Microdiamonds found in Japanese forearc

Michael Oard in his recent Perspective note entitled "Microdiamonds found in Japanese forearc" again questions the possibility that rocks of continental crustal affinity could have actually been carried down to depths greater than 150 km where ultra high pressure (UHP) mineral phases, including diamond, can form. He also questions how, if such a thing did occur, such rocks might be brought back to the surface rapidly enough that the UHP phases did not revert back to their low pressure counterparts.

Although it is true that such a scenario is genuinely problematic for uniformitarian plate tectonics, this scenario is precisely what one expects from the standpoint of catastrophic plate tectonics (CPT). In fact, it is difficult to imagine how moderately large volumes of low density sediments would not get entrained in subduction zones and carried deep into the mantle if plate velocities are increased a *billionfold,* as the biblical timeframe in the case of CPT requires. Moreover, the buoyancy of such rock relative to the surrounding mantle is so large that it displays a strong tendency to rise back to the surface, much like bubbles of air that get carried down as a diver plunges into the water but then promptly rise back to the surface. The return path for the low density sediment generally is along the subduction zone down which it was originally carried because of that path's relatively low viscosity.

As Oard correctly points out, UHP minerals are most commonly found in mountain belts in linear zones that have experienced extreme tectonic deformation. With rare exceptions, these belts occur in zones where a large amount of subduction has taken place, as many lines of evidence indicate.

However, his note highlights the recent discovery of microdiamonds in a somewhat different setting—in a xenolith recovered from an igneous dyke in a forearc environment near Japan. While such a finding is unexpected in the context of uniformitarian plate tectonics, it is relatively easy to account for in the CPT framework.

All that is required in this setting is for a sufficiently large blob of sediment to be carried down to depths greater than 150 km by the rapidly subducting plate that has generated the island arc. Metamorphism in this blob of sediment at these depths produces the UHP minerals, including microdiamonds. The buoyancy of the blob gives it a strong tendency to migrate back up the subduction zone towards the surface. Some of this material gets underplated on the overlying island arc. A subsequent eruption of magma through this underplated zone rips away a piece of rock containing the microdiamonds and incorporates it as a xenolith in the resulting dyke. This explanation, apart from the underplating, is similar to how CPT accounts for UHP zones in their more common continental orogenic belt settings.

Because Oard somehow fails to understand how elegantly CPT accounts for UHP minerals, including microdiamonds, he suggests that huge asteroid impacts might have formed not only these microdiamonds but also the Aleutian Island Arc, and if that arc, presumably most other ones on earth. Unfortunately, he accepts the idea of Ron Samec that the bombardment forming most of the craters on the moon coincide with the time of the Flood on earth. Both seem to be unaware of the radioisotope dating of the moon rocks that indicates strongly that this bombardment of the moon must have taken place during Creation Week, as the careful work of the RATE team implies. Using lunar cratering as a guide, Oard suggests that during the Flood there may have been as many as 36,000 impact craters greater than 30 km in diameter on Earth, with several as large as 4,000–5,000 km in diameter.

But if such an astonishing level of bombardment of the earth really occurred during the Flood, why is not every sandstone deposit on Earth of Flood age laced with an abundance of microdiamonds? And, given that Oard rejects the idea that the pre-Flood ocean floor was recycled into the earth's interior during the Flood, why is there not an obvious and unmistakable record of these many craters preserved on today's ocean floor? Moreover, since many craters are indeed reasonably well preserved in continental rock record, why do these preserved craters not give a true (but much lower) estimate of the actual number of impacts during the Flood?

But these issues are dwarfed by the difficulties Oard has in his impact framework in accounting for the obvious features of today's ocean floor such as the mid-ocean ridge system, some 60,000 km in length, with extreme heat flow along its axis, with sharp offsets in the ridge axis, with strike-slip earthquakes along the perpendicular faults (transform faults) that join the offsets. Other features include essentially no sediment along and near the ridge axis and increasing sediment thickness as one moves away from the axis. They include systematically increasing radioisotope ages for the basaltic basement rocks as one moves away from the ridges, ages that correspond without exception to Flood-age and younger rocks in the continental record. The features also include a sequence of microfossils on the ocean bottom that matches closely the microfossil record on the continental shelves, a record that again points strongly to a Flood age for all of today's ocean floor. In addition there are the deep ocean trenches, most of which occur along the perimeter of the Pacific Ocean. With few exceptions, these trenches are associated with spectacular nearby volcanism (the so-called Ring of Fire) and intense nearby earthquake activity, both of which, together with the depression of the sea bottom corresponding to the trenches themselves, strongly imply that ocean lithosphere has subducted and is subducting even today into the earth's mantle adjacent to the trenches.

