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Geoscientific Model Development Discussions

Interactive comment on "Improved Forecasting of Thermospheric Densities using Multi-Model Ensembles" *by* S. Elvidge et al.

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Many thanks for your review of our paper. We have responded to your comments in order below:

"(1) Line 202: "The model all perform very similarly": To my eyes, the model results in Figure 5 are quite different, especially the variation of neutral density during the storm period, even the standard deviation and correlation are similar. Does it indicate the limitation of using standard deviation and correlation to judge the performance of simulation?"

We agree that there are greater differences in the model performance than was specifed here. In particular the differences during the storm period. We think that the models perform 'more' similarly in this scenario than the others, but perhaps not to

the extent that we have made out. The text has been updated to reflect this fact.

"(2) Line 205-209 and Line 251: the performance difference between the third scenario and the first two is mainly explained as the consequence of the solar activity variation. However, F10.7 actually represents the solar irradiation worse during last extremely quiet solar minimum in 2008 and 2009 than the solar maximum in 2001. It is not clear that the comparison is necessarily worse in the solar maximum when the models are driven by F10.7. Meanwhile, the third scenario includes a much larger geomagnetic storm than the other two, which may also contribute the performance difference in addition to the solar activity change."

Thank you for highlighting this point. Although it is known that F10.7 did a worse job at representing solar irradiance between 2008 and 2009 I do still think that this is the cause for the main differences in model variation between the test scenarios. The poor job that F10.7 did could help explain something about the errors between the models and observations. However since all the models used the same F10.7 value as a driver this "lack of performance" shouldn't have any impact on the model spread. Similarly the larger geomagnetic storm in the third test scenario could have an impact, but probably only during the storm itself. Before the storm hits is when the the model spread is at its least and is only being driven by the F10.7 value. I do think though that it is important to highlight the F10.7 issues during this time, as such we have commented on them in the paper and included a further reference.

"(3) Line 296: "Using the MME as the initial condition in TIE-GCM . . .": The terminology of "initial condition" is confusing. Typically, the initial condition is a one-time thing for the simulation, which is used at the beginning of the simulation period. What has actually been done in this study is to retune TIE-GCM to MME every 6 hours, which is probably different from the initial conditions people usually talk about."

We appreciate that the terminology can be confusing here. To try and address any confusion we have replaced this 'initial condition' discussion (and in other places in

the paper) with the idea of using the MME densities as the starting densities for TIE-GCM..."

"(4) Table 2: The weights are quite different from one scenario to another. It may indicate that the weight MME may not be applicable for forecasting using the weight calculated from historic events."

This is a very useful comment and we have now commented in the paper about the large differences in weightings from one scenario to another.

"(5) The abstract needs to represent the content of the paper better by including more information about the approach and main conclusion."

We have updated the abstract to, without a huge increase in the number of words, better explain the content of the paper.

"(6) Figure 20: the label shows the period of Nov. 2008, which is the second scenario."

This has now been updated.

"(7) Line 252: t → It"

Corrected.

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