Answer to Reviewers Comments for “Response of stratospheric water vapor and ozone to the unusual timing of El Niño and QBO disruption in 2015–2016” by Mohamadou Diallo et al.

Dear Editor-in-Chief, Karen Rosenlof,

We are submitting our revised article titled “Response of stratospheric water vapor and ozone to the unusual timing of El Niño and QBO disruption in 2015–2016”. We thank the two Reviewers for their detailed and well thought-out comments, which helped to significantly improve the paper. We have made substantial changes to the manuscript in order to thoroughly address the Reviewers’ suggestions and comments. Main changes concern:

- an additional new figure 5 describing the fraction of variance of the deseasonalized time series that’s captured by QBO and ENSO in the manuscript as suggested by Reviewer #2
- adding a new paragraph in the discussion as suggested by Reviewer #2
- rephrasing of certain paragraphs in order to clarify the manuscript.

With these changes, we are convinced that the paper is highly relevant for a wide-ranging journal like *Atmospheric Chemistry and Physics*. Please see below our answers point by point to all reviewers comments and suggestions.

Reviewers comments are in bold, followed by our respective replies. Changes in the manuscript are in blue, allowing them to be tracked easily.
Kind regards,
Mohamadou Diallo (on behalf of the co-authors)

Anonymous Referee #2:

Major comments:

1. ***In Sec. 2, two previous studies examining ENSO and QBO effects on stratospheric water vapour are cited, Avery et al. 2017 and Tweedy et al. 2017, which came to contrasting conclusions i.e. the combined roles of ENSO and the QBO. This study has the same goal, and reaches a conclusion that seems closer to Tweedy et al. 2017 (that the QBO had a dominant effect on water vapour following the QBO disruption). But it isn’t clearly described how the current study differs in its approach from these previous two. Is it the use of MLS data? The multiple regression approach? Please clarify what is distinct about this study and how it builds on the previous ones. Some more detailed discussion of how the results compare to the previous studies might also be appropriate in the Discussion section.***

   The two previous studies (i.e. Avery et al. (2017) and Tweedy et al. (2017)) focus on ENSO and QBO, respectively. Combining observations and trajectory calculations with a simple linear regression analysis, Avery et al, (2017) focus on mainly the impact of ENSO on lower stratospheric water vapor and on convective cloud ice. They did mention that a ”simple regression of a 70 hPa shear-based QBO index with MLS water vapour data at 82 hPa shows that the QBO can account for only 0.1 ppmv of the observed zonal mean water vapour anomaly”, but the QBO impact on lower stratospheric water vapor is largely overlooked in this study. Regarding Tweedy et al. (2017), they focus on the impact of QBO disruption on lower stratospheric water vapor using MLS and MERRA data combined with a composite (correlation) analysis. Even though, we use similar observations of water vapor and ozone from MLS, our study differs from these two studies in terms of the method. We use a multiple regression with a lag that takes into account the history of the predictors. Therefore, our regression approach carefully disentangles the impact of ENSO and QBO signals on lower stratospheric trace gases, including water vapor and ozone. In addition, our analysis focus on the global lower stratospheric water vapor response to ENSO and QBO (310-600K), while the two previous studies mainly focus on tropical water vapor time series response at 82 hPa. We have added more specific comments about the method used in the two previous studies in section 2 and a new paragraph recapping the studies of Tweedy et al. (2017) and Tweedy et al (2017) in the discussion section.

2. ***In Sec. 3, random and systematic uncertainties are quoted for the MLS data that seem similar in size to the regression signals reported here (note also the p4, line 5 comment below). It is also noted (p4, line 3) that unrealistic values in the low-latitude UTLS were a problem in previous versions of the MLS data, which sounds worrying since that is the main region of focus in this study. I'm not sure how to compare the reported uncertainties to the regression values. Are these random uncertainties that are applicable to single measurements, such that the regression would effectively beat down the noise? Are there systematic offsets (biases)? More discussion of what these values represent and how they could affect the results would be useful here. It's good that a number of references for data quality are provided (p4, lines 7-9), but a concise explanation of why the regression results in this paper should be believable should also be provided here.***

   We were not precise about the unrealistic values. According to Livesey et al. (2008), these unrealistic values in the tropics were a problem at 215 hPa in MLS v2.2 for O_3 and CO data, which is below the region of interest in this study. We have corrected it in the manuscript. The reported systematic uncertainties are provided by MLS quality control report of the version 4.2 data set (Livesey et al. 2017; Santee et al. 2017) and are applicable to individual profile measurements with a spatial representativeness of 200-300 km along the orbital-track line of sight. The regression results are not affected as these intrinsic uncertainties are applied on the H_2O and O_3 mixing ratios and not the anomalies as the random errors are not relevant for the averaged monthly zonal mean H_2O anomalies used in this study. Hegglin et al. 2013 show that Aura-MLS zonal monthly mean H_2O data show very good to excellent agreement with the Multi-Instrument Mean (MIM) in comparison between 13 instruments, throughout most of the atmosphere (and also throughout tropical UTLS) with mean deviations from the MIM between +2.5 and +5% only. In addition, MLS data is one of the best global coverage observations that we currently get and is widely used. We have corrected it in the manuscript (Page 4, Lines 20-26).

