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eEarth Discussions

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

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I would like to thank Hugo Dominguez and Marco Perez for their kind and encouraging statements. I also appreciate the several personal complimentary conversations and encouragements received from other scientists, but who did not submit formal comment to eED. I want to particularly thank the two Anonymous Referees who did submit interactive comment for their time and thoughtful efforts. The following are my replies to the two formal reviews submitted.

Anonymous Referee #1 begins by stating "the author argues for conservation of the total angular momentum over the studied period, despite observed variations in the kinematics of individual plates". Unfortunately, the caption for Figure 5 did not include the clarification that the empty uncolored circles along the top are the total values ob-

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tained from summing the contributions of the 12 plates (with colored symbols below) over the 62 to 0 Myr time scale. I thought that labeling error had been caught and corrected (but was not), and clearly led the referee to think poorly of the paper and would not find my arguments soundly convincing. However, the abstract does cite the magnitude value of the total angular momentum, and thus it could be noted that that value matches the general value of the line of open uncolored circles in Fig. 5. Another misunderstanding concerns his criticism that my study was "computed with one single kinematic reconstruction by Harada & Hamano (2000). It is only on the Pacific Plate that their clever geometrical reconstruction of past Euler poles is possible because of the existence of at least three traces of past seamounts (former hot spot) locations, on the same plate, have been identified. There simply is no other tectonic plate on the Earth that could similarly provide high resolution Euler pole estimates at 2 Myr intervals by their method. Jason Morgan's scalar quaternion Fortran code provided a convenient way to extend the Pacific plate's high resolution 2 Myr spaced rotation solutions to the other eleven plates for which Euler pole estimates for the past 68 Myr are available. Via the internet, I printed, "Steering Geodynamics Models with Plate Tectonics Reconstruction Soaftware: GMAP", authors M. Turner, M. Gurnis, L. Di Caprio, J. Boyden, J. Clark, J. Cannon, D. Muller, R. Watson, and T. Torsvik. It did not offer Euler pole data I did download GMAP but did not install this complex program. In that package there is a .dat data file, but I do not know what program to use to read it, and therefore was not in position to try using any Caltech/Torsvik set of Euler poles with Morgan's quaternion code I, too, take conservation of angular momentum as evidence of a closed system. In my revised manuscript, probable friction and stresses at the base of the lithosphere are incorporated, as well as gravity being the linkage communication force that provides for the plate tectonic conservation of angular momentum. The revised paper will also contain more details of the computation of the moment of inertia.. I also anticipate having zipped movies being included.. Re; p.24, l. 16, 'means' is a verb, that in this case, is 'to provide a purpose'. Yes, I will add some references to p. 24, l. 16-20, and will also try to improve p.27, l. 5-21. Furthermore, strong support of my 0 Myr maximum plate

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velocity vector directions and length (as seen by the green arrows in both Figure 7 and in the movie) is provided by the International Terrestrial Reference Frame (ITRF) 2005 vector directions determined from GPS, VLBI, SLR, LLR and DORIS observations of present day plate motions.

Anonymous Referee #2 provides a most gratifying and constructive review. I agree with essentially all his/her criticisms, and intend to incorporate those modifications in the revised paper to be submitted soon. I do disagree that the thickness of the lithosphere alone would allow for quite a bit of variability in the angular momentum estimates. I interpret the decrease in relief away from the Mid Atlantic Ridge spreading center to be a consequence of thermal cooling, not a change in mass. This assumption is supported by the fact that almost everywhere, oceanic free-air gravity anomalies are within 40 milliGals (or less) of zero, and hence that the ocean lithosphere is in isostatic equilibrium. That is why I assumed a constant thickness for all plates in the calculations. I have age data for the oceanic crust, but because of isostatic equilibrium, did not believe it is needed, nor warranted. Similarly, that is why I reject a 'ridge-push' as a legitimate force. Friction and strain presumably occur along the fault planes of transform faults, as well as along the base of the plates, but I now conclude that the magnitude of the sinking phase change mass, in the subducted slab, creates sufficient force to overcome both basal-plate and transform-fault friction and strain. I, too, was surprised to not see the plate area and mass curves decrease with older ages because of tack of data pixels to represent older subducted ocean crust. At least a partial explanation concerns the one-degree grid mesh size. Often, upon rotation of two or more grid points back in time, they would occupy the same prior grid cell, which can be identified in Fig. 7 as the increasing number of black dots in the older aged plots Although those grid cells overlapped in the map plots, each was used separately in summing the moments of inertia and angular momentum calculations.

On a recent annual visit to my dermatologist, he asked me why the Earth's moon presents the same hemisphere towards the Earth. I explained that the Moon rotates

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once about its own axis, in its annual orbit around the Earth, and explained that as the Moon gradually increases its distance away from the Earth, the Earth's rotation rate commensurately slows down so as to conserve the total angular momentum of the Earth-Moon system. Later, that evening, I realized that, similarly, it is the force of gravity that links the changes in one plate's velocity (plate angular momentum's) to other plates, so that when one plate slows down, other plate(s) must speed up, retaining a constant total plate tectonic angular momentum

