Solid Earth Discussions

Solid Earth Discuss., referee comment RC1
https://doi.org/10.5194/se-2021-154-RC1, 2022

Comment on se-2021-154

Scott King (Referee)

Referee comment on "A tectonic-rules-based mantle reference frame since 1 billion years ago – implications for supercontinent cycles and plate–mantle system evolution" by R. Dietmar Müller et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-154-RC1, 2022

First off, this is a long paper with many detailed components and one could easily go down a rabbit hole picking at various assumptions one doesn't like. In spite of this there are several fairly straightforward conclusions. The authors frame this work as applying a "tectonic rules" based approach however I think a more useful description would be to say that they are testing a set of assumptions. Because they compare their approach with a series of other approaches, it strikes me that this is a test. The tectonic rules are (lines 198-201): "(1) rates of net lithospheric rotation (NR) are minimized but non-zero, (2) global trench migration velocities are minimized, favouring trench retreat over trench advance, (3) spatio-temporal misfit between plate motion model and present-day hotspot chains is minimized, and (4) global continental median plate speed remains < 6 cm/yr, based on continental plate speed statistics reported in Zahirovic et al. (2015)." I wonder, recognizing that these "rules" are really "assumptions" and/or general results of geodynamic models that may or may not be unique, whether all of these rules are necessary or whether one or two of them are sufficient to produce the results reported in the work. This is important because these "rules" are not really hard and fast rules.

There are many important results from this paper that contribute to our understanding of the past billion years of earth evolution. My biggest concern is that these are not well captured in the conclusions:

First (lines 777-779), these are the assumptions the authors used to constrain the model, that the model results are consistent with the assumptions is useful (e.g., you didn't screw up the optimization) but they are not really conclusions.

Second, the authors focus on the LLSVPs in the conclusions, which are problematic for two reasons:

- as the authors state (lines 736-742) "It is noteworthy that the unoptimised model PMAG, not representing a mantle reference frame, reaches an equivalent accuracy to the optimised models OPT1 and OPT2 (Fig. 14a). This reflects that the present-day mantle structure is largely the result of the post-250 Ma subduction history (Flament, 2019) and that the unoptimised versus the optimised models do not show any dramatic differences in the position of plates and subduction zones during 740 this time (compare reconstructions of the two models at 300 Ma and 200 Ma in Fig. 2a). The post-250 Ma differences in the subduction history between these models are not large enough to create any major dissimilarities between the modelled lower mantle structure at present-day." This is one of the most important aspects of this work and it shows that the "rules" approach used here isn't really significant to achieve the LLSVP structure.
- In the geodynamic modeling, the imposed plate and subduction motions/locations, dominate the flow and significantly disrupt the balance of forces. The fact that the authors don't see a difference between the two different density scaling factors for the LLSVPs is consistent with this. The statement, "demonstrating that the excess density of the basal mantle layer plays a secondary role, in comparison to the imposed plate motion history" may or may not be true. It is an artifact of the modelling approach, not a conclusion. That LLSVPs get moved around by the plate slab system isn't really novel. It was described in Bull et al. (2014) and in King (2015) as well as by many other authors, some of whom are cited at lines 677-679. In fact, in King (2015) I don't impose plate velocities so this is somewhat unique, although to be transparent, many people don't like the approach I use to create long-wavelength plate-like flow.

Next, one of the more interesting conclusions is buried in the middle of the paper (lines 319-322) and should be repeated in the conclusions: "This comparison provides an important insight, namely that the simple lithospheric no-net-rotation rule used to produce the NNR model produces results that are not dramatically different from a model optimised by a set of more general tectonic rules." The next sentence (lines 322-324) speaks to the importance of the result, "This is important because NNR models have been frequently used in tectonic and mantle flow models for practical reasons (e.g., Mao and Zhong, 2021; Zhong and Rudolph, 2015; Behn et al., 2004; Kreemer and Holt, 2001) in the absence of other available mantle reference frames." This gets to the heart of my concern in the first paragraph, which rules are critical and which rules are not? Do we need them all? Does imposing one enforce the others?

