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Interactive comment on "A lithosphere-scale structural model of the Barents Sea and Kara Sea region" *by* P. Klitzke et al.

Anonymous Referee #1

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General comments

This paper presents a compilation of shallow and deep data from the Barents and Kara Sea region in an effort to create a global model of a large, and heterogeneous area with a long and complicated tectonic evolution. This is a titanic effort that the authors have handled quite successfully. However, it faces some problems, being the first of them, in my opinion, the resolution of the different datasets and that of their goals. In this regard, the authors should specify the resolution they have in the calculation of the crustal interfaces. We know the (very heterogeneous) resolution of the data used to map the LAB, but I did not get any idea of how much information has been used to define the crustal structure. If seismic profiles, wells etc have been used, they should be put in a map that could be added to Figure 1. They refer to an appendix that I

haven't found. It is probably better to add the information in Figure 1

Also very important is the superposition of tectonic events and how the LAB has responded to them. I find it difficult to interpret the present day characteristics of the LAB and the deeper part of the lithosphere in relation to old tectonic processes. The lithosphere should re-equilibrate in relatively short (in geological time) periods of time due to, at least, the thermal erosion of the asthenosphere and does not need to accommodate to the formation of surface features that are geographically restricted, although both things responded to orogeny.

I understand that a big effort has been made to generate the LAB depth map of the area, but for this task, two different datasets, with different horizontal and vertical resolution have been used, and an empirical equation is utilized to calculate the LAB depth in places where there are no other data available. The result is a LAB that has very steep gradients and cross-cuts shear wave velocity isolines. That makes the reader to wonder about what the asthenosphere represents. Decrease in velocities between two points maybe due to partial melt but also to compositional heterogeneities. I would be happier with a LAB that would follow a velocity isoline, as it follows an isotherm. If authors are not willing to do this, they should explain why the LAB intersects velocity isolines.

Finally, the high shear-wave velocities in the lithosphere underneath the East Barents Basin are displaced to the east regarding the position of the basin in profile 1. However, profile 1 and 2 find a low shear wave velocity anomaly underneath the Timan Pechora Basin and the South Kara Sea Basins. To me, that is more interesting that the high velocity near the East Barents Sea Basin. Those 'high velocities' are not that high considering the depth in the mantle at which they are observed. However, the Vs values below the TPB and the SKSB are low for those depths. In fact, beneath the East Barents Basin there is a 'drop' of low shear wave velocities at 150 km depth. To me the interpretation is geared towards finding a high velocity anomaly. It should be improved and comment all the 'anomalies'.

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Detailed comments:

Figure 1 is extremely small when you print it. Considering the amount of information it contains is not helpful at all in a printed version. The authors should figure out the way to enlarge it. One parenthesis is missing from SH in the Figure caption.

You refer to Fig. 5c (3.2, line 27), before referring to Figs. 3 and 4. Fix it.

Page 1591, Line 10 and onwards. No deposits of this age appear in the continental domain 'apart from the southwestern most Barents sea'. You refer to the 7500 m of sediment that appear in the oceanic domain (they have COB to the east, so it is oceanic domain?). This also brings the question of what the COB is representing. Oceanic crust vs thinned continental crust? The interpretation of this boundary should be addressed.

Page 1591, line 19. Refers to figure 4b as mid-Cretaceous-earliest Eocene sequence whereas in the figure refers to mid-Cretaceous-Paleocene sequence. One of them should be changed, so they agree. We do not know from the figure if the Paleocene is included or not, although the text suggest it is not.

Figure 3 and onwards: Although easy to understand and straightforward, the acronyms (SKS, EBS etc) are not explained anywhere in the text or figure captions so include them. Besides, in different figures you use different acronyms to name what seems to be the same feature (see below)

Figure 4f represents the depth to the top of the crystalline basement and not its thickness?. So why not putting it together with figure 3 a,b,c,d. It is a 'depth to' and not 'the thickness of'. Besides, in the caption it should say 'Depth to the top of the crystalline basement'.

4.2, page 1593 and onwards. You mentioned that you have calculated the Moho depth with velocity data. What kind of data? P, S waves, from vertical incidence? From refraction? From teleseismic or local tomography? Even though you give references,

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more information should be provided in the text.

Figure 5a: NKB: North Kara Basin? Figure 5b: NKS: North Kara Sea? Is the same thing as before? Isn't it possible to have a little bit more of consistency with the names?

Figure 6: It is striking to see that the LAB topography cross-cuts the velocity isolines. I don't believe that a mostly thermal/viscosity boundary intersects isolines like that. See comment above and explain or change.

Page 1595: Line 8: by large depth gradients. . .whose gradients are those? LAB depth? The Figure you have to refer is probably 5c and not 5d, or if maybe both.

Page 1595: Line 9 and 10. Are you inferring that the lithosphere thickness represents pre-collisional terranes of different characteristics and not the result of the recent tectonic evolution. The decrease in the thickness to the W is clearly related to rifting. To the east, the LAB depth cannot vary too much since you are using a database that has a grid spacing of 2°. I find the discussion and interpretation included in this chapter and Figure 7 too geared to identifying pre-coliisonal terranes. However, the LAB topography has been calculated in a very heterogeneous way and should respond to the present day situation. You yourself partly acknowledge this in page 1597, lines 11 to 15 or in page 1602, line 20-22. Pre-collisional terrenes might have an influence in some cases, but not in all the cases you refer to. I think you should include this type of discussion in the text.

