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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Decreases in wintertime total column ozone over the Tibetan Plateau during 1979–2017" *by* Yajuan Li et al.

Anonymous Referee #1

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1 Overall Remarks

The manuscript investigates interannual, as well as long-term variations of total ozone columns over the Tibetan Plateau and neigbouring regions. Analysis is based on the Copernicus Climate Change Service data-set, and on SLIMCAT chemical transport model simulations. Direct observational data are not used. Main analysis tool is multiple linear regression. In principle this could provide some new and useful information to ACP readers. However, I found it very difficult to grasp meaningful main messages from the manuscript. In my opininion the manuscript presents a largely un-organized smorgas-board of regression results, which may or may not be statistically significant. Results are taken largely at face-value. I did not see any clear scientific questions to

be addressed, nor any stringent logical arguments towards answering such questions. In a similar fashion, the manuscript does cite a large number of papers, but I never see any coherent line-of-thought, how the present results would add new knowledge to what is already out there.

In my opinion the manuscript needs a major rewrite and re-organization. The authors should first decide on their new and major results and then work out these main messages from their analysis. A much more clear and concise presentation is necessary. Right now, I feel that the most appropriate title for the manuscript would be "Regressions that we did, and correlations that we found, for total ozone data near the Tibetan Plateau". This might be OK for a bachelor-thesis, but certaintly not for an ACP paper.

2 Detailed Suggestions

Abstract: EESC says TP ozone should go up since 1997, PWLT and OLR say ozone is going down or staying constant. Does this difference mean anything? Is it even statistically significant? Is EESC even a good / relevant proxy for TP ozone? Does the GH150 proxy explain some of the ozone trend? Is there a trend in the GH150 proxy (or in surface temperature)? Is that relevant for the ozone trend? Do the SLIMCAT experiments provide more information about the underlying processes than the C3S data? The fact that regression of SLIMCAT data gives very similar results to regression of C3S data does not provide new insight. Both should give very similar results, because underlying metorological conditions are very similar, and SLIMCAT should represent chemistry reasonably well. Does the comparison between 2004 and 2008 in SLIMCAT data provide anything different from comparing 2004 and 2008 in C3S data? All these questions come up when reading abstract and paper. None of them is answered satisfactorily.

Section 2.1: C3S is based on model assimilation of meteorological and ozone observations. It would be very important to check if the results in the paper are also valid for real ozone observations (e.g. from the SBUV or GOME series of satellites, or, even better, from a nearby total ozone station) and for real geopotential heights, e.g. from a nearby radiosonde station. Such data should be added to the manuscript / plots.

Section 2.2: Are the SLIMCAT model results really important for the main messages of the paper? If not, maybe just omit them. To me, it never becomes clear what additional insight comes from the (coarser resolution than C3S) SLIMCAT simulations here.

Section 2.3: I found this section, especially the results part, out-of-place and confusing here. To me, a more logical flow would be to present the total ozone time-series first, then, later, the multiple linear regression and its results.

Figure 3 (especially 3b) and Figure 4: How relevant are these for the main new messages of the paper? Is there much value in a simple linear trend over the entire period? Especially, using and discussing this simple linear trend, to me, creates confusion and mis-understanding for the later use of the more comprehensive regression (which includes the additional proxies). I would suggest to omit both Figures and their corresponding text.

Beginning of Section 4: That would be the right place for the description of the MLR / Equation 2 and for the presentation of Table 1.

Figure 10: How would that Figure look for C3S data? How would it look for a single model year? I would assume that each SLIMCAT year would be very similar for repeating meteorology. Discussion of Fig. 10. From Fig. 10 it is not clear to me what causes what. Especially in JJA, there are really no distinctive features near the TP. If there are signatures of circulation cells, e.g. with locally high / low ozone, these circulation cells (Walker, ENSO) should be indicated (e.g. by arrows) in the Figure. As it is now, the discussion of Fig. 10 is largely speculative / hand-waving. Not very convincing to me.

Table 1, 2. Are these correlations and regressions taken for the individual months D, J,

F, or for the 3-month DJF average. From the Figures it seems like the latter is the case. However, this should clarified in the Table captions.

Table 2 and its discussion: Increasing R or R^2 with the addition of new predictors is quite normal. Every additional predictor is likely to pick up some variance, but this may be random and meaningless. This might be the case for the slightly larger R for PWLT over EESC, because PWLT or 2-Linear-Trends add additional predictors. So the authors should be careful not to overinterpret small changes in R. Try it, take any additional random time series, and add it as a predictor. It will increase the overall R. So the real question is: Is the increase in R significant? There are tests for this (e.g. F-test, or adjusted R). These should be used, and small changes in R should not be over-interpreted.

Table 3: Are the stated uncertainties 1σ or 2σ? Please state. Even with 2σ, many of the discussed difference would, at the most, be borderline significant. With 1σ, uncertainties should be doubled, and nearly all statistical significance would disappear.

Table 4: Again, is the given uncertainty / std. err 1σ or 2σ?

Fig. 8 and its discussion: Does this add anything substantial over what is already known, e.g. from *Fioletov and Sheppard* (2003) or *Holten and Tan* (1980)? If not, does it add anything to the salient main messages of the paper? If not, maybe just drop it?

Overall, I think this manuscript needs substantial revision to meet the standards for publication in ACP. Right now, it is too much of a smorgas-board thesis extract.

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