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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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This paper uses results from a numerical general circulation model to make deductions about the processes controlling the distribution of stratospheric water vapour. The model might well be as argued to be as good, or better, than any that has previously been used to study tropical dehydration processes and their implications for stratospheric water vapour (it has high vertical resolution and a relatively sophisticated representation of cloud processes). If the model results were carefully described and used to argue that particular mechanisms were or were not relevant, then the paper would have been impressive. But instead the paper seems to be written in the style of an extended summary of dehydration and associated processes with some assertions that this or that is or is not happening in the model. There is a tendency for the authors to comment on many things that are not really relevant to the main topic of the paper and which are not properly discussed (e.g. possible connection between the SAO and the QBO) and it is often difficult to know whether comments are intended to be interpreted as what is well-known from previous work, as speculative comment (as part of the general conclusions of the paper, or about previous papers by other authors), or as key deductions from the modelling work that is intended to be the focus of the paper.

My overall verdict is that I found the paper interesting to read, but I feel that the conclusions represent at best modest increments to what is known already. There is some good modelling work behind this paper, but the opportunity to use the modelling results to make compelling scientific points has been missed. Of the model diagnostics that were presented, I found the vertical velocity fields potentially misleading and have significant reservations about some of the statements about ”fountains” and ”drains”.

The paper would be much more effective if it put greater emphasis on detailed model diagnostics and was much clearer about what was being deduced from them. My view is that this aspect of the paper must be significantly strengthened and the tendency for wide-ranging speculative comment be significantly reduced if the paper is to be suitable for publication in ACP.

I have made several detailed comments below that are intended to helpful to the authors.

Detailed comments:

p11249 l2: ”IR emission by H2O is the main cooling term?”. I can”t find a strong statement to this effect in the Mlynczak et al (1999) paper – statement, e.g. ”main”, seems surprisingly strong to me. Further reading of Gettelman et al (JGR 2004) and Thuburn and Craig (JAS 2002) clarifies this a bit. Note paragraphs [13]-[15] of Thuburn and Craig (2002) emphasising important role of CO2 – this doesn”t contradict what you say, but you might prefer to emphasise that H2O and CO2 together play the dominant role in the long-wave radiation balance.

p11249 l8: ”stratospheric air is moistened from about 2-4 ppmv at the tropical entry to 5-6 ppmv at the extratropical exit” – my impression (e.g. Figure 3.3 of the SPARC water vapour assessment – is that values of 5-6 ppmv are achieved only in the lowermost stratosphere (where there is a significant contribution from transport from the tropical upper troposphere). Your statement above might be interpreted as ”a typical air parcel is moistened from 2-4 to 5-6 ppmv as it passes through the stratosphere”, which is not really true.

p11249 l20: ’discovery of a water vapour minimum (hygropause) a few kilometers above the tropopause (Kley et al 1979)’ – I believe that the height of the water vapour minimum relative to the tropopause (or to the local cold point) is now accepted something that varies seasonally and can be explained as part of the ”tape recorder” phenomenon (e.g. see p213 of SPARC water vapor assessement). So the idea of the hygropause a few kilometers above the tropopause as a universal feature of the tropical atmosphere is now out-of-date – which is not to detract from the importance of the Kley et al observation at the time.

p11249 l22: Your characterisation of the first group of mechanisms doesn”t seem quite right to me. You characterise this group as emphasising ”the role of overshooting convection that penetrates the tropical tropopause ...’ I”d say the first group is broader than this – emphasising dehydration in convective processes, rather than specifically convective penetration of the lower stratosphere. In particular my reading of the Sherwood and Dessler (JAS 2001) paper is not that convective penetration into the lower stratosphere is required. What is required according to Sherwood and Dessler is that convection overshoots its level of neutral buoyancy (not that it overshoots the tropopause).

p11251 l1: This description of differential heating ’causing’ pressure forces and hence ’causing’ circumpolar flow is not how it would be described by modern dynamicist. It would be better to avoid the use of the term ’forces’ in the first line, replace by a more neutral term such as ’effects’ and then to say that the dynamical reponse of the atmosphere to differential heating is (in part) a circumpolar flow.

