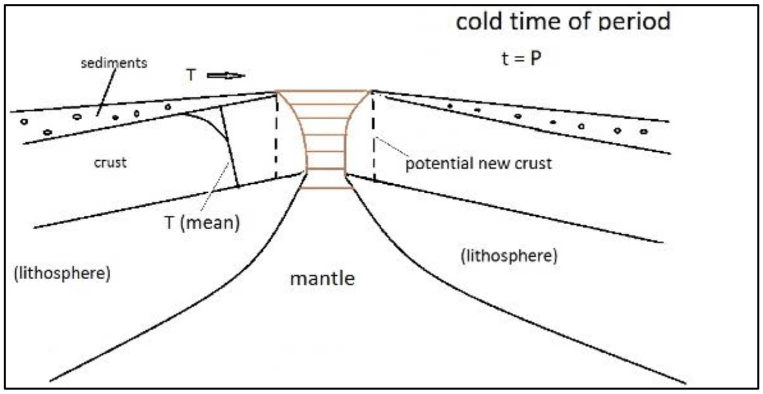

Figure 8: The EC mechanism after one full period after t=0 when the ocean bottom is again cold.

After one full period of a climatic variation (or periodicity) the ocean at the top of the crust has cooled back to about the same as one period earlier as shown in Figure 8. If we designate the potential new crust as E for one period of climatic variation then the ocean crust is forced to move away from the spreading center on each side of the ridge by the amount ½ E. The total spreading rate for one full period of climatic oscillation is E/P, where P is the period of climatic variability.

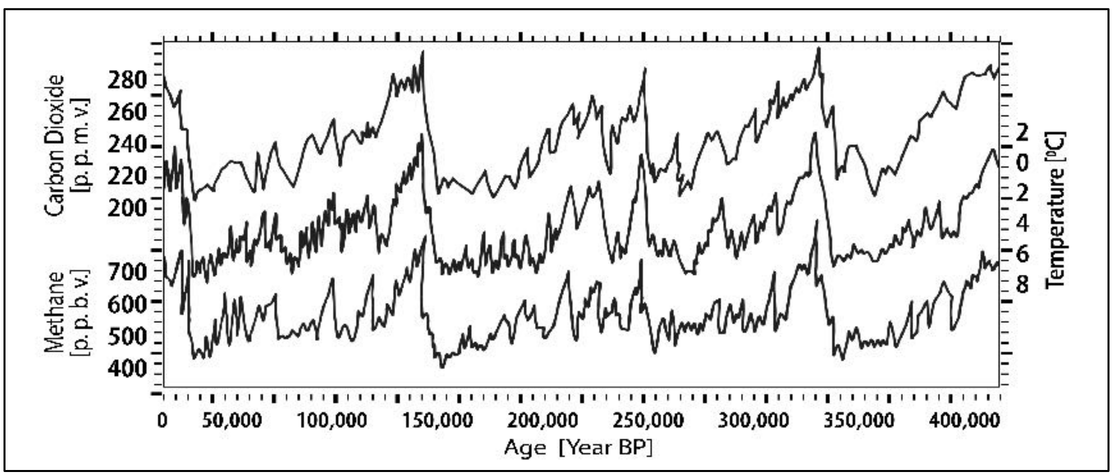

Figure 9: Plots of temperature, concentrations of carbon dioxide and methane. (Redrawn from Wikipedia: Argu.Vostok420Ky.)

As I suggested in the discussion for Figure 5, a time of warming, compression in the oceanic crusts causes the ridge to move away from the continent to which it was once attached (Africa in the case of the Mid-Atlantic Ridge) or to the left in the diagram and towards North America. The distance between Africa and North America will increase by about twice the distance that the ridge moves, or by the amount E. This does not require that Africa remains stationary. However, because Africa is also being pushed from the east by the spreading on that side of the continent it may not move much in the east-west direction with respect to the mantle.

In Figure 9, I show one of the well-known plots of temperature, carbon dioxide and methane redrawn, schematically, from the Vostok ice core data as posted under Wikipedia on the Internet (search "Milankovitch Periodicities" and look under Wikipedia on the Internet). Other variations are in the range of 1 to 10 Ky (thousands of years) and there are, no doubt, many of shorter duration). See Bradley (1999) or the discussion in Wikipedia on the Internet for more information on this diagram and also for how the carbon dioxide and methane data are obtained. The temperatures in the middle plot are relative to the present day temperatures.

The purpose of Figure 9 is only to show the large variations in temperature and its correlation with plots of greenhouse gases. In addition to the large Milankovitch variations, mentioned above, there are many smaller scale variations of climatic change. Some of these as small as a few hundred years might participate in the expansion and contraction of the ocean crust at the ocean ridges and thus in the formation of new crust by the EC mechanism.

Gaia's Role In Climate Variation

If you note the quotations at the beginning of this book they suggest that I have, in the past 50 years, had two main heroes on ideas on how the Earth works, not counting Thoreau. They are Alfred Wegener who proposed the idea of continental drift and James Lovelock who initiated the concept of the Gaia hypothesis. Lovelock proposed that the Earth's environment is governed in a complicated process by life that he calls Geophysiology. In the very recent past I have added a third hero, Don Anderson, who through normal science, has come up with an idea that plate tectonics is a self-driven processes that involves mostly the plates themselves. Anderson's is a true accomplishment given the forces of ideas that dominate the current paradigm of plate motion in his discipline. Thomas Kuhn (referred to later) suggests that it is those who do the normal science who create the anomalies to the existing paradigm and it is these anomalies that eventually lead to what he terms a scientific revolution.

Lovelock, in several books and many papers, suggests that Gaia requires plate tectonics for life to survive and to help regulate the Earth's environment by recycling carbon dioxide from the crust to the atmosphere. If climatic variability causes plate motion, as I suggest in this book, then it is the biota, through positive feedback mechanisms, that may cause most of the climatic variability. Figure 9 illustrates that rapid changes in temperature correspond to rapid changes in greenhouse gases, a positive feedback mechanism. The sensitivity of climate to changes in the greenhouse gases is discussed in the very readable book by William Ruddiman (2005) who suggests that humans began changing climate at the beginning of extensive agriculture, particularly with the growing of rice. To be sure there are other positive feedbacks that are not caused by change in the biota, but it seems that the biological feedbacks are very strong, if not the strongest. Because the EC mechanism requires variations in the climate to produce plate motion can this be a way that Gaia can cause plate motion? It would be a stretch to convince the present scientific establishment that this can happen. Most scientists follow the conventional wisdom in their field and that is probably true of any other of the activities of our species. If I am correct that life governs plate tectonics, time will tell.

An Example Using Flow Over The Mid-Atlantic Ridge

Figure 10 (see Roemmich and Wunch, 1985) provides an example of ocean bottom topography and ocean temperature to help illustrate how the EC mechanism might work in the case of the North Atlantic Mid-Ocean Ridge between North America on the North American plate, and Northern Africa on the African plate. The currents of the North Atlantic Deep Water (NADW) formed east of Greenland flow, at least partly, over this ridge.

The NADW temperature can change with changing conditions of the climate and its speed (or transport rate) can change with the salinity of the water at its formation. Such changes can alter the heat stored in the top of the newly formed crust at the ridge as hypothesized in Figures 4, 5, 7 and 8.

The cross-section in Figure 10 includes plots of temperature above the Mid-Atlantic ridge at 36 ^{o}N; it is redrawn, schematically, from a cruise in 1982 by the Woods Hole Oceanographic Institute. The slope of the ridge is exaggerated in the vertical by about 1000 times so that the rise from the abyssal plains on either side of the ridge is only about the height of a person per football field so that one can easily ride a bicycle up such a slope. Some part of the NADW flows over this ridge. Because the NADW changes in temperature, or flow rate, it can influence the heat storage in the surface of the crust near the ridge. The temperature gradient (change of temperature with depth) just above the ridge is about 1.0 to 1.5 ^{o}C/Km and it is directed downward (or negative).

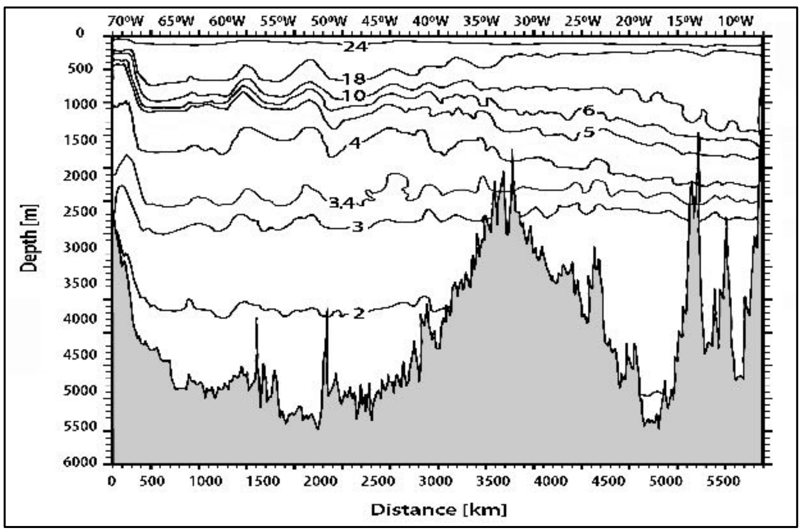

Figure 10: A cross-section of topography and temperature at 36 ^{o}N in the North Atlantic (redrawn schematically, from data taken by the Woods Hole Oceanographic Institute. See Roemmich, D., and Wunch, C., 1985).

Heat Exchange At The Mid-Ocean Ridges

I remind my readers of the statement by Anderson (2007) that I quoted in the Introduction.

Any theory of plate tectonics must explain why the other terrestrial planets do not behave like the Earth."

The terrestrial planets are Mercury, Venus Earth and Mars. In this section I attempt to explain why this observation makes sense. In Figure 11, I provide a schematic diagram of the temperature changes that might take place near the top of a mid-ocean ridge. At a point at the top of the crust the temperature is changed by three sources of heat. They are horizontal advection in the ocean (A), upward conduction in the crust (C) and downward convection (or eddy conduction) in the ocean (D). The dotted line is for warmer than average ocean bottom conditions, the dashed line for colder than average and the solid line for average conditions.

When warmer than average ocean water passes over the ridge (A) is positive and it is negative when colder than average water passes over the ridge. When the ocean cools at the surface, say during an ice age, (A) can be negative and during a warm period of climate it could be positive with warmer than average water flowing over the ridges.

The average temperature gradient in the crust is about 25 ^{o}C/km near the mid-ocean ridges and the term (C) is always positive (directed upwards toward the ocean). In the crust, temperature gradients of at least 50 ^{o}C/Km are possible in some ridges giving a possible range of 70 to 140 mW/m^2 (milliwatts per square meter) of upward heat conduction through the crust.

The average downward-directed temperature gradient in the ocean above the ridge is about 1 to 1.5 ^{o}C/km (see Figure 10). But because the ocean mixes, its eddy diffusivity is some 80 to 300 times its molecular value so the downward heat flow (D) in the ocean towards the crust can vary from -50 to -180 mW/m^2 (the negative sign indicates downward heat flux).

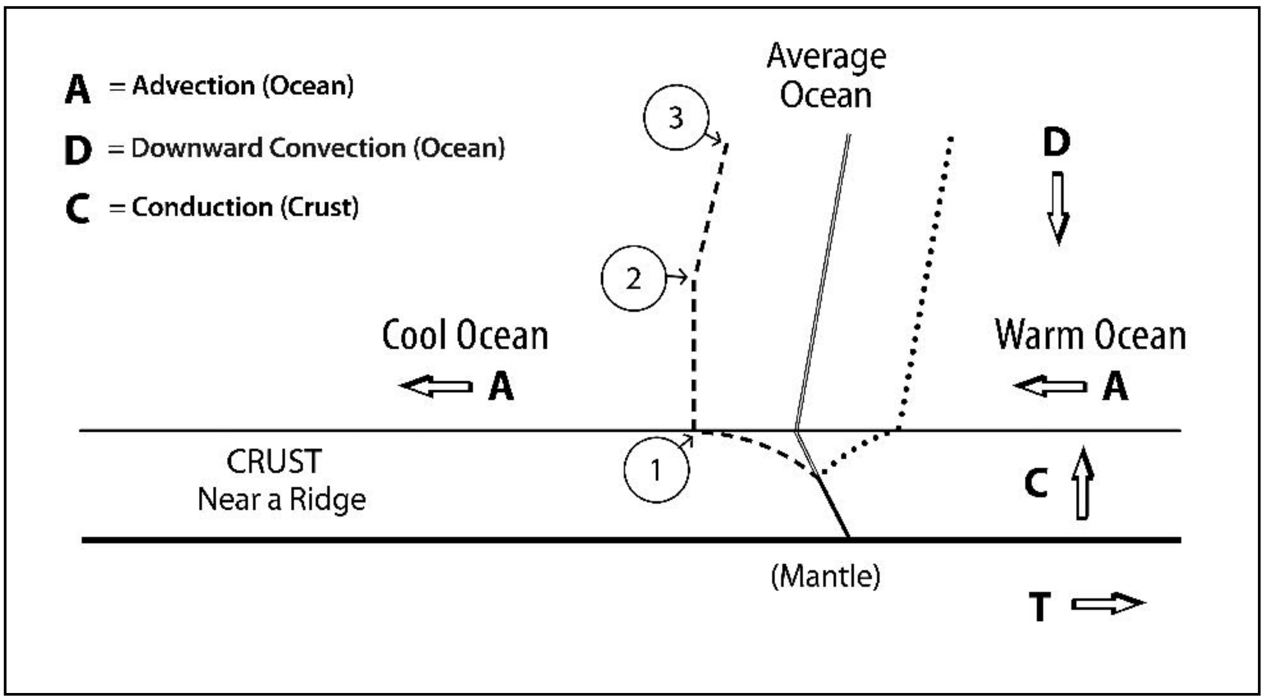

Figure 11: Schematic of how temperature might vary in and above the crust near an ocean ridge.

Because the average of 25 ^{o}C/km gives 72 mW/m^2 of conduction through the ocean crust the downward heat flux in the ocean can be as large, or larger, than the average upward molecular conduction through the crust.

For the "cool ocean" in Figure 11, I show an isothermal layer in the ocean water (from point (1) to point (2)). This is due to the addition of heat to the ocean from the crust. When water is heated from below it becomes unstable and so heat mixes upward to produce the isothermal layer. From point (2) to (3) and above the normal downward temperature gradient exists.

The diagram in Figure 11 illustrates that the change in heat from the ocean above or advection can cause the temperature of the top of the crust near a ridge to warm or cool producing thermal expansion during times of warmer than average crust and thermal contraction during times of colder than average crust. The water temperature does not have to be higher than the top of the crust; it merely needs to vary for the EC mechanism to work. See the Appendix I Figure 1 for more details on the temperature changes near the top of the crust at the ocean ridges.

In response to Don Anderson's statement in the quote above, I repeat that no theory of the movement of the plates has considered forces other than those produced by the thermal activity of the Earth's interior, that is from heat generated mostly by the radioactive decay of some elements in the Earth's core, mantle and crust. The EC proposal, as illustrated in Figures 10 and 11, requires heat flux changes due to the changes in the ocean temperature that are originally caused by variations in the Earth's climate. Note that the fluxes in the ocean water, due to mixing and advection, can be quite a bit larger than the heat flux from the crust below. Therefore, because the Earth has an ocean and an atmosphere, it seems quite logical that they produce, or at least contribute, to the forces that move the plates by the EC mechanism. For the reasons stated here I propose that Gaia can produce the forces needed to move the plates.

Chapter 2

Plate Tectonics

Observations Related to the Plate Tectonics Paradigm

In Chapters 2-4, I present background information that I deem helpful to understand why I promote the idea that Gaia acts to cause plate tectonics through the Expansion-Contraction (EC) mechanism discussed in Chapter 1. In the present chapter, I provide some discussion of the kinds of observations that one needs to look at in order to judge the effectiveness of the various mechanisms (Chapter 4) that have been proposed. In Chapter 3, I present my reasons for choosing the manner in which science works, that is, some philosophy of science that I believe is necessary to understand the various mechanisms and why some are more reasonable than others. In Chapter 5, I then present how the Expansion-Contraction (EC) mechanism explains the observations in a parsimonious way. If the reader skips to Chapter 5 now and then comes back to Chapters (2-4) I think that my chapter order may become a bit clearer.

