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Interactive comment on "Studying local earthquakes in the northern Fennoscandian Shield using the data of the POLENET/LAPNET temporary array" *by* O. A. Usoltseva and E. G. Kozlovskaya

Anonymous Referee #1

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Dear Editor,

Please find enclosed my review of "Studying local earthquakes in the northern Fennoscandian Shield using the data of the POLENET/LAPNET temporary array" by Usoltseva and Kozlovskaya. The authors use data from POLENET/LAPNET temporary deployment in northern Fennoscandia between May 2007 and September 2009 to relocate 34(36) earthquakes, do local earthquake tomography and determine 2 focal mechanisms. The earthquake data is used to discuss seismogenic structures, correlation between geologic units and velocity perturbations and the driving mechanisms of

Fennoscandian seismicity.

In the manuscript, the authors relocate a data set of 34(36) earthquakes but without showing comparisons to how the relocations differ from the catalogue locations, or whether location differences are significant within the uncertainties. They use the relocated events to draw conclusions on Fennoscandian intraplate seismicity in general and the earthquakes of northern Fennoscandia specifically. I do not think that the data is sufficient for this. The authors do "local event tomography" but do not state how many blasts were included, so it seems to me that with only 34(36) earthquakes, the data set is also much too sparse for 3D P-wave tomography. Finally, there are crucial references missing in the manuscript. As it is unclear if this study adds any new information on the earthquakes of northern Fennoscandia I recommend rejection of the manuscript.

Major comments:

1) The authors should make clear how many earthquakes and blasts/explosions they use in the manuscript and which criteria are used to select these events. In section 2 they state that the selected events are recorded by more than 6 stations, but the do not mention how many phases are required (presumably only 7 P phases or more). In Table 3 there are events with azimuthal gaps of more than 180 degrees which degrades the location accuracy. In section 4 it is stated that 34 events were relocated but in the caption to Fig 2 it says 36 earthquakes (and 9 blasts). It would also be interesting to know why the authors did not attempt detection of more events as they had access to a much denser network than the permanent Swedish and Finnish networks and therefore should have been able to detect smaller events.

2) Relocation.

The authors use two different location programs, HYPOELLIPS and their own implementation of a grid search method. The results of the two different methodologies are compared, but no comparison is made to the original catalogue locations. Such a comparisons is necessary, in my opinion, to judge the gain in location accuracy obtained with the additional stations. The uncertainties in the various locations, original and relocations, need to be discussed in more detail to determine the accuracy of the locations. The uncertainties tabulated in Table 2 and 3 (g_er and v_er) and the factor 0.53 are not well explained and should be made clearer. Which method do they come from? Synthetic comparisons of the methods should also be made so that it is clear what differences come from the methodologies themselves.

It is strange that the authors see no benefit in using S-phases in the locations. Does this conclusion hold both for HYPOELLIPS and the grid search method? S should improve the depth determinations significantly, at least the S-phases on the closer stations.

Changes in depth after relocation is perhaps the most interesting aspect and should be given more attention. A plot of the depth distributions before and after relocation would be valuable. And how well constrained are the depths? Table 3 indicates (depending on the exact meaning of v_er) that the depths obtained by the different relocation methods sometimes vary more than v_er. One of the deep events is located at 52-53 km depth. That is in the mantle, according to the author's velocity model. An earthquake in the mantle is very interesting and the authors should investigate this event in more detail to try to determine if it is in fact this deep.

3) Local event tomography.

The authors do not state how many phases from earthquakes and blasts are included in the velocity estimations, except that in their VELEST runs they had a total of 624 rays. The events are not included in the resulting velocity plot sections which makes it difficult to assess the results.

The station corrections from VELEST are compared to topography in Figure 12 and the authors infer a dependency of the statics with topography. Is elevation not included in the VELEST runs? VELEST has various ways to account for uneven station elevations, this should be described by the authors. Could the corrections not be from velocity

variations?

The description of the parametrisation of the tomography models is unclear to me, how many cells are there in the models, are the events relocated in the tomography and how many unknowns does this add up to? This should be compared to the number of data. The checkerboard tests are done with large cells of 75 km to 150 km, but from the results in Figs 13-14 it seems that the model grid is much finer? On pg 3700, line 19, it seems like the grid may be 50x30 km, but this is difficult to reconcile with Figs 13-14, and seems odd if the checkerboard variations is on a 75x75 km scale? The checkerboard tests seem to indicate that the tomography works well down to 75x75 km velocity variations, and there is no difference if the grid is rotated 30 degrees. However, in the results in Fig 15 there is a clear dependency on the grid direction, in 15 a) there is horizontal smearing but in 15 b) the smearing is at 30 degrees. Why is this? How does the smoothing work in the tomography?

