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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Drivers of variations in the vertical profile of ozone over Summit Station, Greenland: An analysis of ozonesonde data" *by* Shima Bahramvash Shams et al.

Anonymous Referee #2

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The abstract summarizes well the content of the manuscript. It is a regression analysis of a 12-year ozone time series at Summit, Greenland. That's it. There are a number of flaws in this work.

First, the column time series is built using an extrapolation method described as "robust" in the abstract (what does it mean here?). The altitude reached by the balloons does not exceed about 20-25 km during winter (see Figure 3). The only information used to extrapolate to 60 km is climatological. One can extrapolate in various ways, but is still left with climatological values above 20-25 km. Since a significant part of the total ozone column is climatological, some of the interannual variability signal is lost.

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Discussion paper

They are essentially analyzing a vertically truncated time series. Sometimes, there is a hint of contradiction: It is stated that Âń Figure 4 shows that these methods agree well for most of the year”, giving the impression the extrapolation methods give similar results. But, then, “The lack of an absolute reference for stratospheric ozone over Greenland make it difficult to choose which method is best. Therefore, the average of the four methods is used for subsequent analysis in this study. Âż There is no justification or validation about the overall extrapolation. The reconstructed ozone column should be evaluated against independent data, which brings us to another critical point. They justify the use of this balloon dataset by claiming that “during winter; many remote sensing instruments for measuring ozone depend on solar radiation”. However, several satellite instruments do not rely on sunlight, notably MLS. Why not combine/use these well-established datasets directly, or at least use them to evaluate the ozone column reconstructed from climatological extrapolation?

Second, the multiple regression analysis (MLR) is not clear to me. If I have understood correctly, Table 5 shows the fraction of observed variance explained by a number of proxies. No error bars are provided. I keep telling my students that numbers without error bars do not mean much. In addition, one of the hypotheses in MLR is that proxies are not correlated (aliasing issue). The problem is that some of proxies can sometimes be correlated in short time series, even if they are not physically correlated. In addition, some of the proxies used here are physically correlated (even if in a short time series, they might not be), for example VPSC and EHF or the influence of solar variability on high altitude ozone being dependent on the phase of the QBO etc. . .. There is the need for assessing properly the effects of these possible correlations on the results and to estimate error bars. I would recommend not to use the standard errors (not reliable for short time series) but rather a Monte Carlo approach.

Third, the outcome of the study does not warrant publication in a journal like ACP. Let’s have a look at the abstract: “ The monthly mean total column ozone reaches a maximum of about 400 DU in April, then decreases to minimum values between

275 and 300 DU in the late summer and early fall. The partial column ozone values peak at different times between late winter and early summer." There is nothing new about seasonal variations of polar column ozone. " There is a positive trend in the total column that is likely due to increases in springtime ozone, however, these trends are not robust given the short period of record". There is a trend but they don't know whether it is significant or indicative of something. At the end, I was wondering what a trend analysis of truncated 12-year time series at a specific location can bring? If the aim is to detect ozone recovery, why not use more data over the entire Arctic region? Then, the results of the MLR on individual proxies are presented in the abstract: "This analysis shows that the variations in total column ozone are due primarily to changes in the tropopause pressure, the quasi-biennial oscillation (QBO), and the volume of polar stratospheric clouds. The eddy heat flux is also important for variations in the partial column ozone in the different altitude regions." Again, it is not possible to see whether the results obtained at this station are significant because there are no error bars, no such results (with error bars) presented for other stations, no comparisons to satellite-based studies or ot other studies, explanations about what the results mean physically. The last sentence illustrates the level of analysis: "The importance of the QBO appears to be a unique characteristic for ozone variations over the Greenland Ice Sheet (when compared to other nearby Arctic Stations) and may be related to the fact that Greenland is particularly sensitive to the phase of the QBO." What does "unique" mean here when compared to 2 other stations only (whose results are not presented and discussed thoroughly)? For the authors, the fact that the QBO is an important driver in the LMR model "may be" related to the fact that Greenland ozone is sensitive to the phase of the QBO. It is self-explanatory, and a bit circular. The authors seem to doubt the MLR results. If ozone was not sensitive to the QBO, it should not appear as significant in the MLR analysis. And, vice-versa. The bottom line is that it is not possible to be conclusive with an MLR over a short time series at a single specific station. This cannot tell us anything new and "robust" about polar ozone.

Fourth, there are too many unnecessary details provided in the text. Everybody knows how to calculate an ozone column from a profile, this could end up in an Annex. Some of the explanations are unclear and longwinded, some parts need to be rephrased. All the authors should (re)read very carefully the entire manuscript. In summary, as it stands, I find it very difficult to be positive. I find the manuscript very far from being acceptable for ACP. Even after tackling the flaws in the methodology, I can't see what the results can bring in terms of new knowledge.

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