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Interactive comment on "Testing the Electrodynamic Method to Derive Height-Integrated Ionospheric Conductances" *by* Daniel Weimer and Thom Edwards

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Referee #1:

The paper presents a model of height integrated ionospheric conductance that is derived from results of other dedicated empirical models. The manuscript is well written, in most part clear and understandable and potentially suitable for publication at Annales Geophysicae.

There are few comments, that the authors shall addresses:

1. The model is a composite of different empirical models. Quantify the uncertainty

and/or error that accumulates by using the empirical models to build another one.

Response:

The results are presented as maps that are calculated for various combinations of preset conditions, such as IMF magnitude, clock angle, and dipole tilt angle. However, the results were never directly referred to as a "model." We took care to not use that word to describe the results. Our view is that an empirical model takes any random combination of input parameters and outputs a result for specified locations and time. We have not created any such program, although the results could, in theory, be used to look-up a map file that most closely matches the desired conditions, as mentioned in response #3. Many more maps of the results are contained in the data archive than presented in the paper. As discussed in the response to item #2, we cannot obtain a good measurement of the uncertainty for each model. However, in comparison with existing conductivity models, our results are known to be within the the expected range of values, so the errors cannot be very great. More recently there have been two papers in the accepted or pre-print stage that have become available to us only after the submission of our manuscript. Our results are within range of the maps shown in these new papers, which provides confidence that our errors are not nearly as large as would be obtained by accumulating the uncertainties (in the range of 20%-50%) that result purely from the high-frequency fluctuations. Carrying out an accumulation of errors with uncertain input values which would be an exercise in futility, particularly due to the unknown influence of the uncertainties in the IMF values, which is discussed in the next point.

Citations of these new papers will be included in the revised paper:

Carter, J. A., S. E. Milan, L. J. Paxton, B. J. Anderson , and J. Gjerloev, Height-integrated ionospheric conductances parameterised by interplanetary magnetic ïňĄeld and substorm phase, J. Geophys. Res., doi:10.1029/2020JA028121 , 2020.

Mukhopadhyay, Agnit, Daniel T. Welling, Michael W. Liemohn, Aaron J. Ridley, Shibaji Chakraborty, and Brian J. Anderson, Conductance Model for Extreme Events: Impact of Auroral Conductance on Space Weather Forecasts, Space Weather, https://arxiv.org/pdf/2008.12276.pdf, 2020.

Liemohn, Michael W., The case for improving the Robinson formulas, J. Geophys. Res., doi:10.1029/2020JA028332, 2020.

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Referee #1:

2. This point relates to point 1. The Discussion mentioned a row of error that might be in the model results. Certainly, the small-scale variation cannot be captured, but this is not what would be expected from the empirical model and is quite clear. Also, as the major source of error, an uncertain solar wind measurement is mentioned. However, it needs to be checked how this is valid and that uncertainties from point 1, may not be more important.

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Response:

We need to make it more clear in a revision that "small-scale variation" includes high-frequency, temporary fluctuations, and such variations prevent an accurate measurement of the standard deviation in a global-scale, spatial mapping. As given in our response to Referee #2, the empirical models all use measurements that were taken as a function of time, while the satellites were moving in space, or in the case of the ground-based magnetometers, fixed in position while moving through local time. The original measurements contain fluctuations at various frequencies, with the most rapid fluctuations, lasting a few seconds or so, often having the largest magnitudes. The end-result models are global-scale, spatial representations of the fields that do not have the temporal fluctuations included. During the fitting process the difference between the

models and input values (the total square error) is minimized, and this error can be calculated. This is where the 20%-50% numbers come from. But since the high-frequency fluctuations are believed to be mostly temporal variations, rather than spatial, the least-square error that is calculated is not a true representation of the accuracy of the spatial models.

