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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Analysis of the ozone profile specifications in the WRF-ARW model and their impact on the simulation of direct solar radiation" *by* A. Montornès et al.

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**Response to "Reviewer Comments", Anonymous Referee #1**

We would like to appreciate all ideas and suggestions proposed by Anonymous Referee #1 in order to improve our research and our work. In this document, we will answer each idea and we will clarify those parts that are unclear.

The paper entitled *Analysis of the ozone profile specifications in the WRF-ARW model and their impact on the simulation of direct solar radiation* was published in the At-



mospheric Chemistry and Physics Discussion in $6^{th}$ August 2014. During the Open Discussion period, the paper was reviewed by two anonymous referees.

The first review was received in $29^{th}$ August 2014 by the Anonymous Referee #2 while the second one was published in $3^{rd}$ August 2014 by the Anonymous Referee #1. We have decided to give a personalized response to each referee. However, some points are common for both or they have a full impact to the entire paper. For this reason, we will present firstly a common block and then we will discuss each review point by point. Hereinafter, we will use R#1 and R#2 such as Referee #1 and #2, respectively.

In order to contextualize the response, the referee's commentary appears before our answer. Each review is quoted in gray. Our response appears with **A:** (from Authors) at the beginning and in black color. Each one is identified with a label composed with a number and a tag: GC (General Comments) and SC (Specific Comments). For example, SC4 refers to the $4^{th}$ specific comment. During the discussion, the reader can find some cross-references between responses for R#1 and R#2. For example, **SC7 R#1** means the $7^{th}$ specific comment of R#1.

Some of the answers are also aimed to the Editor. These responses are: **GC4 R#1**, **SC4 R#1**, **SC8 R#1**, **GC4 R#2**, **SC10 R#2** and **SC67 R#2**.

Regarding the submission of revised manuscript, we wait to the final Editor's decision. At that moment, we will finish the last modifications and updates and we will submit the new version.

Common comments

By our understanding, both referees agree that the first part is interesting but they have doubts regarding if the second one was addressed correctly. Nevertheless, they diverge on the reasons and the solutions about that. In short, R#1's opinion is that we are analyzing the absorption instead of the direct flux whereas R#2's perspective

is that a full WRF simulation (at the least for one time step) could address better the discussion.

A further discussion about these considerations is presented in the following points: **GC1 R#1**, **SC7 R#1**, **SC8 R#1**, **GC1 R#2** and **SC35 R#2**.

Introduction

We have concluded that we can improve the introduction in two aspects: i) a better contextualization of the interest of the paper and ii) a better explanation of the presented ideas.

Further information is presented in **SC4 R#2** and **GC1 R#1**.

Methodology

Based on the ideas from both referees, we propose a set of updates in the methodology in order to increase the scientific significance of the results. Two of them have an important impact in the paper structure and the others introduce small improvements or clarifications in the current text. Through the following paragraphs, we will present a small overview of these modifications. You can find further details in the full text at the particular responses (referred in the following explanation).

In his review, R#2 suggested that excluding the GFDL scheme from the analysis deprives the readers from the opportunity to understand its accuracy relative to the other methods (see **SC12 R#2**). We agree about this consideration and we have decided to add this parameterization to the discussion, thus giving a global vision of WRF. As a consequence, we propose to add some changes on section 2 (providing further details about this parameterization), on sections 3 and 4 (adding this scheme to the discussion) and on the figures (adding the respective maps).



On the other hand, from the commentaries of R#1 (see **GC1 R#1** and **GC2 R#1**), we have concluded that we could show and discuss the vertical profiles provided by each scheme. Moreover, this discussion could be linked with the current results and conclusions. As we understand it, this small study could be interesting to the scientific community as well as for the WRF users because it is missing in the state-of-the-art.

Results

After reading the review from R#1, we have concluded that the used metrics are useless for a good paper understanding (see **GC4 R#1** and **SC8 R#1**). We would like to improve this point.

In the first part (sections 2.2 and 3.1), our discussion was based on the relative error (equation 5 in the paper) without including any figure as a baseline (e.g. monthly maps for the MSR data-sets). We concluded that this way of showing the results is useless if the reader does not have a background about the ozone spatial and seasonal distributions. In order to correct this point, without adding new figures, we propose to replace the relative error by the bias (in Dobsons). As a consequence, equation 5 in the paper, section 3.1, Conclusions and Figures 1 and 2 must be updated accordingly.

