

REVIEW REPORT FOR
**Atmospheric Signals Recorded by Seismometers in the Sub-Seismic Frequency
Band**

published in *Seismica*

ROUND 1 REVIEWS

REVIEWER 1

I apologize for the slow review of this work. I enjoyed reading this manuscript and recommend it for publication. I appreciate the authors care attention to writing so that the manuscript doesn't have lots of typos.

Adam Ringler

Below are some additional comments.

I really like this approach that Poli has developed. It isn't clear to me what the eventual applications of this work could be, it is interesting. It is unfortunate that the amplitude information gets discarded.

Could you add some more details about the processing or make your code available for reproducibility?

Other than simply observing these signals, what might one do with this information?

It would be cool to see some modelling of this phenomena. However, maybe this has already been done elsewhere and I am just not familiar enough with the field to know this.

Other minor comments:

Abstract: Aren't changes in gravity at Earth's surface, changes in ground motion?

Line 16: Do you want analysis?

Line 22: Atmospheric monitoring of what?

Line 26: This sentence needs to be split up.

Your Dahlen and Tromp reference year might be wrong.

Line 92: TA didn't operate any STS-1 seismometers.

Line 144: You should point out that you decimated the LDO to match the VH* as this could confuse folks.

Line 242: Did you use the US network or only TA?

REVIEWER 2

The study by Poli et al. is a nice illustration of stacking and coherence analyses to show the atmospheric tides in seismometer data. The manuscript is clear, well-written. Unluckily,

the novelty is poor since the atmospheric effects in seismometer (like in gravimeter) records have been widely studied in the past. Besides, some misinterpretations are present since the periodic signals at 1 day and its harmonics are mostly atmospheric tides, but also contain some contributions from solid tides (this partly explains the different values for the admittance between gravimeters and seismometers ; another source of error is the uncertainty in the transfer functions at long periods for many seismometers, which underestimate the tides). Moreover, atmospheric effects are mostly local effects (roughly 90% local, 10% regional at mid-latitudes) so estimating an average admittance from the stack is not useful. In order to improve the detectability of seismic data, a local admittance must be computed and used to reduce seismic data before stacking, just as is routinely done with superconducting gravimeter data. In view of the lack of novelty and some scientific flaws, I cannot recommend publication of this paper.

REVIEWER 3

The manuscript focuses on the characterization of acceleration signals recorded by seismometers in the sub-seismic frequency band - and the determination of its atmospheric origin - using a recently introduced stacking approach on a network of broadband sensors.

This manuscript is well written and organized, referenced, and thorough. The first section introduces well the main challenges and the utility of this study. The data processing and main computations are adequately described (when needed, some clarifications are listed in the file attached).

This paper would mostly benefit, in my opinion, from a slightly extended discussion on how the atmospheric effects characterized in this study could be removed from seismological observations, in order to record smaller, subtle tectonic signals.

See annotated pages following this compilation of text comments.

Answers to these reviews are inserted below, following this compilation of text comments.

ROUND 2 REVIEWS

REVIEWER 1

I think the authors have done a good job addressing my suggestions.

I noticed that Reviewer B suggested that 90% of atmospheric effects are local and 10% are regional. If this is true, it would have been nice if they provided a citation so that it could be verified. Without this information it seems difficult to really draw any conclusions from this.

I look forward to seeing this work published,

Adam Ringler

Title : Atmospheric Signals Recorded by Seismometers in the Sub-Seismic Frequency Band

Submitted to : Seismica

Authors : Piero Poli, Josipa Majstorovic, T. Dylan Mikesell

General comments to editor and authors

The manuscript focuses on the characterization of acceleration signals recorded by seismometers in the sub-seismic frequency band - and the determination of its atmospheric origin - using a recently introduced stacking approach on a network of broadband sensors.

This manuscript is well written and organized, referenced, and thorough. The first section introduces well the main challenges and the utility of this study. The data processing and main computations are adequately described (when needed, some clarifications are listed below).

