Replies to the Comments:
The authors thank the reviewers for their insightful comments. In the following, the comments are included in *italic face* while our replies are printed in normal face. In the resubmitted mansicript the changes are marked by colour.

Reviewer #1:
Comment: *The paper describes the analysis of combined time series of HALOE and MIPAS water vapour data with respect to the influence of a solar cycle. The authors claim that they see a solar cycle in the water vapour signal at the tropopause. They use a multi-linear regression method to predict water vapour with the proxies of El Nino and QBO to better understand the temporal variations of water vapour in the lower stratosphere. They assert that the residual can be made smaller by considering also the 11-year solar cycle effect in the regression. The consideration of the solar signal seems to be important because it has a non-negligible influence on the water vapour trend.*
The paper is clearly structured.
One weakness of this paper is the shortness of the water vapour time series because only one full solar cycle is considered.

Reply: We agree that the time series is very short. This is why we have the question mark in the title of the paper. However, with 20 years of data, we have nearly two solar cycles, not only one. Further, the statistical tools used provide uncertainties and consider the autocorrelations in the data. Thus we think we still have some quantitative estimate how reliable the analyses are. The fact that this all can just be coincidence (a caveat which applies to all statistical analyses) has been explicitly made.

Comment: *Another weakness is that the proposed relationship is insufficiently supported by evidence of a viable physical mechanism.*

Reply: It is not necessarily the obligation of the empirical scientists to explain all findings. In the first place, it is the task of empirical science to challenge the theory (Popper, 1934). We present an empirical result and we offer candidate explanations within the current theory and discuss these – in the revised manuscript much more thoroughly than in the initial submission. However, if it was requested for each new empirical finding that it has to be explained within the current theory, this would automatically immunify every theory against refutation or revision. This is certainly not what we want. According to the logic of this request, Farman's famous paper on the detection of the ozone hole (Farman et al., 1985) would never have been published, because these authors could not explain their empirical results within the current theory either.

Comment: *The autors present their results in comparison to other publications. Especially the discussion concerning the 'bottom-up' mechanism is not clear to me, because Gray et al (2010) put the apparent relationship between solar forcing and SSTs into question because this relationship would imply a*

climate sensitivity that is inconsistent with evidence from earlier centuries. If we suppose that such a relationship exist we are still not done.

Reply: Since the review paper by Gray et al. (2010) discusses all hypothesis currently under debate, we now refer to the specific original paper (e.g. Meehl and Arblaster, 2009; Meehl et al., 2009).

Comment: *The next question then is: How can a solar cycle-induced SST signal be imprinted on the lower stratosphere water vapour? Unfortunately, the authors provide only an insufficient physical explanation for such an influence.*

Reply: We discuss possible explanations within the current theory but, as stated above, empirical results stand as such, even if they cannot be explained within the current theory. Further, we have referred to Deckert and Dameris (2008b,a) for an explicit mechanism. This discussion has been extended in the revised version.

Comment: *The authors cite from White et al. (1997) that globally! averaged SST anomalies show highest correlations with solar activity with a lag of 1-2 year. From Decker and Dameris they cite that higher SSTs amplify deep convection locally and induce a strengthening of upwelling. But stronger upwelling leads to a colder tropopause and thus less moisture. Please provide a clear and consistent explanation of the process that leads to the proposed relation.*

Reply: We do not quite understand this comment and suspect that there was possibly a misunderstanding related to the time lag found by White et al. (1997). We have tried to rewrite related text in a clearer way.

Comment: *Because of the weak statistics the authors should avoid bold statements like 'solved a conundrum etc. Conclusions should be stated more modestly. Examples are detailed below.*

Reply: We did not write this. In the abstract this phrase was used in a conditional context. In the conclusion we say 'has the potential to'. In order to make the conditional character of these statements more clear, we have added 'possibly' in the abstract and 'help to' in the conclusion.

Comment: *Abstract, line 12: Due to the fact that less than two solar cycles are represented in the data you cannot conclude stringent from the regression analysis, that there is actual a solar signal at the tropical tropopause. Better say 'here seems to be a solar signal' or 'here is a signal with a 11 year period similar as the solar signal'*

Reply: We have chosen a weaker wording in the revised version.

