Reviewer 1

There are several new figures and case studies, which need to be improved before the paper can be published.

The content of Figs 5-8 and 11 should be described more clearly. I assume that each "line" of dots is the one of one ray? So, depending on the colours seen near the triangles one can estimate, which turning heights were involved?

The description of the visualisations of the multi azimuth simulations was added in Section 3.1.1:

The red asterisk represents the point source. The concentric sectors of circles show regions of ensonification, regions where the signal emitted by the source can be recorded at an infrasound station. The dots, signal ground reflections are organized in a radial pattern. Each of the lines of this pattern represents one azimuth of signal propagation for which the multi azimuth simulation is run; the azimuth step is 3°. The colours of the dots inform about the turning height of the ray and thus provide information about signal ducting in the waveguides.

There is discussion of the effect of the jet stream, but this must be accompanied by presentations of the wind field. It is written that the model sginal sources are estimated based in the jet stream, but how?

The way the source points were selected is now described in section 3.1.1:

The fictitious point sources are located (1) at 55°N and 15°W, (2) at 55°N and 5°W, and (3) at 60°N and 0°longitude. The coordinates of the sources are estimated based on the position of the tropopause jet stream disturbance. Point (1) is located under the northward jet-stream, point (3) under the southward jet-stream, and point (2) is located between those two opposing branches of the jet stream disturbance.

Conclusions are unclear. For example, what does it mean that from Figures 5 – 7 follows that the effects of the streamer event occurs in the limited regions close to the sources? And why does it follow from the InfraGA/GeoAc outputs that signal propagation from sources in the North Atlantic to Central Europe is not significantly modified by the streamer (lines 384-386)?

We thank to reviewer for pointing out the insufficient discussion of the obtained results. The raytracing results together with the observations are discussed in more depth in Section 3.1.1 and in Section 3.1.2 The Discussion and conclusions section was modified accordingly.

I have difficulties to interpret Fig 11. There are 2 red lines, which do not reach the infrasound arrays. There are 2 sources mentioned for the March 10 simulation (lines 433-434), but only one is discussed.

The description of the raytracing results for 10 March 2021 was extended. The model results for the source at 55°N 15°W are provided in the supplementary figure. The results for the source at 55°N 0°latitude are shown in the paper in Figure 10.

Minor comments

Line 252: reflects -> is reflected

corrected

Line 276: at THE stations

corrected

Line 286: in THE infrasound

Corrected

Line 299: coming -> the

Corrected

Line 322: consequent -> following

Corrected

Line 323: streamer -> streamer event

Corrected

Line 328; the decrease -> a decrease

No more in the revised text

Line 348: Expected -> predicted

No more in the revised text

Line 356: Model of -> Modelled

Corrected

Line 358: insert „(maximum height)" after „turning height"

Inserted

Figs 5-8, 11: Use the same size.

We agree with the reviewer that figures of the same size would look better. To the best of our knowledge, the InfraGa/GeoAc version used in the study (downloaded in July/August 2023) does not enable user defined limits of the plotted area, latitude – longitude limits of the plot. The plotted area is set automatically. Therefore, we apologize, we are not able to meet this requirement.

L 533: expected -> modelled

This part of the conclusions was completely rewritten.

L 474 In the conclusion the azimuth distribution is mentioned. This need to be discussed with Fig 14.

done

Reviewer 2

I thank the authors for their substantial revision effort and for addressing most of the reviewers' comments. Especially the methods section has benefitted from the revision. The methodology and the data base are described and explained in more detail now.

However, the revised infrasound-related analysis has some shortcomings and poses new questions. Why do the authors still focus on such few (three) streamer events despite having identified eight events in the considered period (table 1)? Especially if these few events do not lead to promising results, why not looking at the other five events in order to get some more (statistically) significant results? Now the conclusions are (reasonably) relatively vague. Also the figures for analysing the detection parameters are not very meaningful in terms of the research question, i.e. trends (more cases, more/longer time frames before & after streamer events) or significant statistics. I acknowledge that the authors incorporated infrasound propagation modelling, but what is the actual motivation or aim to do so? It is in my opinion not sufficient to simply insert (almost) the same figure with raytracing results four times (differing by the source location). For supporting the analysis quantitatively, e.g. estimates of the attenuation during the propagation could have helped to assess and compare the detected amplitudes.

Additional comments and questions are specified below. Overall, I stick to the impression that the authors have an interesting (and ambitious) concept of using infrasound and gravity wave observation for finding indicators for streamer events. Nevertheless, even when the results are not as convincing as hoped for, the concept itself has to be convincing enough in order to be acceptable for publication in Atmospheric Measurement Techniques. After the first revision, however, I believe that the analysis is in essence not more comprehensive than before the revision and the methods used are not significant enough to convince the reader of this concept.

