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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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We thank the anonymous reviewer for the numerous useful comments. We have revised all textural and unclear statements following the reviewer's suggestions. In this document we list both interactive and supplement comments and how we have addressed them in the revised manuscript.

From Interactive comments:

General comments This manuscript presents some new geophysical models which at-

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tempt 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. RESPONSE/ACTION: We have revised the text and hope that it now meet the points made here. However, we are willing to rework it further, if that would be requested.

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 ge-

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ometry 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. RESPONSE/ACTION: We clearly see the reviewers concerns about our modeling methodology and the shortcomings of the algorithm that creates bodies with gradually increasing density/susceptibility towards the centre. A more detailed response and explanation to our procedure are to be found below in the comments. We have also revised the manuscript text to better address this.

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. RESPONSE/ACTION: That is correct that the software we have been using do not have the ability to do geometry inversion. 2.5D forward modeling was performed and presented in the Supplementary Information of the paper Andersson et al. (2013).

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 emplacement 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 500m of erosion. This would tend to support their interpretation, but I'd be keen to see more evidence if it exists. RESPONSE/ACTION: Although the time for intrusion is rather well-constrained , it is not known how much the erosion in the area have been since then, and there is no geochemistry study done that suggest at what depth the crystallization took place. There is one more reference that discuss the erosion depth, von Eckermann (1948) who suggest up to 2000 meter of erosion. However he base it on a obsolete emplacement model, and we think 500 m that is suggested by Kresten (1990) is more realistic. We are willing to broaden

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the discussion about the erosion depth in the manuscript, if that is requested by the reviewer.

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). RESPONSE/ACTION: We are very thankful for this comments, more response and action is described below in this document.

From supplement file: Smaller comments and highlighted typos or grammatical flaws in the pdf-file. RESPONSE: We have adjusted according to the suggestions and our own inspections of the text.

p3 fig 1: COMMENT: Please include a box showing the location of figures 6, 8 and 9. RESPONSE: Figure 1 covers a smaller area than Figures 6, 8, and 9 so it is tricky to indicate them in Figure 1. We could show a larger area in Figure 1, but would then loose details in the geology map. For Figure 6 the coast line is visible to help to compare the extent of the map compared to Fig 1. For Figures 8 and 9 the seismic profiles help to do the same comparison. ACTION: The figure captions now guide the readers to

use the coast line/seismic profiles to understand the extent of the maps presented in Figures 6, 8, and 9.

p3 fig 1: COMMENT: Even though you have coordinates on this map, i still think it is worth including a scale bar. RESPONSE: Followed. ACTION: A scale-bar has been added.

p3 fig 1: COMMENT: What are these blue lines? I can't see them on your legend. RESPONSE: They are carbonatite dykes. It has the same colour as the wider areas with carbonatites. ACTION: They are now described with one sentence in the figure caption.

p3 & 4, fig 1 and 2: COMMENT: Please include a scale bar. RESPONSE: Followed. ACTION: A scale-bar has been added.

p4, fig 2a-b: COMMENT: Can these images be larger? It is hard to see the detail particularly in the magnetic dataset. It is hard to tell which of these white dots are plus signs, and which are gaps in the grid (because of station spacing). Again, if they were bigger it would help. RESPONSE: Yes, they are small. We keep the plus sign white (in black they take too much of attention from the image). In case it is demanded to have the distribution of gravity stations more visible we might have to split figure 2 into two figures. ACTION: The panels are now aligned horizontally instead of vertically so they can be a bit larger.

p4, fig 2b COMMENT: It isn't clear to me what you're calling the ring structure. I have an idea, but it should be explicit. A qualitative interpretation or at least some annotations which delineate this would help. RESPONSE: This is obviously a shortcoming from us. ACTION: A dashed circle is added to delineate the ring-structure on the map.

