

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.

-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.

-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?

-line 134, “The maximum monthly average....” I am confused, are we talking about medians or averages?

-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.

-line 156, what do the authors mean with “the average variations of monthly median foEs values”? The same stands for line 181.

-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.

-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.

-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>.

-line 209, what do the authors mean with “probability distribution”?

-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.

- line 254, “number of sunspots”, which one? Monthly? Annual?.....
- the trends shown in Figure 9 in my opinion cannot be considered statistically significant.
- lines 289-290, what do the authors mean with “This overall pattern indicates a negative feedback characteristic.”

Minor remarks:

- line 17, it is unclear what is the meaning of “global average”. The same concern stands for lines 136 and 309.
- line 32, consider also the paper by Pietrella et al. (2014), <http://dx.doi.org/10.1016/j.asr.2014.03.019>
- 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>
- delete lines 92-93 from “Additionally....”, they are unnecessary.
- delete lines 102-109 from “Particularly....”, they are not unnecessary.
- Table 1, switch “longitude” with “latitude”.
- 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.
- line 226, cite also the paper by Pezzopane et al. (2015) which is present in the References section but not cited in the text.
- line 243, replace “monthely” with “monthly”.
- line 245, replace “low” with “high”.
- lines 352-355, the references are not sorted.
- line 362, this reference is cited in the text as 2006.
- line 399, the reference Pezzopane et al. (2015) is not cited in the text.