Reply to Reviewer #1

We thank Reviewer 1 for their continued concern with our manuscript. In our response, the reviewer's comments are bolded, our answers are normal weight, and anything that we change in the paper is italicized.

1 general

There are obvious omissions in the discussion that possibly could lead the reader to wrong conclusions (e.g. not discussing the known biases in tropopause temperature in many models or the fact that the correlation between stratospheric water vapor and tropospheric temperature can well be caused by spurious diffusion over the tropopause, see major comments 4–5).

We hope that we have alleviated this problem with our draft, see answers to other comments.

I noticed that you uploaded a supplement containing additional figures that add the models not shown in Figure 2. This is a very welcome addition to the paper in my opinion and I would strongly suggest to move these figures into the main body of the paper, since they add a lot of interesting information and only very moderately increase the length of your paper. If you reduce the size of the figures a little bit, they perhaps would fit on two pages as two figures with sub-panels.

The reviewer makes a good point that the supplement is not well referenced in the paper. To address that, we have added a sentence to page 3, lines 20-21 to more clearly reference the supplement, as well as a short paragraph in section 3.2, page 5 lines 6-9. We do not see sufficient value in moving these figures into the main text of the paper to do that, so we'll leave them in the supplement.

Please consider that you are addressing a broader audience here. What may seem completely obvious to you and some of your colleagues, may not be general knowledge in the wider atmospheric community.

We hope that we have alleviated this problem with our draft, see answers to other comments.

Just a suggestion for a title that reflects a little bit better what you have done: "Contribution of different processes to changes in tropical lower stratospheric water vapor in chemistry-climate models" or if it is ok that the title is a little bit longer "Contribution of the Brewer-Dobson circulation, the quasi-biennial oscillation and of changes in tropospheric temperature to changes in tropical lower stratospheric water vapor in chemistry-climate models".

The Title has been changed to *"Contribution of different processes to changes in tropical lower stratospheric water vapor in chemistry-climate models"*

Partially resolved, but some of my comments in the following address this: Try to avoid exaggerations (e.g. the last sentence of the abstract), discuss other studies that are relevant in the context of your paper and don't draw conclusions that are not supported by the results of your study.

We hope that we have alleviated this problem with our draft, see answers to other comments.

2 Further discussion on old comments and your replies

Major comment 2: I am pleased that you removed the sentence in question and added the additional sentence to page 1, lines 11–12.You write "that is a well-documented phenomenon" and "water vapor is well-known to be a greenhouse gas". This is of course, correct, and of course, I did not question this in any way. Nobody denies that water vapor is a greenhouse gas. But that is not the point here. Maybe I was not clear enough in my explanation and what I aimed at. The point is if there is a feedback on tropospheric temperatures. You need detailed radiative transfer model calculations to show that there is a significant increase in radiative forcing or temperatures of the troposphere by increases in stratospheric water vapor. None of the papers cited by me or you states a priori that there is a relevant radiative forcing of the troposphere from stratospheric water vapor. All these studies run a radiative transfer model, and then draw this as a conclusion by giving some value for the radiative forcing or temperature change. In addition, a feedback requires that higher tropospheric temperatures lead to higher tropopause temperatures, which is even less clear a priori, see major comment 3 in the original review and this document. The second thing is that "well-documented phenomenon" does not really hit the point. It may be well-documented by studies that are known to a certain part of our community, but you are writing for a wider audience here, which may not necessarily know the same papers as you. You can't expect the same a priori knowledge from everyone and it is the purpose of an introduction to point the reader to the relevant literature. That said, I have no objections that you discuss a possible feedback here, as long as you make clear that this is not obvious and discuss the literature, and as long as you make clear that this is not a result of your study. You don't need to delete any reference to that.

This material is all well known by the expert community. We disagree with the assertion from the reviewer that this paper should be written for a wide audience. Our view is that ACP is read by specialists who are familiar with the literature. Expecting us to write this paper for truly broad audience (e.g., Rev. Geophys, BAMS, EOS) would radically change the nature of the paper and we don't feel that's appropriate.

Major comment 3: You state that you have added discussion at page 2, line 10–13. But there is no discussion at this place. Did you confuse pages or line numbers here? Is it the discussion at lines 5–8? I am not satisfied with this. There is still no discussion that the correlation of stratospheric water vapor and tropospheric temperatures due to their long-term increase is basically a model phenomenon and can't be confirmed by the available observations. There is simply no clear trend in tropopause temperatures or water vapor in the observations (e.g. Get- telman, Fueglistaler). In addition, you don't discuss that the correlation between tropospheric warming and increasing tropopause temperatures is not that obvious from a theoretical point of view (e.g. Lin, Shepherd). Quite in the contrary you state "There are good physical reasons for this connection": Please rephrase. This sounds more like an annoyed comment aimed at me than as a statement aimed at the reader. And it is somewhat ironic that you cite the Lin et al. paper here: If I may cite from the abstract: "Given the subtle nature of the balance among all these factors, it might be surprising that almost all GCMs and CCMs predict a warming [. . .] of the tropical tropopause [. . .]", and later (section 4) "In practice, the magnitude of tropopause warming vary vastly from model to model". I may also cite Shepherd (2002), page 778, referring to the sketch showing the conceptual relationship between tropospheric warming and warming at the tropopause "[...] and certainly not as simple as depicted in Fig. 6b". And please see what I have said to specific comment 16. In particular, why don't you mention that this is only seen in models, but there is no clear evidence from observations? In summary, please try to give a more well-balanced discussion here (or in the conclusions, see specific comment 16).

We should have been more specific. Additionally, we modified our discussion of the relationship between tropospheric warming and TTL temperatures (see page 2, lines 5-7)

Major comments 4 and 5: I am not satisfied how you treat these major comments, which are basically ignored. I certainly do not want you to change the scope of the paper or to bloat it with unnecessary information. However, discussing the performance of the aspects of the models which are important for your analysis is crucial for the reader to be able to assess your results and their reliability (especially to assess if these model based results can be transferred to reality).

Interestingly, you discuss the QBO term in some detail, but largely avoid to discuss the ΔT term. Since you ask for specific topics that I would like to see discussed, here is one: Discuss the bias and annual cycle of tropopause temperatures compared to observations, in a similar manner as in Fig. 1 of Gettelman et al. (2010). It is not sufficient to point me to the Gettelman paper. It does not discuss the same models, and I am not able to find out easily if the 6 models that are discussed both in the Gettelman paper and in your paper have the same model version etc. It is also not sufficient to point me to the papers that you added to Table 1. First of all, you can't demand from the reader (or the reviewer) to read through 15 lengthy papers to find out some information that is significant for your paper. Next, by quickly scanning through the cited papers, I am pretty sure that most of them do not contain the relevant information (e.g. tropopause temperatures).

We discuss the QBO term in depth because that's the one where the models do the worst, so it's obviously of most interest. We don't discuss biases in the annual cycle of temperature because that seems entirely irrelevant to this analysis. The first thing we do in our analysis is either average over the annual cycle or remove it. So it's not clear how biases in it will impact the paper. In addition, the goal of our paper is to determine the response of H_2O to changes in the temperature. The temperature doesn't have to be right for the response to be right; nor is the response right if we show that the temperatures are right. These are completely separate quantities.

Another specific topic which is important to discuss in my opinion is spurious diffusion of water vapor across the tropopause. There is an extremely large gradient of water vapor near the tropopause and at the same time, models are well known to be too diffusive compared to reality (especially in the stratosphere, where the effective diffusion coefficients are 100 times smaller than in the troposphere). This problem is well documented in the literature (e.g. Gettelman et al., 2010, page 11, Hardiman, 2015, section 3). It is well possible that the relationship between stratospheric water vapor and tropospheric temperature is dominated by this effect (at least in some models) and not discussing this may lead the reader to the wrong conclusion that he can transfer the results of your study (trends, contribution of the different terms) more easily to the real behavior of the atmosphere than it is actually the case.

We added a caveat to page 4, lines 22-26.

In this respect, I am also not very satisfied with your answer to major comment 4. You say that you have added a caveat to the paper, but in fact you did not address the point I discussed in major comment 4. I was talking about spurious diffusion in the comment, but the caveat you added to the text deals with overshooting convection. This is certainly also an interesting point, but not what I was talking about.

We added a caveat to page 4, lines 22-26.

Another issue is the BDC, which is also not discussed. How well the BDC is represented in the models will have implications for the contribution of the BDC to the trend and variability of stratospheric water vapor in your regression model. E.g., if the BDC is too fast in a model (compared e.g. to $w*$ derived from reanalyses), it will lead to an overestimation of this term in your regression analysis compared to reality.

We added a new discussion to page 3, lines 4-11. That said, we disagree with the premise of this comment. Our analysis measures the response of H2O to changes in the BDC. The BDC doesn't have to be right for the response to be right; nor is the response right if we show that the BDC is right. These are completely separate quantities.

Specific comment 1 (was Page 1 , Line 1 and Page 2, line 14): Was there any reason apart from this comment that caused you to remove the sentence? The aim of my comment was certainly not that you remove the sentence, but that you add the citations. Now there is the unfortunate situation that the sentence is still in the manuscript (in the abstract), but that you can't give the relevant citations (I acknowledge that it is no good idea to cite in the abstract). And to cite the relevant literature is certainly appropriate for this central statement.

We removed the sentence originally on page 2, line 14, because it didn't fit where it was written. Additionally, we modified the first sentence of the abstract to remove the need for a citation; see page 1, line 1.

Specific comment 3: For the reasons given in my review, I still think this is a problematic statement. In addition: In your reply to this comment, you state "we clearly base our conclusions on the detrended analysis". But this sentence explicitly refers to the trend in humidity. What do you want to tell me with your statement? Please also see my detailed comments to specific comment 16 below.

We understand that correlation does not imply causality, and that the trended century regressions are not, by themselves, convincing. But we feel that the totality of the work — trended regressions, detrended regressions, and decadal regressions — provides sufficient evidence to make this statement. As a result, we've left this as is.

