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Solid Earth Discussions
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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.

P. Klitzke et al.

klitzke@gfz-potsdam.de

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We would like to thank you for constructive and helpful remarks, which supported us strongly in enhancing our text. Furthermore, we provide detailed comments on implemented changes in the manuscript according to the referee's concerns. We hope, the answers obtain your approval. In any case we are looking forward to answer further questions, should they arise.

Your sincerely,

Peter Klitzke and Co-Authors

General comments

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[1] Comment of reviewer: 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

Answer to reviewer: We are very sorry for the missing Appendix. Apparently, you received not the proofed version of the manuscript but the very first submitted version which unfortunately contained some mistakes. The Appendix appears in the updated version as Table A1 (find attached Fig. 1). In this table we provide a complete list of integrated datasets for each interface.

[2] Comment of reviewer: 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.

Answer to reviewer: We agree with the reviewer and made respective corrections in the manuscript. However, erosion of the lithosphere may have played a role in the western Barents Sea (e.g. beneath Svalbard).

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Changes in the manuscript: We removed the first part of the discussion where we relate the LAB depth configuration with paleo-terrains (pg. 1595, line 5 to pg. 1596, line 6). Instead, we focus in the discussion more on the younger evolution e.g. the thin lithosphere beneath the western Barents Sea which correlates with the rifting history and formation of the NE Atlantic.

[3] Comment of reviewer: 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.

Answer to reviewer: Indeed in our model the LAB does not follow a velocity isoline. The reason for this phenomenon is that the velocity generally increases with depth. We traced the LAB depth as the depth where the velocity decreases and reverses the trend of depth-dependent increase above. However, the absolute values are in general higher the deeper this inversion occurs. Therefore, an isoline is not meaningful.

Changes in the manuscript: As indicated in the answer to this comment, this is now described in more detail in the manuscript (pg. 1589, line 5-10).

[4] Comment of reviewer: 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’.

Answer to reviewer: We agree that it is a question of definition whether we have a fast anomaly beneath the East Barents Sea region or a slow velocity anomaly beneath the Timan-Pechora and the southern Kara Sea region. In any case, the velocity difference between the two regions is a robust observation. However, Levshin et al. (2007) introduced the velocity model (BARMOD) which we are referring to. In the mentioned study, the authors show shear-wave velocities relative to the 1-D Barey reference model (Schweitzer and Kennett, 2007). The relative velocities in 80 to 100 km depth show a positive anomaly of ca. +4% beneath the eastern Barents Sea while the velocities appear rather ‘normal’ (range of +1%) beneath the southern Kara Sea (see attached Fig.1). In fact, we agree with the editor that the anomaly is ‘dipping’ eastwards as visible in Fig. 6b in the manuscript and is not following perfectly the outline of the East Barents Sea Basin with depth. However, at this stage of investigation it would be philosophical to interpret these structures without 3D density and temperature modelling.

Changes in the manuscript: We added a sentence in the manuscript (pg. 1594, line 12 - 24) where we precise the definition of the high-velocity anomaly (relative to the 1D-reference model Barey; Levshin et al. 2007). Additionally, we include a paragraph discussing the limitation of the BARMOD/CUB velocity model. These depend on the crustal reference model and the local path density (see attached also Fig. 3). The BARMOD velocity model e.g. has a rather high resolution in the western Barents Sea and in the south-eastern Barents Sea, while the path density of Love and Rayleigh waves is rather low towards the southern Kara Sea (Levshin et al., 2007). However, we added a paragraph discussing the low-velocity zone beneath the southern Kara Sea as it could be related to e.g. a plume or underplating.

Detailed comments:

[5] Comment of reviewer: 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.

Answer to reviewer: We enlarged Fig. 1b.

[6] Comment of reviewer: You refer to Fig. 5c (3.2, line 27), before referring to Figs. 3 and 4. Fix it.

Answer to reviewer: done

[7] 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.

Answer to reviewer: We precised the definition of the continent-ocean boundary by adding that this feature characterises the boundary between continental to oceanic crust/lithosphere. Additionally, we rephrased the part where we describe the youngest Earliest Eocene to present deposits.

Changes in the manuscript: Definition of the COB is described in more detail in page 1586 – line 11-29.

[8] Comment of reviewer: 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.

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Answer to reviewer: The description is now consistent through the manuscript.