p11251 l20: The implication that net radiative heating or cooling is small because H2O

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cooling is compensated by O3 heating is misleading. The smallness is because the dynamical forcing away from radiative equilibrium is relatively weak.

p11252 l9: I think that 'extratropical wave dynamics' is too specific. There is clearly an important role for forces exerted in the subtropics and tropics by waves propagating from the extratropics, also for tropical phenomena such as the QBO. Simply replacing 'extratropical wave dynamics' by 'large-scale dynamics' would be safest.

p11255 l26: 'It is assumed that slow air parcel ascent in cold clouds occurs at ice saturation.' I found it difficult to understand the significance of this sentence. Is this a statement about some assumption that is made in the parametrization scheme for ice crystal growth?

p11258 l5: 'successfully simulates ... the QBO' – you presumably mean that the phenomenon of the QBO is simulated, not that the detailed phase evolution of the QBO is predicted in agreement with observations?

p11258 l8: 'we provide a user-friendly graphics tool to select data for geographical regions' – this is more in the style of an advertisement that a scientific paper.

p11262 l14-21: I found it difficult to assess the significance of this good agreement between MIPAS and model. What would the correlation between the two be if some climatological or large-scale mean were taken of the model or the observations? In other words, is the agreement good because the model has succeeded in reproducing the observed state of the atmosphere on, say, seasonal times scales and space scales of 1000s of km, or is it important that the model reproduces variability on shorter time scales and smaller space scales. (The 'nudging' could be playing an important role in either case.)

p11262 Figure 6: Another reason why temperatures and water vapour concentrations might be expected to be decorrelated at higher levels is that local saturation mixing ratio is less important. (This does not explain why there is high correlation between

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temperatures and water vapour concentrations at higher levels during NH winter, but this might result from the spatial organisation of the temperature field.) Is the correlation shown calculated considering different longitudes and days as independent pieces of data? A bit more information would be helpful here.

p11264 l7: ’This may indicate that the strongest dehydration at 100hPa takes place in air parcels that undergo intense radiative cooling, which induces their return to the troposphere.’ These seems highly speculative and not very serious. For one thing it seems to associated experiencing very low temperatures with undergoing intense radiative cooling – which is not necessary at all. Air parcels can cool rapidly as a result of dynamics (and if they do not subsequently warm as a result of dyanmics then they might well radiatively warm).

p11265 l14: ’Indirectly, this suggests that deep convective intrusions moisten the lower stratosphere.’ Again the intended significance of this statement is not at all clear. It seems to be a deduction from the apparent observations of Alcala and Dessler (2006), but if so, why is it being made here?

p11265 l24: ”consistent with the observed hygropause” – again this leaves the misleading impression that there is always a separation between the hygropause and the cold point, which I don’t believe is true – it depends on season. (See previous comment on this topic.)

p11266 l7-12: "The ascent through the TTL is accompanied by adiabatic and radiative cooling" – I”m confused. Parcels are (generally) ascending across isentropic surfaces and therefore surely undergoing net radiative heating. But perhaps, reading further, you are referring to the lower part of the TTL where air outside convection is descending, with radiative cooling. (That is presumably what you are seeing in your model.) See discussion in papers by Folkins and collaborators about the level of zero radiative heating, etc. So this needs clarification. ”If radiative cooling would be stronger it could balance the wave-driven ascent or even induce subsidence.” – this statement needs

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care. What exactly do you mean by ”if radiative cooling could be stronger”? If a cooling was applied then it could be partly balanced by subsidence and partly balanced by a decrease in temperature, with how much of each depending on dynamical details. I”m not really sure why the statement is needed in any case.

p11266 l23: ”This tropopause cirrus ... largely collocates with deep convection, ... ” – is this on the basis of a careful study of where deep convection occurs in the model? Also for what level are you making this statement – you show 4 levels in the Figure and the distributions are different between the levels.

p11267 l17: ’Nevertheless, also in this season the coldest temperatures coincide with the driest conditions.’ This statement would be easier to understand if you made it clearer what evidence supports it.

