Title: The role of edge-driven convection in the generation of volcanism – ii – interactions between edge-driven convection and thermal plumes with application to the Eastern Atlantic.
Journal: EGU Sphere
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Reviewer: Rhodri Davies – The Australian National University

In this manuscript, authors use a 3-D geodynamical modelling approach to investigate potential interactions between mantle plumes and edge-driven convection. Based upon their modelling results, they subsequently link the dynamics and synthetic melt predictions to the intra-plate volcanic record of the Eastern Atlantic Ocean (particularly that associated with the Canary hotspot). The topic will clearly be of interest to a broad audience and the journal's readership.

Despite this, I feel that the paper suffers from a few major shortcomings, in addition to a lack of clarity in places, which should preclude publication in its current form. Although I outline several issues below, my suggestions amount to a major revision: I feel that addressing these points, and adding further clarity and support around major conclusions, will yield an important contribution towards better linking the volcanic record at Earth's surface to dynamical processes within its interior, particularly in the vicinity of step-changes in lithospheric thickness.

Hopefully these points are useful and will allow the authors to improve their study. Thank you for the opportunity to review this work, and my apologies for being a little slow.

Rhodri

Main comments

1. The interaction between edge-driven convective (EDC) cells and mantle plumes occurs both ways, with plumes likely modifying edge-driven cells and cells potentially influencing plumes. Although the authors quantify how plumes (both the conduit and pancake) are deflected during plume ascent in the vicinity of lithospheric steps, there was very little (if any) quantification about how edge driven cells behave prior to, during, and after, plume interaction. In other words, the study focuses on one aspect of the interaction between plumes and EDC, but does very little to shed light on other aspects. My expectation would be that the dynamics and melting expression of the cell adjacent to the lithospheric step changes quite dramatically upon interaction with a mantle plume, and this would have important manifestations in the geological record. However, the paper did not analyse this which, to me, is a major shortcoming: how can you examine the interaction between edge-driven convection and mantle plumes without quantitatively demonstrating how edge-driven convection is affected? Given that this paper builds squarely on the authors previous work (where 2-D edge-driven cells were examined in isolation), it is very important to quantify how results differ to those of that previous study: only by doing so can a reader really understand the role of a plume in this scenarios simulated in the paper. I'm left wondering: how, exactly, do plumes modify edge-driven cells? How does this interaction change with time? How is this manifest through melting and what are the potential implications of this for volcanic composition and volume? I'd strongly recommend that the authors compute some diagnostics that show, more definitively, how these two important melt-generation processes interact: this would really add value to the paper, as not many studies have examined such interactions.

2. I am not convinced by one of the paper's main conclusions, specifically that the ascent of plumes is modified by EDC. I am not doubting the authors results that a plume is deflected during its ascent, generally from beneath the continent towards, and away from, lithospheric steps. My uncertainty comes from what is causing this deflection. Unless I'm missing something (which is entirely possible!), given how the models examined have been set up, there will be a pressure gradient driving flow from beneath the continent (thick lithosphere) towards the oceanic realm (thinner lithosphere), which will be sufficient, in many cases, to deflect plumes in that direction. This is very different from a small-scale, shallow instability at the lithospheric step (i.e. edge-driven convection) inducing this deflection. With this in mind, I am left wondering what is causing plumes to deflect during their ascent in the models shown? Is it the larger-scale pressure gradient, or the shallow flow regime adjacent to the lithospheric step – i.e. EDC? I feel that the authors need to pull these potential mechanisms apart, to provide more support to their conclusions that shallow edge-driven convection is sufficient to deflect a plume. At what stage of its ascent does a plume start to deflect? It seems from the plume stem diagnostics shown and the 3-D snapshots provided that this happens at depth, which (at least to my understanding) fits better with the pressure gradient driving the deflection. The results highlighted on line 289-291 (for thicker continents) are also consistent with the pressure gradient being a key factor. One potential avenue that authors could use to pull these contributions apart would be to run an 'instantaneous flow model' where the thermal and compositional fields in their reference model remain fixed, and flow velocities are computed in response. Is flow driven towards the oceanic realm in this scenario? If so, what are these velocities relative to the ascent velocities of the plume? If these velocities are negligible (as I said, I could very well be wrong) and it turns out that shallow EDC is the main driver of plume deflection, I feel that a careful explanation of why this is the case would really add weight to the paper.