[9] Comment of reviewer: 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).

Answer to reviewer: done

[10] Comment of reviewer: 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'.

Answer to reviewer: We added the top crystalline basement to Fig. 4 in the manuscript since it illustrates the influence of the cumulative thickness variations of the sedimentary megasequences. Now it is clear that larger depressions of the top crystalline crust correlate with domains of larger sediment thickness.

[11] Comment of reviewer: 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,more information should be provided in the text.

Answer to reviewer: As the other surfaces also the depth of the Moho has been modelled by combining results from different studies based on different methods (e.g. refraction seismics, receiver-function analysis, probabilistic inversion model). The respective references for the Moho are given in Table A1 (find attached Fig. 1).

[12] Comment of reviewer: 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?

Answer to reviewer: done

[13] Comment of reviewer: 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.

Answer to reviewer: We don't claim to know that this is a thermal/viscosity boundary. As discussed e.g. by Eaton et al., (2009), a LAB has been mapped by different studies using very different methodological approaches. Accordingly, there are different definitions for describing the boundary/transition between lithosphere and asthenosphere (electrical, petrological, rheological etc.). However, Fischer et al. (2010) show, that partial melt has the potential to strongly affect the viscosity and thus, also the shear velocities. The LAB is cutting higher shear velocities towards the east, due to its deepening and a pressure-related increase of the shear wave velocities with depth. Please see also the answer to comment 3.

Changes in the manuscript: As mentioned in the answer to comment 3, the characteristics LAB are now described in more detail in the manuscript (pg. 1589, line 5-10).

[14] Comment of reviewer: 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.

Answer to reviewer: In the 1st version of manuscript we referred in this paragraph to large depth gradients of the LAB. We discuss in this paragraph of the discussion the LAB depth and paleo-domains. Since this appears too vague due to the resolution of underlying datasets, we agree with the earlier comments of the reviewer, and removed this paragraph from the manuscript (pg. 1595, line 5 to pg. 1596, line 6).

[15] Comment of reviewer: 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.

Answer to reviewer: We agree with the reviewer that the resolution of the CUB dataset is too coarse and we made therefore respective changes in the manuscript. However, the depth variations of the LAB between e.g. the western and eastern Barents Sea are extracted from the same dataset (BARMOD) and we think that this is a robust feature. Changes in the manuscript: We removed the first part of the discussion where we relate the LAB depth configuration with paleo-terrains (see answers to comment 2, 14).

[16] Comment of reviewer: 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.

Answer to reviewer: Corrected.

[17] Comment of reviewer: Page 1597: line 1: There is no figure 3e. Do you mean 3d or 4e?

Answer to reviewer: done

[18] Comment of reviewer: Page 1597: line 6: There is no figure 3f.

Answer to reviewer: done

[19] Comment of reviewer: Page 1597: Lines 17 to 20. Why should the lithospheric thickness of the NKS, SKS 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.

Answer to reviewer: We agree with the reviewer that this part of the discussion is too geared because the resolution of the underlying datasets is partly too coarse. Therefore, we edited the respective parts where we correlate the LAB with paleo-terrains (see answers to comments 2 &14).

Changes in the manuscript: We removed the first part of the discussion where we correlate the LAB depth configuration with paleo-terrains (pg. 1595, line 5 to pg. 1596, line 6).

[20] Comment of reviewer: Page 1599: line 4. Isn't it Figure 4d and not c?

Answer to reviewer: done

[21] Comment of reviewer: Page 1600: line 8: (ii) multiphase extension and not (iii) since (iii) is lithospheric buckling.

Answer to reviewer: done

[22] Comment of reviewer: 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.

Answer to reviewer: Please see answer to comment 4.

[23] Comment of reviewer: 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.

Answer to reviewer: As mentioned in the answer to comment 4, the discussion includes now also a paragraph discussing the low-velocity anomaly beneath the southern Kara Sea. However, according to the velocity reference model Barey, the anomaly beneath the eastern Barents Sea is stronger and the resolution of the underlying data higher there than below the southern Kara Sea (please see also answer to comment 4).

[24] Comment of reviewer: Page 1602: Line 1: Figure 3b-e. There is no figure 3e. Don't you mean Figure 4b-e?

Answer to reviewer: done

[25] Comment of reviewer: 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 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.

