Our response (in blue) to comments by dr Perchuć (Referee #1)

General remarks

In general we were looking for asthenosphere (and LAB), which could be identified as a low-velocity channel in the 50-200 km depth range in Gutenberg's global model of the Earth (Gutenberg 1959). In this paper we are looking for lithosphere-asthenosphere boundary. The data from earthquakes are not as precise as from explosions. So, we do not investigate detailed multilayer lithospheric structure. Comparing to previous investigations, e.g. for FENNOLORA refraction long range profile, we can conclude that LAB depth is close to result of Guggisberg (1086) rather than Stangl (1980).

Detailed remarks

... reading the paper one may have doubts if authors had enough data for such a definite answer. In my opinion they had not. It does not mean there wasn't more data available. The authors used temporal selection (10 earthquakes from years 2008-2011). This choice of events, although guaranteed by good records, is absolutely insufficient for defining the structure of upper mantle for such a vast area as Baltic Shield. My concerns are raised especially by a quantity of data. The authors wrote that they had chosen 10 earthquakes and after quantitative analysis, they had erased "poor quality and noisy seismograms" from the sections. They did not mention what was the total number of the analysed records. However sections shown on figures consisted of around 350 analysed records. I realize that it would be difficult to present all the data but this amount seems to me precariously small. The data we used were recorded during period 2008-2011, including data from new net of Swedish stations. During this time we used all strong enough events recorded to far distances. In the beginning of data collection 17 strong events were choosen, however 8 from them were not enough good quality for far distances. For final 10 discussed events we had totaly 631 traces, which number was reduced after "cleaning". The example of "dirty" and "clean" section is shown in ResFig. 1. After cleaning, the number of 61 traces for this event was reduced to 41 traces in this section.

Lack of seismic data from seismic projects such as FENNOROLA, EUGENO-S, the TOR in the analysis resulted in a significant reduction in the reliability of the final conclusions. Taking into account as far as possible all existing data from a much longer period of time (particularly the use of data from experiments with controlled sources) would allow more accurate determination of the nature of the upper mantle, and I am convinced it would forbid from drawing final conclusions as explicit. Therefore, I believe that if the authors intended to answer the question posed in the title, they should have taken into account data from the projects listed above, make a quantitative analysis and only after this answer the question. Our idea was not to reinterpreted the data from such a projects as refraction/explosion FENNOROLA and EUGENO-S, but to use new good quality data. The passive TOR project (as well as SVEKALAPKO) is quite different story, with use of surface waves and receiver function.

That is why I think the article should be rewritten with added discussions on the currently existing views on the structure of the lithosphere presented by other authors, eg. Guggisberg, Hauser, Stangl and others. Both sections and models presented by above mentioned scientists are fundamentally different from the results and ideas delivered by the authors of the paper. In this paper we are looking for lithosphere-asthenosphere boundary. The data from earthquakes are not as precise as from explosions. So, we do not investigate multilayer lithospheric structure. This was done just Guggisberg and Stangl. However, their results are not the same, even not similar – see for that ResFig. 2. Both models have high and low velocity layers, but they differ significantly, and the difference in LAB depth reaches 100 km! So, I agree that also "models presented by above mentioned scientists are fundamentally different".

The authors are convinced – on the basis of nine - branches on traveltimes of non-existence of the LAB. Having a very diverse data (both in terms of area, as well as in the scope of registration) does not allow for clear identification of the structure of the upper mantle for such a large area. In the second point of Conclusions one can read: "No evidence was found for the asthenosphere beneath the

Baltic Shield – it has to be deeper than 200 km if it exists at all. Even if it exists deeper it could not be detected by “refraction” method. For distance larger than about 2000 km a shadow zone in the first arrivals would be masked by deeper waves from the “410” and “660” km boundaries". So, it doesn't means "non-existance of LAB". See examples with different LAB depth in ResFig. 3, which expains ides for looking for LAB from the shadow zone of first arrivals.

Excluding the event 10 (registrations to 300 km) for a number of branches on traveltimes, registrations start with more than 500 km (events 2, 3, 7, 9) and even above 1300 km (event 8). This configuration of data limits the statement about distribution of velocity directly beneath Moho. For distances above 1000 km seismic rays carry on information for tens of kilometers below the Moho. As written in general remarks – we were looking for far distance shadow zone corresponding to asthenosphere. In the case of Figure 8 clear LVZ is seen in distance of about 1800 km a clear "shadow zone" is visible (marked in Figure 8 by arrow). Similar shadow zone is observed for event in Spitsbergen (both in section and synthetics – Figure 9a,b)

Although the authors used the map of Moho (Grad et al. ~~2011~~2009), for the first data on traveltimes they extrapolated wave Pn. This is not always justified. The authors believe that the wave Pn recorded for the distance of 2000 km (Fig. 4b, in the text this is only mentioned) but when it comes to the analysis of individual traveltimes, they separate by themselves the waves in the first impulses on Pn and P waves. Likewise the final provisions of each branch of the traveltimes - many of them end up well before 2000 km (event 1 - 1500km, event 4 -1800km, event 6 - 1600km), which does not allow for precise determination of depth and thickness of the potential LAB. Adding to analysis data from FENNOROLA, is of fundamental importance because, for example event 4 is located exactly in the region of SP-I and SP-H and event 5 directly in the region of SP-B. Seismic sections from these shot points are of a rather good quality, from ca 10 km to the distance of well over 1500 km. The comparison with FENNOROLA sections is shown for shots B(nort) and I(south) with not "located exactly" but not far away events 5 and 4, respectively (shown in ResFig. 4 and ResFig. 5). The FENNOROLA sections show a detailed structure of the lower lithosphere, while our models produce an envelope of first arrivals, and do not differ significantly from explosion data.

