Solid Earth
Discussions

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Interactive comment on "Seismic visibility of a deep subduction channel: insights from numerical simulation of high-frequency seismic waves emitted from intermediate depth earthquakes" *by* W. Friederich et al.

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Response to comments of Reviewer T. Gerya

- *Upper boundary of subduction channel is assumed to be planar whereas numerical thermomechanical simulations often suggest that upper boundary of the serpentinized channel in the forearc mantle can be strongly corrugated (e.g., Gerya et al., 2006; Angiboust et al., 2012; Baitsch-Girardello et al., 2013). This may affect resulting seismograms in a significant way and needs short discussion.*

Our model of the deep subduction channel (DSC) is intended to represent an end-member model that avoids complexity and makes simple assumptions where detailed knowledge is not available. In the initial stage of the study, it was not at all clear that the presence of a DSC would completely modify the guided wavefield. Now that we know it does, further studies may investigate the influence of plausibe modifications of the propoerties of the DSC, such as geometry but also anisotropy and heterogeneity. We agree that a corrugated upper boundary of the DSC would influence the shape of the guided waves and certainly produce scattering. Crucial here is the ratio of the characteristic spatial scale of the corrugations and the dominating wavelength. If the ratio is about 1, scattering is strongest. Another controlling factor is the amount of deviation from planarity. Without doing numerical simulations, we are currently not in a position to speculate under which conditions the characteristic channel pattern breaks down or changes shape.

- *Would also be good to clearly indicate possible "channel features" on the seismograms from the Hellenic subduction zone — they are difficult to see for inexperienced readers.*

The point here is that we do not see these signatures either, because the data are not suited for this purpose. The channel signature only becomes visible in entire record sections comprising seismic traces from seismographs densely placed on a line or profile. The available data only tell us that we do observe long-lasting, high-amplitude P-wave signals in the forearc. How these signals correlate along a seismic line and whether they would display the pattern described in the manuscript is unknown.

- *P. 1464, section 10. Gerya & Stoeckhert, 2002 is not a numerical modeling study. More appropriate reference is Gerya, T.V., Stoeckhert, B., Perchuk, A.L. (2002) Exhumation of high-pressure metamorphic rocks in a subduction channel - a numerical simulation. Tectonics, 21, Article Number: 1056.*

Thanks for recognizing this mistake. We used the wrong bibtex-key when citing the paper. The mistake will be corrected in the revised version.

- *P. 1464, section 25. "turbulent flow". This term is typically used for describing inertia driven flows in low-viscosity fluids. Better to use some other term, e.g. "chaotic flow" or "strongly perturbed flow". P. 1471, section 5, should be "dry diamOnd eclogite"*

The manuscript will be changed accordingly.

Response to comments of anonymous Reviewer 2

- *Abstract: Some mention of the possible trade-offs between different assumptions (background model, source location etc), and how this paper attempts to reduce that trade-off would be good.*

The details of wave propagation certainly depend on the velocities chosen for the channel and the surrounding material. Complexities in the crust will also play a role. However, we think that the decisive and controlling factors that shape the observed wavefield at the surface are (1) the low-velocity property of the DSC and (2) the existence of a velocity contrast between subducted crust and subducted lithospheric mantle. They are responsible for the guided wave trains leaving the subduction channel as shown in Fig. 8. And the major differences between wavefields leaving the channel (see Fig. 8a and b) are caused by the difference in width and length of the wave guide with and without DSC. We do not think that changing the velocities in the lithospheric mantle or within the DSC or in the mantle above the DSC within reasonable bounds will significantly modify the characteristics of the guided waves as long as the low-velocity character of the DSC is preserved. We will add a sentence to the abstract that refers to the controlling role of low-velocity property of the DSC.

- *p. 1465,l.15-25: These are strong and important statements ("must be highly heterogeneous"), some references would therefore be appropriate. In particular, this heterogeneity inside DSC should be rather important for all aspects of the manuscript, thus more detailed explanation in terms of heterogeneity scale and amplitude is expected as well.*