Comment: *Page 12354, line 15: Why is this a conclusion from your results?*

Reply: Because we have also inferred the phase shift between the solar proxy and the water vapor signal. The fact that the phase of the water vapour signal changes with altitude by the same amount as the mean age of stratospheric air does suggest this. The mean age of air is a measure of the Brewer-Dobson-circulation. Thus, inference to the best explanation (Harman, 1965) leads to the conclusion that the signal is transported by the Brewer-Dobson circulation.

Comment: *Page 12354 line 17: 'The negative water vapour trends obtained ... can solve the water vapour conundrum...' This sentence is not understandable. Please give a more detailed explanation.*

Reply: This has been described in more detail in the revised version.

Comment: *Page 12364, line 12ff: The comparison of phase shift and age of air calculations is not understandable. Could you please provide a formula and more information about the age of air you used for your calculation. It is neither clear to me why you cite the paper of Stiller et al. in which only the time period of AOA from 2002-2010 is recognized.*

Reply: We cite Stiller et al. because this is the only global altitude resolved empirical age of air data available. Well, not quite true, now there is a new paper by Haenel et al. (2015) but for our purpose these data are equivalent, and at the time of submission of our paper these data had not been available. The time period considered by Stiller et al. admittedly is shorter than ours, but these authors have found that age of air changes only slowly. Thus the temporally averaged mean age distribution found by Stiller et al. is considered a good approximation for the time period investigated in our paper. The relationship is quite simple, it is basically the kinematic equation

$$v = \frac{dr}{dt}$$

we discretize this:

$$v = \frac{\Delta r}{\Delta t}$$

From the age of air analysis we know the ages of air at e.g. a lower and a higher point in the atmosphere. The difference is the time the air needs to travel from the lower to the upper point, i.e. Δt. Δr is just the difference of the altitudes of these two points. With this we have v.
Then we do the same for the (presumably) imprinted signal: The phase shift represents the travel time. Δr is just the difference of the altitudes of these two points. From this we can again calculate v. This velocity is very similar to that derived from the age of air. This suggests that the effect is not generated locally but transported with the Brewer-Dobson circulation.

We have included an equation and have tried to provide a clearer verbal description.

This formalsim, however, is only valid in approximation: In Fig. 10 we see that the inherent time lag has a coherent pattern but is not exactly constant. The reason is this. It is well known that the mean age of stratospheric air overestimates the pure transit time of a signal. Another example would be the comparison of the water vapour tape recorder with a tropical age of air profile. We expect that in the tropical pipe the discrepancy between age of air and transit time increases with altitude and the signal transport time is overestimated by the age of air. This is what we see in Fig. 10. as the time lag slightly decreases with altitude.

Comment: *Fig. 10 Page 12386: Please explain the sentence: Positive values represent delays of the solar signal in water vapour larger than the stratospheric mean age of air. It is not clear to me, how you calculated the values of fig. 10.*

Reply: We show that the observed phase shift is composed of an inherent time lag of about 25 months and the stratospheric signal transport time which is approximated by the age of air. Thus, we substract the temporally averaged local age of air (Stiller et al., 2012) from the observed phase shift. In the figure caption we say " i.e. difference of the phase shift of the solar signal in water vapour and the age of stratospheric air" which seems to be a quite equivalent wording to us.

Comment: *Page 12365, line 6: Explain what you mean by "in some cases".*

Reply: in some altitude/latitude bins. We have replaced this in the text.

Comment: *Page 12366, line 25: Explain, in more detail, what you have done or omit this sentence. It is not traceable why on the one hand a temperature anomaly of 1K magnitude with respect to a saturation vapour pressure over ice can explain half of the amplitude of water vapour and on the other hand a 2K variation from the regression analysis would explain 2/3?*

Reply: The related text has been rewritten. However, it must be noted that it is no wonder the different approaches to explain the observations lead in a quantitative sense to different answers. This is because each potential explanation involves other approximations and idealizations. To our knowedge there exist no global measurements of cold point temperatures (with emphasis on 'point'), thus this effect can only be studied theoretically. The problem is the limited altitude resolution of temperature profiles measured by satellite instruments. Radio occultaion would be the only adequate source of information for this purpose but these are only available since 2000. We think that a saturation pressure study as a first sensitivity study is adequate to give a flavour of the order of magnitude of the expected effect. However, saturation pressure analyses have

their own problems (e.g. supersaturation) and thus we have complemented this analysis with an empirical regression analysis. This solves the supersaturation problem but has the disadvantage that it is based on temperatures which are not cold point temperatures in any rigorous sense. Thus no perfect agreement between these different approaches can be expected. In the revised manuscript, this paragraph has further been elaborated.