Presented by Hauser and Stangl (1995) sections of SP-D to North (and not only) have a completely different character in the wave pattern from the section presented by the authors (see previous remark). To this results, it is necessary to respond, in both quantitative and qualitative analysis. Data from other projects, which are omitted by the authors, are data from EUGENO-S (SP-10), which prove interpretations of Hauser, Stangl, Guggisberg. It would be necessary to use also data from BABEL and TOR projects, which would enable wider view on the issue raised in the article. As mentioned earlier and shown in model comparison the Stangl and Guggisberg are completely different (ResFig. 2). How they can prove interpretations??

Weakness of data collected:
1. Considerable distances between the events (see Fig. 2) effects in lack of links of the various branches of traveltimes. It would be worth to see a map with marked locations from which the authors had information on the structure of the upper mantle. The final drawing on Fig. 14a is too general. In literature exist several more detailed maps showing thickness of the lithosphere. We agree: distances between the events are "considerable", in literature exist several more detailed maps showing thickness of the lithosphere. Some description one can find in manuscript, page 4: "First determinations of the lithospheric thickness beneath the Baltic Shield were obtained from analyses of fundamental-mode and higher-order Rayleigh surface waves. The dispersion of higher-mode data have been interpreted by Nolet (1977) to distinguish the thick lithosphere of the Baltic Shield from the thinner Western European lithosphere (see also Zielhuis and Nolet 1994). Cara et al. (1980) using higher modes found no need for a low-velocity zone in the mantle beneath northern Eurasia. They also argue that a nearly constant 4.5-4.6 km s^{-1} S-wave velocity is required in the uppermost 200 km. Using Rayleigh-wave dispersion data for the Fennoscandian region, Calcagnile (1982) found lid thicknesses up to around 135 km in the Bothnia – north-central Finland area with weak, if any, shear velocity contrast to the underlying layer. The surrounding areas are characterized by lid thicknesses up to around 75 km only. A stronger low-velocity zone with lid contrast 0.25÷0.45 km s^{-1} may be found in the Caledonian and the Baltic Sea area (Calcagnile 1982). An updated map of the

lithosphere-asthenosphere system in Europe (Panza 1985; Calcagnile and Panza 1987) shows much larger lithospheric thickness of the Baltic Shield, in the range 110-170 km, increasing to >190 km in its central part. The Baltic Shield model obtained later by Dost (1990) shows an absence of the low-velocity layer, while density seems to be lower in 200-350 km depth". Other maps (e.g. by Artiemieva) are not seismic in their nature, in the sense of Gutenberg's model.

2. Uneven distribution is not conducive to a two-dimensional modeling of wave field although it does not preclude it. Misunderstanding. This data set can not be interpreted as 2D. As written in page 6: "Recording stations are not linearly aligned which would permit for easy 2D interpretation of the structure along profile, but scattered in a wide corridor of a few hundreds km width. This means that two stations at similar epicentral distance may lie in locations with significantly different altitude and Moho depth. For modelling the lower lithosphere and asthenosphere along profiles, the original data needs to be time corrected for topography and the Moho depth for each event and each station location". Which was done.

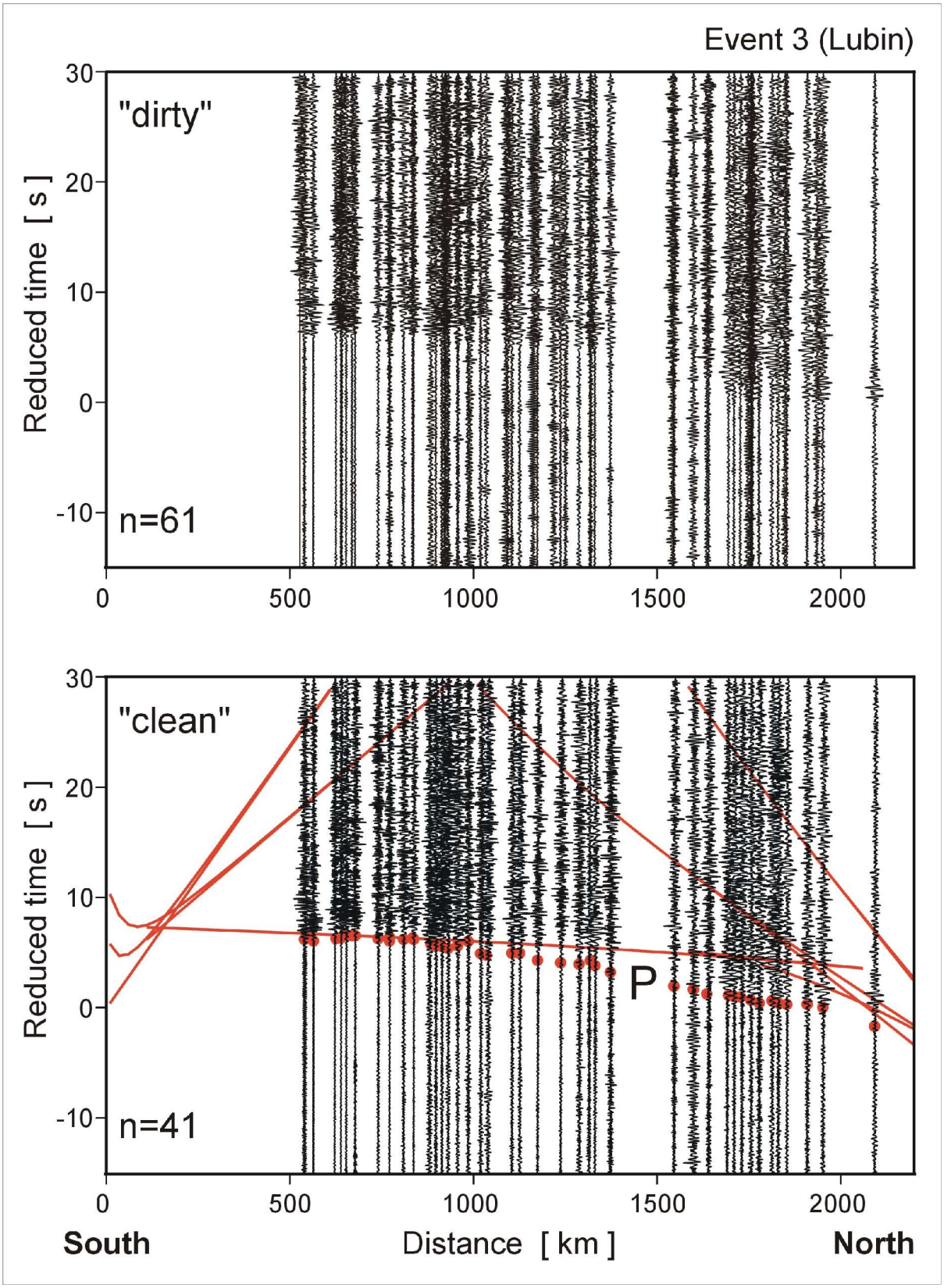
3. Other weakness is a significant distance between the experimental points on individual branches (eg on Fig. 12 - between the distances from the event 3 from about 280 km to 550 km, authors have only one experimental point (red dot).) Yes, the distance between the experimental points is for a few sections significant, but crucial are records at far distances.

