Atmos. Chem. Phys. Discuss., 15, C7849–C7854, 2015
www.atmos-chem-phys-discuss.net/15/C7849/2015/

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD
15, C7849–C7854, 2015

Interactive Comment

Interactive comment on "The millennium water vapour drop in chemistry-climate model simulations" *by* S. Brinkop et al.

S. Fueglistaler (Referee)

stf@princeton.edu

Received and published: 11 October 2015

General comments

Brinkop et al. present a study of the sudden drop of water entering the stratosphere in the year 2000 using the Chemistry-Climate Model EMAC. The model is forced with observed SSTs, and the QBO is imposed by nudging with observed stratospheric winds. An additional run where the model is nudged against ERA-Interim is presented. The paper is generally well written, with some technical details requiring clearer description (outlined below). My main concern with the paper is that the numerical model results presented do not support the key statements in the text - I am looking forward

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

to reading their rebuttal.

First, Figure 4 shows clearly that only the fully nudged model run (which I must assume to be almost identicaly to the ERA-Interim data used to nudge the model) qualitatively reproduces the drop in water entering the stratosphere as observed by HALOE. I just can't see how you can reach the conclusion from your model calculations that ENSO via SST pattern is key to the problem, when the runs that are forced with observed SSTs (and even QBO!) completely fail to produce a drop around the year 2000. While suspecting ENSO/SSTs to be involved is completely reasonable, the challenge is to demonstrate that this is indeed the case, and your model results - along with other model results (e.g. SPARC CCMVal2) - fail to demonstrate this connection. The conclusion that "appropriate boundary conditions" are required is not helpful given that only your fully nudged run - where essentially every variable is set to prescribed values (P24913/L5ff) - gives the qualitatively correct result.

Second, given that the fully nudged run (RC1SD) is presumably very similar to reanalysis data, it is not surprising that you get a drop - this has been known for a decade (see e.g. Figure 2 of Fueglistaler and Haynes (2005)); the challenge with the drop today is proper attribution to processes (see above), and accurate quantification and reproduction of the magnitude. As shown in Fueglistaler et al. (2013), all model reconstructions using a wide range of available temperature data give what you also find: a drop, but the magnitude of pre/post 2000 is smaller than observed by HALOE; however, if one compares to SAGEII, the agreement is much better. The manuscript does not mention this conundrum, and is vague in terms of assessment of the success of the model result (first, it is noted that there is a "small" discrepancy, while later the discrepancy is quantified to be 50% - see comments below).

Specific comments

ACPD
15, C7849–C7854, 2015

Abstract, line 11: You date the "start date" to the "early days of 2000"; Fueglistaler (2012) argue, based on their Figure 9, that the drop dates around October 2000; it would be helpful if you could comment. (See also my comment below for P24916/L28 that the text does not explain how you determine this date.)

Further: "We show that the driving forces ... are tropical sea surface temperatures ..." As stated above, I don't think that your model results support this statement. Rather, your Figure 4 demonstrates the failure of the SST-based model runs. The question then is whether (i) the model fails to correctly reproduce the effect of SSTs on the TTL, or whether (ii) some other process is involved.

P24911/L10: "This has become the big conundrum ..." suggest to reformulate.

P24911/L13: "An increase in stratospheric water is expected..." This is a bold statement, not supported by any reference. I assume that your statement is not based on theoretical arguments, but on model results - in which case it would be fair to cite the papers (I assume that you think of CCMVal results, so please refer this work here).

P24912/L12: "Randel and Jensen (2013) state ..." I found this section unclear; are you saying here that your paper is to some extent a rebuttal to their statement concerning model results? Also, since you argue here that your model runs perform better than those referred to by Randel and Jensen (line 27: "... indicating that it is possible to ...") it would be useful if you could briefly list here what **exactly** is better in your model than in those that you compare to - "appropriate boundary conditions" (Line 28) is very vague. In any case, as already state above, I don't think that your results support your claim.