4) Discussion and conclusions

The authors draw conclusions that in some cases are not well based on the data.

On page 3702, line 18-19 they state that the results of the study "...provide new knowledge about the processes that cause intraplate seismicity in northern Fennoscandia." Causative processes is however not treated in the manuscript.

Pg 3702, line 21-23, "The lateral heterogeneities in the upper crust in our velocity model are show general good correlation with the surface geology" This is difficult to assess from Figs 1b) and 15. Perhaps the velocity models could be placed transparently on top of the geology? Statements on page 3703 also indicate mismatch at the Finnish-Swedish border.

Pg 3703, lines 16-19. This sentence is unclear, what is the role of the tomography results here?

Pg 3703, lines 20-24. That the earthquakes show good correlation with postglacial faults was shown very clearly by Lindblom et al. (2015). That study also indicated that there is less of a north-south directionality in the seismicity, along the BBMS, than there is a NE-SW direction, in line with the PGFs. This is also indicated in the FENCAT data shown in Fig 16 in the manuscript. The 34(36) earthquakes in this study do show a north-south pattern, but could that be an artifact of too sparse data? In addition, this manuscript does not show a correlation between seismicity and PGFs, as the PGFs have not been drawn in Fig 2 and in Fig 17 the deep events are mostly unrelated to the mapped PGF scarps.

Pg 3704, lines 1-10. It is a little unclear if the authors mean that the Lainio-Suijavaara (note spelling) and Lansjärv faults are in the BBMS. As defined by the authors in Figs 16-17, the Lansjärv fault is not in the BBMS and only part of the Lainio-Suijavaara fault is in the BBMS. As there have been deep earthquakes on the Lansjärv fault (34 km, Arvidsson 1996), the Pärvie fault (35 km, Lindblom et al. 2015) and the Burträsk fault (>30 km, Juhlin & Lund, 2011), the statement on lines 5-6 is incorrect.

Pg 3704, lines 15-17. Arvidsson (1996) discussed the large postglacial earthquakes 10,000 years ago, which occurred just as the ice sheet disappeared. He did not refer to the current seismicity.

Pg 3704, lines 18-20. The World Stress Map 2008 does not show faults, but for northern Fennoscandia has 4 thrust faulting stress indicators, 1 normal faulting indicator and 5 indicators of stress direction but not regime. In addition, the remaining rebound stresses are very small today so these stress indicators are unlikely to show rebound stress.

Pg 3704, lines 21-26. It should be pointed out that in the Steffen et al. (2014) modelling paper a fault dipping at 75 degrees is only activated if the assumed coefficient of friction in the rock is very low (mu = 0.2), a value which is likely to too low in the Baltic Shield. In addition, the manuscript has not shown that the deep earthquakes occur at the PGFs, on the contrary, Fig 17 indicates that the deep events in this study occurred away from

the PGFs. Finally, Steffen et al. (2014) have reverse faulting mechanisms in their model events, which is not in agreement with the strike-slip events of this study.

Pg 3704 - 3705, lines 27-28 + 1. The earthquake activity along the Swedish-Finnish border, or the BBMS, is a well known and described feature in Fennoscandian seismology. This study does not show that the seismicity here is deeper than on the PGFs.

6) Additional references.

Much work has been done lately on the issue of postglacial faulting (PGF) and seismicity in connection with PGFs. The authors should update their maps with the fault scarps from Sutinen et al. (2014) and Mikko et al. (2015). Lindblom et al. (2015) presented the full earthquake data set in northern Sweden and its relation to PGFs, and a special earthquake study on the Pärvie fault. This paper is especially significant for the manuscript here as it is likely that most earthquakes that this manuscript locates in Sweden are part of the data set used by Lindblom et al. (2015). PGFs and seismicity in Fennoscandia was discussed extensively in Korja and Kosonen (2015). Redfield and Osmundsen (2013,2015) presented a model for the Fennoscandian seismicity based on hyperextension and Bungum et al. (2010) discussed tectonic versus rebound driving mechanisms.

Smaller issues: - The language needs polishing, especially the use of "the" and "a".

- Pg 3690, l 22: "shift" and "uplift" are not usually used for focal mechanisms. The sentence can be ended after "... strike-slip type."

- Pg 3690, l 22-24: The BBMS is well established as seismogenic before this study.

-Pg 3691, l 3-4. For what purpose(s) are the permanent seismic networks not dense enough? It should be pointed out that the Swedish and Finnish seismic networks have expanded significantly in the last 10-15 years, also in the north. See Korja & Kosonen (2015).

-Pg 3691, l 10-11. None of these references studied local earthquakes.