4. On the presented map the regions of the lithosphere (Fig. 14 a) "navy blue dot" zone was defined on the basis of three stations and three data (Fig. 13). If so, then ...?! If I misread, I'm sorry but I ask for some explanations. Only our comment could be that three good quality points are better then 30 bad quality ... Why ignore them?

5. Velocity of reduction in Fig. 8 is rather not 8 km/s, is it? Sorry, it is 8 km/s, however form of this travel time could be suprise for somebody who never used records over 1500 km distance.

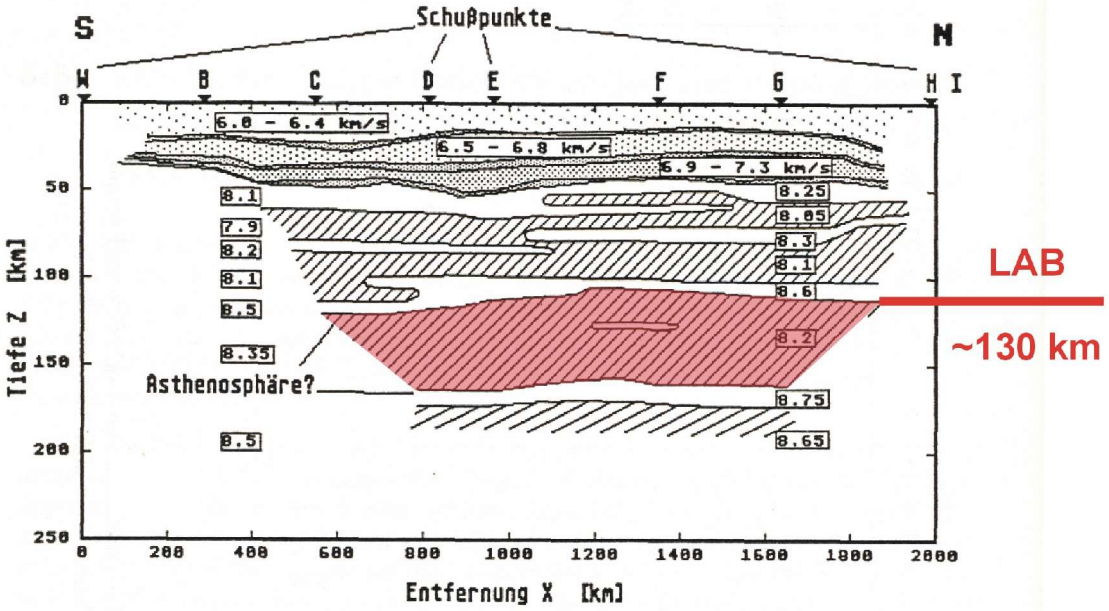
6. The composition of drawings with sections is inconsistent. Individual sections are presented with additional figures, causing quite much a confusion. In some cases, additional figures are: map, in other synthetic seismograms, in one case the sections P and S, yet another map and histogram, and only one model. I do not understand what is inconsistent? section is section, map is map, histogram is histogram, and all of them were precisely recognized by Referee #1.

However I am very much interested in results, I expect to see this paper in new version. My suggestion is to take into consideration all the remarks and then send it for publishing. Moreover the answer to the question "LAB or LID? - LID beneath the Baltic Shield!" I would treat with more modesty. It is rather doubtful to see this paper in new version, while Referee #2 categoricaly suggests rejection.