Specific comment 6 (was Page 1, line 8): I am not satisfied how you treat this comment. You neither deleted or changed the sentence, nor did you explain to me in your reply what you mean by "superior" and to what the statement refers in a satisfactory way. This comment was one of the more important specific comments I made, since this statement is in the abstract at a rather prominent position, and it is just an unproven and unclear statement. You should try to avoid the impression that you put this sentence into the abstract just to create interest for your article, without anything really supporting this statement. Since you refer to the Gettelman paper in your reply: Do you mean that applying a multiple linear regression model is better than just looking at plots of stratospheric humidity and tropopause temperature? Then, why don't you write it, neither in the reply to my comments, nor in the abstract? And if this is really what you mean, is it really worth mentioning? It was certainly not the intention of Gettelman et al. to do a multiple regression analysis and for the purpose of their paper, it was sufficient to show the plots. And there are studies, including your own studies, which already used multiple linear regression. So, what is the point here?

We disagree with this comment. We feel this is a new and novel way to look at models' regulation of stratospheric water vapor. Obviously, readers will render the final verdict.

Specific comment 7 (was Page 1, line 11): It is nice that you refer to the LDPs now, but unfortunately, the sentence is not quite correct. The coldest temperatures in the TTL are not necessarily at the location where an individual trajectory has its LDP, which may cross the tropopause at a warmer location. I suggest to rephrase the sentence so that the statement is correct.

This sentence has been modified; see page 1, lines 16-18.

Specific comment 16 (was Page 7, line 13, now lines 26–27): That is referring to the identical sentence on page 1, line 4 (old manuscript) and the comment referring to it (specific comment 3). There needs to be more discussion here, and I find the statement here problematic. You can't draw the conclusion that the trend in the warming of the troposphere drives the trend in stratospheric water vapor from your trended regression analysis (as you admit in line 26–27, page 3 in the old manuscript). Any timeseries with a trend will fit your stratospheric water vapor time series. I.e., it is just not correct to say "we find". I suggest to change the sentence to "We find that in our trended regression analysis, the trend in stratospheric water vapor is explained largely by the trend in tropospheric temperature." That has a completely different meaning, in particular, it does not imply that the change in tropospheric temperature is the indisputable under- lying reason for the trend in stratospheric humidity in the models. In addition, it does not imply that in reality, a trend in tropospheric temperature will imply a trend in stratospheric humidity. I am aware that you write "in the CCMs" in this sentence, but there is no discussion in the paper that the trend in stratospheric humidity and in tropopause temperature are basically a model phenomenon. The observations of water vapor and temperature do not support this conclusion clearly in the moment. In addition, it is also not a priori clear from a theoretical point of view. See my major comment 3 of the original review again for this.

See new discussion on page 3, lines 25-28.

3 New Comments

Page 1, line 2: Better: “We analyze the trend and variability [. . .]”. With- out interannual variability in at least some of the variables, you would not be able to fit the explanatory time series without ambiguity to the water vapor time series (i.e. if all variables would only contain a trend, the error bars would go to infinity and the fitted values would be arbitrary).

We amended this sentence (see page 1, lines 1-3).

Page 1, line 7: “Many of the CCMs [. . .]”. Rephrase or delete: a) This is an unproven statement, in particular since you explicitly refuse to give information about model performance in this paper. b) This is far too generic. Models may perform well in some variables, but no so good in others, and this will also vary from model to model. Be more specific. c) It is unclear what observations you are referring to. d) In particular referring to the trends in water vapor and tropopause temperature: This is a particularly bad example for a “credible” prediction. It is unclear from observations and theory, and is mainly based on the belief that the models do model these particular aspects of the climate system well.

We have modified the sentence to explicitly say that we’re talking about the performance in the regression. While the author is correct that this sentence does not give all of the details, it is located in the abstract so a general overview is most appropriate. More details about the comparisons are found in the text (starting on line 22 of page 5).

Page 1, lines 11–12: Please write “increasing it will lead to additional warming of the troposphere” and not “of the climate system”. That is too generic. More stratospheric water vapor cools the stratosphere, so this statement is obviously not quite correct.

In response to a different comment, we’ve removed this sentence.

Page 1, lines 11–12: “Stratospheric water vapor is a greenhouse gas”. Change that to “Water vapor is a greenhouse gas”. If a gas is a greenhouse gas or not does not depend on the altitude. It is defined as a gas absorbing in the thermal infrared. And then start a new sentence “Increasing stratospheric water vapor will lead to additional warming of the troposphere, as shown by [citations]”

While we agree with the reviewer that water vapor is generally a greenhouse gas, the phrasing of our statement is in our opinion completely clear. We’ve therefore left this sentence as-is.

Page 2, line 8: Does the correlation of 0.91 refer to the trended or de- trended variables? It would be really helpful for your argumentation if the interannual changes would be correlated.

In light of the previous comments and our responses, we believe that this point has been sufficiently litigated. For these reasons, we have removed the reference to the 0.91 correlation between TTL temperatures and tropospheric warming.

Page 2, line 19: Better “worked well in reproducing trend and variability”? It is no surprise that it is easy to fit a variable with a trend to another variable with a trend.

We feel our phrasing is equivalent and have left this sentence as it was in the previous version.

Page 2, line 22: What do you mean by comparison to observations? Do you mean to apply the same regression model to time series of observations and to compare the results?

Yes, this is what we mean. We feel this is completely clear as written.

Page 2, line 23: Here applies the same comment that I had to Page 7, line 8–9 (original manuscript, specific comment 15).

This is solved in the conclusions now, but not here.

We have edited this (page 2, lines 20-21), but we also think this point exaggerates our claims. We are not evaluating the models with just a linear regression — the regression is based on well-known and understood physics, so the ability to reproduce the linear regression DOES tell us something about the quality of the fit.

Page 3, line 25: Don't exaggerate. Can we agree on "good job"?

"Excellent"* has been changed to *"good job"

Page 4, line 1–2. Half of the models shows an explained variance decreased by more than 0.2. That is not "slightly" smaller. Suggestion: "moderately".

"slightly"* Has been changed to *"moderately"

Page 4, line 32 to Page 5, 4: The term "standardized regression coefficient" is a little bit unfortunate. It confused me several times when reading this section, because it suggests something different than actually intended. This is not a regression coefficient, but something like a "variability of the fitted time series" or "standard deviation of the fitted time series" or "square root of the explained variance". Please change.

"standardized regression coefficient"* has been changed to *"regression coefficient using standardized variables"

Same paragraph: I noticed that in several of the models (e.g. CMAM- CCMI, GEOSCCM, GEOSCCM-CCMI), the variability in the stratospheric water vapor time series mostly comes from the variability in BDC and QBO, with almost no variability in the ΔT time series. That means that the magnitude of the fitted trend in ΔT is very dependent on the magnitude of the interannual variability of the QBO/BDC in these models, since the ΔT term, which is almost a pure trend, will fit "what is left from the trend" after matching the interannual variability and trend of the QBO/BDC time series. This may be worth mentioning, since this is a good example of an effect on the ΔT trend which is not "physical", but "numerical".

We added a short paragraph to the end of page 5 lines 6-9.

Page 7, line 21: "A new way"? See specific comment 6.

We modified this sentence, see page 7 line 29 - page 8 line 1.

Reply to Reviewer $\#2$

In the revised version, Smalley et al. have addressed some issues raised by reviewers. However, the authors have decided to ignore the suggestion that more in-depth analysis would make the paper stronger. It is repeatedly stated that the linear regression shown in this paper is superior to "simply comparing $[H_2O]_{entry}$ to observations"; these statements make little sense to this reviewer (obviously the questions posed are different), and also does not help to make the paper stronger (I recommend deleting these sentences).

We hope that we have alleviated this problem. But, in reference to your example (assuming the last sentence of the abstract), we disagree with this comment. We feel this is a new and novel way to look at models' regulation of stratospheric water vapor. Obviously, readers will render the final verdict..

My main concern remains that a linear regression is useful to detect correlations and common properties, but not more. In my opinion the paper overstates the results when claiming that the linear regressions shown provide insights into processes.

We agree that a linear regression by itself does not prove causation. But a linear regression combined with physics can be used to evaluate the relationship between each process and $[H_2O]_{entry}$. In this case, there are good physical reasons to believe that the terms in our linear regression affect $[H_2O]_{entry}$, as well as observational evidence to support each one. We reference previous literature demonstrating this in our paper. Thus, we disagree that we have overstated the value of our linear regression.

Case in point is the Brewer-Dobson result where some models have a positive BDC coefficient (CNRM-CM5-3, NIWA-UKCA; p6/L29: All pages/lines refer to the manuscript version with changes highlighted). The paper does not explain to the reader why the authors expect the coefficient to be negative, and it would have been easy to check a few model fields as to why the coefficient in these models is positive. A brief mechanistic discussion (there's plenty of papers discussing the Newtonian cooling and ozone/other tracers that can be used for reference) would be helpful.

We have added a new discussion to the conclusions (see page 8, lines 16 - 22) discussing the physical mechanism that connects BDC variability to the TTL.

The revised manuscript has a supplement with 13 figures with proper caption but no text whatsoever. I could spot only one reference to the supplement in the manuscript (p3/L13). Please integrate the supplement better with the manuscript, and provide a very brief description (along with a title like "Supplementary Material for ...") in the supplement.

We added a sentence to clearly reference the supplemental material on page 3, lines 20-21, as well as a reference to the supplement in the section 3.2 page 5 lines 6-9, and added a title to the supplemental material.

Finally, the text deserves some careful checking; for example the first sentence of the introduction ("... so increasing it will lead ...") or the last sentence of Section 3.1 ("... show something similar ...").

