

Interactive comment on “Can subduction initiation at a transform fault be spontaneous?” by Diane Arcay et al.

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General Comments

Arcay et al. present a parametric study of spontaneous subduction initiation, using numerical models, with a view to constraining the conditions required for this process to occur. This work comes at the perfect time. “Spontaneous” subduction initiation is being used as a mechanism to explain many features of the rock record around subduction zones in many recent studies, in a number of scientific areas. A full parameter study of the dynamic feasibility of this mechanism has not yet been undertaken and as such, I believe this work to be very important and will be useful to many. The modelling has been undertaken carefully and rigorously. The authors have checked a number of

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modelling assumptions that they have made to see whether they would influence their results, and discussed the others. As such, I believe the results of this study are robust. The majority of the conclusions drawn towards the end of the manuscript summarise the results well and are fair. However some are perhaps too strong in places (specifically with regards to the Izu-Bonin-Marianas system, or IBM). I am not convinced that this study implies that subduction initiation via older-plate-sinking is impossible, simply that it has highlighted that it requires very particular conditions and perhaps a mechanism to weaken the top of the sinking/bending plate as it progresses. I agree that the study of all recent subduction initiation events implies that all (but one) do not fit the spontaneous model (especially given the specifics of how spontaneous initiation occurs in this study) and that this is a significant observation. However, the IBM remains a stand out for many reasons and it seems likely to me that this is because the IBM is the only example of spontaneous subduction initiation in this set. It would explain why fore-arc-basalt is only found at the IBM for one. What this study has done for me is put hard limits on what conditions must have been like at the time of initiation at the proto IBM, rather than the other way around. It has also demonstrated how dynamically unlikely, and therefore rare, this type of event must be. These are still very powerful conclusions. This is actually what you glean as a reader from reading the current abstract already, so this is good. The careful consideration of the limits of “reasonability” of the parameter space is something that is of particular note in this paper: it would be great to see such a method adopted in all geodynamic parameter studies! A particular criticism I would have is towards the language used through the manuscript. This makes the manuscript difficult to read and my fear would be that it would put many people off attempting to do so (a shame when the science is good). I am not able to go through and correct the grammar, word choice and sentence structure throughout the entire manuscript as this is a lot of work. There were also a few places where I found it difficult to assess the science due to confusing use of language (I was brought close to suggesting that my revisions are “major” because of this). I would strongly recommend that the authors seek help from a native English speaker or a professional translator.

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Given the importance of this work, and the care with which it has been undertaken, I would recommend this manuscript for publication, provided the comments below are at least considered and the language is corrected throughout.

Specific Comments (by section)

Abstract The abstract contains everything that I believe it should and is structured well. However, like the rest of the paper, it suffers from the confusing use of language. Some examples just from the abstract: “We propose a new exploration of the concept of “spontaneous” subduction” – “We present a parametric exploration of the feasibility of “spontaneous” subduction initiation”? “in recent subduction initiations” – “from recent subduction initiation events”? “The basic parameters to simulate OPS are” - “The parameters which exert the strongest control over whether OPS is feasible or not are. . .” ? Etc. In addition: “We find that all mechanical parameters have to be assigned extreme values to achieve OPS, that we consider as irrelevant” – It seems in the paper than the parameters don’t all simultaneously have to be set to extreme values? Also “irrelevant” would imply that this is an unimportant result, when it is really one of the key results of this paper! Is this what the author intends to write here? I would argue that it is very relevant.

Introduction This introduction is a very thorough overview. In terms of content, I have very little to add. Just one small comment/question: is it uncontested that the forearc of the IBM has been consumed by subduction erosion? There are many studies which assume otherwise. I would perhaps reword this part to reflect this. Figure 1 is clear to me.

Model Setup 2.1 Does the code have a name? Fig 2: It might be good to put the meaning of symbols used ($L_w(A_o)$ etc.) and it might be good to label isotherms (perhaps just in the inset?). Why is the 1400 isotherm so irregular? Is this the initial condition?

