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Comment

Interactive comment on "Pacific Plate slab pull and intraplate deformation in the early Cenozoic" *by* N. P. Butterworth et al.

Anonymous Referee #2

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This paper uses a geodynamic model to predict the motion and deformation of the Pacific plate over the Cenozoic. The authors argue that the geodynamic model matches the direction and changing Euler pole positions of the Pacific over the Cenozoic as well as ideas of the plate deformation deduced from intraplate volcanism again over the Pacific. If this paper were to be published, then there would need to be a clear discussion of the significant limitations of the modeling approach.

There is no doubt that a lot of work went into making the models and it's nice to see a collaboration between those who construct the models with those who synthesize the observations from many directions. Unfortunately, there are substantial problems in the underlying methods, their assumptions and the model predictions. The problem with the models span issues with the geodynamic methods and assumptions as well

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as the application of the plate reconstruction.

The model outcomes don't provide a good or even a realistic match to observed plate motions. The problem is that that strain within the plates is much larger than observed for intraplate deformation; an eyeball estimate is that the modeled Pacific 'plate' has a strain of about 1 over its surface. The modeled Pacific is not rigid (contrary to what is observed to first order) but deforms as fast as it is moving. This means that the balance of forces, resisting and driving, and the constitutive relation used are entirely unrealistic. Any deformation that would be associated with the intra-plate volcanism really couldn't be seen in these models that get surface deformations that are so wrong.

There are problematic characteristics of the geodynamic model. The first is the simple homogeneous viscosity structure of the mantle in which the whole mantle viscosity is set to 10ˆ21 Pa-s which is inconsistent with a large amount of reasoning and observations in support of a rather strong gradient in viscosity from the upper to the lower mantle which amounts to about two orders of magnitude viscosity increase. This is important for the descent and force balance on slabs (and therefor plates) because once slabs reach a depth of 400 – 600 km, there is a strong resistance to plate motion. In the current model, the slabs are essentially daggling near the top of a fluid layer with little resistance beneath. This is one reason, but not the only one, why the 'plates' are stretching so much from ridge to 'trench' (i.e. the strain of one mentioned above). Another thing I don't understand is how the following two statements can be simultaneously true: "Each isosurface bounds a homogenous region characterised by an effective density and viscosity" (or Table 1 slab viscosity = 100 x mantle viscosity) and "The simplified rheology structure is free to deform visco-plastically". If the slabs are deforming plastically, then the viscosity inside the isosurface is no longer constant. This needs to be clarified.

I'm confused about how the sab structure is generated for each of the time segments that are studied. The authors state (P. 152, L.1-3), "We run four subduction driven models which start with surface reconstructions at 62, 52, 47, and 42Ma and include

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the previous 10 million years of subduction material as an initial condition." I found this to be ambiguous. Do they mean the whole time-dependent model starts at 72 Ma, or do they mean that the structure at 52 Ma, is built up from 62-52 Ma, etc.? This is critical because it relates to why the Pacific plate changes motion in their model. Does the buoyancy field for this reconstruction have any extra slab at 52 Ma? I think it does and I think that this is the reason why they have the Pacific plate changing direction. Subduction in the Izu-Bonin-Mariana started at 52 Ma (there is little uncertainty in this observation from detailed study of the IBM forarc) and the Tonga-Kermadec may have initiated at about 50 Ma. So, this must mean that slabs instantly appeared with some down-dip length and this is not at all plausible. Also, what happens to the Izanagi slab? There is a ridge between the subducting Izanagi and the Pacific in the NW Pacific that subducts circa 55 Ma in the Seton et al model, but now at 52 Ma, there seems to be a fully coherent slab connected to the Pacific plate in their model in the NW Pacific ocean. These are all critical aspects of the reconstructions which are ambiguous in the write up but yet control the model outcomes.

Other issues: P. 148. L. 10-11: "during a period of heterogenous plate tessellation" – I do not know what this means. Also, "heterogenous" is misspelt.

P. 152: L10-11, "basal drag (due to induced slab-suction) are the only significant model driving forces". Why is drag a driving force?

L12-14, "At this time, the pull due to Junction slab attached to the Pacific only originates from 4% global slab material". Ambiguous. Do you mean that the Junction slab has only 4 % of the driving force of the slabs at this time or 4 % of the driving force in general?

L. 19, Where does the 287 degree for the modeled Pacific motion come from since it is clear that the "Pacific" isn't a plate at all (i.e. it is stretching faster than it is moving).

P. 155: L. 9-12, "At times when the large subduction zones bound the Pacific Plate, motion in our model is well constrained and our velocity directions are consistent with kinematically derived plate motions of Seton et al. (2012)". What do you mean "well constrained"? Also how can you say that the directions are "consistent" when the model isn't even predicting motions within the Pacific with any degree of plateness?

L. 12-15, "However, the magnitudes of our modelled velocities are unrealistically amplified near major subduction trenches as that portion of the slab begins to rapidly descend, as such we normalise the vectors to the maximum velocity predicted by the kinematic reconstructions". I don't understand the normalization, this is a geodynamic model and it needs to predict both directions and correct magnitude of velocities.

P. 156 L. 1-3 "The location of the Euler pole quantifies the direction of rotation for a given plate and thus provides a good measure of correspondence between alternate models". This statement and measure of a single Euler pole for the model really obscures the fact that there is no one Euler pole that fits the motion of the Pacific region. In fact, if one plotted the Euler poles for each subregion with the boundaries of the Pacific plate from the model, the author would find a scatter of points that subtends a region that far exceeds the lat-lon boundaries of Figure. 6.

L. 17, "subducting slab topology is congruent in influencing plate motion changes". I do not know what this means.

Figures.

Fig. 2 The magnitudes of the velocity vectors need to be indicated. From the text, it seems to suggest that the kinematic models have explicit magnitudes and the dynamic model normalized velocities. If this is the case, then this is unacceptable.

I don't know what the shaded relief below the color scale represents.

"The aqua to magenta colour scale represents the non-dimensional von Mises Criterion of our model, with aqua representing minimal plate deformation and magenta representing the maximal deformation". This is an explicit mechanical model and so the convention is the actual quantitative values (presumably in MPa) should be given.

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“Numbers on the colour scale are derived from non-dimensional model displacements.” What numbers on the color scale? I couldn’t see any.

“The smooth, homogenous style of deformation is at the borders of divergent and passive margins is likely due to convection cells acting in the intervening space between plates” What convection cells? Is the high frequency information visualized is a model output, then my intuition would be that this is numerical noise, given the scale the of the mesh shown in Fig. 1.

Interactive comment on Solid Earth Discuss., 6, 145, 2014.

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