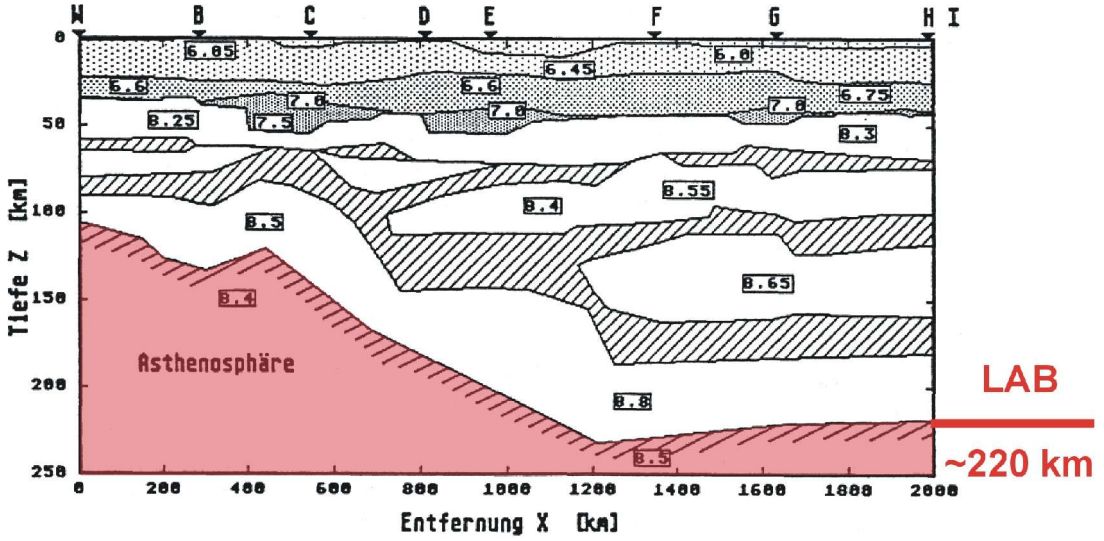

Event 3 (Lubin)
"dirty"
n=61
"clean"
n=41
P
Reduced time [s]
30
20
10
0
-10
0
500
1000
1500
2000
South
Distance [km]
North

ResFig. 1.

Stangl (1990)



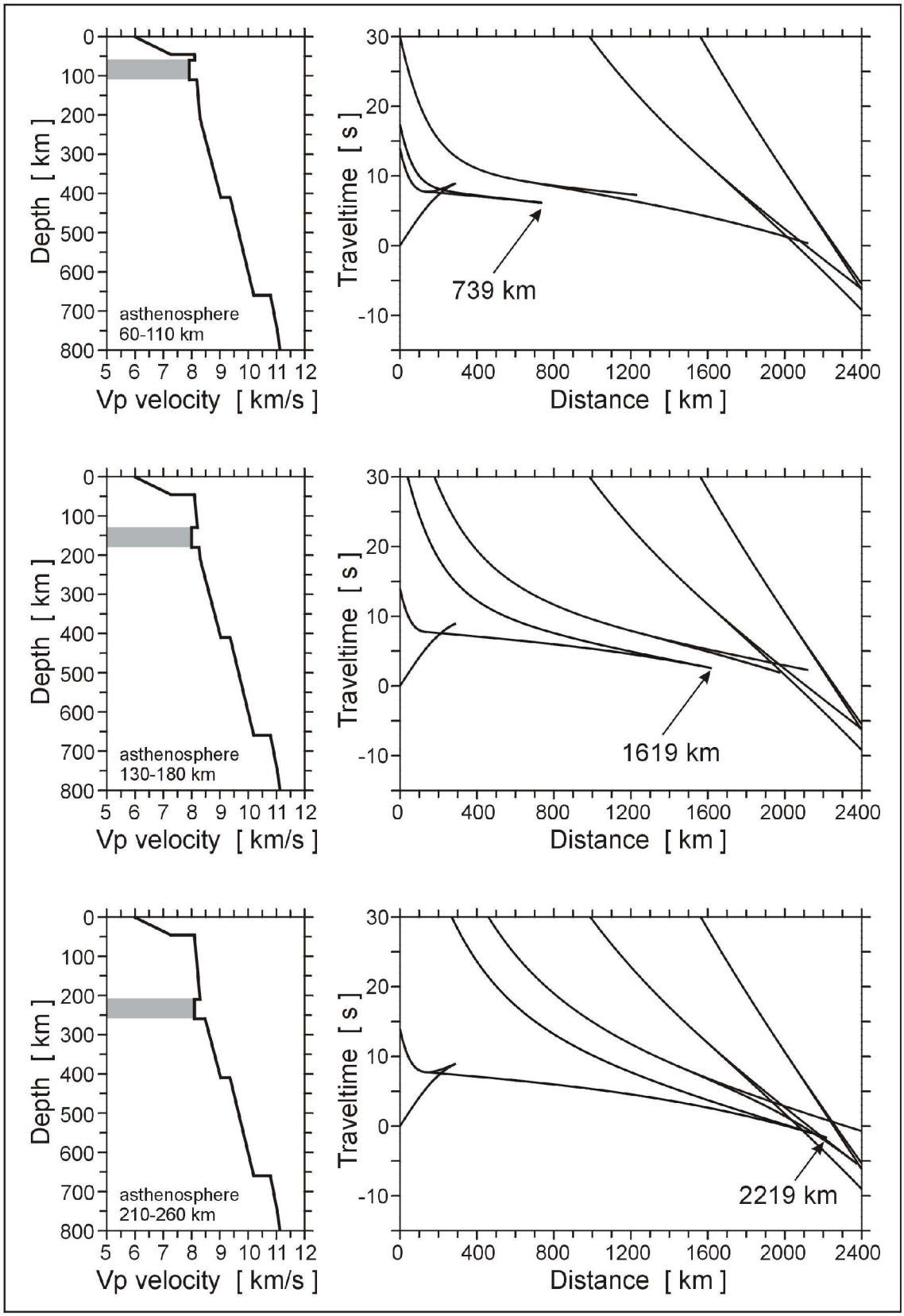
Figur5.22: Zweidimensionales Strukturmodell der Lithosphäre in Skandinavien für die P-Wellen-Geschwindigkeit. Schraffierte Bereiche = Zonen erniedrigter Geschwindigkeit.



