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. In black are reported your questions or comments, in red our answers.
We start with a reply to the General Comments: you have well understood that this is the first of hopefully further steps of study.

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 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.
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 previous flow scenarios already proposed are based over a smaller amount of measurements with respect to ours.

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.
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, 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 maximum to minimum dispersion). So, we decided to postpone any further discussion about this point, that we will approach differently.

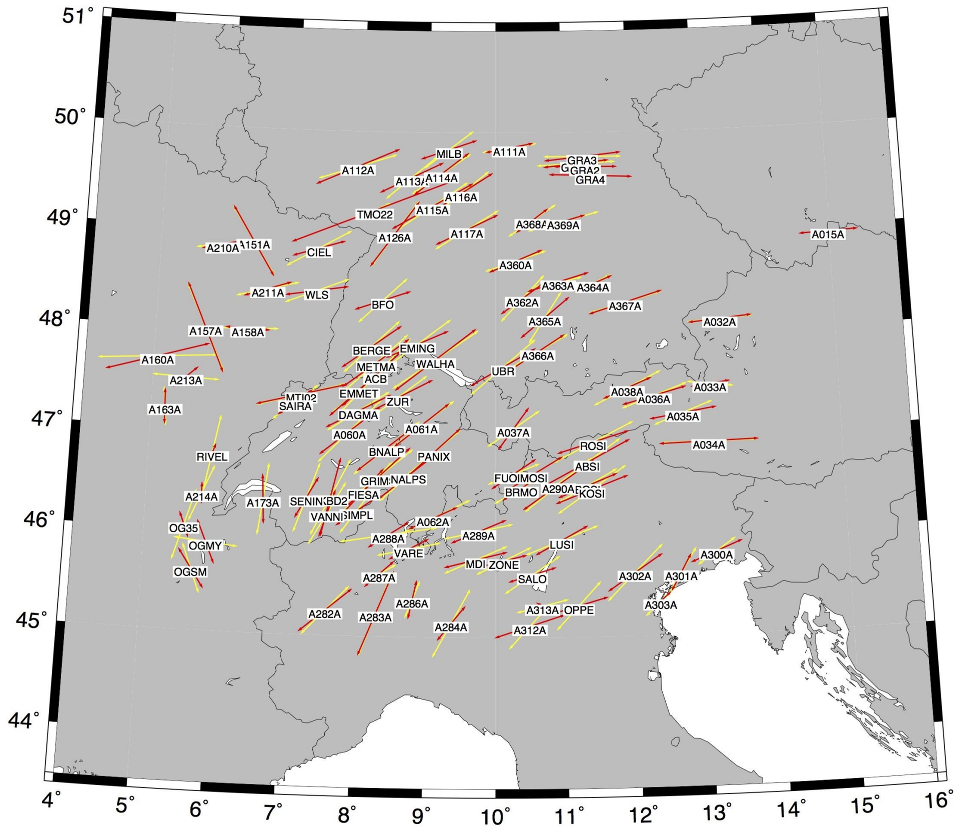

Figure 1-RV-1 -- Map of average SKS directions computed using the circular average (yellow) and the one obtained by stacking misfit surfaces from individual SKS splitting solution (red).