This paper would mostly benefit, in my opinion, from a slightly extended discussion on how the atmospheric effects characterized in this study could be removed from seismological observations, in order to record smaller, subtle tectonic signals.

Provided the authors answer to the comments listed below, I recommend this paper to be published in Seismica.

Specific comments and questions

In the comments below (listed as they occurred in main text), *blue italic text* refers to texts extracted from the main text.

Page 2, lines 53-54

« *However, broadband seismic stations are far more numerous and have recorded continuous data over several decades on a global scale.* »

What is the number of currently-deployed (very-)broadband seismometers, compared to superconducting gravimeters. And the corresponding duration of oldest, ongoing observations?

Page 3, lines 72-73

« *The objective of the current study is to quantitatively establish that permanent harmonic signals recorded by seismometers at sub-seismic periods originate from pressure variations from atmospheric tides.* »

What would be the other possible mechanisms (if any) that could explain such permanent harmonic signals?

Page 3, line 89

« *We used data from the USArray recorded during the period Jan. 2012 to Dec. 2014.* »

This sentence seems to contradict the two years-long dataset described in the Abstract (« *Using two years of collocated vertical seismic and pressure data* », line 14) and in Section 2 (« *... over the entire two-year analysis period* », line 94).

Page 3, line 89

« *The seismological data were recorded using broadband and very broadband seismometers (Streckeisen STS-2 and STS-1, Guralp CMG-3T, Teledyne-Geotech KS54000).* »

Is one (or several) of these sensors more sensitive to sub-seismic periods than the others?

Page 3, lines 98-99

« *Before the deconvolution, bandpass filtering was applied to the data with cutoff: $f_1=2.3148e-6$ Hz, $f_2=2.8935e-6$ Hz, $f_3=3.333e-4$ Hz, and $5.0e-4$ Hz.* »

Such cutoff seems slightly arbitrary to the readers at first. Explaining further those values may improve the readability ($1/f_1 = 5$ days, $1/f_2 = 4$ days, $1/f_3 = 3000$ s, $1/f_4 = 2000$ s).

Also, $f_3 = 3.333e-4$ Hz is larger than the frequency corresponding to 1 hour ($= 2.778e-4$ Hz), defined by the authors as the maximum frequency of the sub-seismic frequency range (1 to 24 hours, line 37). Is there a reason to set $f_3=3.333e-4$ Hz instead of $f_3=2.778e-4$ Hz? Out of curiosity, would the main conclusions of the present study hold with a slightly higher-frequency content (say, up to 1mHz, or even above)?

Page 4, lines 105-106

« *We retained only the 10-day segments without gaps and zeros to avoid additional corrections to these data.* »

What is the proportion of the total dataset retained at this step?

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« *Each time window T is 10 days, and the time series data were tapered using a Hanning window before computing the Fourier transform.* »

Is a demeaning also necessary prior to the computation of the Fourier transform?

Page 5, lines 144-146

« *To better understand the relationship between pressure and acceleration signals observed at sub-seismic frequencies, we estimate the coherence and pressure-normalized transfer function (TF) between channels LDO and VHZ.* »

A discussion (on similarities and differences) about the physical meaning of the coherence and the transfer function (whose Formula (2) and (4) are closely related) will in my opinion improve the readability of Section 4.

Figure 2(a), page 6

I would change the major x-ticks from one tick every 100,000 seconds to one tick every day ($= 86,400$ seconds), to improve readability and in relation to the other subplots (expressed in cycles per day).

Figure 2, page 6

« *Red dots (b-i) indicate local amplitude peaks.* »

How is the number of local amplitude peaks defined, and the frequency ranges within which the search is done?

Page 7, lines 154-155

« Furthermore, we observe a nearly linear increase in coherence across the analyzed frequency band, from lower to higher frequencies, which is consistent with previous studies based on collocated gravimeters and barometers. »

What is the physical meaning of this linear trend? Would this behavior be related to inertia or elastic displacement (listed as pressure mechanisms influencing geophysical observations in Line 35 of Page 2), or to an entirely different mechanism?

