Does the catastrophic plate tectonics model assume too much uniformitarianism?

Michael J. Oard

It is my contention that the catastrophic plate tectonics model assumes too much uniformitarianism. It assumes that plate tectonics has been proved. However, the data supporting this interpretation have been misinterpreted and there are alternative ways to view the data; for instance, the data on the 'fit' of the continents, the magnetic 'anomalies,' basalts older away from the mid-ocean ridges, and the Wadati-Benioff zones. Baumgardner seems to accept microfossil dating sequences and a relative polarity time scale, both of which are dependent upon radiometric dating and manipulation of data. Two assumptions of Baumgardner's elegant computer model are questioned, namely his assumption that the Cambrian/Precambrian boundary is the Flood/pre-Flood boundary, and that the Flood resurfaced the ocean floor. Several details of his model seem problematic.

It is obvious from John Baumgardner's first submission to this forum that he believes strongly that plate tectonics has been proved. Plate tectonics was indeed a rapid paradigm shift in the 1960s. It provided a powerful stimulus for intellectuals to conform to what seemed like a bold advancement of geological knowledge. As I stated in my first contribution to this forum, hindsight shows that the paradigm shift was too hasty. Many believed without adequately examining the data. Much of the newer information collected since the 1960s, such as from deep sea drilling, are not favorable to the paradigm, despite automatic fitting of the raw data into the paradigm. Several of these data were presented in my first submission to this forum.

In my second submission to this forum, I will delve into several aspects of plate tectonics that Baumgardner thinks are so well proved, generally in the order presented in his paper. I will then question several of the assumptions and initial conditions in his model.

In his abstract, Baumgardner lists four major tectonic changes that any Flood model must incorporate. I would question the belief that the pre-Flood oceanic lithosphere was completely replaced, and that plates or 'continents' have moved over a spherical Earth for thousands of kilometres. His third tectonic change, vertical tectonics, I readily accept because the data are clear,[1,2] for instance marine fossils found in strata of most mountain ranges of the world. I am not sure of the significance of the fourth deduction of thick crustal roots below the world's high mountain ranges.

At the end of the first paragraph of Baumgardner's introduction, he states:

> ' … the prevailing uniformitarian mindset prevented the revolution from reaching its logical end, namely, that Earth had experienced a major tectonic catastrophe in its recent past.'

I believe the problem goes much deeper. The uniformitarian mindset has also *misinterpreted the evidence*, and Baumgardner seems to have accepted too many of these uniformitarian deductions.

The 'fit' of the continents and the mid-ocean ridges

Baumgardner points out the impressive 'fit' of the continents across the Atlantic Ocean as evidence of plate tectonics. Plate tectonics is only *one* interpretation. Before the paradigm change, this 'fit' was not enough to persuade scientists in favor of plate tectonics. I have already discussed this 'fit' in my first submission. I even suggested an alternative interpretation. It is important to add that the topographic 'fit' is not perfect; for instance, to make the fit Central America must be deleted, continents rotated, and Africa increased in size. Furthermore, the supposed geological and paleontological 'matches' across the Atlantic are exaggerated.[3] Besides, there are many possible fits of the continents based on topography, geology and the fossils.[4]

Baumgardner further points to the mid-ocean ridges with their higher heat flow as evidence of plate tectonics. The existence of mid-ocean ridges (which in the Pacific are not in the mid ocean) are the data, while plate tectonics is the *interpretation*. I have also briefly discussed this subject in my first submission and provided an alternative interpretation. There are many problems with the plate tectonics interpretation for mid-ocean ridges.[5] The geometry of the pattern is too precise for such a chaotic process of upwelling magma and spreading of plates; for instance, the pattern of numerous 900 offsets of the ridge and overlapping ridge segments. Another problem is that magma is *limited* below the axis of the mid-ocean ridge,[6] and the inferred magma is spread out over several hundred kilometres perpendicular to the ridge axis.[7] Moreover, volcanic eruptions are surprisingly common on the flanks of the mid-ocean ridges, while they are supposed to occur at the ridge axis.[8] The pattern of inferred magma in the subsurface and volcanism along the

ridge flanks seems more consistent with simple differential vertical tectonics with extensional fractures between the ridge and the abyssal plains.

Do magnetic anomalies prove seafloor spreading?

