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Solid Earth Discussions

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

Anonymous Referee #1

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General comments This manuscript presents some new geophysical models which attempt to resolve the geometry of the Alno Carbonatite complex in central Sweden. The authors make some interpretations about the geometries resolved in these models, and draw conclusions regarding the emplacement of the carbonatite complex. The early part of this manuscript shows promise , however I have some concerns about the modelling process in the methods section which I have explained below for the authors to consider. I believe these concerns make the manuscript not appropriate for publication in its current form. There are some general comments here, and also a number of comments / annotations in the attached pdf.

Introduction The introduction reads well, however I think it would have more of an impact on a wider audience if more of a theme was set for the paper. There needs to be some explanation as to why this research is significant, as it stands, the introduction could do more to offer this. The authors touch on what I would consider at least part of their research significance in the discussion - that is, the structure they're modelling can be prospective for REE. Later they mention diamonds. Either way, I think it is important to make it clear to the audience why they should read the MS. I also think there should be some mention as to the significance of this research in the abstract, especially considering many people will decide whether to read the paper or not based on the abstract.

Methodology / modelling It appears as though the authors have collected a considerable amount of petrophysical data (both magnetic susceptibility and density, but have also done remanence studies). They have convincingly shown that the majority of their rocks are dominated by induced magnetisation (rather than remanence) and have also pointed out that there is an inherent non-uniqueness associated with potential field datasets (which is encouraging). Where I think that the paper falls short is in the design of the inversion work. The authors have set up their model and performed a property inversion which (although they have restricted their densities and susceptibilities to invert within their measured ranges) - is for the most part an unconstrained inversion. They then use a number of different susceptibilities to generate isosurfaces to build a geometry for the carbonatite complex and speculate as to which of these susceptibilities produces the most geological result. The result is interesting but is poorly – even arbitrarily – constrained. There is scope to go further to constrain their model by relating it more closely to the geological data. In my opinion inversion currently presented by the authors is not geological because the inversion algorithm is essentially transforming the gravity or magnetic dataset into one of many possible density or susceptibility distributions (characterised by a 3D grid/voxet made up of cells). This tends to result in a property distribution which gradually increases in density or magnetic susceptibility toward the centre of the modelled body. There are 2 problems with this: 1) intrusive bodies are likely to be much more homogeneous regarding their petrophysical properties and are unlikely to increase in density or susceptibility as you move closer to the centre of the structure and 2) when you build an iso-surface around the modelled body it could almost be as large or as small as you like depending on the magnitude of the property chosen. Ideally, you would use a density or susceptibility hopefully constrained by field measurements but instead the authors have chosen a range of susceptibilities and speculated as to which is the most geological.

I suggest that what the authors have presented is a very good starting model only and it should be improved before publication. One way of accomplishing this could be to take the average value of their measured densities and susceptibilities for each individual rock type and assign it to the starting model. ie - give the carbonatite complex, the host rocks and whatever other rocks are included in the model a representative value from the measured densities/susceptibilities, then perform a geometry inversion which alters the boundaries between these geological bodies. This may be problematic for the authors since I believe the tools they're using cannot perform geometry inversions. In this case, I think it's worth still setting up the model with constant densities and magnetic susceptibilities (while always making sure it remains consistent with surface geological data), and running a forward model. I believe the residual would be more valuable, and illustrate where there are potential problems with the geometry. From here the geometries of the model could be altered manually before re-running the forward model. This can quickly turn into a laborious process, but because of the uncertainty associated with potential fields, geological modelling should be recursive and the value lies with tying the model geometries as closely as possible to available geological constraints.

Discussion If the authors re-create their model as described above, a chunk of the discussion will need to be re-written. However, one part of the discussion I think could be clarified is the depth of emplacement. When the authors talk about a shallow magma chamber, I presume they are talking strictly about crustal architecture rather than em-

placement depth, but I'm not completely clear. The authors use the term “emplacement” which tends to suggest that they're arguing that the complex traversed through the crust and crystalised (or emplaced) at shallow crustal levels. Potential fields cannot answer the question of whether the complex crystallised at shallow depths, and this claim should be supported by geochemistry of some sort. They do reference other work which suggests there has been $\sim$500m of erosion. This would tend to support their interpretation, but I'd be keen to see more evidence if it exists.

Figures Figures are high quality and I believe of publication standard, however I think the reader would benefit from larger figures in some cases (especially the mag/gravity images). Comments regarding this have been made on the pdf. I think annotations on several figures (again - particularly the magnetic and gravity imagery) could be improved to help the reader with understanding the text. For example, the authors refer to a ring shaped magnetic structure. At first, i thought this was the (obvious) circular magnetic high. But upon further reading, I believe the authors are referring to some other structure they're interpreting in the data, but I'm still not sure what it is. Annotations would help with this, and possibly a qualitative interpretation which explicitly delineates these structures. Keep figure 6B, but I don't think figure 6A is necessary. Removing a 1st order polynomial trend is common practice and its unnecessary to include a grid of it here. So long as the trend is described in the text, I think that's sufficient.

Referencing For the most part, referencing appears to be in order. I have made some additional suggestions where I believe references are required, but I cannot find any references in the text which are not listed in the back (and vice versa).

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2017-3/se-2017-3-RC1-supplement.pdf

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