Atmos. Chem. Phys. Discuss., 7, S4064–S4067, 2007
www.atmos-chem-phys-discuss.net/7/S4064/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simple measures of ozone depletion in the polar stratosphere" *by* R. Müller et al.

R. Müller et al.

Received and published: 15 August 2007

Initial reply to the main point of Reviewer #1

We thank reviewer #1 for his review. The main issue stated up front in the review is that "*low total ozone does not necessarily result from enhanced chemical destruction but may be caused by dynamical factors. This fact, I believe, is a well known one.*"; a point that recurs throughout the review. Of course, we agree with the reviewer on the fact that total ozone is influenced strongly by dynamical factors and that this fact is well known since decades, for example [1, 5, 3].

The intention of our paper is not to discuss the impact of transport on column ozone but rather discuss the *consequences* of this impact for designing measures of polar

ozone loss. We emphasize that minimum ozone is currently used as a measure of polar (driven by heterogeneous chemistry) ozone loss in a number of important papers and assessments, for example [6, 4, 7, 2]. Similarly, the March mean Arctic total ozone value is used in a number of assessments and review papers clearly in the context of chemical polar ozone loss.

In particular, the 2006 WMO/UNEP scientific assessment of ozone depletion made use of absolute annual minimum ozone poleward of 60 degrees latitude as a measure of the severity of polar ozone depletion and compared observed values with those from coupled chemistry climate models. The inter-comparisons between the measurements and models highlighted a number of problems with this metric, but because this metric has been in standard use for a long time, it was felt that its use in the latest assessment was unavoidable. There was certainly no material in the published peer reviewed literature that highlighted the deficiencies of the minimum ozone metric. The results from this paper were presented at a CCMVal workshop in Leeds, UK, in June 2007 and there was agreement within the community that the use of absolute minimum ozone is problematic and that a paper published in the international literature to justify the discontinuation of its use would be helpful. This paper fulfills that need.

However, we learn from the review that is important that we make these points more clearly in the paper and that we have to link our work better with the literature on extreme ozone minima. We will do so in the revised version of the paper.

A more detailed, point-by-point response to the reviewers comments will follow later in this discussion.

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