Review report of the manuscript entitled, "Calibrating estimates of ionospheric long-term change" authored by Christopher Scott, Matthew Wild, Luke Barnard, Bingkun Yu, Tatsuhiro Yokoyama, Michael Lockwood, Cathryn Mitchel, John Coxon, and Andrew Kavanagh.

Investigations on the ionospheric trends is an important area of research. Researchers have been using hmF2 and foF2 from ionograms to address this issue. The authors of this work dwell on the corrections for possible uncertainties that may be introduced in the use of these parameters for assessing the long term trends given the fact that there is ambiguity in the rates of changes in the hmF2 in different studies reported in the literature. Thus, the current manuscript attempt to investigate the efficacy of deriving long-term ionospheric trends in hmF2 using empirical formulae and to investigate the extent to which this can reconcile the difference in trends derived from the global network of ionospheric monitoring stations.

Thus, the authors suggest using foF2/foF1 ratio to qualify the values of hmF2 for a mid-latitude location. For that they use common data from the MU radar (from 1986-2020) and the ionosonde at Kokubunji (1957 – 2020) to arrive at the frequency rations of F2 to F1 regions to "calibrate" the hmF2 values from ionosonde.

This is an interesting effort which needs to be encouraged. There are, however, several loose ends, incomplete information, which need to be addressed before this article can be considered further.

Many thanks for your consideration of our article and for the helpful comments. We address each of these in turn below.

1. *The formation of F1 layer has seasonal dependence. So, in order to provide an appropriate correction, which is more "accurate", why not use the data from only that season? It will reduce the number of points, however, the values considered for such a season over 35 years may provide a different (better?) correction?*

Yes, the presence of F1 has a seasonal dependence and this does indeed introduce a seasonal bias into the equation. The prominence of the F1 layer in ionograms is controlled by the underlying thermospheric composition which, in addition to the seasonal variation, also undergoes long-term changes driven by geomagnetic activity. The main focus of the current article is to demonstrate that there are long-term biases in hmF2 introduced by the formula and that these will differ by location. Cutting the data by season will not remove these long-term biases although some times and months will be less affected than others.

In response to reviewer #2, we also investigated the influence of the foF2/foE ratio and this too was found to have a similar bias where the ratio fell below ~2.5. This resulted in a similar seasonal bias as for foF2/foF1 since the ratios are both driven below their bias thresholds by the suppression of foF2 during the summer months.

We agree that is important to highlight the presence of a seasonal bias and have added to this section, including plots of how the foF2/foF1 and foF2/foE ratios vary with time of day and season.

2. *Line 20: How much is the role of radiative cooling by CO_2 in the thermospheric cooling?*

The work by Roble & Dickinson referred to here investigates the impact of simultaneous doubling of CH4 and CO2 concentrations in the lower atmosphere. As such, the work does not separate out the effects but does note that

'The model uses the complete $C0_2$ radiation code of Dickinson [1984] which includes both LTE and non-LTE $C0_2$ radiational processes and considers hot bands, isotopic bands, Voigt line shapes and radiative transfer as significant for mesospheric cooling. In the lower thermosphere $C0_2$ cooling rates are proportional to the product of atomic oxygen concentrations and the poorly known rate of energy exchange between O and $C0_2$. For our calculations we assume a quenching rate of 10-12 cm3 s^{-1}.'

They later add that 'The cooling is caused primarily by enhanced $C0_2$ emissions'

We have added the latter statement to our introduction for clarity.

'Roble & Dickinson (1989) note that the modelled cooling is caused primarily by enhanced CO_2 emissions.'

3. *Line nos: 92 and 96, correct the citations.*

Thank you for noticing this. Citation repaired.

4. *Is there any study that describe the relationship between stratospheric cooling with ionosphere? If it is there then provide the reference.*

We do not know of any such study. A search of the literature did not reveal any papers of relevance to this specific area.

5. *The title of section 1.2 can be different as it does not match with the text.*

Yes, this is a valid point. We have changed the section name to 'Investigating long-term trends in the height of the upper atmosphere' which we feel is sufficiently generic that it now covers the contents of the section

6. *Line 126: define M3000F2.*

Thank you for noticing this. As M(3000)F2 is discussed in detail in the next section, we have referred the reader to the subsequent section at this point in the manuscript.

7. *In equation 2, what is M?*

Thanks for spotting this. M represents the M(3000) factor for a general layer. We have added the following text to make this clear;

'where M represents the M(3000) factor of a general layer.'

8. *In eq. 5, define M_o.*

Thank you for spotting this. We have added the following text to explain this term;

'where M_o = MUF/foF2.'

9. *Line 236: Insert 'and' before ΔM.*

Done as suggested.

10. *Equation 4 has been used for the estimation of hmF2. According to Bradley and Dudeney, (1973) this relation is not valid for the values of x_E < 1.7. Then, how the estimation has been carried out? In addition, what about those ionograms when the signal of E-layer is insignificant even in daytime?*

Our analysis does not calculate hmF2 values where x_E < 1.7 irrespective of time of day.

We have added the following text in the section describing the ionosonde data analysis to state this more fully;

'Following the analysis of Jarvis et al (1998) it was initially assumed that foE = 0.4 MHz at night. Where x_E < 1.7, no value of hmF2 was calculated.'

11. *Mention the R square values in Figure-2.*

We have added the R-square value (for each of the sub-panels in the revised figure) to the figure caption.