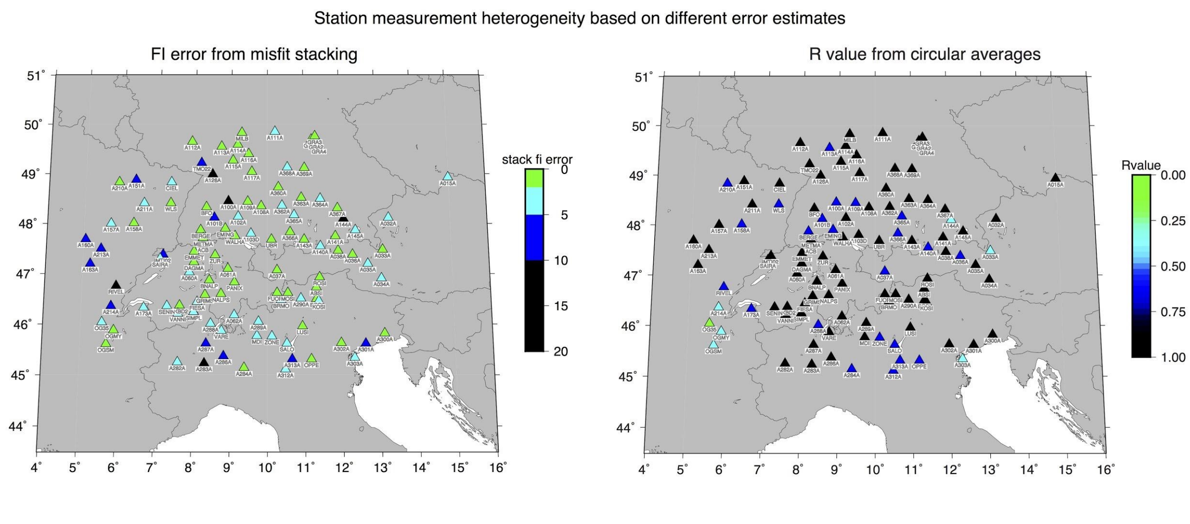

Figure 2-RV1 -- Maps of phi error given by misfit stacking (left) and R value obtained by circular mean computation (R varies from 0 to 1 corresponding to a maximum to minimum dispersion).

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.

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?
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 125-130**, 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.
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.
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).
Changed as suggested

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.
Traditionally the typical period of SKS phases is around 10 s and we are interested in the kind of heterogeneities that can be sampled by this type of wave. For this reason we computed Fresnel zones at 10 s (as we already have done in previous papers, see Salimbeni et al., 2013, 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 265-270**.

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.
For station A037 all data are already plotted in Fig. 4 panel d, but for completeness ***we also 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.

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.
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.
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.
Added as suggested, in **Figure 7**.

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

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

Dear Referee #2,
in the following you will find our answers and/or description of changes we applied to our manuscript following your useful suggestions. In black are reported your questions or comments, in red our answers.

The manuscript investigates the mantle anisotropy beneath the Alpine region with updated data set from the AlpArray stations. The new results provide comprehensive knowledge on the mantle flow in the study Region due to the interaction between the European and Adria plates. It is well organized, I have only minor comments.

Vertical resolution is the problem of SKS splitting. The authors calculated the Fresnel zone and concluded strong anisotropy in the asthenosphere. It is useful. Another method is to make quantitative compression between delay times and lithospheric thicknesses. If lithospheric anisotropy dominates, a strong positive correlation should be clear. Otherwise, asthenospheric anisotropy is required. In this case the delay times should be as accurate as possible, so the stacked averages are better because individual measurement usually overestimates the delay times.
In the **Discussion** section "Depth and source of SKS anisotropy in the Alpine region" to clarify we added the following paragraph (lines 280-290): "***A quick comparison between LAB depth variations in the Alpine region and delay times does not yield a clear correlation, which should also support a non-lithospheric source signal. Most S-receiver function based LAB estimates generally indicate LAB depths in the 90-140 km range beneath the Alps (Geissler et al., 2010; Miller and Agostinetti, 2012; Bianchi et al., 2014) decreasing to 80-100 km north of the European Front (Geissler et al., 2010). In contrast, Plomerova et al. (2010) infers lithospheric thicknesses of up to 230 km beneath the Alps and ~100 km north of the Front. LAB estimates thus vary between studies and the actual boundary remains elusive especially in on-slab locations, where slabs plunge almost vertically, but a large-scale decrease towards the north is a common feature. However, delay time variability (Figure 4a) does not mirror this large-scale decrease, providing further support for an asthenospheric source.***"

