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

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We thank the reviewers and the editor for taking time to critically evaluate our submission. The manuscript has been greatly improved on the advice of the reviewers. Our responses to specific comments are below, with the reviewer comments italicised for context. The updated manuscript is attached as a supplement to the Reviewer 1 response for referral.

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.

It is expected that the intraplate strain is unrealistically high in our model, since we do not use a highly viscous plate core or the plastic effects that play a role in realistic plates. This implies that our modeled deformation is expected to be more diffuse than deformation in nature, and the force balances around the plate are unrealistic. The constitutive relationship of the material that we use for the plate can be regarded as unrealistic, but models of the semi-rigid rheology of the plates as part of a global model has so far only been applied to present-day plate motions (e.g. Stadler et al., 2010), never to sequences of models for the past. We therefore regard our model as a reasonable step forward. This has been expanded upon in the 'Plate deformation' section of the manuscript.

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 10^{21} 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.

We seek to simulate the slab pull force on the plates. For this we assume that a given convergence history between a subducting and overriding plate is related to the amount of slab material contributing to the pulling force. We use 10Myr of convergence history as an estimate for the amount of upper-mantle slab material that will contribute to the pull force (Conrad and Lithgow-Bertelloni, 2002; Billen, 2008). Assuming an upper mantle sinking rate of $\sim$50 mm/yr, this means with 10Myr of convergence history the extent of our pulling material will be around 500 km i.e well within the upper mantle. However, we do not impose a sinking rate, instead the kinematic model dictates the depth and amount of slab material. We find that some of this material is below 660km (a max depth of about 1000km). The model is essentially instantaneous, and we assume that the strong resistance to plate motion that the lower mantle would have, is not considered. Furthermore, viscosity in the transition zone drops due to high water content, as some recent publications suggest, and high resistance may not be expected. Also there does not appear to be compelling evidence from plate kinematics to show that such a strong resistance influences plate motions. It happens only in models that have constant viscosity in the upper mantle, and much higher viscosity in the lower mantle. But this is also only a 1D approximated model of the mantle, which is no more valid than many others. Viscosity scaling is a post-processing step, with the most important choice of parameters in the models being relative rheological values between the relevant isosurfaces (i.e. the slab and the mantle). This has been expanded upon in the 'Model Setup' section of the manuscript.

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).

We are not measuring the strain, but the von Mises yield criterion. This is determined from the strain in each model element, but is not a total plate strain. This is a measure of 'likelihood' for a plate to fracture. However we do not know the actual regional plate rheology (rather we only know the estimated model rheology). So, we can only infer where there are existing plate weaknesses (not implemented in the models, but discussed qualitatively) and where the value for von Mises is high then you are likely to get plate rupture (expressed through volcanism) especially if some other stress in/on the plate is induced. This has been expanded upon and reworded in the 'Plate deformation' section of the paper.

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.

The viscous deformation in the model mimics a complex flow and is described in more detail elsewhere (Grzhibovskis et al., 2011, Quevedo et al., 2010). The Stokes flow describing the fluid will find a solution that has a geometrical setting very different from the initial setup. This has been rectified in the text.

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 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.?

The slab structure is constructed from the relative motion of the plates in the plate reconstructions for the 10Myr prior to the model being run. For example, the 52Ma model, uses subduction history between 62–52 Ma to generate the slab structure driving this geodynamic model.

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?

Yes, the buoyancy field is driven by the 10 Myr of subduction history before 52 Ma.

I think it does and I think that this is the reason why they have the Pacific plate changing direction.

Absolutely, we argue that the changing slab morphology and amount of material can (at least partially) drive a plate motion change. We use plate kinematic reconstructions, which are ignorant of plate driving forces, as input and subsequently compare the geodynamic model with the plate kinematic model, so that we can isolate plate motion changes which are 'slab-driven' compared with plate motions which are inferred from hotspot migration.

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.

Yes, there is a ridge that subducts parallel to the trench. Undoubtedly this would have had to stop spreading by the time of its subduction. We take into account the highly thinned lithosphere around this area, but we consider it to be mechanically attached to the top of the slab. Thus, in the model, the western flank of the Izangi ridge is still aiding the pull-force acting on the Pacific plate at 52 Ma. This has been included in the test in the '52 Ma reconstruction' section of the paper.

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.

Thank you, fixed, and this sentence has been clarified to contextualise the general state of the plate configuration during this period. The reader is directed to Morra et al. (2013) for more information.

