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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.

Anonymous Referee #2

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GENERAL COMMENTS: This paper examines the effects of deep subduction channels on the seismic wavefield by means of 2D numerical modeling. I recommend the manuscript for publication in Solid Earth subject to minor corrections. The paper is timely, interesting, well thought-out and written, with a lot of attention to detail both in setting up the mineralogically and dynamically driven background model, as well as analysing the effects in the wavefields and seismograms. The many approximations and inherent lack of knowledge about the constituents of these deep channels and their surrounding structures leave the unfortunate, yet inevitable impression that few if

any constraints on comprehensive detectability within real seismic data can be drawn from such studies: The parameter space spanning the assumptions in the background model, limitations in modeling (e.g. 2D isotropic), lack of knowledge on precise source location, and lack of corresponding seismic experiments is too large to allow for reliable conclusions. However, this is no criticism but rather important to recognise for the wider community, I only suggest that these trade-offs be mentioned more directly and clearer in the discussion. Most of my comments below are of a semantic and clarifying nature, thus only minor corrections are necessary.

SPECIFIC COMMENTS

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.

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.

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?

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.

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?

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.

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.

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).

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.

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

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.

P.1477-1480: While interesting, this is quite lengthy to follow, and the (impressive) eye for detail may somewhat obscur 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.

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.

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.

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).

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.

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.

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.

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.

FIGURES: In general, a lot of the figures lack information/data such as colorbars, legends, time stamps, and some of the axis labeling is too small.

Fig. 6: Time? What do the colors denote? Is this a linear color scaling, any threshold being applied?

Fig.7:Time? More telling may be a differential wavefield (I know this is not the default in SPECFEM2D, but should not be too difficult to compute.

Fig:9: Times in panels? (c) Time?

Figs6-9: I suggest to reduce the number of these figures, certainly join Fig 9 into one. Panels differ in size from figure to figure; some more coherence would be preferred. Wave propagation movies as supplementary material would help as well.

Fig.10/11: These two figures should be merged, possibly even within the same record section... or showing the differential waveforms within the record section.

Fig. 12: Panels are too small, especially labels. The trade-offs within these different realizations are rather large, and beg the question whether much can be distinguished altogether, given the aforementioned uncertainties in the background model. Additional trade-offs that should be mentioned are slab angle, velocity heterogeneity amplitides, frequency dependence. Again, a comparison including different background models would help a little bit.

Fig. 13: I do not see the necessity for this figure. What source is taken?

Fig. 14,15: Larger panels, please.

Fig. 16: I wonder whether fewer panels, or a zoom, filter, threshold etc would possibly

highlight the crucial bits within these complex wavefield which do not differ much in most places.

Fig.17: Larger panels/labels please, at least the width of the page/text.

TECHNICAL CORRECTIONS

p.1463, l.1: -> "decades after the establishment of plate tectonics".. would be clearer.

l.8: P/T "P/T" -> "Pressure/Temperature (P/T)"

l.12: -> "some cooling associated with decompression"

l.15: -> "time windows"

p. 1464,l.1: Explain "UHP"

l. 26: -> "provenance" may be more common? (also p.1465,l.2)

p.1465,l.7: -> "block-in-matrix" (also l.12)

p.1466,l.20: -> mineralogical

p.1469,l.3: grammar/wording in this sentence is difficult to follow.

p.1470,l.4: ->"calculated" ("L" missing)

p.1471,l.16: Fig 2 [FULL STOP missing].

p.1484,l.19: -> "The limiting factor", or "A limiting factor"

Interactive comment on Solid Earth Discuss., 5, 1461, 2013.