Comments on the revised version of ms. se-2020-179 “Moho topography beneath the Eastern European Alps by global phase seismic interferometry” by I. Bianchi et al.

The manuscript still needs minor corrections before it can be accepted for publication. Although some effort has been made by the authors to reduce the length of the introduction in the revised version, there is still a need to improve its organization and reduce its length by focusing on the actual objectives of the study. I also suggest to update the discussion/conclusion by commenting on the results of a recent study that was published recently, and that uses the data of the densest broadband seismic array operated in the Alpine region, the Swath-D experiment (Sadeghi-Bagherabadi et al., 2021).

More detailed comments are listed below.

1) Abstract, l. 7: you write that your method “well images the topography of the Moho in regions where it shows a nearly planar behaviour… from the Bohemian massif to beneath the Northern Calcareous Alps”. Is this really because the Moho topography is planar, or because it is reflective (corresponds to a strong velocity contrast)?
2) Abstract, l. 9: what is a “typical” crust-mantle boundary?
3) Abstract, l. 10: “absence of an Adriatic crust made of laterally continuous layers smoothly descending southwards”. So, what is present? Is it a “structurally complex and faulted internal crustal structure” as suggested in the next sentence (but for the Alpine crust)?
4) Abstract, l. 11: why do you conclude on a “structurally complex and faulted internal Alpine crustal structure”. This contradicts the earlier sentence when you write that the Moho of the Northern Calcareous Alps is clear. They are not part of the Alps?
5) Introduction, l. 16-18: useless details in sentence “After the closure of major and minor oceans, … continental parts of the much smaller plate Adria collided”. Could be summarized to “After the closure of the Alpine Tethys, the European continental margin collided with the small Adria plate”.
6) Introduction: in fact, the previous comment is one example of an unnecessarily long and detailed sentence, and there are quite a few like that in the introduction. I am not sure that it is useful to cite all the experiments that have produced geophysical images in that region. You should focus the introduction on the key question that you address in that paper, which is the apparent Moho gap of Spada et al. (2013).
7) Introduction, l. 21-22: is there really “a general agreement that the European and the Adriatic Moho are offset across the plate boundary in the Alps”? Which publications state that?
8) Introduction, l. 23-44: I would suggest to avoid listing here all CSS experiments in the Alps and to keep only those related to the Eastern Alps.
9) Introduction, l. 45-47: you write that most information about the Moho is derived from CSS experiments but you refer to publications such as Diehl et al. (2009) that only deals with earthquake sources. There is no contradiction, but this reference is inaccurate in this context.
10) Introduction, l. 48: you should explain what the “Moho triple junction” of Brückl et al. is, because it is probably one of the questions that you want to address.
11) Figure 1: you use similar thin plain lines (of different colors) to show very different features such as tectonic structures, the triple junction of Brückl et al. that refers to

the Moho structure and to outline the area of Moho gap by Spada et al. This makes the figure confusing. I would suggest using different types of lines, following geological standards for the Alpine front for example and a filled polygon for the Moho gap area.

12) Introduction, l. 62-64: the sentence on recent ambient-noise tomography studies brings no useful information. I guess you mean that these ANT studies are more valuable for imaging velocity heterogeneities than imaging Moho depth variations. This is right, but it should be better explained. Moreover, some of the works you cite don't even reach Moho depth while others do and provide clues on the topography of velocity contours used as proxies for the Moho. This is worth mentioning.
13) Introduction, l. 69: rephrase unclear sentence ".. and stacking primarily global phases; waves that travel across the core…".
14) Introduction, l. 74: what do you mean by "considerably greater than zero"?
15) Introduction, l. 75: correct "Alpine reflectively". Do you mean reflectivity of structures of the Alpine crust?
16) Introduction, l. 75-76: sentence "In other…2019)" is out of context.
17) Section 2.1, l. 88-89: did you discard entire event recordings or did you only discard time windows with multiple phases? Please rephrase.
18) Section 2.2, l. 97: please rephrase "selecting minus the causal result and muting the delta pulse".
19) Section 2.2, l. 106: I guess "rupture effects" means "earthquake source effects".
20) Section 2.2, l. 112: by "reflectivity from the lithosphere at the source", you probably mean "spurious signals from the lithospheric structure at the source side".
21) Section 2.2, l. 114: step without "s"
22) Results, l. 161: replace "especially receiver-side reflectivity is shown on these images" by "these images mostly show receiver-side reflectivity"
23) Results, l. 171: you write that you decide "to focus (your) interpretation on the Moho topography in the northern part of the profile". This is surprising at this step of the paper because the most interesting objective is the "Moho gap" in the southern end. Do you mean that you quickly give up on bringing in new constraints on the most interesting southern part, and that you will not discuss this part further?
24) Results, l. 182: by "suggest the signals representing at least in parts internal crustal structure", do you mean that the amplitude difference between signals at crustal depth in the northern and southern parts suggests that at least part of the signals in the south side can be attributed to actual crustal structure?
25) Results, l. 182-185: The sentence "Unfortunately, the 3D crustal structure of the Eastern Alps below 15 km depth is still poorly known … with reference to the tectonic style and geologic evolution of the orogeny (e.g. Willingshofer et al., 2013; Rosenberg and Kissling, 2013, and references therein)" is too long and unclear, and it is partly wrong. I would consider that the crustal structure of the Eastern Alps, with TRANSALP and EASI, has been studied by as many tomography studies as the Western Alps with the CIFALPS profiles and ECORS-CROP. The crustal structure of the Central Alps is more poorly know since it has only been studied by the NFP-20 deep-seismic sounding profiles, and no dense passive seismic experiment. The reference that you give (Kissling et al., 2006) presents a synthesis of what was known at the time of writing, that is before a number of recent experiments in the Western and Eastern Alps, including EASI. You should update your reference list. I don't know