Magnetic anomalies or 'stripes' are seen as great evidence for plate tectonics. According to Baumgardner, they provide a relative time scale not based on radiometric dating. However, it is doubtful that this relative timescale, correlated with continental volcanism, is really independent of radiometric dating methods. William Glen states:

> 'Potassium-argon dating of young rocks was the key to the development of the polarity-reversal time scale, just as the scale was the key to the confirmation of seafloor spreading.'[9]

The volcanic samples used to construct the polarity time scale for the first 4 million years of geological time were collected from various places on the Earth and dated by the K-Ar method.[9] Of course, the fossils are made to agree with the time scale, so fossil sequences also are not independent of radiometric dating methods:

> 'Therefore Savage provided advice and later, in 1964, coauthored an important paper with Evernden, Curtis, and Gideon T. James that demonstrated essentially perfect congruence between K-Ar ages and the mammalian age designations of biostratigraphers.'[10]

Index fossil dating is related to K-Ar dating and paleomagnetism in what I believe is a powerful reinforcement syndrome—a type of circular reasoning,[11] It is not hard to find juggling of radiometric and fossil dates into *agreement*.[12,13] Baumgardner seems to accept microfossil sequences on the ocean floor and on the continental shelves as a fact:

> 'These microfossils ranged in stratigraphic affinity from lower Cretaceous to late Pleistocene, with stratigraphic age of the fossils just above basaltic basement increasing progressively with distance from the ridge axis.'

Why should a creationist accept these microfossil sequences that are based on evolution, uniformitarianism, and radiometric dating? Tammy Tosk[14] pointed out some of the many problems, assumptions, and manipulations in the foraminifera microfossil sequence.

There are numerous problems with interpreting magnetic anomalies (stripes) as evidence of plate tectonics.[15] The stripes are not really alternating reversed and normal magnetism, but are only slight changes in magnetic *intensity* that are *inferred* to be a series of reversals. It was once assumed that the magnetism of the ocean floor would be in the top 600 m of basalt. However, deep drilling showed that this is not true.[16] There still are many questions associated with ocean crustal magnetization.[17]

There now is enough information to propose an alternative hypothesis for stripes within the vertical tectonics model. It is known that changes in magnetic intensity can be due to magnetic susceptibility differences. Magnetic susceptibility is the degree to which a rock can be magnetized by an external magnetic field. Differences in magnetic susceptibility can be caused by intrusive igneous rocks, injected in linear extensional cracks during differential vertical motion between the mid-ocean ridges and the abyssal plains.

Among the evidence for this hypothesis is the Havre Trough, north of New Zealand and part of the back arc for the Kermadec Trench, and the Red Sea anomalies. The Havre trough has linear magnetic anomalies of nearly the same magnetic intensity as stripes. However, the Havre Trough anomalies are *not* interpreted as the result of seafloor spreading but due to magma of different magnetic susceptibility pushing up through linear dikes.[18] The linear magnetic anomalies of the Red Sea continue onto the adjacent land. These 'stripes' have been attributed to deep-seated igneous dikes.[19]

Are basalts older the farther away from the mid-ocean ridges?

Baumgardner points out that the first deep sea drilling expedition of the Glomar Challenger in 1968 to the South Atlantic discovered that the top of the ocean basalt layer increased with age away from the Mid-Atlantic Ridge. Furthermore, hundreds of subsequent deep-sea drill holes supposedly have documented that the ocean floor is no older than Mesozoic (actually the mid Mesozoic or Jurassic), and that Africa had been joined to South America prior to the mid Mesozoic. Baumgardner claims that these deductions are *independent* of radioisotope methods. I maintain that these deductions are based upon the presumed evolutionary sequence of microfossils, discussed in the previous section, and are tied to radiometric dating.

During Leg 3 of the Glomar Challenger expedition, nine holes were drilled from the Mid-Atlantic Ridge to Rio de Janeiro, Brazil.[20] The sediments were dated by the evolutionary microfossil dating schemes, and the sediments above the basalt basement 'increased in age' with distance from the ridge axis. These ages supposedly matched the ages based on magnetic anomalies. Of course, the investigators assumed that the age of the basalt basement was the same age as the basal sediments, which is an unprovable assumption.[21] Should we creationists base our models on such uniformitarian/evolutionary dating sequences? The evolutionary microfossil age sequence *is based* on radiometric dating; it is not independent of radiometric dating as claimed by Baumgardner. Maxwell *et al.* state:

> 'Estimated uncertainties in the paleontologic ages have been determined from the stratigraphic correlation by Berggren. However, a more recent compilation, *based on radiometric dates within stratigraphic sequences on the continents*, gives somewhat differ-

ent ages for some paleontologic stages, particularly during the mid-Tertiary [emphasis mine].'[22]

Many of the 'dates' for the deep-sea drill holes are Tertiary. It is interesting that those who developed the catastrophic plate tectonics model believe the Tertiary is post-Flood.[23] This would mean that the Tertiary sediments and contained microfossils from the ocean bottom, the continental shelves and the continents were all laid down *after* the Flood. This would also imply that rapid seafloor spreading, based on Tertiary dates of ocean floor basalt, continued into the post-Flood period! Furthermore, how would we account for such a precise, worldwide sequence of microfossils (as deduced by the evolutionists) deposited *after* the Flood, if one believes these index fossils are really time indicators within the creation-Flood model?