It is not the purpose of this book to present the full history of the development of the concept of plate tectonics, but a brief review might be helpful. In my opinion an excellent source to read about the history of this scientific revolution from Wegener's continental drift to sea-floor spreading that led to the idea of plate tectonics in the 1960s and 1970s is Walter Sullivan's book *Continents in Motion* (1992) and I take much information from that work. I also highly recommend H. W. Menard's book *The Ocean of Truth* (1986), a fascinating story by a scientist who was in the middle of the plate tectonics revolution. It is to my liking, because he brings in simple ideas of the philosophy of science and how science works from the view of a scientist.

More than fifty years after the rejection of Alfred Wegener's concept of continental drift, by most geologists, a large body of research, much sponsored by the great need of the U.S. military to understand the oceans, led to the idea of seafloor spreading. This concept originated in papers of Robert Dietz of the Lamont-Doherty research group and Harry Hess of Princeton in the early 1960s. In his book "Continents in Motion" Sullivan (1992) mentions a statement by Hess after Dietz presented a seminar at Princeton University on the relative motion of transform faults: "You have shaken the foundations of our science." Hess, a leading figure in marine geology of the time, later wrote an informal paper to his colleagues entitled "an essay in geopoetry" that convinced many geologists that the ocean was spreading at the mid-ocean ridges. Hess produced a diagram that was similar to one that Arthur Holmes (1944) included in his book on the proposal that convection in the mantle produced the growth of new crust at the ridges. Dietz gave Hess' concept the name "seafloor spreading." The respect that the Geological community had for Hess and Holmes may be one reason why the idea of convection currents exists today despite its obvious inconsistencies (Fischer, 2014).

Confirmation of seafloor spreading came, from among other research, the work of a graduate student, Frederick J. Vine, and his advisor D. H. Matthews of Cambridge University in England. They proposed that the reversals of the Earth's magnetic field, that are fixed when rock solidifies, show up as regions of bilateral symmetry of these magnetic anomalies on either side of an ocean ridge. This proved, to many marine geologists, that new ocean crust is produced at the ocean ridges causing seafloor spreading. The actual rate of spreading could then be measured.

The idea of seafloor spreading led to the concept that the Earth is covered by about twelve, or so, rigid plates divided by three types of boundaries. This was proposed by W. Jason Morgan in 1967 and as far as I have researched the name "plate" was first proposed by McKenzie and Parker (1967) although the development of ideas such as this can be traced to a large number of scientists.

There is no doubt that the plate-tectonics model is an important paradigm in geology (in the Kuhnian sense), because it gives a framework to explain how the Earth's crust works. Many philosophers of science including Imre Lakatos (1968), a colleague of Karl Popper, an advocate of falsification in producing scientific progress, suggested a change in the idea of the term "paradigm' described by Kuhn and I do not disagree with this modification. Kuhn (1964), after all, did have many uses of the term "paradigm." I shall continue to use Kuhn's term "paradigm" in this discussion because I think that many nonscientists are familiar with essentially what it means.

However, ideas on the <u>mechanism</u> of plate motion are still not to the level of a theory, or paradigm, because there are many different concepts on the forces of motion and much disagreement among geologists. To examine the multiple hypotheses of motion let me first discuss those properties, or observations, related to the plate tectonics model that should be considered in an attempt to examine the various mechanisms that have been proposed. An accepted mechanism must fit the characteristics of a good theory as I suggest in Chapter 3. Another way of saying it is that a mechanism must predict important observations.

The plates on the Earth as they occur today are shown in Figure 12 redrawn from a United States Geological Society (USGS) map (also see the back cover). Morgan (1967) and others discuss the three types of boundaries of the plates: They are divergent and convergent boundaries and transform faults. The most important of these related to the thesis of this book are the divergent boundaries where new oceanic crust is being formed. Why are the ridges associated with continental plates, such as those in the Atlantic, around three sides of Africa or around Antarctica located very nearly midway between continents? Why do these mid-ocean ridges migrate over time? Why do continents such as North America move about twice as fast as the Mid Atlantic Ridge moves away from Africa and Europe?

Why are the ridges of the eastern North Pacific being covered by the North American continent implying that they do not migrate? We need to explain why the spreading rate varies from place to place along the ridges and why there is variation in height from one part of a ridge to another and between ridges at different locations. Why, for example, does the ridge of the East Pacific Rise spread faster than those in the Atlantic? Why is the spreading rate faster in the South Atlantic than it is near the Equator? Why do some ridges have triple points? Because tension could pull plates apart, according to many observers, what causes them the stay together (or be "rigid" as many geologists assume)? What force can produce compression to hold a plate together?

The second kind of motion associated with the tectonic plates is that convergent boundaries exist where there are deep-sea trenches and sinking slabs of oceanic lithosphere, crust and sediments. These are mostly associated with oceanic plates in the Pacific. How deep do the slabs move after they sink into the mantle? Are they blocked by the 670 km discontinuity or do they sink all the way to the core-mantle boundary? Do the slabs of oceanic crust collide against the continents where they undergo delamination of part of the oceanic crust? Delamination is the scraping off of material from the subducting oceanic plates, including sediments, crust and lithosphere and also scraping from the bottom of continental plates under which they are subsiding, that all sink into the mantle. Do the slabs, and other material, that sink in the trench regions, flow under the continental plates above the 670 km discontinuity to melt and again produce fertile material that rises in the ridges associated with continental plates causing sea-floor spreading? If so why does the fertile material melt and rise under the ridges to form new crust? Or do they sink to the bottom of the mantle or to the top of the Earth's core?

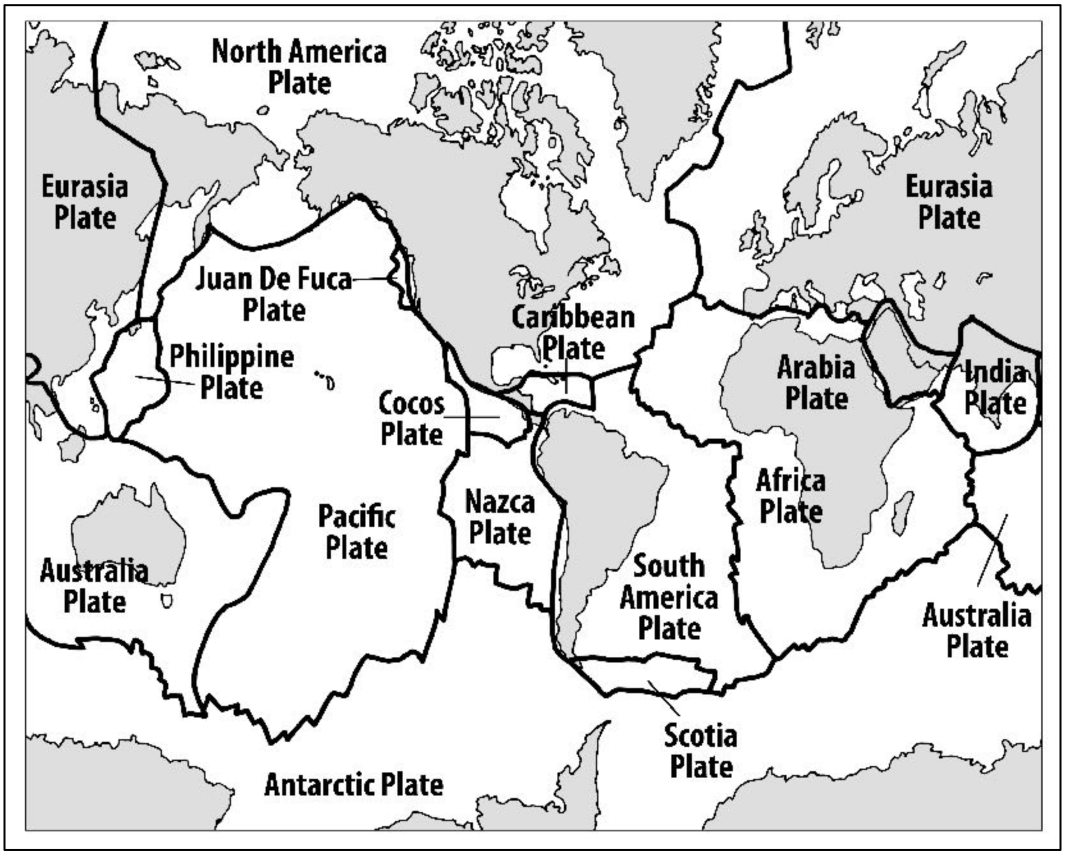

Figure 12: Schematic diagram of the plates on Earth.(redrawn from a USGS map).

Do the slabs that flow down in the trench regions "roll-back" as Hamilton (2007) proposes so that the continental plates are "pulled" toward the trench? Is the upper mantle under continents warmed because it is insulated by the continent? If the heat flow through the crust is greatest in the region of the ridges why is it not hotter under continents than beneath ridges as would be expected due to insulation of the mantle? If it is hotter under continents how do we explain why ridges are elevated with respect to the abyssal plains? Should not greater upward heat loss under ridges make the upper mantle cooler under ridges?

The third type of motion is the lateral motion associated with the transform faults between blocks of crust moving at different speeds away from the ocean ridges. What causes the variation in motion? Could it be differences in the push against continents?

It has been a quarter of a century since our original paper on the EC mechanism of plate motion was rejected for publication. I did not pursue the idea more vigilantly as it seemed to me to be a very difficult process to get it accepted and I was involved in other research that took much of my time. In that paper we presented four alternative hypotheses on what causes the plates to move.

Although our EC idea can be thought of as a "top-down" process, I did not know about that concept until 2012 when I read a paper by Don Anderson (2001) who proposed the idea. In Chapter 4, I compare six mechanisms, but more exist. They are presented as multiple hypotheses and are divided, as well as can be understood, into those mechanisms that involve heating and movement from below (bottom-up) and those that require only the plates themselves, usually using gravity as the force causing the plates to move (top-down mechanisms). In all cases the possible anomalies are presented. Some anomalies I think consist of what is essentially a rejection, but others only suggest a weakness in the hypothesis.

Chapter 3

Some Thoughts on How Science Works

"Blind commitment to a theory is not an intellectual virtue; it is an intellectual crime."
-- Imre Lakatos

The Scientific Method

Conventional wisdom is very hard to change. The dominant idea on how plate tectonics works is that mantle convection moves the Earth's plates. If one enters the internet via one of the search engines (for example, see "Google Images" after searching "mechanism of plate tectonics"), one finds a large number of diagrams devoted to some form of mantle convection. Very few who believe in this mechanism attempt to eliminate convection arguments based on the simple ideas of how science proceeds. Along with Anderson (2007) and several of his colleagues I pose that there are a lot of anomalies to the mantle convection model and that conventional wisdom should be changed. I discuss this further in Chapter 4.

In this chapter, I offer some suggestions on what I have gathered from others on how science works. I give short reviews of three works that are important to my thinking. First let me discuss some ideas on the scientific method, then on Thomas Kuhn's ideas on scientific revolutions, or Lakatos on "research programmes," and lastly the arguments on what makes a good scientific theory.

The first idea is that of T. C. Chamberlin, (1897), from "Studies for students: The method of multiple working hypotheses," *Journal of Geology.* 5, 837. I found this paper by reading John R. Platt's article in *Science* entitled "Strong Inference" (see below). I agree with Platt that Chamberlin's paper should be read and digested by all graduate science students. Chamberlin warns that if one holds a "ruling theory," probably one's own idea or an idea on which one has worked, one does not consider that it might be wrong, or rejected, by further observations or experiments. Instead one tries to prove that his ruling theory is correct. Chamberlin states: "the moment one has offered an original explanation for a phenomenon which seems satisfactory, that moment affection for his intellectual child springs into existence, and as the explanation grows into a definite theory his parental affections cluster about his offspring and it grows more and more dear to him." And later: "there springs up also unwittingly a pressing of the theory to make it fit the facts and a pressing of the facts to make them fit the theory." Chamberlin also rejects the method of the "working hypothesis" that was considered to be a major element of the scientific method of the time. He states that the working hypothesis can all too often become a ruling theory. He then suggests that the use of multiple hypotheses is the best approach. By considering multiple hypotheses of a given phenomenon the "...investigator thus becomes the parent of a family of hypotheses; and by his parental relations to all is morally forbidden to fasten his affectations unduly on any one."

I suggest that the EC mechanism, proposed in this book, is just one of a multiple of hypotheses on how plate tectonics works and researchers should consider the step-wise exclusion of hypotheses until one remains including one's own hypothesis. If none remain un-excluded the researcher should look for new hypotheses. Several of the most important (in my view) hypotheses (I call them mechanisms) are considered in Chapter 4.

I note, with an apology, that my own infatuation with what I have called the EC hypothesis is very much in danger of being a “ruling theory” in my own mind. It should be considered, accordingly, as one of a multiple of hypotheses. I still cherish it as my “intellectual child” and time will tell if my faith will be rewarded at some long time in the future! Yes, it has good fit to many of the common observations related to the Earth’s plates, as I suggest later, but it has to be proven that it can work, that it has no “fatal” flaws.

The second of my favorite discussions on how science should work is John R. Platt’s, 1964 article entitled “Strong Inference,” *Science* 146, 3642. Platt's strong inference is essentially Bacon's scientific method, or inductive reasoning (see Francis Bacon, "The New Organum and Related Writings" or read about it on the web). Platt says that strong inference is based upon the systematic application of:

“1. Devising alternative hypotheses;
2. Devising a crucial experiment (or observation program), or several of them, with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more to the hypotheses;
3. Carrying out the experiment (observation) so as to get a clean result;
4. Recycling the procedure, making sub-hypotheses to refine the possibilities that remain; and so on.”

The major idea is to devise an experiment (observation in most of the Earth Sciences) to exclude at least one of many hypotheses that are available. According to Platt, Bacon believed that the inductive approach could be learned by anyone. Platt quotes Bacon: "My way of discovering sciences goes far to level men's wit and leaves but little to individual excellence, because it performs everything by the surest rules and demonstration...Truth will sooner come out from error than from confusion." Bacon's way of induction requires that nature be analyzed by a series of exclusions before a hypothesis can be accepted. The latter would be when it cannot be excluded.

Platt quotes Leo Szilard from a conference on Biophysics in 1958: “The problems of how enzymes are induced, of how proteins are synthesized, of how antibodies are formed, are closer to solution than is generally believed. If you do stupid experiments, and finish one a year, it can take 50 years. But if you stop doing experiments for a little while and *think* how proteins can possibly be synthesized, there are only about 5 different ways, not 50! And it would take only a few experiments to distinguish these.” I suggest that geologists and geophysicists step back and think about the mechanisms proposed that might produce plate motion and to devise observation programs to disprove as many as possible. Instead, most scientists plan observations to prove a mechanism to be correct.

Platt goes on: "The difficulty is that disproof is a hard doctrine. If you have one hypothesis and I have another hypothesis, evidently one of them must be eliminated... Perhaps this is why so many tend to resist the strong analytical approach - and why so many great scientists are so disputatious."

Note that Platt uses the term “hypothesis” in the list above but in this discussion I have used the term “mechanism” to mean essentially the same thing. My friend Gene McLaren (personal communication, 2014) uses the term “operational definition” in the same sense and many scientists use that term. In any case, no matter the term, what needs to be explained, in the case of the Earth Sciences, is usually an observation like those mentioned in Chapter 2 (ridge migration, height of ridges, rate of sea floor spreading and the like). I discuss this in more detail below in the section on the Characteristics of a Good Theory.