Answer to reviewer: Levshin et al., (2007) show shear-wave velocities of BARMOD relative to the 1-D Barey reference model. The relative velocities in 80 to 100 km depth show a positive anomaly of ca. +4% beneath the eastern Barents Sea while the velocities in the same depth appear rather 'normal' (+1%) beneath the southern Kara Sea (see attached Fig. 2). We don't claim to know the origin of this high-velocity anomaly but discuss possible ideas. The Uralian Orogeny was the last major geodynamic event in immediate proximity to the eastern Barents Sea. The compressive setting may have induced the increased velocities beneath the eastern Barents Sea. Additionally, the high velocity anomaly follows in depth of circa 80 km the basin outline, which may indicate that this anomaly might be older than the main subsidence phase of the basin (Permian-Triassic). The basin subsidence (and the continuous fill with sediments) would last until isostatic equilibrium is reached. Since, the observed free-air gravity is showing no larger anomalies across the shelf, studies assume that wide parts of the Barents Sea are at present in isostatic equilibrium (e.g. Ebbing et al., 2007).

Changes in the manuscript: As outlined in the answer to comment 4, we added a sentence in the manuscript (pg. 1594, line 12 - 24) where we precise the definition of the high-velocity anomaly (relative to the 1D-reference model Barey; Levshin et al. 2007). Furthermore, we add a paragraph discussing the limitation of the BARMOD/CUB velocity models. The BARMOD velocity model e.g. has a rather high resolution in the western Barents Sea and in the south-eastern Barents Sea, while the path density of Love and Rayleigh waves is rather low towards the southern Kara Sea (Levshin et al., 2007). However, we added a paragraph discussing the shallow low-velocity zone beneath the southern Kara Sea as it could be related to e.g. a plume or underplating.

Conclusions:

[26] Comment of reviewer: Line 6: indicating instead of indicate

Answer to reviewer: done

[27] Comment of reviewer: 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.

Answer to reviewer: Please see answer to comments 2 & 25.

[28] Comment of reviewer: Figure 1: Make it bigger. It has a lot of information. And add another figure with the location of databases used for this work. Answer to reviewer:

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done

[29] Comment of reviewer: Figure 3 and onwards: Acronyms should be explained in the captions.

Answer to reviewer: done

[30] Comment of reviewer: Figure 4: I would move 4f to figure 3.

Answer to reviewer: We added the top crystalline basement to Fig. 4 since it illustrates the influence of the cumulative thickness variations of the sedimentary megasequences. Now it is clear that larger depressions of the top crystalline crust correlate with domains of larger sediment thickness.

[31] Comment of reviewer: Figure 5: Very steep gradients in 5c should be removed or explained in the text

Answer to reviewer: As described in the manuscript the steep gradients are transitional areas which link the domains where the LAB topography is rather flat. Since the depth variations of the LAB between e.g. the western and eastern Barents Sea are extracted from the same dataset (BARMOD), we think that this is a robust feature. As discussed in the manuscript, a reason for the thin lithosphere beneath the western Barents Sea might be the rifting history and/or the opening of the NE Atlantic.

[32] Comment of reviewer: Figure 6: The authors should explain why the LAB crosscuts so many velocity isolines.

Answer to reviewer: Please see answer to comments 3 & 13.

[33] Comment of reviewer: 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.

Answer to reviewer: We agree with the author that the discussion on the LAB depth configuration and pre-collisional terranes is too speculative at this point. Therefore, we removed figure 7 from the manuscript.

[34] Comment of reviewer: 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.

Answer to reviewer: We admit that the correlation of the LAB with paleoterrains is too speculative at this point. Therefore, we relate the depth configuration of the LAB with the younger rifting history of the western Barents Sea and the opening of the NE Atlantic. The calculation of the continental LAB is based on the assumption that the observed greatest shear wave velocity reductions in the mantle are caused by first significant partial melting. Partial melting is assumed to reduce the viscosity even in small amounts (Fischer et al., 2010) and thus, results in significant decrease of shear wave velocities (see also answer to comment 13). The strong depth gradients of the LAB appear not due to the use of different datasets. The different datasets agree where they overlap as e.g. Zhang and Lay, (1999) with BARMOD in the Norwegian-Greenland Sea (see also answer to comment 31). Strongest depth gradients are observed beneath the eastern Barents Sea and Novaya Zemlya, two regions which are both covered by the BARMOD dataset. Consequently, we assume that the eastward deepening LAB with its narrow steep transitional domains is a robust feature. In the manuscript we do not claim to know the age of these velocity anomalies but put forward some plausible hypothesis.