p4, fig 2b COMMENT: Has this data been reduced to pole? or is it TMI? please specify. RESPONSE: It is TMI. ACTION: It is now mentioned in the figure caption. Given that we are in high latitude this would not make a significant change. We decided to skip

this step.

p4, Fig 2 COMMENT: Can you reference the acquisition report? RESPONSE: It is a part of the Geological Survey of Sweden's magnetic dataset over Sweden, there is unfortunately no suitable report to refer to. ACTION: No action.

p4, fig 2 COMMENT: What about the anomaly? This sentence reads as incomplete. RESPONSE: Agree. ACTION: The text has been updated.

page 4 COMMENT: This isn't really an aim of your research - its more part of your method. RESPONSE: That is correct, however we think it has a very tight link to the process, so we think it is a fair statement. ACTION: No action.

page 5 COMMENT: do you mean feldspathic? RESPONSE: No ACTION: No action.

p6, Fig 3 COMMENT: Can a scale be included in A and C? (ie. similar to D). It looks like there might be a compass in A? Its very small, and i don't think it will be seen when this is in print. & I can't see these borehole marks. RESPONSE: We agree that it was too small scale objects (compass and boreholes). We have added scales, however, they can of course only be indicative, since there is a perspective/depth in the photographs. ACTION: Scales are now included in A and C. The borehole marks are not mentioned anymore since they are difficult to see in the photographs.

page 6 COMMENT: It seems to me that this paragraph is out of place. I suspect you're trying to keep all the earlier studies together, but it makes more sense to me to keep this about the geological setting and put this paragraph with the other petrophysical measurements in section 3.1. RESPONSE: Yes, that was the idea, but we see your point. ACTION: We have moved the paragraph to section 3.1.

page 6 COMMENT: What about structure? are there any faults (mapped by the survey perhaps?) which potentially impact on emplacement? RESPONSE: In the geological map from the survey there are some inferred NW-SE striking brittle structures indicated. The most convincing is along a NW-SE striking step in the topography (visible

in Fig. 7, which shows the SW side have moved up relative to the NE side. However, the topography shift is small (<100 m) and we do not think this is crucial considering the scale of the intrusion. ACTION: No action.

page 7 COMMENT: Not clear what you mean by this. RESPONSE: Up-doming: up-lift or "swell" of the wallrock above the intrusion as more and more of magma is injected through dyke into the rock volume. ACTION: No action.

page 7 COMMENT:"..., whereas the data is presented in the supplementary information": I'm not clear what you mean by this. RESPONSE: There should be a supplementary xlsx-file, which we hope you have got. ACTION: No action. We will follow that it will be posted along with the main article.

page 7 COMMENT: What do you mean by this? What's the difference between a sample and a sub-sample? RESPONSE: For AMS measurement it is standard to measure on cylindrical samples of the length 21 mm and diameter 25.4 mm. When the samples were drilled we tried to make them as long as possible, normally 6-10 cm depending on if open fractures existed or not. A subsample is a trimmed part of a sample. ACTION: Re-phrased the sentence to make it easier to understand what was done.

page 7 COMMENT: This isn't clear - please re-phrase RESPONSE: We agree and it is unnecessary. ACTION: That sentence has been removed.

page 8 COMMENT: If you didn't use a vacuum chamber to saturate the samples, you're not accounting for porosity. I can't comment as to the potential porosity of your rocks - possibly low? in which case saturating samples would have little impact - still, i think it's worth a comment here. RESPONSE: We didn't have access to any vacuum chamber. However, the porosity is very low (in general <1% (Yan et al 2016)). ACTION: We have added a few sentences to clarify this.

page 8 COMMENT: This is vague. Didn't you focus on the carbonatities because

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they're the focus of your research? RESPONSE: Agree. ACTION: The sentence has been re-phrased.