Done

~~Testing chemistry-climate models' regulation~~ Contribution of different processes to changes in tropical ~~lower-stratospheric~~ lower stratospheric water vapor in chemistry-climate models

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Abstract. ~~Climate models predict that~~ Variations in tropical lower-stratospheric humidity ~~will increase as the climate warms, with important implications for the~~ influence both the chemistry and climate of the atmosphere. We analyze ~~the trend~~ tropical lower stratospheric water vapor in 21^{st}-century simulations from 12 state-of-the-art chemistry-climate models (CCMs), using a linear regression model to determine the factors driving the trends and variability. Within CCMs, warming of the troposphere primarily drives the long-term trend in stratospheric humidity. This is partially offset in most CCMs by an increase in the strength of the Brewer-Dobson circulation, which tends to cool the tropical tropopause layer (TTL). We also apply the regression model to individual decades from the 21^{st} century CCM runs and compare them to a regression of a decade of observations. Many of the CCMs, but not all, compare well with ~~observations~~these observation, lending credibility to their predictions. One notable deficiency ~~in most CCMs is that they~~ is that most CCMs underestimate the impact of the quasi-biennial oscillation on lower-stratospheric ~~humidity~~water vapor. Our analysis provides a new and potentially superior way to evaluate model trends in lower-stratospheric humidity.

4 Introduction

Stratospheric water vapor is well-known to be a greenhouse gas (e.g. Manabe and Wetherald, 1967; Forster and Shine, 1999; Solomon et al., 2010; Maycock et al., 2014)~~, so increasing it will lead to additional warming of the climate system~~. Because of this, understanding the processes that control the humidity of air entering the tropical lower stratosphere (hereafter $[H_2O]_{entry}$) has been a high priority of the scientific community since Brewer (1949) first described the stratospheric circulation.

It is now well established that the fundamental control over $[H_2O]_{entry}$ comes from the ~~coldest~~ cold temperatures found in the tropical tropopause layer (TTL) (Fueglistaler et al., 2009b), ~~frequently referred to as the Lagrangian dry point,~~ and that variability in these temperatures translates into variability in $[H_2O]_{entry}$. The most well-known example of this is the so-called “tape recorder,” in which the seasonal cycle in TTL temperatures is imprinted on tropical stratospheric water vapor (Mote et al., 1996).

On interannual time scales, variability in $[H_2O]_{entry}$ originates from ~~processes such as~~ variability in the Brewer-Dobson ~~Circlation~~ Circulation (BDC) variability (Randel et al., 2006a; Castanheira et al., 2012; Fueglistaler et al., 2014; Gilford et al., 2016) and the quasi-biennial oscillation (QBO) (O’Sullivan and Dunkerton, 1997; Randel et al., 1998; Dunkerton, 2001; Fueglistaler and Haynes, 2005; Choiu et al., 2006; Liang et al., 2011; Castanheira et al., 2012; Khosrawi et al., 2013; Kawatani et al., 2014; Tao et al., 2015). ~~More recently, Dessler et al. (2013, 2014) suggested~~ Dessler et al. (2013, 2014) suggests that the temperature of the troposphere also exerts an influence on $[H_2O]_{entry}$ ~~. This was based on the well-established observation that models predict a warming TTL during global warming e. g., Gettelman et al. 2010; in the models analyzed here (described in the next section) the tropospheric temperature is highly correlated with TTL temperatures, with a mean correlation of 0.91. There are good physical reasons for this connection Lin et al., 2017. In addition , Dessler et al. 2016~~based primarily on an analysis of satellite measurements of $[H_2O]_{entry}$. This is mainly caused by radiative heating of the TTL from increased upwelling radiation from a warming troposphere (Lin et al., 2017). In addition to this mechanism, Dessler et al. (2016) demonstrated in two CCMs that a warming climate also ~~caused increased amounts of water to be~~ increased the amount of water directly injected into the stratosphere via deep convection, providing another mechanism for tropospheric temperature to affect $[H_2O]_{entry}$.

Putting these factors together, Dessler et al. (2013, 2014) demonstrated that observed $[H_2O]_{entry}$ anomalies could be accurately reproduced with a simple linear model:

$$[H_2O]_{entry} = \beta_0 + \beta_{\Delta T}\Delta T + \beta_{BDC}BDC + \beta_{QBO}QBO + \epsilon \tag{1}$$

Where ΔT is the temperature of the troposphere, BDC is the strength of the Brewer-Dobson circulation, QBO represents the phase of the QBO, and epsilon is the residual. Dessler et al. (2013) analyzed the 21^{st} century trend in one chemistry-climate model (hereafter, CCM; they are similar to general circulation models, but with a more realistic stratosphere and higher vertical resolution in the TTL) and found that the regression model worked well in reproducing the CCM’s $[H_2O]_{entry}$ trend over the 21^{st} century. They concluded that the increase in $[H_2O]_{entry}$ was driven by the increase in tropospheric temperatures, which was partially offset by a strengthening BDC.

Dessler et al. (2013)’s regression method provides a novel way to examine the regulation of $[H_2O]_{entry}$ in CCMs and compare it to observations. The purpose of this paper is to ~~use this technique to examine a set of CCMs, with the goal of providing insight into the realism of the models~~see whether this linear decomposition of $[H_2O]_{entry}$ variability holds in most CCMs and whether the same factors dominate.

5 Models

We analyze model output from 7 CCMs participating in Phase 2 of the Chemistry-Climate Model Validation Project (CCMVal-2) (Morgenstern et al. (2010); SPARC (2010)) and output from 5 CCMs participating in Phase 1 of the Chemistry-Climate Model Initiative (CCMI-1) (Morgenstern et al. (2017)). Table 1 lists the model specifics and documentation.

We use simulations from the REF-B2 scenario of CCMVal-2. In this scenario, greenhouse gas concentrations during the 21st century come from the A1B scenario, which lies in the middle of the SRES scenarios (IPCC, 2001). Ozone-depleting substances come from the halogen emission scenario A1 ~~described by~~ (WMO, 2007). CCMVal-2 specifics can be found in SPARC (2010) and Morgenstern et al. (2010). We use the refC2 scenario of the CCMI-1. In this scenario, greenhouse gas concentrations come from the RCP 6.0 scenario (Meinshausen et al., 2011) and ozone-depleting substances come from the halogen emission scenario A1 ~~described by WMO (2014)~~(WMO, 2014). CCMI-1 model specifics can be found in Morgenstern et al. (2017). In order to maintain a consistent reference period between models, our analysis covers 2000-2097, which we will hereafter refer to as "the 21st century ".

For each model, we fit CCM $[H_2O]_{entry}$ using the multivariate linear regression (MLR) model described above. We use tropical average 80-hPa water vapor volume mixing ratio as a proxy for $[H_2O]_{entry}$ (all tropical averages in this paper are averages over 30°N-30°S).

For our BDC index, we use 80-hPa diabatic heating rate (see Fueglistaler et al. (2009a) for details). Within models, studies have shown that the strength of the BDC increases throughout the 21st century, primarily resulting from increasing greenhouse gases (e.g. Austin and Li, 2006; Garcia and Randel, 2008; Li et al., 2008; Oman et al., 2008). Observations generally confirm that tropical upwelling into the lower stratosphere has strengthened (Bönisch et al., 2011; Randel and Thompson, 2011; Young et al., 2012;

However, the BDC is not a directly observable circulation, and different variables including trace gas abundances, residual velocity, mean age of the air, and diabatic heating have been used (Rosenlof et al., 1997; Randel et al., 2006a; Okamoto et al., 2011; Seviou

Thus, depending on the variable used, the strength of the connection between the BDC term and $[H_2O]_{entry}$ may change.

The tropospheric temperature index is the 500-hPa tropical average temperature~~, and for~~. For the few CCMI-1 simulations that only produce variables on hybrid pressure levels (CMAM, CCSRNIES-MIROC3.2, and MRI-ESM1r1), we choose a hybrid pressure level close to the 500-hPa pressure surface (See Table 1). ~~All of these choices are similar to those used by Dessler et al. (2013, 2014).~~

For the QBO index, we take the standardized anomaly of equatorial 50-hPa zonal winds (anomalies in this paper are calculated by subtracting the mean seasonal cycle). By examining 21st century 50 hPa zonal winds (shown in supplement figures), we find that only 5 of the 12 models simulate a QBO (table 1). As a result, we do not expect the QBO to significantly impact $[H_2O]_{entry}$ in many of the models.

All of these choices are similar to those used by Dessler et al. (2013, 2014). The MLR returns the coefficients for each regressor in Equation 1, along with an uncertainty for each coefficient. Unless otherwise noted, we use 95%-confidence intervals in this paper. Autocorrelation in the residuals is accounted for in the uncertainties following Santer et al. (2000). Finally, we will illustrate results with the MRI model; figures showing results derived from the other models can be found in the supplement.

6 21^{st} Century Analysis

We first analyze the long-term trend in $[H_2O]_{entry}$ over the 21^{st} century. To do this, we calculate annual average values of $[H_2O]_{entry}$ and perform a MLR against annual averages of the indices for BDC, QBO and ΔT. For consistency, all annual average time series have had the 2000-2010 mean subtracted out. Most models simulate $[H_2O]_{entry}$ increasing during the 21^{st} century (Gettelman et al., 2010; Kim et al., 2013). However, recent observational studies have concluded that no significant historical trend in water vapor entering the lower stratosphere exists (Scherer et al., 2008; Hegglin et al., 2014; Dessler et al., 2014).

Figure 1 shows that the fits to most of the models generate adjusted R^2 values greater than 0.8. The NIWA-UKCA century MLR has the lowest adjusted R^2, with a value of approximately 0.6. Overall, this result confirms the result of Dessler et al. (2013) that the regression model does ~~an excellent job~~ a good job job reproducing the models' $[H_2O]_{entry}$. Because we have left long-term trends in the time series, we will refer to this as the “trended analysis”.

6.1 Detrended 21^{st} Century

One concern with the trended analysis is that the $[H_2O]_{entry}$, BDC, and ΔT time series are all dominated by long-term trends. In such a case, an MLR may produce a high adjusted R^2 even if there is no actual relation between the variables. To eliminate the influence of long-term trends on adjusted R^2, we detrend each variable using a Fourier Transform filter (Donnelly, 2006) to remove long-term variability ($>$ 10 years). We then use the MLR on the detrended $[H_2O]_{entry}$ and the detrended indices. Detrending by removing the long-term linear trend yields similar results.