2.2 Is the method of using a conductive lid with a constant thermal gradient really valid for young plates? Is the value of 0.75, for the “overcooling” correction grounded in

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anything? If it is then it is probably worth mentioning.

2.5 I like the summary figure 3. These are not all the parameters varied however. Would it be possible to encompass the fact that the asthenospheric temperature, width of thermal step and the presence of a plume were also tested here for completeness? **2.5.1** Gamma c is close to 0.08, not 0.8 using this equation. However, forgive me if I am wrong, but I do not see where the term $(1-w)$ comes in. Anderson theory of faulting gives us: $\Delta\sigma_{xx} = (2f_s (p_{lith} - p_w)) / (\alpha(1 + \alpha f_s^2) \alpha^{(1/2)} - f_s)$ And $\lambda = p_w / p_{lith}$ so surely. . . $(\Delta\sigma_{xx}) / p_{lith} = (2f_s (1 - \lambda)) / (\alpha(1 + \alpha f_s^2) \alpha^{(1/2)} - f_s)$

This also makes more sense to me when thinking about the mantle, where you would expect no pore fluid so you rightly use $\lambda=0$. In your current equation, why should w play any role in this case? Of course using this line of reasoning assumes an interconnected fault network within the material considered. I do not see a problem with this (in the crust at least) as the author is searching for the lower bound limit here, but I think that this is worth stating. With regards to explaining the low brittle parameters for the mantle, see my comments below discussing Peierl’s creep. **2.5.3** The author has made the effort to correct for the fact that different studies use different stress exponents but not corrected for the fact that different studies use different rheological prefactors. These prefactors effectively normalise each flow law and as such, the activation energy and rheological prefactor cannot be thought of as independent. In general, experimental flow laws with higher activation energies have lower rheological prefactors and vice-versa. Therefore here, the author is likely significantly over-estimating the variability in experimental flow laws (a better way of doing this is to take all the experimental flow laws one wishes to consider and finding their average and standard deviation and using these as bounds for example). If the author has applied a form of normalisation, either to ensure a constant upper mantle viscosity (which I know is commonly done) or with the original experimental flow laws in mind, then there is no problem, although I would ensure that this is made clear in the text. Side note: I see that the effect of

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the crustal activation energy is very limited in the results section, so if running these models again is necessary, but difficult, then perhaps it is worth leaving out the investigation of activation energy? 2.5.4 The last paragraph would be a good introduction to a whole new section as from here on in as it seems like the rest of section 2 is now results and not model setup. I would personally just call this section “Results”.

2.6 I would make clear that the 65% are non-OPS. The last “almost OPS” mode paragraph is very confusing. It would be better to say that “in 40% of models which appear to start to show OPS behaviour, freeze up within. . . .” Or something similar, rather than talk about these models as if they are proper OPS.

2.8 Fig 6: This regime diagram is great. It might be useful to have points on the diagram corresponding to the actual models run. I feel the individual sections below would benefit from having their own regime diagram where the parameter being looked at has one of the axes (eg. for 2.8.4. it would be good to see how the critical mantle brittle parameter varies visually). However, I do understand that having hundreds of regime diagrams is not useful and it is difficult to put them together for such multi-dimensional results. 2.8.2 “The aforementioned results are obtained when crust weakening is supposed to be localized at the TF only ($L_w = 0$ km).” Some of the non-OPS mode examples in figure 4 clearly have $L_w > 0$ Does “aforementioned” just refer to section 2.8? 2.8.3 What is L_w in this case? 2.8.4 This is a great point. There is another mechanism that would help facilitate plate bending and that is Peierl’s creep. Including this mechanism may have a similar effect to decreasing the mantle friction coefficient. This is perhaps a point for the discussion, but I think it is worth bringing up. 2.8.5 I find the result that changing the ductile strength of the crust and TF has little effect unsurprising as these regions are most likely to deform in a brittle manner in the case of subduction initiation. This and the fact that the only time that changing the activation energy has any effect is when the plates are effectively crustal plates, would indicate to me that changing the ductile behaviour of the mantle, and not the crust, would have the larger effect and is the more worthwhile investigating. If it comes to re-running models, then I would