Figure 4(a), page 9 + Figure 2(g,i), page 6

Can we explain why the coherence between seismic and pressure data (or directly the stack of pressure data in Figures 2g and 2i) is high at frequencies close to 7 and 9 (and to a lesser extent 11 and 13) cycles per day (cpd), and not at frequencies close to 8, 10 and 12 cpd?

Page 7, equation (5)

Why the use of this specific mean for the computation of the average transfer function, rather than an arithmetic mean as in Eq. (3) for the computation of the average coherence?

Page 7, lines 165-166

« The average magnitude of the admittance is 4 times larger than the theoretical expectation from Newtonian attraction. »

It may be worth citing Equation (1) here.

Page 8, lines 183-184

« As a result, both the pressure-acceleration coherence and the pressure-normalized transfer function represent an average in time and space. Consequently, we cannot resolve spatial or temporal variations in the admittance between the different datasets. »

Would a computation of the coherence on a portion of the dataset be possible, in order to estimate potential spatial and temporal variations? For instance: focused on sensors located North versus South, or West versus East, or data recorded during Summer versus Winter?

Page 10, lines 215-217

« Although the value and phase of the transfer function indicate the physical origin of the acceleration peaks at sub-seismic frequencies, applying this transfer function to correct acceleration data is not straightforward. As we average over time and space, our estimate may not capture potential spatio-temporal variations in the admittance... »

Have you tried to correct the acceleration data anyway? As stated above, since the removal of atmospheric contamination is mentioned several times, a practical discussion on how to correct for atmospheric effects will benefit to this - already very solid - study.

Typos

- Page 1 line 6 : Department of Geosci*en*ce —> Geosci*en*ce
- Page 2 line 38 : (Zürn and Wielandt, 2007) —> Zürn and Wielandt (2007) ?
- Page 3 line 89 : (Fig. 1a) —> (Fig. 1)

Dear Piero Poli, Josipa Majistorovic, Dylan Mikesell:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Atmospheric Signals Recorded by Seismometers in the Sub-Seismic Frequency Band". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after revisions. As you can see, three reviewers have seen your work. The most experience reviewer (B) is actually against the publication of this work. Despite this, I suggest to continue the review process, and I ask you to consider that set of comments as well, together with those from the two other reviewers.

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Prof. György Hetényi
University of Lausanne, Switzerland

Reviewer A:

I apologize for the slow review of this work. I enjoyed reading this manuscript and recommend it for publication. I appreciate the authors care attention to writing so that the manuscript doesn't have lots of typos.

Adam Ringler

Below are some additional comments.

I really like this approach that Poli has developed. It isn't clear to me what the eventual applications of this work could be, it is interesting. It is unfortunate that the amplitude information gets discarded.

We thank Adam Ringler for his comments and the time he took to review our work. In the manuscript, we have clarified that seismometers can complement gravity observations at sub-seismic frequencies (which represents a significant advantage given that seismometers are far more numerous than gravimeters and have been recording continuously for much longer periods), help track atmospheric processes (on a global scale, rather than being limited to local, single-point measurements), and reveal the dominant influence of atmospheric noise in seismometer data from Newtonian attraction, while taking care not to overstate our conclusions.

Could you add some more details about the processing or make your code available for reproducibility?

We are happy to share the code, although it is very simple—essentially performing spectral multiplications on large datasets—so it does not involve any advanced techniques. The core of the code is:

```
mean = np.mean(data, axis=1)  
  
data = data - mean[:, None]  
  
sig_tmp = data * window.hann(data.shape[1])  
  
freq = np.fft.fftfreq(sig_tmp.shape[1], dt)  
  
spec = np.fft.fft(sig_tmp, n=npadi, axis=1)  
  
c = np.conjugate(spec) * spec[:, None, :]
```

Where *data* is a matrix with *N* rows representing stations and columns representing the samples (for a total of 10 days). The rest of the code mainly handles data management. For example, downloading and processing the data was done using ObsPy, which is cited and publicly available.

