Reply to Reviewer #2

This contribution describes the effect of quieting following the COVID19 lockdown measures on the noise level in a regional seismic network located around the Etna, Sicily, Italy. The subject is of interest, in particular in the framework of the "Social seismology" SE Special issue. The paper is well-written, the structure is in general well shaped and the figures are of good quality (although many labels should be enlarged). Therefore, I think that the manuscript deserves to be published in SE after minor to moderate revision. However, there are a number of point that, in my opinion, should be reworked in the final version of the manuscript.

> We thank the reviewer for the positive comments.

The most valuable contribution of the manuscript is documenting that a seismic noise reduction can be observed in areas far from large cities, where human activity still affect the background seismic noise via ship transit, touristic excursions etc.. Even some stations which seems to be installed at remote places reflect the decrease of activity following lockdown. I think that this point has to be highlighted through the manuscript and in particular in the abstract and conclusions.

> Thanks for your advice, we added a couple of sentences about that in abstract, Results and discussion and conclusions sections.

In general, the manuscript makes a good job in presenting the data, but tends to be too concise in the interpretation part. Sections 2.2 and 2.3 are merely descriptive of the results presented in the corresponding figures. The reader has to wait till section 3 to learn something on the information included in the figures. I propose to include here the discussion on the differences observed between sites, the tentative origin of noise at each site etc. included now at Section 3.

> I see your point; however, we cannot write results in the "Materials and methods" section. Indeed, by putting only data and analysis descriptions in the "Materials and methods" section, the readers, not interested in the technical details, can skip such a part of the manuscript and still get all the information about the main paper results.

Figure 6 and 7 provide essentially the same information that Figs 2 and 3, presented in a different way. I will appreciate a comment on which are the advantages of each kind of representation. Are there features only observed these representations and not in the RMS or spectra?? If yes, it will be interesting to comment. Otherwise, the figures can be seen as redundant.

> Actually, figures 6 and 7 are necessary to explore the spectral content of the anthropogenic seismic noise and contain information not present (or at least not that evident) in both spectrograms and RMS amplitude time series. Hence, in our opinion, such figures cannot be considered redundant. Among the figures you cited,

probably the one we commented less was Figure 2, showing the spectrograms. Hence, we added some comments about these at the beginning of the section 3.

Regarding the comparison with mobility data, I think that the message that seismic data is consistent with other data is best passed using a graphic as that presented at Suppl. Fig A2 than using just correlation coefficients. I suggest to start the section using a new figure composed by the a) subplot of Suppl fig. A2 and the submitted Fig. 10. The submitted Fig 9 will move to Fig 10. In this way the reader will first see an example of correlation between RMS and mobility data for specific station, then see the overall correlation and finally see the differences between stations and mobility data.
> Done, thanks for your advice.

The discussion on Spearman correlation and t-test and p-values is unclear. I think that the original Figure 9 has to be used shown the station with good or poor correlation with mobility data, in some graphic, easy to interpretate way.
> We changed a bit the discussion of these data. We hope that it is clearer now.

In the discussion (line 220) it is stated that only the ESAL/Facebook correlation does not match the criteria, but, in my opinion, some of the stations (EFIU, HSRS, ESML, HPAC) clearly show a good correlation between seismic RMS and quietening, while for the rest, the correlation is less clear. This point should be clearly stated, noting that the relationship seismic noise/mobility is not always clear. In the Conclusions section it is correctly stated that the effect is strongly station-dependent. I think that this dependency should be better described here. In any case I would enhance the fact that, even for stations with poor correlation there are evidences of changes in the seismic noise values.
> As above mentioned, we have modified the discussion on these data. In particular, we have clearly written that correlation between seismic noise and human mobility is strongly station-dependent, some stations show very good correlations, others less. In any case, the p-value is lower than 0.05 in all the comparisons, suggesting how the obtained Spearman correlation coefficients are significantly different from zero. In addition, we performed again the analysis by using seismic RMS amplitude in the band 10-40 Hz (in place of 10-30 Hz). By doing so, the correlation ESAL/Facebook now also matches the criterion p<0.05.