Figure 1 shows the adjusted R^2 for the detrended calculation. For most of the models, the adjusted R^2 for the detrended MLR is ~~only slightly~~ moderately smaller than that for the trended one. This confirms that the long-term trends in the data tend to inflate the adjusted R^2, at least a bit~~, and also~~. But we also confirm that the models' detrended ~~variability are~~ $[H_2O]_{entry}$ is also well represented by the same linear model (Equation 1). Large differences do exist for some CCMs. For instance, the CCSRNIES trended century MLR captures approximately 90% of the variance in $[H_2O]_{entry}$, while the detrended century MLR only explains about 40% of detrended variance; the CNRM-CM5-3, NIWA-UKCA, and WACCM show something similar.

6.2 Physical Process Effects

The coefficients from the trended and detrended calculations are listed in Tables 2 and 3 respectively. The product of the regression coefficient and its index quantifies that process' impact on $[H_2O]_{entry}$. As an example, MRI $[H_2O]_{entry}$ increases by about 1.2 ppmv during the 21^{st} century (Figure 2). The regression shows that this is the result of a large increase in $[H_2O]_{entry}$ due to ΔT increases (1.5 ppmv) that is offset by a strengthening BDC, which reduces $[H_2O]_{entry}$ by approximately 0.3 ppmv. The regression finds virtually no change in $[H_2O]_{entry}$ in response to the QBO.

Figure 3 shows that $[H_2O]_{entry}$ increases as ΔT increases in all models and that the ΔT regression coefficients are similar for both trended and detrended MLRs. The coefficient for individual models ranges from 0.1 to 0.6 ppmv K^{-1}, with an average of 0.32 ppmv K^{-1} and a standard deviation of 0.15 ppmv K^{-1}. It is worth pointing out that the models can get the right answer for the wrong reason. For example, spurious diffusion of water vapor through the tropopause has been shown to be an issue in models (e.g. Gettelman et al., 2010; Hardiman et al., 2015). This may impact the relationship between $[H_2O]_{entry}$ and tropospheric warming, thereby biasing our results. However, Dessler et al. (2016) was able to accurately simulate the stratospheric trend in two CCMs using a diffusion-free trajectory model, showing that, in some models at least, this is not an issue.

This figure also shows that the BDC coefficient is generally negative, meaning that a strengthening BDC reduces $[H_2O]_{entry}$. This ~~is consistent with previous research, which showed that a stronger BDC reduces TTL temperatures and lower-stratospheric water vapor (Randel et al., 2006a; Gilford et al., 2016).~~ relation arises from well established physics that a strengthening BDC should cool the tropopause, reducing water vapor entering the stratosphere (e.g. Holton et al., 1995). This anticorrelation between BDC strength and TTL temperatures has been observed (e.g. Yulaeva et al., 1994; Flury et al., 2013), and this has been identified as the cause of the stratospheric "tape recorder" (Mote et al., 1996). This anticorrelation has also been identified as the cause of the large drop in $[H_2O]_{entry}$ around 2000 (e.g. Randel et al., 2006b; Dhomse et al., 2008). The coefficient for individual models ranges from -12. to +4.3 ppmv (K/Day)$^{-1}$, with an average of -3.55 ppmv (K/Day)$^{-1}$ and a standard deviation of 4.45 ppmv (K/Day)$^{-1}$. Two models (CNRM-CM5-3 and NIWA-UKCA) yield positive BDC coefficients, indicating potential problems with these models. And the ~~MRI-ESMr1 produces, relative to other similar models, much larger BDC coefficients than MRI~~magnitude of the MRI BDC coefficients are about two times larger than those produced by MRI-ESM1r1. This could explain why the detrended adjusted R^2 value for ~~MRI-ESMr1~~ MRI-ESM1r1 is so much smaller than that of MRI.

Figure 3 shows that all QBO regression coefficients are small, generally within ± 0.04 ppmv, with even the sign of the effect in doubt. Interestingly, one of the CCMs not simulating a QBO, CMAM-CCMI, produces the largest QBO regression coefficients of 0.082 ±0.04 and 0.077 ±0.04 ppmv for the trended and detrended calculations, respectively. Among CCMs that do simulate a QBO, the ensemble average QBO regression coefficient does not differ much from the same quantity (approximately 0 ppmv) for the other models. We will discuss this further in the next section.

As can be seen in the plots for individual models in the supplement, the variability in $[H_2O]_{entry}$ in a few models comes almost entirely from the variability in BDC, with almost no variability in the ΔT time series (other than the long-term trend). That means that the ΔT term, which is almost a pure trend, will fit whatever is left after matching the interannual variability and trend of the QBO time series.

We have also calculated the long-term linear trend of $[H_2O]_{entry}$ for each model, as well as the trend in each component of $[H_2O]_{entry}$, as determined by the multivariate fit (e.g., the trend in the components plotted in Fig. 2). We find that ΔT makes the largest contribution to the trend in $[H_2O]_{entry}$, with a smaller negative effect from the a strengthening BDC on $[H_2O]_{entry}$, and a trend of close to zero for the QBO (Figure 4).

To provide additional information about the relative contribution from the individual terms in eq. 1, we have also calculated ~~standardized regression coefficients~~the regression coefficient using standardized variables. To do this, we take each regression coefficient and multiply it by the standard deviation of the associated regressor index. The values are listed in tables 2 and

3 and they confirm that, in the trended calculations, ΔT is the dominant cause of the trend in $[H_2O]_{entry}$. The BDC acts to reduce the trend, but its overall impact is much smaller than ΔT.

In the detrended calculations, the standardized ΔT regression coefficients are smaller than those from the trended calculations, while the magnitude of the BDC coefficients remains relatively constant. ~~For variability associated with short-term variability, this suggests that the BDC is~~ This results in the BDC being more important than ΔT for short-term variability. In all of our calculations, we find that the QBO has little impact on $[H_2O]_{entry}$. ~~Again, we will discuss this further in the next section.~~

7 Decadal Analysis

Ideally, we would compare the results of the last section to observations. Unfortunately, we don't have 100 years of observations to test the models against. Instead, we will compare regressions of 10-year segments from the CCMs to regressions of 10-years of observations. This will help us evaluate how good the models are and provide us with an indication of how representative a single decade is.

~~Specifically~~To do this, we split the 21^{st} century of each CCM run into 10 decades (2000-2010, 2010-2020, 2020-2030, 2040-2050, etc.) and fit each individual decade using the regression model (Equation 1). The regression calculation used on each 10-year segment is identical to the century analysis, except monthly averaged anomalies of all quantities are used instead of annual mean anomalies. Following Dessler et al. (2014), decadal regression terms are lagged in order to maximize MLR fit: we lag ΔT by 3 months, the BDC by 1 month, and the QBO by 3 months. These lags reflect the time between changes in each index and the impact on $[H_2O]_{entry}$.

Figure 5 shows the median ± one standard deviation of the ten decadal adjusted R^2 values generated by each CCM. The ensemble average is 0.61±0.25, with some spread among the models. Also plotted are the adjusted R^2 from two regressions of the tropical average Aura Microwave Limb Sounder (MLS) 82-hPa water vapor mixing ratio observations from ~~Dessler et al. (2014)~~2004-2014 (Dessler et al., 2014). One regression uses Modern-Era Retrospective Analysis for Research and Applications reanalysis (MERRA) ~~data~~ (Rienecker et al., 2011) and the other uses European Centre for Medium-Range Weather Forecasts interim reanalysis (ERAI) (Dee et al., 2011) for the ΔT and BDC indices; the QBO index ~~is standardized anomaly of monthly and zonally averaged equatorial 50-hPa winds obtained from the NOAA Climate Prediction Center (http://www. cpc.ncep.noaa.gov/data/indices). The MLS data covers the time period 2004-2014.~~ in both regressions are from observations, as calculated in Dessler et al. (2014).

Many of the models have a range of adjusted R^2 values that overlap with the observational regression. However, ~~of~~ the models producing the smallest decadal adjusted R^2 values~~: these are~~, CCSRNIES CNRM-CM5-3, and NIWA-UKCA, are also the models that produced the poorest fits to long-term detrended $[H_2O]_{entry}$. ~~In particular, CCSRNIES CNRM-CM5-3, and NIWA-UKCA, have the smallest adjusted R^2 values for both detrended and decadal~~ This provides some evidence that analysis of just a decade of $[H_2O]_{entry}$ can provide insight into the long-term behavior of that quantity.

Figure 6 shows the median and one standard deviation of each coefficient (values are listed in table 4), along with the coefficients from the regression of the MLS data (taken from Table 1 of Dessler et al. (2014)). We find that the CCMs agree unanimously that increases in ΔT are associated with increased $[H_2O]_{entry}$, though the CCM ensemble tends to underestimate the observational estimate. The only models that don't fall within both observational ranges are CCSRNIES, CMAM-CCMI, and CNRM-CM5-3.

In addition, the spread between the different decades for a single model tends to be small. The coefficient for individual models ranges from 0.01 to 0.4 ppmv K^{-1}, with an average of 0.15 ppmv K^{-1} and a standard deviation of 0.11 ppmv K^{-1}. This ~~gives us some~~ provides additional confidence that the comparison between the CCMs and one decade of observations is meaningful.

Figure 6 shows that there exists significant spread in the CCMs' decadal BDC regression coefficients. The coefficient for individual models ranges from -8.4 to +2.9 ppmv (K/Day)$^{-1}$, with an average of -3.55 ppmv (K/Day)$^{-1}$ and a standard deviation of 3.58 ppmv (K/Day)$^{-1}$. On all timescales, we expect a strengthening BDC should cool the TTL and reduce $[H_2O]_{entry}$, so the coefficient should be negative. We see that the median is indeed negative for all CCMs except for the CNRM-CM5-3 and NIWA-UKCA ~~, both of which yield a positive median BDC coefficient~~ (these models also generated positive BDC coefficients for the century analysis).