Scientific Revolutions

"The way in which a new scientific truth usually becomes accepted is not that its opponents are persuaded to declare themselves enlightened, but rather, that its opponents gradually die off and the following generation grows up accepting the truth from the start."
--Max Planck

Both Chamberlin and Platt suggest how science should work to speed up progress. In his *Structure of Scientific Revolutions*, Univ. Chicago Press, Chicago, Thomas S. Kuhn (1962) discusses the history of science and how he thinks it actually proceeds. "Normal science" takes place in what Kuhn terms a paradigm. When normal science produces enough anomalies to an existing paradigm someone, usually young or new to the field, proposes an idea that leads to the next paradigm. According to Kuhn this starts a scientific revolution. Thus, Kuhn suggests that science proceeds in a series of steps rather than in a linear fashion as we are taught to believe from our early lessons in elementary schools. In his 2007 book and many papers Don Anderson, who practices normal science, suggests that mantle convection does not explain some crucial observations such as mantle heterogeneity and that the subducting slabs, formed at the ocean trenches, do not always penetrate deep into the mantle. As mentioned earlier, and discussed in depth later in this book, it is also hard to explain differences in spreading rate, height of the ridges, triple points and some other important observations using the convection mechanism. These are important anomalies to the current wisdom that deep mantle convection, starting at the Earth's core, produces plate motion and they will be discussed in more detail in Chapter 4.

Kuhn proposes that after a scientific revolution takes place many younger scientists follow with work that he calls normal science within the new paradigm. Many of the older scientists in the existing paradigm do not change their viewpoints and even continue to dispute the newer ideas. Many take their older ideas to their graves. Over time the anomalies, from normal science lead to the next scientific revolution.

In an interview with John Horgan of *Scientific American* in 1991 Kuhn states that he is "pro science and pro paradigms." Horgan paraphrases Kuhn: "They provide the secure foundation needed for scientists to organize the chaos of experience and to solve ever more complex puzzles." Adherence to paradigms and to the conservatism of science enables it to produce "the greatest and most original bursts of creativity" of all human experience. I believe that this is what is happening in the field of plate tectonics today, but as anomalies build up some newer ideas like Anderson's "top-down tectonics" may take hold. I might pause to ask why there are "original bursts of creativity" as mentioned by Horgan above. Is this because the conservatism of science holds back new ideas, like a dam, until the water finally flows over the dam, so to speak, and then new ideas come forth?

There are many science philosophers who disagree with the ideas of Kuhn, many on the basis that science becomes a "popularity contest" given Kuhn's assessment, and scientists jump on a "bandwagon" of the new paradigm. For example Imre Lakatos (1970) suggests that instead of a sharp break from one paradigm to the next science undergoes a change in "research programmes" within the discipline. The most progressive "core programme" gains success while others decline. While I believe that this argument has many benefits, especially in the revolution from the shrinking Earth paradigm to that of plate tectonics, I also find that to generalize how science works across all disciplines is too difficult a task and the philosophers tend to be speaking to others in their discipline and not to the world outside of philosophy. It was not until I read Will Durant's "The Story of

Philosophy" that I was able to comprehend what Hegel and Kant were talking about! It seemed to me that they invented their own language.

Let me restrict the argument to the one "revolution" to which we are concerned in this book, the change from the idea that the Earth shrinks and that features such as mountains are caused by vertical, not horizontal, movements of the crust to the idea that horizontal movements produce many of the Earth's geological features. Alfred Wegener poked large holes in the "fixist" idea when he found many anomalies to the idea that continents do not move. But just take one case, bioclimatic evidence. When glaciologists found strong evidence that glaciers existed in what is now tropical Africa, dated some 250 million years ago, while for the same time period, that coal deposits were also found in Greenland and as far North as Spitsbergen the idea that the continents were at that time in the same locations as they are today could not be defended. Nevertheless, it took another 30 to 50 years before the idea that continents move with the advent of the new data on seafloor spreading in the 1960s. To say that the defense of the continental "fixist" notion was anything more than irrational, if this evidence is carefully examined, is not defensible. To say it with more emphasis the geologists of the time were irrational and not good scientists. Lakatos defends this irrationality using terms that scientists tend to defend the "core" of their "research programme" with a variety of thought processes. As we shall see later on in this book the geologists who defended the "fixist" notion merely used disputation and not science to defend their ideas. So why I do not agree with all of what Kuhn has to say on the idea of paradigms when he seems to present many forms of that term, his idea is at least understandable to the philosophical laypersons like me and probably to the audience of this book.

I have heard from many geologists the speculation that from 1929, the date of Wegener's last reprint of his book to about 1960, if a scientist wrote a proposal to defend the idea that the continents moved, that proposal would be rejected by the geological community in the United States. That is not irrational, because geologists were imbued with the shrinking Earth concept and had written many papers on the subject. The shrinking Earth was the current paradigm (Kuhn) or the progressive programme (Lakatos). However I have also read that after 1970, or so, if a geologist wrote a proposal to defend the notion of only vertical motions and the non-movement of continents that would also be rejected by the geological community. Research sponsors would be more inclined to fund newer ideas (the progressive programme) of seafloor spreading and plate tectonics to discover new "truths" than to try to replenish some of the older ideas.

Kuhn's idea that the scientists of the "rejected" paradigm cling to their older ideas can be illustrated by the response of Sir Harold Jeffreys, one of the great geophysicists of his time, to the ideas on continental drift, seafloor spreading and plate tectonics. In the sixth edition of his book *The Earth: Its Origin, History and Physical Constitution* published in 1976, long after the concept of plate tectonics was accepted by the geological community, he took exception to all of the evidence such as the "jig-saw" fit of continents, fossil evidence, seafloor spreading, plate boundaries, mountain formation and the like. In my opinion none of his arguments were adequate. Let me quote here only one of his statements regarding the climatic data mentioned above (glaciers in Africa, etc.).

"The meteorological evidence comes from former glaciations…The ready acceptance of this evidence springs mainly, I think, from the popular belief that meteorology is an easy subject; everybody knows when it is raining. But also everybody (in this country at any rate) finds fault with the weather forecasts if he reads them. The latter fact is a true indication of the difficulty of meteorology-even experts in it go badly wrong in trying to predict for 12 hours or less."

He goes on to expound on the difficulty of meteorology and weather forecasting. But it is climatic evidence that needs to be examined not weather forecasts! His argument is merely disputation at its worst and a great scientist like Jeffreys should not be involved with such nonsense. So, when Kuhn states that many scientists take their ideas to their graves I think that he is right and that irrationality is part of science. It will always be so.

The Characteristics of a Good Theory (or of a Hypothesis)

In the philosophy of science literature there is an extensive discussion on what makes a good theory. My first introduction to the idea came from Aaron Ihde (personal comm., 1961) when I was a graduate teaching assistant for his course called "The Physical Universe" at the University of Wisconsin. Idhe suggested that good theories should:

1. Explain any observations of phenomena or results of an experiment;
2. Be understandable, at least in a general way, to the interested lay person;
3. Be reasonable so that they are testable (some, like Karl Popper, say falsifiable and some like Francis Bacon that there should be the possibility that they can be disproved by experiment or observation);
4. Be economical or parsimonious (Occam's razor) and
5. Be predictive or fruitful leading to new observations or hypotheses.

The most important characteristic is that theories should explain what is known, that is, the observations that have been made related to the theory that it can explain. Alternatively, a theory should predict an observation (as in #5).

A theory should be "understandable" (item # 2) to the interested lay person in a very general way without such a person being able to decipher the mathematics or other details on the origin of the theory. Most people understand Einstein's theory of relativity in a very general way or even quantum theory even though most of us do not know why light has properties of waves as well as quanta.

The third characteristic relates mostly to the fact that theories should be subject to exclusion by experiment or observation. Some "all encompassing" theories cannot be excluded as the originator of such theories merely changes the context of the idea when challenged. In some cases no experiment or observation can be used to exclude such a theory.

Occam's razor (#4) can be illustrated in the arguments that Copernicus used to explain that if the sun was at the center of the planetary system (the heliocentric model) the retrograde motion of the outer planets, such as Mars, could be explained in a manner that is more efficient than that of the Ptolemaic of geocentric theory (the Earth at the center of the universe). The geocentric model requires the addition of epicycles to explain retrograde motion, a phenomenon that occurs only when the Earth, Sun and Mars, or another of the outer planets (those beyond Earth) are in alignment. The heliocentric model requires no extra frills, parts or assumptions. In the Copernican model Mars does not retrograde, it merely appears that it does, because the Earth, in an orbit closer to the sun, passes Mars making it look like Mars is moving backwards with respect to the background stars. His explanation is more natural, more parsimonious, as I like to say, so that we can use Occam's razor to cut away the geocentric model even though it may explain most of the observations to some degree, always with modifications to adjust for where it did not explain some aspect of planetary motion.

The last characteristic, that a theory be predictable, can also be used to affirm or reject the idea (or mechanism as used in this book). If no reasonable prediction can be made by a theory then it is not very useful. Thus, if one of the mechanisms proposed for producing plate motion does not predict a well-known observation then that mechanism can be set aside (or excluded to use the stronger language).

I discuss several key observations of the Earth's plates in Chapter 5 to show how they can be explained by the EC mechanism in a natural way so as to adhere to the concept of Occam's razor.

Disproof Vs. Disputation

Walter Sullivan (1992, 2nd Edition, first published in 1974), mentioned before, discusses the scientific revolution that changed ideas in the Earth Sciences from the paradigm of continental fixity and a shrinking Earth to the present conventional wisdom of plate tectonics. Sullivan starts with the ideas of Alfred Wegener on the movements of continents and discusses the elegant research that took place in the 1950s and 1960s by marine geologists that confirmed Wegener's concept and led to the current concept of plate tectonics.

But why did it take fifty years? In the Introduction to his book Sullivan states:

"The manner in which this theory has gained acceptance underscores the fallibility of scientists and the fact that fashions prevail in science as they do in clothing or hair styles. Yet it also demonstrates how, to those with vision, seemingly disparate, unrelated discoveries can suddenly be brought together into a theory that not only is plausible but explains a variety of age-old problems."

Sullivan confirms Kuhn's idea that a scientific revolution can be brought about quite suddenly and that science proceeds in a stepwise manner. Some say that the plate tectonics "revolution" took place in the 1960s when the marine geologists took a serious look at the ocean bottom. However, there were many southern hemisphere geologists (for example Alexander Du Toit and Samuel Warren Carey), many European geologists (Van Waterschoott Van der Gracht), and even some English Geologists (Arthur Holmes) who thought that a "revolution" had already started. So when a revolution begins is a matter of who believes in it. The "plate tectonics revolution" did not start for most American geologists until the late 1960s and, of course many geophysicists did not recognize the change in the science of the Earth as a "revolution" (for example, Jeffreys, who was English). Personally, I think that the change from the old paradigm (continental fixity) to the new (continental mobility) started in earnest with Wegener, a meteorologist. The problem is that most geologists did not take a serious look at the evidence he presented as we shall see in the following.

To illustrate this point I quote from the preface of the last reprint of Wegener's book on the origin of continents and oceans (1929). He quotes Alexander du Toit:

"As already stated, we must turn almost exclusively to the geological evidence to decide the probability of this hypothesis (continental drift), because arguments based on such evidence as the distribution of fauna are not competent here; they can generally be explained equally well, even less neatly, by the orthodox view that assumes the existence of land bridges, later sunk below sea level."

Well, even though the "land bridges" were completely imaginary, as many ideas in science are, du Toit wrote a book in 1937 entitled "Our Wandering Continents." But his point that scientists in their own discipline should be satisfied with a new way of thinking is one of the features of science in all times.

Sullivan was not a scientist, but a writer on science topics for the New York Times. His work is an example of how some non-scientists can understand what is going on in science more clearly than can be seen by most scientists working within a paradigm. He discusses a symposium on continental drift organized by W. W. J. M Waterschoot van der Gracht, a Dutch geologist, on behalf of the American Association of Petroleum Geologists held in 1926 in New York. Van der Gracht stated that he was personally in favor of the idea of continental drift. However, the response of the "big guns" of geology in the U.S. could only be called disputation. Sullivan quotes Rolin T. Chamberlin (T.C. Chamberlin's son) of the University of Chicago: "Geologists might well ask if theirs could still be regarded as a science when it is possible for such a theory as this to run wild."

Charles Schuchert of Yale disagreed with the "fit" of the continents of Africa and South America, an argument that lasted until a Symposium in 1966 when Sir Edward Bullard produced maps of the "jig-saw" fit between continents on either side of the Atlantic Ocean that were obtained mathematically. And even after that 1966 Symposium Sir Harold Jeffreys, a proponent of the continental "fixist" concept, published a short paper in the Proceedings of the Royal Academy saying, to the effect, "I simply disagree that they fit." But by that time the Marine Geologists had won their case that seafloor spreading had proven the idea that continents moved. Jeffreys (1976) took the idea that continents are fixed in place to his grave.

Also in the 1926 Symposium Bailey Willis of Stanford University said that Wegener's book (1924 version) "leaves the impression that it has been written by an advocate rather than an impartial observer."

Edward Berry, professor of paleontology at Johns Hopkins, stated that Wegener's method, "is not scientific, but takes the familiar course of an initial idea, a selective search through the literature for corroborative evidence, ignoring most of the *facts* that are opposed to the idea, and ending in a state of *auto-intoxication* in which the subjective idea comes to be considered as objective fact." (italics mine). Wouldn't it have been better if Berry selected at least a few of the "facts" that Wegener ignored? But, also, did Berry prove that it could snow at the equator in Africa and India 250 million years ago if these land masses were where they are today? Did he show that at that same time coal deposits could be laid down in Greenland, Spitsbergen and Alaska, now regions of very cold climates? Evidently, one who is disputatious, because of reputation does not need to prove what he believes, or disprove what he doesn't!

As my friend Gene McLaren (personal communication, 2014) points out what is needed is an "operational definition of measurable variables." Wegener's operational definition was that the continents "plowed" through weaker oceanic crust driven by a "pole fleeing force," but most geologists knew that this was impossible. So they rejected the continental drift hypothesis. Nevertheless, the geologists did not put faith in the climatic, fossil and other evidence that Wegener presented that showed that the idea that continents were always where they are now was impossible.

The final quote that Sullivan takes from of the 1926 Symposium is by Chester Longwell of Yale: "The theory shows little respect for the time-honored ideas backed by weighty authority... Its daring and spectacular character appeals to the imagination of both the layman and the scientist. But an idea that concerns so closely the most fundamental principles of our science must have a sounder basis than imaginative appeal." Did Longwell try to disprove the many anomalies to the continental "fixist" idea? Not very likely I think.

Does "weighty authority" exemplify the "truth"? The Pope had weighty authority when he gave Galileo the choice to recant his ideas that the sun was at the center of the solar system or be tortured. Galileo recanted, rather than be tortured, but then gave two volumes of his work to one of his students for her to smuggle out of Italy by carriers for others in the scientific community and they eventually reached many including Newton. See Bertolt Brecht's (1937-30/1943) play on "The Life of Galileo." It has been purported that Newton, after he formulated his laws of motion and gravity said: "I stood on the shoulders of giants." The giants, I suggest, were Copernicus, Gallileo, Tycho Brah and Kepler, but not the Pope.

So what I have said, so far, on the idea of disputation agrees quite well with Thomas Kuhn's concept of scientific revolutions. Even though there may be many anomalies to a paradigm, those who are in the midst of the normal science within that paradigm either ignore or dispute the new ideas that may result from the anomalies. I give one more example.

When our seminar group sent our original 1989 paper to the journal *Global and Planetary Change* we (or at least I) did not expect that it would be published as mentioned earlier, but I did expect it to be disputed. One of the reviewers said: "It is crucial not to become dogmatic in geophysical concepts (or any science), so new and different ideas should not be stifled simply on that basis. It would be comforting (and more convincing) if the proposed ideas were based on a thorough understanding of the concept, ideas and models that have been proposed to date by others. This has not been satisfactorily demonstrated in this paper. Not only were most of the crucial contributors to the field not cited (names of several authors were given parenthetically), a clear understanding of their contributions was not at all evident in this manuscript."

