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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.

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In the paper "The deep Earth origin of the Iceland plume and its effects on regional surface uplift and subsidence" the authors use numerical global model of mantle convection in order to study the spatial and temporal evolution of the Iceland plume over the last 60 Myr. The paper procures very good insights into the Iceland plume motion path and the evolution of dynamic topography magnitudes in the North Atlantic throughout the Cenozoic.

Given this is an interesting modeling result that fits available offshore geological and geophysical observations reasonably well, I think this paper is suitable for publication in Solid Earth with fairly minor revisions. I detail these below.

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Discussion paper

First, I would urge the authors to show in more detail the evolution of spatial distribution of mantle plume itself (for example, in terms of temperature distributions) rather than only its reflection in the dynamic topography. Surface response to the plume-lithosphere interaction can be quite complex (e.g., Burov and Cloeting, 2009, 2010). Presented on Figure 3 surface dynamic topography shows its very interesting and unobvious evolution: widespread uplift due to initial impact of the mantle plume ($\sim$ 60 Ma) is soon relayed by its two-times decrease in spatial extent ($\sim$ 55-50 Ma) and subsequent propagation of narrowed elevated zone eastward up to the Norwegian margin ($\sim$ 20-0 Ma). However, this does not permit to understand how flows upwelled hot material underneath the lithosphere over this period of time. Evidently, this does not refer to simple lateral spreading of axisymmetric plume head. Therefore, vertical cross-sections and/or horizontal slices throughout modeled temperature distributions are necessary to illustrate the Iceland plume evolution. Their comparing with available tomography data (e.g. Rickers et al., 2013) would also enrich the paper.

Second, I am inclined to ask: if we derive dynamic topography evolution for the first 60 Myrs after the Iceland plume arrival from the preferred model M5 of Hassan et al. (2015), would it be tremendously different than presented here or not? It would be also very interesting to look through dynamic topography evolutions and the Iceland plume motion paths in cases of other models where initiation time has been varied. Global maps of plume eruption locations for the experiments with different initiation times could be also appropriate.

Finally, the paper lacks a final conceptual figure summarizing the main inferences and conclusions.

Nevertheless, despite these remarks and several very minor points below, I am convinced that presented results will be appreciated by a broad community of geophysicists, geologists and geodynamicists interested in numerical convection models and plume-lithosphere interaction in the North Atlantic.

Minor points:

1. Introduction. Reference “Hassan et al., 2016” is not present in the references list.

2. Section 2.2.3. I encourage the authors to avoid the model labels used in previous papers but not specified here (“preferred case M5. . .”).

3. Section 3.1. According to results presented in Hassan et al. (2015), lowermost-mantle convergent flow near plume nucleation site is only observed in the model with purely thermal plume (Figure 4 in Hassan et al., 2015). On the contrary, “thermochemical” plume of preferred model M5 arises above the lowermost mantle characterized by very irregular lateral flow (Figure 7 in Hassan et al., 2015). Moreover, at certain time slices of model M5, upwelled mantle plume appears to be underlain by divergent flow (see Figure 7f in Hassan et al., 2015).

4. Section 4.2.5. The statements “Along the east margin of Greenland, at Nugssuaq. . .” and “west Greenland margin at Kangerdlugssuaq” contradict to left upper inset in Figure 4. Such inaccuracies complicate considerably the reading.

5. Section 4.2.5. Considerable model overestimate of the total dynamic uplift at Nugssuaq should be explained.

6. Figure 1. Contours of the continents are almost undistinguishable. I have not found “the thin black dotted arrow. . .” mentioned in figure caption.

7. Figure 2. Abbreviations (“AMP”, “RMP”, “RMP-fixed” and “RMP-moved”) should be deciphered explicitly.

8. Figure 3. Used here paleocontinental reconstruction is not cited.

9. Figure 6. The best-fit profile “15-17-9” deserves to be presented in a separate figure.

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