

IN DEFENSE OF MY PAPER “On the Forces of Plate Tectonics.”

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In this reply I would like to discuss the comments by Warren Hamilton with which the Editor Irina Artemieva says she is in agreement. My purpose in this reply is to convince Dr. Artemieva to re-consider the rejection and to follow through with a review in the normal process

First, Dr. Hamilton states that I (Jon Scott) am a “*non-scientist with a keen interest in the mechanism of plate tectonics.*” The “non-scientist” part is not true (non-geologist would be correct). The “keen interest” is correct. I also attach a mini-biography of my life that shows that I have participated in organizing several national programs of research, although none of which involved plate tectonics mechanisms. My undergraduate work at Cornell was in Biochemistry with a minor in Agronomy and my PhD degree from Wisconsin (Madison) was in Meteorology with a minor in Plant Ecology. My thesis dealt with the energy balance of ice-covered lakes in cold climates (Wisconsin etc.) and that is where I got the idea that it takes a mechanism with compressive force to move the plates around the Earth. There seems to be good reason that the mechanism producing an increase in the area of ice on lakes is much like the mechanism I propose for the ocean crust. If it works in one situation, why not in another that is very similar?

I published papers on many subjects including mixing of lakes, coastal ocean and great lakes circulation, solar energy, acid deposition in forests and the distribution of plant communities as governed by the physical environment. One invited paper was published by Elsevier in the *Handbook of Vegetation Science*. I was a member of the Department of Earth and Atmospheric Science at the University at Albany, State University of NY for 34 years and the chair of that Department for six years. I hope this clarifies that I have at least done scientific research for most of my career.

Around 1987 I decided to pursue interests other than basic scientific research and became more interested in the history and philosophy of science. I had been teaching about the concept of continental drift in several courses starting in 1964 and that led me in 1987 to organize a student group to see if there was any merit to the idea that plate forces were similar to the way the area of ice grows on lakes that I term the Expansion-Contraction (EC) mechanism.

I have great respect for Warren Hamilton and have read many of his papers and his website, on the nature of the trench regions and back-arc basins, and also on his idea that the planets other than Earth do not have evidence of plate tectonics. He, like one of my Wisconsin Professors, Reid Bryson, was any early proponent of the concept of continental drift. In his 2007 paper Warren relates the mocking that he received from geologists when he defended the continental drift idea.

The most interesting argument Warren Hamilton makes, to me, is in his 2007 paper (*Geol. Soc. Am.*) in which I believe he casts legitimate doubt on the “bottom-up” mechanisms (convection and plumes) and gives his argument for his own mechanism that involves gravity as a force (the trench rollback process). He then goes on to explain his trench rollback force that I think is very realistic, but as I state, gravity forces are necessary, but not sufficient to explain completely the plate tectonics process. This agrees with Don Anderson’s (2007) definition that a ridge push force is needed (see the Introduction to my paper for Don’s definition). Without a ridge push force the gravity mechanisms, by themselves, cannot explain many of the most important observations such as how the ridges become elevated and why the ridges between continents are about halfway between the continents. On p 3 of Hamilton’s 2007 paper he states: *“The African plate is rimmed on the west, south, and east by spreading ridges that similarly lengthen and change shape as they migrate away from the continents as required to stay midway between diverging continents (emphasis mine).* What causes this “requirement”? Only a ridge push force can explain this in a physically realistic manner.

The ridge push force also explains why plates are under compression, why they migrate (the MAR moves westward with respect to Africa etc.). That is why I adhere to Don Anderson’s definition of plate tectonics which does mention that there must be a ridge push force. Unfortunately, Don and others have not come up with a ridge push force that would keep the plates in compression and thus intact. The EC mechanism does provide a ridge push force that produces compression in the plates so when combined with the gravity models the obvious observations of the ocean crust are explained.

I now wish to comment on specific statements that Warren Hamilton makes in his discussion of why he rejects my paper: The first is that my paper is *“an update*

of an unpublished 1989 manuscript. Most of the references predate that.” Yes, it is an “update” of that earlier paper, but is very different in many respects. I do use many references that were quoted in the earlier version, where they are still relevant, but I also quote much new work published in the last decade or so including some recent papers by Hamilton and many by his colleague Don Anderson. The mechanism is slightly modified from the original because it deals with temperature changes at the tops of ridges versus our original idea that the EC force applied generally to the bottom of the ocean.