Page 1596: line 20: By 'Beside the property-induced impedance decrease'. . .do you mean 'Beside the change in seismic impedance due to contrasting physical properties of the rocks? So write it properly. Salts only have high velocities when compared to their densitiy, but still, they have low velocities.

Page 1597: line 1: There is no figure 3e. Do you mean 3d or 4e?

Page 1597: line 6: There is no figure 3f.

Page 1597: Lines 17 to 20. Why should the lithospheric thickness of the NKS, SKS

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and the BS be similar? They have had different evolutions. The possible existence of a North Kara terrane does not explain everything. I find that this discussion is too biased to find evidences of pre-collisional terranes (which of course, may exist) in the LAB topography. But you have to try to be more objective in the discussion and consider all the factors that have an influence.

Page 1599: line 4. Isn't it Figure 4d and not c?

Page 1600: line 8: (ii) multiphase extension and not (iii) since (iii) is lithospheric buckling.

Page 1600: line 20: The spatial correlation between the EBSB and the high shear wave velocity is not straightforward. The anomaly is not such a high velocity anomaly but it neighbors a low velocity anomaly, which is different. And, it is not exactly below the basin. I think this is a weak point in the discussion.

Page 1600: line 29: There is a low velocity anomaly in the upper mantle underneath the SKS. To me, that is more striking than the one below the EBSB because it is right below and because it is really a low shear wave velocity. If you don't comment this anomaly, why do you comment the one in the EBSB? Both represent the present state of the lithosphere and do not need to be related to the origin of old basins. The youngest sediments in the area are in SKS and not in the EBSB (Figure 4b, mid-cretaceous to Paleocene) and this is the one that has a low shear-wave velocity anomaly that you don't discuss at all, but I think you have to.

Page 1602: Line 1: Figure 3b-e. There is no figure 3e. Don't you mean Figure 4b-e?

Page 1603: Line 21. Again you use a present day 'high shear wave velocity anomaly' in the EBSB (I insist, those velocities at not high for depths higher than 100 km, and this 'anomaly' is located at around 300 km to the east of the depocenter of the basin) to explain the formation of a basin in Paleozoic times. If you insist in this hypothesis, you also should explain why this anomaly persist for such a long time and also, if it formed

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a basin in the Palezoic, why doesn't it contribute to deepen the basin now, when you actually see the anomaly (there is not much sedimentation in the EBSB since the Mid-Cretaceous)or better, why there is no basin exactly on top of it right now.

Conclusions:

Line 6: indicating instead of indicate

Paragraph 3: I don't believe the relation of that supposed high velocity anomaly with the formation of the basin in the Paleozoic. See above and discuss.

Figure 1: Make it bigger. It has a lot of information. And add another figure with the location of databases used for this work.

Figure 3 and onwards: Acronyms should be explained in the captions.

Figure 4: I would move 4f to figure 3.

Figure 5: Very steep gradients in 5c should be removed or explained in the text

Figure 6: The authors should explain why the LAB crosscuts so many velocity isolines.

Figure 7: The color scale does not allow to see the differences between the lithospheric thickness of the NKS and the SKS. You should make it clear that the strongest influence in the LAB comes from present day tectonics.

In general, the depth at which you locate the LAB is confusing. The criteria you follow, purely mathematical, makes your LAB to crosscut velocity isolines. Also, using different datasets makes you come up with a LAB with very strong gradients (Figure 5c shows places where the LAB is almost vertical!). I understand that's the data you have, but the results should be treated with caution. There are many places where different terranes have been involved in Paleozoic collisions and the actual LAB configuration only reflects the present day tectonics. You should discuss why the LAB and the lithospheric velocity configuration have persisted through time.

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Bibliography needs to be revised. Some reference do not agree with the way they appear on the refrence list (e.g. Malyshev et al., sometimes appear as 2012 and 2013 and not as 2012a and b, as it is in the bilibography; the same applies to Henriksen et al., which in the text appears at least once as 2011 and not 2011a or 2011b).

Also some references in the text are missing in the bibliography (e.g., Echtler et al., 1998; Eaton et al., 2009;Johansen et al., 1993; Kumar et al., 2005; Müller et 18. 2008; Zhang and Lay, 1999) and most of those appearing in table I (e.g. Fielder & Faleide, 1996; Hjuelstuen et al., 1996, Engen et al., 2006; Engen et al., 2009; Glebosvki et al. 2006; Gramberg et al, 2001; Brekhuntsov et al., 2011; Faleide et al., 1993 a and b??? Johansen et al., 1993; Kontorovich et al., 2010; Norwegian Petroleum Directorate; Henriksen et al., 2011; Aplonov et al., 1996; Drachev et al., 2011; Gramberg et al., 2001; Myklebust, 1994; Ritzmann et al., 2006; Skillbrei et al., 1991; Dahl-Jensen et al., 2003; Minakov et al., 2012; Ritzmann et al., 2006).

Interactive comment on Solid Earth Discuss., 6, 1579, 2014.