In the second part (sections 2.3 and 3.2), we computed the bias in the ozone absorption (equation 22 in the paper) normalized with respect to the radiation at the top of the atmosphere. As a result, we compared two percentages and we lost the physical sense of the results. To improve this point, we propose to show the results as W $m^{-2}$ multiplying the previous results by the incoming radiation at the top of the atmosphere. Sections 2.3 and 3.2, Conclusions and Figure 3 must be updated accordingly.

Figures

Both referees noted that the quality of the images could be improved. We agree about these considerations. The original figures were created using the R language. We have been testing the NCAR Command Language and it clearly improves the quality of the results. We would like to improve all figures (map size, mosaic distribution, color palette and labels).

In order to avoid a larger document, we save the new figures until the final revision of the paper. Please, contact with us if you need further details, and we will provide them.

**Response to general comments**

**GC1 R#1:** In this paper, the authors aim at analyzing i) the ozone representation in the shortwave (SW) radiative transfer schemes of WRF, and ii) the impact of the biases in this representation on the predicted direct solar radiation. Three different ozone representations have been analyzed: one which is shared by the Goddard, New-Goddard and Fu-Gu-Liou SW schemes, and two more used in the CAM and RRTMG SW schemes, respectively. The two objectives are clearly set out at the beginning of the paper but, to my view, the interest of the study is not sufficiently well motivated.

The methods used are appropriate to achieve the proposed objectives. However, I have concerns regarding how the second objective was addressed. In my opinion, the interest of the paper has not been clearly set out. This work deals with stratospheric ozone. Thus, in principle, it has high interest for WRF's applications in the stratosphere. However, only total ozone has been verified and nothing is said regarding how the vertical profile of ozone is represented in WRF. This limits the interest of the study for "stratospheric" applications. In any case, authors should add comments on how important is a correct vertical distribution for the vertical distribution of heating rate,



and the coincidences and differences in this with respect in the analyzed data bases. It is claimed that this study has interest for solar energy, more specifically, for solar energy forecasting. However, in my opinion, this importance should be better contextualized.

**A:** Regarding to the second objective issue, we will provide a full explanation about the ideas and procedures to address in the **GC1 R#2** response. In the following lines, we will focus our discussion on your commentaries about the stratosphere and the solar energy forecasting.

We agree that the work deals with stratospheric ozone. However, it also deals with solar modeling. In its travel throughout the atmosphere, a solar beam is absorbed and scattered by atmospheric gases (e.g. water vapor, ozone) and particles (e.g. aerosols, cloud droplets). Therefore, to compute the solar radiation reaching the surface is requires understanding all these physical processes that occur in the atmosphere.

In a clear sky atmosphere, basically two gases absorb the solar radiation: water vapor and ozone. In the troposphere, water vapor absorbs solar radiation in some near-infrared (near-IR) spectral bands.

On the other hand, ozone is located in two atmospheric regions with a different impact on the radiative transfer. Most ozone ($\sim$90%) is located in the stratosphere while the remaining ozone ($\sim$10%) is found in the troposphere. The stratospheric ozone absorbs solar energy in a few bands of ultraviolet (UV) and photosynthetic active region (PAR) whereas the impact of the tropospheric ozone on the radiative transfer is negligible.

Hence, all approximations assumed on modeling these contributions have an impact on the accuracy. Of course, other elements such as aerosols or clouds have a higher impact and, at the same time, they are more difficult to be improved. In contrast, as we presented in the paper, ozone introduces smaller errors that, moreover, could be easily reduced. From our perspective, an understanding of the physical processes is significant *per se*, beyond the impact on the output accuracy.

When we started to work on this study, we were worried because the lack of literature analyzing the impact of the ozone modeling on radiative codes and we decided to introduce a scientific discussion about that. In the first part, we presented a detailed information of the ozone treatment in the radiative parameterizations of the WRF-ARW that we considered that could be of interest to many WRF-ARW users and solar modeling researchers.