Comment: *Page 12367, line 10: This sentence is misleading, because you analysed a time lag between solar cycle and water vapour and not temperature. Please omit it or provide a time series of MIPAS cold point temperatures.*

Reply: We assume that the local relation between water vapour abundance and the cold point temperature is instantaneous, i.e. there is no time lag between these quantities.

Comment: *Page 12368, line 26: leave out part of the sentence beginning with: which by their nature are a descriptive rather than an explaining quantity. It is an unnecessary explanation. Trends by nature depend also on the length and the selected period of the time series and are therefore arbitrary.*

Reply: We disagree for multiple reasons. First we find it important to distinguish between descriptive and explanatory terms. While solar cycle, annual cycle, etc have some physical meaning and hint at causation, a trend is a purely numerical thing without any physically explaining power. The only exception is if a 'driver' has a linear trend in itself. Typically, the descriptive quantities are used to fit that part of a phenomenon for which no causal hypothesis is available. The more causal explaining terms are used, the less has to be covered by the purely descriptive terms. Second, if a longer time series is adequately described by a linear trend, this implies that the trend does not change during this period. If if does (by an amount incompatible with the uncertainties), a linear trend is not the adequate representation of the time series. That means that a trend valid for a longer time series must also be valid for subsets of the time series.

Comment: *Page 12369, line 27: The sentence "including the solar cycle ...has the potential to resolve the water vapour conundrum" sounds a bit odd. Better say it in less pretentious words.*

Reply: Since the paper by Hegglin et al. (2014) 'water vapour conundrum' seems to be a common term in this field, thus we prefer to keep it.

Comment: *Page 12369, line 27: "But at least it can be said that in descriptive terms ..." In this sentence you qualify your results, which is suitable. Nevertheless during the whole text you stated that it is the solar signal that is imprinted on the water vapour time series. It would be better to formulate it more cautiously, also in the rest of the text.*

Reply: We have changed the abstract. Further we have scanned the text for too strong wording. The solar cycle is mentioned first in Sect. 4.2., p. 12362 In line 3 we say 'it suggests itself...', which sounds quite moderate to our ears. Par 2 on this page is purely descriptive, except for the last sentence, which reads 'This result suggests that the solar cycle might indeed...'. The remaining part of the page is only a description of what we see in our regression model and involves no hypothesation. Also in the remaining part of Sect 4.2. always a wording involving 'suggest' was chosen as soon as we hypothesize about processes in the atmosphere. In Section 4.3 we only describe what we see, without any speculation, and we leave the point explicitly open if the sun-H_2O connection is causal. Section 5 again begins with a statement involving "suggests". Page 12366 starts with '...to explain a solar cycle...'. The use of the indefinite article was chosen to make clear that we do not insist that there is one; otherwise we would have chosen 'the solar signal'. Remaining parts of Section 5 only summarize the relevant literature, present sensitivity studies but make no claim of a causal relation between the solar cycle and water vapour. The only word in the whole paper which might have been chosen a bit too strong is 'indicate' on p12369 l15, which has been chosen to avoid repetition of 'suggest' which is used again in l. 18. In the revised version, we have now replaced 'indicate' by 'the most likely explanation is'.The remaining part or p 12369 is again purely descriptive, and on p12370 we critically discuss the hypothis involving causation. In summary, we do not see where we use incautious wording.

Comment: *Please add a sentences to the discussion section, that a possible mechansism related to an Atmosphere/Ocean internal variability might also be possible, as you cannot exclude other mechanisms or other forcing factors operating on similar timescales as the solar cycle.*

Reply: This falls under 'coincidence'. We have added this example where we state that coincidence is the only serious alternative hypothesis.

Comment: *Accordingly I recommend the paper for publication only after major revision.*

Reply: We have done our best to revise the manuscript accordingly.