So, does a "clear understanding," and the quoting of existing ideas, need to be a prerequisite for a new idea to be published? What harm would it do to the geological community if an idea were published without including all of the papers written before if the idea was not immediately disproved? It was not clear to us that mantle convection (the subject of the papers we did not quote) was proven so why should we quote them and then "reject" them? We did, of course, read many of the papers on convection in the mantle including some by the authors mentioned by the reviewer. We did not accept them as being reasonable in our way of thinking because they did not explain ridge migration and some of the other key observations mentioned in Chapter 2.

Chapter 4

Current Mechanisms of Plate Motion

"It is remarkable how long men will believe in the bottomless of a pond without taking the time to sound it…Many have believed that Walden reached quite through to the other side of the globe."
-- Henry David Thoreau, *Walden*

"*All truth passes through three stages. First it is ridiculed. Second, it is violently opposed. Third it is accepted as self-evident."*
-- Arthur Schopenhauer

Top-Down Versus Bottom-Up Hypotheses

I have been reading most of Don Anderson's latest work including his ideas on what he calls "top-down tectonics." His statement is that plate tectonics is a "far from equilibrium, self-governing system" and his ideas, and many of his colleagues, on why the plate model may be superior to the mantle convection and mantle plume arguments as a cause of plate movement is in general agreement with the ideas I have proposed herein.

This is a very encouraging work, for my way of thinking, and includes the work of many authors in the Geological Society of America *Special Paper* 388: *Plates, Plumes, and Paradigms*, a monumental document. Most of the papers in this very large volume are difficult to understand completely for the layperson, because non-geologists have some difficulty understanding all of the scientific procedures and some of the language of geology, geodynamics, geochemistry and geophysics. Most authors give summaries that are understandable to the non-geologist and the picture of Anderson's ideas became clear to me on reading much of this work. I have also read Anderson's book *New Theory of the Earth* (2007) and many parts several times over. In his book and in many of his latest papers he discusses ideas on how self-organization of the plates produces the "forced convection" in the upper mantle and not the other way around.

Don Anderson is not familiar with our ideas of the EC mechanism. His paper, *The Earth as a Planet; Paradigms and Paradoxes* published in *Science* (1984) was, I suspect, the launching pad for thinking about geoid highs and gravity-produced mechanisms of plate tectonics. I attended the 1989 meeting of the American Geophysical Union (AGU) in Washington, DC where Anderson was the keynote speaker and the AGU president that year. His talk hinted on the ideas that were to come about his thinking on how the Earth works. I was a member of AGU in 1989 and presented, on behalf of our seminar group, a poster session on our EC mechanism. Only one professional level scientist attended our poster session, John Bird, who was a faculty member of the Department of Earth and Atmospheric Sciences, SUNYA in the 1960s when I was also a member of that department. Bird (Jack as we knew him) encouraged me to go on with our ideas on the EC mechanism as persistence would eventually be rewarded. From my experience, Jack was correct although I guess that after the expected rejection of our paper by the journal "*Global and Planetary Change*" and because of other research work that needed my time, I thought that the geological community may not have been ready for an idea that deviated so far from conventional wisdom in their field. The reviewer's comments were disputatious, and did not disprove our ideas, as discussed earlier, and I suspect that is a regular occurrence when new ideas are presented. I was very familiar with how long it took for Alfred Wegener's continental drift idea to be recognized. Although Wegener's first paper on drift was written in 1912 and the latest edition of his book was published in

1929, the concept was not accepted until late in the 1960s. As mentioned earlier in this book, Sullivan, suggested that most of the disagreement was merely disputation.

Bottom-Up Mechanisms

Mantle Convection

The conventional wisdom on the mechanism of plate tectonics is that deep convection, starting at the core-mantle boundary, produces the driving force to move the plates. Most of the geology textbooks present the concept as dogma. The idea goes back to the early part of the 1900's, but also earlier, when several proposals were made that the Earth's mantle acts as a convecting fluid due to heat coming from below much as a pot of boiling water. Perhaps the most respected of these arguments was presented by Arthur Holmes an English geologist who suggested in 1931 that convection was a possible driving mechanism of Alfred Wegener's continental drift. Holmes, a supporter of the continental drift idea, suggested that radioactive decay produces the heat to drive the convection and the diagram of mantle convection in his 1944 book has been reproduced in many geology textbooks. The primary thought is that the heat coming from the interior of the Earth is larger than would be expected by molecular conduction alone so that some convection motion is needed to move the heat upward to the surface. Holmes' idea was furthered by the work of Harry Hess and because both were highly respected geologists the idea of convection became the current conventional wisdom on the mechanism of plate movement.

As mentioned earlier, if you use an internet search engine on the subject "mechanism of plate tectonics" you will find a multitude of diagrams promoting this convection idea and many websites that suggest that convection in the mantle is the only mechanism that produces plate motion. The idea, as shown in most of the diagrams, is that the upward part of a convection cell is always exactly under a ridge. New ocean crust is produced at the ridge and the convection causes seafloor spreading away from the ridge. Some portray this as the convection forcing the plates away from the ridges by friction, others that the plates ride on a "conveyor belt" of the moving upper mantle (the asthenosphere).

Many papers including much of the work of J. Tuzo Wilson, W. Jason Morgan and others in the 1960s and early 1970s illustrate that convection can occur in a relatively solid mantle, but I find that many assumptions are required if one reads the papers carefully. To start with, the linear geography of the ridges would mean that the convection cells are in rolls. Convection normally takes the form of cells that are nearly circular (see "Benard convection" on the Internet). For convection to explain that the ridges which started right at the boundary of Africa have moved away significant distances to the east and west from that continent the geometry of the convection cells must change drastically. None of the many papers I read on the convection mechanism mentioned that this was a problem for the mechanism.

In my search of ideas from the early phases of the plate tectonics concept (1960s to the 1990s) to understand mantle convection I found only one writer who considered the difficulty posed by the geometry of convection. That was in the book T*he Ocean of Truth*, by H. W. Menard (1986). On p. 184 Menard writes:

"*If the spreading was a fact, the spreading center had to drift, and the problem shifted to how the convection cells went with it or, a startling thought, whether the cells really existed.*"

And a bit later:

"Heezen went on to discuss what he perceived as the principle weakness of the convection hypothesis for the origin of ridges, trenches, and fracture zones as proposed by Dietz, Hess and myself. Once again, the geometry of the convection cells seemed improbable."

I can find no papers, where deep convection is proposed as the cause of plate motion, that show how the convection cells must get larger to account for the migration of the ridges associated with continental plates, that is, for the upwelling from convection to be always exactly under the ocean ridges, but there is an abundance of work on ridge migration (*c.f.* Carbette *et al,* 2004; Masalu, 2007 and Katz, *et al* 2007 for recent discussions). This rather important anomaly to the convection concept is depicted in Figure 13 where I show that the ridges on either side of Africa must move away from that continent over time. In Figure 13a the upward flow of convection cells start out at the edges of Africa. After some time they have moved away from Africa both to the west (Atlantic side) and to the east (Indian Ocean side) as illustrated in Figure 13b. In order for this to happen the convection cells must get larger. The assumptions needed for this to happen shows that this convection mechanism is far from parsimonious and should be suspect according to the Occam's razor idea. Note that this ridge migration is also shown in many diagrams on the Internet (search "maps of the ocean bottom"). In particular, see the map of Heezen and Tharp and the NASA (SEASAT) map. Note that the ridges between continental plates that started out at the borders of the continents of Pangaea are now in the middle of the oceans far removed from their original locations.

There are other problems with the convection mechanism. How does mantle convection explain the variation in rate of seafloor spreading, the variation in the height of the ridges? How does it explain triple-points, or intersections of ridges? Despite my reading of many dozens of papers on convection I find no simple explanation of these observations. How can convection explain that the plates remain intact?

The problem of ridge migration is also shown in diagrams of the break-up of Pangaea and the movement of the continents away from Africa. This is illustrated in Figure 14. During the age of Pangaea, about 250 million years ago, the continents were very nearly attached. At the present time they are separated by thousands of kilometers and the mid ocean ridges that surround the continent of Africa have moved away in both directions and away from Antarctica as well.

In his book Anderson (2007) and in many papers he and several of his colleagues suggest that convection in the mantle requires it to be relatively homogeneous whereas most studies including results from tomography indicate that the mantle is rather "lumpy" due, at least in part, to the subduction of oceanic plates in the deep-sea trenches. This is another anomaly to the convection idea. Anderson also suggests that the plates would not be "rigid" as stated by most adherents of the mantle convection idea and that compression must keep the plates intact. Tension would pull the plates apart.

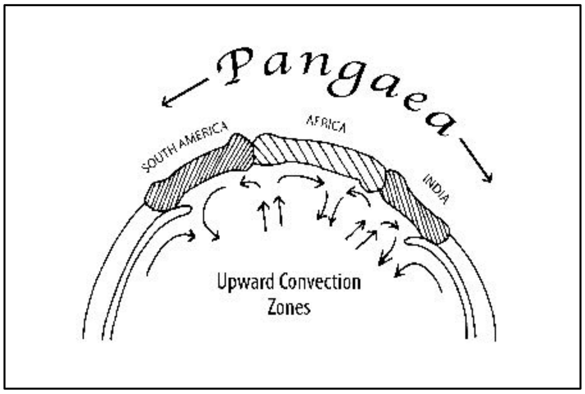

Figure 13a: Possible convection cells at the start of the break-up of Pangaea.

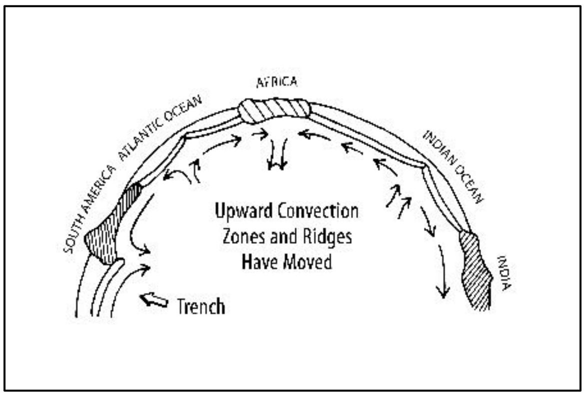

Figure 13b: Possible convection cells after some 200 Million years after the break-up of Pangaea.

Consider one more thought process regarding observations to be explained. Not mentioned in Chapter 2, regarding observations, is what the evidence from the age of islands in chains brings to the discussion of mantle convection. Consider the Hawaiian Island chain. First, visualize that a cell of a large convection pattern causes the Pacific Plate to be moved to the northwest by friction on the plate above (friction is one of the ideas that moves the plates in the convection hypothesis). Thus, the mantle would be moving faster than the plate because friction is not strong enough of a force. If a hot spot plume, presumably starting fairly deep in the mantle, is carried along with the convection then the Hawaiian Islands produced by this hot spot would get younger to the northwest. This is not the case. The youngest of that chain is the island of Hawaii and the islands get older to the northwest.

Secondly, visualize that the convection cell acts like a conveyor belt moving the plate along (another conjecture of the convection hypothesis). Then a deep forming hot spot, carried in the mantle movement, would remain always in the same place with respect to the Pacific Plate. There would be only one island, so this is also not the case shown by the data.

A third idea related to the Hawaiian chain is that a hot spot from deep in the mantle is unconnected to the upper mantle and rises through the moving convection below the plate to form the islands. This would have the correct configuration and the islands would get older to the northwest as they move with the Pacific plate. But one has to consider if this is possible. The upward motion of the hot spot would have to be fast with respect to the rate of movement of the plate.

The final thought would be that the plate movement is independent of the motion of the proposed convection, that is, that the convection does not exist. This would be the case in top-down tectonics where it is the plates that move independently of any flow from below. With this assumption the island age would be in agreement with the island age data with the older islands to the north west of Hawaii. That is the opinion taken in this book.

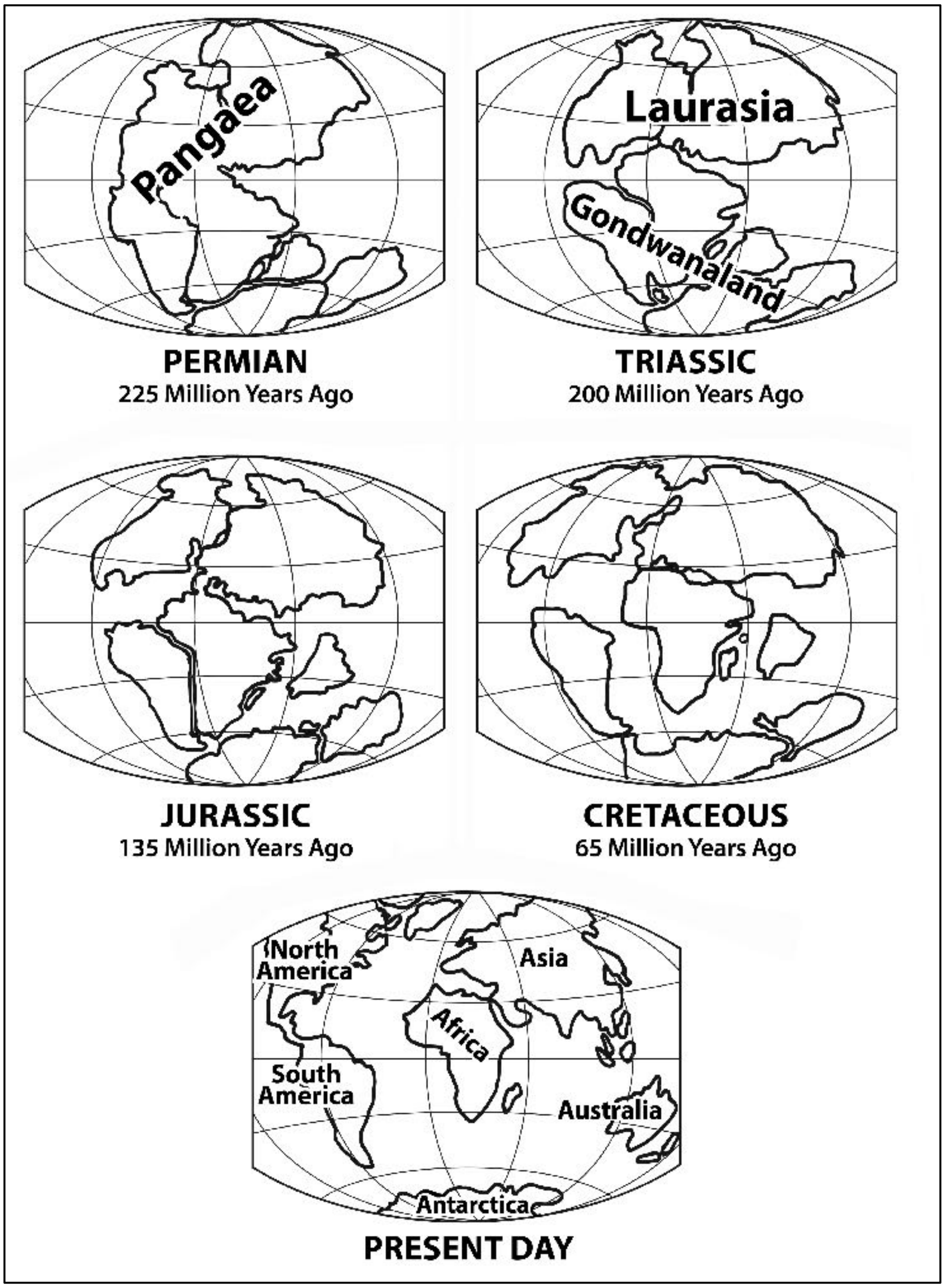

Figure 14: Schematic of the break-up of Pangaea and migration of continents over time.(redrawn from a USGS diagram on the Internet)

Mantle Plumes

Morgan (1972) proposed that deep mantle plumes could explain plate motion and there has been a considerable amount of research on this idea since that time (*c.f.* Kerr, 2013). Tomography studies show that there may be three or more large plumes that originate at the core-mantle boundary, one near Iceland and one near Yellowstone National Park and perhaps one in the Pacific, but some authors propose that there are many more such plumes. As I see it, the problem with the argument that large plumes can cause the plates to move is that because their configuration is so much different from the geographical pattern of ridges and trenches they cannot explain all of the simple arguments discussed earlier in Chapter 2. How, for instance, do the plumes migrate with the ridges between continental plates? How do they explain variation in spreading rate and ridge height?

