Atmos. Chem. Phys. Discuss., 13, C12842–C12850, 2014
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal variations of stratospheric ozone measured by ground-based microwave remote sensing at the Mauna Loa NDACC site: measurement validation and GEOSCCM model comparison" *by* A. Parrish et al.

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We thank the referees for their constructive criticism. Below we report their comments and how we would address them.

Anonymous Referee #1 - (Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31855, 2013.) Received and published: 13 January 2014

Comment: The authors could think about a few adds in order to make the article more

understandable for non-experts, e.g., they could explain why ground-based microwave radiometry seems to be the only measurement technique which can measure the diurnal ozone variation.

Response: Passive microwave instruments can observe spectral lines of ozone (and other species) in emission, as the energy levels of the rotational transitions at millimeter wavelengths are so closely spaced that they are excited by intermolecular collisions at stratospheric temperatures, and are in local thermodynamic equilibrium. Because microwave emission observations have no dependence on solar illumination, they can be made at all hours of the day or night. These points were made, in different language, on page 31860 in the paragraph beginning on line 19. The issues involved in making microwave and infrared ozone emission measurements from satellite platforms were also discussed in this paragraph. As a clarification, we would add the sentence "These lines are excited by intermolecular collisions, so no external source of illumination is required for observations and they can be made either during the day or at night." after ". . .wavelengths" on line 21 of page 31860 in a revised manuscript..

Comment: In the introduction and later, I am missing a reference to a related new study: "A climatology of the diurnal variations of stratospheric and mesospheric ozone over Bern, Switzerland S. Studer, K. Hocke, A. Schanz, H. Schmidt, and N. Kämpfer Atmos. Chem. Phys. Discuss., 13, 22445-22485, 2013". For example, Parrish et al. wonder that the night-morning differences are larger in the model world than in the observations. I think, Studer et al. experienced the same.

Response: Studer et al. appeared in APCD on 29 August 2013, when our manuscript was in a late stage of preparation. Other referees have requested that we place our results in context with those already published. We plan to do this and would include Studer et al. in this discussion.

Comment: In the introduction, it could be emphasized that observational results of the tiny diurnal variation in stratospheric ozone were quite uncertain until now. I don't be-

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lieve much in the TIMED/SABER results on the diurnal ozone variation at stratospheric altitudes. They look quite shaky and seem to be not consistent. Actually the present Parrish et al. study is most convincing since their radiometer measures the complete daily cycle at an high-altitude station. The observational results of Haefele et al. and Studer et al. (2013) are also good and in agreement with Parrish et al. but a rest risk remains in the data retrieval of Haefele and Studer because of the high tropospheric opacity at a low altitude station such as Bern or Payerne. Thus the main point seems to be that Parrish et al. give for the first time a really clear observational evidence of the daily cycle of stratospheric ozone. I would suggest that the authors communicate this crucial point in a clear manner in the revised version.

Response: We would make this point by comparing the typical tropospheric opacity value at Mauna Loa to the value it would have at a low altitude midlatitude station in the instrument description section in a revised manuscript.

Comment: p.31858, line 6, the equation for photolysis of O2 is missing

Response: Thanks for noticing this. We would add it in a revised manuscript.

Comment: p.31878, last sentence: "The good agreement between MWR, Aura-MLS, UARS-MLS, and SMILES suggests that the last three, together with the model, can be used to estimate such adjustments over a wider range of latitudes. "I disagree with this statement. The authors did not make a model validation at polar latitudes where model simulations of ozone photochemistry, polar vortex variations and tides are much more difficult than at mid-latitudes.

Response: We agree that this is an overstatement and would remove it in a revised manuscript.

Comment: Acknowledgments: I am missing an acknowledgment to the ISSI ozone team where most of the authors participated.

Response: Thanks for noticing this. We would add the acknowledgement in a revised

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manuscript.

Referee 2:

Comment: Analyzing the magnitude of the diurnal variation of stratospheric and mesospheric ozone is nothing really new. In the introduction I would have expected to see a more comprehensive summary of previous work (historical and recent) on the subject. Whilst most of the early work focussed on the mesosphere, some of these studies contain also results for the mid to upper stratosphere and at different latitudes. In order to place the new work in an appropriate (historical) context, I suggest that the authors amend this part and refer the interested reader to relevant earlier work addressing ozone diurnal variation.

Response: We would do this in a revised manuscript, referring to the measurements by Haefele et al. (2008), Connor, et al. (1994), Ogawa, et al. (1996), and Studer et al. (2013), and discuss some differences between these results. We chose these papers because they report results from the stratosphere as well as the mesosphere. Because the focus of this work is on the afternoon enhancement in the stratosphere, we don't plan to include results from papers that present only results in the mesosphere. We would add this material at line 19 on page 31860.

