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Comment on angeo-2022-12

Anonymous Referee #1

Referee comment on "Arecibo measurements of D-region electron densities during sunset and sunrise: implications for atmospheric composition" by Carsten Baumann et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2022-12-RC1, 2022

AnGeo, Arecibo measurements of D-region electron densities during sunset and sunrise: implications for atmospheric composition
Carsten Baumann et al.

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The paper presents Arecibo ISR D-region measurements from 2016 for sunrise and sunset conditions.
The observed electron densities are compared to SIC and WACCM-D models, discussing the mutual similarities.
The paper is well structured and nicely written which makes it a smooth and pleasant read.

The topic is well introduced, followed by the descriptions of the Arecibo measurements as well as used 1D and GCM models and their comparison.
The authors highlight the asymmetry of sunrise and sunset ionization conditions mostly caused by the different ionization and recombination rates.
One limiting factor I see is that the observational data only consists of two sunrise and four sunset measurements, which is of course rather sparse, but the high quality makes the results convincing.

specific comments:

1) I couldn't find any statement about the solar and geomagnetic conditions during the

measurements, only when it comes to using GCM 2005 data given an equivalent solar condition.

2) L19: electron density measurement techniques are introduced: in situ, VLF radio wave reflections, ISR measurements.
Later on, L33, suddenly MF radar techniques are mentioned if not highlighted as it is in the discussion section.
I'm confident the MF techniques, given the system is well capable of it, is more useful and reliable than inferring VLF radio wave propagations...?
Perhaps MF techniques could be mentioned already in L21?

3a) Fig1: I suggest to adjust the color scales to higher electron densities max. 5e4 or 1e5 to limit the saturation for the E-region peak, even though it's not in the focus of this paper. But it will beautify the plot.
3b) Fig1: I assume the obvious gaps have been excluded for the subsequent statistics, but couldn't find a note?
3c) Fig1: Judging on that plot the noise floor, so the sensitivity, is near 10-100 el/cm^-3. Especially with densities below 10, I'd be very careful near that noise floor... From Fig2 and Fig3 it doesn't look like you applied a kind of SNR selection, do you?

4) L102: A "25% trimmed mean" is used to explicitely suppress sporadic E layer echoes. How good does this suppression work considering the echoes are pretty intense. Perhaps adding a plot with an example to the manuscript or only as a reply comparing to e.g. median?
Do you apply the same method to suppress the airplane/ship clutter? @ ~L98

5) L110: At 80 altitude... -> At 80 km altitude...

6) L141: I agree the years 2005 and 2016 were quite similar talking about the solar activity. I guess for that purpose that's sufficient, but what about the dynamics? From my impression WACCM-D is nicely reproducing daily means at late summer for these altitudes, not that sure about the time scales you're looking to, though.

7) L152: Nice idea to use multiple longitudes to create a higher SZA resolution... I'd worry about horizontal transport effects (dynamics).?. 1° longitude corresponds to roughly 100km displacement.

8) L240 (, L285 and somewhere earlier): "Both models employ similar ionospheric reaction schemes." I think that's not strictly correct as you pointed out earlier SIC and WACCM-D incoporate different amount of pos./neg. ions, and thus also possible reactions. I suppose to relax it by "equivalent", but not similar.

"cosmetics":
- consistent use of value and ° without a space, L106, L108, L109, L111, L112
- L232: ..."altitudes between 90 and 75 km altitude." -> remove the latter

Again, I enjoyed reading the well prepared manuscript and I support its publication after minor corrections and adressing the raised concerns.