Atmos. Chem. Phys. Discuss., 6, S4857–S4861, 2006
www.atmos-chem-phys-discuss.net/6/S4857/2006/

Atmospheric Chemistry and Physics Discussions

ACPD

6, S4857–S4861, 2006

Interactive Comment

Interactive comment on “Stratospheric dryness” *by* J. Lelieveld et al.

Anonymous Referee #3

Received and published: 27 November 2006

Overall: I don’t think this paper definitively advances the debate on TTL dehydration (e.g. its conclusions on the role of overshooting convection on the basis of ozone comparisons are overstated). It has some new and somewhat interesting model-measurement variability comparisons. It advances a novel nudging approach, though I am not sure the advantages of this approach are stated as clearly as they might be. I am not in a position to comment on the originality of the QBO/SAO influences in stratospheric water vapor, though some of this seems new. The paper lacks a single main focus or argument, and sometimes seems like a process-oriented paper, and at other times like a model development paper. I would recommend potential publication with a serious attempt to address Major Issues 1 and 2. It would be especially important to document the mean and inter-annual temperature/water vapor variability of the model. Without these, it is hard to interpret anything else in the paper.

Major Issues

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

1. Quantitative Seasonal/Inter-annual Comparisons:

I would have liked to see a figure of the seasonal variation of the tropical mean cold point (or 100 hPa) temperature and water vapor mixing ratio in the model, and a comparison of each with some dataset. Figure 2 is interesting visually but doesn't give a quantitative impression of the water vapor mixing ratio biases in the model. Similarly, Figure 4 doesn't give an impression of the quantitative accuracy of the seasonal cycle of TTL temperatures. It would also have been useful to see a comparison of the model with the tropical mean HALOE record on inter-annual timescales. The ability to reproduce this variability would be an important test of the model. The paper seems to avoid these types of comparisons, and instead, focuses on PDF statistics. These comparisons of the variability are useful, but by avoiding quantitative comparisons of mean properties on the seasonal/inter-annual timescales, you get the feeling the paper is avoiding an important part of the debate, and does not go as far as others have with the HALOE dataset (e.g. Fueglistaler). One wants to know how a model responds in a mean way to forcings. There may be problems with our knowledge of the factors that drive water vapor variability on inter-annual timescales, as well as problems with the HALOE dataset itself, but they are what we have to go on for now. From a climate perspective, the variability is secondary.

2. Comparison with Ozone Mixing Ratios and Evidence for Overshooting

The paper does not reproduce the low ozone events observed by MIPAS and HALOE. This is interpreted as a lack of overshooting in the model. I see this as an overinterpretation. I am skeptical that any kind of ozone measurement by itself would ever present unambiguous evidence of convective overshooting plus mixing, as opposed to simply very high altitude non overshooting convection. How would one distinguish them? Also, "low" is defined here as less than 1000 ppbv, considerably larger than typical tropospheric mixing ratios of 20-60 ppbv, so that any tropospheric remnant would be potentially tiny. The SHADOZ ozonesonde dataset would have been much more useful in identifying low ozone events than a satellite measurement. Several papers have

ACPD

6, S4857–S4861, 2006

Interactive Comment

EGU

used this dataset to look at the incidence of low ozone events as a function of altitude (Folkins, JGR 107, 10.1029/2001JD001178, 2002 and Solomon, GRL 32, 2005). The frequency of low ozone events is very rare above 100 hPa, suggesting that, if there is overshooting convection above the tropical tropopause, it has undergone considerable mixing with ambient stratospheric air. Overshooting was originally invoked as a dehydration mechanism. Here, its absence from the model is being invoked as the reason why the model has a dry bias. I agree that overshooting could in principle be drying or moistening. However, there are numerous other reasons as to why the model might have an unrealistic dry bias. In this context quantitative comparisons on the seasonal variation of cold point temperatures would be useful.

Other Comments:

The height and pressures given in the first sentence are inconsistent. 150 hPa is roughly 14 km rather than 12 km, and 12 - 4 = 8 km would be roughly 380 hPa, not 200 hPa. In the next paragraph it refers to an extra-tropical exit of 100 hPa, about 16.6 km, which is strongly inconsistent with the mean height of the extra-tropical tropopause. These errors are distracting and should be fixed.

Current thinking is that the ozone layer has stabilized and may be showing early signs of recovery, so "being thinned" would be incorrect if a trend is implied. In any case, it's not clear why one would want to motivate this paper by a reference to changes in ozone levels.

Introduction, line 20. The hygropause is just a seasonal manifestation of the tropical tape recorder. It is true that it did historically play a role in motivating ideas about convective overshooting, but it doesn't any more. The current debate is really about the mechanisms behind the recorder mechanism, i.e. how the cold point temperature determines the entry water vapor mixing ratio. The real motivation here is puzzles such as why, for example, the tropical mean cold point temperature does not correspond to the tropical mean water vapor stratospheric entry mixing ratio. To emphasize: my point here is not that the authors are unaware of the tape recorder (they obviously are). My point is that the hygropause is not driving the debate as an outstanding issue, and hasn't in the recent past.

paragraph starting on line 21. The simplified fountain concept is no longer taken seriously. There is a series of papers (associated with authors such as Holton, Gettelman, and Fueglistaler) that have emphasized the role of horizontal advection through cold trap regions in accounting for the observed stratospheric water vapor entry mixing ratio. I.e., even though upwelling tends to happen almost everywhere in the TTL, the upward transport of air through the TTL is sufficiently slow (on the order of months) compared to timescales associated with horizontal advection, that air is repeatedly transported through and processed by the cold trap regions, so that the entry water vapor mixing ratio is largely set by the temperature of the cold trap regions rather than the tropical mean. If one wants to characterize the debate in terms of two schools of thought, it is a debate between overshooting convection and horizontal advection. The historical overview given here is dated.

Section 2, paragraph starting line 27. The motivation behind the use of the level of clear sky radiative radiative heating to define the base of the TTL is that, once an air parcel detrains from a convective cloud, radiative heating should primarily determine whether the air parcel ascends upward across PT surfaces into the stratosphere, or descends downward back to the surface. The existence of cloud radiative heating inside cumulonimbus clouds is irrelevant in this context. Clouds can perturb radiative heating rates outside clouds, and clouds can later form in situ, and introduce a diabatic forcing into a post-detrainment air parcel, as shown in the Hartmann article. However, this article makes no attempt to estimate the overall impact of in situ cirrus on clear sky heating rates, e.g. by averaging over some distribution of in situ cirrus and a distribution of their heating rates. The full sky heating rates given in Corti are not directly relevant to this discussion, since you are mainly picking up a signal from inside cirrus anvils. This paragraph misrepresents the intent behind using clear sky heating rates as a TTL

ACPD
6, S4857–S4861, 2006

base definition and should be modified.

Section 4. Model nudging. It wasn't clear to me why one wouldn't just use analysis winds and temperatures for this study. I gather it may be because analysis winds/temperatures are not suitable for long integrations and/or would not have a QBO/SAO? The motivation should be clearer.

page 11264, line 12. "reproduces the dehydration mechanism" Not sure what this meant. In reality, there are likely to be a variety of dehydration mechanisms.

page 11266, line 12. A cooling can't balance an ascent. An ascent would give rise to an adiabatic cooling, which would need a heating to balance it.

page 11269, line 7. By "injection of water" is meant that water vapor in the volcanic plume survived transport across the tropopause to reach the stratosphere? Seriously?

page 11271. The original "drain" was attributed to the mixing of cold overshooting air. Here you seem to have a radiative drain. These differences should be noted.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11247, 2006.