

First of all, we thank the Reviewer for the insightful comments. We used this second round of revisions trying to further improve the quality of the manuscript.

Following the suggestion of Reviewer #2, and as requested by multiple Reviewers during the first revision, we decided to add the analysis of the complete 2022 dataset: such analysis was not originally presented as the dataset was not available at the moment of the first submission. In light of this, we valued unnecessarily the analysis of 2019 data, at first introduced to study the "long-term" variations of noise levels: due to the recent changes in the network, the data from 2019 could only provide a partial coverage of the network. Based on the whole 2022 dataset, we also developed the Italian Accelerometric Noise Models: these should provide more exhaustive results and can be of interest to future projects.

The spatial and temporal variability has been studied with more emphasis but, while some patterns emerge from our analysis on a national scale, more localized studies are required to investigate the causative nature of the noise at specific locations.

Considering the mentioned changes to the manuscript, we decided to remove the sections related to noise variations during COVID-19 lockdown and traffic noise: while these analyses can provide some insight into the characterization of anthropogenic noise, their contribution to the manuscript (in its actual stage) is minor. In the following, the relevant changes proposed during the second revision of the manuscript are listed:

- overall changes to the structure of the manuscript to improve its readability;
- removed the COVID-19 lockdown analysis;
- removed traffic noise case study;
- added the analysis of (and limited to) the complete 2022 dataset;
- introduced the Italian Accelerometric Noise models;
- added quantitative comparison with D'Alessandro et al. (2021) models;
- improved spatial and temporal analysis of noise levels;
- expanded Discussion section.

It follows the Reviewers' comments with our detailed replies (in blue): straightforward comments with which we agree are just marked with a green tick (✓).

Reply to Reviewer 1

Dear authors, the manuscript is surely improved from the first run. Honestly, I would have chosen another organization of the work but if the other reviewers like it, there is no further problem also for me, and the manuscript is acceptable for the publication after (very) minor revision. In the following my suggestions... I think that an English native language reader could improve the text. If not possible consider at least that in the Introduction section there is an overuse of the adjective "seismic". Many "seismic" can be deleted or replaced with other synonyms (e.g. seismic events=earthquakes, seismic noise=ambient noise and so on...).

1. Line 23: seismic recorder should be plural (and "seismic" could be deleted) ✓
2. Section 2 (line 68-70): It should be obvious but I add something to say clearly that data are acceleration and using what unit of measure (cm/s², g) ? This information is useful also to understand the method, the PSD computation
We added this information into the Method section as below
"Considering only the vertical components at the stations, each daily recording in acceleration is divided into . . . "
Raw data are stored in counts.
3. Line 94: "the instrument response is then removed from the PSD" I don't understand the meaning. Do you mean that averaging limits the electronic noise produced by the instruments?
After the calculation of PSD of stations, it is divided by the instrument response of each station period-wise.
4. Line 165: a space between 532 and stations is required. ✓

Good luck

Reply to Reviewer 2

The manuscript is relevant because it evaluate the noise level of the Italian Accelerometric network. Among the discussion about the sources of noise and their variability over time, this is the baseline for determining the detection capability of the network in terms of magnitude and distance. In my view, the manuscript remains not mature for publication and I suggest to consider a major revision. This stems from two main points:

1. there is still confusion between results, discussion and observation from previous studies.
2. there is not an exhaustive discussion of the results. Moreover, the author propose reasonable motivation for some of the observed features but did on explore them.

My suggestion is to restructure the manuscript by:

1. removing the section dedicated to covid and to car passages.
We decided to accept the suggestion about removing covid and car passages and the suggestion about adding 2022 data. In the end, we limited the analysis only to the 2022 data and its spatial features. To enrich the paper, we decided to insert the background noise model for Italian strong motion data which was originally part of another article we would like to publish. As a result of this decision, some of the comments/suggestions/questions became irrelevant.
2. strongly focus on the noise level spatial variability over time and space
We believe we provided more information about the spatial and temporal variability of the background noise in the revised version. However, there are various results without or with limited explanation such as noisier weekends in longer periods. Since strong motion stations are not very susceptible in the long period effects, we do not provide any detailed explanations to these results.
3. discuss the motivation for the spatial variability by considering the soil difference at the deployment sites, the urban/countryside distinction, the presence of industrial sites.
We looked at the local soil classes but there is not much of a variation with the soil classes. Furthermore, we compared the noise level with the geological formation. Furthermore, we tried to link the noisy stations with geological settings but there is no clear correlation between geological class and noise levels. Industrial sites may play a major role on the noise levels but we decided to use only the land usage types instead of characterizing more than 500 stations' surroundings manually. In fact, there is a clear example of daily noise level change in DMN station that is located next to a hydroelectric power plant (Figure 1). One can follow the working shifts of the facility quite easily. However, in other stations there is no such clear pattern. Hence, it is hard to justify the noise levels just by industrial sites. This is why we focused more on settlements, in general.