Changes in the manuscript: We edited the discussion of the LAB in a way that the focus is more on the younger evolution rather than old lithospheric provinces. Furthermore,

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we discuss the limitations of the underlying velocity datasets.

[35] Comment of reviewer: 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).

Answer to reviewer: done

[36] Comment of reviewer: 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).

Answer to reviewer: done

Reference list

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Schweitzer, J., Kennett, B.L.N., 2007. Comparison of Location Procedures: The Kara Sea Event of 16 August 1997. Bull. Seismol. Soc. Am. 97, 389–400.

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Table A1

Horizon	Reference	Covered Region
Bathymetry	Jakobsson et al., (2012)	Entire study area
Earliest Eocene	Faleide et al., (1996)	Norwegian-Greenland Sea
	Fiedler and Faleide, (1996)	Norwegian-Greenland Sea
	Hjelstuen et al., (1996)	Norwegian-Greenland Sea
	Engen et al., (2006)	Eurasia Basin
	Engen et al., (2009)	Eurasia Basin
	Glebovsky et al., (2006)	Eurasia Basin
	Gramberg et al., (2001)	Eurasia Basin
Mid-Cretaceous	Brekhuntsov et al., (2011)	S Kara Sea – W Siberian Basin
	Faleide et al., (1993)	SW Barents Sea
Mid-Jurassic	Brekhuntsov et al., (2011)	Southern Kara Sea
	Faleide et al., (1993)	SW Barents Sea
	Johansen, (1992)	Barents Sea
	Kontorovich et al., (2010)	Southern Kara Sea
	Norwegian Petroleum Directorate	Southern Kara Sea
	Piskarev and Shkatov, (2012)	Eastern Barents & Kara Seas
Mid-Permian	Brekhuntsov et al., (2011)	Southern Kara Sea
	Henriksen et al., 2011b)	Barents Sea
	Ivanova et al., (2011)	Barents Sea/ Kara Sea
	Johansen, (1992)	Barents Sea
	Khutorskoi et al., (2008)	Barents Sea
	Nikishin et al., (2011)	Southern Kara Sea
	Piskarev and Shkatov, (2012)	Eastern Barents & Kara Seas
Top Crystalline Crust	Aplonov et al., (1996)	Eastern Barents & Kara Seas
	Drachev, (2011)	Eastern Barents & Kara Seas
	Gramberg et al., (2001)	Western Barents Sea
	Hauser et al., (2011)	Euopean Arctic
	Ivanova et al., (2011)	Barents & Kara Seas
	Johansen, (1992)	Barents Sea
	Myklebust, 1994)	Barents Sea
	Ritzmann et al., (2007)	Barents Sea
	Skilbrei, (1991)	Barents Sea
Moho	Aplonov et al., (1996)	Eastern Barents – Kara Seas
	Dahl-Jensen et al., (2003)	NE Greenland
	Hauser et al., (2011)	European Arctic
	Ivanova et al., (2011)	Barents Sea
	Kostyuchenko et al., (2006)	Eastern Barents & Kara Seas
	Minakov et al., (2012a)	Northern Barents Sea
	Ritzmann et al., (2007)	Barents Sea
Lithosphere-Asthenosphere Boundary	Levshin et al., (2007)	Barents Sea
	Kumar et al., (2005)	NE Greenland
	Shapiro and Ritzwoller, (2002)	Kara Sea
	Zhang and Lay, (1999)	Oceanic Domain
Faults	Faleide et al., (1993)	SW Barents Sea (Mesozoic)
	Faleide et al., (2008, 2010)	Western Barents Sea (Paleozoic)
	Gudlaugsson et al., (1998)	Western Barents Sea (Paleozoic)

Fig. 1.

