

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

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**GENERAL RESPONSE** We thank the reviewer for the useful and supportive corrections. We believe that all suggestions made by the reviewer have been considered accordingly in this revised edition of the manuscript. Major corrections have been effected accordingly, and are highlighted (color red) in the manuscript text. We have modified the manuscript accordingly, and the detailed corrections are listed below point by point:

Major concerns: Comment 1 – 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

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Response to Comment 1 Ĩij The GIRO means Global Ionospheric Radio Observatory database. This has been corrected in the manuscript (see page 1, line 15.

Comment 2 Ĩc 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”

Response to Comment 2 Ĩij The corrections have been effected now in lines 15 and 213

Comment 3 Ĩc 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

Response to Comment 3 Ĩij All IRI related statements in the manuscript have been deleted as suggested by Reviewer 1.

Comment 4 Ĩc 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?

Response to Comment 4 Ĩij The control day value is the average value of the two days before/after the eclipse day (i.e. 6, 12, 24 and 27). These reference days were chosen such that they have similar geomagnetic, interplanetary and solar properties with the eclipse day. The daily average value of the reference days and eclipse day for interplanetary index ( $A_p$  and  $A_p$ ), and solar flux unit index ( $F_{10.7}$ ) ranges 8 – 12 nT for  $A_p$ , 20 – 27 nT for  $A_p$  and 75.6 – 89.1 sfu (1 solar flux unit (sfu) =  $10^{-22}$  Wm $^{-2}$  Hz $^{-1}$ ) for  $F_{10.7}$ , indicating that geomagnetic and solar activities of these days is

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unsettled (see Adekoya et al., 2015 for classification of the activities). This is because under the same classifications, the effect of eclipse in the ionosphere is expected to be noticed when compared with the control day. The calculated daily average of summation  $K_p$ ,  $A_p$  and solar flux indices was obtained from the National Space Science data Centres (NSSDC's) OMNI database <https://omniweb.gsfc.nasa.gov/>. This point has been included in the manuscript (see line 79 - 87)

Comment 5 – Line 241-242, the authors said “The only exception . . . at Millstone . . . H versus  $B_0$  . . .” however, it is clear that R is also low for the two figures of IDAHO.

Response to comment 5 – Thank you for the observation. The statement has been corrected accordingly (see line 262-264).

Comment 6 – 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”.

Response to Comment 6 – After critical observation of the said figures panels, the authors observed that there are no noise in the variations of the parameters during the eclipse window, rather the effect of eclipse was noted in comparison with the control day. Moreover, the digital ionosonde data used were from GIRO, which have minimal/negligible level of noise in the data records (see Reinisch and Galkin 2011; Reinisch et al., 2018). In addition, the reference days were chosen (as explained in Response to Comment 4) in a way that the data are not contaminated by noise, if there is any.

Comment 7 – Line 273-276, as the authors said, “ionospheric F2 parameters ( $NmF_2$  and hmF2), the bottomside profile thickness ( $B_0$ ) and shape ( $B_1$ ) 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,

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especially for such parameters as hmF2, B0, B1 and H

Response to comment 7 Ĩij The use of the parameters in this study is a novel way of observing the ionospheric behavior at lower and topside ionosphere during solar eclipse, and as explained in Response to Comment 6, it is not associated with noise.

Comment 8 âĀĀ 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.

Response to Comment 8 Ĩij The clarification of Figure 3 has been explained to aid the readers' curiosity and understanding in line 221-255 and under figure caption in line 445 – 450.

Comment 9 âĀĀ 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.

Response to comment 9 Ĩij We agree with your submission, and in line with the other reviewer's suggestion, we have deleted appropriately and the sentences have been rewritten both in the abstract and conclusion (see line 13-14 and line 275)

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