An objection to the plate tectonics framework Oard has raised in the past and again in this note is the character of the sediments in the deep ocean trenches. Oard has pointed out that it is common to find trenches adjacent to continents filled with terrestrial sediments, often with little or no evidence for the sort of compression one expects from the uniformitarian framework at a convergent margin. Oard overlooks the fact that in CPT almost all of the plate motions *must* occur during the main phase of the Flood while the strength of the mantle is low—reduced via the runaway process that is unfolding. In the CPT framework, when the gravitational potential energy is exhausted and the velocities drop dramatically, the mantle regains its normal strength and plate motion comes essentially to a standstill—on the order of what we measure today. Subsequently, as the continents rebound, the mountains rise and the water retreats from the continent interiors, sediment is washed from the continent interiors onto the continental shelves, and some of this sediment fills adjacent ocean trenches. At typical convergence rates observed today (for example, by GPS methods), only about 250 m of convergence would have occurred within the trenches since the Flood took place some 4,500 years ago. This is one of several ways in which CPT scenario differs markedly from the standard uniformitarian one.

Another major difference is the near simultaneous uplift of all the high mountains on the earth preceded by a global planation of the surface above where they rose, as described by Ollier and Pain in their book, *The Origin of Mountains,* published in 2000 and reviewed by Oard in this journal. The near simultaneous uplift of the mountains—occurring, according to uniformitarian understanding, millions of years after the tectonic processes had produced the extra crustal thickness required to support these high mountains—is indeed a major problem for the conventional framework. But such a simultaneous uplift immediately after the Flood is exactly what is expected in the CPT framework. Oard did not appear to understand this obvious conclusion or, if he did, he failed to mention it in his review.

In closing, I would like to take this opportunity to encourage Michael to acknowledge that CPT, although as yet far from comprehensive, nevertheless does account amazingly well for a large fraction of the earth's most important tectonic features and does so within the biblical timescale. I encourage Michael to switch to a positive stance regarding CPT, and I sincerely invite his constructive suggestions on how it might be extended, strengthened, and improved.

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Michael Oard replies:

I thank John Baumgardner for providing a CPT explanation for ultrahigh pressure minerals (UHPM) including microdiamonds, but some of his comments need to be addressed as follows.

Ultra-high pressure minerals

We agree that the origin of UHPM is problematic for uniformitarian plate tectonics, but Baumgardner states that the "scenario is precisely what one expects from the standpoint of catastrophic plate tectonics…". I don't believe I have seen such a prediction in the CPT literature. Even if it is reasonable for a few low-density sediments to be trapped atop a subducting plate, almost all should be obducted and plastered onto the edge of subduction zone.

His scenario then stretches credulity at the return of these sediments from the depths. Sediments buoyant enough to fight their way back to the surface would probably obduct in the first place. But the idea can be tested. Do we find abundant UHPM at, and only at, subduction zones? Despite Baumgardner's assertion, UHPM are often found away from subduction zones; like those extending 4,000 km east-west in China, or those in Norway, Nunavut, northeastern Canada, French Guiana, Kazakhstan, Antarctica, British Columbia, the Appalachian Mountains, Poland and the Czech Republic. While uniformitarians may posit ancient subduction zones in these locations, that does not seem possible within the present CPT model.

An ancillary issue is the angle of subduction. CPT posits near vertical motion; while the earthquake foci that supposedly trace today's zones vary widely up to nearly horizontal—like parts of the Peru-Chile system. The Indonesian "subduction zone" bends 3.5° for 100 km landward from the trench axis, increasing to 11° for the next 100 km.[1] While one might imagine buoyant sediments fighting their way back up vertical plates, nearly horizontal retreat staggers the mind. Also, how did the vertical subduction zones in CPT become so flat in these locales?

Baumgardner proceeds to criticize the role of impacts in the Flood before reiterating evidence for CPT. I will deal with these issues inversely.

Ocean bottom features

Mid-ocean ridges require explanation, but their mere existence does not prove CPT. For example, they could be late-Flood volcanic orogens. It is curious that if they are spreading centers the heat flow varies so dramatically along the ridge axis; it averages only a little higher than the rest of the ocean and the continents. Thickening sediments toward the continents would be expected in either scenario. A late Flood, mid-ocean orogen would have covered previous sediments by basalt flows, and the mass of oceanic sedimentation would have occurred at the end of the Flood with the final regression of the floodwaters.