Trenches, subduction and the Wadati-Benioff zone

Baumgardner sees strong support for plate tectonics in what has been interpreted as subduction zones. Again we must distinguish between data and interpretation. The data are the trenches and the Benioff zones, which are more accurately called Wadati-Benioff zones. The subduction zone is the interpretation. These topics have been briefly summarized in my first submission to this forum. They have been more extensively analyzed elsewhere.[24–26] In this section I will add more information than in my first submission.

It is certainly true that volcanic activity and large earthquakes are associated with Wadati-Benioff zones. However, the picture is more complicated. Strong earthquakes are characteristic of only a few Wadati-Benioff zones and usually occur close together in time and space, which is one of the unsolved mysteries in seismology.[27]

Some Wadati-Benioff zones are very shallow. For example the earthquakes in the Nankai Wadati-Benioff zone are detected down to only 50 to 80 km. Besides the gaps in seismicity within about 50 km of the trench and at intermediate levels, earthquakes vary considerably along strike. It is interesting that where one would expect frictional sliding of plates to be the strongest in the brittle upper lithosphere, there are few quakes within 50 km of the trench. Half the deep quakes in the world are associated with the Tonga Wadati-Benioff zone. Deep quakes occur at only two locations on the Peru-Chili Wadati-Benioff zone, so how can plate tectonics enthusiasts draw a deep subduction zone 'to 650 km depth for 4,500 km in length'? If the Wadati-Benioff zone represents an interface between two plates, *the vast majority of presumed subduction slip is really aseismic*.[28]

There are several strange geometric configurations of Wadati-Benioff zones, if they really represent subduction zones. There are several instances in the western Pacific of two Wadati-Benioff zones that are relatively close together but *converge* toward each other, for instance the Tonga and New Hebrides Wadati-Benioff zones. Between these zones, copious convergent geological features are expected, but the area is dominated by *extensional* features.

Another unusual feature for the standard interpretation is the case where a Wadati-Benioff zone changes dip along the *same* side of a plate, for instance along the western edge of the Philippine Sea plate. The Ryukyu Wadati-Benioff zone dips northwest along the northwest edge of the plate, while the Manila Wadati-Benioff zone dips eastward along the west edge of the plate.

It is true that the majority of earthquakes along the Wadati-Benioff zone are of thrust type.[27] However, there are many exceptions and complications. The quakes at intermediate levels generally result in downdip tension, and they are usually not in the plane of the plate interface,[29,27] unlike what is expected if one plate is sliding below another. In the generally aseismic Cascadia Wadati-Benioff zone, the weak shallow quakes are *not* thrust type, but exhibit downdip tension.[30]

Most deep earthquakes display downdip compression, as expected, but there are complications. For instance, the great Bolivian deep-focus earthquake apparently slipped on a *horizontal* plane, cutting across and through what is believed to be the plate interface.[31]

Another interesting complication is that intermediate and deep earthquakes occasionally occur outside the Wadati-Benioff zone. Furthermore, many of the quakes do *not* occur at the plate interface, but below the interface.[27] Recently, the pattern of quakes at intermediate depth at many locations has been found to occur on two planes parallel to the dip of the Wadati-Benioff zone and separated vertically by about 35 km. This has been rather difficult to explain within the plate tectonics paradigm. Although the pattern of intermediate depth double seismic zones are variable, the upper plane is usually downdip compression while the lower plane exhibits downdip tension, which cannot be explained by the unbending of the slab.[32]

It is obvious that the Wadati-Benioff zone represents some type of tectonics. I will delve a little deeper into an alternative hypothesis,[33] which has been suggested by many researchers. Considering only the western Pacific Wadati-Benioff zones, the island arcs possess high lithospheric temperature, high heat flow and volcanism. The backarc basin to the west also has high heat flow. This is anomalous because a relatively *cold* subducting slab should cool the mantle lithosphere below the island arc and backarc basin. Moreover, the island arc has a high gravity anomaly, while the trench has a very low gravity anomaly. When all the data are added up, the picture better supports a rising mantle wedge at the location of the island arc and backarc basin. The trench would be a graben formed subvertically by sinking crust and mantle in front of the subvertical uplift. This rising mantle wedge would account for the gravity anomalies, the high heat flow pattern and volcanism, and the ubiquitious evidence of extension *throughout* the 'subduction zone,' even on the forearc.