Comment: Results obtained in this study on the magnitude of the ozone diurnal variation in the stratosphere (Sections 3 and 4) should then also be compared with results from previous studies by different authors. What is consistent with earlier work and what is new in this study?

Response: As we remarked in response to a comment by referee #1, it is difficult to know how meaningful direct comparisons between measurements made at tropical and midlatitude locations will be, but we would comment on shared features between the present and previously published results iin a revised manuscript, noting the point that there could be differences between tropical and midlatitude results.

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Two comments:

The paper presents an interesting new (reanalyzed) data set and successfully addresses primarily technical and validation aspects, but provides no or only little new analysis of the causes of the ozone diurnal variation in the stratosphere. The manuscript would thus much better fit into ACP's sister journal "Atmospheric Measurement Techniques" (AMT, same special issue).

Finally, in an ACP paper one would also expect a bit more discussion of the reasons for the diurnal variation in the stratosphere (in Section 4, GEOSCCM model comparison).

Response to the two comments above:

We agree that this paper, as it stands, has a mixture of ACP and AMT content. A large part of it is devoted to validation of our measurements, and a smaller part to the model comparison. We would expand the latter somewhat in a revised manuscript, noting where discrepancies between the model and the measurements are also seen in previously published results. We then hope to prepare a follow-on paper, led by a member of the GSFC team, which would consider the physical and chemical mechanisms in more detail.

We note that another paper published in ACP (Hocke, et al., Comparison and synergy of stratospheric ozone measurements by satellite limb sounders and the ground-based microwave radiometer SOMORA, Atmos. Chem. Phys., 7, 4117–4131, 2007) is similar, in that it presents a detailed comparison between SOMORA and satellite based measurements.

We included the technical detail in section 2.2 because we felt that we could not persuade readers that we had taken care to understand and minimize diurnally varying systematic errors in our measurements in just a paragraph or a few sentences. If desired, we could put this material in an appendix in a revised manuscript.

Comment: Concerning the abstract, the observed magnitude and characteristics of the

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observed diurnal variation should be described before validation and model comparison results are summarized.

Response: We would do this in a revised manuscript.

Comment: Some references (For the list, see the published referee report.):

Response: We would include and discuss those that include data in the stratosphere in a revised manuscript.

Technical comments: 31859, 7 "Further refinement of ozone records will reduce the time required to make the detection ..." I would argue that it is even more important to continue monitoring of O3. An extension of existing ozone data records will considerably lower the error of trend estimates.

Response: We agree with this comment and would clarify the language in a revised manuscript

Comment: 31859, 16-27 and 31875, 25-26 When it comes to correcting the effect of ozone diurnal variation in satellite ozone timeseries, the caveat is that ground-based microwave observations have only a relatively rough vertical resolution and cannot be used to obtain the magnitude of the ozone diurnal variation at a given altitude. Therefore, validation of atmospheric models with potentially high vertical resolution is essential.

Response: We convolved model results with the averaging kernels of our measurement system to make the models and measurements directly comparable. In principle, higher measurement resolution would allow recovery of model information lost in the convolution process. However, in this instance, the effects of our vertical resolution are small. We compared the GEOSCCM_strat-chem model output as originally supplied with the convolved output as shown in Figure 8 of the manuscript. These are shown in the attached Figure 1 as the blue and red lines respectively. The differences are < 1% except between 7 and 12h at 1.3 hPa, where they are < 1.5%. We believe that

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our resolution is about at the limit of that obtainable with a fixed, ground-based, remote sensing instrument that is capable of observations over the complete diurnal cycle. Higher resolution is available with satellite-borne limb sounding instruments. We commented on issues with satellite measurements in section 1. We would clarify language in a revised manuscript to make these points.

Anonymous Referee #3

Comment: This paper presents an intercomparison of ozone datasets with the aim to tease out small diurnal variations in stratospheric ozone. The comparisons are thorough, convincing and certainly merit publication. While this is a potentially very good paper, I did feel, however, that they didn't go quite far enough. As the authors do note, there is a rich literature on this from Huang et al and also Haefele and yet the authors do not adequately place their results in context with these earlier studies. Do they agree? For example for Day 85, Huang et al (2010) (their Figure 5) show a decrease in the afternoon at 30 hPa which they believe to be real. It appears, based upon the comments at the very end of Section 4, that the present authors disagree. If so, they should say so.

Response: We do not agree with the 10% decrease at 16h that Huang et al. (2010) reported. We see a 2% decrease at that time and altitude. We would discuss this issue in a revised manuscript.