page 10 COMMENT: How did you treat these sea-ice data during processing? Was the density of the sea-ice/salt water stripped out during the Bouguer corrections? RESPONSE: We did not do any corrections for the water depth since we did not have the exact control on the water depth. We had an attempt to measure the water depth, but the ice-bore failed. However, for most of the profile the depth is around 1.5 m according to the nautical chart. Outside the small islands it deepens to 6 m. By neglecting the water column we underestimate the gravity values in this area. ACTION: This is addressed in the revised version of the manuscript.

page 10 COMMENT: Instrument drift? Free-air correction? What gravity datum were the data tied into? RESPONSE: Instrument drift & Free-air correction were also done. We used a local gravity datum. ACTION: Instrument drift and free-air correction is now mentioned. The local gravity datum is also mentioned in one sentence.

page 10 COMMENT: Please specify a rough estimate of the station spacing that covers the main complex. RESPONSE: The spacing of gravity stations are about 100 m within and about 1 km outside of the intrusion area. ACTION: We have mentioned these in the revised text.

page 10 about fig 2a COMMENT: Can you please describe the gridding? what algorithm? cell size? etc. RESPONSE: We used Minimum Curvature Gridding method implemented in Oasis Montaj with a cell size of 100 m for the preparation of Figure 2a (gravity map). However, for the inversion of the gravity data it was the actual data points (610 data points) that were used as input data and not the gridded data. ACTION: The text has been revised to address this comment.

page 10 COMMENT: This is repetition. ("Two smaller but noticeable magnetic anomalies are also observed in the southern part of the study area, one generated from a major industrial complex (an aluminium smelter), and one in the south-eastern corner

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from the Rödön rapakivi granite intrusion,...") RESPONSE: We agree. ACTION: This sentence has been removed; the rest of the sentence is moved to Figure caption 2b.

page 11 COMMENT: Yes - so how are you addressing this uncertainty beyond inverting within the measured petrophysical properties? RESPONSE: This is related to another comment that comes later, see below. ACTION: -

page 11, fig 6a COMMENT: This 1st order polynomial removal is common and it isn't necessary to include the grid of it here. I don't think including it is detracting from the MS, but it seems superfluous. RESPONSE: We agree it is not something spectacular, still we think it provide a good graphical explanation for a broader group of potential readers. We decided to keep it since you don't think it is detracting the MS. ACTION: We didn't remove Figure 6a, however placed panels a and b horizontally (like in fig 2) to be able to make show them larger.

page 13 COMMENT: Can you clarify how you assigned a standard deviation? RESPONSE: It was estimated taking into account the particular gravimeter we have been using and the method we used to measure elevation of gravity stations (differential GPS). ACTION: No action

page 14 COMMENT: what do you mean "roughly". If the residual density is 0, wouldn't that mean the absolute density is 2670? RESPONSE: Agree. ACTION: "roughly" is removed from the sentence.

page 14 COMMENT: Shouldn't this be 1670 and 3670? Also, why did you choose to start with these values? RESPONSE: 1760 - 3760 was a typo. Considering the intervals: We decided to keep them wide and see how the inversion would respond to that without being biased with the bounds. However, we have also made inversions with tighter bounds and all other parameters kept the same. It turns out the results were at the end quite similar and not so biased to the bounds chosen. We can change to instead present that inversion. ACTION: Typos corrected. No action with replacing the inversion run that is presented, however we can do that if requested in the revised

version.

page 14 COMMENT: How many? 90%? RESPONSE: 99.7% fall in the interval -100 kg/m3 to +380 kg/m3. ACTION: We have clarified this in the revised text.

page 14 COMMENT: Are you saying this is standard practice? This paper is over 20 yrs old and these days I would say people try to learn what they can about remanence and take steps to deal with it. I suggest you're better to say here that you assume the mag anomalies are induced because you've shown (earlier in the MS) that the rocks are not dominanted by remanence. RESPONSE: The code MAG3D that we used do not take remanent magnetisation nor demagnetisation effect into consideration, only induced magnetisation, according to the documentation for it. ACTION: We updated the sentence so it is clear that it is a limitation with the code that it handle induced magnetisation. The next sentence comment that this limitation is alright for our case.