We have already provided a thorough description of the processing steps, including filtering and the water level used in the deconvolution of the instrument response. These details are rarely reported in seismological studies, yet we consider them essential, as such processing choices can ultimately mask or modify subtle features in the data, as discussed in the manuscript. We added more information in the main text.

Other than simply observing these signals, what might one do with this information?

We ourselves have been wondering this. As we progress in our research of these signals (e.g., Poli et al., 2025; this manuscript; on-going research) we are gaining a better understanding of these signals and investigating applications. We plan to continue reporting our findings as we learn more. For now, we have extended our processing and discussion in the revised manuscript by exploring the stacking in time and space domains. We are working to learn if we can use seismometers, despite their high noise level, to study signals at sub-seismic frequencies through time and space. Making interpretations of these signals will be future work, and we are looking into this now that we have confirmed the origin of these signals in the current manuscript.

We believe that observations obtained through alternative data sources and processing approaches are always valuable to the scientific community.

It would be cool to see some modelling of this phenomena. However, maybe this has already been done elsewhere and I am just not familiar enough with the field to know this.

As modeling of atmospheric effects in geophysical measurements was done in several previous studies, cited in our main text, we believe that it is redundant to develop this again.

Other minor comments:

Abstract: Aren't changes in gravity at Earth's surface, changes in ground motion?

This sentence was indeed weird, and we modified it slightly. Note it does not show up in the LatexDiff version; for some reason the abstract section is not caught in the latexdiff algorithm.

Line 16: Do you want analysis?

Fixed.

Line 22: Atmospheric monitoring of what?

We develop this concept better in the abstract.

Line 26: This sentence needs to be split up.

Fixed.

Your Dahlen and Tromp reference year might be wrong.

Fixed.

Line 92: TA didn't operate any STS-1 seismometers.

Fixed.

Line 144: You should point out that you decimated the LDO to match the VH* as this could confuse folks.

As we mentioned in the original main text both datasets have been resampled at 0.001Hz: “we resampled the pressure and acceleration time series from 0.1 and 1Hz, respectively, to 0.001Hz”. We now specify more clearly.

Line 242: Did you use the US network or only TA?

Just TA.

Recommendation: Revisions Required

Reviewer B:

The study by Poli et al. is a nice illustration of stacking and coherence analyses to show the atmospheric tides in seismometer data. The manuscript is clear, well-written. Unluckily, the novelty is poor since the atmospheric effects in seismometer (like in gravimeter) records have been widely studied in the past.

We thank the reviewer for these comments; however, we respectfully disagree with them. Our response to these points is provided below. We also note that the comments focus on a single aspect of our work, while the remainder of the manuscript presents a detailed analysis of the signals composing the “noise” at subseismic frequencies in seismometer recordings. To our knowledge (see also the discussion below), no previous studies have characterized the composition of the seismic wavefield at such high resolution, including harmonic signals up to more than 10 cycles per day.

Besides, some misinterpretations are present since the periodic signals at 1 day and its harmonics are mostly atmospheric tides, but also contain some contributions from solid tides (this partly explains the different values for the admittance between gravimeters and seismometers ; another source of error is the uncertainty in the transfer functions at long periods for many seismometers, which underestimate the tides).

We indeed focus our analysis just on peaks above 3cpd where the occurrence of solid tides is marginally affecting the gravity, in particular at integer fraction of one day. Therefore, we again disagree with this reviewer that we are misinterpreting signals related to solid Earth tides. We are not really studying those here; we simply note in the manuscript that they do exist below 3.2 cpd.

Moreover, atmospheric effects are mostly local effects (roughly 90% local, 10% regional at mid-latitudes) so estimating an average admittance from the stack is not useful. In order to improve the detectability of seismic data, a local admittance must be computed and used to reduce seismic data before stacking, just as is routinely done with superconducting gravimeter data.

We are aware of the difference between local and regional effects, and we have explicitly considered this in the revised Discussion section of the manuscript. Additionally, we now present in the main text further analyses showing that spatial and temporal variability can be resolved, and we honestly discuss the main implications and limitations of using seismological data. These data provide a complementary dataset for studying long-period gravitational processes: although not without limitations, they are more temporally and spatially consistent than other geophysical datasets. Rather than outright neglecting these seismic datasets, we hope our manuscript will spur other disciplines to consider the value of these complementary data in aiding understanding of Earth processes.

