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Interactive comment on "Breaking supercontinents; no need to choose between passive or active" *by* Martin Wolstencroft and Huw Davies

Anonymous Referee #1

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This paper deals with the topic of (super-)continental break-up in a global – i.e. mantle dynamics – perspective based on spherical mantle convection computations. The more specific focus is the "ambiguity" of passive and active mechanisms in this process and the fundamental importance of sublithospheric return flows induced by dynamic processes, here mantle downwelling avalanches. I think this work generally touches on an important topic in geodynamics, particularly for our understanding of the interplay between mantle convection and surface tectonics. Consequently, Solid Earth is an appropriate journal for publication of this work.The manuscript is generally easy to follow and in most cases the argumentation of the authors is clear. However, I do have some moderate concerns related to the originality of the proposed concepts, the

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choice/setup of the numerical simulations and their analysis, and the integration into existing geodynamical concepts. I think these concerns (which I detail below) require moderate revisions before publication of the manuscript.

Specific comments:

1.) The general idea behind this paper (no clear distinction between active and passive break-up mechanisms) seems not really novel. In fact, it is somewhat trivial that any break-up process requires extension in the lithosphere (how else would you do it?). The real point, however, is the cause for the extension. It could be induced by mantle upwellings (e.g. plumes) or alternatively by "far-field" processes, e.g. some more or less remote subduction (see e.g. Bercovici & Long, 2014). I think this has to be clarified throughout the manuscript including the first sentence of the abstract.

2.) Just 3 convection simulations are presented. Ok, such calculations are computationally very expensive, but this is still a little disappointing. Moreover, 2 of the 3 models are isoviscous cases whose geodynamic relevance is very limited as they e.g. fail to generate a strong lithosphere. However, many geodynamic studies have demonstrated that surface strength is very relevant for breaking the lithosphere (if continental or not). To just list a few: Yoshida (2008, GRL, 25, L23302), Rolf et al. (2014, GRL, 41, 2351-2358). Only case 3 thus seems to have good geodynamic relevance, although 2 orders of magnitude viscosity variation is probably not enough to describe an Earth-like convection regime (Solomatov, 1995, Phys. Fluids, 7, 266-274). In addition, when I look at the time series of case 3 in Figure 1, I'm quite unsure if it has reached equilibrium or is still in some sort of transient state. How can you be sure about that? Just based on the heat flow evolution?

Finally, it is unfortunate to see that the analysis jumps directly from isoviscous cases to one with both layered AND temperature-dependent viscosity, rather than including a case with only layered, but NOT temperature-dependent viscosity, just to see the effects of that. Do you expect that the lateral variations induced by temperature-dependent viscosity (which finally determine the strength of sinking slabs) are important for avalanches to occur or not and thus have implications for your discussion?

3.) The chosen Clapeyron slopes are extremely large. You explain this choice by the reduced convective vigour in the models. For the high Ra case, however, surface heat flow is $\sim$85 TW, i.e. roughly 2x Earth's, so convective vigour actually seems higher than Earth's. For the other 2 cases, heat flow is lower than Earth's, but not much. My point is that to make avalanches possible in your model you seem to need very large Clapeyron slopes and I don't think the argument of reduced convective vigour can fully explain that (unless the heat flow comparison is not a good proxy for convective vigour here?). As far as I understood, your model does not feature a density jump across the 660. If you include that, may it be easier to pile up cold material on the 660 using smaller Clapeyron slopes? To be clear, I don't ask for additional cases with smaller Clapeyron slopes here, but I encourage the authors to discuss the shortcomings of their model setup and how they may affect their conclusions in somewhat greater detail. This does not only include the Clapeyron slopes, but also the omission of e.g. compositional variation and the rather small lateral variation in viscosity (see above).

4.) Given that only a very small set of cases is concerned, I hoped to see some more in-depth analysis of those cases at least, but the only presented diagnostic is the average surface heat flow evolution. However, the main physical processes used in the argumentations are a) mantle avalanches and b) return flows. For the former, it would be interesting to see a diagnostic such as the radial mass flux through the 660 as already suggested in Figure 2. The return flow may be more difficult to quantify, but perhaps horizontal velocities at some shallow depth range are an option. Considering the time evolution of such additional measures could clarify causalities here and could even give an idea about how much extensional stress may be induced to the lithosphere (shear stresses/tractions at the base of the lithosphere?).