Reviewer #2:

Comment: *The manuscript is generally well written and is appropriate for ACP. While I think that it is in principle publishable because the idea seems at least plausible, I have to admit that I doubt that the solar cycle really is affecting lower stratospheric water vapor (at least to the extent claimed), and I think the question mark in the title is very appropriate. My reasons for doubt are threefold: (1) The proposed mechanism is that the solar cycle is changing the tropopause temperature, but there is no clear evidence of this;*

Reply: The effect in temperature has not been demonstrated but this does not mean that it is not there. The reason that it has not been demonstrated is that no observational data set measuring the cold POINT temperature with sufficient vertical resolution is available over the full observation period presented here. The only data set that comes close is radio occultation data, however, this data set does not start before 2000.

Comment: *(2) the fit to the solar cycle introduces a large unexplained negative linear trend in water vapor and;*

Reply: Why should the negative trend be unexplained? In contrast, the positive trends from earlier analyses remained unexplained and formed the "water vapor conundrum" (Hegglin et al., 2014) since they could not be explained given the constant or even decreasing tropopause temperatures over the last three decades (Rosenlof et al., 2001; Zhou et al., 2001).

Comment: *(3) it is not surprising that fitting an additional 11 year quasi-periodic term, together with a phase shift can improve the fit, and that this phase shift itself shows a time lag which suggests that the signal is propagating from the tropical tropopause. Indeed, even at the tropical tropopause the phase lag is not zero, as the authors point out. Given that the authors have shown a correlation, but by no means proven the existence of the effect that they claim, the abstract needs to include a few qualifications I would be unwilling to recommend publication without these qualifications in the abstract.*

Reply: We do not understand what the reviewer doubts here. An additional term in general improves the fit, that is true. However, that this term does not appear randomly in phase and amplitude, but the effect seems to be generated at the tropopause and transported with the BDC is a clear finding that tropopause processes seem to play a role. Further, that the phase lag to the solar cycle is not zero, but is in agreement with delays found for the SSTs has been explained in the paper. Further, we do not claim that there is a solar signal but only that evidence as far as available suggests this. We are aware of the hypothetical character of our candidate explanations, thus the question mark in the title and our careful wording.

Comment: *Qualifications required: Abstract Line 13: "a solar signal generated at the tropical tropopause is imprinted" should be rewritten as "may be imprinted."*

Reply: This sentence has been changed to: "We conclude from these results that a solar signal **seems to be** generated at the tropical tropopause **which most likely** is imprinted on the stratospheric water vapour abundances and transported.

Comment: *Abstract Line 16: "the final dehydration point of air is also governed" should be "may also be governed".*

Reply: Agreed and done.

Comment: *Other suggestions: Pg. 12354 line 26: The word "Mainly" is unnecessary and awkward. Please drop it.*

Reply: Criticism accepted. The sentence has been reworded in order to capture all details we intended to say.

Comment: *Pg. 12355 line 27: "get" should perhaps be "produce".*

Reply:
Agreed and changed.

Comment: *Pg. 12357 line 3: "HALOE H2O measurements were frequently validated (Harries et al., 1996; Dessler and Kim, 1999)." Neither of these references help to establish the stability of the HALOE measurements, which is really the key here. A possible reference is Nedoluha et al., "An evaluation of trends..." JGR 2003.*

Reply: This comment is indeed very helpful. Thanks a lot! We have included this reference.

Comment: *Pg. 12360 line 9-11: "A possible drift due to detector-aging and resulting changes of its non-linear response was estimated at approximately –0.05 ppmv/decade –1 in the relevant altitude range." This is good, but again, as for HALOE, a published reference showing MIPAS stability relative to other instruments would be useful. A possible reference is Nedoluha et al., "Variations in middle atmospheric water vapor from 2004 to 2013", but there may be others which the authors find more appropriate.*

Reply: Also this reference supports our case, so we have included it.

Comment: *Figure 1: This is an interesting figure. So is HALOE data below 15km not included in the convolution? Is there always HALOE data down to 15km? If not, what do you do?*

Reply: HALOE data over their full altitude range have been used for the convolution. However, we show the averaging kernels only down to 15 km since the general analysis has been performed between 15 and 30 km. Typical averaging kernels from MIPAS have been applied to HALOE zonal means where available, depending on latitude, altitude, and time. Where not available, a gap in the time series was accepted. These gaps, however, were rare enough not to affect

the general analysis.

Comment: *Start of 4.2: It is not clear why two decreases, one in 1994 (and very temporary) and another 2001 (a seemingly more long-lasting decrease) would lead one to conclude that an 11-year solar cycle is relevant. Unless there was some other compelling reasoning (besides the one given) then I would just start this Section with "We decided to investigate whether the addition of a solar cycle would significantly improve the fit".*

Reply: Here we do not refer to the two abrupt changes but rather to the general shape of the residual. We have reworded the related statement to avoid this misunderstanding.

