We thank Mark Schoeberl for the very helpful comments and suggestions. Here are our replies with which we hope to clarify some misunderstandings.

- **General reply**: Reading the comments of the three referees, we have the impression that we probably raised wrong expectations with respect to what we achieved with our study. All three referees agree in revealing weaknesses in our argumentation, which we believe can be explained and therefore eliminated:

  - Part of the problem is probably caused by misunderstandings (maybe due to unclear formulations), as for instance the precise definition of "drop" (sudden decline versus period of low values thereafter), or the presumable influence of "nudging" as we applied it.
  - Another, more severe part, is in fact caused by the lack of important information, which we erroneously hold back (mainly to shorten the manuscript), although it was found by our analyses. Most importantly, we did not put sufficient emphasis on our simulation RC1SDNT (i.e., nudged, but without mean temperature nudging), which was only mentioned briefly, but not discussed in full detail.

  We hope that we can clarify our findings with additional information and revision of the text, where the misunderstandings occur. The details about that are outlined in our point-by-point replies below.

- *General comments: (1) The authors are not native English speakers and thus the paper needs some through grammatical editing and improved organisation. For example, the first sentence is almost incomprehensible. Long sentences are common in German, but are considered bad grammar in English. A better first sentence would be: Since the early 1980's climate models have predicted a increase in stratospheric water vapor [refs]. Satellite and balloon borne measurements have not yet observed such an increase [refs].*

  Reply: We will shorten the first sentence of the introduction in the revised manuscript.

- *(2) A key missing reference in this work is 'Dessler, A.E., M.R. Schoeberl, T. Wang, S.M. Davis, K.H. Rosenlof, and J.-P. Vernier, Variations of stratospheric water vapor over the past three decades, J. Geophys. Res., 119, doi:10.1002/2014JD021712, 2014.' Reprint This work describes how the QBO, diabatic heating, and the tropospheric temperatures can be use to successfully model stratospheric water vapor including the year 2000 water vapor drop. The implication of this work is that unless models get the three components that contribute to the drop correct, they will not be able to simulate the drop. This paper is extremely relevant to this work, and it is somewhat surprising that the authors were unaware of it.*

  Reply: Thank you for the hint. In the above mentioned study, Dessler et al. use reanalysis data to drive a trajectory model and apply a regression analysis. Here, we simulate stratospheric water vapour with a chemistry climate model. In our revised manuscript, we will refer to Dessler et al. where appropriate.

- *(3) I think that the model discussion (Section 2.1) is totally confusing - at least to someone outside the CCMI world. I feel like a diagram of what models are being used for what components of the simulation. There are references to hindcast simulations, nudging to ERAI, etc. The model sounds like a pile of components - which does not give me confidence in its veracity - nor does its rather poor simulation of the tape recorder (see below).*

  Reply: We are using only one model: The Chemistry Climate Model EMAC. EMAC, as any other model of this complexity, is build of several different components, which are not even mentioned here (see for instance Jöckel et al., 2010[1] for such technical details.) Thus, we are a bit surprised by this comment. Moreover, we stress that EMAC is widely used and has been extensively evaluated (e.g., Jöckel et al., 2015[2]; Righi et al., 2015[3]; Eichinger et al., 2015[4]; Pozzer et al., 2007[5] ; Jöckel et al., 2006; etc.).

  Also it is unclear to us, what you mean with "components of the simulation". As we state in the text, we analysed and compared the results of 4 different simulations, all performed with EMAC, however with different external forcing and with different boundary conditions.

  Nevertheless, we agree that the different simulations could be summarised better in a Table, which we will include (and referred to) in the revised Section 2.1.

  Concerning the apparent "poor tape recorder signal" you will find our reply below.

[1] Jöckel, P., Kerkweg, A., Pozzer, A., Sander, R., Tost, H., Riede, H., Baumgaertner, A., Gromov, S., Kern, B.: Development cycle 2 of the Modular Earth Submodel System (MESSy2), Geoscientific Model Development, 3, 717752, doi: 10.5194/gmd-3-717-2010, URL http://www.geosci-model-dev.net/3/717/2010/ (2010)

[2] Jöckel, P., Tost, H., Pozzer, A., Kunze, M., Kirner, O., Brinkop, S., Cai, D. S., Frank, F., Garny, H., Gottschaldt, K.-D., Graf, P., Grewe, V., Kern, B., Matthes, S., Mertens, M., Meul, S., Nützel, M., Oberländer-Hayn, S., Ruhnke, R., Runde, T., and Sander, R.: Earth System Chemistry Integrated Modelling (ESCiMo) with the Modular Earth Submodel System (MESSy, version 2.51), Geosci. Model Dev., accepted 2016.

