



EGUsphere, referee comment RC1  
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## **Comment on egusphere-2022-15**

Russell Pysklywec (Referee)

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Referee comment on "The role of edge-driven convection in the generation of volcanism – Part 2: Interaction with mantle plumes, applied to the Canary Islands" by Antonio Manjón-Cabeza Córdoba and Maxim D. Ballmer, EGU sphere,  
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This is an interesting contribution that extends previous work of the authors to 3D models, where they explore the interaction between plume and edge-driven convection (EDC), with implication for features in the Eastern Atlantic. In general, the paper is well-written (minor suggestions indicated below). The focus of the paper is quite narrow, but I think it considers some mantle dynamics in EDC and plume behaviour that would be of interest to a broader audience.

I appreciate that the authors are upfront about referring to this contribution as a "companion" study to their previous (2021) work. I think it's fine to write a paper like this, although it does reduce the novelty of the work somewhat--e.g., when this research is portrayed as an extension to the previous work even in the abstract. There are a few places where I think the authors rely too much on a citation to the previous study--detailed below--that should be expanded on.

There are a number of relevant edge-driven convection studies that have been done, but aren't cited here. For example, similar questions on the role of EDC in the western Atlantic have explored features of topographic elevation, volcanism and elevated heat flow. I suggest that a paragraph be added to the Introduction to discuss some of this work: Vogt et al., *Geology*, 1991; Shahnas and Pysklywec, *GRL*, 2004; Conrad et al., *Geology*, 2004; Ramsay and Pysklywec, *J. Geodyn.*, 2011. This would help fill in the discussion on EDC, but also expand the application and broaden the implications for the authors' work.

Line 60: "We conclude that many of the discrepancies... We also find..." I don't think it's necessary or appropriate to put the conclusions in an Introduction section, and would suggest they be deleted here.

Line 73: Is it necessary to say "Kinematic boundary conditions are similar to those in the companion paper." (Again, the manuscript is already full of call-backs to the 2021 paper.) Suggest to delete.

Line 108: Here the authors refer readers to the 2021 companion paper for the density and viscosity formulation. I think these should be included in the present manuscript, rather than just referred to. Many readers will be familiar with all the parameters listed in Table 1, but others won't be. The density and viscosity formulations will give these parameters context.

Line 130: The authors mention that the plume migrates to a dent that is either created by EDC, or the active action of the plume, but leave this unanswered ("...it remains unclear..."). This seems exactly like the type of question this 3d dynamic models should answer: is there some reason why they didn't investigate the behaviour? e.g., prior to the plume impinging, was there local ocean lithosphere thinning?

Line 132: Similarly, the unresolved question on the asymmetry seems like something they could/should answer with the models. The opportunity is there (unless I'm missing something with the modelling approach) it seems odd to leave it unanswered.

Line 277: Fix to "We ran a wide range..."

Line 288: Fix: "distance of the plume to from the edge."

Line 302: Fix to "Therefore, a subset of our models..."

Line 305: Fix flux units:  $4 \times 10^2 \text{ km}^3 \text{ Myr}$

Figures 3, 5, 7: The cropping and dimensions are a bit confusing. (Is the thin black line the bottom boundary on the front face?; how does this reconcile with the depth of the left and right side wall boundaries?) Some appropriate labelling of the x-y-z coordinates would help guide readers on these.