Figur 7.34 Zweidimensionalen Modellierung unter den Hauptprofil des FENNOLORA 79 Projekts.

Guggisberg (1986)

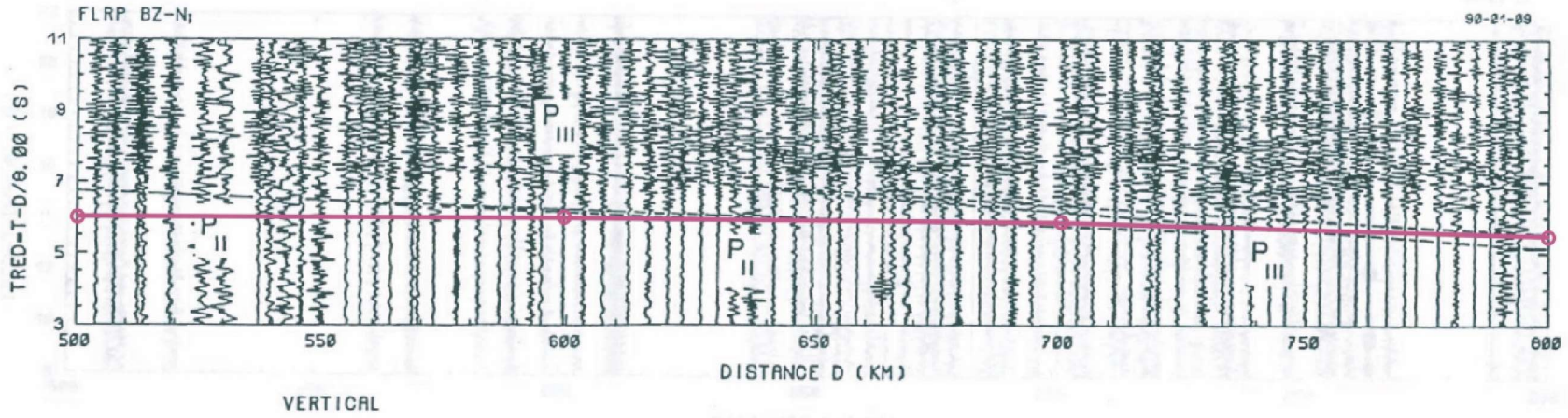
ResFig. 2.


Depth [km]
Vp velocity [km/s]
asthenosphere
60-110 km
Traveltime [s]
Distance [km]
739 km
asthenosphere
130-180 km
1619 km
asthenosphere
210-260 km
2219 km

ResFig. 3.

ResFig. 4.

FENNOLORA SP B to north

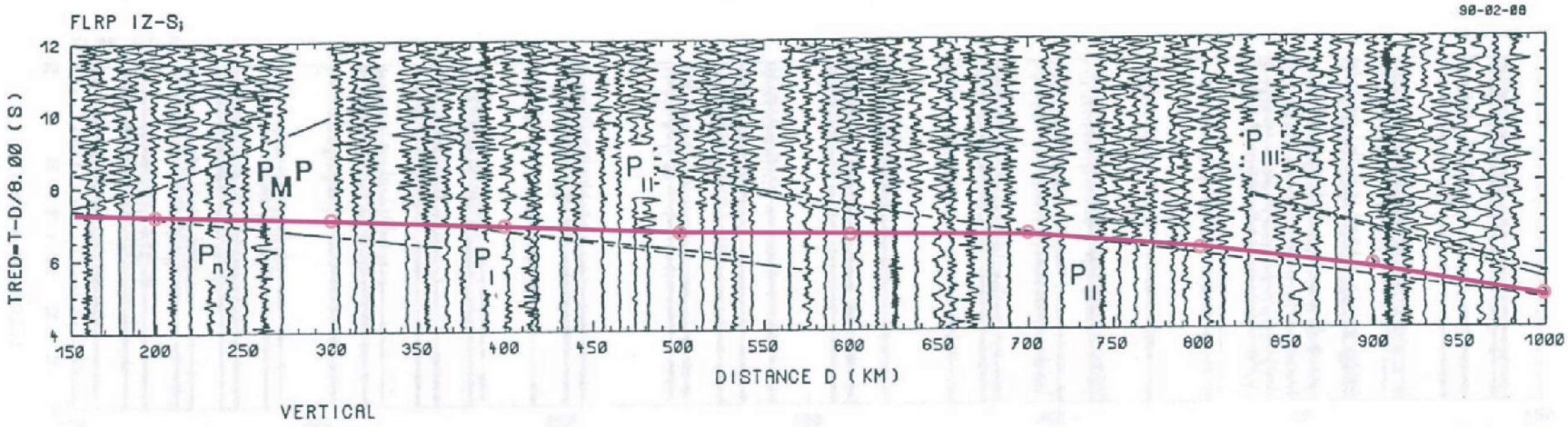


III.3a B-Nord: P-Wellen Seismogramme der Vertikalkomponente

Event 5 (Scania)

ResFig. 5.