It is important to acknowledge that such a detailed study of the admittance, with clear evidence of frequency dependence—particularly at integer fractions of one day—has not been reported previously for seismological data and was observed only through the stacking of data. This result is made possible by the stacking of seismological signals, which are intrinsically noisier than those from superconducting gravimeters.

Further, we do not suggest that seismometer data, which represent single-point measurements, should be processed in the same way as single-point gravimeter data. Rather, we emphasize that seismic data at sub-seismic frequencies contain valuable information that has not yet been fully explored.

In view of the lack of novelty and some scientific flaws, I cannot recommend publication of this paper.

While we can appreciate differing points of view around the present topic, we strongly disagree with this reviewer's conclusions. Our work explicitly acknowledges its limitations, while at the same time providing new insights into the atmospheric effects on seismological data. Among the several novel aspects presented, we demonstrate a clear causal relationship between seismic data and atmospheric dynamics in the sub-seismic frequency range. While this relationship was known in gravimetry, and therefore assumed for seismic observations, it has not been demonstrated explicitly before. This result opens the possibility of using large seismological networks for future studies aimed at detecting geodynamic signals, and it also highlights the potential of seismological networks to provide additional observations of atmospheric waves and dynamics. Although similar considerations apply to gravimeters, the number, availability, spatial coverage, and temporal continuity of seismological stations are currently far superior to those of gravimetric networks.

Recommendation: Decline Submission

Reviewer E:

The manuscript focuses on the characterization of acceleration signals recorded by seismometers in the sub-seismic frequency band - and the determination of its atmospheric origin - using a recently introduced stacking approach on a network of broadband sensors.

This manuscript is well written and organized, referenced, and thorough. The first section introduces well the main challenges and the utility of this study. The data processing and

main computations are adequately described (when needed, some clarifications are listed in the file attached).

This paper would mostly benefit, in my opinion, from a slightly extended discussion on how the atmospheric effects characterized in this study could be removed from seismological observations, in order to record smaller, subtle tectonic signals.

Recommendation: Revisions Required

Specific comments and questions

In the comments below (listed as they occurred in main text), blue italic text refers to texts extracted from the main text.

Page 2, lines 53-54

« However, broadband seismic stations are far more numerous and have recorded continuous data over several decades on a global scale. »

What is the number of currently-deployed (very-)broadband seismometers, compared to superconducting gravimeters. And the corresponding duration of oldest, ongoing observations?

The current number of broadband seismometers composing global seismic networks is on the order of 200 stations, compared with only about a dozen gravimeters. Beyond this difference in number, seismological data are temporally consistent, relatively clean, and readily usable. In addition, large temporary deployments (e.g., USArray), composed of hundreds of high-quality stations, provide a valuable source of information not only on the high-frequency part of the seismic wavefield but also, as we show in this work, on long-period signals (up to hours to days). Nevertheless, we prefer not to engage in a direct comparison with gravimetric data; rather, as stated in the manuscript, we simply suggest that seismic networks represent an additional and complementary source of information on long-period gravity-field variations.

Page 3, lines 72-73

« The objective of the current study is to quantitatively establish that permanent harmonic signals recorded by seismometers at sub-seismic periods originate from pressure variations from atmospheric tides. »

What would be the other possible mechanisms (if any) that could explain such permanent harmonic signals?

This sentence may have been unclear, so we revised it. Our goal was not to determine the origin of these signals, which is atmospheric, but rather to quantify how atmospheric processes affect seismometers.

Page 3, line 89

« We used data from the USArray recorded during the period Jan. 2012 to Dec. 2014. »

This sentence seems to contradict the two years-long dataset described in the Abstract

(« Using two years of collocated vertical seismic and pressure data », line 14) and in Section 2 (« ... over the entire two-year analysis period », line 94).

