

## ***Interactive comment on “Unravelling the internal architecture of the Alnö alkaline and carbonatite complex (central Sweden) using 3D models of gravity and magnetic data” by Magnus Andersson and Alireza Malehmir***

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I have reviewed the manuscript as a general, rather than specialist, reader and provide here my main comments and concerns. I am including an annotated supplementary pdf. Most comments and corrections are annotated there. Here I am focusing only on the most significant points.

My most significant concerns are: a) the introduction should include a clear reason to investigate this carbonatite in detail; b) that the manuscript lacks a clear description of how the modelling was carried out and the model constrained. In the current version of

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the manuscript both points remain unclear, casting doubt on the validity of the preferred results. There must be an explanation added as to how each cell is assigned a value and how the value is constrained using surface geology and rock petrophysics. This same point has been raised by RC1. c) Section 4.2 describing the modelling results need a complete rewrite as it is not possible to follow it. Given these concerns, I suggest major revision followed by re-review.

### **MAIN CONCERNS**

a) The first paragraph in the discussion provides a clear set of reasons as to why to carry out this work in Alnö. I suggest that most of this paragraph should be replaced in the introduction and worked into the text there.

b) Methodology. It is unclear how each cell is assigned a value of density or magnetic susceptibility. The data misfit is taken to zero but just how are the iterations carried out to reach this misfit is not clearly explained. How are the values iterated so as to minimize the misfit and find an appropriate (non-unique solution)? I imagine that this is all in the reference to Li and Oldenburg (1996, 1998a) cited in p. 11, L. 19. This is not sufficient. A brief description of the process must be provided to make the manuscript reasonably self-contained. I was particularly puzzled by a density value modelled that is less than those values measured (density of 2419 kg/m<sup>3</sup> in L. 4 p. 14 is below any measured sample). This does not sound reasonable to me and yet I had no information provided that helped me understand how the range of values in the model were derived.

The associated problem is: what constrains the distribution of these values? Geology? How is that done? We all agree that the results are non-unique, as is stated in the paper, but we need to know that this particular result presented is more likely to represent or approach reality than any other result. Finally, clarity in both the methodology and the use of geological constraints would make the results reproducible.

c) Section 4.2 needs a complete rewrite, organizing the interpretation in a clear and

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organized progression. Start from the beginning. Before interpreting the nature and geometric distribution of rocks, the manuscript must describe what parameters or combination of parameters were used to interpret rock type distribution. It is not sufficient to provide Table 1 with its average values and standard deviations. The text must say how it was used, particularly because each rock type has a wide range of values. I raise one example below (comment regarding P. 17 L. 29).

Throughout this part of the results section, I missed something like what is shown in Fig. 14b. This is a powerful image of the results: the isosurface marking the outer shell of a very high-density zone! Could you not bring this in to the results section. In fact, the entire Fig. 14 should be brought in to the results description. They state powerfully the nature of your model results.

The seismic lines are used to support the interpretation but the reader is not introduced properly to them. They are just thrown in. How is the reader to understand a sentence such as “This is less obvious in the west along Alnö1 and Alnö2 where the density model indicates vertical geology whereas the susceptibility model indicates outward dipping (D2/S2 in Figures 11c, d and 12e, g).” (p. 17)? Where are we to find this information? The actual figure being referred to is Fig. 12c, d and 13 e,g). In Fig. 12a the seismic section Alnö1 is presented, but nothing much is said about it. This needs proper introduction. Likewise Fig. 13a presents Alnö2 and 3. The reader needs a subsection/paragraph(s) introducing the sections and what they show.

P. 17, L.29. The text here attempts to ascribe a specific anomaly to the presence of fenites. This needs to be expanded to provide some real basis to it. There is currently no acceptable reason for this link. Investigating the density values of rocks in Table 1, it seems nearly impossible to tell the fenite apart from other rocks with similar densities. So it must be a very particular combination of density signal and magnetic susceptibility, right?

P. 17 L. 27-30. Need rewriting for clarity. Too many ideas put together and it is hard

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to follow. Likewise the text in p. 18 is unclear, starting with the link between text and figure 12.

Other points: P. 12, L. 3 if the horizontal width of 100m is justified on the distance between station, why make a vertical resolution that is so much shorter?

P. 14 L 26. Did you investigate sensitivity of results to cell size?

Fig. 15. Explain the insets. Also the caption explains your preference for C but the ring is hardly visible in this case. The text says: “The rim would then comprise of a mixture of carbonatite and alkaline rocks, as the petrophysical data suggest high magnetic susceptibility for these rocks”. Where is this statement from? I probably don't understand what is been shown in Fig. 15. Do you mean that all diagrams shown (A-D) reproduce the ring structure at the surface? I am missing something here.

P.24, L. 27 this seems to contradict previous figures and discussion where a  $SI > 0.05$  has been shown in Fig. 14 and the model result showed even higher SI in the model. Can you explain what you mean here to clarify? Conclusions: I don't think that the results implies the existence of a magma chamber. Only that the intrusive volumes are larger at depth: may be multiple intrusions spread in time. Please reword it or remove this conclusion.

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2017-3/se-2017-3-RC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2017-3, 2017.

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