FENNOLORA SP I to south



III.30a I-Süd: P-Wellen Seismogramme der Vertikalkomponente

Event 4 (Varangerfjord)

Our response (in blue) to comments by Referee #2

General remarks
In general we were looking for asthenosphere (and LAB), which could be identified as a low-velocity channel in the 50-200 km depth range in Gutenberg's global model of the Earth (Gutenberg 1959). In this paper we are looking for lithosphere-asthenosphere boundary. The data from earthquakes are not as precise as from explosions. So, we do not investigate detailed multilayer lithospheric structure. Comparing to previous investigations, e.g. for FENNOLORA refraction long range profile, we can conclude that LAB depth is close to result of Guggisberg (1086) rather than Stangl (1980).

Detailed remarks
The title of this manuscript is intriguing, as it is unclear why there should be a contradiction between the LAB and LID. The authors do not define the two terms, but I thought that the LAB is the base of the LID, in which case there is no need to pose the question. The first page of introduction contains description of lithosphere LID) and lithosphere-asthenosphere boundary (LAB). Not changing significantly the title could be changed to "LAB abd LID".

The real content of this manuscript is presentation and traveltime interpretation of seismic record sections acquired on the Swedish seismometer array for local earthquakes around the Baltic Shield. Not only Swedish, also all other avaiable in and around of Baltic Shield.

The seismic interpretations do not add new knowledge about the mantle in the Baltic Shield, and the uncertainties are large, although not discussed in the manuscript. As such, the interpreted velocity models probably include several non-constrained features, but it is unclear which features are robust. Our models are very simply, so they rather do not include non-constrained features. We are looking for a low-velocity channel in the sense of Gutenberg's global model of the Earth (Gutenberg 1959), using classic approach comparing e.g. receiver function and surface waves.

In principle the manuscript addresses significant relevant scientific questions within the scope of SE, but it does not present novel concepts, ideas or tools, although the record sections represent new data. I recommend rejection based on lack of novelty and unsupported conclusions. Yes, we agree.

Detailed review
The seismic sections are sampled with a density of ca. 40-100 km which probably is too coarse for modeling velocity-depth profiles at the detail presented. Further all record sections are constructed for data from a wide angular fan originating from the epicenter of the source instead of along a linear profile, such that substantial lateral smearing may occur. Because of the coarse sampling the interpreted models are highly nonunique and the models may therefore be considered subjective. This is particularly important for the discussion of low velocity zones (LVZ) in the Baltic mantle, because the interpreted data do not possess the resolution required for identification of thin (<50 km thick) LVZs. Low velocity zones (LVZ) in the lithosphere were not modelled, because of not enough good resolution of our data. This was done using much more detailed data, e.g. from FENNOLORA profile. In our case travel time is an envelope of first arrivals – see ResFig. 1 and ResFig. 2.

The profiles are non-reversed and the models therefore can only represent apparent velocity and depth, as the trade-off between velocity and dip cannot be resolved. In fact we use wide band of stations, not in the sense of narrow profiles. In this case we not expect reversed profiles, and after applying time corrections 1D modelling was done. As written in page 6: "Recording stations are not linearly aligned which would permit for easy 2D interpretation of the structure along profile, but scattered in a wide corridor of a few hundreds km width. This means that two stations at similar epicentral distance may lie in locations with significantly different altitude and Moho depth. For modelling the lower lithosphere and asthenosphere along profiles, the original data needs to be time corrected for topography and the Moho depth for each event and each station location". Which was done.
The authors use a standard crustal model for the interpretations despite it is well known that there is significant lateral variation in seismic structure of the Baltic Shield. The authors present single models for each record section, even though it is obvious that a wide range of velocity models may fit

the data to the same degree as the models presented by the authors. These uncertainties are not discussed in the manuscript, which questions the conclusions. Lateral variations in seismic structure of the Baltic Shield are significant, however average velocity seems to be rather stable and well described by our formula (1). Knowing crustal thickness this effect could be well determined. On the other hand P-wave lithospheric first arrivals are recorded in long distance intervals which permitt for precise determination of the velocity. In our models uncertainties are of the order ±0.05 km/s or even smaller.

All seismograms include strong reverberation which could indicate substantial heterogeneity in the seismic structure, but these aspects remain largely unnoticed. The Swave picks similarly indicate substantial heterogeneity from the scatter in arrival times. I am surprised to learn that the asthenosphere is a thin low velocity zone below the Baltic Shield. This interpretation contradicts many other views on this subjects of an asthenosphere continuing down to the transition zone. Some reference should be included for this statement. I miss a discussion of the mid-lithospheric discontinuity (e.g. Rychert, Rondenay, Fisher, Shearer) and its relation to a possible intra-lithospheric LVZ (e.g. Thybo, Gorman, Karato) in relation to the presented models. See previous comment concerning LVZ. We interpret only first arrivals, however problem of reverberation is mentioned e.g. for event at Spitsbergen (Figure 9a,b) and event at Skagerak (Figure 11). This interpretation does not contradict many other views on this subjects, but gives effective model (average) of the lowe lithosphere.

In general, the manuscript include much self-referencing. The references on the evolution of the Baltic Shield are limited to a (self-)reference on the East European Craton, instead of providing an overview of the evolution of the shield itself based on the substantial existing literature. Most of self-references are related to crustal models used for calculation of corrections. I am sorry, but I was a co-author of a few papers concerning crustal structure beneath Baltic Shield. Could you suggest other papers with results for SVEKA'81, FENNIA, SVEKA'91? Please pay attantion that other authors of papers with FENNOLORA, Blue-Road, BALTIC, POLAR are also cited.