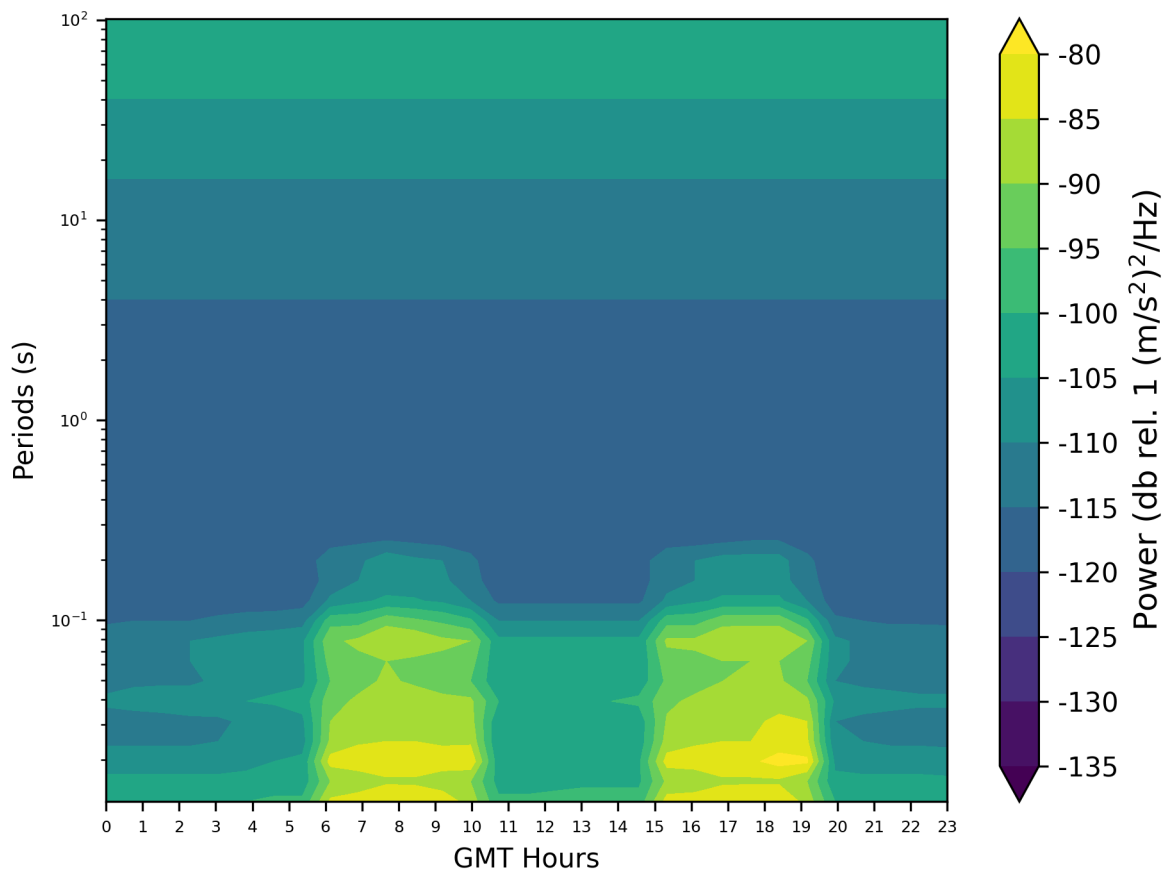


Figure 1: Daily noise variation of DMN station (lat:44.315 lon:7.271).