Comparing to observations, we find that the model ensemble does well. The CCSRNIES, CCSRNIES-MIROC-3.2, CMAM, CMAM-CCMI, LMDZrepro, MRI-ESM1r1, and WACCM decadal BDC regression coefficients fall within 95% confidence of MERRA, and ~~only~~ the CCSRNIES-MIROC-3.2, LMDZrepro, and WACCM fall within 95% confidence interval of ERAI. As with the ΔT coefficient, the spread between the different decades for a single model tends to be small~~; this again gives us some confidence in our comparisons to analysis of a single decade of observations~~.

Figure 6 shows that, for all CCMs, the ensemble average decadal QBO coefficient is approximately 0 ppmv. ~~But even those~~ For those CCMs that do simulate a QBO, ~~as seen in the century analysis, see little impact on $[H_2O]_{entry}$ from it, with an ensemble average of~~ the ensemble average coefficient is 0.02±0.03 ppmv. This is significantly smaller than the response to the QBO in the observations~~, and this appears to be a clear deficiency in the model ensemble.~~ . Only CCSRNIES-MIROC3.2 and CMAM-CCMI decadal regressions produce QBO coefficients approaching those from both observational regressions. Again, CMAM-CCMI does not simulate a QBO, and it is not clear to us why the model does so well in this aspect of our analysis.

Previous studies found that the QBO significantly influences TTL temperatures and subsequently $[H_2O]_{entry}$ (Zhou et al., 2001; Geller et al., 2002; Liang et al., 2011), so the lack of response in the model ensemble appears to be a problem in the models. Previous studies have investigated this issue, finding that a higher vertical resolution within the stratosphere can help resolve the QBO's impact on the lower stratosphere (Rind et al., 2014; Anstey et al., 2016; Geller et al., 2016). Clearly, this needs to be investigated further.

Similar to both the trended and detrended regression analysis, we calculated ~~standardized regression coefficients for the decadal regressions~~the regression coefficients using standardized variables of the decadal analysis, and the values are listed in

Table 4. Within most models, we see that the BDC, on decadal timescales, has the largest impact on $[H_2O]_{entry}$, with ΔT having a smaller impact.

8 Century and Decadal Regression Coefficient Comparison

One interesting question is whether the regression coefficients from the decadal analyses are related to regression coefficients from century regressions. To answer this, Figure 7 shows the coefficients from the trended century regressions of each CCM plotted against the median of the decadal regressions from the same CCM. Also shown is a linear least-squares fit to the points. For the ΔT coefficient, the best fit line is:

$$\beta(\Delta T, century) = 1.21 \pm 0.44\beta(\Delta T, decade) + 0.13 \pm 0.08 \tag{2}$$

All uncertainties are 95% confidence intervals. Thus, the ΔT coefficients from the trended MLRs are slightly larger than those from the decadal MLRs. Using values of $\beta(\Delta T, decade)$ from MLS observations and this fit, we predict $\beta(\Delta T, century)$ of 0.50 ±0.06 and 0.55 ±0.08 ppmv K^{-1} for MERRA and ERAI regressions, respectively.

For the BDC coefficient, the best fit line is:

$$\beta(BDC, century) = 1.16 \pm 0.32\beta(BDC, decade) + 0.56 \pm 1.56 \tag{3}$$

The BDC coefficients from the trended MLRs also have a slightly larger magnitude than those from the decadal MLRs. By fitting the observed values of $\beta(BDC, decade)$ through equation 3, we predict $\beta(BDC, century)$ values of $\beta(BDC, century)$ of -3.45 ±1.09 and -2.34 ±1.09 ppmv $(\mathrm{K/Day})^{-1}$ for MERRA and ERAI regressions, respectively.

For the QBO coefficient, the best fit line is:

$$\beta(QBO, century) = 0.75 \pm 0.40\beta(QBO, decade) + 0.004 \pm 0.01 \tag{4}$$

The QBO coefficients from the trended MLRs are slightly smaller than those from the decadal MLRs. Again, using equation 4, we predict $\beta(QBO, century)$ values of 0.09 ±0.03 and 0.09 ±0.02 ppmv for MERRA and ERAI regressions, respectively.

9 Conclusions

Climate models predict that tropical lower-stratospheric humidity ($[H_2O]_{entry}$) will increase as the climate warms, with important implications for the chemistry and climate of the atmosphere. We ~~demonstrated~~ demonstrate in this paper ~~a new way~~ that the regression used by Dessler et al. (2013, 2014) can be used to quantify the physical processes underlying these model trends and variability in an ensemble of CCMs. Our method is based on regressing CCM $[H_2O]_{entry}$ time series against three processes that have been shown to be important to $[H_2O]_{entry}$: tropospheric temperature (ΔT), the strength of the Brewer-Dobson circulation (BDC), and the phase of the QBO. Our approach provides ~~more~~ insight into model processes ~~than~~ not available by simply comparing $[H_2O]entry$ to TTL temperatures.

We do this on two separate time-scales: 1) the 21st century, and 2) on decadal timescales. ~~We~~ Considering all of our analyses, we find that long-term increase in $[H_2O]_{entry}$, in the CCMs, is primarily driven by warming of the troposphere. This is partially offset in most CCMs by an increase in the strength of the Brewer-Dobson circulation, which tends to cool the tropical tropopause layer (TTL) (Randel et al., 2006a; Fueglistaler et al., 2014). ~~However, for the detrended data, we find a strengthening~~ For shorter-term internal variability, we find variability in the Brewer-Dobson circulation is of greater importance to the variability of $[H_2O]_{entry}$, consistent with Geller and Zhou (2007) and Dessler et al. (2016). The models show little impact from the QBO~~, in disagreement with observations (O'Sullivan and Dunkerton, 1997; Randel et al., 1998; Dunkerton, 2001; Fueglistaler and Haynes~~ ~~this appears to be a deficiency in the models~~.

The coefficients from regressions of individual decades in the CCMs can be compared to coefficients from regressions of observations covering a decade. Overall, the CCM ensemble ~~seems to reproduce~~ reproduces $[H_2O]_{entry}$ observations well, except for the fact that the CCMs simulate little response to the QBO, in disagreement with the observations ~~. In addition~~(O'Sullivan and Dunkerton, 1997; Randel et al., 1998; Dunkerton, 2001; Fueglistaler and Haynes, 2005; Choiu et al., 2006; Liang this appears to be a deficiency in the models.

That said, the good agreement ~~on~~ of the ensemble average hides some spread among the models, particularly in the response to the BDC. Of particular note, the CNRM-CM5-3 and NIWA-UKCA regressions generate positive BDC regression coefficients, contrary to the other models and contrary to our expectations.

Our overall conclusions are encouraging — the models appear to respond to the factors that control $[H_2O]_{entry}$ in realistic ways, providing some confidence in their simulations of $[H_2O]_{entry}$. ~~However, some~~ Nevertheless, our work has pointed out issues that should be resolved. Some models have clear problems, e.g., the models that predict $[H_2O]_{entry}$ will increase with a strengthening BDC. In addition, nearly the entire ensemble does not reproduce the observed variations of $[H_2O]_{entry}$ with the phase of the QBO. This analysis should help the modeling groups refine their models' simulations of the 21st century.

10 Data availability

~~This data~~ Both the CCMVal-2 and CCMI-1 data used in this study can be obtained through the British Atmospheric Data Center (BADC) archive.

Author contributions. KS and AD performed this analysis and wrote most of this manuscript. The other authors contributed information pertaining to their individual models and helped revise this paper.

Competing interests. The authors declare that they have no conflict of interest.

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Table 1. CCMs used in this analysis. The resolution is listed as (lat x lon x number of pressure levels). 31 vertical levels indicates CCM data is given on isobaric levels, while CCMs simulating data on >31 levels are given on sigma (hybrid-pressure) levels

CCM	Resolution	Dataset	Contains QBO	Institution	Reference(s)
CCSRNIES	2.8° x 2.8° x 31	CCMVal-2	No	NIES, Tsukuba, Japan	Akiyoshi et al. (2009)
CCSRNIES-MIROC3.2	2.8° x 2.8° x 34	CCMI-1	Yes	NIES, Tsukuba, Japan	Imai et al. (2013); Akiyoshi et al. (2016)
CMAM	5.5° x 5.6° x 31	CCMVal-2	No	EC, Canada	Scinocca et al. (2008)
CMAM-CCMI	3.7° x 3.8° x 71	CCMI-1	No	EC, Canada	Jonsson et al. (2004); Scinocca et al. (2008)
CNRM-CM5-3	2.8° x 2.8° x 31	CCMI-1	No	Meteo-France; France	Voldire et al. (2013); Michou et al. (2011)
GEOSCCM	2.0° x 2.5° x 31	CCMVal-2	No	NASA/GSFC, USA	Pawson et al. (2008)
GEOSCCM-CCMI	2.0° x 2.5° x 72	CCMI-1	Yes	NASA/GSFC, USA	Molod et al. (2012, 2015); Oman et al. (2011, 2013)
LMDZrepro	2.5° x 3.8° x 31	CCMVal-2	No	IPSL, France	Jourdain et al. (2008)
MRI	2.8° x 2.8° x 31	CCMVal-2	Yes	MRI, Japan	Shibata and Deushi (2008)
MRI-ESM1r1	2.8° x 2.8° x 80	CCMI-1	Yes	MRI, Japan	Yukimoto et al. (2011, 2012); Deushi and Shibata (2011)
NIWA-UKCA	2.5° x 3.8° x 31	CCMI-1	Yes	NIWA, NZ	Morgenstern et al. (2009, 2013)
WACCM	1.9° x 2.5° x 31	CCMVal-2	No	NCAR, USA	Garcia et al. (2007)

Table 2. Coefficients (βs) from regressions of trended $[H_2O]_{entry}$ time series, and the change in $[H_2O]_{entry}$ resulting from each process (βSTD()), where STD() is the standard deviation of each trended process.

