We thank to both reviewers for constructive reviews. We modified the manuscript following their comments. In addition, we changed the format of references to be consistent with the format used in the ACP journal.

Reviewer #1

We thank the reviewer for the careful reading of the manuscript and improving comments.

This study reported a unique observation of co-seismic infrasound wave signature at an unprecedented high altitude of the ionosphere by the continuous Doppler sounding systems (CDSS) in Czechia and Slovakia. This paper is novel in two aspects: (1) Co-seismic infrasound waves can propagate upward to the altitude of ionospheric F layer. (2) The ionospheric perturbations observed by Doppler sounder is an effective characterization of the response to earthquakes. The authors confirmed the presence of infrasound wave using ray tracing simulation, and also revealed the characteristics and underlying mechanisms of the upward propagation of infrasound waves generated by the earthquake. This paper should be published after considering the follow comments.

1. The authors should emphasize the significance of the findings of this paper in abstract and conclusion. e.g., promoting community's understanding of seismic wave propagation, or providing new results or methods for monitoring atmospheric and ionospheric responses to the earthquake.

   Thank you for this comment. We modified/extended the abstract and Conclusion in this respect. See lines 16, 21-22, 31-34 in the abstract and lines 422-423 in the Conclusions.

2. Line 315: The authors indicated the reasonableness of the observed infrasound wave amplitude by the approximately similar time and frequency between the observed and simulated waveforms. However, there is obvious phase delay between them. Please provide some explanation to clarify.

   We modified the related text to be clear (lines 318-319, 323). Basically, the probable reason for the phase delay between the observation and simulation is the same as for the slightly different amplitudes and frequencies. Namely, the real trajectories of infrasound may be somewhat longer than in simulation due to the neutral winds and non-zero zenith angle of propagation. The location of the seismometer used in the simulation may be not the exact location of the origin of the observed waves.

3. I do not quite understand the purpose of checking the measurements of OH airglow emissions. As the author indicated, the high-frequency infrasound wave also presents in the lower altitudes but cannot reach the ionospheric F layer height, but they showed no significant changes in the OH temperature related to infrasound waves.

There are two reasons why we checked the measurements of OH airglow emissions. First, they could provide a useful and complementary information on infrasound propagation from the altitude of the upper mesosphere. Second and importantly, a couple of studies suggested that OH airglow could be used to trace co-seismic signals in the upper mesosphere and lower thermosphere region (see the modified text and added references). However, most of these studies rely on modelling results and observational evidence is still missing. Therefore, we consider it worthwhile to use co-located observations by CDSS and OH airglow instruments (relatively unique worldwide) and present the results regardless if they are positive or negative. Knowing that waves were detected on the ground and in the ionosphere (F2 layer) we used this opportunity to check and discuss the possibility of their detection (including the necessary temporal resolution and signal to noise ratio, SNR) at the emission *height of the OH airglow (around 86 km). We hope that future instruments with improved SNR and temporal resolution (currently in development/testing) could detect the co-seismic waves at the OH airglow altitudes. See also the response to comment 1 by the second reviewer.*

We modified/extended the related text, including additional references, to better explain why it is useful to present this attempt to detect co-seismic waves in OH airglow data.

4. The authors should mention the limitation in the discussion/conclusion that the detection of co-seismic infrasound waves by Doppler sounder needs a nearby ionosonde to provide the electron density gradient at the reflection height.

We added this information to Conclusions. See the last sentence.

Reviewer #2

We thank the reviewer for the thoughtful and useful comments.

Comment on the revised manuscript entitled "Co-seismic infrasound in the ionosphere over Central Europe from the M8.8 Kamchatka 2025 earthquake observed by Doppler sounding at record heights" by Chum et al.

This manuscript presents observations of co-seismic ionospheric disturbances associated with the 29 July 2025 M8.8 Kamchatka earthquake using continuous Doppler sounding systems in Czechia and Slovakia. The authors report the detection of ionospheric signatures at reflection heights of approximately 340 km, which they claim to represent the highest-altitude observation of co-seismic infrasound by HF Doppler sounding to date. The study combines Doppler observations, digisonde measurements, ray-tracing calculations, and full-wave numerical simulations to investigate the propagation and attenuation of earthquake-generated infrasound waves.

The topic is timely and relevant to the communities studying lithosphere--atmosphere--ionosphere coupling and coseismic ionospheric disturbances. The dataset is valuable, and the reported observations are potentially significant. In particular, the apparent detection of co-seismic infrasound at unusually high altitudes and the discussion of frequency-dependent attenuation during upward propagation are interesting and worthy of publication.

However, I would ask the authors to carefully consider the following points before the manuscript is accepted for publication.

1. Discussion of the OH airglow observations

In Lines 353--358, the authors state that "The time resolution of OH temperature measurement by GRIPS is 15 s. This sampling frequency … using the continuous Wavelet Transform (not shown)." This sentences showed that the temporal resolution of the OH airglow observations is insufficient to resolve the higher-frequency perturbations expected in association with the earthquake. If so, the meanings for discussing the OH airglow results is not clear.