These instances are now fixed in the main text. The duration is 3 years (Jan. 2012 to Dec. 2014). Thank you for catching this mistake in the text.

Page 3, line 89

« The seismological data were recorded using broadband and very broadband seismometers (Streckeisen STS-2 and STS-1, Guralp CMG-3T, Teledyne-Geotech KS54000). »

Is one (or several) of these sensors more sensitive to sub-seismic periods than the others?

This is an interesting question that raises several technical aspects, which were not explored in this study. We plan to focus on these technical issues in future research.

Page 3, lines 98-99

« Before the deconvolution, bandpass filtering was applied to the data with cutoff: $f_1=2.3148e-6$ Hz, $f_2=2.8935e-6$ Hz, $f_3=3.333e-4$ Hz, and $5.0e-4$ Hz. »

Such cutoff seems slightly arbitrary to the readers at first. Explaining further those values may improve the readability ($1/f_1 = 5$ days, $1/f_2 = 4$ days, $1/f_3 = 3000$ s, $1/f_4 = 2000$ s).

Also, $f_3 = 3.333e-4$ Hz is larger than the frequency corresponding to 1 hour (= $2.778e-4$ Hz), defined by the authors as the maximum frequency of the sub-seismic frequency range (1 to 24 hours, line 37). Is there a reason to set $f_3=3.333e-4$ Hz instead of $f_3=2.778e-4$ Hz? Out of curiosity, would the main conclusions of the present study hold with a slightly higher-frequency content (say, up to 1mHz, or even above)?

We fully agree with the reviewer and have modified the definition of the frequencies accordingly. The choice of 3000 s was made solely to allow visualization of the signal over an interval of approximately one hour.

Regarding your question, we do observe seismic signals with frequency-dependent coherence at higher frequencies. However, since such signals are already well documented and their relationship to atmospheric dynamics is understood, we did not explore these frequencies further in this study.

Page 4, lines 105-106

« We retained only the 10-day segments without gaps and zeros to avoid additional corrections to these data. »

What is the proportion of the total dataset retained at this step?

From the analysis performed in this study, approximately 70% of the data pass this test. This is a remarkable figure and clearly demonstrates the high quality of seismological data, which surpasses that of other geophysical datasets. Although these other datasets may exhibit higher signal-to-noise ratios at sub-seismic frequencies, they often have limited temporal consistency in their records. We added this information in the main text.

Page 4, lines 112-113

« Each time window T is 10 days, and the time series data were tapered using a Hanning window before computing the Fourier transform. »

Is a demeaning also necessary prior to the computation of the Fourier transform?

Yes indeed, we added this to the main text.

Page 5, lines 144-146

« To better understand the relationship between pressure and acceleration signals observed at sub-seismic frequencies, we estimate the coherence and pressure-normalized transfer function (TF) between channels LDO and VHZ. »

A discussion (on similarities and differences) about the physical meaning of the coherence and the transfer function (whose Formula (2) and (4) are closely related) will in my opinion improve the readability of Section 4.

We added this discussion in the main text.

Figure 2(a), page 6

I would change the major x-ticks from one tick every 100,000 seconds to one tick every day (=86,400 seconds), to improve readability and in relation to the other subplots (expressed in cycles per day).

This is done. The x-axis is now in units of days.

Figure 2, page 6

« Red dots (b-i) indicate local amplitude peaks. »

How is the number of local amplitude peaks defined, and the frequency ranges within which the search is done?