In addition to this, the shortwave radiation absorbed by ozone in the stratosphere is the primary physical process in maintaining its thermal structure. Thus, the stratosphere is a parallel topic but it is not the focus of the analysis. Nevertheless, from your considerations, we recognize that the paper can be enhanced if we introduce more details about the stratosphere on the discussion.

A validation of the vertical profiles is not feasible under the structure of the presented work because a global and climatic ozone profile dataset with a reasonable horizontal resolution does not exist. The ozone-soundings are provided generally from balloon and aircraft campaigns at several sites and for limited periods. Furthermore, many of these sites are located in the United States. We searched this kind of data in the NOAA Earth System Research Laboratory server (ESRL), the Climate Prediction Center (CPC) and the National Climatic Data Center (NCDC) without success.

In contrast, satellite data provide global and climate measurements with good spatial resolution. However, these data are vertically integrated such as in the Total Ozone Mapping Spectrometer (TOMS) Data or the MSR used in the present analysis. For this reason, we decided to use the MSR dataset as a baseline for our analysis.

In order to include your ideas, we propose the following improvements:

- A better contextualization of the paper in the introduction following the aforementioned ideas.
- Add new figures plotting the ozone profiles for each scheme and a description in



section 2.1.

- Link the discussion of the results to those figures.

Moreover, we consider that this type of figures can increase the scientific significance of the paper because similar plots do not exist in the state-of-the-art.

**GC2 R#1:** I miss some comments on the average absorption due to ozone in typical conditions, so that the reader receives a clearer message on the importance of ozone for solar energy.
Since it is claimed that “high spatial and temporal variability” of ozone occurs in the stratosphere, it would be helpful if some figures were given of the expected range of seasonal variability in a point and spatial variability for a fixed season and how they translate to solar radiation extinction. These simple numbers would help to advance the reader the order of magnitude of the corrections that could be achieved with an improved representation of ozone. This could be compared with the typical errors of WRF in solar energy forecasting applications.

**A:** The response to these considerations is linked to the previous one. The new figures related to the ozone profiles can be useful for the reader to understand the order of magnitude of the ozone variability.

In addition, we agree about the idea of including some numbers to help the reader to understand the paper.

Finally, as we detail in **SC10 R#1**, we propose to show the bias in W $m^{-2}$ instead of percentages. This improvement will be useful in order to clarify some ideas.

**GC3 R#1:** One application that is not even mentioned is the modeling of shortwave irradiance in the UV part of the spectrum. The SW schemes analyzed make spectral computations. Could have been this analyzed somehow? Moreover, the latest WRF versions provide broadband direct and diffuse irradiance with RRTMG and New-

Goddard. Could they be used to investigate the impact of ozone misrepresentation on irradiance fluxes?

**A:** Regarding the application for modeling of shortwave irradiance in the UV part of the spectrum, we agree with the reviewer that it is important from the point of view of the radiative transfer models. Nevertheless, we did not mention this application because this is not the "natural" usage of solar parameterizations in mesoscale NWP models and particularly, in WRF.

In general, the shortwave schemes in WRF divide the spectrum in a few bands (from 1 in Dudhia's case to 8-16 in the other schemes). Upward and downward fluxes are computed at each band in terms of different contributions. Some contributions are specific of that band (e.g. ozone in the UV or water vapor in the near-IR) and others are general for all of them (e.g. clouds or aerosols). The total upward and downward fluxes are the sum over all the bands (i.e. spectral integration). In order to reduce computational memory, the intermediate values are generally stored in temporal arrays that are removed at each computation step.

Moreover, note that the shortwave schemes in the model are not designed for spectral applications. In the past, the shortwave schemes were necessary to set the day-night behavior on NWP simulations. Therefore, the greatest interest of this kind of parameterizations was the total absorbed energy (i.e. the heating rate) profile and the total global horizontal irradiance at surface for the land model. A full treatment of the radiative problem was computationally expensive (even nowadays). Therefore, the general methodology was to reduce the computational time by reducing the complexity of the methods (e.g. using a few bands instead of line-by-line computations). With this usage of the radiative codes, accuracy was not a matter of concern because other sources of error had a higher impact.