OH airglow observations would be highly valuable if they were capable of detecting temporal variations similar to those reported by Snively et al. (2013). However, if the time resolution used in this study is unable to resolve such variations, the scientific value of presenting and discussing these observations becomes questionable. The authors should therefore provide a stronger justification for including the OH airglow results or consider removing this section from the manuscript.

You are right the temporal resolution of GRIPS is insufficient to detect 0.05 Hz waves that are expected to dominate at the OH emission layers, but we also searched for a possible detection of weaker 0.005 Hz waves (at this altitude) as described, but without success. We would like to emphasize that joint measurements by HF Doppler sounding and OH airglow instruments are relatively unique worldwide and we believe that an experimental data showing the limitations of current instruments are useful. We hope that future instruments with improved SNR and temporal resolution (currently in development/testing) could detect the co-seismic waves at the OH airglow altitudes.

Unlike the next comment (suggestion to remove Swarm description), we consider it useful to report an attempt to detect co-seismic signatures in OH data, even though this attempt was more or less negative, because of a number of modelling studies devoted to this topic. Another reason for showing and discussing the OH data is that Inchin et al. (2020, 2022) predicted an airglow depletion (similar to the observed drop in OH intensity) around the epicenter due to nonlinear interaction of atmospheric waves. As the other stations observing OH airglow did not observe this drop and our observation is far from epicenter region, the co-seismic origin of the observed drop is questionable as we discuss.

We modified/extended the related text, including additional references, to better explain why it is useful to present this attempt to detect co-seismic waves in OH airglow data.

2. Discussion of the Swarm satellite data

The inclusion of the Swarm satellite analysis also appears unnecessary. It should have been evident from a preliminary inspection of the satellite trajectories that no Swarm spacecraft passed sufficiently close to the region of interest during the period investigated in this study. Consequently, the absence of relevant observations does not provide meaningful information regarding the phenomenon under investigation.

Therefore, there appears to be little justification for describing the Swarm data in either the Data section or the Results section. Unless these observations contribute directly to the scientific objectives of the study, I recommend removing them in order to improve the focus and conciseness of the manuscript.

Thank you for this feedback. We removed the parts related to Swarm in the revised text.

If the authors wish to incorporate additional datasets in addition to the Doppler sounding observations, GNSS-TEC measurements would likely provide more useful information. In fact, TEC observations are already discussed in the Introduction as a powerful tool for investigating ionospheric disturbances associated with seismic events. Compared with the OH airglow and Swarm datasets presented here, TEC measurements would be expected to provide more direct information on the spatial extent, propagation characteristics, and amplitude of the ionospheric response. Therefore, I encourage the authors to consider incorporating TEC observations, if available, rather than presenting datasets that do not substantially constrain the interpretation of the event.

We haven't detected any co-seismic GNSS-TEC changes over central Europe neither in TEC values or in the rate of change of TEC values, which could capture smaller scale perturbations. We now mention it briefly in the text (lines 442-444). We also remind that the main focus of the paper is the comparison of simulated (expected) air particle velocities with the observed values at specific altitudes, which is inherently difficult when using TEC measurements due to the integration of plasma densities along the line of sight to the GNSS satellites.

3. Interpretation of Figure 2

According to Equation (2), the plasma vertical velocity, w_p, should be smaller than the neutral vertical velocity, w. However, Figure 2 appears to show the opposite relationship, with w being smaller than w_p. The reason for this apparent discrepancy is unclear. The authors should explain why the relationship shown in Figure 2 differs from that expected from Equation (2), or clarify whether there is a misunderstanding regarding the definition, derivation, or presentation of these quantities.

This is a misunderstanding. As written in the text (lines 219, 228), the neutral vertical velocity was calculated by equation (3) [not by equation (2)] to account for the compressional term, which also contributes to the observed Doppler shift. However, only advective motion is related to w. It is the last term in equation (3), which makes w smaller than w_p. See also the text around equation (3) and the last sentence of the first paragraph in the Discussion section.

To avoid a confusion, we now specify the used equations also in the Figure 2 caption. In addition, we now emphasize that different scales are used on right and left axes for w and w_p, respectively.

In addition, Figure 2(c) contains several different quantities, including the observed plasma vertical velocity (w_p), the neutral velocity (w) derived from the observations, the numerically simulated neutral velocity, and the result obtained over Slovakia, which is shown as a green dashed line. As a result, the figure is rather crowded and difficult to interpret.

In particular, the green dashed line is difficult to distinguish from the other curves. I recommend improving the figure by using more clearly distinguishable colours, line styles, and/or line widths, and by enhancing the overall readability of the plot. Since the scientific interpretation relies heavily on comparisons among these datasets, the figure should be redesigned so that all curves can be readily distinguished by the reader.

We use the blue dashed lines instead of the green lines now. In addition, the dashed lines are also thicker now.