Trended Regression						
CCM	ΔT		BDC		QBO	
	$\beta_{\Delta T}$	$\lvert\beta_{\Delta T}\rvert$STD($\Delta T$)	β_{BDC}	$\lvert\beta_{BDC}\rvert$STD(BDC)	$\beta_{\Delta QBO}$	$\lvert\beta_{QBO}\rvert$STD(QBO)
CCSRNIES	0.06±0.01	0.08±0.02	-0.67±0.95	0.01±0.02	1.7x10^{-2} ±0.01	7.9x10^{-3} ±0.006
CCSRNIES-MIROC3.2	0.40±0.06	0.39±0.06	-3.4±1.9	0.11±0.06	3.5x10^{-2} ±0.04	2.2x10^{-2} ±0.02
CMAM	0.26±0.02	0.39±0.03	-5.7±1.1	0.07±0.01	8.0x10^{-4} ±0.03	4.7x10^{-4} ±0.02
CMAM-CCMI	0.22±0.05	0.21±0.05	-3.8±2.6	0.06±0.04	8.2x10^{-2} ±0.04	3.8x10^{-2} ±0.02
CNRM-CM5-3	0.27±0.13	0.26±0.13	3.7±5.4	0.09±0.13	1.9x10^{-2} ±0.07	4.9x10^{-3} ±0.02
GEOSCCM	0.38±0.03	0.37±0.03	-6.7±0.82	0.21±0.03	-1.3x10^{-2} ±0.01	3.2x10^{-3} ±0.003
GEOSCCM-CCMI	0.27±0.03	0.27±0.02	-6.6±0.96	0.17±0.03	5.2x10^{-3} ±0.02	2.8x10^{-3} ±0.01
LMDZrepro	0.55±0.04	0.72±0.05	-8.3±2.1	0.10±0.04	1.4x10^{-2} ±0.04	6.8x10^{-3} ±0.02
MRI	0.57±0.03	0.58±0.03	-12.±1.3	0.34±0.04	-4.1x10^{-3} ±0.03	2.0x10^{-3} ±0.01
MRI-ESM1r1	0.36±0.05	0.36±0.05	-3.1±1.4	0.12±0.05	1.7x10^{-2} ±0.03	9.5x10^{-3} ±0.02
NIWA-UKCA	0.20±0.07	0.20±0.07	4.3±4.6	0.06±0.07	-1.0x10^{-2} ±0.07	5.9x10^{-3} ±0.04
WACCM	0.24±0.04	0.21±0.03	-3.5±1.2	0.05±0.02	1.5x10^{-2} ±0.03	4.7x10^{-3} ±0.008

The units of ΔT, BDC, and QBO are ppmv K^{-1}, ppmv (K/Day)$^{-1}$, and ppmv, while the units of $\beta_{\Delta T}$STD(ΔT), β_{BDC}STD(BDC), and β_{QBO}STD(QBO) are all ppmv. The uncertainty is the 95% confidence interval.

Table 3. Coefficients (βs) from regressions of detrended $[H_2O]_{entry}$ time series, and the change in $[H_2O]_{entry}$ resulting from each process (βSTD()), where STD() is the standard deviation of each detrended process.

Detrended Regression						
CCM	ΔT		BDC		QBO	
	$\beta_{\Delta T}$	$\lvert\beta_{\Delta T}\rvert$STD($\Delta T$)	β_{BDC}	$\lvert\beta_{BDC}\rvert$STD(BDC)	$\beta_{\Delta QBO}$	$\lvert\beta_{QBO}\rvert$STD(QBO)
CCSRNIES	0.05±0.02	0.02±0.006	-0.67±0.67	7.1x10^{-3} ±0.005	1.7x10^{-2} ±0.01	3.6x10^{-3} ±0.003
CCSRNIES-MIROC3.2	0.30±0.05	0.08±0.01	-4.3±0.83	0.08±0.02	2.8x10^{-2} ±0.01	1.7x10^{-2} ±0.009
CMAM	0.26±0.03	0.10±0.01	-5.3±0.84	0.05±0.008	7.0x10^{-4} ±0.02	1.9x10^{-4} ±0.006
CMAM-CCMI	0.26±0.05	0.05±0.01	-3.7±1.1	0.04±0.01	7.7x10^{-2} ±0.04	2.9x10^{-2} ±0.005
CNRM-CM5-3	0.19±0.05	0.08±0.01	0.20±1.1	2.5x10^{-3} ±0.01	-3.3x10^{-2} ±0.01	7.1x10^{-3} ±0.003
GEOSCCM	0.31±0.04	0.08±0.009	-6.6±0.65	0.09±0.009	-1.0x10^{-2} ±0.01	1.9x10^{-3} ±0.002
GEOSCCM-CCMI	0.25±0.04	0.07±0.01	-7.1±0.71	0.17±0.03	4.4x10^{-3} ±0.01	2.3x10^{-3} ±0.007
LMDZrepro	0.59±0.05	0.25±0.02	-5.4±1.1	0.05±0.02	-5.5x10^{-3} ±0.03	2.3x10^{-3} ±0.01
MRI	0.52±0.03	0.18±0.02	-11.±1.0	0.24±0.02	-4.6x10^{-4} ±0.02	2.2x10^{-4} ±0.01
MRI-ESM1r1	0.33±0.05	0.09±0.01	-4.3±0.61	0.10±0.01	5.5x10^{-3} ±0.01	3.0x10^{-3} ±0.007
NIWA-UKCA	0.15±0.08	0.04±0.02	2.9±1.6	0.04±0.02	-1.0x10^{-2} ±0.02	5.9x10^{-3} ±0.01
WACCM	0.23±0.05	0.06±0.01	-3.5±0.80	0.04±0.01	1.5x10^{-2} ±0.02	2.8x10^{-3} ±0.004

The units of ΔT, BDC, and QBO are ppmv K^{-1}, ppmv (K/Day)$^{-1}$, and ppmv, while the units of $\beta_{\Delta T}$STD(ΔT), β_{BDC}STD(BDC), and β_{QBO}STD(QBO) are all ppmv. The uncertainty is the 95% confidence interval.

Table 4. Median coefficients from the decadal regressions of $[H_2O]_{entry}$ monthly anomalies, and the change in $[H_2O]_{entry}$ resulting from each process (βSTD()), where STD() is the standard deviation of each decadal process.

Decadal Regressions						
CCM	ΔT		BDC		QBO	
	$\beta_{\Delta T}$	$\lvert\beta_{\Delta T}\rvert$STD($\Delta T$)	β_{BDC}	$\lvert\beta_{BDC}\rvert$STD(BDC)	$\beta_{\Delta QBO}$	$\lvert\beta_{QBO}\rvert$STD(QBO)
CCSRNIES	0.03±0.04	8.7x10^{-3} ±0.01	-1.23±1.34	0.01±0.02	5.26x10^{-3} ±0.02	1.5x10^{-3} ±0.005
CCSRNIES-MIROC3.2	0.10±0.17	0.03±0.02	-3.29±1.44	0.10±0.04	6.05x10^{-2} ±0.01	5.7x10^{-2} ±0.02
CMAM	0.19±0.09	0.05±0.03	-6.06±1.34	0.07±0.02	2.75x10^{-3} ±0.03	9.4x10^{-4} ±0.004
CMAM-CCMI	0.01±0.10	3.5x10^{-3} ±0.02	-4.70±1.29	0.07±0.03	6.13x10^{-2} ±0.01	3.0x10^{-2} ±0.02
CNRM-CM5-3	0.06±0.14	0.01±0.03	2.89±1.44	0.05±0.02	1.84x10^{-2} ±0.02	4.9x10^{-3} ±0.01
GEOSCCM	0.17±0.10	0.04±0.02	-6.31±1.19	0.13±0.03	-1.47x10^{-2} ±0.03	4.9x10^{-3} ±0.005
GEOSCCM-CCMI	0.11±0.16	0.02±0.03	-8.00±1.89	0.18±0.06	2.42x10^{-2} ±0.02	1.8x10^{-2} ±0.01
LMDZrepro	0.31±0.19	0.11±0.08	-2.71±2.71	0.07±0.05	1.27x10^{-2} ±0.01	6.9x10^{-3} ±0.03
MRI	0.35±0.09	0.12±0.04	-8.78±2.91	0.25±0.07	-6.56x10^{-3} ±0.06	4.6x10^{-3} ±0.03
MRI-ESM1r1	0.19±0.04	0.05±0.01	-4.72±0.71	0.13±0.03	1.17x10^{-2} ±0.03	8.9x10^{-3} ±0.02
NIWA-UKCA	0.05±0.29	0.01±0.06	2.11±3.26	0.04±0.05	-1.88x10^{-2} ±0.04	1.5x10^{-2} ±0.03
WACCM	0.15±0.12	0.03±0.03	-2.25±0.85	0.05±0.02	3.84x10^{-2} ±0.03	9.1x10^{-3} ±0.007
MLS/ERAI	0.34±0.17	0.11±0.05	-2.5±0.83	0.17±0.06	1.1x10^{-1} ±0.04	0.11±0.05
MLS/MERRA	0.30±0.20	0.11±0.07	-3.5±1.6	0.15±0.07	1.2x10^{-1} ±0.05	0.12±0.06

The units of ΔT, BDC, and QBO are ppmv K^{-1}, ppmv (K/Day)$^{-1}$, and ppmv, while the units of $\beta_{\Delta T}$STD(ΔT), β_{BDC}STD(BDC), and β_{QBO}STD(QBO) are all ppmv. The uncertainty represents the variability (one standard deviation) in the set of coefficients produced by each CCM. For observations, the error bars represent 95% confidence.

