Atmos. Chem. Phys. Discuss., 14, C6015–C6019, 2014
www.atmos-chem-phys-discuss.net/14/C6015/2014/

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD
14, C6015–C6019, 2014

Interactive Comment

Interactive comment on "Tracing the second stage of Antarctic ozone hole recovery with a "big data" approach to multi-variate regressions" *by* A. T. J. de Laat et al.

Anonymous Referee #2

Received and published: 18 August 2014

General comments:

The paper by J. de Laat et al. focuses on Antarctic springtime ozone 1979-2012 and the authors investigate whether or not the second stage of ozone recovery - defined as the occurrence of statistical significant positive trends in ozone - can be detected. A detailed sensitivity analysis of multi-variate regressions based on 35 mio. regressions - taking into account uncertainties in ozone and proxies - is provided. The authors conclude that less than 30% of the regressions in the full ensemble result in statistical significant positive ozone trends.

The paper addresses an important topic, that has not been covered by previous studies. It is mostly well written and within the scope of ACP. However, I have a few concerns regarding the discussion of the results and some minor corrections, which need to be addressed before publication.

Major concerns:

- The motivation of the paper is clear, but the conclusions remain a bit unclear and the main message is hard to understand. I think this is mostly due to some confusing and contradictory statements related to (1) the significance of the trends and (2) the parameters which have a good explanatory power. I would strongly recommend to focus on the first point which is the main aim of your study.

- You conclude that only a minority of all regressions (0-30%) lead to significant positive trends (p.18618,l.20). However, in the next sentence you state that your results are consistent with the second stage of recovery. Please reassess this conclusion. Maybe some more numbers (trends and significance) could be helpful.

- Regarding the EESC, there are a number of statements which seem to be contradictory and which could lead to confusions. For example p.18612, ll. 6-8 "EESC is the better fit model" vs. p.18618, ll. 12-15 "... fitting the EESC should be avoided" and p.18619, ll.9-14 "... the use of EESC ... is problematic". I would recommend a thorough review; try to explain and dispel this apparent discrepancy.

Minor comments + technical corrections:

p.18592, Abstract
I would suggest to tighten the abstract. The first paragraph (except for the first sentence) should be removed. Focus on the the main message of the study.

p.18593, ll.8-9
change to "... in the Montreal Protocol (UNEP, 2012) and its subsequent amendments."

p.18593, l.8

ACPD
14, C6015–C6019, 2014

Please check the reference "UNEP, 2012". The corresponding entry in the list of references starts with "The Montreal Protocol..."

p.18595, l.11
"sensitive to ..."

p.18596, l.13-14
I would suggest to provide the data sources in the main paper, not in the supplement.

p.18597, l.3-5
Please provide a description how the ozone trend is derived from the EESC basis function.

p.18599, l.12
Typo "Kleiciuck" -> "Klekociuk"

pp.18600-18602, Sects. 2.3-2.5
I would recommend to integrate a short description of QBO and SF in Section 2.5 (Mixed SF-QBO). Having three separate sections may lead to confusions.

p.18603, l.8
Define acronym "NOAA".

p.18603, l.9
I would suggest to include a reference for NCEP/NCAR reanalysis: Kalnay, E. et al., The NCEP/NCAR 40-Year Reanalysis Project, Bull. Amer. Meteor. Soc., 77, 437-471, 1996.

p.18607, l.14
"... scenarios defined above..." ? The scenarios are defined in the next paragraph.

p.18607, Section 2.9
Please provide more information about the MSR ozone data record here.

p.18610, l.6

ACPD
14, C6015–C6019, 2014

Interactive Comment

I couldn't find the exact definition of your 'basic regression'. Please clarify.

p.18610, l.13
Do you have an explanation why the uncertainty on the PWLT is three times larger here than in Kuttippurath et al. ?

pp.18611-18612, Section 3.3
The caption of Figure 4 indicates that (only ?) results for break year 1999 are shown. This is in contrast to what is mentioned in the text (flexible break year BREAK). Please clarify.

pp. 18611-18612, Section 3.3
It would be helpful to see some numbers for the trends in this section; at least the mean trends from Fig.4.

p.18612, l.24 and p.18614, l.1
Please check references to Figures in Supplement (S1<->S2)

p.18613, ll.1-5
I would recommend to delete the second part of the sentence, as the study by Chehade et al. is limited to low and middle latitudes.

p.18613, l.8
Typo "wih" -> "with"

p.18618, ll.18-21
I think this sentence need to be rephrased.

p.18621, l.11
Chehade et al. is now published in ACP.

p.18623, l.25
Typo "Stoarski" -> "Stolarski"

p.18630, Table 5, and p.18631, Table 6

ACPD
14, C6015–C6019, 2014

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Both tables indicate that for the ozone scenario 21-30 September (5th row) no significant trends were found, whereas the fraction of significant trends for all other ozone scenarios is larger and varies as a function of the EP scenario. Do you have an explanation why the 21-30 Sep ozone scenario is so exceptional ? Might this be related to the outlier in 2002 (see Fig. 3, top panel) ? The anomaly for 21-30Sep ozone is much larger than for all other scenarios.

p.18640, Figure 9
The dark gray bars (indicating the significant trends) are hardly visible. I would suggest to use a different color.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 18591, 2014.