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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The intensification of metallic layered phenomena above thunderstorms through the modulation of atmospheric tides" *by* Bingkun Yu et al.

Anonymous Referee #2

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This is an interesting study which combines and extends two previous works by the first author, one of which linked lightening to Es observations, the 2nd linked lightening to neutral Na observations. Here they combine the three datasets and additionally, use a Na model. It is potentially an intellectual advance over previous efforts and relevant to ACP readers. My main concern, and it is a substantive one, is that the model results are very inadequate and not convincing. They do not yet overcome the skepticism that has been presented in the literature (which was not cited by the authors) concerning the reality of these kinds of observations. My overall evaluation of this work is that it holds promise, but publication is premature.

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Discussion paper

To show that the enhancement is due to tides, there are two things I believe are needed- both require a significantly expanded modeling/diagnostic effort.

First, I would first need to see a case without tides for comparison. Then a difference field to show the effect. As it stands Figures 3b-d look too similar to discern substantive differences. If anything, it appears that Figure 3d shows a weaker effect. Thus Line 7 on page 6 seems to be incorrect. In which case, this would argue against the proposed mechanism. If they can better document that tides do cause the Na enhancement, then they need to show what term in their complicated equation is the determining factor. If it is vertical wind, then they need to compare that amongst their various models. Finally, how does this Na variation link lightening to electron density or Na+? They say on page 4 that their model calculates Na+ and e-. In which case, since the ultimate goal of this exercise is to show sporadic E, they need to present the variation of these charged species with respect to the differing tidal inputs.

The second overall obstacle to be overcome are the arguments of Haldoupis in his 2018 review in J.Atm Solar Terr Phys., vol 172, pp 117-121. This is a paper which is not cited by the authors, but needs to be addressed. He is skeptical of the causal link between lightening and Es and a key reason is the time delay. Quoting the authors' previous work he questions what mechanism could produce such an enhancement after such long time delays (34 hours for Yu et al, 2015; 19 hours for Yu et al., 2017). Now I recognize that Haldoupis is not addressing tides, but rather gravity waves. Nonetheless I believe his criticisms are relevant here. Specifically, what about their model produces the Na enhancements at the indicated time? Why 17-18 hours? Superficially, this enhancement time does appear somewhat close to the 19 hour sodium enhancement seen in Yu 2017. But then how does this relate to the 34 hour enhancement? Haldoupis is very concerned that the reported time scales lack physical significance. I share those concerns and believe that the authors need to respond to these arguments.

Other concerns

1. I am confused as to what is the input to versus output from their model. They state on page 4, line 20, that “Na+ and e- are calculated from the solution of ….” But then on page 5 lines 18-19, that “profiles … are input…”. I do not understand how these can both be simultaneously true.

2. Their discussion section does not convince me that tides are the root cause. In other words, why should tides, a global resonance phenomenon, vary due to localized thunderstorm activity over East Asia? Reading the discussion, I’m more inclined to think they are arguing for gravity waves; however, Haldoupis has already rejected these arguments.

3. I do not completely understand Figure 2. For the top panels, what are the grey lines- what are the units? They have 4 colors plotted, but only 3 axes.

4. For the bottom panel, they show lightening strokes. But the axis says “hours after lightning”. How is this defined? What is time zero?

5. What local time corresponds to t=0 in their model? I am presuming they are doing a fully diurnally resolved calculation (otherwise, its validity would be questionable). So does the local matter as an uncertainty? What happens if they change it?

6. I do not see how Figure 4 helps to understand their mechanism. It’s a cartoon- little more. Either enhance the figure (and the argument) or delete it.

7. I know this data has been presented before but I am unable to find out how the 197 nights of lightening data are distributed with respect to season. Can they elaborate? They mention the month of July on page 5 line 12. Certainly sporadic E has a strong seasonality. I would expect, as part of model validation, that they demonstrate that they can reproduce this seasonality.

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