In recent years, the interest for modeling the solar resource by using NWP models has increased and, as a result, also the interest for a good accuracy on global, direct and



diffuse irradiances. Nevertheless, the radiative codes are the same as in the past.

Thus, from our perspective, the radiative codes in WRF are not good tools to be used for spectral purposes. There are other external codes more complex that are ready to perform this type of studies.

Setting aside these set of considerations, note that we are actually analyzing the UV and PAR regions. Let us simplify the problem and imagine a scheme with three bands: UV, PAR and near-IR. Since ozone absorption occurs in the UV and PAR bands, we can write the total direct flux as

$$F_{dir}^{sch} = F_{dir,UV}^{sch}(O_3) + F_{dir,PAR}^{sch}(O_3) + F_{dir,nearIR}^{sch}, \quad (1)$$

when we use the scheme ozone data, and

$$F_{dir}^{MSR} = F_{dir,UV}^{MSR}(O_3) + F_{dir,PAR}^{MSR}(O_3) + F_{dir,nearIR}^{MSR}, \quad (2)$$

when we use the MSR dataset.

If we only consider the ozone contribution, then the atmosphere is transparent in the near-IR. Thus, both terms $F_{dir,nearIR}^{sch}$ and $F_{dir,nearIR}^{MSR}$ are mutually canceled when we compute the bias.

Regarding the second question, we agree with the reviewer when he says that the RRTMG and New Goddard parameterizations provide spectrally integrated direct and diffuse irradiance components. However, this is not required as we detailed in **GC1 R#2**.

In short, ozone does not have an explicit contribution on the diffuse component. The molecular scattering is considered in the Rayleigh's term and it assumes dry air mass without considering any kind of species. Therefore, the ozone dataset does not play any role on the scattering computation.

On the other hand, in the computation of the Beer-Lambert law, we can consider each contribution to the optical thickness (e.g. water vapor, ozone, ...) independent one another. Hence, we can analyze the ozone impact on the direct flux without considering the other elements.

**GC4 R#1:** To my view, the study of the impact of the ozone misrepresentation on the computed direct irradiance has not been totally addressed. This analysis has been done showing maps and numbers of absorption biases, instead of the expected irradiance biases. However, the irradiance biases can be very easily computed by including the effect of solar geometry. Unless these maps are included in the paper and the results analyzed in terms of irradiance biases I don't agree that this paper addresses the impact of ozone errors on the direct solar irradiance. I would encourage the authors to address these issues and the specific comments detailed below.

**A:** The radiative schemes use the ozone data to compute the direct flux (see **GC1 R#1**) while the molecular scattering (i.e. diffuse component) of these molecules is considered in the Rayleigh term without any consideration about the gas species. Therefore, since this paper is focused on the ozone profiles, we only validate the direct flux because the diffuse one does not depend on the ozone data.

In a non-scattering medium, when a solar beam travels throughout a layer, one part of the energy is absorbed $A$ by the medium and the other part is transmitted $T$ to the next layer (energy conservation). In other words, if we consider normalized values

$$1 = A + T. \tag{3}$$

Therefore, when we compare the outcomes using the MSR dataset and the ozone data provided by each scheme, the biases on the absorption and transmission are the same but with opposite sign.

As we normalized the results with respect to the radiation at the top of the atmosphere



(TOA), we plotted the absorption because it was more understandable. All this information appears in the equations. Furthermore the nomenclature is coherent during all the paper. Thus, from our point of view the ozone misrepresentation on the computed direct irradiance has been well addressed.

In the response at point **SC10 R#1**, we propose to show the results with physical units (i.e. W $m^{-2}$). Hence, we can plot the direct radiation instead of the absorption if the Editor thinks that it is better. Nevertheless, from our understanding, this type of plots will not add any new information.

**GC5 R#1:** I would also suggest the authors to use the knowledge acquired in this work to improve the current representation of ozone in WRF (for instance, by including the MSR dataset in WRF and making it available for the SW schemes). WRF is public and freely available for anyone and I am sure that the WRF's community would be thankful.

**A:** This idea sounds good but is not be feasible with the MSR dataset. The MSR considers the total ozone amount instead of the vertical profiles that are required by the WRF model to compute the heating rate profile at each grid-point (see **GC1 R#1**).