If the circular patterns of fast polarizations are result of subduction-driven mantle flow, since the subducting slab is steep here, how to explain similar fast orientations in the high (slab) and low velocity regions at 100 and 200 km depths.
In future developments of this work we'll certainly try to distinguish the more feasible location of the anisotropy we detect and also if a contribution from the slab can be ascertained. At present, tomographic images do not report a unique and coherent shape of the slab. Moreover, for the particular condition of the coexistence of Apennines, Alps and Dynaric slabs, mantle flow directions here may be similar above and below (in front and behind?) the Alpine slab and consequently difficult to be distinguished.
Alternatively, the dipping slab(s) and entrained asthenopsheric mantle could explain part of the circular fast azimuths, if the lithospheric mantle is characterized by a dip-parallel foliation containing the a- and c-axes of olivine. This situation has been envisaged by Song and Kawakatsu, 2012, GRL, to explain the trench-parallel anisotropy along subduction zone forearcs. This is now cited in **section 5.4, lines 360-367.**

Line 124: Grid search method is used here. So it is necessary to clarify the steps for fast polarizations and delay time. A short description to the uncertainty estimation is also necessary.

We now modified the text **at lines 125-130** to include this information: "***Grid search parameters are 0.025 for δt and 1º for φ, and error calculations are based on the Silver and Chan (1991), under the assumption of Gaussian noise***."

Line 135: Describe or show the reference for standard circular means.

Yes, we used the standard circular mean described in Davis J. C. (2002), Statistics and data analysis in geology - 3rd edition, Wiley ed. We add the reference.

Line 194: 1.0 - 2.0 s

Changed as suggested

Line 254: I think Figure 6 is missing. So I cannot check it.

Unfortunately, this is true. In our first submission in fact we missed figure 6. We then uploaded a correct manuscript, but we do not know why you probably received our first version. Hope you can check now in this revised version. We apologize for this.

Figure 2: Label the epicentral distances in the inset. The study region does not seem to be in the center.

Inset figure modified as suggested.

Figure 4d: Label the stations in the map above.

Labels added.

Figure 5: In the map view, there are many NE-SW fast orientations around (6E, 46N), but they are invisible in profile D. Why?

Coloured squares are station measurements, not individual event SKS measurements. However, in profile D, at ~200 km distance, roughly corresponding to the 6E,46N location, coloured squares indicate a large scatter in the fi direction, as expected.

Figure 7: Here are comparison of seismic tomography at 100 and 200 km depths with seismic anisotropy at stations. Maybe you can try to project the SKS splitting to 100&200 km depths and calculate the regional averages respectively.

100 & 200 km piercing point maps are really similar to the 150 km we decided to use. The position of a measurement at the piercing point at 100 km or 150 km differs by about 12 km. This means that different maps at different depths of the piercing points would show substantially the same pattern at the scale we are working in this paper.

Concerning the computation of the regional average directions computed from the measurements at piercing points, we chose to provide a sketch of the possible mantle flow patterns (see **Figure 8**) which we considered significant and helpful based on the average values at stations. However, for other aims, we are also working on what you are proposing, see the abstract EGU2020-13880, "Surface and deep deformation of the great Alpine region from GNSS and seismic anisotropy measurements" by Simone Salimbeni et al.

Dear Referee #3,
in the following you will find our answers and/or description of changes we applied to our manuscript following your useful suggestions. In black are reported your questions or comments, in red our answers.

This paper is well organized and well written. I recommend publication with only minor revisions.

1) The introduction section needs more information about anisotropy. There should be some mention of anisotropic fabrics especially when there is later discussion of lithospheric (A-type) versus asthenospheric (C- and E-type) anisotropy. a. I don't know how applicable this is in the Alps, but it seems like some of the invoked mantle flow is where the wedge would traditionally be located. So B-type fabric may also be worth mentioning.
In this manuscript we started with the idea to give a general view of the study region, where uncertainties on the slab shape, dimensions and positions are still debated. We would work on possible X-type of anisotropy using further results after we clarify some doubts about the slab geometries. In the Introduction we mainly describe the geodynamic information we use in the discussion.
We mention the common assumption of A-type olivine fabrics to interpret the observations and briefly discuss the role of other olivine fabrics **in 5.4 paragraph**.

