

Interactive comment on “Observed and simulated time evolution of HCl, ClONO₂, and HF total column abundances” by R. Kohlhepp et al.

Anonymous Referee #2

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The manuscript by Kohlhepp et al., provides an analysis of long term observations of HCl, ClONO₂, and HF taken by the ground based Network for Detection of Atmospheric Composition Change (NDACC). Though many of the ground based sites provide observations back to the 1990's, the authors focus on the first decade of this century. A suite of models are used for additional comparison to the measurements. This work is a nice review of the observations and, overall, the paper is well written. I believe it is suitable for publication.

However, given the scope of this study, I question whether this work might not be better presented as two separate papers; one paper whose focus is a complete presentation of the data and another paper that presents the model analysis. I will leave that for the authors and editor to decide.

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For the current manuscript there are some concerns that need to be addressed.

General comments: An almost identical analysis has already been published in Chapter 6 of the SPARC Report on the Evaluation of Chemistry Climate Models, June 2010. In this report, the observations were used to evaluate various CCMs. For HCl and ClONO₂ the SOCOL, EMAC, and SLIMCAT models did not do as well as other models presented in that study. For instance, the report states that SOCOL over-estimates HCL in the 500-600K region. No mention of the results from the SPARC report is made in this paper which is somewhat egregious considering the comparisons and analysis are so similar. In fact, the SPARC report does a better job of describing issues pertaining to the models. If the model configurations presented in this paper differ significantly from those in the SPARC report or the conclusions the authors infer from their model efforts are different then some comment needs to be made regarding this to differentiate this work from SPARC. If the result presented here are similar to SPARC then that needs to be stated as well.

While this paper provides a good synopsis of the ground based measurements, it is not clear what scientific questions are being answered here or how this work relates to some of the larger issues being discussed in our field. For instance, the authors report a decrease in HCl and ClONO₂ during the period of study, as expected. Do these results also agree with reports of the initial recovery of stratospheric ozone?

This brings up another point, which is that the analysis in the paper would be greatly enhanced through the use of other available data sets, such as those from the satellites (SAGE I/II, HALOE, etc.). For instance, Newchurch et al., 2003 compare the trend in satellite observations of ozone to HCl and conclude that stratospheric ozone loss is decreasing. Further confirmation (or otherwise!) from ground based observations of the Newchurch et al., study would be very valuable to the community.

Specific comments

Model analysis

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The models presented here are used for comparison to the observations and the authors do a good job of describing the differences in trends between model and measurements. It is a little confusing as to whether the measurements are being used to validate the models or vice versa, though. Please provide a clear description of purpose as to why the models are being used in this study and why these particular models are best suited for this work. It is also not clear why multiple models are needed. If one particular model has shortcomings then why is it used for this effort? What insight into the atmosphere is gained by having multiple models?

There is little discussion as to why the model trends differ from the observations. Do the models not represent the chemical or dynamic state of the atmosphere accurately enough? The authors state on page 32099, Line1 “These different types of models can help to evaluate the contributions of dynamics and chemistry to observed trends” but do not really use the models to determine these contributions.

The models are all using outdated chemical kinetics, with some models still using recommendations from JPL 2002. The JPL 2010 compendium has been available since June 2011. While it is unknown whether updating the chemical kinetics will change the results, the use of the older recommendations leaves some question as to the validity of the model analysis.

Please provide a brief description of the bootstrapping method then refer to Gardiner et al., 2008 for more information. Some readers may not be familiar with this method. The models use boundary conditions from different studies, EMAC considers the IPCC A1B scenario and SOCOL considers the CCMVal2 REF-B1 scenario. How do these scenarios differ and what effect will this have on the final model output?

Instruments

While the differences between instruments may be obvious to the authors and other experimentalists (i.e. A Bruker 120HR vs. a Bruker I20M FTS) it is not obvious to others in the community, such as modelers. If it is important enough to mention the dif-

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ferent instruments used in this study, it is important then to describe salient differences between these instruments and how this will affect observations.

For the comparisons between observational sites closer to the poles a major concern is whether the models can reproduce the polar vortex well. If they are not able to reproduce the extent of chlorine activation and denitrification then comparisons to the ground based data are problematic. Was any screening done to make sure the model and instruments were “seeing” the same atmosphere?

The authors state that NCEP analysis fields of temperature and pressure are used for the instrument retrievals yet ECMWF met fields are used in the KASIMA and SLIMCAT models. No mention is made as to whether the EMAC and SOCOL models use assimilated winds. Can ECMWF met fields be used for the observational retrievals? Does the use of NCEP instead of ECMWF lead to some of the differences between model and observations?

Technical corrections:

JPL 2002, 2006, IUPAC 2004, 2005 are referenced in Table 5, yet there are no corresponding references to Atkinson or Sander in the references section.

Tables 7, 8, and 9. It would be useful to provide another column that contains the average model trend to give the reader an overall impression of how well the models are doing.

Page 32088, Line 18. . . I believe it should be “the World Meteorological Organization”

Page 32101, Line 23 Define MECCA

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32085, 2011.

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