As the dataset provided in CAM shows the best accuracy when it was compared with the MSR data, it could be used for the other schemes.

In fact, from version 3.5 the RRTMG can utilize the ozone profiles available in the CAM scheme with the option o3input in the namelist.input file (as we commented in the paper). This improvement could be added in the other parameterizations with some code modifications.

We will add this suggestion on the Conclusions and we will explore this idea as a future work (see **GC5 R#2**).

**Response to specific comments**

Title: Analysis of the ozone profile specifications in the WRF-ARW model and their impact on the simulation of direct solar radiation

**SC1 R#1:** I don't see the title appropriate for a threefold reason:

**R#1:** i) only the total ozone amount has been analyzed, but not the "ozone profile" (i.e., the vertical distribution of ozone);

**A:** It is true that we are analyzing the total ozone amount. However, the main idea of the paper is to offer to the scientific community a description about the simplifications that are assumed in the ozone treatment within the radiation options of the WRF model and a quantification of the impact of these assumptions.

In section 2.1 we provide a full description of the ozone datasets that can be improved as we suggest in **GC1 R#1**. In section 2.2, we compute and validate the total ozone amount because 4D (spatial and temporal) ozone datasets are not available (further details in **GC1 R#1**).

Therefore, from our perspective, this part of the title is correct because the ozone profile datasets are the subject at matter in which the study is developed.

**R#1:** ii) the impact of the ozone misrepresentation is analyzed, and not the impact of the ozone profile "specifications"; and

**A:** From our point of view, what we have done is analyzing the ozone profile specifications to asses the impact of the ozone misrepresentation. Moreover, as we referred in the previous paragraph, we also provide a full analysis of the profiles in section 2.1 that can be improved after this revision.

**R#1:** iii) the impact on the solar radiation absorption is analyzed, and not the "impact on the direct solar radiation"



**A:** We believe that "direct solar radiation" is justified following our discussion in **GC4 R#1** regarding the relationship between absorption and transmission.

Probably the title could be improved but, from our point of view, it is a good representation of the paper's content.

Section 2.1

**SC2 R#1:** I don't think you have necessarily to distinguish always between the Goddard and New-Goddard SW schemes. The new SW Goddard is essentially the Goddard scheme (Chou and Suarez, 1999) with only few minor modifications (http://www.atmos.umd.edu/∼martini/wrfchem/ppt/WRF_Toshi.ppt). You can mention you are using the new version implemented in WRF and from there on just talk about Goddard SW scheme.

**A:** We understand your point of view and it is true that New Goddard is an updated version of Goddard. However, we think that is better to distinguish both schemes because:

i) they are different model options (i.e. Goddard was not overwritten by New Goddard). Hence, the paper can be more useful for the WRF's users (see **SC12 R#2**),

ii) the source code of New Goddard was rewritten with many differences at computational level. These code changes lead to significant differences in the applicability. For example, New Goddard can not be coupled to the WRF-CHEM, while Goddard can. Concluding, both assume the same approximations but the differences in the code are significant to be distinguished in the paper.

**SC3 R#1:** One more thing is that the reference Chou et al. (2001) is not appropriate because it is for the longwave Goddard scheme only.

**A:** We included this reference in terms of the available literature that is listed in the

code of the New Goddard scheme (module_ra_goddard.F):

```
! Refferences :
!  Chou M.-D., and M. J. Suarez, 1999: A solar radiation
!    parameterization for atmospheric studies. NASA Tech.
!    Rep. NASA/TM-1999-10460, vol. 15, 38 pp
!  Chou M.-D., and M. J. Suarez, 2001: A thermal infrared radiation
!    parameterization for atmospheric studies. NASA/TM-2001-104606,
!    vol. 19, 55pp
!  Matsui, T., W.-K. Tao, and J. J. Shi: 2007: Goddard Radiation
!    and Aerosol Direct Effect in Goddard WRF, NASA/UMD WRF Meeting,
!    Sep 14 2007.
!  Shi, J. J., W.-K. Tao, T. Matsui, R. Cifelli, A. Hou, S. Lang, A.
!    Tokey, N.-Y. Wang, C. Peters-Lidard, G. Jackson, S. Rutledge, W
!    Petersen, 2010: WRF Simulations of the 20-22 January 2007 Snow
!    over Eastern Canada: Comparison with in-situ and Satellite Obse
!    J. Applied Meteor. Climatol, 49, 2246-2266.
```

The code referring to the physical processes in the UV and PAR bands is based on Chou et. al. (1999), while the code referred to the near-IR is based on Chou et. al (2001). Therefore, we chose both references as relevant to the scheme.