I don't understand Suppl Fig A3; P-values are in the order of 0.005 for Google, 0.0002 for Apple and 0.02 for Facebook. Are those order of magnitude differences realistic?? The authors state the p-value of 0.05 is considered sufficient to reject the null hypothesis; while this particular number is chosen?
> The p-value threshold of 0.05 means that the probability, that the result of the statistical test is due to chance alone, is less than 5%, so it would occur once out of 20 times the study is repeated. The value of 0.05

is a commonly accepted significance level used for this statistic test. We added a couple of sentences about that in the section 2.3. As for the obtained differences in p-values, yes, they are realistic.

Noise level variations related to ship activity or touristic excursions is interesting and not often described. I suggest to give more weight to this funny observation.

> Agreed. Indeed, ship traffic data of Lipari port has been obtained and compared with seismic data acquired by ILLI station (installed in Lipari Island at about 2.7 km from the port). Hence, a new figure and several sentences have been added regarding this topic in sections 2.3 and 3. Furthermore, a couple of sentences about the fact that the seismic noise amplitude reduced even in stations installed in remote places have been added in abstract, Results and discussion and conclusions sections.

The section on the improvement on detection capability has a large potential interest, but it is not really developed here. In the main text, the authors just describe Figure 11 and the final discussion includes just a sentence on this subject. If the authors decide to keep the section, a significant improvement will be needed. Figure 11 shows that the number of pickings increase during lockdown, but the relevant information will be if more small magnitude events are detected or if the hypocentral determination is improved during lockdown. This analysis should be taken carefully, taking into account the epicentral distance of the events detected in each period, the occurrence of swarms/aftershocks that could perturb the comparison etc. Otherwise, a better option will be to keep the detection improvement discussion for a next paper focused on this subject.

> We totally agree with you. This topic would need a more in-depth analysis. Following your advice, we decided to delete this section regarding the earthquake detection improvement and keep it for a next narrower, more focused study.

Other points.

L. 31: Not sure that references to papers dealing with pharmacology are needed here

> The aim of lockdown measures, which influenced so unexpectedly the seismic signals, was just to slow down the COVID-19 epidemic to give more time to the pharmacological research to find a cure and/or a vaccine against COVID-19. This is the reasoning behind the references to papers dealing with pharmacology. So, in our opinion, they are needed here.

L. 65: The actual location setting of each location is hard to see in the small size screenshots in Supp Fig A1. I propose to summarize in this paragraph the different setting of the stations; how many are in towns, near roads, in small islands, in open nature etc Also a comment on the kind of installation used in each case will

be useful; different installation types (vault, buried, building basement, insulation system etc) could affect the sensibility to human activity noise.

> We added a description of the installation, as well as the Table 1 summarizing the site conditions in terms of possible anthropogenic seismic sources.

Line 85: The authors should explain why they decided to use the 10-30 Hz band. I suggest to use the submitted Figure 8 to justify this choice.

> Exactly. We selected that frequency band according to both spectral ratio and spectral correlation with the human activity. However, reviewer #1 suggested to extend the analyses at higher frequencies, hence we chose the band 10-40 Hz, and performed again most of the analyses. We added a sentence in section 2.2 to justify our choice.

Fig 2 and 3: I will appreciate more conventional time labels (p.e. 1st and 15th of each month)

> Done.

Fig 2: The bars marking lockdown beginning is difficult to see.

> Done.

Fig. 6 and 7: Labels are too small.

> Done.

Fig. 10: The correlation is calculated between mobility data and a mean RMS profile using all the available data? Please clarify.

> We have rephrased the part of the manuscript, describing this analysis (Section 2.3). It should be clearer now.

L. 130: Using the Spearman correlation coefficient is really justified? Are the results using Pearson really different??

> As we explained in the Section 2.3, since we do not know whether the relationship between seismic noise and mobility data is linear or not, the Spearman correlation analysis is more recommended than the Pearson correlation.

L. 140. The reference to the critical p-value level to reject the null hypothesis should be better explained. As stated above, the numbers in the y-axis of Supp Fig A3 seems very different.

> Yes, they are. We checked the computations, and they are correct. In addition, we added a couple of sentences about the critical p-value level in the section 2.3.