

Interactive comment on “The deep Earth origin of the Iceland plume and its effects on regional surface uplift and subsidence” by N. Barnett-Moore et al.

Anonymous Referee #1

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This paper concerns the prediction of the Iceland plume from numerical global mantle flow experiments constrained by plate motions in a mantle reference frame and next a comparison of predicted and published plume paths and of plume-driven dynamic topography evolution with observations of relative vertical motions of North Atlantic region. The manuscript is well written but nevertheless fails to be convincing on both topics (and I am sorry to conclude that).

Basically, to allow for useful comparison between model predictions and observations insufficient material is presented to demonstrate the significance of the position and geometry evolution of the “model Iceland plume” as well as of the derived model predictions. This needs proper attention because the mantle flow modeling is inherently

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approximate due to, e.g., implementing simple depth-dependent rheology, imposing a free-slip surface boundary condition, ignoring lateral buoyancy contrast in the upper few hundred km, and assuming an initially laterally homogeneous dense layer atop the CMB (to mention a few main approximations). In mantle flow modeling these approximations are often made because mantle viscosity and initial mantle structure are largely unknown (e.g. see King 2016 for some contrasting inferences on mantle rheology) implying there is a large model space to explore. This model space is made smaller by the authors by constraining the surface with lithosphere plate motion (although the pertinent absolute plate motion model is not mentioned) and by imposing past subduction in a mantle reference frame. This however also brings new uncertainties in the problem because absolute plate motions are uncertain (see e.g. Doubrovine et al. 2012 for (large) uncertainties in absolute plate motion poles) and the implementation of past subduction (Bower et al. 2015) is approximate at best. Moreover, modeling of plume initiation is acknowledged in the paper as difficult due to what the authors call the “stochastic nature of plume dynamics” which requires the authors to tweak the model initiation times (or other) in order to have the Iceland plume hit the surface around ~ 60 Ma albeit still in the wrong location (~ 10 degrees to the SE of Iceland).

Then, in what kind of “plume” results the modeling effort in this paper?

Firstly: Is the “model Iceland plume” actually a plume (in the sense of the 0-order geometry of a cylindrical upwelling with possibly a plume head)? I would not know. The model Iceland plume is not illustrated in Figures/Movies and no comparisons are made with existing suggestions of plume position & geometry from seismic tomography or with plume & mantle flow predicted from seismic tomography (e.g. Steinberger et al. 2014). In fact, based on the material presented I would suggest that the “model plume” is more akin to an upwelling sheet as I infer from inspecting the generally elongated SW-NE shape of predicted dynamic topography in fig. 3.

Secondly: The Iceland plume/sheet is predicted in the wrong position and the model requires a uniform Euler rotation of 10 degrees to bring it to Iceland (and the LLSVPs and

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slabs and other plumes are then also rotated). Is the global model still Earth-like after rotation? Are there other plumes in the global model? Do they occur at/near known hot spots? Are remnants of subducted slab properly predicted and is the shape and position of the LLSVPs in accord with inference of seismic tomography? In summary: Is your global mantle flow model Earth-like in these aspects (such that Iceland plume evolution could be Earth-like)? These topics are all not sufficiently addressed nor demonstrated and the significance of geometrical evolution of “model Iceland plume” is elusive (for the reader).

Clearly a large set of key assumptions and uncertainties have determined the “model Iceland plume” as some approximation of the actual plume, but how good this approximation is remains unknown. This is the state-of-the-art of this type of mantle flow modelling and I would not be bothered so much by this if the plume position through time and the predicted evolution of dynamic topography would not play a central role in the story in comparing model predictions with observations and findings from others, which is another main theme of the paper. Either one fine-tunes a model to improve the fit with the observations or one estimates uncertainties in the model predictions to allow for a proper comparison. Neither is done here.

With respect to the vertical motion/displacement part of the paper I want to be brief: In Fig. 5-8, I am struck by the general degree of mismatch between the model prediction (blue) and the other curves and I can really not comprehend how the authors can see this differently. The zero-order approximation of the topography level generally mismatches, which the authors admit, while the first-order linear trends occasionally show similarities (but then not at the right topographic level!).

The authors do not always give a fair and balanced comparison with other work. For instance Iceland plume dynamics modeling based on back-advection of present-day density and temperature structure of the mantle derived from tomographic models (which suggests a different plume trajectory) is marginalized too easily by suggesting a potential problem with the fact that thermal diffusion is ignored during back-advection.

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This may be true for back-advection of small-scale structure (order 100 km), but back-advection of large and smooth mantle structures (> 500 km as derived from the tomographic models used) can be done in good approximation back to at least 60 Myr, which encompasses the Cenozoic Iceland plume evolution, because heat transport through conduction is so slow compared to heat advection. Of course, also that type of assessment of plume dynamics struggles with many key approximations (in fact quite similar concerning the mantle flow computations), but instead of negatively focusing on that work, the authors could have better shown some self-criticism with respect to the potentially huge uncertainty in 3-D geometry and position of their own model Iceland plume (path), which is not addressed at all.

In summary: Although I am in general a fan of this type of research, this paper does not sufficiently convince with regard to the significance of the results obtained.

[Interactive comment on Solid Earth Discuss.](#), doi:10.5194/se-2016-118, 2016.

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