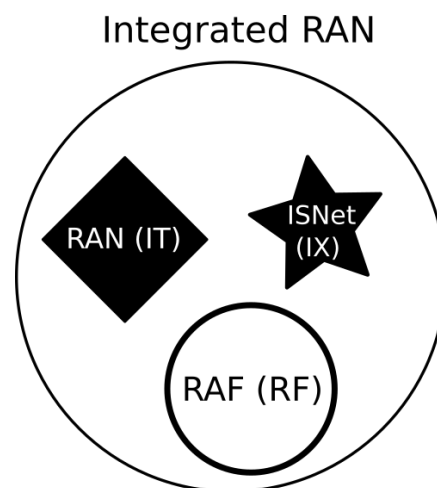
Referee Report:

1. L.23 Earth merits to be capitalised as the other celestial bodies. ✓
2. L.30-31 “To be able to monitor the seismic sources, seismic networks require knowledge about the noise content of the networks”, This sentence is not necessarily true. One could detect events by observing P-picks without knowing anything about the noise level and their sources. This counts when one is interested in a more sophisticated analysis and, for example, determining the detection threshold. Observing P wave may be an easy task in large earthquakes but in small earthquakes P wave arrival can be overshadowed by the noise. Understanding the anthropogenic sources may improve the precision of determining the epicenter of seismic events. We are using a data band-pass filtering routine for earthquake detection with dynamically selected upper and lower bands. The bands depend on the background noise of the station. Hence, for our network, background noises affect the earthquake detection capabilities. Furthermore, the event detection routine that we are currently using in our seismic monitoring task miscalculates the epicenter of an event if there is a car passing near a station at a far away distance from the seismic event when seismic signals reach the stations at the same time. This is actually why we added the vehicle noise section to the paper in the first place.
3. L61-66, This part could be shortened with a clear statement that mention that the lockdown period can be used to determine the anthropogenic component of the background noise. We deleted the sections related with the covid. Hence this paragraph is not a part of the paper anymore.
4. L.73-75, It remains unclear to me why data from 2022 are not included in this study. The authors in an early answer wrote that they were evaluating it. The authors claim they provide a better coverage. 2022 was not available due to storage problems in our servers that have been solved during the discussion process of the paper. As mentioned above 2022 is the only data source of the paper now.
5. L.100 studied by grouping”, better “grouped”? ✓
6. L.108, “20 randomly selected”, I do not understand the motivation for a random selection, and the authors forget to explain it. If the intention was to present the variety of noise levels, how can the reader interpret the differences without knowing the different location, soil etc? At least the 20 should be marked on map. I would suggest to make an arbitrary selection in which different soils and different type of urbanisation are represented. I do not understand why the authors only represent six narrow bands and not the full PSD. The authors do not explain why the “periods of interest” are the 6 reported

at line 109. This is somehow in contrast this sentence at lime 112 “we are mainly interested in periods less than 5s”. Formally speaking, the latter selected bean (5s) is out of this range of interest. The above sentence also contrast with the definition given in line 55 in which 5s is included.

20 randomly selected stations were inserted into the paper for data visualization purposes (a similar example can be seen in Figure 2 of Lecocq et al. (2020, doi: <https://doi.org/10.1126/science.abd2438>)). Since it has no significance apart from the data visualization we removed the image from the paper. We added a new figure (Figure 5) which shows the full PSD of several stations with different land usage types. In terms of periods of interest, we decided to keep the 5s and add the missing information to the necessary parts of the text.

7. L124+ Noise decreases over night, ok. But from figure 6, I see large patches of white markers as in Tuscany in which there is not such a decrease. This is not mentioned neither discussed. As mentioned above this would be the key aspects that make this paper valuable for publication.
[We discuss these stations in lines 186+ and 201+ in the Discussion section.](#)
8. L127-129 we have 5 periods, 5 median values and 6 number of noiser stations. Can't be.
[We added the 5s to the periods and number of periods and median values are matched.](#)
9. L.163, “italian strong motion network” is something different from RAN or Integrated RAN? This was never defined although it is mentioned in the title, here and in the caption of figure S1.
[In Line 163 it refers to integrated RAN. Both in Line 163 and in the caption of Figure S1 are replaced with the RAN. Venn diagram of the Italian Strong Motion network can be seen in the figure below. The entire structure \(integrated RAN\) defines the Italian Strong Motion network. As explained in L45, in the paper RAN refers to integrated RAN.](#)



