Reply to comments by Rhodri Davies on "The role of Edge-Driven Convection in the generation of volcanism – part 2: Interactions between Edge-Driven Convection and thermal plumes, application to the Eastern Atlantic"

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We appreciate the thorough review by D. R. Davies. We agree with most of his formal comments and address them below. A full tracked-changes version will be provided upon submission of the revised manuscript.

Note: When citing our previous work and current manuscript, we remind the reviewer to refer to "Manjón-Cabeza Córdoba and Ballmer" instead of "Córdoba and Ballmer".

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.

Considering this the other reviewers' comments we modify both, our figures and the explanation of the model setting. It is clear that we need to do a better job in these areas since it seems many questions reflect doubts concerning the model setting.
We agree that a detailed analysis of the effects of plumes on EDC should strengthen the paper. We added such analysis in the discussion and, in addition, we include an example of a model without a plume in the supplementary material to better distinguish between EDC and plume effects.

As a reminder, however, we analyze our models are in steady state, and therefore are not suited to study plume arrival.

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.

Again, we need to apologize for not explaining properly the setting of the models. The reviewer seems to suggest that there is pressure-drive flow from the right to the left side of the models (in the perspective of the current figures) due to open boundaries. However, our side boundaries at the left and right are closed (free slip). Only the front and back boundaries are open (the front with imposed Couette-like inflow; the back with free outflow). Accordingly, we do not expect any (or very minor) pressure-driven flow in the model setup.

We now better explain the boundary conditions of the model (which were also confusing for the other referees). We also include a case without a plume in the supplementary material (similar to the instantaneous case suggested by the reviewer) which shows nearly-identical results to our previous (2D) work, demonstrating that pressure-driven flow is indeed very minor or absent. Moreover, the vast majority of small scale convection cells (SSC, "Richter Rolls) are parallel to plate movement, which would not happen if the pressure-driven flow as suggested by the reviewer was strong.

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.

We agree that the title was not a very good fit, and therefore modify it to be more specific (it was also overly long): "[…] part – 2: Interaction with Mantle Plumes, application to the Canary Islands. We add a more detailed discussion of volcanism at the Canaries, but also mention other hotspots on Earth.

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).

We expand the methods section and added several explanations for clarity.

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.).

We add some discussion about the major limitations of our models. We do not agree that a 'straight' edge is one of them, however. This is rather a simplification, which makes our results more general, than a limitation of the methodology.
The melting model has limitations which are arguably much more important than the reactions between pyroxenite and peridotite mentioned in the comment, which have only been addressed in very few geodynamic works (Ballmer *et al.*, 2013; Jones *et al.,* 2017). We add a few lines underlying these limitations (along with those suggested by the reviewer).

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!)

This comment was raised (to a greater or lesser extent) by all reviewers. We therefore expanded the literature in the introduction section. We do not discuss shear-driven upwelling (SDU) in the introduction, however, since we do not have lateral pressure-driven flow in our models (see above), so we do not feel it is useful to mention it in the introduction. We now do include some lines in the discussion section on SDU, where we now also cite Duvernay *et al.* (2021) and some other relevant studies on.

Minor points

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).

Line 24: Due to the main focus of our paper, we expanded our description of the Canary Islands, but kept short the discussion about plumes worldwide. We still added a couple of lines and references to other work.

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

We agree, we added the clarification as suggested by the reviewer.

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.

Overall, we disagree that the direct comparison with Duvernay *et al* (2021) is adequate in terms of discussing the volumes of purely EDC-related volcanism. As mentioned above, Duvernay *et al.* (2019) additionally considered the effects of SDU due to pressure-driven flow and additional geometrical complexities. In a less complex setting, we demonstrated in the peer-reviewed and published companion paper (Manjón-Cabeza Córdoba and Ballmer, 2021) that EDC alone is insufficient to sustain major volcanism, and related volcanism is usually very minor. Therefore, we prefer to keep the statement as it is.

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.

We added these references, although their dynamic, rheological, initial, and melting approximations make these models difficult to directly compare to ours (we would like to remind the reviewer that our models are in steady state).

5. Line 62 – remove comma

We removed it.

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):

We expanded the method section.

- Be specific that you are using the EBA approximation.

  We are more specific now. In fact, we use a simplification of the EBA, since our adiabatic gradient is linear with depth.

- 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.