The DSC hypothesis is entirely based on geological, petrological and geochronological information, obtained from exhumed high P/T metamorphic rocks. A fossil DSC to be exposed at the surface in its entity is precluded by its mere character and complex exhumation mechanisms. High P/T metamorphic terranes, suspected to be related to a subduction channel, are highly variegated in terms of rock composition, from map scale (e.g. Angiboust et al., 2011, their Fig. 2; Agard et al., 2009, their Fig. 4) down to the scale of the individual outcrop (e.g. Federico et al., 2007, their Fig. 1). Based on these observations the length scale of heterogeneity spans orders of magnitude, from meters to kilometers (Krebs et al., 2011; Grigull et al., 2012). The amplitude of heterogeneity is constrained by the contrast in mechanical properties between basic eclogite and serpentinite, these rock types probably representing extremes. In our simple model we therefore assume a composite material with eclogite blocks embedded in a serpentinite matrix, block size being roughly one order of magnitude smaller compared to the wavelength of P-waves. For the moment, we do not see a more appropriate way to account for the structural and compositional complexity revealed by any geological map of high P/T metamorphic terranes. While homogeneity can probably be excluded, heterogeneity requires extreme oversimplification. As long as a systematic survey of heterogeneity dedicated to the needs of seismology in terms of scale and amplitude is not available, we need to start with a simple model. Our assumption is that the major source of seismic velocity heterogeneity of the DSC is the contrast between the eclogite blocks and the surrounding serpentinite matrix. Other aspects such as compositional heterogeneity or variations of mineral assemblage of the matrix are assumed to produce much weaker varia-

tions of seismic velocity. We will add a respective statement to the manuscript. Regarding the scale, here we assume block sizes up to a few 100 meters while wavelength for P-waves is around 3 km. This is the justification for the homogenization approach. This issue is addressed in section 2.6.

- *p. 1466,l.25: "rough estimate only": Statements like these, alluding to the uncertainty in defining the background model, are rather important in the context of the final results. Is it not possible to quantify these statements at least in the model context, but if possible also in the discussion of the results?*

With hindsight this statement appears slighty overexaggerated since typical seismic velocities within subducting lithosphere are fairly well known from seismic tomographies. The same applies for the velocities in the mantle wedge. Uncertainties of seismic velocity within the subducted oceanic crust and DSC are certianly higher. But here, we think that the low-velocity aspect is decisive and not the explicit value of velocity. The velocities predicted from the assumed mineral assemblages are within the range of values obtained by seismic tomography. We will relativate the statement in the revision.

- *p.1467,1468: The description of the subduction zone should be added to Fig. 2, would be much easier to follow than based purely on these bullet points.*

We will add a figure that shows the subdivisions of the model referred to on page 1467.

- *p.1469,l.9-15: Assuming that significant anisotropy plays a role (which some authors suggest, certainly in the wedge), could the authors at least identify a future strategy to derive anisotropic models in a similar fashion?*

Prediciting anisotropic elastic constants from mineral properties requires laboratory measurements of the Grüneisen tensor. For setting up anisotropic models, a more pragmatic approach would certainly be to derive isotropic constants from

mineral properties, calculate seismic velocities of the bulk rock by averaging over minerals and then add a few percent anisotropy to the seismic properties of the bulk rock. This way, models taking into account anisotropy can be realized.

- *p.1470,l.1-5: By means of boot-strapping, would it be possible to get a sense of the variability in such a database? Given the sheer number of assumptions, it is difficult for the reader to keep track of how any uncertain dependencies map into the final model. As such, and more generally, I would have hoped for a different realization of the background model, and analysis of its effects similar to the shifted sources. This is, to my mind, the single most important piece missing in the manuscript.*

The uncertainty of the data base of mineral parameters only plays a role for the subducted oceanic crust (MORB, Fig 3a) with its complicated phase diagram. We presume, however, that the effects of errors in the mineral data base on seismic velocities are much smaller than effects due to uncertainties related to e.g. kinetic delays in metamorphic reactions or influences of temperature and stress gradients. Hence, exploring the effect of varying mineral parameters on the velocity model could be elucidating but is not really relevant. We think that our conclusions on the guided wavefields do not depend on details of the velocity distribution in the oceanic crust. What is important is its low-velocity character at shallow depths and the gradual increase of velocity with depth, reducing the velocity contrast to the mantle below. For the mantle parts of the model (Harzburgite), mostly mineral parameters for the rather simple phases F, G and K (Fig 3b) are required which are well known.