3. Given the manuscript title, I was expecting more background to the volcanic record of the Eastern Atlantic, as, ultimately, this is what the models were set up to understand. What is it about these volcanic provinces that is inconsistent with the mantle plume hypothesis and why? I feel that the manuscript falls short in this regard. If the authors really want to focus on the Eastern Atlantic, more background to regional volcanism should be provided, providing more context for a non-specialist reader. Saying that, the results of this paper are potentially also applicable to other intra-plate volcanic regions such as South America and Australia where interactions between plumes and lithospheric steps have been postulated (e.g. Davies et al. 2015, Rawlinson et al. 2017) - so the paper could potentially be expanded to include such regions, with less of an emphasis on the Eastern Atlantic. Obviously this is the authors decision - but both will be of interest.

4. In its current form, I would not be able to go and reproduce the results in this paper: the models are generally too briefly described. Yes, the authors refer back to their previous study, but I'm not a fan of having to dig out another paper to find some key model information. At the very least, the authors should provide more of a summary of how each component of their models are set-up in this paper (with only the in depth information restricted to the previous paper): I found some of this key information lacking (discussed further below).

5. The limitations of the models and how these may impact results need to be discussed. As with all models, there will be shortcomings and we have to make assumptions, but these should be highlighted to a reader. They can also be used to identify important avenues for future research. The authors are more qualified than I am to identify these limitations, but some aspects that I would recommend covering are: (i) models are 3-D which is great – however, the step geometry is essentially 2-D, extending across the entire length of the 3-D domain. The model therefore misses some 3-D complexity that likely exists on Earth and this should be acknowledged; (ii) melting model – I like the model used and it has some nice features, such as multi-component melting. However, please spell out its limitations for a non-expert (for example, do you consider reactions between pyroxenite melts and adjacent mantle? These are likely to be important.).

6. Results should really be better placed in the context of existing literature. There are a number of studies that have examined edge-driven convection and shear-driven upwelling. As you point out, fewer studies have examined the interaction between these processes and mantle plumes. Most of the key studies are cited, although not really discussed, whereas others are not cited or discussed. For example, the study of Duvernay et al. (2021), which you cite, whilst generally agreeing with the 2-D results of your previous study, can, in places predict melt fractions that seem compatible with some of the Eastern Atlantic volcanics quoted in your paper: part of the differences being due to 3-D complexity in lithospheric geometries incorporated in their models. This should be pointed out, so that a reader better understands the uncertainties around the modelling side. I think reviewing some of this literature and showing how your study builds on, complements and improves on earlier work, is important. These are a number of new, important and potentially very exciting findings in your study: for a reader to appreciate these, they need to be placed in the context of existing literature. The studies that that spring to my mind are (Demidjuk et al. 2007, Farrington et al. 2010, Davies & Rawlinson 2014, Afonso et al. 2016, Rawlinson et al. 2017), although I note that other reviewers have suggested some more (some of which I was not familiar with and will be reading myself!).

Minor points

1. Line 24 – it is stated that 'several predictions of plume theory are not fulfilled at many locations worldwide'. What aspects, specifically? Spell them out. I note that a number of studies demonstrate that thermo-chemical plumes can have a complex surface manifestation (e.g. Farnetani & Samuel 2005, Dannberg & Sobolev 2015) (in addition to some of Maxim's own work) whilst plumes simulated in a spherical geometry at realistic Rayleigh number can explain many of the complexities traditionally deemed inconsistent with mantle plume theory (e.g. Davies & Davies 2009). There are obviously other aspects of the volcanic record that seem inconsistent with mantle plumes, even when these complexities are taken in to account, and I agree that they are, but spell them out for a non-expert, so that they, and others in the community, better understand the motivation for the important work that you're doing (allowing them to better see the novelty in your paper).

2. Line 42 – 'in theory, the return upwelling flow would be enough to generate magma to sustain ocean island volcanism'… *provided that the overlying lid was sufficiently thin to facilitate decompression melting.* I think the additional qualifier is important, particularly for a non-expert.

3. Line 46 – 'very' is superfluous here. The Duvernay et al. (2021) study shows that EDC (and SDU) can account for many of Earth's lower volume (and potentially shorter lived) volcanic provinces – saying that magmatism is 'very' restricted could therefore give a false impression. It is markedly less than the magmatism induced by an upwelling plume, admittedly (as demonstrated in the more recent paper that is currently under review at G3: Duvernay et al. (2022)), but melting nonetheless remains significant.

4. Line 56: whilst it is true that not many studies have examined plumes interacting with EDC, some studies, by for example Koptev, Burov, Gerya, have carefully examined plume lithosphere interaction: it would be fair to

cite these here I think because the dynamical interactions that these studies highlight should be important for controlling magmatism in these settings.