After I had completed the above comments and was about to send them to eED, I received notification that a third reviewers review (Ben Horner-Johnson) had been accepted following closure. Thus the following comments are in response to this additional review. Again, I thank Ben Horner-Johnson for his thoughtful and diligent review. Many of his suggestions will be incorporated in the revised paper to be soon submitted. Here I will concentrate on only a few of his questions, two of which raise sociology issues normally not discussed in scientific publications, but this venue seems to me appropriate. The 2046 model was an attempt to explore the possibility of plate reorganizations near 20 Myr, but in part failed because the filter boundaries for the 3-segment qfilter run were not well chosen by me, and I now anticipate following the reviewers suggestion of deleting those results from the revised text, and if further processing should provide interesting results will publish another article. The quaternion processing code is available via Harry Kuiper's CWMTX web site. Since the absolute motion of the Australian plate is moving northward, and the Pacific plate's motion is eastward (and verified by the ITRF-2005 velocity vector results), if there were thermal 'roller' convection cells driving these motions, then, in my mind, I would have to be able to visualize how the geometric relations between the downgoing 'roller' cell boundary at the north end of the Australian plate could be separate from the east moving Pacific plate 'roller' cell, and that, I could not plausibly do. Hence, for me, that is strong evidence against thermally driven convection cells. I cited the history of the Hess preprint [viewable at ftp://ftp.whoi.edu/pub/users/cbowiin as file named "copy_HHHess_1960_evolution_ocean_basins.pdf"] because it gives proper credit to

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Hess for the 1960 date, and thus clarifying that Hess was the first to recognize that the ocean floor is young, not ancient. Robert Dietz received a copy of that reprint, and in 1961 published his two cited publications without reference to Hess's preprint. The original publication in which Hess expected the preprint to be published in became delayed by reasons beyond his control, and so it was printed in the first Geological Society of America (GSA) publication available. Since most scientists (or people in general) strive to make some important contribution in life, I wanted to emphasize that plate tectonics originated from Hess's original insight. The normal and reversely magnetic polarized bands of ocean crust were discovered and dated soon thereafter, and plate tectonics became confirmed. This eED/eE paper, I consider to be the most scientifically significant contribution of my long career, and one for which, I believe, Prof. Harry Hess would have been very pleased, and it will complete my great sense of regard and obligation to him. That is why the Hess history statement will remain there. And, now, for a long story. The reviewer recommends that Ma be used instead of Myr. That suggestion, however, opens a proverbial "can of worms". An earlier version of the present manuscript was submitted to G-cubed (an American Geophysical Union [AGU] publication), and rejected by a supposedly anonymous reviewer. I was enraged by the review, and replied to the G-cubed editor with a copy to the president of AGU. A copy of that review and my reply can be read by viewing 'Open letter to agu November 2008.pdf' at my publications web site: ftp://ftp.whoi.edu/pub/users/cbowin. The president, in tern put me in contact with AGU's publication chairman who then urged me to publish it in another AGU journal, such as Tectonics. But since AGU's printed journal policy requires use of Ma (for Myr), I said that before resubmission I would need agreement from its editor to allow use of Myr on the many figures involved. Although I could easily make that substitution in the text, changing the many illustrations would involve unwarranted costs. As an active emeritus scientist at WHOI, I initially received $3,000/yr funding research support, but in 2008-2009, because of economic constraints, that amount had been reduced to $1,000/yr, and I did not want to use it towards redrafting charges, when there would still be page charges to come. When

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the Tectonics editor replied that “rules are rules”, I gladly then turned to electroic-earth-discuss.net. Not only do they offer a welcoming welcome, but they are flexible [note publishing your review after the closing of the discussion period], and authors retain their copyrights. Besides, to me, Ma [a for annum] is more reminiscent of a force (mass times acceleration), than an age.

In 2008 my plate tectonic studies lost six months while I tried unsuccessfully to utilize the smoothed Pacific plate Euler poles to extrapolate to an improved estimate at zero million years. That effort was an attempt to overcome Euler pole estimate inaccuracies resulting from bisecting very short arcs used for the 6, 4, 2 Myr estimates by the Harada & Hamano (2000) method. An oceanographer here pointed out the theoretical impossibility of that effort, and so I adopted the published best estimates and simply linearly interpolated 6, 4, 2 Myr values between the 8 Myr qfiltered values and the published 0 Myr data for each plate. This linear interpolation presumably accounts for the small hook in the trend of the filtered Pacific plate Euler poles in its plotted map. I proceeded to compute the angular momentum history for each plate, and then summed the xyz results to determine the total momentum pole location and magnitude. The 2 Myr spaced angular momentum totals remained reasonably constant, thus supporting my hypothesis (after identifying plate accelerations in Bowin & Kuiper abstract T43C-01 at AGU Spring 2005 meeting) that plate tectonics does conserve angular momentum. Finally, I would like to take this opportunity to stand on a small ‘soapbox’ and give my view on anonymous reviews. The Bowin (2000) paper was finally accepted for publication by the editor after my adding the figure demonstrating a single point mass depth for the South American Andes geoid anomaly high. The reviewer who recommended rejection of that paper, because it did not match to the dynamic tomography view of Earth structure, then requested anonymity, which was so acknowledged in the publication. Since my formal training was in geology, having never had a course in geophysics, I apparently view geophysical problems differently than most geophysicists. Thus, I have often received negative initial reviews, but at other times, such views have led to new insights on the structure and history of the Earth and other planetary objects. As scien-

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tists we are supposed to be objective, but as humans, we too, are often also defensive and self-serving. There are many articles for, and against, anonymous review (whether single or double blind), as any internet search will show. The main fear, apparently, is the concern about the possibility of retribution, in either direction, if the reviewer and author become known to each other. Given the variations in human nature, I am sure that acts of retribution have occurred. For myself, I have no recollection of ever having requested anonymity, and I congratulate Ben Horner-Johnson for identifying himself, and I harbor no ill-will against the two reviewers who chose it on the basis of their past experiences. I also want to applaud eED (now SED) for providing this public discourse venue, and thus such commentary need not be explained in the revised paper.

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