The large work in *Geological Society of America Special Paper 388: "Plates Plumes and Paradigms,"* discusses this argument more fully than can I. In particular, the papers in this volume by Anderson (*Scoring Hotspots: the Plume and Plate Paradigms*"), by Anderson and Natland ("*A Brief History of the Plume Hypothesis and its Competitors*"), by Hamilton ("*Plumeless Venus Preserves an Ancient Impact-accretionary Surface*") and especially by Foulger, Natland and Anderson ("*Genesis of the Iceland Melt Anomaly by Plate Tectonic Processes*") all show that the plumes, if they produce plate motion, probably originate above the 670 Km density boundary within the mantle and not very likely from the mantle-core boundary as the plume concept suggests. This is consistent with the arguments of "top-down" tectonics. For a recent discussion see Anderson and King (2014) and the papers they quote.

Top-Down Mechanisms

Gravity Flow Forces

In 1986 Cox and Hart wrote a book entitled: "Plate Tectonics: How it Works" they drew upon a large number of observations and calculations of the various forces that could produce plate motion including ridge push, slab pull, slab suction and various forces impeding plate motion including friction and plate collision. These forces are shown in Figure 15. They suggest that the ridge push force is caused by flow away from ocean ridges by gravity, but it is difficult for me to understand why the ridges are elevated in the first place in this mechanism.

The cause of the elevation of the ridges is not explained in Cox and Hart's book nor is it explained in the papers they reference. Some authors say that it is hotter under the ridges than under continents. Then why is this so if the heat loss under ridges, due to the thin crust there, is greater than under the thicker continents and lithosphere? Should it not be cooler under ridges due to that heat loss? Anderson and King (2014) state: "The hottest regions of the asthenosphere may be as much as 200 ^{o} C warmer than beneath mid-ocean ridges." This is caused by the insulation of the asthenosphere by the continents and lithosphere.

Also, Cox and Hart suggest that the slab pull force associated with the sinking of the oceanic crust at trenches is the largest force causing plate motion. If the slab pull is greater than the ridge push why don't the plates, that are being subducted, break apart from the tension provided by this gravity flow force? Cox and Hart also use the concept of trench suction that pulls the continental plates such as South America to override the oceanic plates. How can the subduction at the trenches of the oceanic plates produce seafloor spreading at the mid-ocean ridges, say in the Atlantic, without tensional forces also pulling the continental plates apart? How does this hypothesis explain variation in ridge elevation, variation in seafloor spreading and triple points? While the GF hypothesis has many interesting features it seems to me that it lacks the explanation of several of the important observations mentioned in Chapter 2.

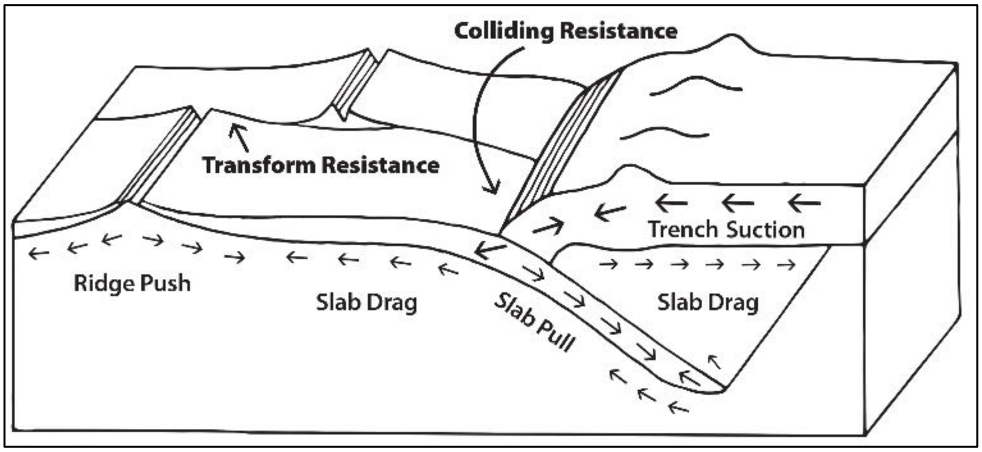

Figure 15. Forces in the gravity flow (GF) hypothesis (redrawn from Cox and Hart, 1986

Plates Driven By Subduction

Another top-down hypotheses used to explain plate motion is the idea that subduction alone can produce plate tectonics. A recent, and thorough, explanation of this argument is by Hamilton (2007), a paper that can be found on the internet. The driving force in this mechanism is the proposition that the oceanic plates at the trenches may sink due to "hinge rollback" causing the oceanic plates to fall downward rather than be pushed downward in a less vertical manner. This rollback leaves a void for the continental plate, such as South America, to fill the empty space. This is similar to the "trench suction" of the Gravity Flow hypothesis mentioned in the last section and would also produce tension in the plate being pulled.

Hamilton argues that the bottom-up hypotheses of plate motion do not explain most of the observations noted earlier in this chapter, most importantly the migration of the ridges. He rejects the plume hypothesis because it does not explain where the actual ridges are located, because the large deep mantle plumes exist only in a few locations and he points to evidence that some plumes like one near Iceland do not have deep mantle origin, but originate above the 670 Km discontinuity. The subduction hypothesis has similar weaknesses. The variation in ridge height, spreading rate and locations of triple points are not explained. Most importantly, the hypothesis does not explain how subduction can cause the ridges associated with the continental plates to be exactly between the plates on either side (such as the Mid-Atlantic Ridge). If the continents, like South America, are pulled by the "hinge rollback" due to trench suction why don't they break apart?

The Supercontinental Cycle (SC)

An interesting proposal on the cause of plate tectonics is in the work by Worsely, Nance and Moody (see their 1988 *Scientific American* paper by Nance, *et al*). It is best described to me by Worsely *et al* in the book *Scientists on Gaia* published by MIT Press in 1993. The authors describe a cycle of about 500 million years where continents collide to form a supercontinent, like Pangaea, spread apart and then come back together again. Their explanation of the break-up of a supercontinent is that the land masses insulate the mantle causing a geoid high. This causes a flow away from the supercontinent by gravity forces. The continents, a half cycle away, now are higher than the ocean crust that has cooled and they also form geoid highs so that they flow back to re-form a supercontinent.

Worsley *et al* present an interesting set of data on the cycle that begins with an ice age (cold climate) when a supercontinent exists, a greenhouse (warm) climate when the continents are spread apart, the reforming of an ice age when the continents are farthest apart, a greenhouse when they begin to "fall" back together and another ice age when they are reassembled. They trace this cycle from a variety of convincing fossil and geologic evidence over three billion years of the Earth's history.

The objection I have to the supercontinent argument is that it is difficult for the hypothesis to explain most of the observations mentioned in the beginning of Chapter 2. For example there is no explanation of ridge migration, variation in the height of ridges, the speed of spreading and what causes seafloor spreading. This hypothesis may relate to the idea that subduction and trench rollback causes the motion of the plates as discussed earlier.

An interesting aside is that T. Worsley was one of the reviewers who rejected our paper that we submitted to the journal *Global and Planetary Change.* His comments were thought provoking in arguing against many of our ideas and since he asked to be introduced to us I quote his comment here:

"This one is a heart breaker. It is extremely well-written, has new ideas but is very selective in using the literature to support it. Major revision that would result in a balanced treatment would, also, unfortunately, result in the author's themselves being forced to reject the hypothesis. These people are original thinkers and I like their way of thinking. Please identify me to them if that is journal policy."

The Editor then gave us the name "T. Worsely" as the reviewer. Unfortunately, I was busy doing other things and did not contact Worsely as that contact might have produced some good ideas. However, Worseley's comments in the margin of our manuscript were illuminating and thoughtful. I discussed each of them within our seminar group, but it would have been better to have reached him directly. That was a bit unfortunate.

Chapter 5
What Does the Expansion-Contraction Mechanism Explain?

I have discussed my viewpoint that the existing mechanisms in Chapter 4 have difficulty in explaining many of the observations mentioned in the beginning of Chapter 2. Here, I attempt to explain those observations using the Expansion-Contraction (EC) mechanism. My friend Gene McLaren puts it another way. "Incorrect predictions of a theory cause it to be revised or abandoned, in which case new hypotheses and theories must be created." I interpret Gene's use of the term "predictions" to be the sense that I am using "observations" in this book as discussed in Chapters 2 and 3. If a hypothesis cannot predict an observation then that hypothesis may be suspect. If the EC mechanism cannot be disproved then I suggest that more thought should be given to the idea of top-down tectonics and to the possible role that the biota play in producing plate movement.

Migration of Mid-ocean Ridges

The migration of the ridges associated with continental plates, such as the Mid Atlantic Ridge (MAR), is a <u>requirement</u> of the EC mechanism, because EC produces compression in the plates on either side of the ridges as they push against the continents as shown earlier in Figure 5 of Chapter 1. Thus, the oceanic crust on each side of a ridge must increase in area and the only way it can do this is to move away from the coast of Africa where it started. At the time of the break-up of the supercontinent Pangaea the MAR ridge was at or near the coasts of Africa and Europe as was North America. That was about 250 million years ago and it has moved a few thousand kilometers to the west. The distance from the MAR to Western continents has also increased by about the same amount, due to seafloor spreading, and thus the New World continents also must have moved westward, with respect to Africa and Eurasia, at about twice the distance that the MAR has moved. The same situation exists for the ridges on the east side of Africa and between Africa and the Antarctic continental plate.

Elevation of the Mid-ocean Ridges

The mid-ocean ridges, such as the MAR, are elevated some 1 to 2.5 km above the abyssal plains on either side of the ridge (see Figure 10). In the convection model, this is explained as being due to heating under the ridge, or by sinking away from the ridge due to gravitational forces in the gravity flow (GF) mechanism. In the EC mechanism the ridge elevation is caused by compression of the plates being forced against the continents on either side as shown in Figure 5. This causes up-warping of the ridge much as it happens on ice-covered lakes. The compression is caused by thermal expansion at the ridge as the plates push against continents on either side much the same as when lake ice pushes against the shores. I propose that when there is a cooling or contraction at a ridge the continents still hold the plates from moving, at least to some extent, and so the ridges remain elevated.

Compression in the Plates (Push versus Pull)

Compression in the tectonic plates holds them together. Various forces, some due to geography or other random circumstances, can change the direction they move and cause them to crash against each other to form mountains, trenches, earthquakes, volcanoes, triple points and the like. Compression is a pushing force while tension in the plates is a pulling force. It would seem that tension can pull the plates apart. Compression by the EC mechanism is explained, again, in Figure 5 where the spreading at the ridge pushes the plates apart against resistance by continents and to some extent by friction.

I refer to several of Don L. Anderson's papers, available on his website, for the need of compression to hold the plates together. For example in his paper "Large Igneous Provinces and the Lithosphere" (2005), in *Elements:* 1 (2005) 271-275, he states at the outset "tectonic plates stay together for a reason; they are strong because they are held together by lateral compression; a cathedral without buttresses to keep its rocks under compression is *ex cathedra."* On the cover page of the paper he shows a picture of the dome of St. Peters Bassilica. In many of his papers Anderson refers to the plates as non-rigid and held together by compression. The standard explanation is that they are very rigid. I heartily agree that the plates are not rigid, because this is how it works on ice-covered lakes even though the ice may seem to be more "rigid" than the Earth's plates; they can still change shape, re-orient, buckle move and can be pulled apart causing areas of open water.

Rate of Seafloor Spreading

In the EC mechanism the rate of sea-floor spreading is explained by the varying rate of sedimentation in the ocean (see Appendix II). The rate of spreading at mid-ocean ridges is indicated by the age of the crust near ridges (*c.f.* many diagrams of the age of the ocean bottom by searching on the internet). The most rapid spreading rate is in the East Pacific Rise west of South America where the sedimentation rate is low. I will touch on the rate of spreading at this ridge later when discussing the "top-down" tectonics of Don Anderson where EC and gravity forces might be combined. The rate of spreading is low near the Equatorial Atlantic where sedimentation rates are high, possibly due to the large rivers entering that area. The sedimentation rate to the North or South of the Equator, in the Atlantic, is lower than at the Equator but probably higher than in the South Pacific near the East Pacific Rise. Deep sediments slow or block the EC mechanism much as deep snow on lakes blocks the ice push from working because it insulates the ice (or crust near the ridges in the case of the Earth) from the diurnal changes in temperature (climatic variation in the case of the crust). Again, the reader may refer to Appendix II where a crude calculation is made to show that the spreading rate may be due to variation in sedimentation rate and therefore in sediment depth.

Heat Flow from the Ocean Crust

As can be seen from many maps on the Internet the heat flow is highest where sedimentation rate is lowest as with the case of seafloor spreading. I attribute this, at least in part, to the insulation of the crust by the sediments, or by oceanic and continental crusts. Note, again, that low heat flow rates through the crust near the ridges are in the Equatorial Atlantic where spreading rates are low, but also in some places around Antarctica south of Africa and in parts of the Indian Ocean. Of course, sediment depth may not be the only cause of variation in heat flow.

Triple Points

Triple points occur in many places with regard to the oceanic ridges as can be seen in world-wide maps such as the map of the ocean bottom as shown on the back cover of this book. Notable triple points are in the East Pacific Rise west of South America (two of them), Southwest of Africa, East of Madagascar, and East of Somalia and so on. This would imply that the mantle convection patterns sharply change direction at these points. Such a change in the direction of convection currents in the mantle is difficult to explain by the mantle convection mechanism. I have read several papers that attempt to do this (*c.f.* McKenzie and Parker, 1967). They are based upon the idea that mantle plumes cause these junctions, but they are unconvincing to me as they do not consider the change in direction of the convection force, if it exists. All have very complicated assumptions and I think that this again casts doubt on the mantle convection idea. I note that such triple points are common on ice covered lakes as mentioned with regard to one on Lake George in Figure 2 and on Lake Mendota shown in Figure 3. I have no simple explanation as to why they occur on lakes except that forces due to the shape of the lake cause differential ice push so that the ice buckles in different places near the center of lakes. Perhaps the same explanation can be used to explain triple points of ocean ridges. I note that compression holds the plates together and when plates collide the resistance forces them to change shape, break into smaller plates and divide possibly to form the triple points on many ridges.

Formation of Mountains

The original reason that I came to believe in the Expansion-Contraction concept was from viewing what happens on ice covered lakes. I thought that compression, like that which produces pressure ridges and ice ramparts on ice-covered lakes, was needed to produce the push and lifting in the formation of mountains. After looking at the cause of mountains such as the Alps and Himalayas it was not clear to me that there was enough force from the existing ideas on ridge push and slab pull to cause the lifting of these mountains. I still don't believe that the ridge push force of mechanisms like the Gravity Flow hypothesis (see Chapter 4) was possible, because I could not find a mechanism to produce the needed elevation of ridges to cause this force. The EC mechanism produces ridge push force in the ridge between Antarctica and Africa that might be the cause of Africa moving northward into Europe to form the Alps. The ridge between Africa and India might do the same in the case of the Himalayas. As I discuss in Chapter 7 there might be some question that the expansion push of ridges by EC is strong enough, especially for short climate variations, because the thermal pulse for such periods may not be deep enough to push the plates apart as in Figure 5. The problem may be in the possible lack of strength of the upper oceanic crust. But as I try to counter this argument small forces acting over long periods of time, like the flow of glaciers, can cause the movement of large masses of the Earth.

