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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The total ozone field separated into meteorological regimes. Part II: Northern Hemisphere mid-latitude total ozone trends" *by* R. D. Hudson et al.

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General

The authors have previously published their finding that total ozone can be divided into 3 regimes (4 in winter) within which the variability in total ozone is small. In this work, they investigate how northern mid-latitude total ozone trends from 1979 to the early 1990s have been influenced (a) by ozone changes within the regimes and (b) by changes in the areas of the regimes themselves. This approach uses a different dynamic perspective to existing ozone trend analyses which rely on correlations with quantities such as EP flux and tropopause height, etc. The overall conclusion is that

about one third of the trend in northern mid-latitude ozone from 1979 to the early 1990s can be attributed to concurrent changes in the regime structure. By implication the remaining two thirds can be attributed to chemical ozone depletion. This finding is consistent with the emerging consensus on the causes of northern mid-latitude ozone trends.

The paper is suitable for publication once the concerns described below have been addressed. They fall into two main areas: greater clarity and precision particularly with respect to the method; and improved reference to other recent work. It is quite possible that this will include some new work and it will almost certainly involve increasing the length of the paper. This is needed because while the method for determining the regimes is described well in Hudson et al (2003), the stability of this approach to any chemical changes in total ozone over time was not addressed and so needs to be addressed here.

Specific Comments

1. I am not convinced that the calculation of the uncertainties is correct. This stems from two main reasons. First, the authors do not justify using a Monte Carlo approach based on a previously calculated error in the mean rather than the more generally used (and so accepted) time series approach. Second, the quoted errors look too small. Underlying these two main reasons are a number of smaller ones.

(i) The reference to a statistical text book [6188, 2] should not be needed unless there is something unusual in how the standard deviation and persistence are calculated - if there is, a better description and justification is needed.

(ii) On page 6187 and following, there is a lack of clarity and precision in the statistical description. Which mean is referred to in the 'error of the mean' - the mean ozone, the mean area or what? Are the quoted uncertainties 1 or 2 sigma?

(iii) What is the effect on the trends [6187,25-27] of iterating to a 5% convergence as

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opposed to a 10% or 1% convergence?

(iv) The zonal ozone trend is given as 3.2 +/- 0.6 %/decade. The central value seems fine, but 0.6 is low for a 2 sigma uncertainty estimate for trends derived from the satellite record starting in 1979. (It is similar to the uncertainty estimate for the combined ground-based record which start much earlier.)

(v) No reason is given [6189, 19-25] why the authors prefer the Monte Carlo approach to estimate uncertainties when other, widely accepted approaches could be used.

The authors need to make their description of the statistical approach much clearer. They are using a new approach in terms of the statistics as well as of the atmospheric assumptions. They also need to show that their calculation of uncertainties gives the same answer as the standard approach or that any differences are understood.

2. As stated above, the stability of this approach to any chemical changes in total ozone over time needs to be addressed. For example, the determination of the regimes is justified schematically in Fig 4 of Hudson et al (2003). The flat ozone values shown there are not reflected in the data shown in Fig 1(b) here. The authors try to allow for this using a new method for boundary determination [6187, 15-28]. A secular trend over time on one side of one region (as seems likely in reality since chemical ozone depletion increases with latitude as a result of vortex dilution) would, I think, change the boundary calculated at the other side of the region using the new technique even if the 'real' boundary had not changed. I think the authors should convince themselves that this effect is small. My concern is that while the new approach seems a fair way to calculate the boundaries on a given day, it may cause some false trends to be induced over time.

3. A major theme recently in this area has been the change in northern mid-latitude trends since the mid-1990s. The authors refer to Hadjinicolaou et al (2002), but not to the work in GRL in 2005 which extends the same work to 2004. The major findings in the latter paper are (a) about one third of the downward trend in northern mid-latitude

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ozone in winter from 1979 to the mid-1990s was caused by dynamical changes, and (b) that the upward trend in northern mid-latitude ozone since then is also dynamically driven. Other authors have made the same point. The authors need to address this point in this paper - visual examination of Figures 5-7 indicates different behaviour in recent years, so it should be easier enough to make some valuable comments.

4. There is a general lack of recent references to statistical ozone trend analyses. The authors need not be comprehensive, but more discussion of other relevant work would be useful. Some papers they could consider are given at the end of this review.

5. I have some questions about the consistency of the figures in this paper with those in their previous work (Hudson et al, JAS, 2003). The most important is between Fig 1(b) in this paper and Fig 2(a) in the 2003 paper. There is a much larger change in gradient in ozone within regimes shown in Fig 1(b) here. For example, the mid-latitude values vary from 340 to 390 DU here, while in the earlier work they range from 340-360 DU. Similarly a close examination of Fig 1(a) here and Fig 1 in the 2003 paper, show some different features. (In addition, the colour scheme in the earlier work is better for a paper concentrating on mid-latitude changes.) It is possible that these are explained by their current use of TOMS v.8 data (rather than v.7 data previously), but I doubt it. Either way an explanation is needed.

6. It would be good to discuss the seasonality of the trends in regime and in ozone. Other work indicates that dynamic effects are largest in winter. Latitudinal ozone gradients are weaker in summer and autumn, and so a change in regime boundary would show a smaller change in total ozone and be less sensitive to the ozone-based analysis presented here.

Techincal Comments

Abstract: should probably include mention of the finding that about one third of the trend in northern mid-latitude ozone from 1979 to the early 1990s can be attributed to changes in the regimes. 6185, 27on: what effect does v8 vs v7 have? 6186, 24:

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Typo? 450K was used in Hudson et al (2003) 6187, 5: I do not think that the latitudinal dependence is small. 6187, 7-10 determination of; upper level trends or fronts? I agree with the message of the end of this para, but I am not sure that the intention is carried through. 6187, 25on: presumably this limits the accuracy of the calculated area to 5%, but this does not seem considered later in the paper. 6188, 3 (eg): error of which mean?? 6192, 20on: too many sig figs on DU numbers. Fig 1(b): How much data does this plot include? Just Mar 11? Also, can representative variabilities of total ozone within a regime around a latitude band be shown? Fig 2: I am not sure the purpose of this figure is. And why only 1980-1990? Fig 4, 6: presumably the monthly means have been removed. Need to say for what period

Possible Additional References

Hadjinicolaou, P, J.A. Pyle and N.R.P.Harris, The recent turnaround in stratospheric ozone over northern middle latitudes: a dynamical modelling perspective, Geophys. Res. Lett., 32, L12821, doi:10.1029/2005GL022476, 2005.

Hood, L. and B. Soukharev, Quasi-decadal variability of the tropical lower stratosphere: The role of extratropical wave forcing, J. Atmos. Sci., 60, 2389-2403, 2003.

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Reinsel, G. C., A. J. Miller, E. C. Weatherhead, L. E. Flynn, R. Nagatani, G. C. Tiao and D. J. Wuebbels, Trend analysis of total ozone data for turnaround and dynamical contributions, J. Geophys. Res., 110, doi:10.1029/2004JD004662, 2005.

I. Wohltmann, M. Rex, D. Brunner and J. Mäder, Geophys. Res. Lett., 32, L09811, doi:10.1029/2005GL022497, 2005.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6183, 2006.