Dear Referee,

On behalf of all co-authors, I would like to thank the Referee for the careful review of our manuscript.

Review

Review of "Deducing spatial characteristics of global thunderstorm activity using the observed Schumann resonance frequencies" by Koloskov et al. The paper sets a valuable goal, namely to provide comprehensive formulas for determining the effective source–observer distance and the spatial extent of the area occupied by global thunderstorm activity based on Schumann resonance (SR) frequency observations. I agree with the authors that the combined use of the first and second SR modes, or magnetic and electric field measurements, has unexplored potential in the study of these important characteristics of global lightning activity. However, I see several substantive issues in the manuscript that need to be addressed, which are related to the presentation and contextualization of the results (see below). Therefore my recommendation is that the manuscript should be returned to the authors for major revision.

Recommendation: Return the manuscript to the Authors for major revision.

Substantive issues:

(1) Distance dependence of the peak frequencies

In Section 2, the authors write the following:

"The nodal zone is also asymmetric with respect to the distance axis. Because the resonance peak vanishes in the nodal zone, the frequency experiences a discontinuity when crossing the 10 Mm distance. As the distance increases from D < 10 Mm to the D > 10 Mm, the peak frequency of the corresponding mode initially decreases. Then, the resonant peak disappears at D = 10 Mm. Afterwards, the maximum of intensity reappears at higher frequencies. The reappeared resonant peak gradually drifts to lower frequencies with growing distance D > 10 Mm."

However, the described distance dependence of the peak frequencies is not consistent with frequency-distance curves presented in earlier studies (see, for example, Figure 2 of Satori et al., 2024 for the electric field component and Figure 3 of Kulak et al., 2006 for the magnetic field component). My expectation is (also based on Figure 1 of the manuscript) that the frequencies increase as the source moves toward the nodal zone, where they drop down and then begin to increase again. The authors need to clarify this important issue.

Apparently, there is confusion here. The cited fragment of the article refers to panels (a) and (d) in Fig. 1. The maps shown in the figure are parts of more general maps that have already become classics in SR research; see, for example, Fig. 4.16 in the book by Nickolaenko and Hayakawa (2002). In particular, Fig. 1a of the article is equivalent to Fig. 4.9 in this book.

Therefore, the statement about doubts regarding our results seems unexpected.

Diurnal variations in the SR peak frequency are known for long time and were discussed extensively in the literature. We show a page from the monograph by Galejs J. (1972) Terrestrial propagation of long electromagnetic waves. Pergamon Press, New York, 362 pp., which demonstrates the impact of the size of the region occupied by thunderstorms on the outline of diurnal variations in the first peak frequency of SR in the vertical electric field.

