Manuscript No.: egusphere-2025-1524
Manuscript Title: Variation characteristics of sporadic-E layer in East Asia based on long-term data

Replies to Reviewer 1's Comments:

The paper "Variation characteristics of sporadic-E layer in East Asia" by Zhao et al. deals with a climatological study on the Es occurrence over East Asia based on a significant number of ionosonde stations. Even though I understand that the authors have done a lot of work to obtain their results, at the same time I think that what they shown is not novel (for instance, "At daytime, foEs values are significantly higher than during the nighttime", "the maximum values of foEs in East Asia generally occur in June, while the minimum values typically occur in December. The foEs values in summer are significantly higher than in winter"). It is unclear what their study brings to the ionospheric community that wasn't unknown before.
Reply: Thank you very much for taking the time to review this manuscript and provide valuable feedback. We greatly appreciate the constructive comments and suggestions, which have helped us improve the quality and clarity of our work.

As you rightly pointed out, while the Es layer in East Asia follows the general variation patterns typical of mid-latitude regions, it also exhibits unique regional characteristics. For instance, the intensity center of the Es layer in East Asia is not fixed but migrates with diurnal and seasonal variations. Although long-term variation trends of Es layer intensity differ across locations in this region, areas with higher intensity generally exhibit a downward trend, while those with lower intensity show an upward tendency. The discovery of these new phenomena provides valuable references for studying Es layer formation mechanisms and atmosphere-ionosphere coupling processes.

(1) My major concern is related to Section 2 and specifically to the dataset used to perform the analyses. There is no discussion about how the authors have obtained the foEs values. Are we talking about manual or automatic values? What kind of ionosondes they considered (Digisonde, CADI, IPS-42,⋯..)? What is the time resolution of their data? Does this time resolution change from station to station? The authors should clarify better to the reader this crucial point of their work.
Reply: Thank you for your comments. All Chinese stations employ the domestically produced CPA-4 ionosonde, while Japanese stations utilize the American-made Digisonde digital ionosonde. The temporal resolution of the data used in this study is 1 hour, with both Chinese and Japanese stations maintaining the same resolution. The

data interpretation method employed is manual analysis. A clarification has been provided in Lines 124-128 of the revised manuscript.

(3) Why Figure 2 refers to 20 and not to 21 stations? Do these plots refer to the whole dataset of each ionosonde station? Do these plots show hourly monthly medians?

Reply: Thank you for your comments. Among the 21 stations, Suzhou and Sheshan are geographically proximate. Considering both this spatial overlap and the presentational challenges of displaying 21 subplots, Figure 2 omits the results from Sheshan station. The data presented in the figures and tables of this paper are derived from the complete time periods specified in Table 1, with no data screening applied. Figure 2 displays the annual averages of hourly monthly median values.

(3) line 134, "The maximum monthly average⋯." I am confused, are we talking about medians or averages?

Reply: Thank you for your comments. We sincerely apologize for the confusion caused by the terminology inaccuracy. The correct term should be "monthly median", not "monthly average". We have corrected this terminology error in Line 143-145 of the original text.

(4) lines 147 and line 150, Wakkanai and Suzhou should be highlighted somehow in the figure. Moreover, I am pretty doubtful about the application of the Kriging method to the foEs values, also because from the figures I see many spots that in my opinion are unreal. This is a critical point characterizing Figures 3, 5 and 7.

Reply: Thank you for your comments. We have marked both Sheshan and Suzhou stations in Figure 1. Kriging interpolation is a classical spatial algorithm widely employed in studies of ionospheric and atmospheric distribution characteristics. The apparent distorted patches in the figures are primarily attributable to the rendering effects of the plotting software.

(5) line 156, what do the authors mean with "the average variations of monthly median foEs values"? The same stands for line 181.

Reply: Thank you for your comments. Figure 4 displays the monthly median variation curves of typical stations across different months. Each year's data corresponds to one curve, and we applied a multi-year averaging method to the monthly medians to enhance the generalizability of the patterns.

(6) lines 176-178, " The diurnal drift in the center of Es layer may be affected by environmental factors such as the diurnal variations of background atmosphere and climate." This sentence is too speculative.

Reply: Thank you for your comments. We have revised this statement as requested. The revised statement reads as follows:

The observed diurnal asymmetries in the intensity of the Es in East Asia may result from variations in the dominant controlling factors of foEs across different periods. During daytime, the electron density of the Es is primarily governed by solar radiation, showing significant latitudinal dependence. However, when solar radiation weakens at night, its controlling effect diminishes, allowing the influence of other factors such as tides and gravity waves to become more pronounced. This may be the cause of the diurnal inconsistency in the Es layer intensity center.

(7) lines 200-201, “However, the occurrence of anomalous phenomena in the mid-latitude Es layer during summer poses a challenge to the wind shear theory.” With regard to this, the paper by Haldoupis (2007) that the authors cite partially solves this puzzle.
Reply: Thank you for your comments. We sincerely appreciate your insightful perspective on the seasonal dependence mechanism of the Es layer. It should be noted that this explanation represents only one of several plausible hypotheses and has not yet fully resolved the scientific inquiry, primarily for two reasons: first, the correlation between meteor flux and foEs in the literature shows poor fitting across multiple time periods; second, the scientific validity of directly equating meteor counts with foEs requires further scrutiny.

