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Comment on angeo-2021-15

Anonymous Referee #2

Referee comment on "Validation of SSUSI-derived auroral electron densities: comparisons to EISCAT data" by Stefan Bender et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2021-15-RC2, 2021

This work compares E-region (90-150 km) electron density profiles, calculated from the average energy of auroral particles as inferred from ultraviolet images measured by two SSUSI instruments, to coincident ground-based electron density profiles measured by the EISCAT radar. The data are grouped by magnetic local time, with comparisons showing good agreement at magnetic local times 3-11 hr using a Maxwellian electron energy spectrum, and at 15-23 hr using a Gaussian spectrum with an increased value for energy needed for creation of each ion-electron pair to account for bounce-electrons that affect the measured UV flux.

Overall, the manuscript presents some interesting and relevant results.

My biggest issue is this: the title, abstract and conclusion all refer to the validation of SSUSI products, but what is presented is not such a validation. The reason lies mostly in Equation 7 that is used to generate the main results in Figures 2-4. Normally a validation effort will assume one established data set is ground truth (EISCAT in this instance) and complete a comparison of the new data set (SSUSI) to this truth. Equation 7 does not do that – it includes the "truth" and the "evaluated" data in both the numerator and denominator. I would expect the denominator to contain just the truth value to show a proper relative deviation from those data. If, for example, the SSUSI results are systematically high, then the delta will be large, but the relative difference calculated using Equation 7 will also increase the denominator and mask/reduce the full offset from the EISCAT result that appears in the numerator. To demonstrate this hypothetically, say that SSUSI = 2xEISCAT, then Equation 6 yields 1xEISCAT or 100% offset from the ground truth. But then Equation 7 yields 1xEISCAT/(3xEISCAT) = 1/3, or 33% "relative difference." If the denominator only contains EISCAT, then Equation 7 yields 1.0, equivalent to the 100% error from Equation 6.

Second, the manuscript needs to include a reference for the method of converting SSUSI spectra to auroral average energies, and provide a brief summary of the process to

include which of the five SSUSI colors are used. This is important to address some areas of the discussion, for example, the valid energy range and associated altitude range under study and the level of increasing "blindness" to higher energies at lower altitudes. First mention is made on L64 of a "valid regime" that is not really explained until later in the manuscript at L198, and even then only briefly and without detail. Another example is later in the discussion (L232) that focuses on quenching of the airglow as a possible source of uncertainty at lower altitudes. While this seems a bit unlikely to me, more explanation of the process to derive energy from SSUSI data is needed to explain quenching as the driving factor of this. To that point, SSUSI only provides an average energy value, not an altitude profile – I would be more suspicious of components in the analysis like the neutral density that has a direct impact on the altitude profile of ionization, especially since NRLMSIS-00 has been recently updated to MSIS 2.0 (Emmert et al., doi:10.1029/2020EA001321) and contains some key improvements at lower altitudes.

It is also important to report on the presence or absence of any cross-validation studies between F17 and F18. If these have not been validated internally as such, then it might be more appropriate to treat F17 and F18 as separate data sets rather than a single SSUSI data set for validation. This is specifically relevant to understand if the larger discrepancy with F17 at lower altitudes, near the peak of electron density (Figures 2 in particular) is significant since F18 does not show the same issue. It may be that it is appropriate to claim validation of the F18 data but not the F17 data.

L65: Explain the conditions under which zero-value Q_o are found – whether a detection limit of the UV measurement or just times when no auroral emissions are present. It is not clear why it is appropriate to eliminate these data from the averaging process, rather than include them as a valid zero-density data point (as explained on L86). The data may otherwise be biased high if zero-value data are unnecessarily tossed out. L86-87 suggests this is just a detectability issue – but not a reason to toss the result and reduce the number of "valid observations."

It should be clarified if any optimization was done, particularly in the selection of 43.73 eV/electron-ion pair (L105) used for the Gaussian distribution of the afternoon data. A brief explanation should be included to explain this, whether tests done to find the best fit, or if these are just as-run results.

As far as organization of the text, there is content in the introduction that would be better placed in the description of the measurements, e.g. L32-42 is better suited in Section 2.1, and some or all of L43-47 in Section 2.2. I think more explanation is needed in Section 2.1, related to part of my major concerns above, to fully describe the data, including why the energy range has restrictions, what they are, and how they connect to the altitude range under study. L183-197 also contains information that should be included much earlier, in the description of the methods chosen for this study.

