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Interactive comment on "The research on small-scale structures of ice particle density and electron density in the mesopause region" *by* Ruihuan Tian et al.

Anonymous Referee #1

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This manuscript describes development of a model and associated calculations for ultimately determining the ice particle and electron density in the mesopause region. The electron density structures are particularly important for producing Polar Mesospheric Summer Echoes PMSEs and one ultimate goal of this work is to contribute to an understanding of the PMSE source region. The model utilizes a growth model for the ice particles (collision and adsorption of water vapor and condensation nuclei), and a velocity model (dependent on the ice particle mass and dependent on gravity and neutral drag forces) to ultimately determine the ice particle density with altitude. A charging model (OML with CEC) and quasi-neutrality is then used to determine the electron density knowing the ice particle density. Results of using this model are used to show

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a reduction in electron density in the source region. These reductions produce radar scatter associated with PMSE.

The manuscript is relatively well organized and well laid out. There are some issues with English grammar and style that clearly should be addressed (there is not an unreasonably large number of these English issues, however) . However, there are some serious issues that preclude publication in Annales Geophysicae AG at this time. A key issue is that the authors have not made a persuasive case of the contribution to the field of this work. They have presented a model and some calculations but not effective tie these to observations to lend credibility to the model results. Also they have not articulated a well-defined, focused issue in the field they want to address. There has been past work in this field with previous models. There is no substantive discussion on how their model is an improvement over past models and what unresolved issues they have been able to solve that past models have not. Therefore, the paper is not suitable for publication in AG in its current form. There must be major revisions and the authors must address these key issues. Further detail of some of the critical weaknesses are as follows:

1. The last sentence (line 23-25) of the Abstract is indicative of the major problem. This sentence is vague. Why is this work important ? The rest of the abstract has not made a case for this. In fact, the last sentence is very well known to be the case from other work! No novelty of this work is stated.

2. The authors mention another well-known work in this field (Lie-Svenson et al. 2003). How is this work an advance over the past work ? This should at least be clearly shown since Lie-Svenson is often used as a benchmark work. Also, the work of Lie-Svenson shows the importance of using ion mass (through the ion continuity equation) on the electron and ion structures in the PMSE source region. The work has been validated through experimental observations. Some of these effects has been described by the work of A. Mahmoudian, On the signature of positively charged dust particles on plasma irregularities in the mesosphere, J. Atmos. Sol. Terr. Phys., 2013 which is

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based on earlier work by Chen and Scales, JGR 2005. Therefore, this implies the authors work is not be consistent with observations since it does not contain ion inertia (it just assumes the Boltzmann approximation) ? No direct substantive comparison with data has been shown in this work to lend any validity.

3. What inaccuracies are introduced into the model due to the fact that an equilibrium charge is considered (equation 22). Lie-Svenson et al and other work consider a dynamically time varying particle charge. This would appear to be particularly important since the ice particle mass/radius is changing.

4. In the model section 2, there appears to be too much detail when the primary equation for the ice particle velocity model is equation 8 (perhaps equation 1 should be stated for completeness). The rest of the approximations may be useful but they can be much more succinctly summarized to shorten this section and eliminate all the equations. The final simplified collision equations may also be useful.

5. In general, one could strongly argue that the plasma (and charging) is much less well modeled in the model equations in section 2 than previous models (ie. Lie-Svenson et al., Chen and Scales). Therefore, it is highly questionable if the current work is an advance since there is no comparison using these past modeling approaches. This, again, goes back to the key issue with the manuscript.

6. The model results in Section 3 show some promising trends but these must be more closely compared to observational data. Also, there appear to be no direct linkages to a specific observation the authors are trying to understand. The authors should strive to do more than demonstrate their model does what is expected from the basic physics. Only general comparisons are made to observations which is not enough for a novel contribution.

7. Again, the authors should strive to see if their model is consistent with observations. For example, the average number of charges is less than one (see line 264) with values of 0.2 and 0.3. Does this indicate that the charging model (using a simple

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equilibrium charge) is insufficient ? Doesn't the particle growth impact what charging model is used. Does the fact that the average charge is less than 1.0 indicate there are positive, negative, and uncharged particles? This has been observed/postulated during experiments? The current simple OLM equilibrium charging model does not take the fact of dynamic particle growth into consideration and may likely be inadequate for what the authors are trying to do (with such small initial particle sizes). This has not been commented on at all. For such low particle charges would a stochastic model (e.g. Mahmoudian) be better.

8. Figure 3 and 4 appear to show the electron density structures. These appear to be on the space scale of 10 meters or less. How do these results compare with other models, e.g. Lie-Svenson et al. Also why are these results an advance over these past modeling results?

Summary: This manuscript is not suitable for publication in AG at this time. If the authors consider a revision (which should be major) the key points the authors should consider are:

1. Making stronger case for why this work is superior to past models (i.e. Lie-Svenson). Certainly the author's model is inferior in terms of the model of the ionospheric plasma (no ion inertia) and charging (no dynamical variation) model. A possible advantage is the ice particle growth model but this would appear to be problematic as well without properly doing the charging model correctly. If the novelty in the ice particle growth does not counterbalance the weakness in plasma and charging models, then there is no real contribution or advance in the modeling.

2. There is no substantive comparison with observational data or a focus of an important unresolved scientific issue addressed. This was not clearly articulated and again is a substantial weakness in the paper. It should be addressed in a summary/discussion section and also noted in the Abstract.

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2019.