Comment: *Pg. 12362 line 26: "Anti-correlation (lowest water vapour for solar maximum)". Given that there is a phase shift applied, this statement is not meaningful without specifying that shift. So it should say: "Anti-correlation (lowest water vapour for solar maximum shifted by XX months)"*

Reply: Agreed and done.

Comment: *Pg. 12363 line 7: "but still visible." should be "but it is still visible."*

Reply: Agreed and done.

Comment: *Pg. 12364 line 25: "The systematic residuals observed when the solar component had not been considered largely disappeared when a solar cycle signal was considered." This is a very awkward sentence.*

Reply: Double negation has been cancelled, which led to a much simpler sentence.

Comment: *Pg. 12365 line 11: "This indicates that, even if one does not believe the solar cycle effect in explanatory terms, it still is important in descriptive terms in order to avoid artefacts caused by the related systematic residuals." I don't understand what this sentence means.*

Reply: An explanation has been added.

Comment: *Pg. 12366 line 27: "suggest that even two thirds of the observed solar component of the vapour variability can be explained by a 2K solar temperature variation." I don't understand the "even two thirds". Does this suggest that a 3K solar temperature variation (which seems very large) is required to explain the full variation? Please make this explicit.*

Reply: Yes, that was what we meant: according to this estimate a 3 K peak-to-peak temperature variation could exlain the observed peak-to-peak signal in water vapour. We do not claim that we can fully explain our observations within the current theory. All we can do is to offer partial explanations and put the results in the context of recent literature. All estimates we can make must rely on various simplifications and idealizations. The whole related section has been extended.

Comments by K. Zhang

Comment: *General comments:*
This paper uses merged time series of stratospheric water vapour from HALOE and MIPAS data to study the influence of solar signal on the temporal variations of stratospheric water vapour. This is an interesting topic regarding a currently debatable issue on long-term water vapour trends in the stratosphere. However, I have several concerns: One of the concerns is that the analysis is primarily based on a multivariate linear regression of water vapour time series. First, we should be cautious that results from this analysis do not simply imply a causal relation between the solar signal and water vapour variations.

Reply: We know since David Hume ('A Treatise on Human Nature' (1739) and 'An Enquiry Concerning Human Understanding' (1748) that no observation can ever reveal causation. This does not only apply to regression analysis but to all empirical research. The only exception might be controlled laboratory experiments, but these do not help a lot in climate research. The concept of causation is a theoretical concept, not an empirical one, but that does not mean that hypothesizing about causal relations is per se inadequate. The tentative causal explanations of the solar cycle signal in our H_2O data are clearly presented as hypotheses ("candidate explanations" p12366 l.1) and the risk associated with inferring causation from statistics is explicitly mentioned (footnote on page 12370). In summary, we can see nothing inadequate in our hypothesizing.

Comment: *Many places in this paper use "control", "influence" and etc., to which should be paid more attention.*

Reply: A hypothesis involving causation can only be described using words implying causation. The hypothetical character of the related statements is always made clear by the wording ("suggests to be" etc)

Comment: *Second, the water vapour time series used in this study still contains a seasonal cycle, and you should be aware that the large correlation may be dominated by the seasonal cycle, instead of by the real signal. Therefore, I suggest conducting regression analysis on water vapour time series after removing the seasonal cycle.*

Reply: The seasonal cycle is considered in the regression analysis via a harmonic component of a period length of one year plus a series of overtones.

Comment: *Furthermore, one main conclusion of this paper is that the negative water vapour trends could be obtained when considering the solar cycle impact, which is also based on linear regression including an 11-year solar cycle proxy. It seems that you tried to remove the negative trends intentionally.*

Reply: We consider this accusation which is based on pure speculation as unscientific.

Comment: *This may raise a problem that the removal of the water vapor trends may just be a mathematical result rather than a real imprint of solar signal trends.*

Reply: In our paper we clearly distinguish between the empirical evidence and the inferred hypothesis.