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consider looking at this instead (although I should say that there is technically nothing wrong with it as it is!). The plume head having little effect is a very interesting result, particularly as many people invoke the influence of plumes to catalyse spontaneous subduction initiation. I know this section is short, but I would say it deserves its own heading. 2.8.6 It would be good here to emphasise that the brittle parameters were inverted for models which originally displayed OPS, and then do not after the inversion. It took me to read the supplement to understand this. Likewise, it would be good to emphasise that the models being looked at when increasing the fault width, originally did not demonstrate OPS. The fact that OPS occurs independent of the width of the step change in thermal profile is a very interesting result!

Analysis 3.1 Surely the important criterion for mode 2 to occur is for the younger plate to be weak enough to stretch or break and therefore move with the sinking older plate? What was it that led the author to believe that it was more to do with coupling to the asthenosphere? Is there an aspect of the model set-up which means that the YP is always free to move? If there is a reason then it would be good to clarify this in the text.

3.2 Apart from the wording, this section is clear.

3.3 I am glad that the author discusses the free surface here. Perhaps this is the point at which Peierl’s creep could also be mentioned? I would also argue that the concluding sentence here is quite strong. As the rest of this section alludes to, the primary parameter that needs to be tuned “beyond a reasonable value” is the width of the weak layer at the top of the model. The process suggested by Dymkova and Gerya 2013 surely offers a mechanism by which this weakening could happen? I personally see this result emphasising the need for such a weakening mechanism, rather than suggesting that OPS is impossible.

3.4 Again, apart from the wording, this section is clear. However, I would add that Reagan et. al 2019 has suggested that subduction initiation really did occur in 0.5-1 Myrs at the IBM (given the very short duration of fore-arc basaltic magmatism).

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Discussion 4.1 Spontaneous initiation would also be easier in 3D simply due to the extra degree of freedom. For example, the model by Zhou et al. 2018 suggests that the sinking plate is able to sink in one place initially and then subduction initiation propagate away from this point. This takes far less energy than requiring that the whole older plate sink along the entire transform fault simultaneously (what is effectively modelled when modelling in 2D). I agree that permeability through the mantle is likely lower than Dymkova suggest, and this is a very good point to raise here (although I do not think that it necessarily negates my comment for section 3.3). Another feature common to models of initiation, not included in this study, is a strain history dependent rheology (damage). I do not actually think that it would affect the results of this study significantly, though I would say that it is worth a mention at this stage.

4.2 This section is clear. However, the conclusion of Reagan et. al 2019 (the most recent study informed by the most recent drill core data) is actually that subduction initiation must have occurred very rapidly (<1 Myrs). In this case, the modelling results presented in this paper are not at odds with subduction initiation having been spontaneous at the IBM. However, I do agree that post-initiation velocities in the models presented in this paper are unrealistically high once subduction is established and this remains an issue. I would argue that these unrealistically high velocities are, at least in part, the result of 2D modelling: in 3D the subduction zone would “unzip” more gradually. The plate that has not yet started sinking would prevent the sinking part from reaching such high velocities.

4.3 I particularly like how this study presents “failed” or “aborted” subduction initiation events as existing on a spectrum with the successful ones. If anything this could be emphasised more!

Conclusions The conclusions are well structured and summarise all the key points. I would perhaps also mention a few of the other strong conclusions that can be drawn from this study: the thermal blurring having no effect; an incident plume having little effect etc. The only other recommendation I would have is that the second to last

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sentence is worth rewording/softening; especially as it would seem that the geological record is not necessarily at odds with the catastrophic mode simulated in this study (see general comments).

Tables All three tables are very valuable. If feasible, Table 3 would really benefit from colour-coding (given its scale!) although I am aware that this may not be possible.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-63>, 2019.

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