eEarth Discuss., 4, C7–C11, 2010
www.electronic-earth-discuss.net/4/C7/2010/

eEarth Discussions

Interactive comment on "Plate tectonics conserves angular momentum" *by* C. Bowin

Anonymous Referee #2

Received and published: 19 January 2010

Carl Bowin's paper presents a plethora of ideas that are thought provoking and that should stimulate further work on plate motions. The paper was enjoyable to read because it provides a good deal of history and a bit of a plate tectonics overview, and it is generally well written. The paper's main flaw is that it states very firm conclusions but does not provide sufficient evidence to back the conclusions. It therefore would need to undergo rather extensive rewording to be publishable. It currently reads more like a summary of Bowin's opinions and speculations on plate tectonics rather than a thorough scientific test of one or more hypotheses. The paper's main strengths are that opinions and speculations are mainly plausible (I certainly would agree with much he has written), several of the methods are innovative (the quaternion interpolations of stage poles is great), and rarely has such a range of fundamental plate tectonic reconstructions and interpretations been assembled in one place.

As an example, the basic hypothesis that plate motions conserves angular momentum is quite plausible and the manner in which Bowin has attempted to test it is commendable. The reasoning and analysis seem sound, with the exception that it is very difficult to get a feel for the size of the uncertainties in the analysis. How much of the angular momentum might not be conserved by the plates alone? I think the uncertainty is much too large to conclude that basal tractions on the plates and motions of the mantle are not factors. Assumptions about the thickness of the lithosphere alone would allow for quite a bit of variability in the angular momentum estimates. As the plates are reconstructed back in time, those uncertainties must grow as the geometry of the plates, their areas, and the locations of plate boundaries all become more speculative, particularly along subduction zones. With that said, I think what Bowin should conclude is that it is possible that surface plate motions conserve angular momentum based on a good plate motion data set and some fairly basic assumptions about plate thicknesses and plate geometries through time. His conclusions are thus feasible but the test is inconclusive because the observations allow for other scenarios.

Similarly, the conclusion that gravity from subducting slabs (slab pull) is the only driving force for plate motions is certainly not proven in this paper or elsewhere. I don't think there is much debate that slab pull is the most significant driving force, although few would claim it is the only significant force. A short-coming of the paper is that it rejects other driving forces without providing sufficient evidence for rejecting them. In fact, the references on plate driving forces is one area that is lacking in the paper. It would be worth while to add a paragraph (probably inserted in p.29 where Conrad and Lithgow-Bertelloni are referenced) that better describes studies that find the existence of plate driving forces other than slab pull and studies that give more information about the relative significance of the slab pull force. Some potential studies to cite are:

* Iaffaldano and Bunge, Relating rapid plate-motion variations to plate-boundary forces in global coupled models of the mantle/lithosphere system: Effects of topography and friction, Tectonophysics. Vol. 474, no. 1-2, pp. 393-404. 1 Sep 2009;

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* Wu et al., Earth and Planetary Science Letters, vol. 272, no. 1-2, pp.412-421, 30 Jul 2008;

* and references therein (or search GEOREF).

Another place where a more modest wording and better referencing would be appropriate is when conclusions are made about changes in plate motions. To claim that this study has found that plates undergo accelerations and decelerations in a manner that would suggest other studies have not is very problematic. There have been hundreds of studies that have looked at plate motions and the accelerations and decelerations that plates undergo. Nearly every geomagnetic polarity timescale has some form of crude plot showing changes in spreading rates (e.g., Cande and Kent, 1992), and these are studies that don't even focus on such changes.

Similarly, references for plate-hotspot motions needs to be expanded. The bend in the Hawaiian-Emperor track has been re-dated by Sharp and Clague (2006) to about 50-47 Ma, which is consistent with the discussion in this paper. I assume Bowin knew that but referencing that observation is necessary. Other studies (and references within these studies) that would be worth using are:

Sharp, W. D., and D. A. Clague (2006), 50-Ma initiation of Hawaii-Emperor bend records major change in Pacific plate motion, Science, 313, 1281– 1284, doi:10.1126/science.1128489.

Wessel, P., Y. Harada, and L. W. Kroenke (2006), Towards a self-consistent, high-resolution absolute plate motion model for the Pacific, Geochem. Geophys. Geosyst., 7, Q03L12, doi:10.1029/2005GC001000.

Wessel, P., and L. W. Kroenke (2007), Reconciling late Neogene Pacific absolute and relative plate motion changes, Geochem. Geophys. Geosyst., 8, Q08001, doi:10.1029/2007GC001636.

I would suggest that the paper be accepted for publication after significant revision,

including the following:

* Much rewriting to clarify what the study has found versus what the author envisions (see above).

* Better referencing of background material (plate forces, plate motions, plate reconstructions, hotspot studies, etc.).

* The section on interpolating stage poles should be expanded. An example should be added using either a real or synthetic plate pair with a few stage poles. A figure showing how the number of iterations and other changes in parameterization affect the reconstructions is needed. Currently, Bowin mentions various parameters but they have no meaning to the reader as their effect has not been documented nor discussed. Overall, the method is a black box. As this may be the most cited part of the study, it is certainly worthwhile for this section to be expanded.

* The section referred to as “Results and conclusions”, really must be changed to something like “Results and speculations”. The author needs to make a very large effort to separate what are advances from his study versus speculations that future studies may seek to investigate and test.

Other more minor changes:

* The paper sometimes jumps from topic to topic without clearly linking the topics, e.g., p.25 (lines 12-13).

* The discussion of the age of the bend in the Hawaiian-Emperor track (p. 25 lines 10-13) needs to be consolidated with later discussion and include the Sharp and Clague (2006) reference.

* Better explain, possibly through adding a figure, what is being discussed about gravity (p. 26). The gravity analysis apparently motivated the author but the audience is left in the dark about the motivation.

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* The Fortran code, movies, and other ancillary material should be combined in zip file. I looked over the movies, both at WHOI and that were included with the paper, and many of the images have low resolution. I also think that perhaps some of the movie files are missing. The stage pole interpolated movie was missing the smooth interpolation and instead showed only the main stages. Possibly I was looking at the wrong files. Clear labeling and clear links from the text to the files would be a big improvement.

* Quoting an unpublished manuscript by oneself should be avoided (p. 28 Lines 18-21).

* Figure resolution is generally very low. If possible, higher resolution figures should be provided, e.g. Figures 1 and 2.

* Figure 5 & 6 captions need to define the hollow circles.

* In Figures 5 & 6, I am surprised by the lack of change in plate area over time. The log area plot may be masking the changes. In any case, one need only look at Figure 7 to see that the Pacific and other plates change in area over time, which makes one wonder about the accuracy of the nearly horizontal lines in Figures 5 & 6.

* Capitalize “Pacific” and many places.

* Page 38: I am not quite sure what “migration of a pattern of opening” is about. If this pattern is of such significance, some clearer definition of it is needed.

* Page 38 line 18: “it grew that way”. This is not a very informative insight.

* Page 38: Remove conclusion #5. Claims about future work certainly are not worthy of being conclusions.

Interactive comment on eEarth Discuss., 4, 21, 2009.