Finally, the interesting aspects of the reconstruction (lines 793-804) is the second part of the paragraph starting with LLSVPs and the present day pattern (which as I mention above) the authors point out is really controlled by the past 250 Myrs and not the period covered by the tectonic assumptions.The reconstruction differs from the assumption from the Oslo researchers that the piles are more or less stationary and from the results of Zhong, Rudolph, Zhang (and others in the Colorado group) that should an oscillation between degree 1 and degree 2 anomalies but (as I recall) these were mostly in the equatorial region. This work does see some transition between degree 2 and degree 1 (lines 705-710) and the evolution appears to be quite different than what has been seen in previous work. I'm something of a visual person and it would really be helpful to find a sketch diagram of the LLSVP locations through time. I realize there are different figures with temperatures in the deep mantle (Figure 9, 11, 12, 13) but it would be helpful to link the text more closely to the figures. The upper mantle structure (Figure 10) feels like a distraction and not really part of the story. It certainly doesn't help me. With all those figures I'm not sure they add to my understanding or overwhelm. I really think this part of the paper could be streamlined.

The idea of two thermochemical piles at the poles is quite interesting and could/should have interesting implications for the dynamo. There is no mention of that here.

When I look at Table 1, I wonder why there is a thermal diffusivity but no coefficient of thermal expansion, etc. It seems somewhat odd to call out this one material property and not list them all.

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The effect of plate motion history on the longevity of deep mantle heterogeneities,
Earth and Planetary Science Letters,
Volume 401,
2014,
Pages 172-182,
ISSN 0012-821X,
https://doi.org/10.1016/j.epsl.2014.06.008.
(https://www.sciencedirect.com/science/article/pii/S0012821X14003859)
Abstract: Understanding the first-order dynamical structure and evolution of Earth's mantle is a fundamental goal in solid-earth geophysics. Tomographic observations reveal a lower mantle characterised by higher-than-average shear-wave speeds beneath Asia and encircling the Pacific, consistent with cold slabs beneath regions of ancient subduction, and lower-than-average shear-wave speeds in broad regional areas beneath Africa and the Central Pacific (termed LLSVPs). The LLSVPs are not well understood from a dynamical perspective and their origin and evolution remain enigmatic. Some numerical studies propose that the LLSVP beneath Africa is post-Pangean in origin, formed as a result of return flow in the mantle due to circum-Pangean subduction, countered by an older Pacific LLSVP, suggested to have formed during the break up of Rodinia. This propounds that, prior to the formation of Pangea, the lower mantle was dominated by a degree-1 convection pattern with a major upwelling centred close to the present-day Pacific LLSVP and subduction concentrated mainly in the antipodal hemisphere. In contrast, palaeomagnetic observations which proffer a link between the reconstructed eruption sites of Phanerozoic kimberlites and Large Igneous Provinces with regions on the margins of the present-day LLSVPs suggest that the anomalies may have remained stationary for at least the last 540 Myr and further that the anomalies were largely insensitive to the formation and subsequent break-up of Pangea. Here we investigate the evolution and long-term stability of LLSVP-like structures in Earth's mantle by integrating plate tectonics and numerical models of global thermochemical mantle dynamics. We explore the possibility that either one or both LLSVPs existed prior to the formation of Pangea and improve upon previous studies by using a new, true polar wander-corrected global plate model to impose surface velocity boundary conditions for a time interval that spans the amalgamation and subsequent break-up of the supercontinent. We find that, were only the Pacific LLSVP to exist prior to the formation of Pangea, the African LLSVP would not have been created within the lifetime of the supercontinent. We also find that, were the mantle to be dominated by two antipodal LLSVP-like structures prior to the formation of Pangea, the structures would remain relatively unchanged to the present day and would be insensitive to the formation and break-up of the supercontinent. Our results suggest that both the African and Pacific LLSVPs have remained close to their present-day

positions for at least the past 410 Myr.

Keywords: mantle convection; LLSVP; Pangea; thermochemical piles; tectonic plates

King, S. D., Mantle convection, the asthenosphere, and Earth’s thermal history, in Foulger, G. R., M. Lustrino, and S. D. King eds., The Interdisciplinary Earth: A Volume in Honor of Don L. Anderson: Geological Society of America Special Paper 514 and American Geophysical Union Special Publication 71, p. 87-114, 2015.