[3] Righi, M., V. Eyring, K.-D Gottschaldt, C. Klinger, F. Frank, P. Jöckel, and I. Cionni, Quantitative evaluation of ozone and selected climate parameters in a set of EMAC simulations, Geosci. Model Dev., 8, 733-768, doi:10.5194/gmd-8-733-2015, 2015.

[4] Eichinger, R., Jöckel, P., Brinkop, S., Werner, M., and Lossow, S.: Simulation of the isotopic composition of stratospheric water vapour Part 1: Description and evaluation of the EMAC model, Atmos. Chem. Phys., 15, 5537-5555, doi:10.5194/acp-15-5537-2015, 2015.

[5] Pozzer, A., Jöckel, P., Tost, H., Sander, R., Ganzeveld, L., Kerkweg, A., Lelieveld, J.: Simulating organic species with the global atmospheric chemistry general circulation model ECHAM5/MESSy1: a comparison of model results with observations, Atmospheric Chemistry and Physics, 7, 25272550, doi: 10.5194/acp-7-2527-2007, URL http://www.atmos-chem-phys.net/7/2527/2007/ (2007)

- *(4) The main point of this paper is to show that ENSO, combined with the west phase of the QBO can product sufficient upwelling (cold temperatures in the UTLS) to produce the water vapor drop. The 'event analysis' appears to support their conclusions - at least with the nudged model. The main problem I have is that there are other water vapor anomalies not associated with ENSO (for example the most recent). By not analyzing these events, their conclusion is foregone. This problem can be repaired if the authors analyze some drop events not linked to ENSO and compare them with ENSO linked events.*

  Reply: ENSO, through SST changes, triggers upwelling, which is then followed by a strong decline of water vapour when the La Niña event decays, and if the QBO coincidently changes its phase from west to east. This finding is not new (Dessler et al., 2014). This also holds for the "most recent event" (you refer to) in our time series (see for instance Figure 11). Thus, we think that you misinterpreted our text and we will revise it accordingly.

  Yet, we see your point to mention those anomalies not associated with ENSO: they do not exhibit large amplitudes of water vapour: This is for instance the case after year 2000. Here, we have a moderate El Niño in 2002/03, but no La Niña and therefore no large drop. Similarly, the period 1977 to 1981 of the simulation RC1 shows no ENSO and therefore no large drop.

- *Specific comments: (pg:line) 2:1 Water vapor is an important greenhouse gas in the troposphere. It isn't so clear it is as important in the stratosphere. I would delete this sentence and instead reference the Solomon et al. [2010] paper showing the impact of stratospheric water vapor on the surface radiative balance.*

  Reply: Since our analysis does not include tropospheric water vapour and its greenhouse effect, we will indeed remove the corresponding sentence. However, it is beyond dispute that stratospheric water vapour is important for the stratospheric temperature (Forster and Shine, 1999[6]; Grewe and Stenke, 2008)[7] and has a strong influence on the surface radiation balance (Solomon et. al., 2010).

- *2:12 The analysis by Dessler et al. shows that the to reproduce the water vapor field, you need the QBO, among other things. This means that models that fail generate a QBO (which is most of them) will naturally fail in generating the water vapor time series. I note that this model does include a QBO (4:27) which is good.*

[6] Forster, P. M. and Shine, K. P.: Stratospheric water vapour changes as a possible contributor to observed stratospheric cooling, Geophys. Res. Lett., 26(21), 3309-3312, 1999.

[7] Grewe, V. and Stenke, A.: AirClim: an efficient tool for climate evaluation of aircraft technology, Atmos. Chem. Phys., 8, 4621-4639, doi:10.5194/acp-8-4621-2008, 2008.

Reply: Yes, indeed. Thanks to Giorgetta et al., 2002.[8] We will add this information.

- *4:22 What hindcast simulations? - what are you talking about here?*

  Reply: A hindcast simulation covers the past, in contrast to a forecast simulation, which projects the future.

- *4: I think it would be helpful to have a table describing the models and their differences (RC1SD, RC1SDNT, RC1, RC2) that show up in Figure 4 as a quick reference.*

  Reply: We will include such a table in the revised manuscript (Section 2.1). Note, however, that RC1SD, RC1SDNT, RC1 and RC2 refer to different EMAC simulations, not to different models.

- *5:2 I object to the words 'model data' better is 'model output'*

  Reply: Maybe even better "model results" depending on the context. Will be corrected.

- *5:22 Fair point!*

  Reply: Thanks.

- *6:5 What is 'It' - the chart, the measurements, the curve?*

  Reply: The measurements. Will be corrected.

- *6:8 'specified dynamics' - below you call this 'EMAC .. nudged mode' I think you should be consistent, or I am not understanding something.*

  Reply: "Specified dynamics" is the more general term (introduced by CCMI), of which "nudging" (more precisely Newtonian relaxation) is one methodology. This will be explained in the revised Section 2.1. Moreover, we will use "nudged mode" in conjunction with EMAC throughout the revised text.

