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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.

Anonymous Referee #1

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General Comments:

I find this paper really interesting and explores a really interesting geologic region with some question on the sources of anisotropy and flow related to the Alpine subduction. The authors measure SKS shear wave splitting for the AlpArray and conduct many different analyses and test different mantle flow hypotheses. The paper is written very well, and I enjoyed reading it. I think the data is very fascinating and has improved the understanding of the Alps. I like how the authors use the stacking misfit approach. I think this is a good tool to use in these studies. I also like that they compare their results to surface wave anisotropy and seismic tomography. I just think they could go

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Discussion paper

a little further in a future study. I do have a few minor suggestions that I think would make the paper a little clearer to the reader.

Specific Comments:

1. Compare SKS splitting results to recent flow models for this region, similar to what Venereau et al. (2019, Gˆ3) 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.

2. 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.

3. 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, Gˆ3) 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.

Technical Comments:

Line 138 – what are the azimuthal bins for the misfit stacking?

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.

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 backazumthial 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. backazimuth plot. This could be an interesting future study.

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

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.

Figure 4 – Should add plate motion arrows to plot. It's really hard to see the SE purple fast directions – recommend a more contrasting color – maybe green?

Figure 7 – a and b are not labeled on the figure.

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