REPLY TO REVIEWER #2

The paper is well structured and written, the conclusions are supported by the analysis of the data presented and therefore the paper can be accepted for publications in ACP after considering my comments below:

Thank you very much for this appreciation and also for the time spent for this evaluation.

1.Section 2: Since the authors analyze a large number of data sets with different characteristics, a summary table would help the reader to have a clearer view. As it stands now is rather confusing.

Done. Please find Table 1 and lines 162—168.

2.Section 2.3. Is there any reasoning why the authors use QBO10 and QBO30 and not QBO50 and QBO30. Please add an appropriate comment.

QBO at 10 hPa was selected to represent the changes in the middle stratospheric ozone, as we are finding the ozone trends in the whole stratosphere (note that some studies use QBO at 50 hPa to study the variations near tropopause). In addition, most studies that deal with vertical trends consider these two QBO (30 and 10 hPa) indices. Therefore, in order to compare with previous studies (e.g. contributions of proxies), we decided to keep QBO10 and QBO30.

However, we have also performed the regression analysis with QBO50 and QBO30. The resulting trend values are very similar to that with QBO10 and QBO30, where the differences are within 2%. This has been mentioned in the text in lines 357—363.

3.Section 3.1. What is the added value to merge SAOZ and Dobson data? Does this improve the temporal coverage of the time series? Are there systematic differences between the two instruments? How are these treated?

As both column instruments are at the same place, we averaged those data to make the data more representative and robust. In addition, the merged data also cover an extended time period than the single instrument measurement record, which is important for the regression analyses, as the PWLT trend values depend on the data record too. There are some minor differences between both data sets. This has been mentioned in the text in lines 245—255.

4.Why the authors stop their analysis in 2010 and do not include 2011 a very interesting year for ozone in the Northern Hemisphere?

Our regression analyses coincide with the availability of the proxy data (e.g. heat flux) and we do not have some of them to extend the analysis. Therefore, we could not extend the data sets for 2011. In addition, we badly need at least another 7-8 months for revising the analyses including years 2011—2013, which is well beyond the time limit of a revision deadline. Therefore, we will present the extended analysis results in a future publication.

However, in order to check the robustness of the method and estimated vertical trends, we have analyzed another data set (GOZCARDS) for the revised version. Analyses with the new data also yield very similar vertical trends in ozone for both periods with slight differences in trend values derived using PWLT functions in the middle stratosphere over 1997—2010. We hope the reviewer will find it as an appropriate decision. Please find the new Figure 8 and the text in lines 525—571.

5.Page 7089 Line 21. As it is written the reader understands that Pinatubo generally explains about 10 DU of the ozone variability even many years after its eruption. Please rephrase.

Done. Please find the revised statement in lines 302—304.

6.Page 7090. Lines 1-3. Are these differences in July and August significant? Is there any explanation?

No, these negative trends are not significant. Since the higher PWLT during the post-EESC maximum period can be attributed to the dynamics (e.g. WMO, 2011), the lower dynamical drive during the summer months can be the reason for the negative trends in July and August. This has been mentioned in lines 315—323.

7.Page 7090, line 19. "It suggests the influence of other parameters". This statement is too generic. The authors should mention what could be missing from their analysis and eventually have impact on their results.

We mean the dynamics, although much of this is considered through the heat flux. This is why there are differences between the trend estimates from PWLT and EESC functions during 1997—2010. On the other hand, we have used most proxies that could influence the ozone change. It is unlikely that any other (known or unknown) proxy data could change the results drastically. Maeder et al. (2007) has already discussed this issue in detail. This has been mentioned in lines 347—352.

8.Section 3.2. It is not very clear how the authors combined the results of the analysis of the different instruments, which cover different periods and different layers. At the end they present results that correspond to layers, so it is not clear how the synergy of the different measurements has been applied. Please be more specific.

In order to estimate the trends, we needed a long-term data. Therefore, we constructed such a data set using satellite and ground-based measurements for the period 1984—2010 in accordance with the then availability of measurements. The method and synergy are given in detail in Nair et al. (2012). We have exempted a detailed description of this procedure in this manuscript to avoid repetition. However, a concise text is also presented in lines 394—401.

Reference:

Maeder, J. A., Staehelin, J., Brunner, D., Stahel, W. A., Wohltmann, I., and Peter, T.: Statistical modeling of total ozone: Selection of appropriate explanatory variables, J. Geophys. Res, 112,D11108, doi: 10.1029/2006JD007694, 2007.