Chapter 6
Mechanisms Combined and the Eclogite Cycle

Anderson's "Normal Science" and Anomalies to Convection

Scientists like Don L. Anderson and his colleagues are producing new anomalies, through normal science, and a new paradigm will result. I have called this Anderson's "top-down" tectonics. My first encounter with Anderson's thinking was in 1987 when I read his paper "The Earth as a Planet: Paradigms and Paradoxes" (1984). I was impressed with the title with its reference to Kuhn's paradigms and also with the quote preceding the text. The quote is from the Life and Letters of Thomas Henry Huxley, a strong supporter of Darwin's Theory of Evolution (often called "Darwin's Bulldog") and reads:

"Sit down before fact as a little child. Be prepared to give up every preconceived notion, follow humbly wherever nature leads or you shall learn nothing."

Because I was prone to use quotations, in my writing and teaching, as can be gathered from the opening of this book, I was curious to read more of Anderson's ideas. At the time it was not clear to me that Anderson was heading toward the idea of top-down tectonics, but the paper did provide me with ideas on how the elevation of continents via geoid highs, such as under the "super-continent" Pangaea, could lead to their break-up and to suspect ideas related to mantle convection.

After the unsuccessful attempt by our seminar group to publish the idea of the EC mechanism in 1989, I put it aside until recently after a talk with one of the original authors, Greg Hakim, who thought that it may be time to bring up the idea once more, but on a website (and I decided later, also in this book). After delving more into the libraries I then came across more of Don Anderson's papers and through his website was able to print out, and read, a large number of them. I commend him for making his work available to the public. I have read some dozen or more papers of Anderson's recent work and even though I am not familiar with all of the geochemistry and geodynamics he discusses it made sense to me that he was coming to the same conclusion as we did in 1989. It is the plates themselves that govern their movement, even though forces we proposed were different. Here are some of the Anderson papers I enjoyed the most:

In 2001 "Top-down tectonics" *Science* 293 (2001): 2016-2018. Anderson introduces the idea that plate motion is governed by the plates and not the mantle. He views plate tectonics as a "far from equilibrium dissipative and self-organizing system that takes matter and energy from the mantle and converts it to mechanical forces (ridge push, slab pull) which drives the plates" (see also Anderson's book *New Theory of the Earth*). As mentioned earlier this is not far from the idea that the movement of plates is governed by forces that come from *Gaia* and *Oceanus* (the EC mechanism) in that they both do not require deep mantle convection to cause the plates to move.

In 2002, "Occam's Razor; Simplicity, Complexity and Global Geodynamics." *Proceedings of the American Philosophical Society*, Vol. 146, No 1 (2002): 56-76. How can a scientist in the middle of the "normal science" in his field publish on such philosophical ideas as Occam's razor? Not many do, but I applaud his effort. It is a very good paper.

In 2004, "Plate Tectonics; The General Theory: The Complex Earth is Simpler than you think", *Geological Society of America Special Paper 413 (in press in 2004)*. I feel that this is Anderson's best statement of the idea of top-down tectonics. It is also an articulate discussion of how difficult it is to overcome conventional wisdom. In this paper he deals with the nature of science with discussions of simplicity, Occam's razor and a great amount of the evidence in favor of the plates driving themselves rather than being driven by mantle convection. It is a must read for anyone interested in the mechanism of plate tectonics.

Also in 2004, “Plates and Plumes.” by Don L. Anderson and James H. Natland. Submitted to *Nature* but rejected. This paper seems to me to be a rejection of the plume hypothesis as being too complicated to pass criteria that it can be falsified as in Karl Popper's ideas. It is a great paper and should have been published!

“Plates, Plumes and Paradigms,” *Geological. Society of America Special Paper 388* (2005). Earlier I mentioned this huge volume on the plume vs. the plate hypotheses. The gist of this paper, one of many of Anderson's contributions to the volume, is also contained in a chapter in his book "New Theory of the Earth." It explains to me that some of the eclogite that forms in subduction in the deep sea trenches can remain in the upper mantle to provide fertile material for new crust at mid-ocean ridges.

Whether or not the EC mechanism will be included as part of a change in the conventional wisdom on the mechanism of plate tectonics I think that Anderson's ideas will.

The Gravity Flow Forces Combined with EC in the Eclogite Cycle

To wrap up the discussion of how the EC mechanism may play a role in moving the Earth's plates, I present a discussion on how the forces of the gravity flow arguments, that Cox and Hart (1986) and others discuss, might be combined with the EC ridge-push forces to propose that this might produce the forces needed to explain plate motion.

Let me start with a diagram in Figure 15, discussed earlier in Chapter 4 as the Gravity Flow GF mechanism. The schematic shows an oceanic ridge to the west of a continent (such as South America) and the convergence of the oceanic Nazca plate with the South American continental plate in the trench west of that continent (the diagram faces to the north). The largest force is called "slab pull" where the slab of lithosphere, crust and sediments is pulled down by gravity because it is cooler than the mantle into which it is sinking. In Chapter 4, I mentioned that I suspected that this force is the largest, because it seems to me that it would produce tension on the subducting plate causing it to break apart, but, in any case it seems to me to be a real force. The "ridge push" force at the oceanic ridge (the East Pacific Rise in this case) is caused by the pull of gravity of the crust, lithosphere and sediments flowing away from the elevated ridge. This is likened to the flow of a glacier downhill. The other force that could cause plate motion is the "trench suction" force. I am very skeptical about such a "force" because it could only happen if the slab sunk down vertically to make room for the continent (South America in the case suggested) to fill in the "void." I have read some papers that illustrate that slab pull is caused by a localized convection cell beneath the oceanic crust, but do not find that this idea will pass the Occam's razor test! It is difficult for me to see that slab pull could cause South America to "over-ride" the oceanic plate (Nazca Plate in this case) without a significant force from the east (Mid-Atlantic Ridge) especially if there is "colliding resistance" between the plates, as shown in the diagram, and if the slab that is sinking is in contact with the bottom of the continent. The force from the east can only be explained by an elevation of the Mid-Atlantic Ridge and as discussed in Chapter 4 it is hard to see what causes this elevation, because it is most likely that the asthenosphere beneath the ridge is cooler that on either side.

In Figure 16 (not to scale) I show both the Mid-Atlantic Ridge (MAR) to the right and east of South America, the East Pacific Rise to the west and the trench west of South America where a slab of the Nazca plate is being subducted under South America (again looking to the north). If the GF and EC forces are combined the ridge push force of the GF mechanism, if it exists, is added to the ridge push force of the EC mechanism caused by the spreading at the ridge as in Figure 5. The same two forces are active at the East Pacific Rise to the left (west) in the diagram. In addition, the slab pull force of the GF mechanism is pulling oceanic crust away from the East Pacific Rise (EPR). This additional force may partly explain why the sea-floor spreading of the EPR is the fastest of any ridge on Earth. Note that the subduction to the west of the EPR, such as in the Tonga trench, would have the same effect as the subduction under South America to help cause the EPR to be a fast spreading ridge.

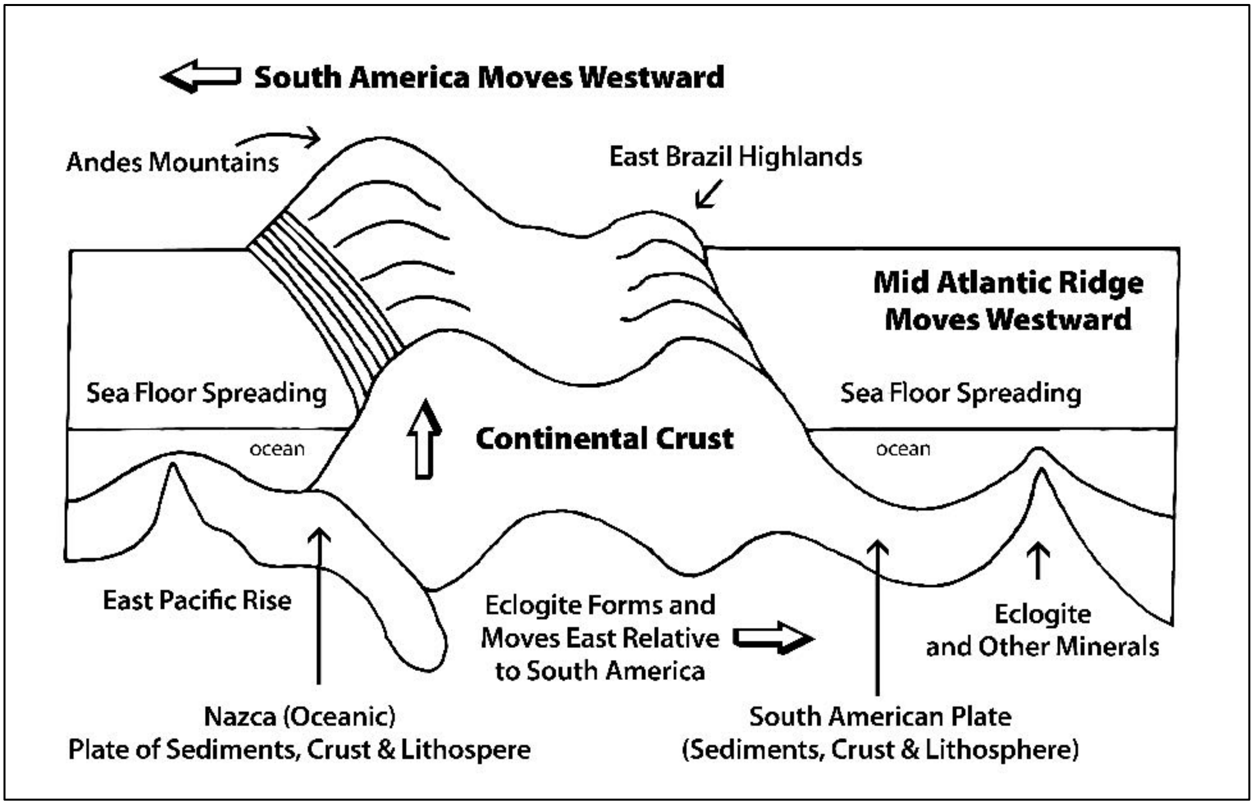

Figure 16: How a combination of the gravity flow (GF) and EC forces may cause plate motion in the region of South America

In his *New Theory of the Earth* (2007) Anderson describes the eclogite cycle (Chapter 5). In Figure 16, I attempt to show how his ideas might work if combined with the EC forces. Eclogite (and other minerals) are formed by the sinking of the slab under the continents, South America in this case. The sediments, including water, on top of the oceanic crusts, the crusts themselves, and the lithosphere are pulled down (or pushed by the ridge push force) into the mantle. Additionally, Anderson describes the delamination process where the subducting slab scrapes off part of the bottom of the continent, under the Andes by way of the example shown here. This adds some continental crust into the eclogite mix which, as Anderson describes, has a variable density. The various eclogite constituents sink into the mantle to a level of their density after being heated and melted by the mantle and the continent moves over the area. Much of the subduction material remains in the upper mantle and can then provide fertile magma under the mid-ocean ridges. In the EC mechanism reduced pressure from upwarping causes melting of minerals (above their solidus) that rise under ridges to provide material for seafloor spreading as discussed in Chapter 1 and Figure 6.

Chapter 7
Some Possible Problems with all of the Mechanisms

Six mechanisms (one can also call them "operational definitions") for producing plate motion considered here are: (1) mantle convection (CC), (2) mantle plumes (MP), (3) gravity flow (GF), (4) the supercontinental cycle (SC), (5) subduction alone (S) and (6) crustal expansion and contraction (EC). I believe that in all cases there are possibilities of flaws in the thinking and assumptions. Perhaps some of these cannot be resolved. The mechanisms are discussed in Chapters 1 (EC) and 4 (others) in more detail.

First, it seems to me that the convection current mechanism (CC) is the most unreasonable as it does not pass the Occam's razor test that it can explain, parsimoniously, most of the observations considered in Chapter 2 including migration of mid ocean ridges (I think the most serious anomaly), variation in the elevation of ridges, variation in the rate of seafloor spreading and existence of triple points in mid-ocean ridges, without a lot of assumptions. In particular I can find no reference in the literature regarding how the convection cells can move, that is, get larger, so that up-welling regions are always directly under the mid-ocean ridges associated with continental plates. Also, as Anderson (2007) points out, it is most likely that deep convection requires that the mantle be relatively homogeneous. Anderson and many of his colleagues, and Hamilton (2007) point out from tomographic evidence, that the subducting slabs associated with the trenches of oceanic plates, most likely do not sink deep into the mantle as most convection models assume. Rather, they posit that much of the subductiing slabs and eclogite that forms in these regions sink to levels above or near the 670 Km discontinuity, but not always below that. The various forms of eclogite can be recycled to form new crust in the seafloor spreading at mid ocean ridges.

The problem with the plume hypothesis (MP) is that three, or even many more plumes, cannot explain most of the key observations. The plumes are just not in a geographical position, for example, to produce sea-floor spreading, variations in spreading rate, migration of ridges and many other observations considered in Chapter 2. That plumes exist is not the argument even though there is debate about how deep the plumes begin (at the core or at layers in the upper mantle). See Anderson and King (2014) for more on this problem. Furthermore, such plumes would have to migrate so that they are exactly under the ridges which also migrate. As in the case of the CC mechanism the plume would be "decoupled" from the ridge pattern.

With regard to the gravity flow (GF) mechanism I pose that it is difficult to explain that the ridges are elevated some one to three kilometers above the abyssal plains on either side of the ridges. This elevation is needed to explain the ridge push force. Some pose that heating beneath the ridges causes this elevation (as in the CC hypothesis). But why doesn't the higher heat loss that occurs in the thin ocean crust near the ridges cause the upper mantle there to be cooler than under the continents and lithosphere where heat loss is reduced because of insulation? I also suggest that if the slab pull force is the largest of the forces of this mechanism it then becomes difficult to believe that the slabs do not break as they pull away from the ridges associated with oceanic plates. To my knowledge, there is no evidence that they do break.

I find that the subduction hypothesis (S) has many interesting features as presented by Hamilton (2007). It utilizes the concept of hinge rollback of the subducting slabs in the trench region as the only force needed to cause plate motion. But how does the pulling of continents toward the trench regions explain that the mid-ocean ridges, in say the Atlantic, stay exactly in the middle of the ocean between two continents? The hypothesis also has difficulty explaining many of the other key observations mentioned in Chapter 2.

The super-continental cycle (SC) mechanism has difficulties similar to the other arguments presented above. There is no easy explanation of the observations of the nature of the ocean bottom listed in Chapter 2. This mechanism has many interesting facts that support it, but I cannot accept it unless it can explain the important observations such as ridge elevation, variation in spreading rate and the like.

There are at least three possible problems with the EC mechanism that were pointed out to our seminar group by response to a request for comments sent to about 40 Earth Scientists. The first is that the top of the crust at the mid-ocean ridges may be weaker than most of the crust below. Therefore, could it be crushed at the time of expansion instead of producing a compression force to move the plates, especially for the shorter periods of climatic variation? I have no answer to this possibility, but one can examine Appendix 1 to make a judgment on this. My reading of the literature is that oceanic crust is quite strong, but even small forces acting steadily over time, like glaciers "flowing" downhill might produce enough compression to move the continents and that is the opinion I take here.

Secondly, it must be pointed out again that on lake ice the expansion is in all directions due to random ice cracking, whereas it is proposed in Chapter 1 that the expansion is only perpendicular to, and near, the ocean ridges. My only answer to this is that the lithosphere grows thicker quite rapidly with distance away from the ridges preventing large cracks along the ridges (for example see Figure 5).

The third possible weakness in the EC mechanism is that temperature variation at the bottom of the ocean would not be large enough to cause the expansion and contraction of the crust. In our 1989 paper we did not believe this to be true because we found that there is much evidence of large changes in ocean bottom temperatures from a search through the literature. Some of these temperature changes occur in rather short times.

No doubt many observers will find more possible objections to the EC mechanism. Until such objections disprove the mechanism then it should be considered as a plausible alternative to the conventional wisdom. Despite Thomas Kuhn's observation on how science is conducted, disputation is not a reason to reject the EC mechanism.