12. *Correct the caption of Figure 2, such as, write +/- at the place of pm, and change the symbol by < for the case of SZA values.*

Done, as suggested.

13. *In Fig. 1, there is a significant spread in the hmF2 values derived from ISR values which is not present in ionosonde derived values. Can author comment on this?*

This spread in ISR data is due to the fact that the monthly means for ISR data contain fewer points than the monthly median ionosonde data. This point was also raised by reviewer #2.

We have added the following text to the caption of figure 1;

'It can be seen that the ISR data are noisier, due to there being relatively fewer data points from which the mean values are calculated'

We have also added a subpanel to figure 3 (and subsequent comparisons) to show the number of years contributing to each value when the ISR data are averaged.

14. *Line 341 – 342: daytime is when SZA < 90^0. In these sentences it is mentioned incorrectly.*

Thank you. We have corrected this.

15. *5 is incomplete. The text points to the ISR data as well (388 – 389), but it does not exist in the Figure. The figure caption too alludes to this. ISR hmF2 also to be plotted in this figure, similar to the format in Fig. 3.*

Thank you. This figure has been updated to match figure 3 in format.

16. *Lines 411-412: Explain how the percentage error is reconstructed using gradient and offset.*

The text has been changed to describe this more explicity;

‘The resulting gradient and offset of each fit were used to derive a modelled height for an arbitrary ISR height of 250 km. The differences between these two values were used to reconstruct the percentage error in hmF2.’

17. *Lines 438- 439: This statement may be substantiated by an appropriate reference.*

We have added a reference to Millward et al, Ionospheric F2 layer seasonal and semi-annual variations, J. Geophys. Res., 101, 5149-5156, 1996.

18. *Lines 462 – 463: In the estimation of range bias, why inospheric profile is integrated only up to hmF2 as topside ionosphere also affects the radio wave propagation?*

The range bias is introduced by the radio wave propagating through an ionised medium. Since it is the bias in the height of the F2 peak we are interested in, the signal delay needs to be integrated along the path of the radio wave between the ground and the F2 peak, accounting for the upwards and downwards path of the signal.

In order to make this point more explicit, we have added the following text to this section;

‘Since it is the bias in the height of the F2 peak we are interested in, the signal delay needs to be integrated along the path of the radio wave between the ground and the F2 peak, accounting for the upwards and downwards path of the signal.’

19. *Lines 467-468: can one carryout a simple subtraction like this?*

Yes. The ISR uses time of flight of the radio signal to estimate the height of the F2 layer, assuming that there is no signal delay introduced by interaction with the underlying ionisation. By modelling the delay introduced to the time of flight by the signal interacting with the underlying ionisation and comparing this with the known (modelled) height of the layer, an estimate can be made of this bias over a range of diurnal, seasonal and solar cycle conditions. While this is not an absolute measure of the delay occurring in the real-world data, it is sufficient to estimate the relative change in bias across diurnal, seasonal and solar cycle conditions and to demonstrate that this effect does not affect the conclusions drawn from the analysis using uncorrected ISR data.

We have added the following text to make this clear;

‘By modelling the delay introduced to the time of flight by the signal interacting with the underlying ionisation, and comparing this with the known (modelled) height of the layer, an estimate can be made of this bias over a range of diurnal, seasonal and solar cycle conditions. While this is not an absolute measure of the delay occurring in the real-world data, it is sufficient to estimate the relative change in bias across a representative range of conditions.’

20. *One can expect that the neutral wind changes with the magnitude of geomagnetic activity. Can author comment on this aspect?*

This is a very valid point. While we discuss this in the introduction, we have added the following text to the discussion section in order to emphasise this point.

As noted in the introduction, geomagnetic activity may also induce change in global thermospheric circulation with changes in the meridional wind modulating the height of F2-layer. Titheridge (1995) reviews the magnitude of these effects. A poleward wind would move ionisation to lower altitudes, where the loss rate is higher. This would lead to a decrease in the peak F2 ionisation. Under such circumstances, the foF2/foE ratio could potentially become sufficiently small that, in addition to the genuine change in layer height, the empirical formula would start to underestimate hmF2. More work is needed to deconvolve the relative magnitude of these effects but whether driven by changes in the wind-field or local changes in composition, geomagnetic activity can lead to long-term bias in estimates of hmF2.

21. According to these results, the composition effect that varies with geomagnetic activities, should be taken care of for the long-term study. It is mentioned at the line number 119 that regression model is not significant alone to remove the effect of geomagnetic activities. But, in Fig. 8, am index shows clear proportional behaviour with composition proxy. Then, how do author think that correcting the compositional variations in the hmF2 variation will give better result? Could the authors compare their results with the study of Xu et al., (2004) to validate/assess their method?

We agree that we ought to say more about the comparison between the results of Xu et al (2004) and our analysis. However a direct comparison is not possible since both analyses use different version of the empirical formula used to estimate hmF2. We have added the following qualitative comparison;

When conducting their analysis of the Kokubunji data, Xu et al (2004) used the formula of Bilitza et al (1979). While a direct comparison cannot be made with the current analysis, the variability in long-term trends observed by Xu et al (2004) (a difference between long-term trends in noon and midnight hmF2, with the seasonal variation at these two times being opposite to each other) is consistent with a maximum bias occurring around noon in the summer months. Xu et al (2004) also conclude that geomagnetic activity was not significant in the regression model used to remove the effects of geomagnetic and solar variability. Nevertheless, our analysis indicates that variations in the bias of hmF2 estimates is likely driven by geomagnetic activity.