  Yes, the mesh is uniform in the vertical dimension. We now clarify the vertical resolution and explain the choice of grid spacing in the text.

• 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!).

We now explain our assumptions for the inflow velocity boundary condition in the text. We calculate the Couette flow by using the upper velocity boundary condition (bottom is equal 0) and assuming constant stress. Under Newtonian conditions the calculation that ensues is straightforward.

• 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.

Sorry for the typo. In fact, the inflow is not free, but rather constrained by the viscosity profile and the upper velocity boundary. The outflow is free/unconstrained except for the condition that all outflow must be perpendicular to the boundary. We better describe this in the text now.

• 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.

As described in the previous paper, it is both, temperature and composition of the lithosphere. We better specify this in the text.

• 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.

For some nodes of the mesh, we are switching between a zero vertical flow and free vertical inflow condition to obtain the preferred plume buoyancy flux. Once this value is reached, however, no such switches occur during the steady state, in which model results are evaluated. During the design of the models, we had to choose between injection, constant radius or constant buoyancy flux. We wanted to avoid injection because it causes artificial dynamic pressures that would influence how plumes interact with EDC. Constant radius is a more common approximation in these cases, but considering how different distances (between the plume and the edge) influence plume flow, the related differences of buoyancy flux between plumes can be significant, therefore making the comparison difficult (especially as far as melting volumes go). Finally, we settled with constant buoyancy flux because this parameter is better constrained by observations (*e.g.*, dynamic topography) than the radius of the plume (King and Adam, 2014). We add an explanation to the methods section.

• 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.

We add a viscosity profile in the supplementary material to complement that of the companion paper (Manjón-Cabeza Córdoba and Ballmer, 2021).

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.

The reviewer's assumption is correct. Due to the compsumption of latent heat of fusion, the plume excess temperature is not meaningful in the melting region. However we add in the text the DT of the plume before the melting region (one third of the model, depth=220 km). We would like to add that, as mentioned above, the adiabat is imposed as a constant gradient and the models are incompressible. Under these approximations, DT does not change significantly with depth.

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 edgedriven 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.

We agree that it is not surprising given our setting. We add the relevant references to the text. However, effectively they show that complex edge geometries boost magmatism for right or acute angles. It is less clear that obtuse or reflex angles, such as those present around the African cratons, will behave similarly to Duvernay 2021. An exception of course, may be the Cameroon Volcanic Line (added the text).

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.

We add a figure showing a case without a plume in the supplementary material.

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).

We add citations.

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.

Also in response to the comments of previous reviewers, we reorganize the figures.

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.

We address this comment by adding text in the discussion section where the references suggested by the reviewer are included.

13. Line 297 – final sentence of paragraph – I'm not sure I follow what is meant by this sentence sorry.

Line 297: We apologize; we reword the end of the paragraph.

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.

We respectfully disagree. First, we believe that the reviewer is underestimating the magmatic fluxes of the Canary Islands. In our previous work, we analyzed only the rates at El Hierro (Carracedo *et al.* 1998), since this value was enough to surpass our melting rates. However, the archipelago has two main shield-building active centers (La Palma and El Hierro). La Palma alone displays volcanic fluxes of 1 km^3 / kyr (Day *et al.* 1999). For the whole archipelago, independent calculations place the extrusion rate at 1-10 km^3 / kyr

depending on whether the Timanfaya eruption at Lanzarote (the largest Atlantic Tholeiitic eruption outside Iceland) is considered an anomalous event (Longpré and Felpeto, 2021). These estimates can easily be doubled when considering underplating and plutonism beneath the islands (Klügel *et al.*, 2005). Taken together, the rates in the Canary Islands are significantly larger than those obtained by Duvernay *et al.* (2021) for EDC models even when considering geometrical complexities that do not exist near the Canary Islands (0.8-0.9 km^3/kyr).
Nonetheless, we think the paper of Duvernay *et al.* is relevant for other locations in the Eastern Atlantic (Cameroon Volcanic Line), since their results related to the geometry of cratons are robust, and we expand the discussion regarding this work.
We also added to the methods section our calculations of a consistent thermochemical profile of the oceanic lithosphere via the pre-calculation of a simplified mid-ocean ridge model (an important difference with Duvernay *et al.*, 2021). This parameterization of the oceanic lithosphere in our models was specified in the companion paper, but we decided not to include it here for brevity. We apologize if this made things more complicated and thank again the referee for his thorough review.

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