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Interactive comment on "Spatial regression analysis on 32 years total column ozone data" *by* J. S. Knibbe et al.

Anonymous Referee #1

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1 General comments

This article undertakes a statistical regression on a long term (32 year) global dataset of column ozone that has been generated from the assimilation of monitoring data from various satellite instruments. A variety of established explanatory variables is included in the regression analysis. The analysis is undertaken on two-dimensional i.e. not zonally averaged fields. Two different ways of quantifying the seasonal variation in ozone are used, either via physical explanatory variables or via harmonic functions. In addition, the authors quantify the rate of recovery of the global and Antarctic ozone layer.

The topic is well suited for ACP. The article is well written, and the mathematics used are sufficiently well explained. The mathematical analysis itself seems sound, although I believe that care has to be taken in the interpretation of some results, see detailed comments below. In general, after reading the article, I have two concerns - a) What is really new? and b) Are those parts of the results that do seem new robust? I understand that performing the regression analysis on 2D (+time) fields adds a new aspect, as well as the two different ways of quantifying the seasonal variation of ozone itself. However, after all the detailed analysis, the conclusions are a bit disappointing: Most statements made concern zonal averages, most of them are essentially well known from previous studies – and those that are not (asymmetric response to solar signal?) are not explained. Thus, while I do not see a strong point to reject publication of this paper, I believe that the paper could be improved a lot by critically interpreting the results and emphasising and interpreting the new aspects in the conclusions (possibly reformulating parts of discussion/conclusions). Detailed comments below.

2 Detailed comments

Section 2.2: While the authors correctly state that high correlations between explanatory variables are problematic, the variables used do show strong correlations. It is unclear to me why the choice of explanatory variables is restricted to PV and EP in the SH high latitudes, when EP, GEO and DAY show even stronger correlations in the NH. To be consequent, the restriction should be applied to the NH as well, or at least better motivated why not.

Section 2.3: Is the separation of seasonal ozone variations, described by the physical variables EP, GEO, PV, and DAY (or the harmonics in STAT) from the seasonal response to the non-seasonal explanatory variables of group A unambiguous? For

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example, doesn't the introduction of the harmonic variation of the QBO interfere with the harmonic parametrisation of ozone variability in the STAT model? If not, then a sentence or two would help to clarify here.

p 5335 l 3 ff and Fig 3 top left, also Fig 4 top left: I find it surprising that the EESC response is so low in Arctic spring (and in the Arctic in general). This should be at least mentioned in the paper. How does this go together with the strong catalytic depletion of Arctic ozone in cold winters? Why wouldn't an explicit treatment of springtime Arctic ozone depletion be justified in the NH, as done with EESC2 in the SH?

p 5337 l 25ff, Figures 4 and 6: The zonal asymmetry of the EESC2 (and EESC) regression coefficient is striking, particularly in the STAT model. Is this robust? This should be mentioned / discussed a bit.

p 5338 l 5ff: 1) Why is the solar signal so weak in the area of strongest insolation i.e. the tropics? 2) The strong and zonally asymmetric solar signal at high southern latitudes is indeed surprising. I wonder if this could be an artefact; if we ignore the stretch over Antarctica the picture looks much more credible (and more symmetric) to me. Although EESC and SOLAR should not correlate, just from looking at the neighbouring figures I wonder if there is a compensating mis-attribution between EESC2 and SOLAR?

p 5338 l 25ff, and p 5345 l 18ff: Given the strong correlation between EP and DAY, the attribution to DAY is not so clear to me, especially at high latitudes. While the mathematical regression model does not care too much, physically it does make a difference whether the origin of ozone is in situ chemical production (DAY) or transport (as we could relate to EP), and I'm not convinced that the regression analysis is able

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to distinguish between them. This should be discussed briefly.

p 5340 l 21 ff., and p 5344 l 6 ff: How come this low explanatory power of the PHYS model at high northern latitudes, given that it has a multitude of explanatory variables in, including dynamic proxies which are usually believed to control high latitude O3 in the NH (in connection with EESC loading, see above)?

p5344 l 21ff: It is not clear to me why the age of air that fits best should decrease again from the mid-latitudes to polar latitudes – while at the same time the PWLT method gives results more in line with increasing age of air towards the poles, as one would expect. Thus I wonder whether it is justified to say that the EESC parametrisation has a better performance at high latitudes.

pages 5344ff, Sections 4 and 5: I would suggest re-organising the discussion and conclusion sections a bit. The first paragraph of p5346 re-appears in very similar words in the conclusions (p 5347 l 25ff), this doubling is unnecessary – actually the paragraph in the conclusions gives more information than the one in the discussion. If the authors agree, the discussion could mention some of the points raised above, also mentioning uncertainties (EESC not important in the Arctic? DAY implying in situ chemical production important in the Arctic?). On the other hand, I find the current conclusions section a bit unsatisfactory. The second paragraph contains mostly well known information (aside from the solar cycle statement which remains unexplained), the third is very similar to the discussion and could be shortened, while the fourth is an outlook to future work. I miss some clear conclusions – take home messages – from the current work. What about non-zonal statements that can be made with this spatial analysis? Also, the recovery discussion is not mentioned at all.

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3 Technical corrections

p 5352 l 6: “Furthermore...” This sentence is hard to read, I suggest reformulating it a bit (“which” may be wrongly read as relating to “pattern”)

p 5326 l 17: “chloride” should be chlorine

p 5327 l 21: and all other occasions: Fioletov et al. (2007) should be (2008)

p 5330 l 10: Stiller et al (2008) - missing reference

p5330 l 14: From the results section, it seems that ages of air of 3, 4 and 5.5 years were used?

p 5331 l 1: “accuracy” one too many

p 5333 l 26: This sentence is ambiguous, for clarity I would suggest to say “only PV and EP south of 55S” – provided that I understood it correctly

p 5334 l 2: The values are not consistent with Fig 2 caption

p 5334 l 12: $X_{i,j}$ should be X_j

p 5339 l 10: “DAY en EP” typo

p 5340 l 4: Figure numbers are wrong, I believe Fig 7 should read 6 and Fig 5 should read 4 here

p 5340 l 21: Fig 8 should be Fig 10?

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p 5344 l 2: subsection heading is unnecessary

p 5344 l 21: “have not” - has not

p 5347 l 8: “methodologically” - methodology
p 5348 l 9: “has” - have

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5323, 2014.