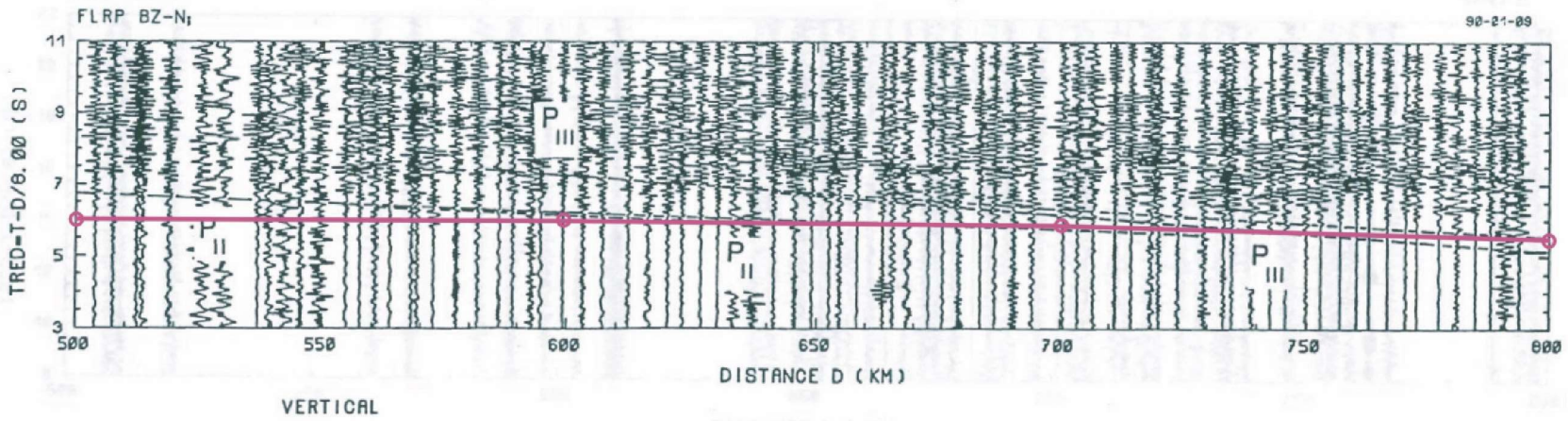
The references to interpretations of the FENNOLORA data (the key high resolution seismic data set on the Baltic Shield) are only to the thesis by Guggisberg (1986) instead of his published papers, and other mantle interpretations of the same dataset are not even mentioned, e.g. Stangl (thesis and EGT volume paper), Perchuc, Abramovitz. Likewise, new regional interpretations of the Baltic Shield based on teleseismic recording are not referenced (e.g. TOR Working Group, Olsson, Eken, Brunetton). The thesis by Guggisberg (1986) give a good documentation of seismic sections and models. Our idea was not to discuss all previously published results, but to show LAB from the new data ... See comparison with Stangl model in ResFig. 3. The results of Guggisberg and Stangl are not the same, even not similar – models in ResFig. 3 have high and low velocity layers, but they differ significantly, and the difference in LAB depth reaches 100 km!

The discussion of the Moho map does not refer to interpretations of data from the BABEL, Eurobridge, EugenoS experiments. One earthquake had its epicenter close to the northernmost FENNOLORA shotpoints; it would be relevant to compare the data. See ResFigs. 1 and 2. Some data sections are merely presented but not interpreted. There is no discussion of the Moho map, simply we used only digital data for crustal time corrections. About profiles: BABEL is rather nearvertical reflection profile, EUROBRIDGE is outside of frame we used, EugenoS (profile 4) was used, it is just left/down profile in the frame (Figure 3).

The authors do not identify any lvz from refractions, but this is also impossible and therefore not surprising. The use of English language may be improved, and the wording lacks precision. Some locations referred to in the text are not shown on the maps. In our opinion LVZ were identify for two events (Figures 8 and 9). For other we conclude tn the second point of Conclusions: "No evidence was found for the asthenosphere beneath the Baltic Shield – it has to be deeper than 200 km if it exists at all. Even if it exists deeper it could not be detected by “refraction” method. For distance larger than about 2000 km a shadow zone in the first arrivals would be masked by deeper waves from the “410” and “660” km boundaries". So, it doesn't means "non-existance of LAB".

ResFig. 1

FENNOLORA SP B to north

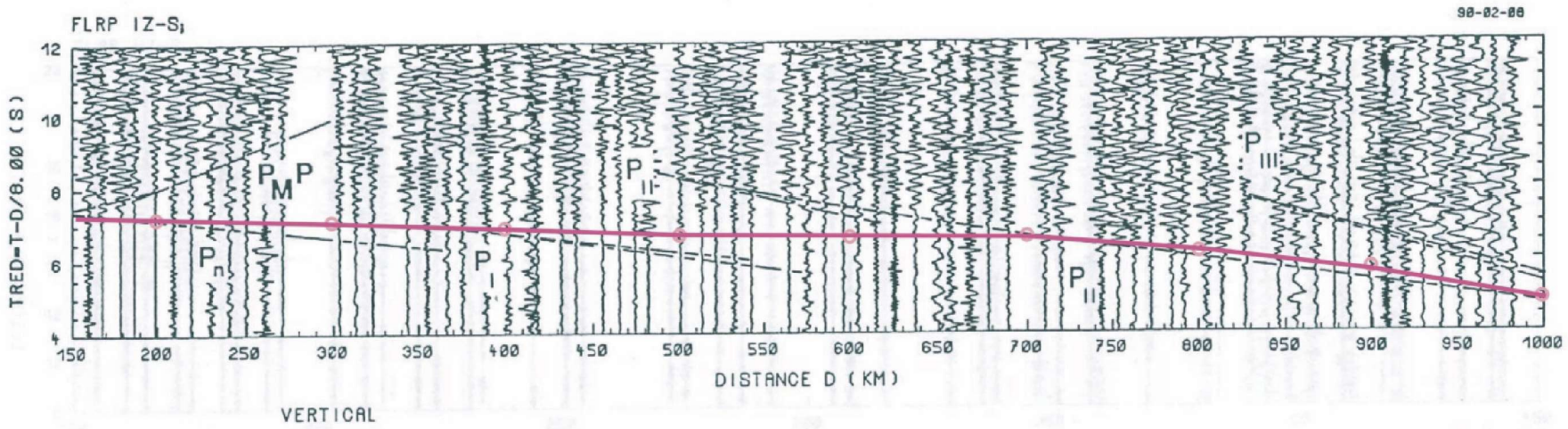


III.3a B-Nord: P-Wellen Seismogramme der Vertikalkomponente

Event 5 (Scania)