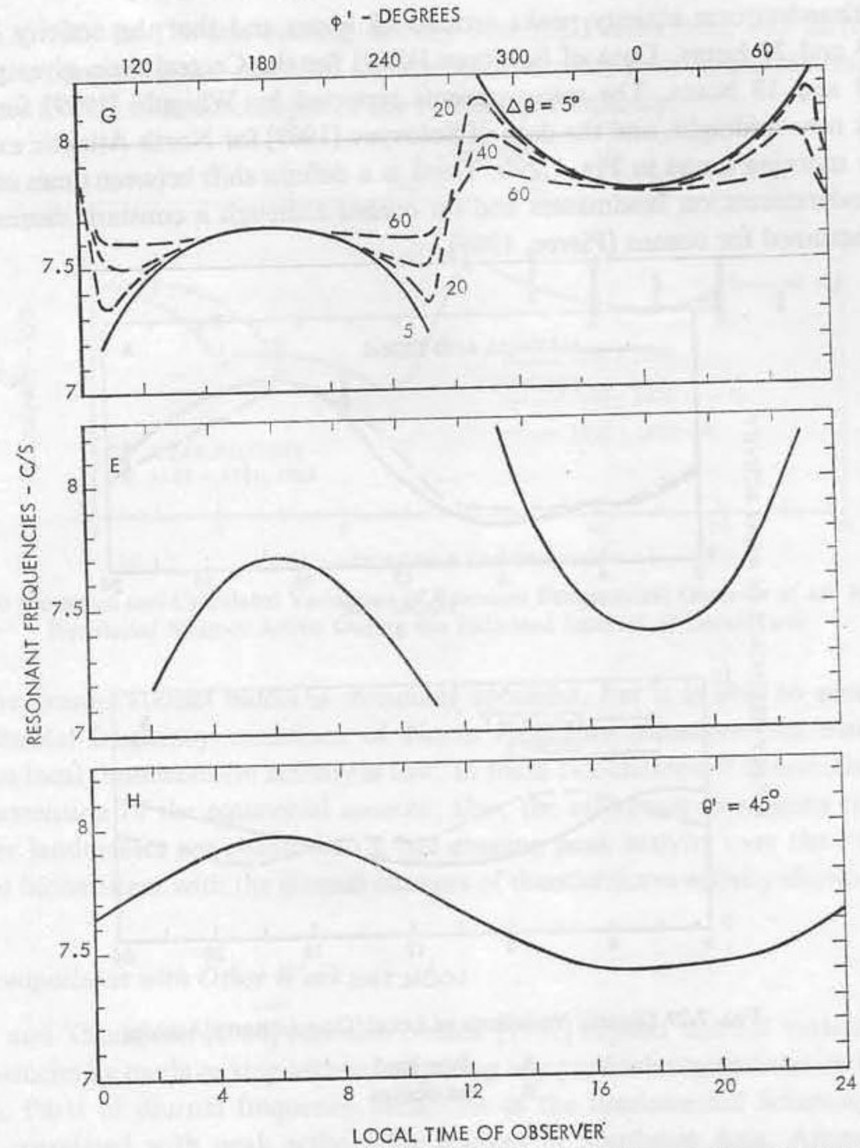

FIG. 7.28 Diurnal Variations of Resonant Frequencies; Equatorial Sources at Sunset Time (18 hrs or $\varphi' = 0$); Observer at Polar Angle θ'. $G-E_r$ of a Source Distributed Over $\Delta\theta$; E, $H-E_r$ and H_φ of Localized Sources

observed in several measurements [Balser and Wagner, 1962b; Nelson, 1967]; but there is also a comparable frequency change around midnight which is contrary to observations. These calculations are based on the assumption that the sources are symmetrical with respect to the day and night boundary at sunset.

As one may observe, the cited fragment of manuscript in Review describes the variations of the first peak frequency in the vertical electric field spectrum rather accurately.

Concerning the magnetic field component, in addition to the |*H*(*f*, *D*)| distribution map in the aforementioned book (2002), there are two papers containing such map:

Nickolaenko A.P., (2015) "Deducing Parameters of the World's Thunderstorms from the Schumann Resonance Records (Once Again About the Point Source Model in the Schumann Resonance Studies), Telecommunications and Radio Engineering, 74 (2):147-161, 2015.
and
Nickolaenko A. P., A.V. Koloskov, M. Hayakawa, Yu.M. Yampolski, O.V. Budanov, V.E. Korepanov (2015), 11-year solar cycle in Schumann resonance data as observed in Antarctica, Sun and Geosphere, 10, No. 1, 39–49 2015.

These papers address the monotonic increase of the first peak frequency in the horizontal magnetic field spectrum relevant to increase of the source-observer distance.

(2) The lack of units in Figure 1.
Figure 1 does not indicate whether the values shown on the colormap are calibrated or in arbitrary units. For the sake of comparability with other works, it would be benefitial to display calibrated values.

Thank you for the comment! We think that particular relief of the 2D maps is important here, and it remains preserved regardless the particular units of the field amplitude. Nevertheless, **we will add the information to the text** that the specific field amplitudes in the unit frequency band are measured in mV/m and μA/m, provided that the source current moment is equal to 10^8 A·m.

(3) The impact of the geographical location of the station on the calibration curves The authors determine the calibration curves for a single station (Vernadsky), but do not discuss the important aspect of how the geographical location of the station affects their results. How different would the calibration curves be for other stations?
Could these calibration curves be used as an approximation at other stations? These are important questions that the manuscript should address.
This is particularly important in light of the fact that, in my opinion, the authors provided the calibration curve for the Nagycenk station in equations (3a) and (3b) and not for the Vernadsky station. This also needs to be clarified.

The objective of our study was demonstrating the fundamental possibility of simultaneous estimating the distance to the center of the field source and the source's size. This possibility is relevant to an arbitrary observer position, which might modify the range of variations in the source – observer distance.

We had in mind the Ukrainian Antarctic Station, which provides now the longest continuous SR records. This is why we applied its coordinates in our computations. At present, the SR monitoring is usually performed using two orthogonal horizontal magnetic field components. In previous publications, the calibration curves were given for the diurnal frequency range in the vertical electric field recorded at the Nagycenk Observatory. Of course, similar computations might be made for an arbitrary field site.

(4) The use of the second electric mode
The manuscript does not discuss an important limitation of the use of the 2nd electric SR mode, namely that although the peak frequency does indeed vary monotonically with source-observer distance in the distance range shown in Figure 3, there are two nodal zones outside this range (see Sátori et al., 2024, Figure 2).
This means that if there are sources in these regions, they can significantly affect the observed peak frequencies.