-Pg 3691, l. 21. There are no organizations in the Acknowledgment, only persons. For this study, the data from the Finnish and Swedish seismic networks should be acknowledged.

-Pg 3692 l. 8-10. The Arvidsson (1996) study does not talk about current seismicity, and the Wu et al. (1999) study concerns GIA modelling. Other papers should be quoted here, such as Slunga (1991), Bungum et al. (2010). Redfield & Osmundssen (2013,2015) etc.

-Pg 3692 l. 11-12 A more recent reference is Lidberg et al., J. Geodyn. (2010).

-Pg 3692, l. 13-15. These references should be changed to Kujansuu (1964), Lundqvist & Lagerbäck (1976) and Olesen (1988), or the more recent Kuivamäki et al. (1998), Lagerbäck & Sundh (2008) and Olesen et al. (2004).

-Pg 3692, l 16-20. Add Olsson et al. (2008) and Eken et al. (2007).

-Pg 3693. The introduction should have a paragraph on the results in Lindblom et al. (2015) as they are important for this study.

Section 2.

-Pg 3693, l 22. The Helsinki catalogue is commonly referred to as FENCAT and not HEL, as it has data from all the Nordic countries.

-Pg 3692, l. 25-27. It would be good to write out where these locations are, and what activity goes on there. Quarries in both locations?

-Pg 3694, l 11-14. Could this be radiation pattern effects? There are only 2 stations closer than 100 km in the figure, and some of the more distant stations also have relatively small P.

Section 3. The purpose of this section is a little unclear as it is mostly related to velocity models and not picking as such. The authors discuss phase arrivals from various discontinuities and in terms of phase identification this is important, but perhaps less

so in terms of picking the first arrival. The authors do not state if they use classifications of the arrivals in terms of how clear they are, or if they can associate uncertainties to the arrivals. I find this section unnecessary long.

-Pg 3694, l 21-23. Which layers were merged, and how?

-Pg 3695, l 1. For shallow events the take-off angle is always greater than 90 degrees.

-Pg 3695, l 17-18. What do you mean by "Particular analysis..."?

-Pg 3695, l 27-29. This of course also depends on the velocity model.

Section 4. -Pg 3696, l 15. What is meant by the "method is uniform for arbitrary complex velocity models"?

-Pg 3696, l 20. Why such a large model? 800x800 km? You only use stations out to 250 km.

-Pg 3696, l 22. What is "short distance conversion"?

-Pg 3697, l 29-1. The events in FENCAT are usually well investigated with respect to blasts. Do you have other indications except for the depthh that these are blasts? Have you looked at polarities and spectrograms?

-Pg 3698, l 4-5. The station distribution, and higher density, surely plays a role here?

-Pg 3698, l 10-11. It is the maximal gap, not "The minimal angle..." I presume?

-Pg 3698, l 12. Ref to Fig 17.

Section 5.

-Pg 3699, l 27-28. Why is the N-S nodal plane the most likely fault plane?

-Pg 3700, l 1-2. What do you mean by "fault stretching in longitude direction"?

-Pg 3700, l 3. Remove "... which shift prevails over uplift."

-Pg 3700, l 4-7. This information is redundant and can be removed.

- Fig 17. The focal mechanisms here is a subset of available mechanisms in northern Fennoscandia, and they are generally more varied than the strike-slip events shown here. See e.g. Korja & Kosonen (2015), Arvidsson & Kulhanek (1994).

Section 6.

-Pg 3700, l 12-13. Did you use VELEST without relocating the events in the new 1D model?

- Pg 3701, l 1. Table 5 is a little unnecessary as the change is very small. This could be stated in the text instead.

Tables

2) How come the second event has a station as close as 25 km and the others do not?

5) This Table is not necessary seeing how small the differences are.

Figures

1) Add a larger scale insert to show where the study area is located in Fennoscandia. Indicate which stations are permanent and which are temporary, perhaps with different symbols. Spelling: Lainio-Suijavaara, Lansjärv, Suasselkä. Write "Blue boxes". The ref to the faults should not be Muir Wood as they were described by other people. See similar comment above for page 3692.

2) Add the faults to the map. Use unfilled symbols to make it easier to see. Use FENCAT instead of HEL.

3) These are too small, it is very difficult to see the text. Why use the FENCAT distances (delta) and not the relocations?

4-5) Write that these are the first arrivals.

6) What are the distances to the stations from the event?

8) This figure is not much discussed and can be removed.

10) This is not an essential figure as onset uncertainty is not discussed much. How are the traces filtered?

13-15) The green lines outlining the well resolved region are hard to see.

17) In Table 3 there are 7 deep events. Here only 6?

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