(8) lines 207-208, “They also predict that the modulation of tides by planetary waves is achieved through nonlinear interference [Xu et al., 2022].” Concerning this topic, this is not the right reference. Consider: Haldoupis and Pancheva (2002), http://dx.doi.org/10.1029/2001JA000212; Haldoupis et al. (2004), http://dx.doi.org/10.1029/2003JA010253; Pignalberi et al. (2015), http://dx.doi.org/10.1016/j.jastp.2014.10.017; Pezzopane et al. (2015), http://dx.doi.org/10.1016/j.jastp.2015.11.010.
Reply: Thank you for your comments. We sincerely appreciate your reminder and have corrected the relevant references in the revised manuscript.

(9) line 209, what do the authors mean with “probability distribution”?
Reply: Thank you for your comments. We sincerely apologize for the confusion caused by unclear expression. The term "probability distribution" here specifically refers to the occurrence probability of foEs values exceeding 5 MHz. We have corrected this statement in Line 223 of the revised manuscript..

(10) line 246, the authors say that "The Es layer in East Asia exhibits a clear negative correlation…" but I wouldn't say that. On the other hand, what the authors write referring to Table 2 highlights that the negative correlation is not as clear as it is claimed here.

Reply: Thank you for your comments. We sincerely apologize for the confusion caused by the unclear expression. The analysis results indicate that foEs exhibits a weak correlation with sunspot numbers, rather than a significant one. We have revised this statement accordingly in the revised manuscript line 258-260.

(11) line 254, "number of sunspots", which one? Monthly? Annual?......

Reply: Thank you for your comments. The sunspot number data used in this study are monthly mean values. A clarification on this issue has been provided in line 266 of the revised manuscript.

(12) the trends shown in Figure 9 in my opinion cannot be considered statistically significant.

Reply: Thank you for your comments. We concur with your perspective. While environmental changes exhibit long-term characteristics, and the observation period covered in this study may not be sufficient to fully capture the long-term variation patterns of foEs, we present these statistically observed phenomena as data-driven findings for readers' reference.

(13) lines 289-290, what do the authors mean with "This overall pattern indicates a negative feedback characteristic."

Reply: Thank you for your comments. Statistical results indicate that regions with high foEs intensity exhibit a long-term downward trend, while areas with low intensity show a long-term upward trend. This pattern is analogous to the "negative feedback circuit" principle in electronics, hence we describe it as possessing negative feedback characteristics.

Minor remarks:

(14) line 17, it is unclear what is the meaning of "global average". The same concern stands for lines 136 and 309.

Reply: Thank you for your comments. The term "global average" here refers to the average value level of foEs on a global scale.

(15) Line 32, consider also the paper by Pietrella et al. (2014), http://dx.doi.org/10.1016/j.asr.2014.03.019

Reply: Thank you for your comments. We have added this reference as requested in the revised manuscript.

(16) lines 57-59, consider also the following papers: Pignalberi et al. (2014), http://dx.doi.org/10.1016/j.jastp.2014.10.017, Pezzopane et al. (2015), http://dx.doi.org/10.1016/j.jastp.2015.11.010

Reply: Thank you for your comments. We have added citations to these two references at the appropriate locations in the revised manuscript.

(17) delete lines 92-93 from "Additionally….", they are unnecessary .

Reply: Thank you for your comments. We have removed this statement from the revised manuscript.

(18) delete lines 102-109 from "Particularly….", they are not unnecessary.

Reply: Thank you for your comments. We have removed this statement from the revised manuscript.

(19) Table 1, switch "longitude" with "latitude".

Reply: Thank you for your reminder. We have completed the corresponding revisions in the revised manuscript.

(20) line 140, when talking about Kriging, those are not the right references to cite. Consider the following ones: Kitanidis PK (1997) Introduction to geostatistics: application to hydrogeology. Cambridge University Press, Cambridge; Matheron G (1963) Principles of geostatistics. Econ Geol 58:1246–1266; Oliver MA, Webster R (1990) Kriging: a method of interpolation for geographical information systems. Int J Geogr Inf Syst 4(3):313–332.

Reply: Thank you for your comments. We have replaced the relevant references in the revised manuscript.

(21) line 226, cite also the paper by Pezzopane et al. (2015) which is present in the References section but not cited in the text.

Reply: Thank you for your comments. We have added a citation to this reference in the revised manuscript.

(22) Table 1, switch "longitude" with "latitude".

Reply: Thank you for your reminder. We have completed the corresponding revisions in the revised manuscript.

(23) line 243, replace “monthely” with “monthly”.

Reply: Thank you for your reminder. We have made the corresponding corrections in the revised manuscript.

(24) line 245, replace “low” with “high”.

Reply: Thank you for your reminder. We have made the corresponding corrections in the revised manuscript.

(25) lines 352-355, the references are not sorted

Reply: Thank you for your reminder. We have made the corresponding corrections in the revised manuscript.

(26) line 362, this reference is cited in the text as 2006.

Reply: Thank you for your reminder. We have made the corresponding corrections in the revised manuscript.

(27) line 399, the reference Pezzopane et al. (2015) is not cited in the text.

Reply: Thank you for your reminder. We have added a citation to this reference in the revised manuscript.

Thank you for your great effort and valuable time spent in reviewing this paper. We sincerely wish that with the careful revision of the paper, the revised manuscript is acceptable for publication.