A:

Thank you for your insightful comment on our paper. I'd like to take this opportunity to elaborate on the concept and scope of our study.

Our primary project LISA related to AEOLUS observations investigates streamer events within a selected timeframe, specifically encompassing about eight streamer periods. Due to the constraints of the project timeframe, we have chosen to focus on three particular events characterized by calm periods before and after each event, as recommended by the second reviewer. Consequently, our study is based on these three events as case studies.

The limited duration available for our research necessitated this focused approach. While we acknowledge that examining a longer period and incorporating more events could potentially enhance our findings, the lack of AEOLUS measurement data—crucial for identifying streamer events—restricted us to the data at hand.

A significant objective of our paper is to evaluate the effectiveness of ground-based infrasound and ionospheric Doppler sounding in detecting or reflecting changes during streamer events in the middle atmosphere. Our findings indicate that microbarograph infrasound measurements picked at an arbitrary location are not reliable predictors of streamer events due to the fact that the disturbance at the tropopause/lower stratosphere formed by the effect of the streamer event influences infrasound arrivals in a limited region in the neighborhood of the signal source. The exact location of that region varies from event to event depending on the position of the tropopause/lower stratosphere disturbance. Conversely, our study demonstrates that Doppler sounding can reveal certain effects during streamer events, providing valuable insights.

Thank you once again for your comment, and we hope this explanation clarifies the rationale and objectives of our research.

Specific and technical comments:

Line 103: "Our study will also focus on the utilization of Doppler…"

Done

Lines 148-152: Again, this seems to be inconsistent with Fig. 1 and line 181. I understood from the response of the authors that this is an exceptionally strong event, but there is another streamer event somewhere else (L149), i.e. over the Pacific/US coast in this case.

We understand that other streamers can be identified. But we focused mainly on the Atlantic sector.

Line 157/158: add "i.e. beyond 50° N / <300 DU" (large spatial size, low TO3 concentration)

added

Table 1, second line: "Detection frequency range" – how many frequency bands are used in this PMCC configuration? Which PMCC version?

The number of frequency bands was added into Table 2. DTK-GPMCC software is used for processing of infrasound detections.

Line 241: Vertical profiles

Corrected

Line 244: in the lower stratosphere

No more in the revised text

Line 245: What's the motivation to incorporate infrasound propagation modelling?

The explanation was added in section 2. Data and methods

Line 351-353: Or is it because the amplitudes are too low? What is the distance and the expected transmission loss at the respective frequencies? Long-range propagation through the thermospheric waveguide is rather unlikely.

We thank to the reviewer for this comment. We performed single azimuth simulations from the three fictious point sources to PVCI and WBCI arrays to obtain info about attenuation of the 0.2 Hz signals. For arrivals to WBCI, the simulation was also run for the frequency 0.04 Hz. The amplitude loss of the 0.2 Hz signal propagating in the thermospheric waveguide is higher than 100 dB at the distance of 2000 km; the amplitude loss of the 0.04 Hz signal is 60 to 80 dB relative to the amplitude at the distance of 1 km from the source. The amplitude of the 0.04 Hz signal observed at WBCI is 0.2 Pa. It would require unlikely high source amplitudes of the order of ~100 to 1000 Pa. We agree with the reviewer that thermospheric ducting is unlikely. The text was modified.

Fig. 5-8: Four single figures showing propagation modelling results are not necessary (combine or put into supplements).

The number of figures showing raytracing results was reduced in the text. The other figures are available in supplementary materials.

Line 425: The trace velocities mentioned of >0.377 km/s around line 403 rather indicate thermospheric (or at least upper stratospheric) propagation, following Lonzaga (2015), not tropospheric.

We thank to the reviewer for pointing out our mistake. The text was corrected.

Line 556: and what about the amplitude in that case? I expect it to be lower for thermospheric ducting

Please see answer to the comment 10). The text was corrected.

Line 570: Is this conclusion based on the slightly larger GW amplitudes (section 3.2.1)? That conclusion is not fully clear at this point, it lacks a more detailed discussion towards this (amplitudes -> propagation).

We have modified text in the section 3.2.1.

The former figure 4 and 5 were a useful step towards a systematic analysis of events and calm periods. In case of another revision, they (or similar systematic analyses) should be included again, with a detailed discussion.

Based on the reviewers' recommendations in revision 1, which we appreciated and agreed with we selected those streamer events for further analysis that are immediately followed or preceded by calm periods. In the available dataset, only two appropriate periods occurred, namely the period from 3 November to 25 November 2020 with two streamer events separated by a quiet period and the period from 28 February to 25 March 2021 when a streamer event was preceded and followed by calm periods. A statistical study based on only three streamer events and three calm periods that occurred in different seasons of the year on the top of that is in our opinion not valid. We therefore present two separate case studies in the paper.