10. L.178 and following, The discussion in this paragraph is not exhaustive. In frame d) we have large patterns indicating “no variation” while in frame f) the Pianura Padana is dominated by blue. This cannot be neglected. These are, in my opinion the key aspects that would make this manuscript valuable.
[About the weekday-weekend difference in the previous version of the paper observations of the review. In the revised version in which only the 2022 data is taken into consideration, results are slightly different. We discuss the possible explanations of the results in Lines 207+.](#)
11. L.181-184, this is a clear sample of my general comment. We are in the section “Discussion” dedicated to the discussion of the results and the section mention previous results and give motivation for the results of this paper. But this is not enough, how can I be certain that we are observing wind or sea or whatever else if the authors do not show it. One could compare noise variation with wind speed, or with sea storms, or traffic data. This is in my opinion an incorrect approach to data analysis.
[We decided to delete this paragraph.](#)
12. L.193, I am confused, Noiser or quieter? Where can I see this?
[Weekdays are noisier than the weekends. We changed "weekends" with "weekdays".](#)
13. L.226-232, What is the added value of this study with respect to what observed by Poli et al, or by Piccinini et al? The latter also discusses spatial patters and economical motivations. It is not enough to write that noise generated by human circulation decreased when people where locked down. This is not a novel discovery. . . .
[As mentioned before covid related parts of the paper are erased. Hence this part is not a concern anymore.](#)

14. L.233-236, This are “results” not discussion of them.
As mentioned before covid related parts of the paper are erased. Hence this part is not a concern anymore.
15. L255 and 244, for those not familiar with the area, DTS2 appears located in two different places.
In Line 238, we provide the information about the building where DST2 is located. However, at Line 228 we provide the geological settings of the station.
16. All toponyms should be marked on maps, authors cannot presume that the reader knows where Ischia, or Naples or Palata are located on maps.
We added the cities mentioned in the paper into the maps. We added a small map inside Figure 10 to show the location of Trieste on map. Locations of the stations presented in Figure 5 are not provided on the map since the significant information of the stations are their land usage not their location on map.
17. Section 5.3, This section presents some results, and it does not include a discussion of them in the contest of the paper.
Sections related to vehicle noise are deleted.
18. As remarked in my previous review, accuracy is crucial when writing and when reporting information. The coordinates of Palata differ in section 5.3 and in figure 15. Moreover 41.886, when truncated to a two digit number is 41.89.
We deleted the coordinates provided in Figure 14 since they are already given in Section 5.3.
19. L274 “have higher noise levels than the AHNM” should be “have noise levels higher than AHNM”? ✓
20. L278, the example of CSA7 is confusing, the station was never mentioned before (except in one figure of the supplementary material). I think that CSA7 should be explicitly inserted in the discussion or results section before citing it in the conclusion. How can the reader understand this example?
We deleted the example of CSA7.
21. L278 “some of these stations” some is vague. I think the author could evaluated the number of percent of station located in towns.
We updated Table 5 by adding the land usage distribution of the stations that exceed the AHNM and referring it in L243.
22. L279 “the true nature of the ground motion if there is a strong earthquake nearby” this is a generic and vague statement. Data and analysis could be used to provide a quantitative result. How many can record the full waveform of a magnitude 3 or 4 or 2.5 with a proper Signal-to-Noise ratio?
We agree with the vagueness of the sentence. We added a paragraph to discussion and added Brune’s corner frequency values to Figure 4 to show the capabilities of the network. We also provided an earthquake example in supplementary material to show a real case example of the effect of background noise.
23. L280 “capabilities of the stations” is a vague concept.
We rephrase the sentence as follows "Depending on the nature of the future station installations and studies, noise levels of RAN (Figure 4) may give an insight into the suitable locations for the deployment.".
24. L281, again, “The surrounding conditions for RAN stations within settlements are variable and have noticeable effects on the noise levels” is this a result of this study? How the authors distinguished the different condition for stations within settlements? How can we get to this conclusion?
Analysis of the background noise levels of DST2 and CARC stations can be an example on this. They both located in settlements with different background noise level.
25. L289, Why this is observed at some stations and not at others? Instrumental difference, site difference?
Day-night variations reduce significantly in the longer periods. If we plot all the subfigures in Figure 7, in longer periods we would get almost no daily variations. Furthermore, in the longer periods our stations are ‘deaf’ hence the interpretation of the daily variation of these periods are tricky. We deleted the sentence and add another conclusive sentence related with day-night variance: "The difference is relatively low in the stations located on the mountainous parts of North-East Italy." and in Discussion we add: "In North-East Italy, there are several station with relatively low daytime-nighttime difference. These stations are located far away from all settlements and located on mountainous parts of Italy.".
26. L295 The author touch the fact the accelerometers are “deaf” and, in absence of strong ground motion they record the self noise of the instrument. Should not this pointed out at the beginning to restrict the detection capability of the instrumentation used instead of using it as an empirical conclusion?
It can be expected to not see the long period effects in strong motion stations. However, there are not so many studies about this topic. Cauzzi and Clinton (2014) mentioned briefly that there are not significant variations in long periods but previous studies do not provide any specific analysis. In this paper, we show that in long periods accelerometric networks do not provide and seasonal variations. So

