

Thank you very much for taking the time to review our manuscript. We appreciate your positive evaluation of our work and your recommendation for publication after minor revisions.

We have carefully considered all of your comments and suggestions. We have revised the manuscript accordingly, and we believe these changes have significantly clarified our findings and improved the overall quality of the paper.

Below, we provide a point-by-point response to your specific comments (reply in blue, and referee comment in black), indicating the exact changes made and their locations in the revised manuscript (with changes highlighted in the track-changes version).

General comments:

GC.1.: The description about the state of the art 2D capabilities of RT models vs. justification of your approach has not found its way in the manuscript apart of just stating a single benefit of 1D version ("high vertical resolution while keeping the computational cost manageable"). In other words, the manuscript still misses weighting in on potential advantages a straightforward approach of a 2D capable model would have as well as making a credit to such models (or existing photochemical correction schemes) vs. benefits of your approach. In the AC you provided a good and detailed discussion which should be part of the state of the art description and for the motivation of the study.

We agree. We have changed the text as follows (L302):

"The method used in this work does not intend to replace the powerful and straightforward 2D/3D RTM, but to provide a simplified and computationally less expensive approach accounting for photochemical changes along long twilight light paths while still working with a 1D box-AMF calculations. Note that this study is aimed to systematically investigate the variability of AMFs across a wide range of solar zenith angles (SZAs), species, and timescales. Using the 1D (open-source) version of MYSTIC in conjunction with our photochemical averaging allowed us to maintain high vertical resolution while keeping the computational cost manageable for the large number of simulations required for this systematic study."

GC.2." The newly provided description "The considered concentration of a given layer for a given solar beam, each time the beam crosses this layer, is the mean value between: the concentration at the top of the layer and the "local" SZA at this point (theta2 for op1 in Fig. 3c, for instance), and the concentration at the bottom of the layer for the "local" SZA at this point (theta1 for op1 in Fig. 3c). This is true except if one beam does not reach the bottom of the layer (as optical path between op1 and op2 in Fig. 3c). In that case, the mean is performed for the values at the top of the layer and the minimum altitude reached (SZA=90°)." is absent from the updated manuscript. You should definitely put this information about how $c_{\{b,l,i\}}$ is obtained in the manuscript.

We agree. We have introduced it in the text as follows (L213):

"where subscript b is for each solar beam, l is for each layer, and i is for all the partial optical paths, sb,l,i , and the corresponding concentrations, cb,l,i , that each beam finds when crossing the same layer (red lines in Fig. 3: $sb,l,1 = op1$, $sb,l,2 = op2$, $sb,l,3 = dz$). The value of cb,l,i is obtained as the mean between the concentration at the top of the layer for the "local" SZA at this point (ϑ_2 for op1 in Fig. 3c, for example), and the concentration at the bottom of the layer for the "local" SZA at this point (ϑ_1 for op1 in Fig. 3c). This is true except if one beam does not

reach the bottom of the layer (as optical path between op1 and op2 in Fig. 3c). In that case, the mean is calculated for the values at the top of the layer and the minimum altitude reached (SZA=90°)."

Detailed comments:

DT.1. You have ignored the suggestion to use the equation formatting. Please make this equation as Eq1., the mathematical definitions of AMF few lines later should also be separated as equations.

We apologize, we misunderstood. We thought the referee was referring to the format of the equations within the text, not to equations separate from the text. They have now been introduced as separate equations (1) and (2).

DT.50. In logical terms, wide use of something does not make it true. Extensive validation is the argument but this should be properly referred to in the manuscript. E.g. in Sect. 2.1. references to the validation work done with emphasis on the polar atmosphere and, most important, twilight scenes for all 4 species must be added and still existing uncertainties mentioned. Alternatively, one can soften the statement in the conclusions making it conditional, e.g.: "Assuming that the model is accurate; this confirms the validity of the method employed and the approximations considered."

