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Interactive comment on "Fall vortex ozone as a predictor of springtime total ozone at high northern latitudes" *by* S. R. Kawa et al.

Anonymous Referee #1

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This is an excellent paper that describes a significant correlation between November ozone in the lower polar stratospheric vortex with March total column ozone in the polar stratosphere. The correlation exists on both decadal and interannual time scales. The data sets used are clearly described and the results are clearly presented. Of special interest is the discussion section that adds to the results by describing attempts at finding additional fall/spring correlations.

While March ozone has been examined extensively, less attention has been paid to November ozone. The authors here find that lower stratosphere vortex ozone in November is a predictor of the end of winter March polar total column ozone. Since March polar column ozone depends on dynamics, the November vortex ozone is presumable also a predictor of the coming wave dynamics. It will be interesting to see if this correlation continues to hold over the coming years.

It is also interesting that persistence, the most obvious possible cause of the fall/spring correlation appears not play a role in the correlation. That is, more total ozone in the fall does not imply more total ozone in the spring, only the vortex ozone is correlated, and that persistent dynamics does not appear to play a role in that, strong fall heat fluxes does not imply an active winter. However, high fall vortex ozone should correlate with an active winter along with its associated high spring total ozone.

March column ozone interannual variability is known to depend strongly on dynamics, however, the cause of the interannual variability in November vortex ozone is not as well understood. This paper makes a first attempt at finding the source (or sources) of variability, building on the work of Kawa et al. (2002), and identifying early season wave activity as a likely source of variability.

Another interesting prediction comes from from one outlying point in the correlation that may be explained by Mt. Pinatubo aerosol. The authors make the different effect of aerosol on total column and lower stratosphere clear, so that the uncorrelated points of November 2002 and March 2003 is plausible. However, confirmation of aerosol as the cause of the 1993 outlier will probably have to wait until another large aerosol producing event.

In summary, the authors present an unexplained correlation that should provide a basis for further research: both in understanding the cause of November vortex ozone interannual and decadal variability, and in identifying the mechanism that links the two seasons on these time scales.

Specific Comments:

1) Page 157 line 27: The reference to Fig. 1 here puzzled me. I don't see a direct connection to the text. I guess the point is that 50DU is within about 15-20% of the

total column ozone, while the current percentage trends in Cl and Br are better known, implying that current ozone predictions are not as well understood. I think this could be made clearer. Also, a reference for the 'no one can predict... better than about 50DU...' statement would be good here. I assume it's from the WMO (2003) referenced just before it. In this paragraph references to WMO might benefit from being more explicit, including a chapter, and or section, to back up these more explicit points.

2) Figure 1: I think that somewhere, maybe in the figure caption, it would be good to give a little of the history of the Fig. 1 to let the reader know that this figure is an update of earlier published figures (WMO, 2003, Figure 3-2, in turn updated from Newman et al., 1997) and not completely original to this paper. It may also be good to summarize what is already known about the March ozone interannual variability and the decadal changes in variability from previous work. For example, is this the first time the greater variability in the 1990's in the March total polar ozone has been noted?

2) Page 163 beginning on line 11: The "separate model test" needs more description to relate it to the rest of the paragraph. What is the model? What is the significance of the reported result? How does it relate to the decadal change in March ozone being discussed?

4) Page 164, Line 25: The sentence that starts: "In terms of the analysis presented here the key parameter is dynamical variation...". Does this follow from the paragraph in which chemical loss of wintertime ozone is discussed? This seems to be dismissing chemistry as playing a role in the noted fall/spring correlation, but I do not quite follow why that should be. As stated, dynamics has been shown by Chipperfield and Jones (1999) to dominate over chemistry, but does that imply that chemistry is not a major part of the fall/spring correlation? Maybe the sentence can be re-worded.

5) Page 165, line1: Maybe the statement that "Interannual variability here is relatively small ..." needs elaboration. From Fig. 4, the interannual variability of the November vortex ozone seems similar to that of March polar ozone (˜15% compared to 25%). It's

smaller, but it seems to vary a lot. I may be missing something here.

6) page 166, paragraph beginning on line 24: From context, I would assume that the ozone in the 50-year run was not interacting with the radiation/dynamics. For the off-line run the ozone cannot interact with the radiation/dynamics. The non-interaction is implied later in the paragraph. Still, it might be good to add a sentence to state explicitly that the models do not have interacting ozone. Also, it is implied that the models do not have a QBO. Is this true? This could be clearer.

7) page 167, line 20 and following. The authors point out that tropical easterlies (the QBO) could modulate the wave activity. I think I follow how this could work, so that similar 100 hPa heat fluxes could have different effects on March total ozone, but I fail to see the how this could produce the fall/spring correlation, as stated in the last sentence of the paragraph. Maybe this could be expanded on somewhat.

4) Fig 5. The dashed line is not defined. I assume it is a linear fit. It probably correct, but to my eye, it looks as if a steeper line would fit better. It might be worth while to also show the linear fit when 1993 is neglected. As mentioned in the text, this improves the correlation dramatically (from 0.6 to 0.78) and can be justified by Mt Pinatubo aerosol effects.

Technical Corrections:

1) Anderson and Knudsen, 2002 is missing from the reference list. Hood et al.,1997 and Newman et al., 2002 were in the reference list, but are not found in the text.

2) Page 166, line 16 and throughout the text: "mbar" Maybe it should be "hPa", depending on Journal units policy.

3) Page 162, line 10: "For 24 data points..." Should that be "26 data points"?

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 155, 2005.