Conclusions

The Expansion-Contraction (EC) mechanism proposed in this book has many positive attributes. It explains many complex features of the ocean bottom topography in a natural way. Although some of the problems mentioned in the last section, and no doubt others not mentioned, may indeed be serious it is not yet clear that they are "fatal" to the mechanism. Many, who support the conventional argument of mantle convection, will contest this idea, but they have to disprove, not dispute, the concept. Those who support the conventional wisdom must still explain how convection can change to explain the migration of ridges and the occurrence of triple points. All of the existing mechanisms require some restrictive assumptions to explain some of the key observations. Some may say that I chose these observations in a selective way to cast a positive view of our EC mechanism. That was not my intention. I chose the most obvious observations of ocean bottom features, readily shown in many textbooks and diagrams on the internet.

If the EC mechanism cannot be disproved then it may be an important addition to the idea of top-down tectonics. It will take a very long time to convince the geologic community that top-down and EC are important to the concept of plate movement. This author may be long beyond his sojourn on this planet before that happens and so will not be around to witness the event! Primarily, it is the purpose of this book to bring forth the EC mechanism to the interested laypersons and Earth Scientists who may have doubts about the existing explanations of what causes the movement of the Earth's plates. Note that I have considered plate tectonics as the present paradigm in the Earth Sciences and have chosen not to discuss other possible mechanisms of how the Earth's surface works such as the idea that the Earth is expanding. I leave it to the reader to examine other mechanisms.

The EC mechanism has a distinct advantage as it is the only one so far proposed that explains plate motion involving forces other than those that originate in the mantle or the plates themselves. I propose that *Gaia*, in causing the large variations of atmospheric temperature by positive feedback mechanisms, is the important factor that eventually results in plate motion. *Gaia* acting through her son *Oceanus*, changes the temperature of the ocean bottom water near ridges resulting in seafloor spreading. If I am correct then Lovelock's Geophysiology may be invoked to explain plate motion, probably to a large extent, through what I call biomagnification of the weak signals of climate change such as those produced by the Milankovitch periodicities or other shorter variations in the climate signal.

From the discussion in this book, I conclude that all six of the mechanisms considered herein used to explain the forces that drive the plates on Earth have some possible inconsistencies. However, if we rule out the conventional wisdom that deep mantle convection causes the plates to move, and some other convective approaches, it may be possible to combine the EC and gravity flow (GF) mechanisms as shown in Figure 16. This would be consistent with the idea of "top-down tectonics" suggested by Don Anderson (2001). If these arguments hold then the mantle movement (I call it "forced convection") is driven by the plates, not from mantle heat. Anderson states that this is viewed as "an open, far from-equilibrium, dissipative and self-organizing system that takes matter and energy from the mantle and converts it into mechanical forces (ridge push, slab pull) which drives the plates." It is the plates that drive tectonics, not the mantle. This is not to say that there may not be motions in the mantle driven by density differences such as plumes, as many suggest. Rather, I conclude that they do not drive the motions of the plates even though they may be important sources of some surface manifestations such as island chains and the like. Only time will tell if the suggestions and speculations of this book are realized because it takes a very long time to change major thought processes in science. My speculations will be accepted only if there are no proven "fatal" flaws in my EC arguments.

References

There are many papers quoted in Scott (2013), the link at the beginning of the website tectonicsdrivenbyclimvariation.com, and the interested reader should download that to obtain many of the references that led to the original unpublished paper by Scott *et al* (1989). There is also a list of references of those quoted in the Appendix that follow that section.

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Appendices

The discussion so far in this book is intentionally simple and involves approaches devoted to how science works. It is directed to the layperson interested in science. In these Appendices I offer some ideas that may not interest the layperson, because they may be difficult to follow. They are (I) the depth that the pulses of temperature might penetrate from the top of the ocean crust near the ridges; (II) how the EC mechanism may explain the variation in spreading rate governed mostly by the rate of sedimentation falling near these ridges and (III) a suggested proposal on how the supercontinent cycle might work assuming that the EC mechanism is correct.

The result of the calculation of thermal pulses (Appendix I) is relevant to the discussion in Chapter 1 regarding heat exchange shown in Figure 11 as to how deep the temperature may change near the top of the crust. It is also relevant to discussion in Chapter 7 on a possible problem with the EC mechanism regarding the strength of the top of the ocean crust. The variation in spreading rates at ridges (Appendix II) has not been undertaken for any of the other mechanisms for which I am familiar. This crude approximation is used only to show that, if the EC mechanism is viable, it can produce the variation in spreading rates due to the geographical variation of sedimentation in the oceans. My illustration of the supercontinent cycle (Appendix III) is based upon the idea that if the EC mechanism actually works then the plate movement will not be stopped by anything less than collision of continents to form new supercontinents far away from one such as Pangaea.

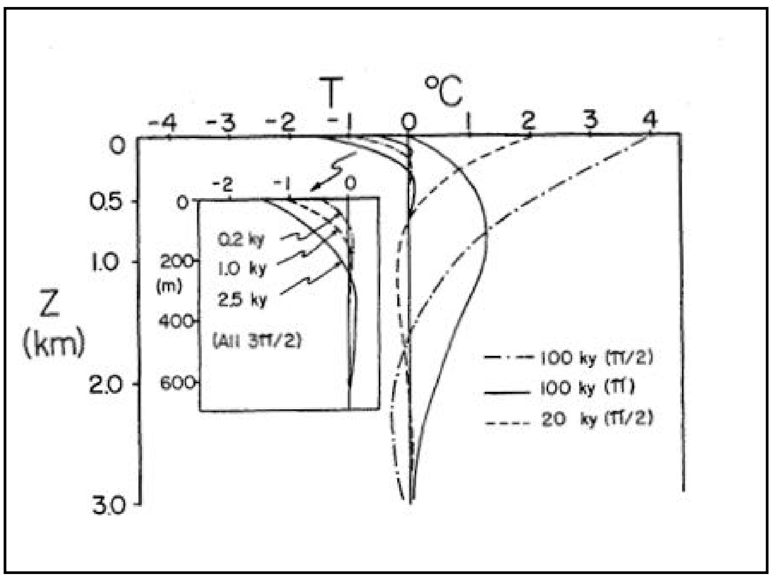
Appendix I

Thermal Pulses in the Crust

One criticism of the EC mechanism is that the pulses of temperature change are not deep enough to allow the expansion at the ridges to push the plates away from the ridge as illustrated in Figure 5. To illustrate this possible problem the profile of temperature with depth is calculated for a variety of periods of climatic variation. The temperature profile in the ocean crust near the ridges is obtained from the one-dimensional heat conduction equation given by Ingersoll *et al* (1954),

$$T = T_{rc}\, e^{-z\,(\omega/2K)^{1/2}} .\ \sin\left(\omega t - z(\omega/2K\right)^{1/2}$$

where T is the temperature at depth z, for given values of the temperature range at the top of the crust (T_{rc}), frequency ($\omega = \pi/P$), period P, diffusivity of the crust K and time t. Using a diffusivity of 1×10^{-6} m^2/s for basalt (Ingersoll, *et al* (1954) the profiles of T are shown in Appendix Figure 1 for periods of 0.2, 1.0, 2.5, 20 and 100 Ky (thousands of years) with the appropriate value of T_{rc} being merely an educated guess, because they cannot be well-known. The temperature ranges for the 20 and 100 Ky may be larger than many physical oceanographers would accept, but they are used mainly to illustrate the depth to which the thermal pulses travel. That depth depends only on the period (P) and diffusivity and not on the T_{rc}.



Appendix I Figure 1: Thermal pulses in oceanic crust near an ocean ridge for periods of 100, 20, 2.5, 1.0, and 0.2 Ky with (T_{rc}) ranges of 4, 2, 1.5, 1.0, and 0.5°C respectively.

Appendix II
The Effect of Sedimentation Rate on Seafloor spreading

The cause of variation in seafloor spreading rate is not easily explained in the existing models of plate motion as discussed in Chapters 4 and 7 and also by Forsyth and Uyeda (1975) and Cox and Hart (1986). For example, why is spreading rate very high in the East Pacific Rise, between two oceanic plates, and in the Southern Indian Ocean between two continental plates? The thought process suggests that if the EC and Gravity forces are added together then the subduction in the East Pacific may aid in the speed of spreading in the South Pacific (East Pacific Rise) with the aid of the ridge push force of the EC mechanism.

But why is the spreading faster in the mid-ocean ridge of the South Atlantic than it is in the ridge of the Atlantic near the Equator between Africa and South America? These are ridges between two continental plates. These observations are not adequately explained in the current mechanisms, and after a diligent search, I find no attempts are made to explain them. In this section of the Appendix an attempt is made to show that these observations can be explained using the EC mechanism due to the regional variation in the sedimentation rate, and therefore in the sediment depth. On ice-covered lakes the presence of snow reduces the growth in area of ice due to the insulation of the ice from the air above (Zumberge and Wilson, 1953). Similarly, sediments above the crust near the oceanic ridges will insulate the crust from ocean temperature changes above the ridge and variation in the sedimentation rates could lead to a regional variation in rates of seafloor spreading.

The sediments on the ocean floor are unconsolidated and cannot participate in the seafloor spreading process assuming the EC mechanism is correct. They insulate the crust to reduce the thermal pulse near the top of the ridge area where I have presumed that the EC mechanism operates. In this discussion I have not attempted to update the research on sedimentation rates and depths that our seminar group found in 1989, because the calculation is mostly for illustration purposes, to show only that a mechanism exists (EC) to explain a variation in spreading rate. It is not an attempt to calculate the actual spreading rates that exist. That would be too complicated and would require many assumptions as to how changes in climate produce changes in the ocean bottom temperature near the ridges.

The sediment depth varies from near zero on sections of the ocean ridges to as much as 1000 m near the continental margins of some basins, but it rarely exceeds 500 m (various sources in Hill, 1963; Fairbridge, 1966), Near-bottom currents can be quite strong (Fairbridge, 1966, p 576; Armi and Millard, 1976; Hendry, 1985) and therefore erosion from the exposed hills near the ridges can lead to re-suspension and deposition in valleys that are removed from strong currents that may exist above the ridges (*c.f.* Berggren and Hollister, 1977; Johnson,1983; Hollister and McCave, 1984; Richardson *et al* 1987). The highly variable sedimentation rate, perhaps caused by re-suspension, is shown by data of Ku *et al* (1968) who find rates that vary by several fold within short distances.

The very loose sediments near the ocean-sediment interface have porosities of 50-80% and are therefore largely water. At sediment depths of 50-70 m the porosity is typically 30-40% and porosity decreases to 20-30% below 200 m (Chillinger, 1958; Nafe and Drake, 1963). Non-moving ocean water has a diffusivity of about 1.4 x 10^{-7} m^2/s and sediment material about 7 x 10^{-7} m^2/s. Thus, the thermal diffusivity of the sediment will increase from about 2 x 10^{-7} m^2/s to about 5 x 10^{-7}

m^2/s at about 100-200 meters below the ocean bottom (*c.f.* Bullard, 1963; Nafe and Drake, 1963) and it should not vary much from 5 x 10^{-7} m^2/s below that depth. The low diffusivity layer with high porosity near the water-sediment interface may not exist in some regions where bottom ocean currents are strong, as previously discussed; therefore, packing down of the sediments is possible.

As mentioned above the calculation of spreading rates is problematic due to the high variation in sediment depth and diffusivity and is only exemplary. An "ideal" ocean is assumed that has constant sedimentation and spreading rates near the ocean ridges. This would produce a linear increase of sediment depth away from a ridge obtained from the relation,

$$D_s = L(S/\tfrac{1}{2}R) = 2L(S/R), \qquad \text{(A1)}$$

where D_s is sediment depth at distance L from a ridge, S is sedimentation rate and ½R is half the long-term mean spreading rate (R for one side of a ridge) which is assumed to be constant over time for a given region.

It is assumed, further, that the sediment diffusivity does not vary with depth and that the temperature range at the ocean bottom (T_{rb}) can be determined for a variety of climatically-forced periodicities such as those shown in Figure 9. An estimate of the temperature range at the sediment-crust interface (T_{rc}) can then be obtained from the one-dimensional heat conduction equation (Ingersoll *et al* , 1954) given by,

$$T_{rc} = T_{rb}\, e^{-D_s (\pi/K_s P)^{1/2}} \qquad \text{(A2)}$$

where the term (π/K_sP) is under the square root, D_s is the sediment depth from equation (A1), K_s is the sediment diffusivity and P is the period of climatically-forced variation of T_{rb}. This equation does not hold strictly for the condition of varying diffusivity with sediment depth that changes at the sediment-depth interface (Lettau, 1984). However, because the crust diffusivity (K_c) is much larger than K_s it is assumed that the thermal pulse at the sediment-crust interface is not altered strongly. A crude estimate of T_{rc} is warranted because T_{rb} is based only on speculation at this point. In the calculation (Appendix Table 1) a diffusivity of 3 X 10^{-7} m^2/s is assumed.

The range of temperature at the sediment-crust interface (T_{rc}) is then used to calculate E_{pn}, the amount of spreading over one period of temperature fluctuation,

$$E_{pn} = \alpha F(T_{rc}) \qquad \text{(A3)}$$

where F is the fetch (including both sides of a ridge) or 1000 Km for each 500 Km additional segment of crust, and α is the one-dimensional coefficient of thermal expansion taken to be 7 x 10^{-6} per $^{o}C^{1}$ based upon data from Skinner (1966). Note that the total fetch for each increase in segment is always 1000 Km because the increment of 500 Km must be multiplied by two (one increment for each side of the ridge). The longer the period the more 500 Km increments is included in the calculation.

It is not possible to determine with any certainty the total fetch that might be involved in the EC process. For short periods F is determined by the sediment depth which damps the thermal pulse at the top of the crust. When T_{rc} of equation A2 is below 0.1 ^{o}C it is assumed that EC no longer works and thus F is determined by sediments which insulate the crust. For long periods F is arbitrarily limited to 4000 Km on each side of a ridge, or 8000 Km total. This may be large in the opinion of some physical oceanographers but reducing the fetch does not significantly affect the calculation of spreading rate.

Finally, spreading rate for individual periodicities (P) is obtained from,

$R_p = \Sigma E_{pn}/P$ (A4)

The calculation is stepped through equations A1 through A4 for incremental distances of 500 Km from a ridge. That is, D_s, T_{rc}, E_{pn} and R_c are obtained for distances of 250, 750 Km etc. which are assumed to apply to each 500 Km segment of the crust. Total E_p and R_p are the sums of the incremental values for each period and are shown in Table A1.

The sedimentation rates of 2 and 4 m/My for equation A1 are estimated from Ku, *et al* (1968) for South Pacific and South Atlantic central ocean basins, respectively. They obtained values as low as 0.6 m/My near the ridges, but the data used here are averages excluding stations near rivers and those from the Antarctic region where sedimentation rates are fairly high due to biogenic sediments.

The calculation in Appendix Table 1 illustrates conditions for the region of the East Pacific Rise west of South America. Assuming that the data for T_{rb} are not unreasonable then the total ΣR is in good agreement with the observed spreading rate in that region of about 10 cm/y. The largest total crustal displacements for a given periodicity (E_p) are for long periods, but the fastest spreading rates (R_p) occur for intermediate and short-period climatic fluctuations. When the long term spreading rate (½R) is halved and the sedimentation rate is doubled the conditions for a South Atlantic ridge are approximated. This produces spreading rates for shorter periodicities that are strongly reduced by the sediment damping of T_{rc}. For the South Atlantic spreading rates are obtained that are slightly less than half of those calculated for the East Pacific Rise, or about 5 cm/y.

The effect of each of the independent variables in Equations A1 through A4 is shown in Appendix Table 2. An arbitrary base case is used with a period of 2.5 Ky, ocean bottom T_{rc} of 2 ^oC, sedimentation rate of 2 m/My, half spreading of 5 cm/y, diffusivity of 3 x 10^{-7} m^2/s and fetch (F) of 7000 Km. Each variable is changed as shown in Appendix Table 1b.