L39-43: More detail is important here, regarding the scan time, in-track imaging, and sampling overlap of adjacent scans. The pixel resolution on L40 is better stated as 10 km

x 10 km, although this is only at the nadir, so a range should be included that corresponds to the range of data used in the analysis. No information is provided as to any restrictions placed on the angle from nadir for the SSUSI overlap to keep the 10 km dimension of the bin appropriate for the overlap determination. Other details should address how the data were processed to account for satellite motion, and co-adding of bins or remapping of detector pixel (as noted on L60) into larger image bins for the data product. It would be helpful for several areas of the discussion to include a figure showing a scan and overlap with EISCAT as well, to orient the reader. This would also help in explaining the calculation of the mean of the profiles in L125-126, and in L208 to demonstrate the relative size of imaging areas between the two measurement methods.

The numerous footnotes seem unnecessary and distracting – it would be reasonable and appropriate for the material there to be placed into the body of the manuscript.

Other comments:

L7: Explain "high-resolution" in what sense (temporal, spatial, spectral), and compared to what.

L32: Clarify if SSUSI is one of five sensors on each DMSP, or if there are five DMSPs containing a SSUSI instrument.

L33: Please provide a more accessible reference for the SSUSI sensors, rather than just an organizational promotional document (i.e. something with a DOI).

L34: The "observing times" noted here are confusing and should be clarified. I think maybe UT is not intended here, and perhaps should be LT instead. In any case, it is not clear why the observing time is restricted – unless perhaps these are the times where overflight of EISCAT is available. If this is just noting the local time of the ascending/descending nodes of the sun-synchronous orbit, then that should be clarified (including which node). However, those nodes are defined at the equator and not as critical to the polar observations presented here.

L35-36: The logic of this statement is perhaps a bit crossed. It is more correct to say that ONLY the data from F17 and F18 are compared because F19 was lost. Separately, if F19 was launched in 2014 and lost in February 2016, it would be important to know if data are available in that year of collection that could still be analyzed.

L39: Here again, it is not clear if this is a 12 vs 24 hour notation, or an ascending/descending node time range, or something else.

L41: Clarify that this is the auroral zone above a specific region, e.g. above EISCAT, that is imaged multiple times, not the entire auroral zone that takes a full day to build imagery as the Earth rotates under the satellite.

L49: Provide a reference for UVI, if possible.

L54: Section 4 presents results, Section 5 contains the Discussion.

L58: Footnote 1, clarify the stellar calibration – certainly, there is more than a single star that is used, but there are also shortcomings to the approach (e.g. point sources in a wider field of regard) that do not mitigate some of the uncertainty in the data. Such systematic errors are important and linked to my request to include a report on cross-validation of F17 and F18 data.

L67: Rectify the references here with what is on L45.

L70: Modify "as well as…" to make consistent with the rest of the sentence, perhaps as "and can be configured for a number of experiments…"

L99: Explain why electron bounce is important in the afternoon sector but not the morning MLT sector.

L112: Justification should be given for making the assumption of steady-state for an auroral zone precipitating particle region, which seems highly dynamic, or quantify the level of uncertainty added.

L115: Remove or provide specifics on the "SSUSI internal document" referenced here.

L142: Modify "centered for all satellites…" to reflect something that notes that the bimodal MLT distribution is related to the ascending vs descending nodes of the orbit (I assume), and perhaps has more data in the evening because the zero-flux restriction cuts out proportionally more morning data (again, I assume…). This is important to understand, as this "imbalance" is noted in L148. It seems unlikely to be related to the coincident overflight availability, but rather to the filtering of samples that is done.

Figure 1: Add the notation on what the colors represent in the caption. More detail should be given on these "pulse codes" and why they might be relevant to their interpretation, accuracy, etc.

L158: Refer back to equation 3 here as well.

L196: Some discussion should be given to at least some of the differences and improvements that could be made, and their potential impact, even if they are not explicitly evaluated for this work.

L214: It is not clear how high-resolution data are really validated given the extensive group averaging that is completed in this study. To do so would require a more extensive point-by-point analysis to claim validation in a single 10deg x 10deg image bin.

L218-220: These first two sentences are a bit confusing, particularly whether "variability" refers to the measurements of EISCAT, derived from SSUSI, or the comparison between the two. As I read it, this is saying that the variability in the sample set exceeds the uncertainty of the measurements, which does not equate to being a validation.

L226-232: Six different mis-spellings of the SSUSI acronym within these seven lines of text.

L236: Typo, "virtually"