ResFig. 2

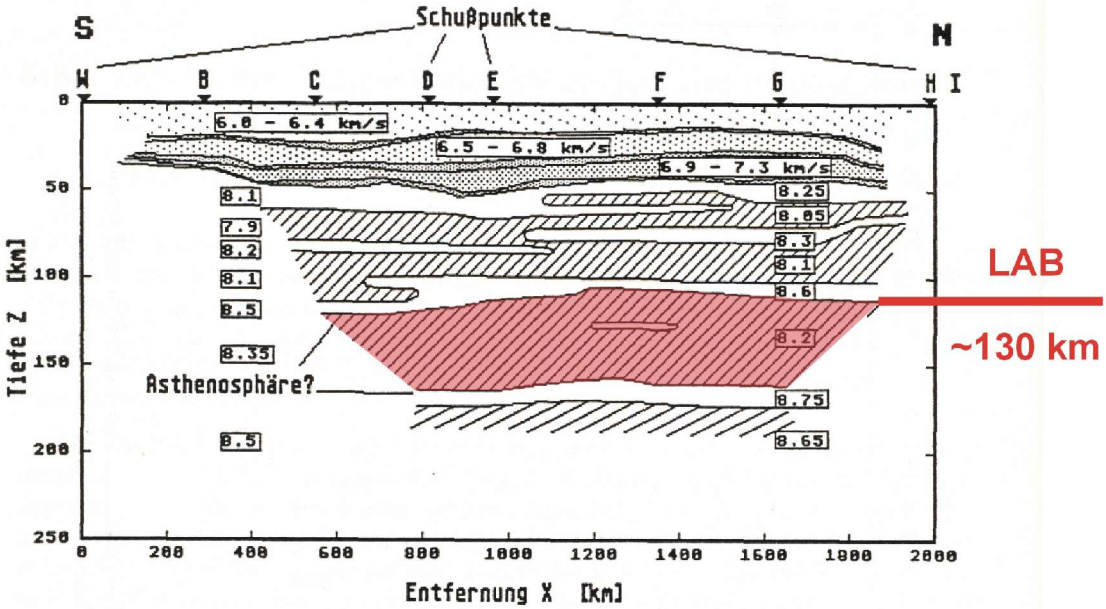
FENNOLORA SP I to south



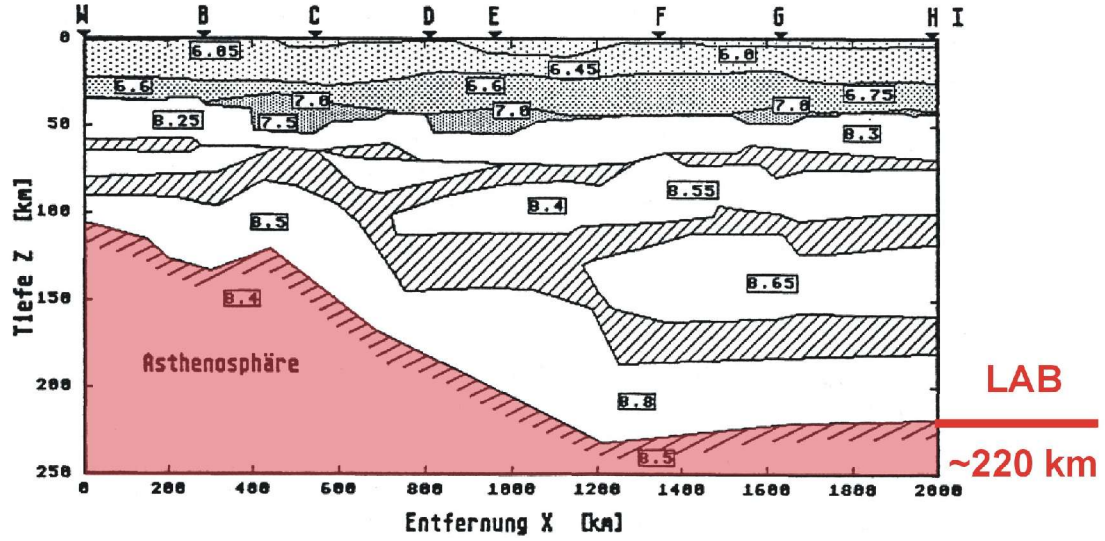
III.30a I-Süd: P-Wellen Seismogramme der Vertikalkomponente

Event 4 (Varangerfjord)

Stangl (1990)



Figur5.22: Zweidimensionales Strukturmodell der Lithosphäre in Skandinavien für die P-Wellen-Geschwindigkeit. Schraffierte Bereiche = Zonen erniedrigter Geschwindigkeit.



Figur 7.34 Zweidimensionalen Modellierung unter den Hauptprofil des FENNOLORA 79 Projekts.

Guggisberg (1986)

ResFig. 3