page 14 COMMENT: Need to expand on this a little. What understanding are you talking about here? RESPONSE: The understanding that we have got from our previous studies, and of course also from earlier work. ACTION: The revised manuscript should better address this.

p.14 COMMENT: Are you saying this because its low mag? or because you know what the minerals are and you know they're paramagnetic? If so, perhaps a few examples of the minerals? RESPONSE: No, it should be ferromagnetic, not paramagnetic in the text. ACTION: The text has been updated. Sorry for the confusion.

p.15 COMMENT: The calculated response is sufficiently close to the observed, but this isn't surprising given this inversion is mostly unconstrained. Given the large number of possible solutions and uncertainty with potential fields, i think it would add value to try to relate these results back to the known geology - (further comments on attached document). RESPONSE: Remark: The geological map while looks highly detailed, it is most probably just connecting a few surface observations. The main island is poor in outcrop especially at its central part and geologic entities, carbonatite dykes, are too

small to be resolved by any of the two data sets. Forcing the geological map to be a shallow layer of constraint was considered but believed to be not useful since we did not trust the details in the geological map. At the end, the less constrained models were thought to be more reliable than biased with low-resolution surface geology. Some of the dykes are so small that they cannot be even presented in the cells considered in the inversion. ACTION: We keep the less constrained inverted models and think has good values to be presented without being biased with the geological constraints.

p.17 COMMENT: This has no context here since you haven't described your model yet. RESPONSE: Agree. ACTION: Shifted the order on this and next sentence.

p.17 COMMENT: Is this a name? should be "Bay" (upper case). If so, check and replace throughout. RESPONSE: Yes, it is a name. ACTION: Replaced Klingefjärden bay with Klingenfjärden Bay in the document.

p.17 COMMENT: But you're interpreting a density distribution, so doesn't make sense to talk about the gravity anomaly. RESPONSE: For describing the location, we think this is fine as they both relate. ACTION: No action.

p.18 COMMENT: Not clear what you mean by this. RESPONSE: Agree ACTION: The sentence has been re-phrased.

p.19 (This comment is split in two pieces) COMMENT: This is hard to see. As with the mag data, perhaps some annotations on the seismic would help. RESPONSE: We think it would mask the image. COMMENT: There is an obvious SE dipping reflector on Alno1 which is not mentioned at all. What is the significance of this structure? RESPONSE: That is the strongest indicator of the up-doming, it is mentioned earlier in the manuscript. Some are also visible dipping towards NW. ACTION: We added one sentence about this towards the end of the paragraph.

p.20 fig 12 COMMENT: Red line is hard to see in this figure. RESPONSE: Agree, however changing it to a darker colour will risk mixing it up with the gravity plot. ACTION:

No action.

p.20 fig 12 COMMENT: So why do you think these property distributions are different? are they imaging the same structure? or something else? RESPONSE: It is likely that the paramagnetic samples represent Trend 2 and this effect cannot easily be resolved by this type of inversion. ACTION: We have addressed this in the main text.

p.20 fig 12 & p.21 fig 13 COMMENT: Need to refer to figure 1 here for the line location. RESPONSE: Agree. ACTION: Text updated.

p21 fig 13 COMMENT: Please reference the work which created the depth migrated sections - was it Andersson 2013? RESPONSE: Agree. ACTION: The ref is now in the caption.

p22 COMMENT: All this feeds into the significance of your research. If this is why you think your research is significant, it needs to be written into your intro and ideally the abstract too. RESPONSE: Good point. ACTION: We have added a sentence about this in the abstract.

p23 fig14 COMMENT: These figures need a scale and orientation. RESPONSE: Agree. ACTION: We have added that.