The total spreading (E_p) over one period increases when the P increases to 20 Ky but increasing P also reduces R_p . This decrease in R_p occurs because of the division of E_c by P in Equation A4. Halving T_{rb} also halves E_p and R_p because T_{rc} is a linear function of T_{rb}. Doubling the sedimentation rate (S) from 2 to 4 m/My decreases R_p by a significant amount because S affects the sediment depth and T_{rc}, and therefore R_c decreases exponentially with D_s.

Halving of the long-term spreading rate (½R) has the same effect as doubling S for the same reason. Doubling the diffusivity (K_s) increases R_p in a complicated way because it appears under the radical in Equation A2. Finally, a decrease in the fetch from 7000 Km to 3000 Km decreases R_p, but by a relatively small amount because T_{rc} decreases exponentially with distance from the ridge center.

Appendix II Table 1: Results of the calculation of spreading rates using Equations A1 through A4. The total spreading is calculated for conditions that might approximate those in the South Pacific Rise west of South America. The variables are defined below the table and in the text.

P	T_{rb}	S	½R	K_s	F	E_p	R_p
100	5	2	5	3	8	240	0.24
40	3	"	"	"	"	130	0.32
20	2	"	"	"	7	75	0.4
5	"	"	"	"	6	54	1.1
2.5	2	"	"	"	5	41	1.7
1	1.5	"	"	"	5	24	2.4
0.16	0.3	"	"	"	3	2.1	1.3
0.08	0.3	"	"	"	2	1.4	1.8
						Total = (ΣR_p)	9.3

Appendix II Table 2: Effect of changing the variables used in Appendix Table 1. The variables and units are the same.

P	T_{rb}	S	½R	K_s	F	E_p	R_p
2.5	2	2	5	3	7	48	1.8
20	"	"	"	"	"	75	0.4
2.5	1	"	"	"	"	24	0.9
"	2	4	"	"	"	29	1.2
"	"	2	2.5	"	"	29	1.2
"	"	"	5	6	"	58	2.3
"	"	"	"	3	3	30	1.2

The variables are:

P = period of climatic forcing (Ky)
T_{rb} = range of ocean bottom temperature for a given P (°C)
S = sedimentation rate (m/My)
½R = half the long-term spreading rate (cm/y)
K_s = molecular diffusivity of the sediments (in 10^{-7} m^2/s)
F = fetch over which the calculation applies (10^3 Km)
E_c = total expansion rate and contraction over one period (m/y)
R_c = calculated spreading rate (cm/y) for each period
ΣR_p = total calculated spreading rate (cm/y) for the region (S. Pacific)

The main point of this exercise in Appendix Tables 1 and 2 is to illustrate that spreading rates of observed speeds can be obtained using a few of the major periodicities of climatic variation. It is <u>not</u> to determine actual spreading rates because the input data are merely guesses. These calculations will not change much with reasonable assumptions regarding the magnitude of the independent variables such as sedimentation rate, diffusivity of the sediments and range of temperature at the top of the ocean ridges. While many will disagree with the input data used here I think that this exercise at least points to the fact that the EC mechanism, if viable, can cause variable spreading rates. To my knowledge no other mechanism has a reasonable way to do this without many assumptions that are not indicated by observations. My friend Chris Walcek (pers. com. 2014) has criticized this calculation because it uses the known spreading rate (½R) to "calculate" the spreading rate! This criticism is, of course, warranted, but my defense is that I am not really trying to calculate a spreading rate. I am merely trying to show that the spreading rate in the EC mechanism varies because of varying sedimentation rate. Authors of no other mechanism have attempted to show how the spreading rate can vary.

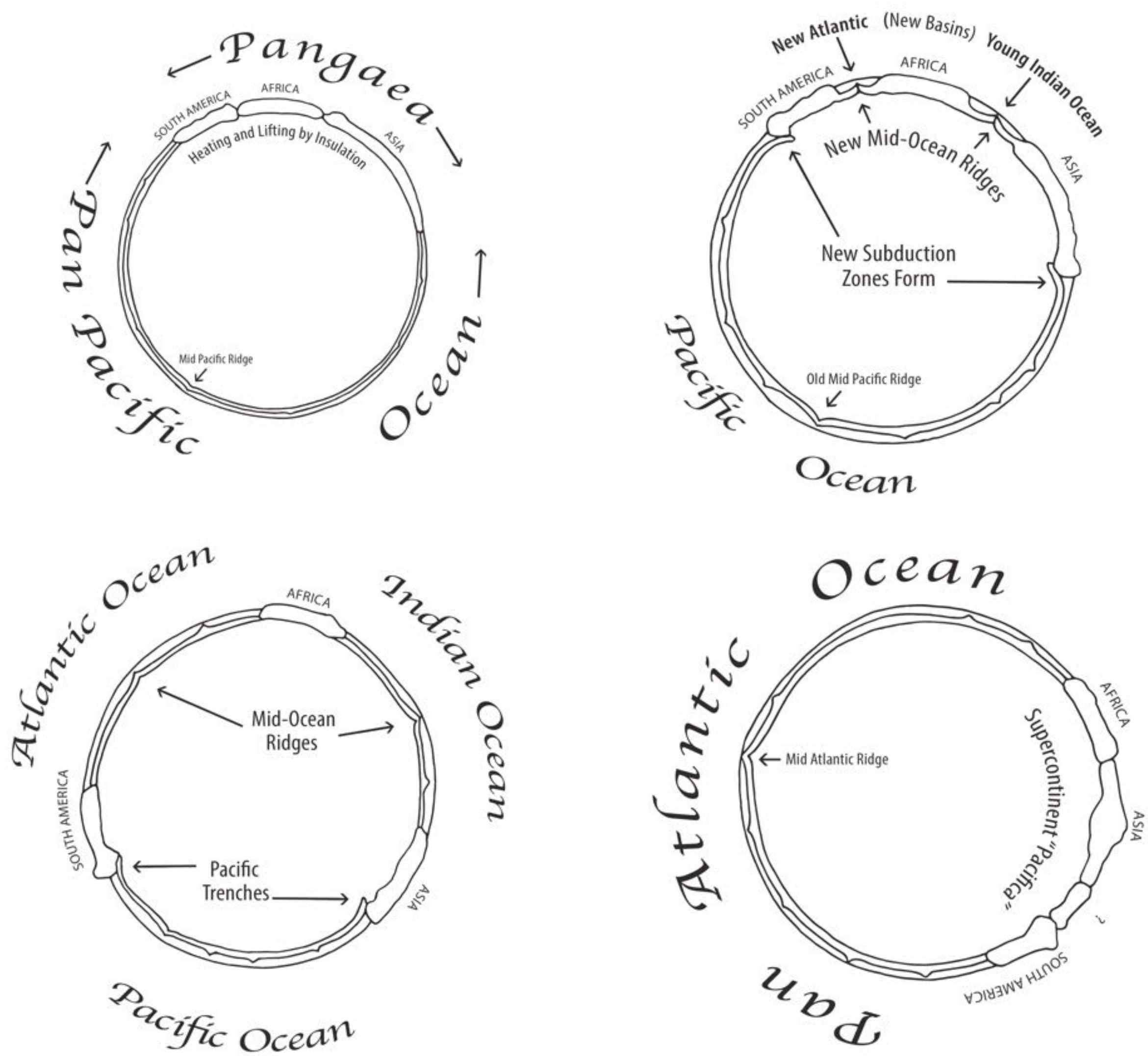
Appendix III
A Supercontinental Cycle Using the EC Mechanism

If the EC mechanism is correct then seafloor spreading at the ridges between continents should continue to move the continents and the present ridges between continental plates, like the mid-Atlantic ridge should continue to move (migrate) until something prevents them from moving and that is assumed here. It is assumed that climate variation like that shown in Figure 9, or something similar to it, continues for a long geologic time? If the continents were to collide on the side of the Earth roughly opposite to the supercontinent Pangaea that collision would stop the spreading. The Pacific Ocean is presently about 15000 Km wide from about North America to Australia. If the present Mid Atlantic Ridge and the Indian Ocean ridges keep on producing new sea floor crust and lithosphere for roughly another 200-300 million years, at a combined rate of about 5 cm/y this would close the Pacific and a new Supercontinent would form in the region that is now the Pacific Ocean.

The highly speculative scenario for this re-aggregation of a new supercontinent (call it Pacifica) is loosely sketched in Appendix Figure 2. This suggestion is different from that described by Worseley *et al* (1993) discussed in Chapter 4 (see also McElhinney and Valenco, 1981 and Walker, 1986, p. 160). It is suggested here that the continents do not reverse direction (approximately) due to change in the elevation of continents as in Worseley *et al* (1993), forming new geoid highs beneath them but they reverse due to the formation of a new supercontinent that stops the seafloor spreading that exists between the continental plates. The total time for a reversal is about the same as in Worseley *et al* (1993) if the calculation using a combined spreading that averages about 5 cm per year is correct.

Starting in Appendix Figure 2a, I speculate that the continents are being pushed together to form Pangaea by seafloor spreading from the Mid Pacific Ridge in what I term the "Pan Pacific Ocean." This is the same ridge that exists between the Pacific oceanic plates of today, but that ridge is now being covered by the "new world' continents of North and South America. Heat building up under Pangaea causes uplift to form a geoid high (Anderson, 1984).

In Appendix Figure 2b I show that the continents of Pangaea begin sliding away from the geoid high and new "young" oceans are being formed that now expand by the EC mechanism. The new continents push against the old Pan Pacific Ocean continental plates to form new subduction zones as Pangaea breaks up. The old "Mid-Pacific Ridge" still operates, but at that time is in the middle of oceanic plates. This is shown in Appendix Figure 2c and is meant to illustrate, approximately, the locations of the continents of the present day. I do not show the present trenches, such as the Tonga Trench and those near Japan and the Philippines, and the back arcs associated with them, because this is meant to be a simple schematic. Finally, the continents reaggregate in what is now the Pacific Ocean as illustrated in Appendix Figure 2d to form what I term the supercontinent "Pacifica." The total process takes about 500 million years from one supercontinent to the next.



Appendix III Figure 2: (a) upper left, (b) upper right, (c) lower left and (d) lower right.

References Quoted in the Appendices

The references quoted here are those obtained in the original paper by Scott, *et al* (1989) and have not been updated since the late 1980s. There has been a great amount of research to verify that the ocean circulation is deep and fairly strong and that the sedimentation rates discussed here have been updated by much research. I leave it up to the serious student to fill in the literature on these subjects. It is not likely that new data on sediments will alter the major conclusion of this Appendix.

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Bios

Jon Thoreau Scott attended The Ferrer Modern School devoted to the idea of "freedom in Education" from 1934-1946 (age 2 to 14). That experience may explain his unusual approach in this book. In 1954 he obtained a BS degree in Biochemistry from Cornell University and spent three years in the U. S. Air Force as a pilot and radar controller ending with a year in Alaska in 1957. In 1964 he obtained a doctorate in Meteorology from the University of Wisconsin, Madison. From 1963 to 1996 he was a member of the Department of Earth and Atmospheric Sciences (presently the Department of Atmospheric and Environmental Sciences) at the University at Albany – State University of New York (UAlbany). Jon has a wide range of research interests including the energy balance of ice-covered lakes, Langmuir circulations in small lakes, coastal currents in large lakes and the Atlantic Bight, vegetation-environment relations in the Adirondacks, the decline of Adirondack red spruce trees possibly due to acid deposition and solar energy. He participated in several national research programs including the International Biological Program and the International Field Year for the Great Lakes. Jon taught a wide variety of graduate and undergraduate courses at UAlbany. He was a Director of the Environmental Studies Program and a Chair of the Department of Atmospheric Sciences. He lives with his wife Kathleen in a passive solar home in Altamont, New York.

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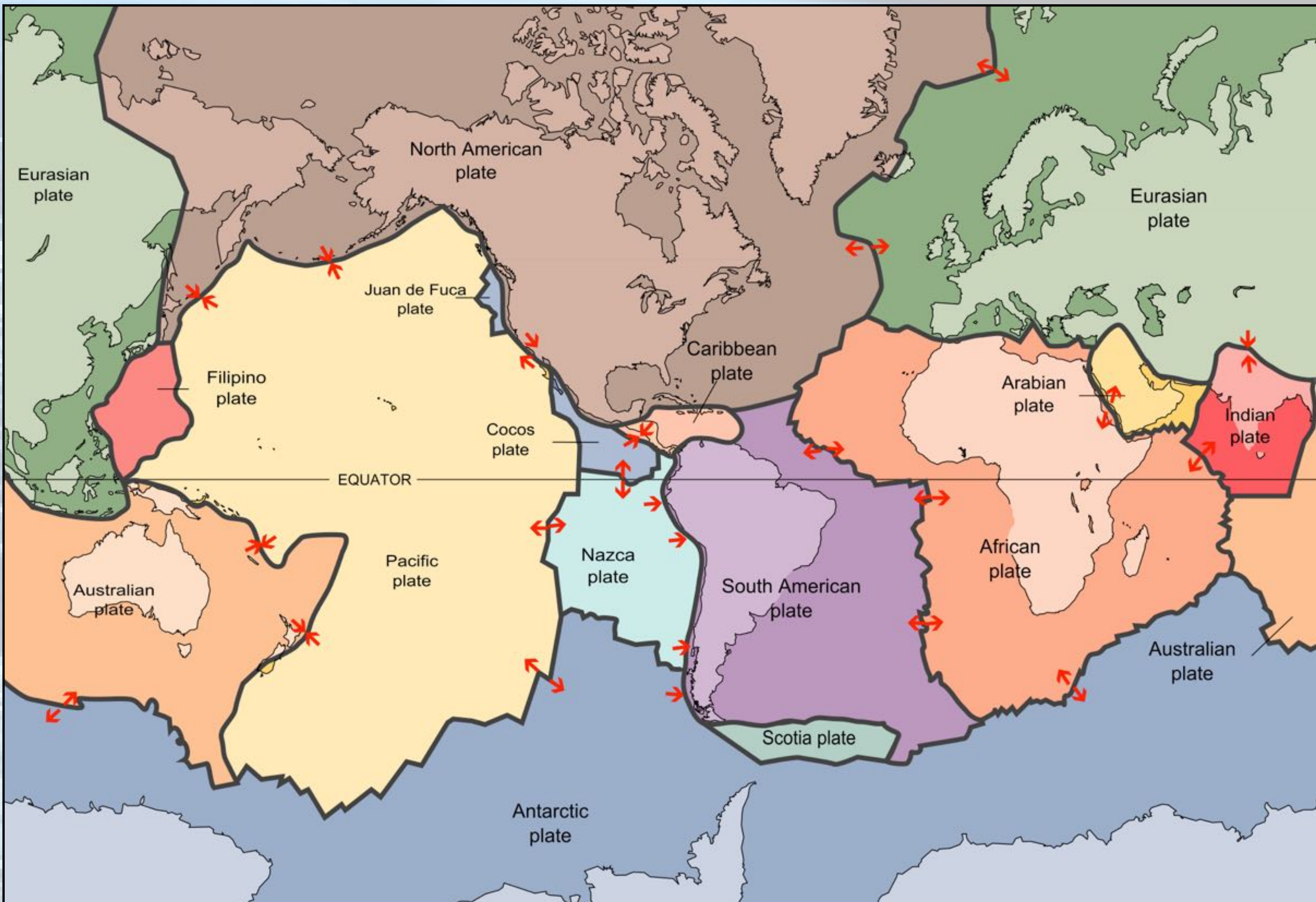

Earth is the only planet that is known to have plate tectonics. It is also the only of the four terrestrial planets that is known to have an atmosphere of mixed gases (including nitrogen, oxygen, carbon dioxide and methane), large oceans and life. The Expansion-Contraction (EC) mechanism of plate tectonics, discussed in this book, is the only mechanism that proposes that life, through the variations in climate and ocean temperature, may be partly, or even largely responsible for plate motion. The EC mechanism explains the common observations of the ocean bottom features in a natural way whereas "bottom-up" mechanisms require assumptions that amount to what Thomas Kuhn calls anomalies. Therefore, the author suggests that the EC mechanism may be combined with other "top-down" concepts of plate motion to replace the conventional wisdom that tectonics is caused by either mantle convection or mantle plumes.

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