In the revision we will add a more detailed discussion about the differences between the IMF values that are measured by sentinels at the L1 orbit, and what impacts the Earth's magnetosphere. The issue is illustrated well in the 2018 paper by Borovsky that was already cited, that this referee is invited to inspect. Further evidence is given in a paper that was published online only recently, having a key point that "the solar wind measured by a single spacecraft at L1 often does not impact Earth in a homogeneous manner":

Burkholder, B. L., Nykyri, K., & Ma, X. (2020). Use of the L1 Constellation as a Multi-spacecraft Solar Wind Monitor. Journal of Geophysical Research: Space Physics, 125, e2020JA027978. https:// doi.org/10.1029/2020JA027978

As the submitted manuscript already indicated, the full, three-dimensional structure of the IMF cannot be determined unless additional sentinels are placed in orbit around L1. On the topic of the variable time delays we are adding these two references in response to Referee #2:

Weimer, D. R., D. M. Ober, N. C. Maynard, W. J. Burke, M. R. Collier, D. J. McComas, T. Nagai, and C. W. Smith. Variable time delays in the propagation of the interplanetary magnetic field. J. Geophys. Res., 107((A8)), 2002.

Weimer, D. R., and J. H. King. Improved calculations of interplanetary magnetic field phase front angles and propagation time delays. J. Geophys. Res., 113, 2008.

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Referee #1:

3. The conclusion repeats earlier finding from other authors and mention the shortcomings of the model results submitted here, but that the results might be of some value to the community. This again calls for a quantification, if rough, of the errors expected. To which application the value is expected?

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Response:

The results could be used in AMIE-like assimilation models and numerical simulations of the coupled magnetosphere-ionosphere system. As we have not created an actual empirical model, the results would need to be used by plugging in our derived values for the IMF conditions that most closely match those being simulated, with interpolation and filling in of missing results. As done by Mukhopadhyay et al [2020], validation of the accuracy is obtained by testing whether or not there is an improvement in various metrics and skill scores in the predictions of ground-level magnetic perturbations. This type of test would be the best way to validate the results. Another possible test would be to feed the output of a FAC model or AMPERE measurements into a potential solver that uses the derived conductivity values, and compare the output with the electric potential patterns that are expected.

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Referee #1:

4. L 23 says that earlier formulars are confusing and that the authors applied a simpler formula. What does justify the simplification?

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Response:

Several versions of the conductivity formulas have been published, as cited in the paper. These mathematical equations contain different notations for the same calcula-

tions, but with identical results. Some formulas contain symbols for cyclotron frequencies, others use mobility coefficients, and some may explicitly include all of the terms, such as magnetic field strength, that go into the calculation of such quantities. In some texts the summation over different ion species is only implied. It is the differences between the various formulas, and their variations, that can be confusing, not the formulas themselves; the paper revision will need to state that more clearly. Equations (1) through (4) get to the same result as the others, but explicitly shows the summation over the different ion species; the simplification is just that (3) and (4) are used to simply formulas (1) and (2) by replacing multiple symbols with these two.

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Referee #1:

5. I had sometimes the impression that not the most recent developments of certain areas are referred to, such as for AMIE, SECS method, or NRLMSIS. Please check about newer developments in these fields.

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Response:

There have been too many variations of the AMIE and SECS methods to keep track of or cite them all, so the earliest or the most well-known papers are cited. The more recent NRLMSIS model, named simply NRLMSIS 2.0, is described in a paper that was only recently submitted and accepted (17 September 2020) so it could not be included in the first draft of this paper, but it can be referenced in the revision.

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Referee #1:

6. What is the opinion of the authors if direct observations near the E region would enhance their findings, such as is provided by a satellite mission like Daedalus?

(https://daedalus.earth)

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Response:

Daedalus would provide the opportunity to study more direct impacts of plasma flows in the E region, as the referee suggests. For example, this data would likely be interesting as a follow-up to an earlier work on the relationship between FAC and solar index work, particularly the FAC response to the solar indices representing the extreme ultraviolet emissions [Edwards, T. R., D. R. Weimer, W. K. Tobiska, and N. Olsen, Field-aligned current response to solar indices, J. Geophys. Res. Space Physics, 122:5798–5815, 2017]. While the Daedalus data would not have a direct impact on our conductivity calculations, it is possible that these data could confirm the existence of features shown in our Figures 6 through 9, and the influence that the IMF clock angle has on the conductivity distribution. Daedalus observations may perhaps be useful in future version of the IRI and/or NRLMSIS models.

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