Comment: *One simple example would be that if you include any other unrelated factors (eg., population) with significant trends instead of the solar cycle, you could also possibly remove the water vapour trends. But this does not imply that the water vapour time series contains signals of these factors. This is an important but confusing result in this paper which you need to clarify.*

Reply: In our manuscript we critically discuss the alternative explanations. However, wouldn't this way of reasoning, when performed with sufficient rigor, lead any empirical analysis ad absurdum?

Comment: *Specific comments:*
1. It will be better if you could see the solar signal in the decomposed water vapour time series using the Empirical Orthogonal Function (EOF) analysis.

Reply: We have decided to use multilinear regression because this method is able to connect the observations with possible causes. EOFs may better describe the observations but have no explaining power. The fact that the basic functions used in multilinear regression are not orthogonal is considered in the error analysis.

Comment: *2. Abstract, page 12354, lines 12-15: "We conclude from these results that a solar signal generated at the tropical tropopause is imprinted on the stratospheric water vapour abundances and transported to higher altitudes and latitudes via the BrewerDobson circulation". Even we could get the former part of the conclusion from this study, the latter part seems to be more likely an assumption.*

Reply: Any other explanation for the fact that the local phase shift difference

coincides with the local age of air? What we do here is inference to the best explanation as suggested by C. S. Peirce (1903). We admit that this kind of abductive inference can never be absolutely certain, but this is the fate of all natural science. Scientific knowledge will always be fallible (K. Popper, cit. loc.).

Comment: *3. Abstract, page 12354, lines 15-17: The conclusion that "the tropical tropopause temperature at the final dehydration point of air is also governed to some degree by the solar cycle" is not supported by this study.*

Reply: This a a conclusion from our study and abducted from the data. This is part of the hypothesis developed in this study. To make the hypothetical nature of the statement clear, we now write "may be".

Comment: *4. Page 12360, line 2: "The third method proved most robust and was finally selected." It needs more explanation why you chose this method. Or you could list literature supporting this method.*

Reply: The other two candidate approaches suffered from sparse statistics or sampling artefacts, respectively. This is now mentioned in the paper.

Comment: *5. Page 12361, lines 24-25: "The residual time series appears to be dominated by a systematic harmonic feature of a period length of about eleven years." How did you find out that it is dominated by an 11-year harmonic feature?*

Reply: By visual inspection of the fit residuals (c.f. Fig. 3). In the context of discovery (Reichenbach, 1938) this is an adequate method. Even dreaming is an adequate method in the context of discovery, if the idea later can be corroborated by more objective means. That is how August Kekulé discovered the ring-structure of benzene. He dreamt of a snake biting its tail. More quantitative analyses are only required in the context of justification, and that is what we do, by showing that inclusion of the solar cycle term indeed decreases the fit residuals significantly.

Comment: *6. Figures 4 and 6: How statistically significant are the improvements?*

Reply: The propagation of the data errors through the regression model leads to uncertainties of these amplitudes of generally less than 2% within the tropical pipe and less than 5% outside. In other words, the signal is 50 to 20 times higher than its uncertainty (p. 12363, l19/20) and has thus high statistical significance. Even under consideration of empirically determined auto-correlation, the amplitude is 4–5 times higher than its estimated uncertainty over a larger part of the altitude/latitude range (*ibid.*). This means the results are still significant.

Comment: *7. Page 12363, lines 16-17, and Figure 8: Larger amplitudes do not imply larger effects.*

Reply: We do not understand this.

Comment: *You have largest amplitudes in the tropical tropopause region, which may be due to that the variance of water vapour is largest in this region. I think the amplitudes of the fitted terms that represent the solar cycle in the regression should largely reflect the amplitudes of the water vapour variability. In order to find out the regions where the solar cycle has larger effects, you may need to compare the amplitudes to the water vapour variability.*

Reply: The variability of water vapour is largest at the region where the variability is caused. This holds for all processes causing water vapour variability. For older air, the variability decreases due to mixing.

Comment: *8. Figure 10: The figure shows positive values (the phase shift of the solar signal in water vapour - the age of stratospheric air) within 50∘N/S and between ∼15 and 23 km in the tropics, which may be primarily due to the low age of air in these regions. Similarly, the negative values may be due to the high age of air in higher altitudes. You need to elaborate why you want to look at the difference between the phase shift of the solar signal in water vapour and the age of stratospheric air.*

Reply: The text has been revised and an equation has been added.

Comment: *Technical comments: Page 12354, line 26: change "Mainly ..." to "It is ... that ...";*

Reply: The wording has been changed. See related comment of reviewer #2.