Baumgardner has always made much of the sea-floor age distribution, but, even if a creationist accepted the dating, hardly any of the 949 DSDP and ODP sites reached unambiguous basement away from the ridge,[2] and Layer 2 basalts are often mixed with sediments. And the entire dating scheme is destroyed by the presence of rocks recovered at the ridge dating back to Precambrian (see Smoot[2] and Pratt[3] for numerous references).

Citing biostratigraphy seems out of character for a creationist since it is a reconstruction of the evolutionary history of the fauna. The match of deep sea and shelf assemblages is a matter that requires creationist investigation, not ideas based on evolutionary research.

Deep-sea trenches

Perhaps I was the only one to lose the logic of his discussion on trenches, but will respond as I can. Baumgardner states that I overlook that in CPT the plate motion occurred during the main phase of Flood. But what is the "main phase"? In 1994, CPT coincided with the onset of the Flood. Since 2002, it seems to have migrated to the middle of the Flood, probably because criticisms regarding Wilson Cycles forced a re-evaluation of the Pangea breakup. Some have suggested a previous and mysterious episode early in the Flood, coinciding with Rodinia, but there is no evidence.

There are other internal inconsistencies. How did the steam jets at the ridges provide the 40 days of rain if the episode of CPT began in the middle of the Flood? It would benefit all of us to have a published CPT reconstruction showing the breakup of Rodinia, the re-assembly of plates as Pangea, and the resetting of the unique initial conditions of the current (Pangea) modeling to connect to the current work.

Back to the trenches, there is nothing in his discussion to discriminate between trench sedimentation with or without CPT. However, I believe that there is. Baumgardner proposes that the trenches were once 20 km deep, rebounding late to their current depths. If so, that rebound should be evident, at least in part, in the sedimentary record of those trenches with sediments.

Mountain uplift

Baumgardner times the major orogenies *after* the Flood by continental rebound. But CPT demands many exceptions: the mid-ocean ridges, volcanic ranges caused by subduction, and continental collisions during the Flood (especially if there were multiple cycles). Again, the late formation of mountains is an observation, not a theory. It fits the uniformitarian scenarios as well as the non-CPT diluvial framework. Though he was right to cite Ollier and Pain[4] in noting the youth of mountains, Baumgardner should also have cited their conclusion that most mountains cannot be explained by plate tectonics. In particular, the model needs to explain intraplate mountain ranges, such as the Transantarctic Mountains. The assumptions and lack of detailed information in CPT seems typical of many mega-models.

Furthermore, if mountain ranges such as the Zagros rose after the Flood, why is there no human record of such a stupendous event, even preserved as a legend or myth? According to Genesis, there would have been ample observers in the area. Like other problems with a too-low Flood/post-Flood boundary, this one can easily be resolved by recognizing that the boundary is higher in the rock record, and that these events were a part of the late Flood realignment of crustal topography.

In summary, CPT, like other Flood theories, unfortunately suffers under too little hard data and too much uncertainty.

Meteorite impacts

Regarding the role of impacts during the Flood, Baumgardner asserts that most of the moon's craters formed at creation, not during the Flood. Apparently, this is based on old radiometric dates from moon rocks and a concomitant belief in the relative accuracy of these dates. Both the reliability of dates in that sense and the origin of moon craters are areas for research at this time, not conclusions. For example, Spencer[5] made a case that all inner system impacts ranged throughout the Flood, and even after. Also, impacts at creation would cause several problems. First, we must ask if impacts would be "very good". Second, since the moon was created on Day 4, widespread impacts would have likely killed organisms on Earth, contradicting the theological point that death was a result of Adam's sin (Romans 5:12).

Baumgardner's questions about the dearth of microdiamonds and more impact evidence on the ocean floor, and estimating total impacts from the 170 known continental craters are good questions that need to be addressed by investigation, with open minds on all sides. At present, I believe that much of the evidence of Flood impacts was destroyed in the subsequent tectonism, erosion, and sedimentation. Indirect evidence may exist in Precambrian rocks. While Baumgardner views these as pre-Flood, I prefer a case-by-case empirical assessment, rejecting the global correlations of the uniformitarian time scale.

Closing statement

Like many other creationists, I welcome the research that has been done within the model of CPT. Where Baumgardner and I part ways is in his insistence that his model be accepted as absolute fact rather than a working hypothesis. I would also point out that his model carries a heavy burden of too much reliance on uniformitarian plate tectonics, radiometric dating, and biostratigraphy. It has also left unexplained a host of geological observations contrary to plate tectonic interpretations. We can all work together and maintain a professional respect, but only by avoiding dogmatic assertions about models that cannot carry that weight. Science advances by the clash of ideas, not the uncritical acceptance of the theory of the day.

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