Behm et al. (2006) which is an unpublished PhD thesis whose citation is useless. You also cite Lu et al. (2020) that covers the entire Alps, and not only the Western and Central Alps, and provides the Vs structure at depth >15 km in contradiction with your sentence. Qorbani et al. (2020) does cover only the Eastern Alps to ~40 km depth, also in contradiction with your sentence. Molinari et al. (2020) and Sadeghi-Bagherabadi et al. (2021) also focus on the crustal structure of the Eastern Alps. That's a lot of publications on the crustal structure of the E-Alps in the end! The problem of the lack of clear images of the structure of the lower crust and Moho beneath the Tauern window is obviously not due to the lack of data. I don't understand what you mean by "and with reference to the tectonic style and geologic evolution of the orogeny". Please clarify.

26) Results, l. 185-186: In the next sentence, you write that you expect a complex crustal structure and you cite a review paper (Handy et al., 2015) that deals with palinspatic reconstructions and slab geometry. Again, a tomography paper that shows that imaging the lower crust is particularly difficult beneath the Tauern window, like Hetenyi et al. (2018) is more adequate. You should maybe erase these 2 sentences and leave only the one of l. 187-189, which is much more correct and accurate.
27) Discussion, l. 207-210: When you write "the strength of (your) new results lies in the continuous assessment of the lateral variation of the Moho interface… in the northern part of the profile", you seem to forget the RF results of Hetenyi et al. (2018) who were the first to provide a continuous image of the depth variations of the Moho beneath the same profile. This is surprising as the first author of the present paper is a co-author of Hetenyi et al. (2018). You should start the discussion by comparing with their results. This sentence is also contradictory with the one of l. 224 "we conclude the Moho is well imaged univocally by all methods in this northernmost section". If all methods work well in that part of the profile, imaging the same Moho as others cannot be the strength of your new results.
28) Discussion, l. 210: You cannot tell that the Moho model of Spada et al. is more accurate than the one by Brückl et al. only because the first one better fits your Moho depth estimate. The three Moho depth models depend on the velocity models used to convert time to depth. You use the Vp model by Brückl et al. shown in Fig. S10. I would therefore expect your Moho depth to better fit the one of Brückl et al., which is apparently not the case. You should rather comment on that than on the accuracy of the 2 other models.
29) Discussion, l. 212-213: precise that Hrubcová et al. (2005) deals with the Bohemian massif.
30) Discussion, l. 215, 218: "latest at 300 km"? "anyways"? replace "one strong impedance" by "a strong impedance".
31) Discussion, l. 229-230: your GloPSI analysis fails to image the strongly dipping Moho resulting from the RF analysis at 400-550 km distance. You provide a number of possible explanations for that difference including the difficulty to image dipping boundaries with GloPSI or an anisotropic mid-lower crust. Why don't you firstly discuss the quality of the RF signals at these locations in Hetenyi et al. and also their migration model that you mention later in l. 253-254? As you are first author or co-author of the RF papers, you are the best expert to compare these results in more details.

32) Discussion, l. 255-256: comparison with the Western and Central Alps is useless as the geological context if different. You should erase the sentence “In accordance.. Alps” which does not provide any interesting information.
33) Discussion, l. 257: “a number of studies have proposed models of the deep structure beneath the Alps”. You rather mean “beneath the Tauern window” or “beneath the high Eastern Alps east of 13°E” (because TRANSALP is in the E-Alps, and it can image the Moho).
34) Discussion, l. 262: correct “charachteristics”.
35) Discussion, l. 263-265: do you really believe that the solution is in a better 3-D model from local earthquake tomography to improve the migration of RF, as suggested in your sentence “Obviously…across the plate boundary”? I don’t. You cite the ANT study by Sadeghi-Bagherabadi et al. (2021) that uses data of the very dense Swath-D array. This paper shows a depth section along the EASI line where the Moho depth is computed from the Vs contours 4.1-4.3 km/s and compared to the RF Moho of Hetenyi et al. (2018). If these contours are a good proxy of the Moho, it is almost flat and continuous at 50 km depth in the Moho gap region where Hetenyi et al. propose 2 strongly dipping Moho surfaces. Although Sadeghi-Bagherabadi et al. has been published very recently, I would suggest that you mention this surprisingly simple result, in particular because it was computed using the densest 2-D array ever installed in the Alpine region. And because Swath-D exists, I don’t think you can conclude that there is a need for increasing the station density in that region (last sentence).
36) Conclusion, l. 276: “..due to the southern dip of the European plate”. Don’t you rather mean “the southward dip of the European Moho”?