- *6:28 The model average (in Gettelman et al., 2009) is cold biased but only by about a degree. More models are above the model mean than below - and the spread is large - almost 10K. I think that the text could be more precise.*

  Reply: We will modify the text to include the additional information.

- *7: I think that it is obvious that the cold tropopause temperature anomaly reduces water vapor and that signal propagates into the stratosphere (a bit asymmetrically). Something like this statement would be a nice way to summarize the discussion of Figure 3. Figure 3. Please put titles on the individual figures.*

[8] Giorgetta, M. A., Manzini, E., and Roeckner, E.: Forcing of the quasi-biennial oscillation from a broad spectrum of atmospheric waves, Geophys. Res. Lett., 29(8), doi:10.1029/2001GL014756, 2002.

Reply: We will add a summarising sentence to the end of Section 3 and put titles on the individual figures.

- *8:11 How does it point to a shift in 'temperature relevant processes'? What are you talking about here? Why does this point to ENSO? I think something is missing here.*

  Reply: What we mean is simply: Between the different simulations (here RC1 and RC1SD) we see a small phase shift of those processes, which affect temperature (i.e., QBO, upwelling (due to ENSO), ozone). As a consequence the time of occurrence of the drop is also shifted accordingly between the two simulations. Since this is trivial, we will omit the sentence in the revised manuscript.

- *8:25 Please provide a reference to this statement 'propagates. . . lower stratosphere' Maybe reference Calvo et al. 2010?*

  Reply: We will add adequate references for the ENSO effects on the stratosphere. (Scaife et al., 2003, Randel et al., 2010, Calvo et al., 2013).

- *9:25 I think it is obvious from Figure 6 that the causal relationship is weak to non existent. Why all the discussion about it?*

  Reply: Figure 6 shows the relationship between ENSO (vertical bar = major phase of El Niño) and subsequent large drop (red asterisks). So, unfortunately we do not get your point.

- *9 Okay, I get that RC1SD has problems for Mt. Pinatubo and I also get that you should see a surface temperature anomaly for (Fig. 7) - I think that the authors can just state this rather than waste time discussing it. Fig. 7 only shows that RC2 is not useful - which the other figures already indicate.*

  Reply: We will reduce this paragraph as suggested. For the revision (see our replies to the other referees) we will improve our argumentation for which we also need the SST time series of RC2. Thus, we need to keep it.

- *9:26 I am confused by this sentence. Are you now saying that there is no relationship between ENSO and water vapor drops?*

  Reply: No. The sentence is indeed misleading. What we want to say is: In the RC2 simulation (forced with simulated SSTs) large drops occur, however, a connection with a preceding ENSO event as analysed from the observations and from the other simulations is not as clearly visible .

  Furthermore, the correlation between upwelling and temperature is weaker in RC2 (compared to the other simulations, see Table 1). The reason are the different horizontal SST patterns which are not as those observed. This affects the dynamics, e.g., stratospheric winds and thus wave propagation. We will clarify this in the revised text.

- *10:7 You should also reference the Dessler et al. paper noted in the general comments here.*

  Reply: Will be done.

- *10:14 There are also positive anomalies in non ENSO years such as 1976, 2000, 2006. Figure 8 Please title the figures. Figure 8 - the tape recorder signal looks funky to me. The amplitude is way too weak. The authors need to comment on this or provide more explanation. It would be useful to produce the observations in Figure 9 along with the model runs. I am attaching a tape recorder figure. The amplitudes is a lot larger.*

  Reply: 1976 and 2000 are ENSO years, more precisely La Niña years. To avoid a misunderstanding, we will replace "ENSO year" by either "La Niña year" or by "EL Niño year" as appropriate throughout the text. 2006 is indeed no ENSO year. The upwelling is influenced by other processes. We will clarify in the revised text that positive anomalies are not solely caused by SST changes in the tropics.

  We think that panel a) and b) together with the caption are sufficient.

  Tape recorder: This is a misunderstanding. The tape recorder signal is well reproduced by our model (see Eichinger et al, 2015). In our Figure 8, however, we de-seasonalised (i.e., subtracted the seasonal cycle) in order to show the anomalies to illustrate the inter-annual variations. The revised caption will be improved.

- *10:21 Ozone anomaly? Where is this shown?*

  Reply: We have no additional figure for ozone. But the negative correlation between upwelling and ozone is stated in the manuscript.

- *10:25-30 Just use correlation coefficient (not Pearson's correlation coefficient). These correlations aren't very strong.*

  Reply: This comment is in contradiction to one of the comments by Stephan Fueglistaler, who asked us to present also the formula. Thus, we must name it correctly.