Since the WRF-ARW User's Guide lists the primary references of each parameterization, we have decided to follow that nomenclature to avoid misunderstandings on the readers. A complementary explanation is given in **SC13 R#2**.

**SC4 R#1:** Could you provide details and/or references on the origin of the ozone profiles used in each SW scheme?

**A:** Sorry, we can not. We tried to search this information when we wrote the paper, but without success. In general, this information does not appear on the source code or in



the based papers. Finally, we decided that this information was not mandatory for the paper. However, we could try to contact the authors to ask them if the Editor thinks that it could be valuable to enhance the paper.

**SC5 R#1:** I don't understand: "The RRTMG scheme includes two ozone profiles as a function of the season (winter and summer). Nevertheless, this granularity is useless due to the fact that the final used profile is computed as a composition of both, without considering the day of the year. Therefore, only one profile is considered for any latitude and season." Could you please explain better? Why is it useless? Is it not used in RRTMG?

**A:** As we summarized in Table 1, the ozone data of this parameterization is stored in a subroutine O3DATA located in a file denoted module_ra_rrtmg_lw.F (note that the letters *lw* are not a mistake).

Given one grid-point, this routine has three inputs: the pressure at the relative ETA levels of that point, the starting index for the vertical levels (in all the cases, 1) and the ending index for the vertical levels (in all cases, the number of vertical levels). In order to reduce the discussion, we will disregard the difference between full and half levels.

As output, this routine returns the ozone profile interpolated to the ETA levels.

When you begin to read the code, you find 4 arrays with a dimension of 31 elements each one: O3SUM, O3WIN, PPSUM and PPWIN where O3 denote the ozone mixing ratio (kg/kg), PP the pressure (hPa), SUM summer and WIN winter.

These arrays store two ozone profiles: one for summer and one for winter.

In the next step, two new arrays are built: PPANN and O3ANN, both with a dimension of 31 elements.

In the PPANN is stored the PPSUM array (this process is completely arbitrary).

Given one element K, the O3ANN(K) is computed as

```
O3ANN(K)=O3WIN(K-1)+(O3WIN(K)-O3WIN(K-1))/(PPWIN(K)-PPWIN(K-1)
         (PPSUM(K)-PPWIN(K-1))
O3ANN(K)=0.5*(O3ANN(K)+O3SUM(K))
```

Finally, this array is interpolated to the ETA levels and returned to the main code flow.

You can note that, although this scheme contains two ozone profiles, this information is not actually used. As a result, we have a single profile that is invariant on latitude and time.

When we prepared that paper, we decided to summarize this information because we believed it was not important for the purpose of this study. The key point it is that this parameterization only uses one scheme. As we indicated in the paper, this scheme can work with the CAM dataset but we chose the original settings.

The paragraph has been reworded to be more clear.

Section 2.2

**SC6 R#1:** How did you re-grid the datasets to 1 deg x 1 deg ?

**A:** As both MSR and ERA-Interim are defined in a regular lat-lon grid, we used a simple bi-lineal interpolation.

Section 3

**SC7 R#1:** Why can you validate the RRTMG's ozone amount but not the impact of its misrepresentation on direct solar irradiance? Why do you only validate the impact using the Goddard and CAM ozones? I don't understand this point.

**A:** We can validate the RRTMG's ozone amount because we can isolate the vertical



profile (see **SC5 R#1**) and integrate vertically from the surface to the top.

In contrast, we can not validate the impact of its misrepresentation on direct solar irradiance as we will argue in the following lines.