We think that the agreement between our calculations and the measured SCDs is what truly validates both the method and the model. However, to soften this conclusion, we have followed the referee's suggestion and modified the sentence as follows (L582):

"Assuming that the model is accurate; this confirms the validity of the method employed and the approximations considered."

DT.5. *"...Vertical Columns Densities (VCDs), which only depend on the concentration vertical profile of the considered molecule."*

Better write, to be more exact: "which are the integral of the concentration vertical profile of the considered molecule along altitude"

This has been corrected in the text (L57).

DT.6. *"It should be noted that the equivalence between these two definitions is strictly valid for weak absorbers, where the change in intensity due to absorption does not significantly alter the radiation field. While this approximation is appropriate for the species and spectral ranges investigated here, it could be problematic for strong absorbers, such as ozone in the short-wave UV."*

Please add some reference(s) here.

The text has been modified as follows (L63):

“It should be noted that the equivalence between these two definitions is strictly valid for weak absorbers, where the change in intensity due to absorption does not significantly alter the radiation field [Platt & Stutz, 2008]. While this approximation is appropriate for the species and spectral ranges investigated here, it could be problematic for strong absorbers, such as ozone in the shortwave UV [e.g., Rozanov & Rozanov, 2010].”

DT.9 *“To our knowledge, the only publication providing explicit OCIO AMFs values for SZA up to 95° obtained from measurements is Pinardi et al. [2022]”*

I think this statement needs to be clarified given that their VCD used in the SCD/VCD ratio is not a directly measured quantity but are an estimation by a geometrical approximation and even is taken just at a single SZA (i.e. it is not taken at the actual SZA which should be according to your definition introduced earlier): You could add the citation from Pinardi et al. after your current statement: “To be more precise the AMF in Pinardi et al. is “defined as the ratio of the measured slant column to the vertical column estimated at 70° of SZA, assuming that at this solar elevation a simple geometrical AMF can be used.”

The text has been modified as follows (L96):

“To our knowledge, the only publication showing OCIO AMFs for SZA up to 95° obtained from actual measurements is Pinardi et al. [2022]. In that work, ground-based DOAS instruments at 9 NDACC stations covering polar regions from both hemispheres are used to validate OCIO measurements by the GOME-2 satellite instrument. Ground-based OCIO values are offset-corrected using a Langley plot approach making use of empirically derived OCIO AMFs. To be more precise, the AMF in Pinardi et al. is defined as the ratio of the measured slant column to the vertical column estimated at 70° SZA, assuming that at this solar elevation a simple geometrical AMF can be used.”

DT36. *Given your explanation before, it is much better understandable now. What is still missing, is mentioning of the box model output resolution in the SZA domain and how concentration values at theta1, theta2, etc are obtained from that output (e.g. by linear interpolation?).*

The last paragraph of Section 2.1 (TOMCAT/SLIMCAT model simulations) has been modified as follows (L180):

“The 1-D model is integrated each day with a 5-minute timestep (meaning the SZA step is always less than 0.6°) and the fields of the target gases are saved for each timestep, providing a dataset of concentrations at varying SZA for every day. The profiles at the SZAs used in our calculations are obtained by linearly interpolating the photochemical model SZA values.”

Technical comments:

L293 *“.* An assessment of polar stratospheric clouds effect can be found in e.g.”: This sentence is incomplete, the citation as in the former manuscript is missing.

The reference was in the Word file but it disappeared when transforming to pdf. It is in the text now (L301):

“An assessment of polar stratospheric clouds effect can be found in e.g. Pukite et al. (2022)”

L399 “Thus, this seems” -> “Therefore, this seems”

[This has been corrected in the text \(L410\).](#)

L428-429: “...but also the vertical profile of the species ((1): different profiles for different SZAs...” -> “...but also the vertical profile of the species: (1) different profiles occur for different SZAs ...”

[This has been corrected in the text \(L440\).](#)

L735: There are two doi links provided; the second link should be removed.

[This has been corrected in the text \(L746\).](#)