- *10:29 decreases at lower altitudes.*

  Reply: Will be corrected.

- *11:4 also see Schoeberl et al. , J. Geophys. Res, 113 Doi: D24109, 2008 for vertical velocity calculations using the observed tape recorder.*

  Reply: We will add this reference.

- *11:14 Basically what the authors are saying here is that they assume that all these other processes (like convection) and that the water vapor drop is primarily tempera- ture driven. However, during ENSO there is a significant collapse in western Pacific convection which provides about 30% of the water vapor to the UTLS. Thus I don't think that it is a good idea* *to assume that convection is unimportant. In any event, I presume these models include convective processes and water vapor transport so that they are probably included in the correlation.*

Reply: We do not assume that convection is unimportant. Actual, in our simulations the effect of convection on the TTL temperature and moisture is included. What we wanted to say is that the contribution of directly injected water vapour and ice particles into the stratosphere and thus their contribution on the water vapour concentration is not considered in our analysis.

- *1:21 But there are strong upwelling periods not near ENSO .. so the data doesn't quite fit the hypothesis. In fact the biggest (model) upwelling is in 2005 in Fig. 9 - nowhere close to an ENSO event.*

  Reply: We only say that strong SST anomalies trigger increased upwelling. But we never stated the reverse argument, namely that every increased upwelling was necessarily triggered by an SST anomaly in the tropics.

- *11:30 The episode analysis described in the next few pages and in the Figures 10 - 14 but I have to wonder if the results are also true for drop episodes not associated with ENSO. For example, just pick a period of temperature decline not associated with ENSO. The authors are selecting events and making an assumption that the water vapor drops are all produced by the ENSO events - the correlation is weak it seems from the previous figures. While ENSO events appear to contribute, they are just one component of the whole system producing the drop such as the phase of the QBO.*

  Reply: That is an interesting point. We expect that we will not find the QBO anomaly changing from positive to negative values, and a clear signal in upwelling anomaly together. We will perform this analysis for the revision.

  In the context of the "assumption" you mention, it is unclear what "drops" you refer to. Just to clarify: The large drops (sudden decline of water vapour) we selected are all preceded by a strong El Niño/La Niña event, as outlined also above. However, we do NOT claim or assume, that no other trigger is possible which also can cause steep declines ("drops").

- *13:31 So you conclude that it is the coincidence of ENSO upwelling and west phase of the QBO that produces lower temperature anomalies and the water vapor drop. Yes?*

  Reply: Yes, in case "ENSO" means "La Niña" with reduced upwelling, and "west phase" means "transition from west to east phase". We will make the formulation more precise in the revision.

- *14:1 what level is Figure 15 plotted for, 80hPa? What are the units, vertical velocity? Figure 16. The 72/73 case sort of blows your hypothesis since the lag is 36 months - longer than a QBO cycle.*

Reply: Figure 15 is for 80 hPa for the upwelling as written in the figure title. We will add the information also to the revised figure caption.

The normalised upwelling anomaly is calculated by division of either the maximum or the absolute value of the minimum. For the SSTs it is defined accordingly. Therefore, the results are dimensionless. We will add this definition to the revised figure caption.

El Niño usually starts a period in which upwelling correlates strongly with the SSTs (not in all cases, see e.g., 1982/1983). This strong correlation lasts until La Niña ends. Thus, the water vapour maximum is reached with the maximum of La Niña. The length of the ENSO period (start at El Niño until maximum of La Niña), however, shows large variations, roughly between 1.5 and 3 years. Conditions for a large drop are therefore only given at the end of this period, i.e., at the maximum of La Niña. This period we call "lag" and therefore most probably caused a misunderstanding, since "lag" implies a cause-to-effect relationship, which we do, however, not claim. In the revision, we will eliminate the word "lag".

- *15:1-14 RE SSTs – more convection – more waves. How is that relevant to this study?*

  Reply: This paragraph describes how the SST signal propagates into the tropopause region (namely by convection triggering waves which break and cause upwelling). Thus, it provides background information, but is not directly relevant for our analyses.

- *15:17 First use of UTLS - please define.*

  Reply: Will be corrected.

- *15:24 Please provide a reference to ' cold point is slightly too high' - how do we know this. How would too high a cold point lead to reduced variability (next sentence) I can see that it might lead to an overall bias but not reduced variability.*

  Reply: The reference to Gettelman et al.,(2009) will be added here (note that it is already cited in the text). The variability is affected by the absolute value due to the non-linearity of the Clausius-Clapeyron relation.

- *16:12-15 What are you trying to say here?*

  Reply: Urban et al. (2014) show that in the period 2008 – 2011 water vapour is uncorrelated to cold point temperature. The reason is so far unknown. We do not analyse it here, because it is beyond scope. We will reformulate accordingly.