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Interactive comment on "Mantle flow below the central and greater Alpine region: insights from SKS anisotropy analysis at AlpArray and permanent stations" *by* Laura Petrescu et al.

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General comments

Dear Referee 1,
in the following you will find our answers and/or description of changes we applied to our manuscript following your useful suggestions.
We start with a reply to the General Comments: you have well understood that this is the first of hopefully further steps of study. Within 2-3 days we upload the revised version of the manuscript so you can follow our changes.

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Discussion paper

Specific comments

REF1: Compare SKS splitting results to recent flow models for this region, similar to what Venereau et al. (2019, G3) did in Alaska. I think the authors should add a figure comparing their results to a flow model and would be a nice visual to add to the text on possible flow scenarios. I did a quick literature search and found a few flow models.
ANSW: We accept the suggestion and add a new Figure 8 with a final sketch and some references in the Discussion paragraphs. It is worth to note that most of the flow scenarios already proposed are based over a smaller amount of measurements with respect to ours.

REF1: Seems some of the stations have complex anisotropy such as the stations in Figure 6b and should be further analyzed in a future study. I recommend the authors use a special symbol (perhaps on Figure 6a?) for any station that may exhibit complexity since the average fast direction may not be a good representation of the dominant fast direction at those stations and should require future study.
ANSW: We agree that some stations have a more heterogeneous pattern of measurements. We underline it showing three examples in Figure 4d (stations A037A, A061A and A300A). We tried to differentiate the amount of heterogeneity in the anisotropy,Âă computing average values with two methods and following the approach according to which a large heterogeneity is found when the difference between the average values is large. Unfortunately we did not find any particular results as you can see from the maps here attached (Fig.1-RV1: Map of average SKS directions computed using the circular average (yellow) and the one obtained by stacking misfit surfaces from individual SKS splitting solution (red); Fig.2-RV1: Maps of phi error given by misfit stacking (left) and R value obtained by circular mean computation, that varies from 0 to 1 corresponding to a minimum to maximum dispersion). So, we decided to postpone any further discussion about this point, that we will approach differently.

REF1: A general comment (not really suggesting any changes): In general, I find averaging fast directions to not be a the most accurate determinant of the dominant fast

direction. I think fitting the data to a 1-D upper mantle anisotropic model or making a correction to the observations are better options, since averaging can be affected by a sawtooth pattern (see Eakin et al., 2019, G3) and sample bias in event backazimuth. They can sometimes differ by 15 degrees based on my personal experience. However, I do not suggest the authors to make any changes in this aspect, just a thought. Taking an average at each station is pretty common, but I wonder if future shear wave splitting studies should consider some better techniques in finding the dominant fast direction.
ANSW: We agree with the reviewer. We are indeed aware that anisotropy average values may mask a more complex (and interesting) distribution. However, the aim of this manuscript was a general view of the anisotropy distribution given by a huge amount of data and a very large studied region. So, a large scale image of the situation. By the way, as already stated in our previous answer, we here applied two different methods to compute average direction values, with the purpose to recognise and motivate differences (see Fig. 4 panel d). For the broad view, we decided to discuss the map of average values, but in future studies we will certainly focus on back azimuthal distributions that here is briefly represented in Figure 4c.

Technical Comments:
Line 138 – what are the azimuthal bins for the misfit stacking?
ANSW: We use misfit stacking to obtain a single solution per station. Thus, we do not stack misfit surfaces in azimuthal bins. Instead, we stack results from all events recorded at a station. The final solution is equivalent to a joint linearization of all SKS phases recorded at the station. This stated at lines 135-140, in the Method and Data paragraph.

Line 145 – The clockwise implies the authors know the direction of rotation. The authors should maybe use the word “circular”, since we don’t know if flow is clockwise or counterclockwise.
ANSW: Changed as suggested

Line 250 – This is also just a general comment. I don't recommend any actual changes, just potential for a future study since it would be beyond the scope of this study. I am curious how well the surface wave anisotropy compares to the shear wave splitting when you model for 2 layers of anisotropy. The authors state there is little backazumthal variation in Switzerland, but it's difficult to rule out since it's difficult to see the details in Figure 4. Why don't the authors try this two-layer model on a couple of stations with a fast direction vs. back azimuth plot. This could be an interesting future study.
ANSW: Yes, we are going in this direction, mainly with permanent stations (longer dataset available). Our first attempt on the TUE MedNet station, which has been operational for more than a decade (located at the boundary between Swiss and Italy) did not show significant back azimuthal variation to justify a two-layered structure.

Line 256 – This should be an equation, not within the text. For example, it should be on a separate line and labeled, equation (1).
ANSW: Changed as suggested

REF1: What is the period of choice? Since the authors use a bandpass. It might be good to show the width of the Fresnel zone for 3-25 seconds in Figure 6b, since this is the bandpass window the authors use.
ANSW: Traditionally the typical period of SKS phases is around 10 s and this is the reason why we computed Fresnel zones with this value (as we already have done in previous papers, see Salimbeni et al., 2015, JGR). However, we looked at Fresnel zones for the corner frequencies of our filter, but we did not find them relevant enough to be discussed in the paper. This has been clarified at lines 255-260.