From equation 20 (in the paper), we know that the absorption can be computed as

$$A(\tau/\mu_0) = 1 - \int_0^\infty W(\lambda) e^{-\tau_\lambda/\mu_0} \mathrm{d}\lambda \tag{4}$$

where the optical thickness $\tau_\lambda$ from the TOA to a level $z$ may be expressed as

$$\tau_\lambda(z) = \int_z^\infty k_\lambda \rho q_{O_3} \mathrm{d}z, \tag{5}$$

where $k_\lambda$ denotes the mass absorption cross section and $\rho$ is the dry air density.

This integral requires the vertical information of the ozone mixing ratio and the dry air density.

The mass absorption cross section is the ability of one molecule to absorb a photon given a particular wavelength. Nevertheless, in the atmosphere, the molecules are not isolated and they interact the ones with the others. As a consequence, monochromatic absorption is rarely observed because the energy levels during energy transitions are changed due to the external influences. Therefore, the radiation absorbed during consecutive energy transition is non-monochromatic and the spectral lines are broadened.

In virtue of the kinetic theory of gases, the dependence of the $k_\lambda$ on temperature and pressure can be demonstrated. Hence, as $\tau_\lambda$ is a function of the height and this is a function of the temperature and pressure, the integral can not be computed without a detailed information about $k_\lambda$.

Regarding the spectral integration, the best method to compute equation 4 is the line-

by-line (LBL) calculation. However, this method is not computationally feasible because would require many thousands of computations at each grid-point. Instead of this, some approximations are assumed in terms of the gas and its spectral behavior. For example, the water vapor absorption has a high variation with the wavelength and the K-distribution method is required (see Liou (1980)).

Other gases as the ozone show a lower variation with the wavelength and an effective $k_\lambda$ is used for each spectral band. This coefficient is previously computed using the LBL at a reference value of pressure and temperature. Finally, this value is scaled to the pressure and temperature of each layer in order to consider the dependency on these magnitudes.

In WRF, New Goddard and CAM use this second approximation while the RRTMG uses the K-distribution method.

Moreover, the dependency of the ozone absorption with pressure and temperature is small and New Goddard and CAM do not scale this magnitude (see, for example, Chou and Suarez (1999)).

Therefore, in New Goddard and CAM, $k_\lambda$ can be assumed as a constant with height and $\tau_\lambda$ is

$$\tau_\lambda(z) = k_\lambda \int_z^\infty \rho q_{O_3} \mathrm{d}z, \tag{6}$$

or using the hydrostatic equation and considering the entire atmosphere

$$\tau_\lambda(p_\mathrm{s}) = \frac{k_\lambda}{g} \int_0^{p_\mathrm{s}} q_{O_3} \mathrm{d}p. \tag{7}$$

The integral is directly the ozone amount described by equation 3 (in the paper). Therefore, we can evaluate the radiative absorption without any information about the vertical



distribution.

In contrast, the RRTMG considers the dependence of $k_\lambda$ on height and we need to know the vertical structure of the atmosphere to solve $\tau_\lambda$.

Since the vertical profile from the MSR dataset is not available, we can not compute this integral for the baseline dataset. For this reason, we did not include the analysis of the RRTMG scheme.

Chou, M.-D. and Suarez, M. J.: A Solar Radiation Parameterization for Atmospheric Studies, NASA Tech. Memo, NASA/GSFC, 104606, 40, 1999.

Liou, K. N.: An Introduction to Atmospheric Radiation, vol. 84, International Geophysics Series, Academic Press, New York, 1980.

Results

**SC8 R#1:** It would be interesting to add annual results, not only monthly. As mentioned earlier, direct solar irradiance biases (in W m$^{-2}$, and in % would be also interesting) should be shown instead of absorption biases.

**A:** We think that, the annual results are a good way to summarize the results but they do not contribute with new information since ozone shows a well-defined seasonal pattern. Then, in our opinion, it is not necessary.

Regarding the direct solar irradiances, we agree about your consideration. We propose to update all the figures to show the results in W m$^{-2}$. This update involves some modifications in the Methodology (section 2.3) and in the Results discussion (section 3.2).

Figures and Tables

**SC9 R#1:** Split both Fig. 1 and Fig. 3 in two figures.

**A:** We agree about this point. We will wait until the Editor's decision.

Others

**SC10 R#1:** Technical Corrections Needs careful proof-reading for English grammar and style.

**A:** We have tried to address and to improve this point.