Second review of “Deducing spatial characteristics of global thunderstorm activity using the observed Schumann resonance frequencies” by Koloskov et al.

The authors have decided not to incorporate changes into the manuscript for the vast majority of my comments; however, I believe that in some cases, the changes/additions would be important. Below, I will respond only to such points.

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

On behalf of all co-authors, we express our deep gratitude for your thorough second review and valuable guidance. We fully appreciate the Editor’s and Reviewer’s point that important physical clarifications presented in our previous responses must be transparently incorporated into the manuscript itself for the benefit of all readers.

In this revised version, we have added the necessary explanations directly into the manuscript. At the same time, we made sure to preserve the core purpose of our paper, which is to introduce a simple and efficient dual-parameter retrieval method. Below is our point-by-point response (in green):

(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.

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.

Yes, I agree that there is confusion here. I didn't mean to question the correctness of Figure 1, but I think the quoted sentence should read as follows (suggested changes highlighted in bold):

"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 ***increases****. Then, the resonant peak disappears at D = 10 Mm. Afterwards, the maximum of intensity reappears at* ***lower*** *frequencies. The reappeared resonant peak gradually drifts to* ***higher*** *frequencies with growing distance D > 10 Mm."*

See also Figure 4.20 in Nickolaenko & Hayakawa (2002).

We agree with the Reviewer's description of how the peak frequency behaves near the nodal line. To make this clear to the reader, we have updated Section 2 of the manuscript. The text now explicitly explains that as the source moves closer to the nodal zone ($D < 10$ Mm), the peak frequency first increases and then drops in the nodal region. As the distance increases beyond $D > 10$ Mm, the peak frequency drifts toward higher values. We have also added a reference to Figure 4.20 from Nickolaenko and Hayakawa (2002) to support this explanation.

(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.

Yes, I definitely agree with the last sentence, and I think you should discuss this aspect in the manuscript.

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.

Okay, but equations (3a) and (3b) given for Vernadsky station in this manuscript are exactly the same as the equations given by Nickoleanko (2024) for Nagycenk (see the 6th paragraph of the Discussion). Is this just a coincidence? Please clarify, not only here but in the manuscript as well.

Thank you for bringing attention to this important point. Equations (3a) and (3b) were obtained using a uniform Earth-ionosphere cavity model, which gives a baseline relationship between the diurnal frequency range and the source size ***W***. Because a uniform resonator model uses a symmetric global structure, this analytical expression provides a good first approximation for both mid-latitude (Nagycenk) and high-latitude (Vernadsky) observatories. To avoid any confusion for the reader, we have added explanatory notes in Section 3 and Section 4 to clearly state the modeling assumptions behind these equations.

(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 high-latitude 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.

Yes, I agree, but you should discuss this in the manuscript.

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.

Yes, I agree, but you should discuss this in the manuscript.

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.

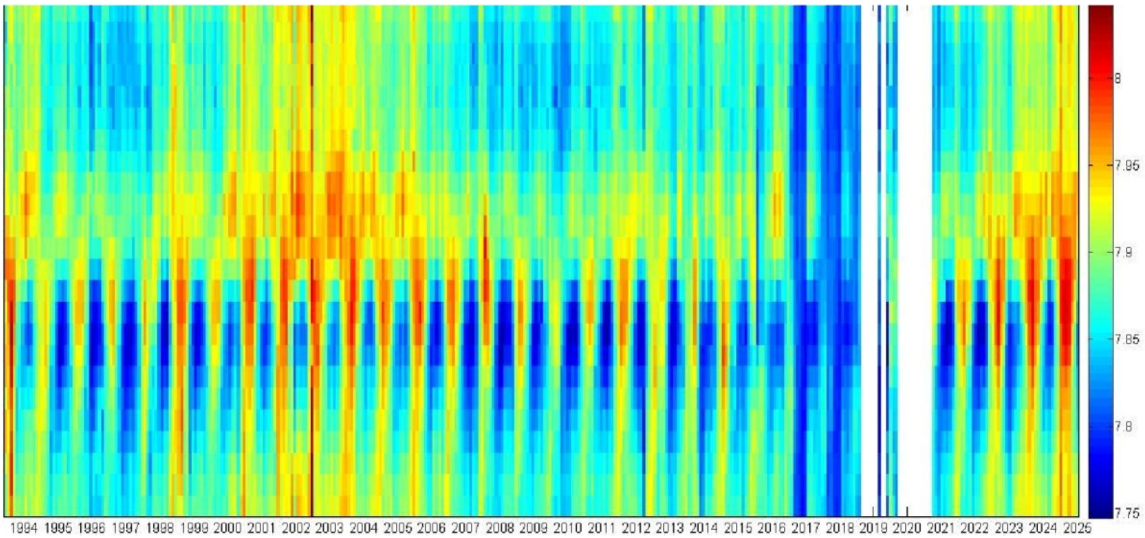
With the reference, I just wanted to point out that there are multiple node zones in the case of the second electrical mode. I didn't want to question the value of the manuscript at all.

Thank you for this valuable comment. We agree with this point and have added a paragraph in Section 2 to address this operational limitation. In the revised text, we clarify that higher-order modes, such as the second electric mode, have secondary nodal zones where the signal amplitude drops significantly, as discussed by Sátori et al. (2024). When multiple global thunderstorm sources are active near these secondary nodes, determining the peak frequency becomes more complex. However, we also note that this issue is less critical for high-latitude sites like the Antarctic station 'Akademik Vernadsky'. For polar observatories, the typical distances to the primary tropical thunderstorm centers remain mostly within the favorable 7–12 Mm window, which stays clear of these secondary nodal regions.

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)

In fact, Nagycenk has been in operation from 1993 to the present; however, there were data quality issues from 2018 until the renovation in 2021 (see the figure below). I still believe that "one of the longest" would be a good compromise.



We sincerely thank the Reviewer for providing the detailed context and the clarifying figure regarding the Nagycenk observational record. We agree with this convincing argument and are grateful for the correction. The text in the manuscript has been updated to state that UAS "Akademik Vernadsky" *"hosts one of the longest series of SR observations worldwide (and the longest continuous record in the polar regions)."*

Once again, we wish to express our deep appreciation to the Reviewer for his thorough analysis and the substantial time devoted to evaluating our work. His insightful comments helped us identify key oversights that we had previously missed, and addressing them has significantly improved the quality and clarity of the manuscript for future readers.

Thank you,

Oleksandr Koloskov

(on behalf of the co-authors)