2) Did the authors examine different frequency bands for splitting?
No, we did not.

0.3 Hz seems on the high end. a. How does this frequency band compare to the previous studies of splitting in the region?
It is not uncommon to use this corner frequency. Other SKS studies use a similar filter bandwidth of 0.04-0.3 Hz (e.g. Darbyshire et al., 2015, Lidell et al., 2017, Venereau et al., 2019) or an even higher corner frequency: 0.09–0.35 Hz (Eakin et al., 2015). In the Alpine region, some authors did not apply any filtering (Bokelman et al., 2013, Qorbani et al., 2015), while others used 0.02-1 Hz (Wustefeld et al., 2007), 0.02-0.2 Hz (Barruol et al., 2011; Salimbeni et al., 2018) or a range of bandpass filters between 2–10s, 5–20s and 10–50 s (Walther et al., 2013). To clarify, we added more information about our choice at **lines 140-145**.

b. Might some of the variability in splitting be coming from the Alpine crust since high frequencies are being included?
The expected crustal contribution to the SKS signal is incomparably smaller than our estimated dt values. In the manuscript we mentioned that estimated delay times associated with the crust range between 0.1-0.3 s (Silver, 1996) or 0.1-0.5 s (Barruol and Mainprice, 1993) while our average delay time is 1.4 s, requiring a mantle contribution.

3) In the Methods or Results sections, please include error information such as maximum allowable and average errors in phi and dt.
We added the following information in the **Method section at lines 126-130**: "***We allow measurements where the SKS phase is clear, the particle motion is successfully***

linearized, and the misfit surface minimises around one δt-φ solution pair. The majority of allowed individual measurements have φ errors less than 20° (81% of our results, while 19% are between 20° and 40°) and δt errors less than 1 s. After misfit stacking, errors decrease significantly, with single station measurement errors averaging to 2.0o for φ and 0.025 s for δt"

4) Did you use Splitlab? SplitRacer? Sheba? Or your own splitting program?
We now modified the text to include this information. In the second paragraph **at lines 124-125**, the text now reads: *"To determine the φ and dt parameters [...] by minimizing the energy on the tangential component,* ***as implemented in the Sheba software (Wüstefeld et al, 2010).***"

5) How well do the null orientations agree with the splitting directions in each region?
In the **Results section, lines 175-179**, we added : ***"These directions are similar to the estimated anisotropy fast axes throughout the region (Figure 4a). This is expected since SKS waves polarized in the direction of anisotropy should not exhibit splitting."***

a. The nulls in the NW alps obviously don't have a lot of splits at those stations, but is that telling of downwelling?
In our opinion, the lower amount of null measurements in NW Alps (Swiss to be more precise) may be due to the fact that cross checking prevailing back azimuth directions (plot bottom right in Figure 4b) and prevailing directions of splitting anisotropy here, it is quite low the probability to have null measurements.
In addition, previous geophysical studies for this region did not refer of any downwelling. A low amount of nulls is a weak support for this hypothesis, mainly if it is highly possible the cause previously described.

6) Did the authors try modeling splitting with layered anisotropy in the NW Alps where single station splitting variability is high?
We are going in this direction, mainly with permanent stations (longer dataset available). Our first attempt on TUE MedNet station (located at the boundary between Swiss and Italy) was not very satisfactory. The back azimuthal variation probably is not due to a two layers structure, but we have still a lot of data to analyse with this purpose.

7) In figure 4c, I think it would help to show the null pierce points as well a. That might be what the red dots are, in which case that should be in the caption.
Red dots are the stations (information lacking in the caption and now added). We tried to do what you suggest, but having so many measurements, plotting also nulls makes a map hardly readable. For this reason we decided to show them separately.