p11267 l27: ’water vapour at the stratospheric entry is controlled at lower altitudes and 5-10K higher temperatures than in NH winter’. This statement might be true, but I am not sure how you are deducing it.

p11268 l15: ”increased” would be better than ”enhanced”

p11268 l24: ”Furthermore relatively humid years seems to be associated with strong East Asian rainfall in summer ...” – if this is a serious conclusion of the paper, on the basis of the model simulations, then more detail needs to be give.

p6 l15-21: You are saying here that your model gives evidence that convective penetration and small-scale phenomena are not needed to dry the stratosphere or to explain the hygropause. But these arguments have been made elsewhere – e.g. recent sequence of papers by Fueglistaler and others in JGR (2004 and 2005) re explaining observed water vapor on basis of large-scale temperatures, SPARC water vapour report re explaining displacement of hygropause relative to cold point tropopause and seasonal variation of this displacement.

p7 l-9: ”Our model results indicate that the former process, i.e. in-situ tropopause cirrus

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formation, contributes most to dehydration." – How exactly have you deduced this? If it is a conclusion of your modelling study then the reader needs to know how it has been arrived at.

p11269 l10: 'The enhanced flux of water into the stratosphere is a consequence of radiative heating by sulfate aerosols in the lower stratosphere, which moderates the efficiency of the dehydration mechanism'. This sounds an authoritative statement – but what is the basis for it and is it directly relevant? First it is a statement about the initial conditions for the water vapour integration – whether or not the model could reproduce this mechanism does not seem to be relevant. Second – are you implying that the aerosol particles have the effect of heating the lower stratosphere – in which case it seems misleading to me to talk about the 'efficiency of the dehydration process', which I associate more with whether the water vapour concentration is actually reduced as a result of reducing the saturation mixing ratio. Third – if you are implying some other effect of the aerosol particels then what is it and is there a supporting reference?

p11271 l8: I think that understanding of whether or not there is a "stratospheric drain" has advanced significantly since Sherwood (2000). See, Hatsushika and Yamazaki (JGR 2003) and Fueglistaler et al (JGR 2004), for example. Your discussion does not really bring out the fact that much of an apparent (but arguably not a real) stratospheric drain may be associated with descent along isentropes. Your identification of "drains" seems to be based on vertical motion in pressure coordinates (presumably taking account of latitude-longitude structure, which is not shown explicitly in the paper) and therefore the significance of these drains for vertical transport remains unclear.

p11272 l3: 'The model results furthermore suggest that the SAO plays a role in triggering the QBO ...' – this has received only minor attention in the paper and it is surprising to see it mentioned in the conclusions – given that the emphasis of the paper is stratospheric water vapour. My view is that if this is seen as significant then it should be properly considered (and discussed in the context of previous work on the QBO) in a separate paper.

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p11272 l17: Your association of the dry bias and the high bias as resulting from lack of convective penentration to the 75 hPa level is interesting, but seems speculative at present. For example, you claim that temperatures are well captured by the model – why aren't the temperatures adversely affected by the lack of convective penetration?

p11273 l7: 'Our model results suggests that fountains are most distinct ...' – not clear to me on what evidence you are basing this conclusion. As noted earlier – perhaps you have considered the latitude-longitude variation of vertical velocity, which is not shown explicitly. But, again, note that vertical velocity in pressure coordinates is often a poor guide to vertical transport (and perhaps gives the danger of identifying false fountains or false drains – false meaning little importance for vertical transport).

p11273 l29: I don't see much hard evidence for the direct role of monsoon convection in leading to larger water vapour transport into the stratosphere in NH summer. The various recent studies – Bannister et al (2004), Fueglistaler et al (2004), Bonazzola and Haynes (2004) all seem to imply that the pattern of the large-scale circulation allows air to enter the stratosphere without sampling very cold regions – of course the large-scale circulation is strongly affected by the monsoon convection, but the effect seems to be indirect rather than direct.

p11274 l8: Again, the comment about volcanic eruptions seems enigmatic to me – is the effect through increased lower stratospheric temperatures, or by some other mechanism?

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