p23 fig14 COMMENT: How did you decide on these density/susceptibility values? Is this your final model? It looks like you go on to assess possible susceptibilities in figure 15, but there you have used entirely different susceptibilties to this figure. Why would you not just use the susceptibility that was measured on the rocks? RESPONSE: What magnetic susceptibility would be the correct to choose from our lab measurements? As shown in Figure 4 the magnetic susceptibility of the samples from Alnö Island vary from ca. 0.0001 SI to 0.2 SI. It is the rocks with high magnetic susceptibility that dominate the overall response from the subsurface, so it would not make sense to show an image of the isosurface of e.g. 0.0001 SI; because that would assume there is no rock with higher magnetic susceptibility than 0.0001 SI. However, it would be good to only show

for instance 0.1 or 0.2 SI, which is close to the highest measured mag susc that we have measured. We thought it would be easier to display and interpret the results by showing five different isosurfaces in the higher, but not top, of the range of magnetic susceptibility. Because of that we have presented isosurfaces of 0.01, 0.02, 0.03, 0.04, and 0.05 SI, and discussed around them. That we use complete different isosurface in Figure 14 than in Figure 15 is of course confusing, and it could be motivated to show another, e.g. 0.02 SI isosurface, although that would be a repetition in figs 14 and 15. For gravity/density it was easier to pick a density contrast since lowest and highest density do not differ with many magnitudes as for magnetic susceptibility. Still one could argue that it would be better to display another isosurface than +250 kg/m3 (2920 kg/m3 in absolute density). ACTION: We did not take an action on this. We are happy to follow this if this does not satisfy the reviewer.

p24 COMMENT: I'm not sure why the ring structure would be affected by water depth anyway? RESPONSE: We agree, but since water has lower magnetic susceptibility than most rocks, we wanted to convince the reader that it is not the effect of topography. ACTION: The two sentences "Water depth varies in the bay from a few meters between the small islands and close to shorelines to 40-50 m in the deepest parts of the bay (Figure 1). Thus the magnetic ring-structure is not an effect from the water depth and represents a true geology." are removed from the MS.

p24 COMMENT: When you say "bowl" are you talking about the whole thing in fig 15? Or is the bowl part of the structure? It becomes confusing when you mention the narrow "wall". I'm not following how the wall can be narrow. Do you mean the shape of the body begins to taper in B? It might help to describe orientations. for example - is this the SE margin of the modelled body you're describing? This section which describes the model geometry needs clarification. RESPONSE: With "bowl" we mean the shape of the isosurface/the magnetic body in and around the Klingefjärden Bay, as said in the text for scenario A. "The wall" refer to the sides of the "bowl". It would probably be best visualized by making an image of an N-S profile through the isosurface of the mag susc

model, but can also be seen in Figure 10b rather easy. ACTION: Not required.

p24 COMMENT: why are you making assumptions (or speculating with the other examples) about the susceptibility when you have measured data? RESPONSE: Although we have measured susceptibility for a wide range of rock samples, it is not that straight forward to set one magnetic susceptibility to use. We have to also consider overall bulk susceptibilities. ACTION: Not required.

p25 fig15 COMMENT: This compass orientation is important, but could be made clearer. The N, S, E, W letters are hard to see on the grey background. RESPONSE: Agree. ACTION: Fixed.

p27 COMMENT: This is probably repetition since you've already said that more comprehensive surveys are required above. RESPONSE: Agree ACTION: The sentence "Future studies can also benefit from some marine gravity and magnetic measurements to provide even higher resolution data for potential field modelling." has been removed.

p28 COMMENT: Is this the same as the 'fossil magma chamber' you mention earlier? RESPONSE: That is correct. ACTION:We change so that this is consistent throughout the manuscript.

p28 COMMENT: I think this is the first time i've seen this name. If so, its far too late to be introducing it in the conclusions. RESPONSE: We agree. ACTION: The text has been modified and moved to the geological introduction. Also added the names of the intrusions on the map in Figure 1.

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