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Interactive comment on "The role of Edge-Driven Convection in the generation of volcanism I: a 2D systematic study" *by* Antonio Manjón-Cabeza Córdoba and Maxim Ballmer

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Dear Editor, dear authors,

The manuscript "The role of Edge-Driven Convection in the generation of volcanism I: a 2D systematic study" by Antonio Manjón-Cabeza Córdoba and Maxim Ballmer presents a useful contribution to the lively debate about the potential role of edge-driven convection in the formation of intra-plate volcanism. This debate, triggered by the influencial work by King and Anderson (1998) illustrate the enigmatic features of

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intraplate volcanism, such as the Canary Islands in the east Atlantic, and the complexity in explaining it using geodynamic models. This work provides a clear, systematic and well-written contribution. I would very much like to see this work published, but have a few issues that I think should be addressed, and would make this work stronger.

The thermal initial condition of the models seems to be chosen rather arbitrary. They are probably based on typical observations of thermal thicknesses of oceanic and continental lithosphere, without considering the stability of such lithosphere for the given choice of rheology and buoyancy. Indeed, most models rapidly develop a different geometry after initiation of the model, which illustrates that they were not stable to start with. I am not suggesting to change this: in fact, I think it is important to illustrate the dynamics of EDC over the wide range of physical parameters, even if it would lead to rapid initial model parameters. But a discussion on the choice of thermal initial conditions is probably useful.

The models don't have a bottom thermal boundary layer, as the initial temperature and boundary conditions follow the adiabat. This suppresses (large scale) convection and probably leads to long-term cooling of the model domain, and therefore suppression of any intraplate volcanism. The authors ran models with increased internal heating, where more melting is observed. Kaislaniemi and van Hunen (2014) also applied such increased heating to counteract cooling due to absence of a bottom TBL, and to create a long-term steady average mantle temperature, and I suggest to add such argument to justify both the lack of a bottom TBL and increased internal heating.

Crustal buoyancy is achieved through using the same mechanism that is used for depletion buoyancy, using the parameter F. So depletion buoyancy and crustal buoyancy are lumped together in the same mechanism? With Delta rho_F = -100 kg/m3, this suggests that crustal buoyancy is only 100 kg/m3 for F=1. That is lower than the real buoyancy of crust. Perhaps this isn't important, as all is needed is to have it buoyant enough to keep it floating. Can you please comment on this a bit further?

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Throughout the manuscript, EDC and SSC are treated as separate phenomena, and indeed, they are. But the transition from one to the other is not always clear. Do you use a clear definition on what to call EDC and what SSC? I think it would be useful to add a sentence here that clearly defines these two modes of convection.

In the models, the rheology is chosen such that oceanic lithosphere of a certain age starts to undergo convective instabilities (SSC). This makes sense. But continental lithosphere has the same rheology, and, being so much older, would therefore never be stable. This can be seen in Figure 3. Other authors used a stronger rheology for continental mantle lithosphere to prevent this. Could the authors elaborate a bit on what would be needed to prevent this in their models?

Smaller comments and typographical errors are highlighted in an attached annotated version of the manuscript.

Jeroen van Hunen Durham, 30 August 2020

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-120/se-2020-120-RC1-supplement.pdf

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