Of course, the use of higher modes is linked to a narrower range of distances. However, such a possibility is still better than nothing. Furthermore, the limitation is less important to the highlatitude observatories. Our Fig. 1c demonstrates that the distance to the source is reliably determined in the 7–12 Mm band when using the second SR mode frequency.

It is clearly that simultaneous monitoring of SR in the vertical electric and two horizontal magnetic fields suggests broader possibilities. However, to our knowledge, such three-component observations are not available at present.

We consider the reference to the work by Sátori et al. (2024) inappropriate, as it did not address the problem of distance finding being one of our principal goals. The comment by Reviewer does not exclude the possibility of simultaneous estimating the effective size of the region occupied by global thunderstorms and the distance to its center.

(5) The use of log-log scale for displaying and fitting the calibration curves
The authors use a log-log scale to display and fit the calibration curves (unlike in Nickolaenko and Rabinowicz, 1995), which makes it difficult to perform a quick estimation for the expected frequency changes based on the figures. I believe it would be beneficial to present them on linear scales as well, at least in the Appendix.

Our goal was in obtaining the best functional approximation of the calculated data. The resulting formulas correspond to very high determination coefficients *R*. We see no problems in their practical application, since the natural logarithm function is built-in into any calculator.
Switching to nomograms with linear axes instead of formal description seems less practical to us, since any reader might build the necessary plots in arbitrary coordinates by using formulas.

(6) Figure captions
The figure captions are very brief and do not describe every detail of the figure. It should be possible to interpret the figures without reading the main text.

We do not share this opinion. It would be preferable for a reader to pick information from the complete description in the text of the paper, rather than from a "digest" below the figure. Besides, implementing the Reviewer's suggestion would lead to repetitions and an unjustified increase in the volume of the article.

Minor issues:
Line 16 There is a "-" after "distance" that should be deleted.
Will be done, thank you!
Line 34 I think "form" should be replaced by "forming".
Will be done, thank you!
Line 49-52 I think it would be appropriate to cite Kulak et al. (2006) somewhere here.
Declined, see below.
Line 133 The authors state that the Vernadsky station "hosts the longest series of SR observations", but this is simply not true (see Nagycenk observations which started in 1993). The authors could write "hosts one of the longest series of SR observations".
We do not agree. The UAS data cover the period from 2002 to 2024 (22years). The data from NCK cover period from 1993 to 2009 (16 years)
Line 191 There is an unnecessary comma after "size W".
Will be done, thank you!

Many thanks for particular remarks!

References
Kulak, A., Mlynarczyk, J., Zieba, S., Micek, S., & Nieckarz, Z. (2006). Studies of ELF propagation in the spherical shell cavity using a field decomposition method based on asymmetry of Schumann

resonance curves. Journal of Geophysical Research, 111(A10), A10304. https://doi.org/10.1029/2005JA011429

Nickolaenko, A. P., & Rabinowicz, L. M. (1995). Study of annual changes of global lightning distribution and frequency variations of the first Schumann resonance mode. Journal of Atmospheric and Solar-Terrestrial Physics, 57(11), 1345–1348. https://doi.org/10.1016/0021-9169(94)00114-4

Sátori, G., Bozóki, T., Williams, E., Prácser, E., Herein, M., Albrecht, R.I., and Beltran, R.P. (2024). How do Schumann resonance frequency changes in the vertical electric field component reflect global lightning dynamics at different time scales? Journal of Geophysical Research: Atmospheres, 129, e2024JD041455. https://doi.org/10.1029/2024JD041455

We are grateful to Referee for reminding us of these works. After some consideration, we decided to follow some of these suggestions.

Unfortunately, the first work has raised serious doubts during its reviewing by NAP. The title of the work is misleading. Besides, the resonance curve of any real circuit is asymmetric in principle. Thus, the electric (parallel) resonance has a frequency response reaching a constant at the zero frequency, and it decreases to zero when the frequency increases to infinity. The magnetic (series resonance) is also asymmetric, as its frequency characteristic turns to 0 over a finite interval from $f = f_{res}$ to $f = 0$, and over the infinite interval from f_{res} to infinity.

The cited work did not demonstrate how to resolve this natural asymmetry from that authors intend to exploit in the SR studies.

We did not cite the second work owing to editorial demand to limit the self-citation. Fortunately, the main results of this work were included in the book by Nickolaenko and Hayakawa (2002). So, we follow the Reviewer's suggestion with pleasure.

We also mentioned the third work in the text. Thank you!

Concluding our Response, we sincerely thank the Referee for her kind attention, valuable time and efforts in helping us to improve the manuscript.

THANK YOU!