REF1: I am also confused by this paragraph. The authors assume the anisotropy is due to something deeper than 200km, but multiple layers or a dipping layer of anisotropy in the upper mantle could induce changes in fast direction. I do agree that station A037A could not be due to two layers, but it is possible for dipping layers. I think

these two stations are interesting and the author should plot all of the data for these two stations. Is there any backazimuthal pattern at these two stations? I think it's fine to keep this Figure and this analysis in the paper, but I think the authors should say this requires further study and that there could be other causes that are not related to a deeper source.

ANSW: For station A037 all data are already plotted in Fig. 4 panel d, but for completeness we included in Figure 6b the polar plots for both stations for which Fresnel zones have been reported. Certainly the pattern is not simple, both are temporary stations and the back azimuthal coverage is insufficient to reliably provide an interpretation for multiple or dipping layered structures. However here shallower anisotropy measurements (Pn or surface wave azimuthal anisotropy, Figure 6a) show different results with respect to our prevailing anisotropy directions, consequently we attribute SKS anisotropy to a deeper possibly asthenospheric mantle origin. We are currently working on a second paper exploring more complex structures using data from permanent stations.

REF1: You could investigate the depth possibility by looking at SKKS or other XKS phases that have different inclination angles than SKS. I just think the authors should not jump to the immediate conclusion that the anisotropy is related to the deeper mantle without further analysis.

ANSW: SKS-SKKS differences are mainly attributed to lowermost mantle structures, such as D" layer topography, which is at 2800 km depth. In our paper, we only suggested that anisotropy should be deeper than 200 km. Nevertheless, a comparison with SKKS phases and inferences on the core-mantle boundary anisotropy would be a really interesting pursuit as well.

Figure Comments:

Figure 3 caption – a.iii and b.iii. (top) The description for this does not make sense. I don't understand what these three small figures are, and I think the authors should

clarify in the caption. There is a description for these panels, but I am not sure which of the three the authors are talking about.

ANSW: We modified the caption and figure to make it more explicit. The new caption now reads: “Time windows of the radial (black line) and tangential (dashed line) components showing the SKS phase before and after the correction.”

Figure 4 – Should add plate motion arrows to plot.
ANSW: Added as suggested in Figure 7.

It’s really hard to see the SE purple fast directions – recommend a more contrasting color – maybe green?
ANSW: Changed as suggested

Figure 7 – a and b are not labeled on the figure.
ANSW: Changed the caption

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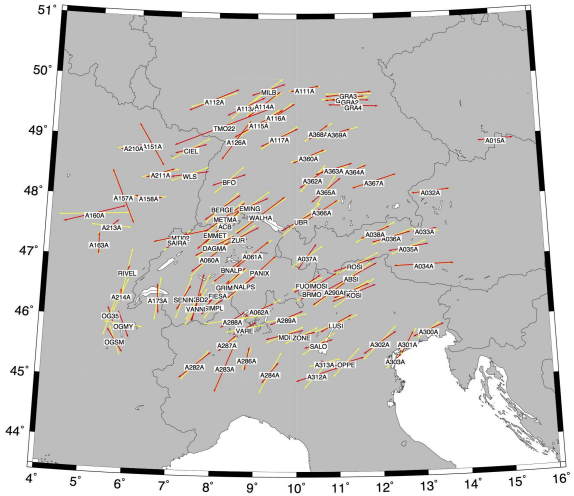

Fig. 1. Figure 1-RV1

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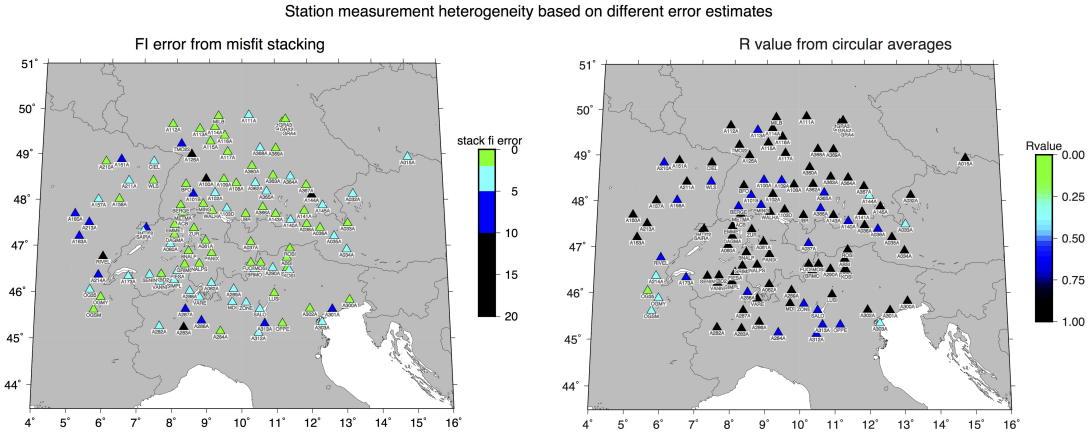

Fig. 2. Figure 2-RV1

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