5. Line 62 – remove comma.
6. Methods: as noted in main comments, several details of the modelling approach are lacking. This sections needs to be written more fully. Some key points for me (there are likely others):
   - Be specific that you are using the EBA approximation.
   - Is your mesh spacing uniform in the vertical dimension? Have you run resolution tests to confirm that these plume models are fully-resolved? I note that you mentioned this in your original paper, but these models are more complex and likely demand higher resolution, so wanted to confirm.
   - Line 75 – you specify a Couette profile at the inflow boundary that is consistent with the viscosity profile – spell out how you do this (from personal experience, it's not particularly straightforward, and requires explanation – unless again I'm missing the obvious!).
   - You have 'free-inflow' and an 'unconstrained' outflow boundary – are these fully unconstrained or do they essentially prescribe a hydrostatic pressure? Again, it's important to spell this out as they will drive very different flow regimes.
   - Line 89 – linearly interpolated transition. What is linearly interpolated along the transition? Age? Temperature? Depth of LAB? There will be subtle (but important!) differences between each.
   - Lower boundary condition – I find this highly unusual and it requires justification – you maintain an (almost) constant buoyancy flux with an open boundary condition by changing the radius over time. Why? Why not inject material at a constant buoyancy flux which will naturally be handled through the outflow boundary condition? There will clearly be a motivation behind your choice – but again, this needs to be explained – essentially you are switching between a zero normal-flow and an inflow boundary condition by changing r, which is unusual in finite element modelling.
   - Provide your viscosity relationship and a figure showing viscosity as a function of depth both inside and outside of the plume. Without this relationship, the key material property in your simulations is hard to visualise - and Section 3.4 is more challenging to interpret as a result.
7. The paper examines plumes with an excess temperature of 100-200K. I assume this is the excess temperature at the base of the model? Could the authors comment on how these temperatures change with depth and, specifically, what they are in the melt region for each case? In an EBA model, plume excess temperatures change with depth, so it'd be nice to have this information for comparison with other studies.
8. Line 118 – it is stated that conclusions from 2-D study hold in 3-D. This is true in this simplified geometry and it is indeed nice: but you are essentially assuming a 2-D step, so it is not overly surprising. As demonstrated in Davies & Rawlinson (2014), Duvernay et al. (2021), complex 3-D lithospheric geometries can lead to coalescing edge-driven cells, and secondary instabilities, which are further complicated by shear-driven upwelling and background mantle flow. These complexities can have important impacts on the flow regime and associated melting in the vicinity of lithospheric steps. This should probably be highlighted somewhere.
9. Line 127: 'this displacement suggests some interaction of plume flow with EDC-related flow' – see main comment 2 above. Likewise line 246 – 'plume deflection, caused by the effects of EDC'. I think you need to more clearly demonstrate cause and effect here.
10. Line 265: 'Since the vigour of EDC decreases with increasing viscosity' – also fair to cite Davies & Rawlinson (2014) here, in addition to Duvernay et al. (2021), both of which examined this sensitivity (amongst others).
11. Section 3.4 – effects of mantle viscosity: could you add a comparable image to Figure 7 showing the plumes in these cases? It may help a reader try to understand the puzzling results highlighted on lines 260-264.
12. Discussion – line 279 – end of paragraph 1: in this study, the buoyancy flux of the plume is one of the most important components controlling plume lithosphere interaction, but I think it's presumptuous to state that it is the *main influence on hotpost magmatism*. The models examined in this study are idealized. On Earth, the LAB is far more complex, and several studies argue that lithospheric structure is a key control on how plumes and EDC induce magmatism, particularly beneath continents such as Australia and Africa, which host large changes in lithospheric thickness over small length-scales. In addition, work by Burov, Gerya, Koptev (etc. . .) demonstrates that the rheology of the crust and lithosphere will likely play a huge role on how plumes (and EDC) induce volcanism in these regions. With this in mind, I would suggest re-framing that statement - and acknowledging the other important factors not considered in your study.
13. Line 297 – final sentence of paragraph – I'm not sure I follow what is meant by this sentence sorry.

14. Line 306 – I find this statement interesting. The results of Duvernay et al. (2021) suggest that EDC could be sufficient to generate magmatic fluxes such as those observed in the Canary islands. Part of the reason that Duvernay et al. (2021) got higher melting rates was the addition of 3-D complexity, as noted above. I would therefore suggest toning down the statement that your previous paper 'clearly showed that EDC alone is insufficient to generate such magmatism'. The differences between melting rates in your study and Duvernay et al. (2021) probably need to be carefully examined (and I am not suggesting doing so as part of this paper) – but at this stage, I think your statement is too strong.

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