Still in the second paragraph of Hamilton’s remarks the above is followed by the statement: *“It contains many misconceptions and it mentions little that is both correct and relevant to its message.”* If one is to state that there are misconceptions they should be pointed out! If one finds something that is “not correct” then what is it? Without these clarifications such comments amount to disputation and not disproof. The scientific method works best when a theory (paradigm, hypothesis, model) is subject to disproof (see Platt, *Science* 1964) using T.C. Chamberlin’s (*Jour. Geol.*, 1897) idea of the testing of multiple hypotheses. Unfortunately, our peer review system relies mostly on disputation as Dr. Hamilton has observed in his paper on Zombie Science with Don Anderson ([mantleplumes.org/zombie science](http://mantleplumes.org/zombie-science)). Disproof is rarely attempted. Such comments as those made by Hamilton above are a main reason why our peer review system does not work. See also Charlton (2008) as quoted by the aforementioned Anderson and Hamilton entry (2008) in mantleplumes.org.

I will come back to this point at the end of this reply to Dr. Artemieva.

In the third paragraph of Hamilton’s remarks he discusses how my EC mechanism works. In the first sentence, he states: *“Mr. Scott proposes a climate-controlled mechanism of alternating ridge pullapart and compression to explain seafloor spreading and the motion of oceanic lithosphere toward subduction systems.”* This is true, but he leaves out the fact that the EC mechanism also causes the growth of continental plates including the Antarctic plate that has no subduction associated with it. In fact, plates like those associated with the Mid Atlantic Ridge are not associated with large subducting slabs, only those of the Pacific Ocean plates are.

Hamilton then adds a sentence in parentheses: *“(I have long argued that the masses of plates hold them together and added by the common trenchward inclinations of seafloor and lithosphere/asthenosphere boundaries provide propulsion by forcing subducting plates toward their exits from the surface; and that the slabs sink subvertically-they do not slide down their dips-which pulls subducting plates away from ridges, “sucks” overriding plates toward rolling back hinges, and accounts for the non-crumpling of plates.)* This is the gist of Warren’s plate motion mechanism and I have said, repeatedly, that, while very reasonable, it is only part of the solution as to why plates move (Section 2.1 in my paper). A ridge push force is needed. Warren argues that the oceanic (i.e. Pacific) plate bends down at trenches and does not rub into the continental plates that it passes under. Thus, for example, the South American Plate is “sucked” to the west.

My main argument with this parenthetical sentence is the statement that “...the masses of plates holds them together.” It has long been a tenant in the geological community that the plates are “rigid.” Rather, I agree with Don Anderson’s 2005 paper that plates are like cathedrals or igloos and are held together by compression, not rigidity. In Hamilton’s argument, the South American Plate is “pulled” to the west and the “masses of the plates” holds it together. Common sense says that it would be pulled apart and this is Anderson’s point. Anderson’s statement is: “Rocks are strong in compression and weak in extension – the St. Peter Principle.” His paper is in *Elements* in 2005. It is an important statement about stresses in the crust.

After the parenthetical statement in the third paragraph Dr. Hamilton’s discussion is a very astute description of how my EC mechanism works and I have no argument with this.

The substance of Hamilton’s rejection of my paper is in the third paragraph and I have to state that his words amount only to opinion and not to substance. Unfortunately, this is how the peer review system works. Evidently disproof is not required, but disputation is allowed. Let me start with the first sentence *“Scott describes the behaviors of ridges and subduction systems as he imagines them in these terms, but makes no mention of the abundant evidence for actual behaviors, which to me precludes his mechanisms.”* This is simply not true if one reads my manuscript. I refer to such important observations as ridge elevation, ridge

migration, that the ridges between continental plates are nearly halfway between the continents and that seafloor spreading varies regionally. Are not these “actual behaviors”? No other mechanism explains these observations. A ridge push force is needed.

Hamilton adds parenthetically “(*Ridges are long continuing pullaparts with extensional structures;...*)” What “pulls” them apart? Gravity flow (or trench suction) cannot pull Africa away from South America while, at the same time, the mid-ocean ridge (i.e. MAR) is elevated. In the convection current mechanism, the pull apart idea is reasonable, but the position of the pull apart is not. The upward flow from deep in the mantle would have to move (migrate) and that is physically impossible.

In the next sentence, Hamilton adds: “*He attempts no physical analysis of his speculative thermal and mechanical suggestions which to me are extremely implausible.*” He is correct that I attempt no physical calculations of the mechanics of how EC might work in this paper; it would take many pages. I do some of this in my unpublished book *Top Down Tectonics: The Role of Oceanus and Gaia* in which I provide calculations as to just how the EC process works. But to say “which to me are extremely implausible” is merely disputation and Hamilton, co-author of a paper on Zombie Science, should realize that it is nothing more than that and that too many peer reviewers resort to such opinions. I present the EC mechanism as a proposal to be considered by geologists who are becoming more and more uncomfortable with the current ideas on plate motion and thus some young person could provide some of the interesting calculations of things like crustal strength and the cause of extension by the EC mechanism (during a cool climate, for example) and how EC might be either in accord, or not, with the observations of the trenches at the tops of oceanic ridges.