- *Section 2 in general: A survey over different types, classes and styles of DSC would be better instead of mixing all properties into one "preferred model". What about the mineralogical and dynamic settings of other zones (circum-Pacific), even if undetected? To me, this predictive capability" of numerical modeling seems to be one of the most attractive components.*

As we tried to emphasize in our outline, the concept of a DSC is based on geological, petrological and geochronological information obtained at the surface. For the most part, such information is highly fragmentary (though compelling in its nature and internal consistency), and typically obtained after exhumation in a collisional belt. A "quenched" DSC structure or snapshot of an active system is not available for analysis. As such, numerical simulations are the only way to depict possible histories of DSCs, choosing starting and boundary conditions which at least appear realistic or common sense. There is a lot of ambiguity and bias. As such we feel that - at present - we are still far away from any chance to subdivide DSCs into classes and styles. This may change as soon as presently active subduction channels at depth can be identified by seismological means. Only in this case true snapshots may be correlated with current state and history of the respective subduction system, and eventually open the chance to identify different types. Our present contribution is thought to provide a small step into this direction.

- *p. 1472: Somewhere within this page, I believe anisotropy both within the slab and in the ambient mantle should be discussed, including the relative validity in neglecting (considering other approximations in defining the database).*

It is hard to speculate on the effect of seismic anisotropy in the slab and ambient mantle without having done simulations. We are aware from seismological work that there is weak anisotropy in the slab and also in the mantle wedge. We do not believe that anisotropy in the ambient mantle would massively change the behaviour of the guided waves. Large-scale anisotropy in the DSC or oceanic crust is much more relevant.

Since the motivation of our work was to search for first-order effects of a DSC on the wavefield in the forearc, we avoided to bring in other complications into the model the effects of which might later be wrongly attributed to the presence of a DSC.

- *p.1474,l.9-10: How about attenuation in the wetter parts of the model, and wedge? Must result in a strong amplitude effect in particular for shear waves.*

Yes, we agree. Stronger than assumed attenuation in the wedge would definitely reduce the amplitudes of the guided wave phases. But, these phases are strong and remain visible as we know from seismic observations in the forearc of subduction zones. In addition, the signatures of models with and without DSC differ with respect to the large-amplitude phases and should remain visible (if they exist) also in case of stronger attenuation in the wedge.

- *p.1476, section 3.1: Please discuss the limitations of using 2D modeling in the context of the derived background model.*

2D modeling implies line instead of point sources leading to less geometric spreading than in 3D. Thus, amplitudes in 2D are overestimated. In addition, 2D seismograms are phase-shifted with respect to 3D ones. For analogous reasons as discussed above with respect to attenuation, we believe that both effects are uncritical for the generation of the channel signature.

- *section 3.2: Explosion source: I have some reservations about using such sources in a study like this one. In the end, the aim is to show what effects from the DSC may be visible in seismic records. Thus, it does not help to argue against shear waves "obscuring important features of the P wavefield". A compromise would be to use a double couple (as done later), but then analyze the divergence and curl components of the wavefield separately. Again, I wish to emphasize that the crucial bit is not to create a (unrealistic) scenario in which the effects are visible, but to at least attempt to use realistic structure and sources to see *whether* it can be detected, even if those assumptions are (inevitably) still strong.*

This is a valid argument. But it is not really harmful in this special case. What matters in the simulations is the energy radiated into the very narrow sector con-

taining the DSC and the subducted oceanic crust. Assuming that earthquakes exist in nature that do radiate maximum energy into this sector (if there aren't any detecting a DSC would become really hard) we find it legal to replace the double couple mechanism by an explosion because results will only marginally differ. In this way we could avoid scaling problems in the snapshots owing to the larger amplitudes of the S-waves. Moreover, Fig. 16a and b, for which we used a double couple source with suboptimal P- and S-wave radiation into the channel, demonstrates that the different signatures of P-waves in models with and without DSC are still obervable. In addition, taking the divergence of a double-couple wavefield suppresses the S-wave signals and should result in snapshots not much different from those for an explosion source.

- *P.1477-1480: While interesting, this is quite lengthy to follow, and the (impressive) eye for detail may somewhat obscure the bigger picture. How about highlighting a few (less) effects, and carry those through to the end. Maybe a reorganization into paragraphs tracing each effect from beginning to end may help the reader in keeping track.*

We tried hard to give the reader an understanding of the complicated wavefield. Apparently, we did not fully succeed, maybe because we developed too much sympathy for the fascinating details of guided wave propagation. We shall try to condense the material in this section in the revised version.