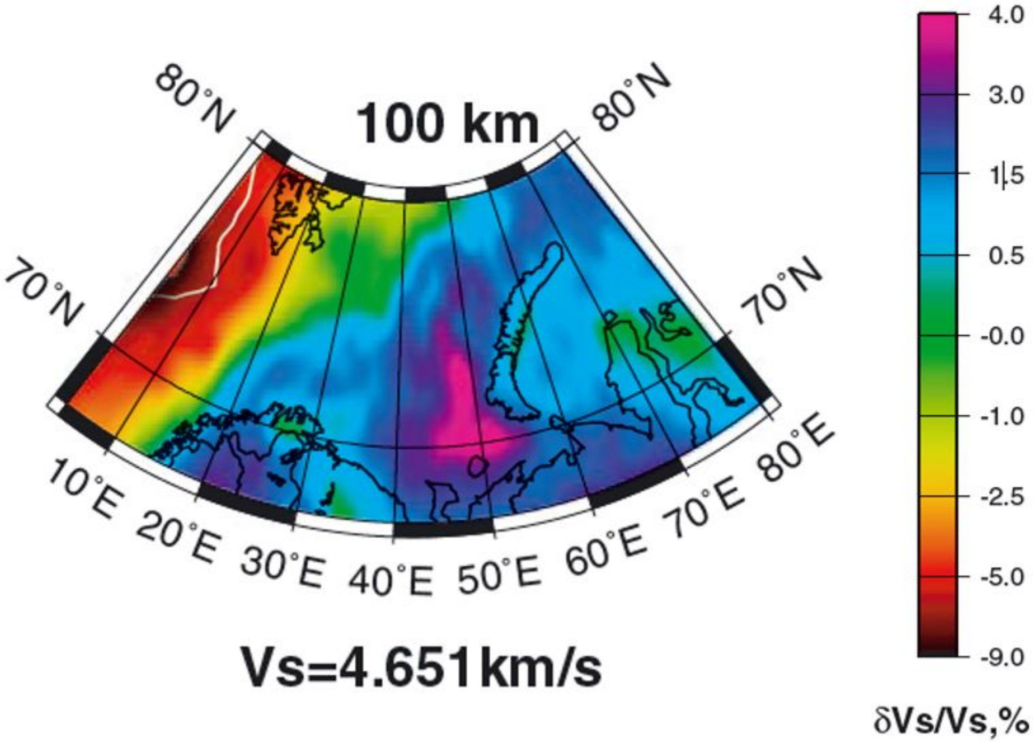
100 km
80°N
80°N
70°N
70°N
10°E
20°E
30°E
40°E
50°E
60°E
70°E
80°E
Vs=4.651km/s
4.0
3.0
1.5
0.5
-0.0
-1.0
-2.5
-5.0
-9.0
δVs/Vs,%

Fig. 2.

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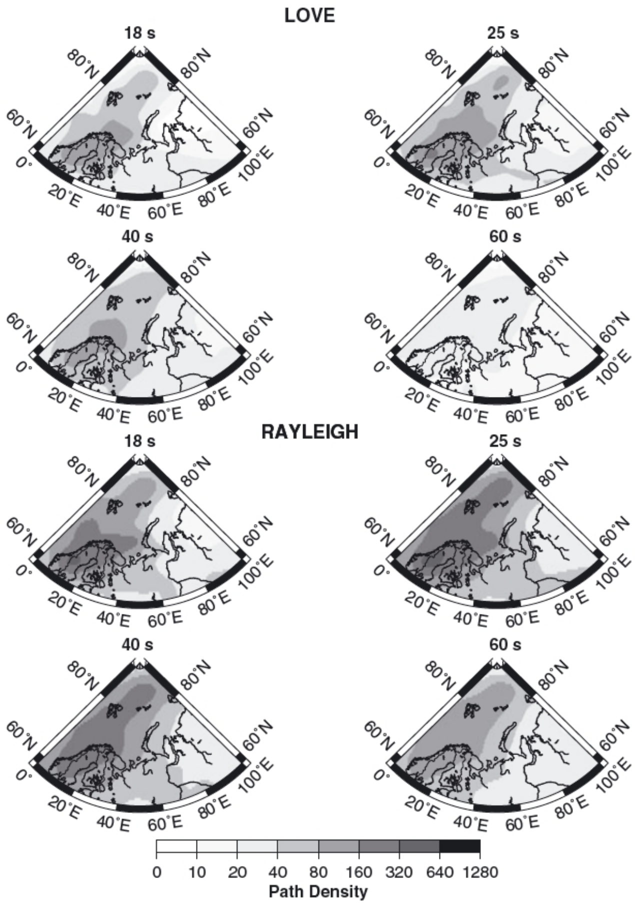

Fig. 3.