Comment: The final comparison of the present paper is limited to March. One wonders if another figure for a solstice case would be any different For example, Huang et al [2010] show both day 85 and day 180 analyses and Haefele et al have a section on seasonal variations. Perhaps at the relatively low latitudes of Hawaii, seasonal variations are less important. But this would be good to clarify. I do not think the above comments would require much work to address; however, I certainly would suggest adding another figure, for a different season, to complement Figure 8 and another couple of paragraphs of discussion where they put their results in context.

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Response: Model results for March were the only ones available at the time our paper was submitted. Results for other months only became available in January 2014. We propose to replace Figure 8 with two similar figures showing results for winter and summer, because the contrast between these two seasons is the largest. Results for spring and autumn are nearly identical and generally lie between the winter and summer results.

Comment: Since the existence of the afternoon stratospheric ozone enhancement is not a new result, their results are more of a confirmation (albeit the most comprehensive that has been presented) rather than a discovery and this should be explicitly stated.

Response: We would replace the sentence “However, ground-based microwave measurements by Haefele. . .”beginning on page 31860 line 14 with “Haefele et al. (2008) also reported the afternoon enhancement based on their ground-based microwave measurements and attributed it to continuing ozone formation during the day through reaction (2) and the relatively high density and consequent low O/O3 ratio” in a revised manuscript.

Very minor comment: For Figure 3, I was a bit confused (line 15 of text) Which of the colored curves is the best? Is it the black curve? Is this what is used to create the bottom panel?

Response: The black curve is best, and it was used to make the lower panel. We will clarify the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31855, 2013.

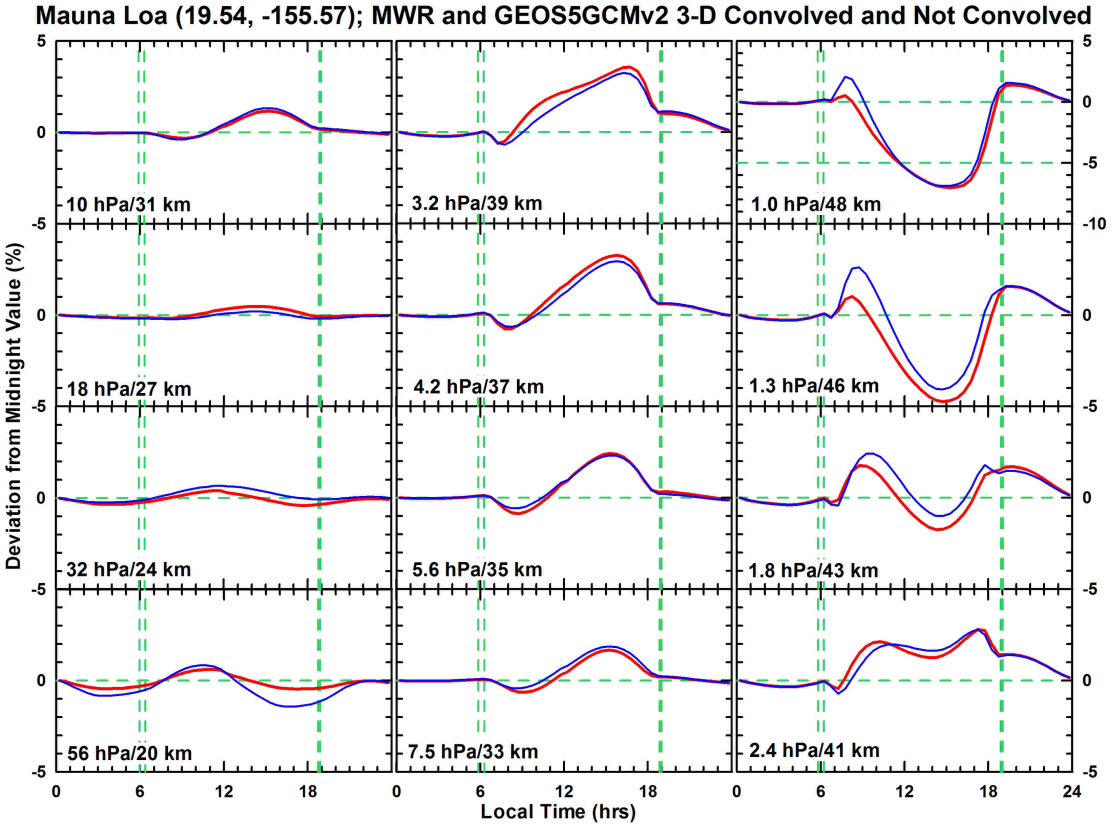

Fig. 1. GEOSCCM_strat-chem model output, blue lines; as convolved with the MWR measurement system averaging kernels, red lines.

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