

In the revised manuscript “Tracing the second stage of Antarctic ozone hole recovery with a “big data” approach to multi-variate regressions” by *De Laat et al.* most of the reviewer comments were addressed in detail, and the manuscript adjusted accordingly.

However, due to the fact that there was an error in part of the calculations, the main message and many details of the different regression results had to be changed. This resulted in some inconsistencies in the text, where, it seems, some paragraphs have been newly added, but older paragraphs have not been adjusted. I can only recommend publication after a thorough consistency check has been performed, and the following concrete comments have been addressed:

- The abstract is the only part of the manuscript where the place where the numbers “30-60%” (line 25) are stated. These are important numbers and should definitely be mentioned and discussed in the manuscript. It should also be mentioned how the authors determine these numbers.
- The last sentence of the abstract should be rephrased and worded with more care. It is not clear what the words “choices” (line 35) refers to.
- Section 4 (Discussion) is one of the parts of the manuscript where inconsistencies in argumentation seem to have crept in. In the paragraph from line 696 to 700, the authors argue that an increase in time series length does NOT necessarily result in an increased statistical significance for the trends. However, in the paragraph from line 684 to 688, it is stated that “the number of statistically significant trends” increase “with increasing length of the period over with the trends are calculated”. And in the last sentence of this section, this statement is repeated. The authors have to be very clear and precise in their wording to avoid confusion of the readers here, and make sure that their story and message of the manuscript is consistent.
- Line 689 to 695: Especially the last sentence might have to be rephrased. This study is only looking at Antarctic total column ozone in spring, not vertically resolved ozone. The paper by Solomon et al. (2005) clearly shows and discusses that the eruption of Pinatubo had an effect on October ozone mixing ratios at the South Pole and Syowa at 100 hPa and 150 hPa. They also state that “the influence of the volcanic enhancement of ozone depletion is more readily seen in the profile data than in the total column since it has its largest effect at a limited range of altitudes, ...”. While the findings of the study here about the importance of volcanic aerosol for ozone depletion might be correct for total column ozone, it might not be for vertically resolved ozone. (Also in one of the comments to reviewer suggestions, the authors state that “new research (including this paper) combined with longer time series of ozone suggests no influence of volcanic aerosol on Antarctic ozone.” I think this really has to be stated more careful and more precise)
- Line 732 to 735: The “robust gradual small increase” is not significant. Not

- even close for the 95% CI (+1.66+-4.74 DU/yr). I am not sure why it is discussed here as something positive that came out of this study.
- Line 760 to 766: This paragraph needs to be rephrased. It suggests that the reason why the authors argue that regression analysis cannot properly detect ozone recovery because of the lack of a proper time window definition, although the authors stress before that it is the uncertainties on the basis functions (and their combination) that causes the uncertainty in the detection. Also, the phrase “full ensemble” appears somewhat out of the blue here.
 - Line 767 to 769: This sentence is again contradictory to the findings described in Section 4.
 - Line 769 to 772: It is not clear how the authors get to the statement that the second stage of ozone recovery of Antarctic spring time ozone will be detectable before 2020. It is not at all discussed until this point how the findings of the regression method can be used for predicting a year when ozone recovery will be statistically significant. This is a bold statement that has to be explained in detail and its respective analysis has to be shown. If this statement is only a vague guess, then I think it should be removed.
 - Table 5, caption: It has to be mentioned here for which time period the shown numbers are valid. I assume it is for the break year 1999 and period ending in 2000?

Solomon, S., Portmann, R.W., Sasaki, T., Hofmann, D.J., and Thompson, D.W.J.: Four decades of ozonesonde measurements over Antarctica, *J. Geophys. Res.*, VOL. 110, D21311, doi:10.1029/2005JD005917, 2005.