We used a local peak finding algorithm (code below). We know that there are many small (noise-related) peaks. We kept the largest peaks within ± 0.75 cpd from each integer fraction of the day. Hence, the red dashed lines in Figure 2 are the actual integer fractions of a day and the red dots are the peaks identified nearest to each theoretical fraction.

```

def get_peaks(d, pc):
    f = 24/pc
    # Find peaks in d
    peaks, properties = find_peaks(d)

    # Filter peaks to those closest to integer values from 1 to 15
    target_vals = np.arange(1, 16)

    # Find the closest peak for each target value
    filtered_peaks = []
    filtered_f_peaks = []

    for val in target_vals:
        if len(peaks) > 0:
            # Calculate distances from current target value to all peak frequencies
            distances = np.abs(f[peaks] - val)
            # Find the index of the closest peak
            closest_idx = np.argmin(distances)
            # Only include if the distance is reasonable (e.g., within 0.75)
            if distances[closest_idx] <= 0.75:
                filtered_peaks.append(peaks[closest_idx])
                filtered_f_peaks.append(f[peaks[closest_idx]])

    filtered_peaks = np.array(filtered_peaks)
    filtered_f_peaks = np.array(filtered_f_peaks)

    # Get the peak values at the filtered peak indices
    peak_values = d[filtered_peaks]

    return filtered_f_peaks, peak_values

```

Page 7, lines 154-155

« Furthermore, we observe a nearly linear increase in coherence across the analyzed frequency band, from lower to higher frequencies, which is consistent with previous studies based on collocated gravimeters and barometers. »

What is the physical meaning of this linear trend? Would this behavior be related to inertia or elastic displacement (listed as pressure mechanisms influencing geophysical observations in Line 35 of Page 2), or to an entirely different mechanism?

We discuss the mechanism in the revised text.

Figure 4(a), page 9 + Figure 2(g,i), page 6

Can we explain why the coherence between seismic and pressure data (or directly the stack of pressure data in Figures 2g and 2i) is high at frequencies close to 7 and 9 (and to a lesser extent 11 and 13) cycles per day (cpd), and not at frequencies close to 8, 10 and 12 cpd?

At present, we are not able to provide a physical explanation for the observed frequency dependence of the coherence and admittance. Addressing this behavior is the focus of planned future studies; however, resolving it will likely require detailed atmospheric modeling, which is beyond the scope of the present work. These are the interesting types of questions that we think should be interrogated with this new seismic observable.

Page 7, equation (5)

Why the use of this specific mean for the computation of the average transfer function, rather than an arithmetic mean as in Eq. (3) for the computation of the average coherence?

The main issue in using the arithmetic mean are the outliers, as large amplitude values generated by spikes or other issues in the data. In equation 3 their effect is reduced and marginal as the amplitude will always be bounded between 0 and 1, while here some issue in pressure (P) data will give rise to very anomalous values.

Page 7, lines 165-166

« *The average magnitude of the admittance is 4 times larger than the theoretical expectation from Newtonian attraction.* »

It may be worth citing Equation (1) here.

Fixed.

Page 8, lines 183-184

« *As a result, both the pressure-acceleration coherence and the pressure-normalized transfer function represent an average in time and space. Consequently, we cannot resolve spatial or temporal variations in the admittance between the different datasets.* »

Would a computation of the coherence on a portion of the dataset be possible, in order to estimate potential spatial and temporal variations? For instance: focused on sensors located North versus South, or West versus East, or data recorded during Summer versus Winter?

We have now investigated both the temporal and spatial variability, and we report the results in the new Figure~5. In addition to describing this new analysis, we have added a dedicated discussion.

Page 10, lines 215-217

« *Although the value and phase of the transfer function indicate the physical origin of the acceleration peaks at sub-seismic frequencies, applying this transfer function to correct*

acceleration data is not straightforward. As we average over time and space, our estimate may not capture potential spatio-temporal variations in the admittance... »

Have you tried to correct the acceleration data anyway? As stated above, since the removal of atmospheric contamination is mentioned several times, a practical discussion on how to correct for atmospheric effects will benefit to this - already very solid - study.

We have not yet applied the data correction, but we plan to test this effect on real data in the future. In this article, however, we chose to focus on the observations and the relationship between pressure and seismic signals. Developing a correction will require additional work, as we honestly discuss in the article.

Typos

- Page 1 line 6 : Department of Geoscience → Geoscience

Fixed

- Page 2 line 38 : (Zürn and Wielandt, 2007) → Zürn and Wielandt (2007) ?

Fixed

- Page 3 line 89 : (Fig. 1a) → (Fig. 1)

Fixed