reader can compare the long period results, for example Antony et al. (2022), and see the differences between broadband and strong motion records in long periods.

27. L298-299 “an average reduction in the noise level of 1.0 dB (and up to 2.9 dB at 0.0625 s) during the daytime”. Neither in the manuscript nor in the supplementary material this numbers (1.0, 2.9 and 0.0625) can be found by myself
As mentioned before covid related parts of the paper are erased. Hence this part is not a concern anymore.
28. Figure 1, D’Alessandro et al is 2020 in the figure and 2021 in the caption. ✓
29. Figure 4. Again on the care of details, The figure as nine frames labelled from a) to I). In the caption it is mentioned a-g). Moreover the latter is for 80.6s but this period is never discussed in the manuscript. “Vertical components are presented in the following figures and Electronic Supplement.” This is not clear.
We changed the annotations and now they cover all subfigures. We mentioned about the vertical components since we only use the vertical components and exclude the horizontal ones. However, to better explain this fact, we specified the selection of the vertical component at Line 59 of Data section and add a sentence to Line 83 in Method section.
30. Figure 5, the noise model are NLNM and NHNM by Peterson or A. . . by Cauzzi and Clinton, the caption is confusing.
Model acronyms are wrong and they correct ones are ALNM and AHNM defined by Cauzzi and Clinton.
31. Figure 6, The caption is not correct. I suspect the figure represents the different for each station and for each period between the median of the noise level at daytime and nightttime.
Reviewer is correct. In both Figure 6 and Figure 7 differences are presented between day and night and weekday and weekend.
32. Figure 14, I fear that the authors while using the image from Google did not follow the reproduction rules set by NHESS and by google. please check.
Source of the satellite data is added to the caption of the Figure.
33. Table 3, caption. Stations or number of stations? “with higher” or “with noise level higher”. ✓
34. Table S1, To me “evolution of the sensors” means how each sensor evolved/changed”. Looking at numbers, I suppose the authors are referring to the change over time of the number of sensors divided by type.
Since we only use 2022 data, this is irrelevant now. In Table S1, we provide the periodwise limits of Lower (IALNM) and Higher (IALNM) Limits of the Italian Accelerometric Noise Model.
35. Figure S1, The authors did not describe what is the difference between the single maps or, in other words, what the number on top of each frame is. Moreover, it is a common practice to label each frame with a letter or a number.
We updated the titles of the figures and now periods are specifically written as a title of each figure. Furthermore we labelled each subfigure with Roman letters.
36. Figure S2, il the title, “Difference” should not be capitalized. In the title the author us the dash without spaces (Weekday-Weekend) while in the caption the use it with spaces (2019 - 2022). Should this follow the same rule?
Figure S2 is changed after the revision. Now Figure S2 provides information about Carciotti Palace (formerly Figure 12).
37. Figure S2 caption, punctuation is messy
Figure S2 is changed after the revision. This figure does not exist anymore.
38. Figure S3, I suggest to include also M5 and M6 in this figure. And to carefully discuss it. How many station would miss to record correctly the full waveform for the ground shaking of a M5 or M4? Moreover, different lines should be described in the caption.
M5 and M6 are added to the Figure 4 in the manuscript also to Figure S3 in supplementary material. We also add the interpretation of the Figure S3 in the supplementary material.
39. Table S3, caption. The authors miss to mention what is higher than AHNM, I suppose they are referring to “Stations with noise level higher then AHNM”.
Reviewer is correct (except it is Table S2). Caption of the table is updated as "Stations with noise level higher then AHNM in the network (median and 2.5 statistics).".
40. Table S3. Period and AHNM require the measure unit besides them. No of station should be no of stations.
Periods are in seconds. "(s)" is added next to "Period" in the first column.