Response to Reviewer #3

Arecibo measurements of D-region electron densities during sunset and sunrise: implications for atmospheric composition

Carsten Baumann, Antti Kero, Shikha Raizada, MarkusRapp, Michael P.Sulzer, Pekka T.Verronen, JuhaVierinen

We thank the reviewer for assessing the quality of our manuscript. The effort to identify shortcomings and minor flaws is also highly appreciated. and for taking the time to review our manuscript. The constructive comments have been taken into account to further improve the manuscript quality.

In the following we will address all comments point by point.

The authors should give the full name for WACCM-D and GCM in the abstract. In the text, full names should be given in the first place where the abbreviation appears.

Thank you for the corrections, we changed the manuscript accordingly in the abstract and introduction section.

Fig. 1: Is 10^4 ***cm*** $^{-3}$ ***the maximum obtained value of electron density? I ask, because I have the impression that this value is given on large parts of the displayed graphs. I have impression that higher values were obtained but that they are seen as*** 10^4 ***cm*** $^{-3}$ ***due to the limitations of the domains in the display.***

The reviewer is correct, that values above 10^4 cm $^{-3}$ have the same color as 10^4 cm $^{-3}$. This issue was also pointed out by reviewer #1. However, the color scale maximum of the figure has been intentionally set to 10^4 cm $^{-3}$ in order to highlight the lower electron densities of D-region and lowermost E-region. The aim was also to show data from altitudes not being analyzed in detail later on, so that a more complete picture of the lower ionosphere is presented. The caption of figure 1 has been extended with a sentence, so that the choice of the color scale is justified.

Lines 122-123: D-region heights is located between 50-60 km and 90 km. For this reason, the part "... the D-region with an altitude range from 20 to 150 km. " should be rewritten.

The reviewer is correct that this sentence is ambiguous with respect to the D-region altitude range. The sentence has been split in two and now reads: "The SIC model is a one-dimensional ionospheric model designed specifically for the D-region. It covers the altitude range from 20 to 150 km including an ion chemistry for the most prominent ions."

To my knowledge, the SIC model is primarily used for polar region analyzes. The authors should explain the possibility of applying this model

(its original version and the version including meteoric smoke particles) to the area observed in this study. Is it necessary to make some corrections (eg those related to the chemical composition, the influence of the magnetic field, etc.) in these versions of the model to make their application relevant to other areas, or changes depending on observed areas and observation periods can be made in the input files?

The reviewer is correct, that the SIC model has been frequently used for polar latitudes. However, the ion chemistry scheme is not changed for the present study covering low latitudes. Only the SZA, photoionization, galactic cosmic rays are different at low latitudes. These differences can slightly change the resulting ion composition and of course diurnal electron density progression. SIC also solves for ozone-related chemistry, so that part of the neutral atmosphere responds as well. NRLMSISE-00 provides the major species depending on location and solar activity. The effect of the magnetic field on the ionosphere is not handled within SIC, as the D-region is a highly collisional plasma and currents do not play a significant role here. The only parameter that has been changed is the vertical eddy diffusion coefficient. For clarification we have added the following sentence: "SIC has been extensively used to model the high latitude ionosphere in combination with EISCAT radar observations. It's application to low latitude D-region like in Arecibo (Puerto Rico) however, does not need very specific changes. Photoionization and ionization due to galactic cosmic rays are calculated for the location in question. Of course, particle precipitation as ionization source is turned off and besides that only a slight adaptation of the vertical diffusion coefficient is needed. The individual ion species and involved ion chemistry remains untouched."

Does the model use Eq. (1) for calculations of the effective values of the parameters related to the respective processes, or does it consider the reactions of a single type of particles (and consequently coefficients corresponding to these processes considered in particular)? The authors should explain this in the text. In case the first variant is applied, the names of the corresponding coefficients should be written and it should be explained how the corresponding effective coefficients are changed in accordance with the observed conditions. In case the second variant, Eq. (1) should be rewritten with sums and corresponding indexes and all these quantities should be explained in the text.

The reviewer is indeed correct, that the formalism used in Eq. 1 is not fully clear. We have adopted the summation formalism and the equation now reads:

$$\frac{d[N_e]}{dt} = \sum_i q_i - \sum_j \alpha_j [e^-][I_j^+] - \sum_k \beta_k [e^-][N_k] + \sum_l \gamma_l [N_l][I_l^-] + \sum_m \gamma_m^p [I_m^-]. \qquad (1)$$

In order to further clarify, the following sentence has been added: "The summations and their indices indicate that the ionospheric reactions (Verronen 2006) are handled with their corresponding reaction partners."

line 204: γ and γ_p are the effective coefficients related to the collisional

electron detachment and electron detachment by solar photons, not the collisional electron detachment and electron detachment by solar photons.
We incorporated the correction of the reviewer into the manuscript.