Comment: *Page 12355, line 2: delete "atmospheric";*

Reply: We think that the term 'region' alone without 'atmospheric' is typically associated with a region on Earth. To avoid confusion, we prefer the unambiguous over the elegant.

Comment: *Page 12360, line 2: change "proved" to "was proved to be";*

Reply: agreed and changed.

Comment: *Page 12360, line 5: change "within" to "for";*

Reply: agreed and changed.

Comment: *Page 12360, line 10: change "was" to "were";*

Reply: agreed and changed.

Comment: *Page 12360, lines 21-22: rephrase "they merely help to describe but not to explain the temporal variation."*

Reply: Not understood what is wrong here.

Comment: *Page 12360, lines 25: change "not" to "that are not";*

Reply: agreed and changed.

Comment: *Page 12361, line 20: change "were" to "was";*

Reply: We think that 'data' is plural. Singular would be 'datum'. The Oxford Advanced Learner's Dictionary of Current English, 1989 edition, p.302, bottom of col 1 says: "There is uncertainty with some nouns as to whether they are singular or plural: *This data is correct* and *This data are correct* is both acceptable.

Comment: *Page 12362, line 22: delete "a priori";*

Reply: We think it is important that we have the qualification "before the empirical analysis" and we think this is exactly what 'a priori' means in all contexts from Bayes and Kant to Shannon and Rodgers.

Comment: *Page 12364, line 3: change "which implies" to "implying" or rephrase the whole sentence;*

Reply: Agreed but the whole part has been reworded anyway.

Comment: *Page 12368, line 3: add a comma after "White et al., (1997)";*

Reply: agreed and done.

Comment: *Page 12368, line 6: change "which both is" to ". Both of them are";*

Reply: Agreed and reworded similar as suggested.

Comment: *Page 12368, line 6: change "to" to "with";*

Reply: Agreed and done.

Comment: *Page 12368, line 8: delete "until recently";*

Reply: We inted here to emphasize that this has recently been challenged.

Comment: *Page 12368, line 15: delete the second "a";*

Reply: Agreed and done.

Comment: *Page 12368, line 17: change "attribute" to "attributed";*

Reply: Agreed and done.

Comment: *Page 12368, line 19: change "is" to "was";*

Reply: Agreed and done

Comment: *Page 12368, line 21: add "anomalies" after "water vapour";*

Reply: Agreed and done

Comment: *Page 12368, line 23: delete "only";*

Reply: Agreed and done. We have inserted in the previous text 'can **fully** be described' to maintain the emphasis we intended to express by 'only'.

Comment: *Figure 2: change the names of each line to match the text descriptions better, for example, "HALOE adapted" → "HALOE avg_ker"; "MIPAS shifted" → "MIPAS de-biased";*

Reply: 'HALOE avg_ker' is ambiguous because it suggests that something was done with the HALOE averaging kernels but the MIPAS averaging kernels have been applied. 'MIPAS-debiased' would be misleading because the bias seems to be a HALOE issue. Thus we prefer the original legend. Further details are given in the figure caption anyway.

Comment: *Figure 5: I assume that the green line represents the linear term of the regression, please briefly indicate what the green line represent in the top panel;*

Reply: Agreed and done.

Comment: *Throughout this paper, there are lots of long sentences using attributive clauses. It will be better to shorten these sentences.*

Reply: We have tried to improve the manuscript with respect to this.

Comment: *In many places, past tense and present tense are not properly used.*

Reply: Corrections have been applied where appropriate.

Further Corrections:

We have realized that there was an equivocation with respect to the term 'coincidence', even within one sentence. On the one hand we have used this term as a synonym for 'accident' or 'chance', on the other hand for the more neutral term 'co-occurrence'. We have reworded the related text.

Stephan Füglistaler has brought to our attention that a residuum between observed and modelled water vapor of similar shape and phase as shown by us in Fig. 3 has been found earlier in his paper Fueglistaler et al. (2013), their Fig. 8. The fact that the HALOE observations could not been modelled by trajectory calculations on basis of reanalysis data can be interpreted that either the reanalysis cold point temperatures are not correct and miss a term in their variations, or that the water vapor residual variation observed by us cannot be traced back to cold point temperatures, or that the HALOE data reveal a drift in the respective period. We have included this information in the text.

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