- *p.1480,l.16: This is clearly a very central issue and concern of the study: Earthquake sources are notoriously difficult to locate especially at such depths, and the mentioned, binary trade-off may well mean that this issue is unresolvable in terms of seismic evidence for the DSC. This should be made clearer and picked up again in the discussion.*

First, I would like to clarify that we get the channel signature for sources in the oceanic crust **and** in the DSC. The no-channel signature is only obtained for a

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source in the subducted lithospheric mantle. Regarding an experiment, it may happen that we can record earthquakes from both regions. If we can observe a channel signature for one and a no-channel signature for the other, this binary trade-off could even become a means for high-precision location of intermediate-depth earthquakes.

- *p.1481, shear waves: Chances are of course, that shear waves do not help but obscur any compressional evidence for DSC. Again, a study with divergence and curl may elucidate such questions.*

We think that Fig. 16 (source depth 108 km) shows that P- and S-wave signals are fairly well separated in time, if the source is deep enough. Therefore, shear waves obscuring P-wave signal should not be an issue. However, late P-wave and converted PS-signal may obscure S-wave evidence. Again Fig. 16 demonstrates that differences in the S-wavefield can still be clearly identified (phases 3a, 3b, 3c in the left panels and phase 3 only in the right panels).

- *section 3.4: This section on actual data seems to add little to the content of the paper. I do not necessarily suggest to leave this out as data is always crucial, but maybe a little more focus on this section would be useful (especially given the detail to which the synthetic results were analyzed).*

The data available to us from the Hellenic subduction zone do not allow to identify a channel signature because station distances are much too large and stations are not located on a line to correlate phases. We will reduce the number of panels here and elaborate on the text.

- *p.1483,l.23: Do you know of surveys where this is the case? As this is a prerequisite, might be best to start from such data sets.*

To my knowledge, the MEDUSA experiment in the Ionian region (e.g. Suckale et al., 2009) was a farly dense deployment of 40 stations crossing part of the

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forearc.

- *p.1484,l.6-18: Potential 3D effects should be discussed around here. In general, little reference is given to other seismological modeling attempts related to guided waves in low-velocity subduction channels: Rietbrock and co-workers (e.g. the dissertation by Sebastian Martin, Potsdam, 2005, and related publications) have worked on this in 2D; and Igel et al., PEPI 2002 modeled a simplistic low-velocity layer and analyzed guided waves in 3D.*

On page 1483, we already note that our model is only 2D while the slab in the HSZ exhibits considerable curvature and may have a corrugated shape. We shall mention previous work on guided waves in the introduction of the revised version.

- *p.1485, l.6: what would be a worse guess? I believe such considerations are important when sampling such a vast and uncertain parameter space.*

A worse guess would be a very simple model assuming constant seismic velocities in the slab, the wedge and the oceanic crust. Here, we attempted to produce a velocity model consistent with assumptions on temperature, composition and mineral assemblage that produces for example velocity gradients in the slab and the mantle wedge and temperature dependent quality factors, and a steady velocity increase with depth inside the oceanic crust. Especially the latter feature is highly relevant for the development of guided waves.

- *l.25: "affect major seismic arrivals": Only if the source is inside DSC, which, again, is very difficult to constrain with real data: I would imagine source location algorithms would have to consider the existence of a DSC in their inversion algorithm. Some thoughts on how source mislocation could be affected, or better yet, used in the context of a DSC would be helpful.*

Just to clarify things, the channel signature appears for sources in the oceanic crust **and** in the DSC. In total, the vertical extension of oceanic crust and DSC

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is close to 20 km. Discriminating the source location would require a depth location with an error of less than that. This is ambitious but could be achieved in well-instrumented subduction zones. A seismic experiment targeting the DSC should also take into account the necessity of highly accurate locations of intermediate depth earthquakes. But, as said before, much would be gained if we could observe both signatures from two different earthquakes. Whether a DSC would affect source locations depends on the station locations. For stations in the forearc with wave paths parallel to the DSC, the DSC modifies the wavefield as shown in our manuscript. For stations in the backarc with wave paths at high angle to the DSC, we suppose that the DSC is not really important because of its very limited size. In addition, the DSC mainly affects the guided waves while the first arrival P-phase typically used for location (phase 1 in Fig. 9) is largely controlled by the velocity in the mantle part of the slab.

- *Remarks to figures:*

Most of the criticism referring to the figures is justified. We shall improve label sizes, add colorbars and also reduce the number of figures in the revised version. We shall also check whether plotting differential seismograms helps in highlighting differences between seismogram sections. Thanks also for the technical corrections.

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