

## ***Interactive comment on “Global Ozone Chemistry And Related Datasets for the Stratosphere (GOZCARDS): methodology and sample results with a focus on HCl, H<sub>2</sub>O, and O<sub>3</sub>” by L. Froidevaux et al.***

### **Anonymous Referee #1**

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This paper describes the construction of an important and comprehensive long-term dataset for key trace gases in the stratosphere. It is important to have a peer-reviewed and citable description of datasets like this GOZCARDS dataset. The manuscript serves this purpose very well, and I support publication in ACP.

My main criticism concerns the length of the manuscript. Over 100 journal pages, almost 30 figures, plus appendix and supplement are a tall order. I wish the authors would have made the effort to be more concise. At this point it is probably difficult to

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make the paper much shorter, but I have some suggestions:

### **1 Major comments**

GOZCARDS HCl sample results: Given recently published results, e.g. Mahieu et al. 2014, I feel that it is not necessary to discuss the HCl variations at various levels in such detail. I think Figures 7 and 8 already present all the relevant information. Figures 9 and 10, to me, are not needed (a look at Fig. 7 gives almost the same information). The discussion on pages 5865 last paragraph to 5868 first paragraph is also not necessary. I think it provides no really new or essential information, and is not necessary for the main topic of the paper, which is description of the overall dataset. Moving the important citations to the early part of section 3.3 would be enough, in my opinion.

GOZCARDS H<sub>2</sub>O sample results, pages 5873 to 5876: This reads almost like a review paper. Again, I think many readers will be familiar with the overarching issues, and would appreciate a briefer and more concise discussion. Figures 15 and 17 already give a very good description of long and shorter term water vapour variations, even without much text. Is Figure 16 really needed? Are all the (confusing and overall similar) thin and dashed lines needed in a dataset description paper?

GOZCARDS Ozone sample results: Since GOZCARDS is primarily a stratospheric ozone profile dataset, I wonder of the long comparison and discussion with the Ziemke and Chandra 2012 column dataset is really that helpful. As the authors show, this dataset seems to definitely have a problem, and generally TOMS and OMI are not recommended for long-term trend analyses any more. It seems to me that this part of the current paper dates back a few years. There are several more recent analyses of ozone profile trends (Tummon et al. ACPD 2014, Harris et al. ACPD 2015, WMO 2014). I feel it would be much better to tie in the GOZCARDS data with these publications, and

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drop the comparison with the Ziemke and Chandra column data. At the same time, the discussion could be shortened, and number of figures in this part could be reduced.

Summary and conclusions could also be shortened. There, and in several other places, it seems to me that the manuscript resulted from adding newer references to an older previous manuscript version, and discussing them later / separately. E.g. Section 5.5.2 about diurnal ozone variations seems like a tag-on coming from more recent interest and publications on this topic. The reference to Mahieu et al. 2014 paper on page 5893 restarts the discussion again, making the paragraph unnecessarily long.

The water vapour summary on pages 5894 and 5895 is also very long. It should be shortened and focused.

Overall, the manuscript would benefit greatly from a thorough attempt at removing redundancies and shortening plus focusing on the main topics: Data set generation and sample results.

## 2 Minor comments

page 5851, lines 15 to 17: I find this sentence confusing and hard to understand. Maybe change to: In the upper stratosphere, however, SAGE II is not used after June 2000, when the (NCEP temperature-dependent) data conversion from a density/altitude to a mixing ratio/pressure grid exhibits a shift in the Southern Hemisphere. Instead HALOE data are used as a transfer reference from SAGE II before 2000 to the datasets starting in 2004.

page 5852, line 15: Here, and in most places, WMO 2011 should be supplemented by WMO 2014, or replaced by it. WMO 2014 was published in December 2014.

page 5854, line 6: Probably good to add "converted to mixing ratio versus pressure using their enclosed NCEP temperature profiles, and" after "were"

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page 5854, line 8: Add a typical number of MLS profiles per day. By the way, also related to the next paragraph: How is the standard error of the mean calculated in Figure A1(b)? From the Appendix and the Figure caption I get the impression that it is calculated by standard deviation divided by the square root of the number of profiles. This assumes that all the profiles are uncorrelated. This may be true for the few and far spread occultation profiles, but for the many close emission profiles it is probably not true.

page 5854, lines 15, 16: cross-sections (UV, visible, near IR, far IR, microwave) could also be an issue.

page 5855, 1st paragraph: Maybe add WMO 2014 as well?

page 5856: I am missing a mention of SAGE screening. Maybe mention the discussion in the Appendix here.

page 5860+5861: I find this whole discussion lengthy and difficult to understand. Could it not be shortened? Is it not simply like this: First AURA-MLS and ACE-FTS are shifted to have the same average level in 2004 and 2005. This level is equal to their combined average level. Then HALOE (which has fewer data in 2004 and 2005) is also shifted to this level.

page 5865, line 23: With the addition of the Mahieu et al. 2014 references, and one or two additional sentences, section 3.3 could end right here. The rest of the section, until page 5868 line 15, is not needed, in my opinion.

page 5868, around line 25: Is it certain that these low H<sub>2</sub>O values are wrong? Is there a reference for that? Could H<sub>2</sub>O from the gas phase have gone into Pinatubo aerosols, which consist of a sulfuric acid + water mixture? I have not done the numbers, so maybe this just shows my ignorance.

page 5873, line 15 to page 5876, line 6: This is very lengthy, half a water vapour review paper. I think it should be shortened substantially. A few key messages, no literature

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review.

page 5877, lines 1 to 7: I am missing clarity here. There are two aspects: 1.) SAGE retrieval needs density profiles. For V6.2 these came from NCEP operational, for V7 these came from MERRA. The effect of the different sources on SAGE ozone (number density vs. altitude) is very small 2.) Conversion to mixing ratio vs. pressure needs temperature/ pressure profiles. There, a large artefact arises in the SH upper stratosphere when NCEP is used instead of MERRA. If GOZCARDS would use MERRA here (and not the NCEP that comes with SAGE V6.2) the problem would be much smaller. I think this needs to be made clearer.

page 5878, line 20: Give some references for SAGE-HALOE comparisons, e.g. E. Remsberg et al., and others.

page 5879, line 27: I don't think SAGE ozone drifts. What drifts is NCEP / the conversion to mixing ratio versus pressure. Please correct.

page 5881, line 17 to page 5887 line 21: Two very long subsections (review papers) that present lots of references, but very little really new information. Please shorten substantially.

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