In his next sentence, Hamilton adds: “*His hypothetical small and erratic volume changes decrease downward in the upper crust, but their volume change does not operate vertically; instead being transmitted thousands of km laterally as horizontal motions of entire plates.*” This seems to be an incomplete understanding of how the EC mechanism works and I doubt if I can explain it unless I sat down with Warren and discussed EC with him. First, the volume changes are not “erratic.” They are systematic and depend on the driving forces due to

temperature variations by climatic variations such as those due to the Milankovitch periodicities. Second, the volume changes do not operate vertically, but do so horizontally. The mechanism is like a ratchet in that it works only in one direction. New crust is formed when MORB is allowed to flow upwards into the “crack” formed by the contraction of the crust at the top of a ridge. It solidifies to form new crust and the procedure cannot go backwards. When the next warm period arrives the expansion due to the warming causes seafloor spreading and pushes crust away from the ridges. Note that I say “pushes” as opposed to the original convection current mechanism, proposed by Arthur Holmes around 1933, where crust is pulled away from the ridges.

In the next sentence: *“Nothing quantitative is done with stresses, strains, temperatures, and times.* One can make the same statement about almost any proposal to cause plate motion and thus the statement makes no sense to me. It would be nice if I could know how strong the crust actually is. I doubt if it takes much force to move the lithosphere over a somewhat elastic asthenosphere. I always thought that convection below a “slippery” low velocity layer was not strong enough to cause mountains, but that is only an opinion. There is not enough friction to do this, I think.

In the next sentence, Hamilton states: *“His melting model incorporates unbowing of ridgecrests, far out of equilibrium with isostatic equilibrium...”* I do not know what “unbowing” means, but many places on Earth are, at least temporarily, not in isostatic equilibrium. That was shown to be the case many years ago especially with respect to regions of mountain formation like the Himalayas. My EC model incorporates compression against continents to cause the ridges to be forced to rise. This produces melting underneath the ridges by release of pressure. The minerals melt as the reduced pressure causes them to pass their solidus into a melted form. They can then be the MORB that produces new crust.

The last sentence includes the opinion that *“...his manuscript incorporates no awareness of either the geoscience database or of physical mechanics.”* Where have I heard this before? When wants to be disputatious one states that the author does not understand the literature (database?) or the physics. Pardon me, but that is just not the case. For example, where do I fail to understand the literature, or the

physics? What physics do I not understand? I doubt if Dr. Hamilton can state specifically where that is or he would do so. His disputations are merely Just that.

I summarize this long reply with a “plea” to re-consider the rejection of my paper. What harm could its publication do if some geologists, or geophysicists can disprove (not dispute) it?

Because Warren Hamilton is quite familiar with the way the peer review keeps “zombie science” alive (see mantleplumes.org/zombiescience) he should not resort to the same techniques that many scientists use to keep wrong ideas “alive.” In the Anderson and Hamilton zombie science paper they quote Charlton’s 2008 paper that also deals with “zombie science.” Charlton’s title is “*Zombie science: A sinister consequence of evaluating scientific theories on the basis of enlightened self-interest.*” Do I think that Dr. Hamilton rejects my paper due to self-interest so that his career will be enhanced? I think not. He, like me, is too old to think that such a procedure will enhance his career. He is about 90 years old and I am 84! Neither of us are interested in promoting our careers. Jon Scott is only interested in promoting what he thinks is the “truth” and will probably never know whether his EC model is accepted or disproved. So far the latter is not the case.

But I do think that Hamilton’s rejection, without substantial evidence (only opinion), is much like the rejection of continental drift, seafloor spreading and plate tectonics by Sir Harold Jeffreys in 1976, long after these ideas were well established. Jeffreys, one of the world’s great Geophysicists, was wrong, but as Thomas Kuhn states in his book, scientists take their ideas to their graves and ignore the new paradigm that results from a scientific revolution. We are all prone to that weakness and want to hold on to our concepts. If someone disproves my EC mechanism of a ridge push force I will probably still hang onto the idea as T.C. Chamberlin (1897) discusses, because it would be what he calls my “intellectual child” or in his other term my “Ruling Theory”! I hope that I have convinced my readers so far that I have used Chamberlin’s concept of testing multiple hypotheses. That has been my intention.