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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Symptoms of total ozone recovery inside the Antarctic vortex during Austral spring" *by* Andrea Pazmino et al.

Anonymous Referee #2

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The goals of this paper are described in the introduction: "...to provide an update of ozone evolution inside the Antarctic vortex during the last decades taking into account the vortex baroclinicity. The main aim is to determine the different contributions to ozone interannual variability and to estimate the post 2001 total ozone trend and related significance for different periods: September...and mid-September to mid-October when the maximum ozone loss is reached."

I concur with much of what the other reviewer articulated, in particular these points from DeLaat's review:

1. "The presence of this exhaustive list of issues and questions would be less of a problem if the paper introduced new concepts or new ideas, but the paper mostly builds on previous work and confirms what other papers have also concluded."

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Discussion paper

2. “This paper does not address these issues, nor are results put in the context of this work.”

3. “The few time series that are looked at are then seen as the truth, every wiggle becomes meaningful, and too much attention is given to the formal statistical significances, whereas structural uncertainties are important as well. For example, we have shown that rather arbitrary choices with regard to the proxies used in the regression have a strong impact on the formal statistical trend errors. We therefore argued that structural uncertainties are much larger than the formal statistical trend errors, which is important for confident statements about whether recovery has started or not.”

I especially agree with DeLaat’s concerns about the ‘structural uncertainties’ in this regression analysis, so please address all issues described in his review. In addition, there are other issues below related to ozone data sets that need to be addressed in a revised manuscript. If revisions are made that address both DeLaat’s and my review, this paper could be published in ACP.

Specific topics of Concern

The composite satellite total ozone time series, referred to as SAT. The merging of satellite data sets into a single record is something to be done very carefully. Instrument measurements have bias and drift, and combining data sets in order to extract small trends (i.e., ozone recovery) requires a great deal of care and a good deal of knowledge about each instrument’s characteristics and sampling pattern (i.e., coverage). I see no evidence here that any such considerations were used when combining the data sets. In fact in Figure 4, the difference between the assimilated ozone time series (MSR) and the SAT shows big jumps! There is a large trend from 1990-2005. Does this represent an unphysical trend (i.e., changes in the observing system) in the assimilation, or is this coming from how the individual data sets in the SAT were merged? Have you tried your trend analyses on the 5 merged ozone data sets referenced in Weber et al. [2017]? Without any discussion or justification of how the data sets were merged in this study,

I don't see how the trend results presented here (and especially their uncertainties!) can be taken seriously.

The 'range method' is not clearly explained. I understand that you are using it to see the sensitivity of the calculated trends to the definition used for the area of depletion, and I get that you calculate different areas depending on which isentropic level is used, but exactly how are you deciding which levels to use? Are you averaging over all the 400-600K level results? Only some of them? Do you choose the same range for each year? The details of this methodology were not made clear. It's interesting that in the end you conclude that the 475K results are as good as the other definitions. Is this because this is an altitude where there is some of the most severe depletion? An explanation for this result should be offered.

The satellite instruments used (all UV sensors) do not see to the south pole in early September. The analysis calculated results for the polar region for the entire month of September, but measurements cannot be made at the highest latitudes in early September. Thus the 'September average' will be more strongly weighted by lower latitudes and later September dates. Please describe how the satellites' sampling of the polar area varies over September and what this does to the 'September averaged' quantity. This may impact the meaning of the trend results as they will include more of the late September, higher dynamical variability measurements.

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