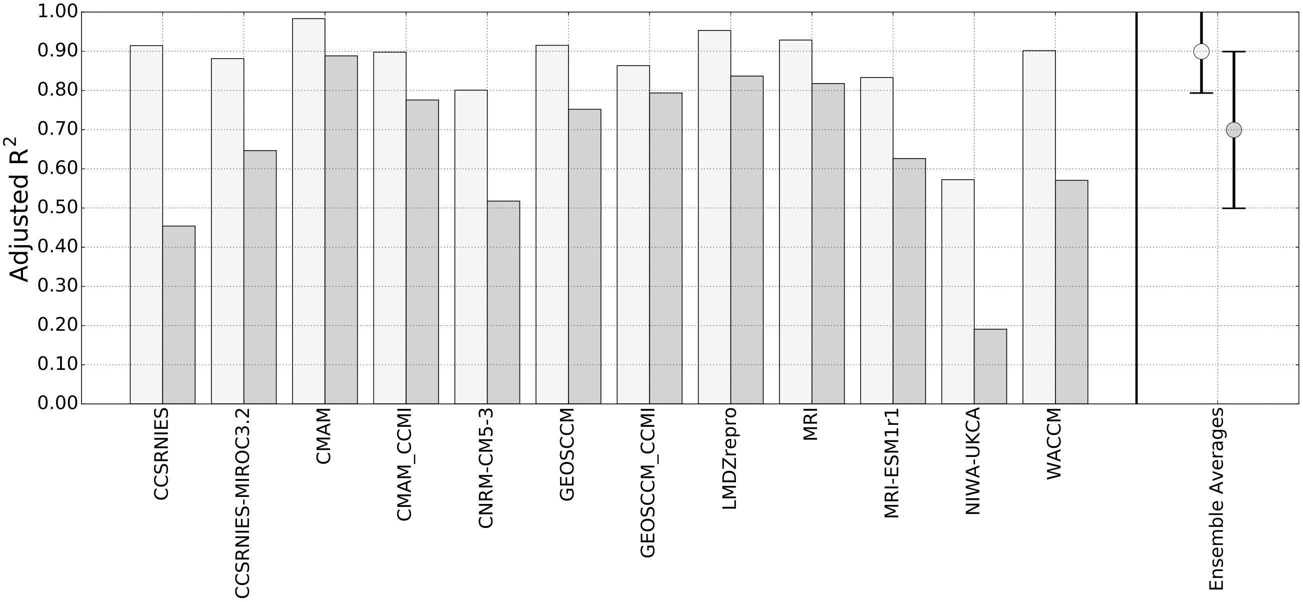

Figure 1. Bars corresponds to trended (light grey) and detrended (dark grey) adjusted R^2 values for annual-averaged data. The circles represent the ensemble mean, with error bars indicating ± one standard deviation of the CCM ensemble.

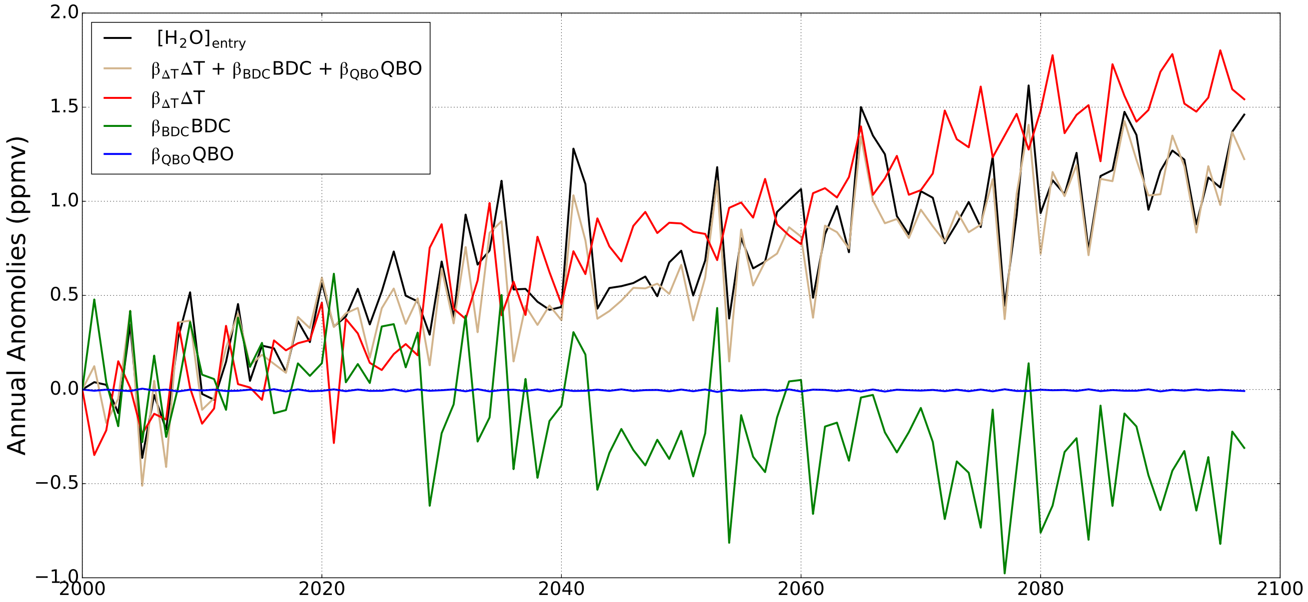

Figure 2. Time series of annual-averaged anomalies of $[H_2O]_{entry}$ from the MRI (black), and its reconstruction using a multivariate linear regression (brown). The red, green, and blue lines are the ΔT, BDC, and QBO terms from the regression, respectively.

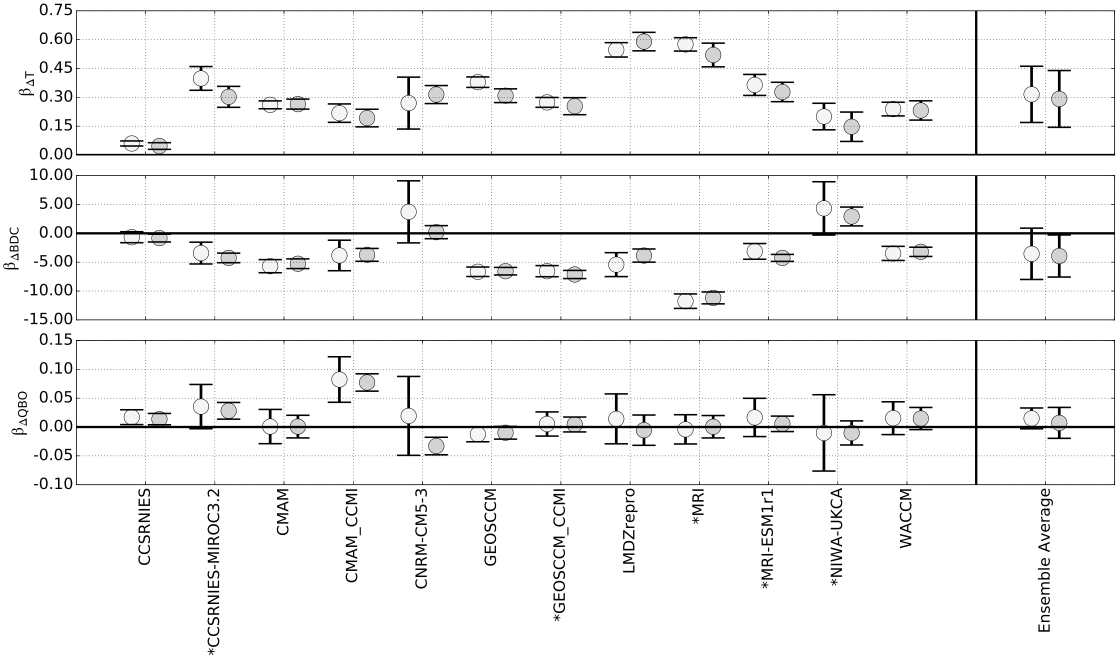

Figure 3. Circles show detrended (light grey) and trended (dark grey) coefficients for each model; error bars correspond to 95^{th} percentile confidence interval bounding each regression coefficient. An asterisk indicates models simulating a QBO. The ensemble mean corresponds to the average of all model coefficients. The ensemble mean coefficients are also represented by a circle, with associated error bars correspond to ±one standard deviation of the ensemble. The units of $\beta_{\Delta t}$,β_{BDC}, and β_{QBO} are ppmv/K, ppmv/(K/Day), and ppmv, respectively.

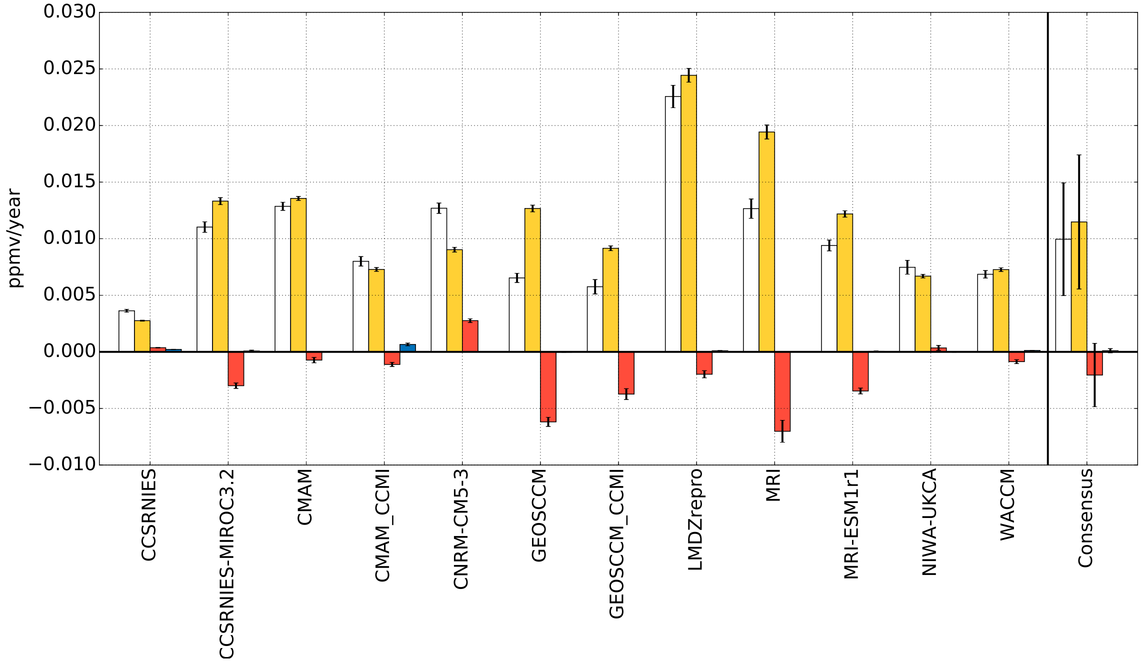

Figure 4. Trends in $[H_2O]entry$ (white) resulting from ΔT (yellow), BDC (red), and QBO (blue) predictor time series assuming the other predictors are held constant~~. Each predictor trend is then compared to the trend of the full regression (white)~~. Error bars represent 95% uncertainty. For many models, the contribution of the QBO is too small to be seen.