But, isn't the Wadati-Benioff zone and area of underthrusting an area of convergence as shown by GPS and

other geodetic systems? There are many complications, plate tectonics being just one interpretation of the data. GPS data around 'subduction zones' show that the convergence is not restricted to the plate interface but is spread out across a considerable area perpendicular to the plate boundary.[34] Furthermore, the motion at the 'interface' is relative, *based on the reference frame*. One does not really know whether the average motion at the interface is underthrusting or the opposite, reverse faulting and overthrusting. If the Pacific plate is the reference and is considered stationary, which appears to be the case,[35] then the Western Pacific island arcs would represent an uplifting, deep reverse fault that becomes an overthrust at shallow depths. The earthquake motions in the Wadati-Benioff zone would be expected to be more chaotic than assumed by plate tectonics.

Most of the features of 'subduction zones' are opposite to what one would expect if one plate is sliding underneath another. It is rather amazing that the data support more the opposite motion—slantwise vertical uplift of the island arc.

The catastrophic plate tectonics model

I will offer only brief comments on Baumgardner's elegant model. Baumgardner lists two logical imperatives for any Flood model: 1) the Flood/pre-Flood boundary is at the Precambrian/Cambrian boundary, and 2) there is no ocean floor older than Mesozoic and hence all the current ocean basalt must be generated in the Flood. These 'imperatives' must be demonstrated within a creationist paradigm, not just assumed.

Creationists are divided on the location of the Flood/pre-Flood boundary. There are a number of possible Flood explanations of why the Precambrian has no or few fossils. This is a subject of research.[36]

The second 'imperative' stems from a belief that the geological column is an *exact* Flood sequence. This also needs to be demonstrated, not only locally but regionally and globally. Consequently, what does it mean that no ocean floor is older than 'Mesozoic'?[37] This is a deduction based on index fossils and radiometric dating and assumes the evolutionary progression of these index fossils. There could be other reasons related to the Flood of why 'Paleozoic' fossils are rare in ocean sediments.

Baumgardner points out that under certain rheological conditions rocks can deform rapidly. I agree with this deduction, but add that this information could more easily result in rapid vertical tectonics during the Flood than horizontal motions of thousands of kilometres. It takes special forces to move 100-km-thick plates *horizontally* over a spherical Earth. Besides, continents are suppose to possess deep roots, which should cause a considerable drag for horizontal motion of plates.[38]

Baumgardner refers to the 'resurfacing' of Venus. Why is this evidence for plate tectonics on Venus? Why couldn't the surface of Venus be created with a generally smooth surface, and not that long ago?[39]

One must remember that the catastrophic plate tectonics model is just a *model*. Models have many problems representing the natural world.[40] Especially problematic are the initial conditions that start the model and the physics of the model. I have worked with the output of atmospheric models for 30 years, and the initial conditions and physics of these models are well represented in each model run. But after simulating several days, the model becomes inaccurate. That is why weather forecasts can be so poor. Much more is known about the initial conditions and physics of atmospheric models than rheological models.

With that said, I and others find several details of the catastrophic plate tectonics model creative and worthy of further research though problematic. First, Baumgardner's model starts with an initial condition of gravitational instability. This seems like a special initial condition that cannot be verified. The whole model of runaway plate tectonics depends upon this initial condition.

Second, the idea of jets of steam reaching the escape velocity of the Earth and cooling the water and ocean floor is creative, but is it viable? These jets are supposed to pull along liquid water and cause cavitation, pulverizing the rock into silt and clay. But isn't cavitation significant only in *shallow* water?[41] Supersonic velocities may overcome the shallow water requirement, but this needs to be worked out.

Third, can runaway subduction of a plate cause *all* the continents to subside, so as to receive sediments? It seems like this mechanism may have a problem for continents far from subduction zones, such as Europe, Africa, and Antarctica.

Fourth, Baumgardner states that pelagic material is accreted to continental margins. However, accretion zones are composed predominantly of terrigeneous *clastic* sedimentary rocks and not oceanic pelagic sediments.[42]

Fifth, Baumgardner has an interesting idea on trenches rebounding upward tens of kilometers, resulting in extensional features in trench sediments. I must point out that many trench sediments are predominantly horizontally layered, showing little deformation, while other trenches lack sediment altogether.[43] There are other questions that can be asked of his model.

Conclusions

In conclusion, Baumgardner's model is elegant, but contains many problems. The most serious seems to be assuming plate tectonics has been proven and too much uniformitarianism. It is also tied indirectly to radiometric dating.

I do not believe we have arrived at a geophysical mechanism for the Flood, yet. The real data used to support plate tectonics, such as Wadati-Benioff zones, can support alternative models. I believe vertical tectonics, possibly associated with meteorite impacts, has strong potential but has its own problems. I am not committed to any model, but have been in the data-gathering mode for 25 years. I

believe, as a matter of principle, that at this stage in Flood research we need several competing models when there are so many unknowns.

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