Atmos. Chem. Phys. Discuss., 12, C4562–C4565, 2012
www.atmos-chem-phys-discuss.net/12/C4562/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of transport and mixing in autumn on stratospheric ozone variability over the Arctic in early winter" *by* D. Blessmann et al.

Anonymous Referee #1

Received and published: 11 July 2012

General Comments:

This paper does a nice job of diagnosing the physical correlations and, by inference, causation behind observed interannual variability of ozone in the NH polar vortex in early winter. The bottom line is that variability in lower stratospheric polar ozone is mostly a result of varying degrees of dynamical mixing of air from lower latitudes into the vortex region through the fall season, and that the mixing is driven by wave activity propagating up from the troposphere. This is all consistent with previous analyses and conceptual models. The authors have done a lot of work in assembling the data, producing the model runs, and generating the correlation statistics and figures for 17 years. The analysis builds a compelling case for the mechanistic explanation of the resulting ozone variations. The paper is well–written, organized, and contains thoughtful graphics.

Two things, however, seem lacking: why should we (the reader) care and what is new in the findings? The paper could really benefit from an explanation of why the issues discussed are important or meaningful in a broader context. If, as the intro states, uncertainties remain in understanding the quantitative effect of dynamical processes on the high latitude ozone layer, then what is the range of the uncertainty, in which processes, how is it improved in this work, and what are the significant implications for assessment of stratospheric ozone and its changes?

Furthermore, it is not clear what we have learned new here. There is nothing unexpected or novel stated in the conclusions or abstract. In fact, several places in the text summarize the bottom line of the analyses as already shown by others. For example, we don't need extensive tracer simulations to produce a conceptual diagram like Fig. 12. This is pretty common knowledge. Corroboration of previous findings can potentially be useful, publishable work, but the discussion is not organized in that way. A better clarification in the discussions of what here is new, better, and/or confirmative would help to make this paper a useful part of the ongoing scientific dialogue on this topic appropriate for ACP.

Specific Comments:

A good addition to this paper would be to test the assertions regarding the realism of the model simulation in Section 2.2 by tracking observed ozone. One could use the ozone data set to initialize transport in the various origin regions and see how passively transported ozone compares to the vortex receptor regions (data from south of 30N could be filled in from MLS or other sources if necessary). This would help to answer if chemistry matters over the time frame of the simulation, if the mixing parameterization is reasonable, and if the model produces a realistic balance between mixing

ACPD

12, C4562–C4565, 2012

and descent.

P 15091, lines 22-25, Discussion surrounding Fig 10 is misleading. Fig 10 high correlations cluster around the zero subsidence line. They don't show higher correlation with lower initial theta (as stated in the text) any more than higher initial theta (and perhaps less). There is no apparent contradiction of general potential temperature gradients. Discussion should be modified accordingly.

P. 15092 and Fig. 12 caption, associations between subsidence and wave-driving/mixing are not precise (and apparently contradictory to P. 15086, lines 4-6). Preference for one pathway or another is not necessarily associated with lower or higher net subsidence; it is likely due to varying contributions from different wavelengths breaking at different altitudes. Overall, higher wave driving leads to more subsidence, which is the nature of the Brewer-Dobson circulation. Resulting vortex ozone is balance between mixing and subsidence. I recommend seeking the help of a card-carrying dynamicist to revise this discussion accordingly.

It doesn't seem that much additional information is added with Figs 13-15 to confirm what we already know from previous work, ie., EP flux is correlated with mixing and, hence, higher ozone in the lower stratosphere vortex. Perhaps just skip to Fig. 15, although the weak correlations are not very convincing that a significant portion of the variance is captured by these diagnostics.

P 15095, line 5: What are 'These mechanisms' that had not been examined previously but have here? In line with general comments above, please explain better what is new or informative in the conclusions.

Technical Corrections:

Abstract, line 2: 'fair amount' is not a useful descriptor. Be quantitative, give numbers. Abstract, line 10: 'related to' -> 'correlated with'. P 15084, line 22: Fahey and Ravi cited reference is about summertime, not autumn. P 15085, line 9: Not correct. Variability

ACPD

12, C4562–C4565, 2012

continues to increase each month in Fig. 1. Modify text. P 15085, line 1: ‘declines to’ -> ‘declines toward’. P 15085, line 28: ‘enforced’ -> ‘enhanced’. P 15086, line 23: ‘ higher as’ -> ‘ higher than’. P 15088, line 22: dele ‘used’. P 15089, line 1: ‘in’ -> ‘at’. P 15089, line 4: dele ‘already’. P 15089, line 9: ‘mark’ -> ‘identify’. P 15090, line 10: insert ‘correlated with “more mixing and” higher ozone‘. P 15091, line 17: insert ‘same “data” as Fig 9…‘. P 15095, line 6: dele ‘relatively new method of‘. Fig. 4, shade origin box used to produce Fig 5 distribution. Combine Figs. 6, 7. Fig. 9 caption, insert ‘originating from “all potential temperature levels within” the equivalent latitude interval…’

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15083, 2012.