ACPD
15, C7849–C7854, 2015

P24914/L2: Please be more specific what "slightly nudged" means; reference to Jockel et al. (2015) is not sufficient since the QBO is a key factor. Of particular importance here would be over which pressure levels you nudge the model (to the Singapore wind, I presume?).

P24914/L25: It is not quite correct to state that previous studies focused mostly "on its absolute value"; see e.g. Fueglistaler and Haynes (2005; their Figure 2a); Fueglistaler (2012); Fueglistaler et al (2013). Also, note that the focus on some period-average is not a deficit of the studies you quote here, but is due to the fact that the year 2000 drop is unusually long; and the long duration is - aside from the magnitude of the drop - the main reason we're interested in this event. None of the oscillations in the satellite record after the pre/post-2000 change comes even close.

P24915/L14ff: "In Fig 1 we show that our RC1SD simulation is able to closely reproduce the water vapour fluctuations as observed ..." and "is in accordance with observed values." and later "... drop in 2000 is slighty underestimated (about -0.12ppmv),..". Later on you quantify the mismatch as 50As pointed out above, your results are in line with previously reported results; the remaining problem is the exact magnitude.

P24915/L12ff: The temperature dependence of Clausius-Clapeyron indeed poses a challenge for water vapor amplitudes in the presence of a mean temperature bias; however one can address this problem by analysing the amplitudes in terms frost point temperature variations (see Fueglistaler et al. (2013) for a discussion of the impact of a mean temperature bias on H2O variations; their Figure 5b/para33 is for the annual cycle, but extension to interannual variability is simple.)

ACPD

15, C7849–C7854, 2015

P24916/L28: Here you state that the observations have a larger drop by 50% in the tropics - whereas above (p24915/L23) you wrote "slightly underestimated". Also - please explain how exactly you determine the "drop date"; as noted above, we have argued that the drop occurs around October (Fueglistaler 2012; and follow-up papers) - please explain the difference.

P24917/L19ff: It is rather confusing that your temperatures (Fig 5) seem to give a different picture than your water vapor (Fig 4); for example, in Figure 5 the black and red lines are reasonably similar, which cannot be said for Figure 4; please explain.

P24918/L24: Statistics based on ad-hoc thresholds are generally not useful; and I am concerned that your analysis here (0.5ppmv for one model run, 0.2ppmv for another model run) falls into this category. Please show that this is not a concern here, or remove the analysis.

P24919/L2ff: "... eruption of Mt Pinatubo had a significant impact on temperature and water vapour ...". Please provide a reference for this statement; see also detailed discussion in Fueglistaler (2012), and Fueglistaler et al. (2014; ACP): Observations suggest that part of the aerosol warming tendency was offset by an increase in dynamical forcing of upwelling. Models generally have problems to reproduce this effect and therefore produce a massive moistening of the stratosphere - which is what you also find in your additional sensitivity run mentioned below on line 7.

P24920/L26/Figure 9 Please be specific which equation and terms you use.

P24921/L11f: Can you clarify - are you saying that nudging to ERA-Interim slows down the upwelling in the TTL? Or is this simply an artefact arising from a difference in the

ACPD

15, C7849–C7854, 2015

pressure level of the cold point tropopause in the free-running simulation relative to reality/nudged version?

P24922/L9: You could test in your model calculations whether the subtropics are involved; if it's only speculation please omit.

P24923/L1ff: If I understood correctly, you said earlier that the QBO nudging is equal in all model runs - why then this difference here? At what level do you truncate the nudging?

P24923/L13ff: I could not quite follow your reasoning here. ENSO is related to surface temperature anomalies, so I don't understand what you mean by "under normal SST conditions the influence (of ENSO, I assume?) on upwelling is smaller." What do you mean by "normal"? Please explain.

P24923/L13ff: Are you saying that you accept a time lag (between cause and effect) *varying* between 6 and 34 months? Please correct me if I misunderstood, but a scientific cause-effect relationship requires a well-defined time lag.

Summary and discussion: Based on my comments provided above, it is clear that I do not agree with much of this section; I will not reiterate here my points raised above.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 24909, 2015.