3. ***In Sec. 4 (p5, line 30) it says that the differencing of residuals gives results similar to direct calculations. In that case, why not just do the direct calculation? Perhaps the lead author's previous work explains this, but a concise explanation should be given here. If there's an advantage in doing it this way, what is it?***

   There's not a specific advantage to doing it in this way. We have previously proven that the two methods are consistent (Diallo et al. 2017), therefore, we use it as we do not need to reconstruct the time series after the regression.

Comments by page & line number:

1. ***p2, 1: "This moistening" - are you referring to methane oxidation?***

   "This moistening" refers to the moisture transport from the troposphere into the stratosphere. We have rephrased it.

2. ***p2, 22: On p9, line 15, you say that easterly shear in the tropical lower stratosphere speeds up the shallow branch of the Brewer-Dobson circulation. But here you say that westerly shear is associated with enhanced poleward transport. These seem to contradict each other, please clarify.***

   The QBO modulation of the BDC includes enhanced tropical upwelling during the easterly phase, while the westerly phase reduces the tropical upward motion, but enhances the horizontal mixing and transport toward the polar regions (e.g. Plumb and Bell, 1982; Trepte and Hitchman, 1992). We have rephrased it to improve the clarity.

3. ***p2, 26: "A major" –> "Another major"***

   We have rephrased it.

4. ***p2, 30: Here it says that El Nino cools the lower stratosphere, but you go on to say (p3, line 16) that El Nino warms the tropopause. Please add some additional comments here to explain the distinction between the tropopause response to El Nino and lower stratospheric response. As shown in Mitchell et al 2014 (Signatures of naturally induced variability in the atmosphere using multiple reanalysis datasets), Fig 15b shows tropospheric warming and stratospheric cooling with a node near the tropopause. How robust is the tropopause response to ENSO? If tropopause warming is a distinct regional feature of the ENSO response (you go one to discuss the distinction between zonal-mean and regional responses to ENSO), this would be a good place to introduce and describe those differences.***

We have rephrased these sentences and added a discussion of the distinct ENSO effect on the zonal mean and regional temperature anomalies. Please see page 2, lines 34-35 and page 3, lines 1-4.

5. ***p3, 25: "contains" –> "describes"***

   We have rephrased the sentence.

6. ***p4, 5: Why are O3 uncertainties give as percentages but H2O uncertainties are given in ppmv? Since the figures show O3 and H2O changes in percent, percentages for all these uncertainties would be useful.***

   The O3 and H2O uncertainties are just the values provided in the literature by the MLS team as reported in Santee et al., 2017.

7. ***p4, 21: Unclear what "properly" means here, suggest delete it.***

   We have deleted it.

8. ***p5, 5: What does "sorted out" refer to? Please be more specific.***

   "sorted out" meant "selected from a range of possible values". We have rephrase it.

9. ***p5, 11: Not sure what "breaking the easterly-westerly phase asymmetry" refers to in this context.***

   We mean the breaking of the regular cycle of easterly-westerly. We have rephrased it.

10. ***p5, 21: Insert "as expected" before "due to", since the upwelling is not actually observed.***

    We have rephrased it.

11. ***p5, 31: "basis functions" - do you mean the predictor time series (indices)? Please clarify what is meant here.***

    Yes, we mean by basis function the ENSO and QBO predictor time series. We have rephrased it.

12. ***p6, 8: "controlling" –> "warming"***

    We have rephrased it.

13. ***p6, 14: Insert "as expected to be" before "due to" (for same reason as the p5, line 21 comment).***

    We have inserted it.

14. ***p7, 9-10: If the H2O anomalies are delayed, how can they be in phase with the O3 anomalies? Perhaps say "roughly in phase", if this is what you mean.***

    We have rephrased it.

15. ***p7, 13: "by enhancing" –> "consistent with"***

    We have rephrased it.

16. ***p9, 4-5: Based on eyeballing Fig 4, I think I agree. But could this conclusion be made more quantitative, e.g. by saying what is the fraction of variance of the deseasonalized time series that's captured by QBO and ENSO? Or plotting the residual of the full regression in the same style as Fig 4? (Perhaps to include as supplemental so as not to clutter Fig 4.)***

    Thanks for this good suggestion. We have added a new figure describing the fraction of variance of the deseasonalized time series that's captured by QBO and ENSO in the manuscript. This figure confirms that the dominant part of variability in the water vapor anomalies is induced by QBO and ENSO, with QBO contributing the largest part. Please see Page 9, lines 21-25.

17. ***p9, 7: "controlled by" –> "dominated by" seems more appropriate to me since the responses here are linear by definition (because multiple linear regression has been used to diagnose them).***

    The analysis is based on multiple regression with a lag, allowing the history of the predictors to be taken into account in a manner not possible with a simple linear regression. The method becomes linear only after the optimal lag is found. We have rephrased it.

18. ***p9, 9: "predominated" –> "dominated"***

    We have rephrased it.

19. ***p10, 8: Not clear what "robust" means in this context; suggest delete it.***
    We have rephrased it.
20. ***p10, 18: "led positive" –> "led to positive"***
    We have rephrased it.
21. ***p10, 29: "turn out to be" –> "are"***
    We have rephrased it.