

Interactive comment on “Solar eclipse induced perturbations at mid-latitude during the 21 August 2017 event” by Bolarinwa J. Adekoya et al.

Anonymous Referee #2

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The manuscript is a good attempt to use NmF2, hmF2, B0, B1 and H to analyse ionospheric perturbation of solar eclipse. However, it is not well organized and written. In my opinion, the paper only can be reviewed again after major revisions. My specific suggestions are shown below.

Line 15, please give a brief introduction to “GIRO database”, or at least give the full name of GIRO, otherwise it is difficult to know what kind of ionospheric parameters are used in your research.

Line 15, it is weird to use “percentage obscuration”. In my opinion, the percentage of obscuration or the obscuration percentage is better. Similar to Line 211 and Line 213, there are “percentage concentration of the components” and “percentage deviation”

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Line 22, “Need for IRI model to capture eclipse caused perturbation”, it is not a complete sentence. Further, line 255-267, the authors said “IRI model doesn’t capture the conditions of the ionosphere during solar eclipse”, but didn’t show any figure or table to support this judge. And I don’t think IRI is a good tool to study ionospheric variations during solar eclipse.

Line 78-79, the authors said “The control day value is the mean of the values obtained on respective days . . .” Specifically, which days did you choose as the control day? Was there geomagnetic storms in that period of time? Did you get the mean of the values by weighting?

Line 241-242, the authors said “The only exception . . . at Millstone . . . H versus B0 . . .” however, it is clear that R is also low for the two figures of IDAHO.

Figures 1 and 2, for hmF2, scale height, bottomside, the variations of them are not very clear, especially at the stations of Eglin AFB, Boulder and Millstone Hill. I mean the noise is too large to get the valuable information. So it is a little far-fetched to draw your conclusion in “3 Result and Discussion”.

Line 273-276, as the authors said, “ionospheric F2 parameters (NmF2 and hmF2), the bottomside profile thickness (B0) and shape (B1) parameters of electron density and the plasma scale height (H), which are not often used for eclipse study”, so have you considered that why these parameters are seldom used in eclipse study? I guess that is because the useful information is probably covered by the noise, especially for such parameters as hmF2, B0, B1 and H.

Figure 3, how did you get this figure? I mean, for a certain electron density profile, there is only one NmF2 and hmF2. You know, NmF2 is F2 layer peak electron density and hmF2 is F2-layer peak density height. But in figure 3, it is very confusing that DNmF2 is varying with the change of Dhmf2. I guess you mean Ne and corresponding height. Maybe my understanding is wrong, Please explain this further for helping readers understand this clearly.

In abstract and conclusion, the authors said “predicting one another”. However, in the body of this manuscript, I didn’t find which parameter is predicted. More importantly, the correlation between these parameters is not strong enough to predict each other. So it is not proper to judge that “Hence their relationship in predicting one another is established” If the authors want to prove that these parameters are predictable, they should provide some supporting figures or tables, instead of a very indiscreet sentence.

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