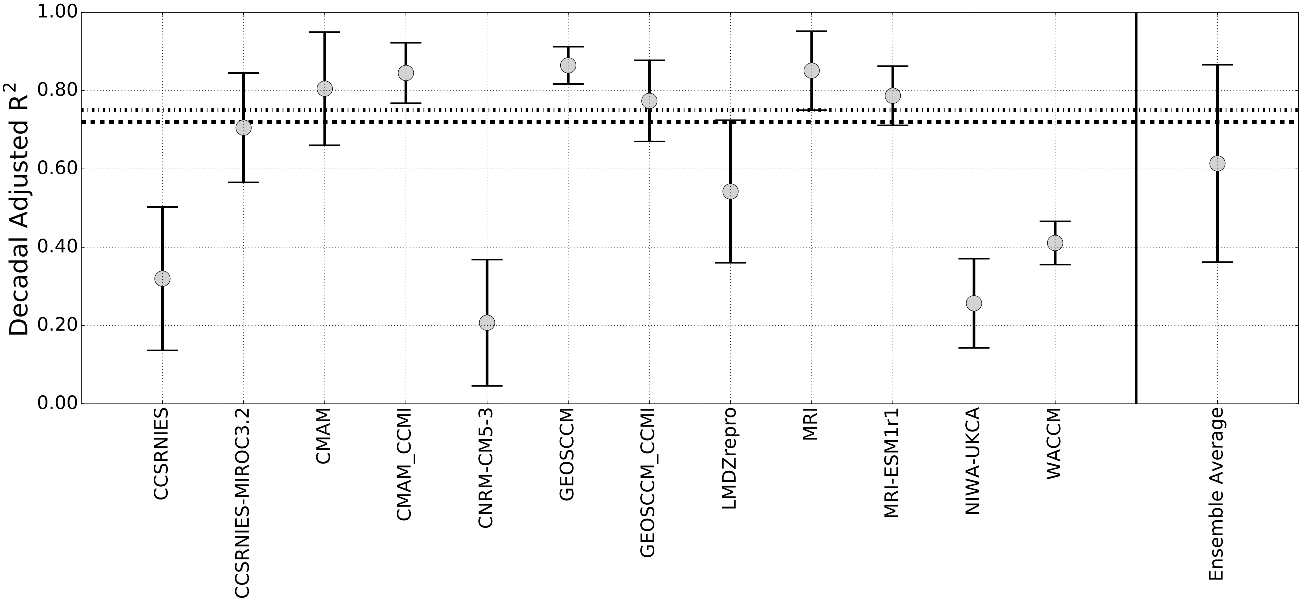

Figure 5. Circles represent the median of the adjusted R^2 value of the decadal fits. Errors correspond to the ± one standard deviation of the adjusted R^2 values. The CCM ensemble average is also plotted, along with error bars corresponding to ± one standard deviation of ensemble set of decadal adjusted R^2 values. The lines are adjusted R^2 values from observations combined with reanalysis (ERAI (dotted) and MERRA (dashed)) from Dessler et al. (2014).

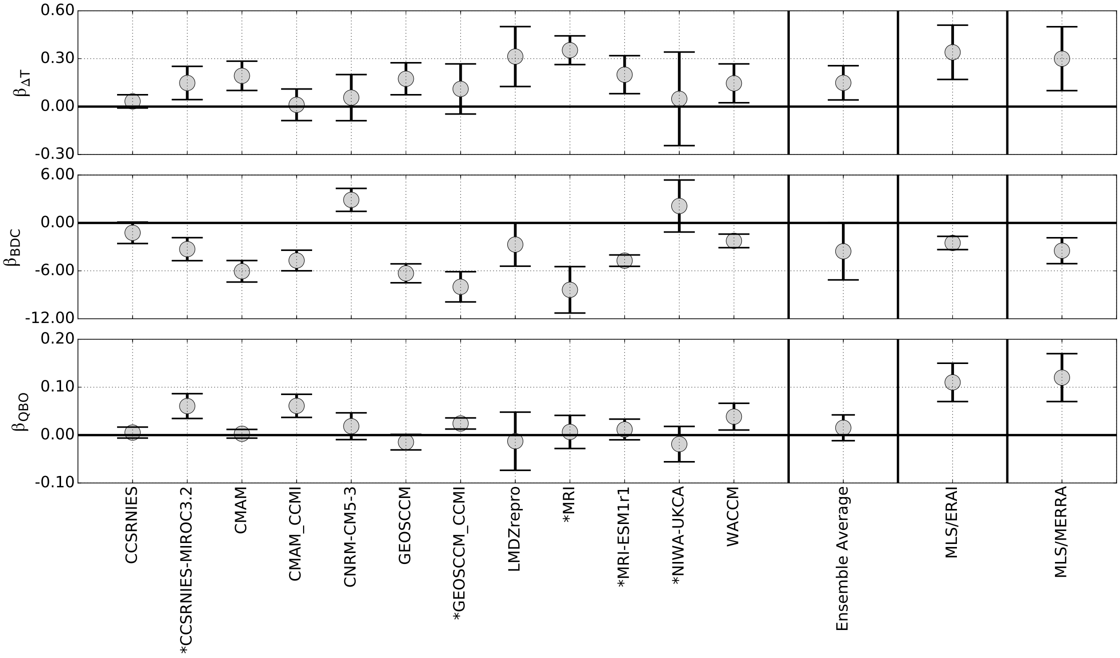

Figure 6. Circles represent the median decadal regression coefficient from each CCM, and error bars correspond to ± one standard deviation. An asterisk indicates that the model simulates a QBO. The ensemble mean corresponds to an average of all model coefficients. The ensemble mean coefficients are also represented by a circle, with associated error bars correspond to ±one standard deviation of the ensemble set of coefficients. Estimates from observations combined with reanalysis (Dessler et al., 2014) shown, along with 95^{th} percentile confidence interval. The units of $\beta_{\Delta t}$,β_{BDC}, and β_{QBO} are ppmv/K, ppmv/(K/Day), and ppmv, respectively.

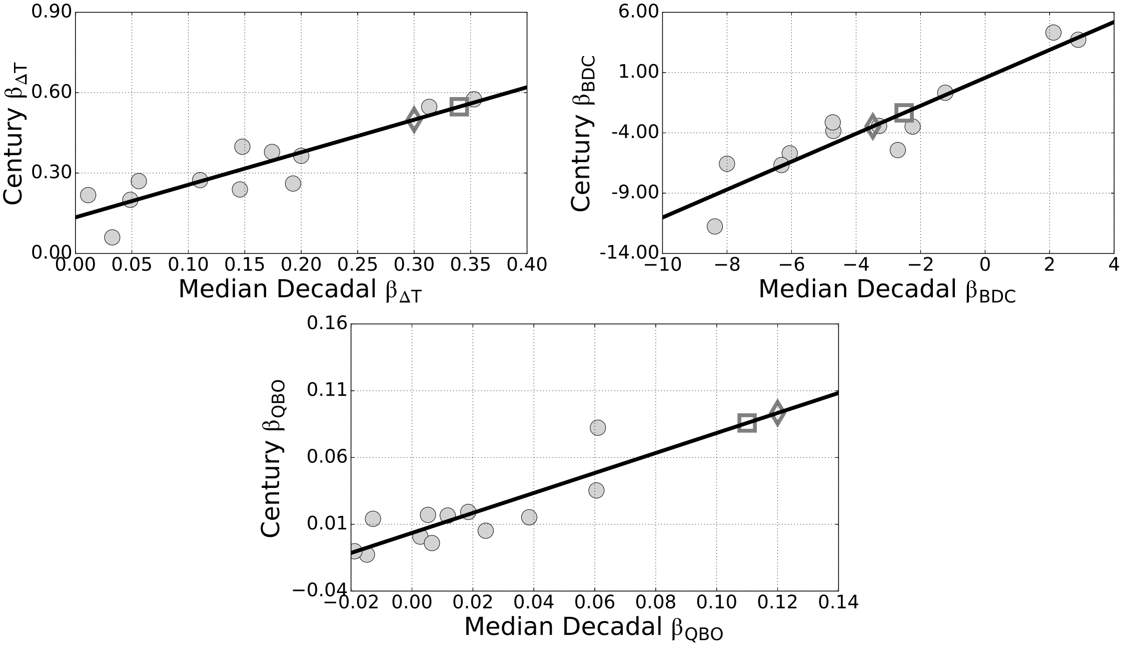

Figure 7. (Top Left) Scatter plots of trended ΔT regression coefficients (ppmv K^{-1}) vs. median decadal ΔT regression coefficients (ppmv K^{-1}) from each CCM. (Top Right) Same as top, but for BDC coefficients. (Bottom Middle) Same as top left and top right, but for QBO coefficient . Black lines in all plots correspond to a best fit line between the trended and decadal coefficients, and the observational coefficients ERAI (square) and MERRA (diamond) are fitted to each line (from Dessler et al. (2014)).