5.) Again, concerning the conservation of mass: I agree that return flows are required in the described geodynamic settings. However, what is perhaps less known is the wavelength over which they occur. Here, your models indicate the longest possible wavelength (i.e. degree 1). Later on (Figure 4, lines 181-186), you relax this view by comparing to Zhong et al. (2007), however, this deserves more discussion. What may control this wavelength, etc.? Personally, I find your degree-1 concept (e.g. Figure 4A) problematic, because it seems difficult to have a stationary supercontinent antipodal to a persisting complex of surface convergence (which seems required to pile up material on top of the 660). Instead, I would expect the continent to either move to this convergence zone and/or to break-up before an avalanche may occur. In a shorter-wavelength flow like degree-2, however, the supercontinent may be kept stationary, e.g. by surrounding subduction. This may be discussed a little more.

Further minor comments:

- line 12/13: “For non-global . . .”. This sentence is not really clear (at least at this point). What do you mean with “the geometry of the mantle”. The flow pattern?

- line 40: You may want to consider the work of Brandl et al. (2013) in your referencing, which discusses evidence for elevated temperatures below Pangea from a data-based approach: Brandl et al., 2013, Nat. Geosci., 6, 391-394.

- line 48/49: “Storey (1995) concluded . . .” This sentence is unclear to me. Do you mean that parts of the Gondwana break-up occurred with volcanism while other parts did not?

- line 60ff: Here, it seems relevant to add some discussion about the likely role of continents in plate-mantle coupling. After all, thick continents are likely to increase e.g. the magnitude of shear tractions at the base of the lithosphere (e.g. Zhong, 2001, JGR, 106, 703-712, Conrad & Lithgow-Bertelloni, 2006, GRL, 33, L05312). So, the presence of continents may in a way help to induce stresses in the lithosphere that eventually cause their own break-up.

- line 73ff: Regarding the model description: What is the numerical resolution used in the TERRA models? Also, I think you should state clearly that while you are interested in continental break-up, continents are not explicitly included in your model.

- line 77ff: Is there any density increase associated with the phase transition at 660 km (see my comment 3 above)? It is also worth to note that you ignore all other phase transitions.

- line 80ff: When defining the Rayleigh number, you use “kappa” (thermal diffusivity), but you don’t give its value in Table 1. Ok, it is straight-forward to compute it from the other parameters in that table (kappa = k/rho/c_p), but I suggest to either give this relation somewhere or to list kappa in the table explicitly.

- line 109, This sentence describes the spikes in surface heat flow in Figure 1. While these are easy to spot for case 1, case 2 does not feature them clearly nor does case 3, which seems to feature only one peak, but then does not seem to have the same equilibrium state before and after the peak (heat flow is quite different). So, are these heat flow peaks really characteristic for the discussed regime?

- line 152-155: This paragraph is extremely short and quite speculative. I don’t think it is worth to call this a separate section. Since my other comments probably require additions to the manuscript, I actually suggest deleting this paragraph. If you want to keep this section, however, it should be extended and more explicitly linked to the discussed models.

- line 167/168: How do you actually estimate from your results that the events take 10s of Myr? Just from the presented heat flow curves? Also, please add a reference here when you mention that this is comparable to Earth’s timescales.

- line 178: I suggest to add a reference to Yoshida’s works here, too (e.g. Yoshida & Santosh., 2014, Geosci. Frontiers, 5, 77-81).

- Table 1: Consider to include the symbols for the physical properties used in eq. (1) in the table, e.g. alpha for the thermal expansivity. I would also appreciate values for the reference viscosity, right now it is only given implicitly via Ra.

- Figure 2: I think this figure would benefit from a time series of the “absolute radial mass flux” similar to the one of surface heat flow as already presented. Ideally, this should be done Figure 1, too, because then the reader could get an idea of how a mantle avalanche is linked to the surface heat flow spikes and perhaps get an idea about timescales, which would improve the (very short) discussion in section 5.3.

Some very minor suggestions:

- line 8: “long-range” –> ”long-wavelength” (?)

- line 74: “The values” –> “The parameter values”.

- line 93: “set with” –> “set up with” (?)

- line 101: “by the temperature change” –> “by the superadiabatic temperature change”

- line 107: delete “slightly”

- line 175: Check sentence structure: “including by diking”?

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