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

Changed to "Slab-pull due to slab material mechanically attached to subducting plates and the resulting slab-suction induced by these down-going slabs are the only signifi-

cant model driving forces".

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?

4% of the total slab material attached to all subducting plates at this time (assuming 10 Myr provides the slab history). This has been changed in the manuscript to read "At this time, the Junction slab attached to the Pacific consists of 4% of all global upper-mantle slab material." And the reader is referred to the Appendix where slab volume calculations are shown.

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).

For each modeled scenario and kinematic reconstruction, we report the mean direction of motion (summed over all velocity vectors) of the Pacific as a bearing toward North. The Pacific is moving and deforming. We now rectify this in the 'Model results' section, "For each model and kinematic reconstruction, we report the mean direction of motion of the Pacific as a bearing. Although the rigidity (the plateness) of the geodynamic modelled plate is low, we compare plate motions with the rigid kinematic models, as a simple test for whether are reproducing plate motions sufficiently."

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?

This has been reworded as follows, "At times when massive subduction zones bound the Pacific Plate, motion in our models is expected to better predict plate reconstructions, because the models are primarily driven by the pull of slabs mechanically attached to the down-going plate. Thus we find, the direction of motion of the Pacific plate is closer to resembling the kinematically derived plate motions of Seton et al. (2012) during these times."

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.

Here we use a similar approach to Morra et al. (2012). We originally did not attempt to match plate velocities by changing plate rheology or mantle rheology, as the number of parameters available would certainly allow us to match the available observables; instead we compare the modeled direction of motion with the reconstructed direction of motion. We report the mean direction of the Pacific plate's tangential model velocity vectors. In the original manuscript we normalised (scaled) the plate velocities to typical Earth-like plate speeds, after setting key parameters for the model rheology. However we note that an important step in the model is choosing realistic relative (as opposed to absolute) parameters. In our revised post-processing methodology we convert non-dimensional model parameters to realistic numbers, using the same renormalisation as Morra et al. (2010), $\eta/(\Delta\rho \cdot g \cdot r)$, where η is the mantle viscosity, $\Delta\rho$ is the differential density of the mantle and the External surface, g is gravity, and r is the radius of the Earth. We find a mantle-Earth differential density of $\Delta\rho$=60 kg·m^{-3}, and a mantle viscosity of η=1×10^{21} Pa·s best match kinematic reconstruction velocity magnitudes for the model parameters we have tested.

Later we compare matching plate motions of the deforming modeled plates (not being rigid) and the rigid kinematic reconstructions by calculating stage rotations of the entire Pacific plate for all model times.

In this way we aim to characterize which plate motions are compatible with the modeled slab pull and which are not.

This is now clarified in the "Kinematic vs. geodynamic model plate motions" section of the paper.

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.

Because of this limitation we offer the two comparisons of plate motions (as above). The stage rotation for a given geodynamic model is the best-fit Euler pole of the entire motion of the plate derived from the modelled plate motion vectors. As the Pacific is deforming in the model, indeed there would be a spread of Euler poles that describe different areas of the plate's rotation. This limitation has been discussed in the "Kinematic vs. geodynamic model plate motions" section of the manuscript now also.

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

Changed to "revealing that the subducting slab topology is an important factor in dictating plate motion changes."

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.

We originally left out the magnitude because we were more concerned with modelling the direction of plate motion. However, now we match both the velocity magnitudes and directions of the geodynamic model to the velocities of the kinematic reconstructions. This is now discussed in more detail in the text, as mentioned above.

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

Thank you for picking this up. Numbers were removed from the scale as they imply we know the actual von Mises values, which would be true if we knew the actual properties of the plate. But because we have such a simplified model setup and only seek to identify regions of lithosphere likely to yield (stressed by slab pull), we instead used "High" and "Low" markers to reflect that we do not know the actual stress/deformation in the plate from these models. We now provide a dimensionalised (40-400 MPa) range of the model von Mises values, based on the chosen model parameters, and discuss the limitations of doing this. This is discussed in the text (Section 'plate deformation') and references to the deformation have been clear we are talking about the likelihood to yield (and not the strain).

"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.

This has been further investigated and is a post-processing artefact owing to using the reconstructed topology as a boundary for modeled output. As the modeled Pacific has actually moved and subducted, and no new material is produced at mid-ocean ridges,

the grid of the von Mises values is interpolated to the reconstructed edge of the Plate boundary.

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