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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the seasonal dependence of tropical lower-stratospheric temperature trends" *by* Q. Fu et al.

Anonymous Referee #2

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This paper considers trends in lower stratospheric temperatures, as revealed by MSU channel 4, over the period 1979-2007. It is argued that considering the seasonal cycle in the trends aids explanation of the trends and, in particular, indicates that changes in the strength of the Brewer-Dobson circulation (BDC) are playing an important role, with a strengthened BDC in December-February and June-November and a weaker BDC in March-May.

I think that the findings of this paper are interesting and add significantly to our picture of past and likely future changes in the BDC. My criticism of the paper is that in places explanation could be clearer (see details comments below). In particular I found the explanation on pp21826-21827 generally confusing and my general feeling was that the fact you had to use different methods for different parts of the year (different menthods for different parts of the SH seasonal variation and a different method for the NH seasonal variation) to extract the dynamical signal undermines confidence in your results. A clearer description of the method used for the SH would help and for the SH you could alternatively simply accept that the dynamical trend in the Novemer-May period is small.

I also wasn't convinced that the many plots show latitude-longitude structure (e.g. Figure 3 and Figure 7) helped the argument much. If you feel that seeing the detailed latitude-longitude structure is an important part of the arguments you make then that needs to be clearer.

Detailed comments:

p21821 l18: reference should be Rosenlof and Reid (2008). You say that this paper shows 'observational evidence of an accelerated BDC' – it is true that their Figure 13 shows long-term changes in tropical upwelling but over the restricted period 1994-2004, not over the 1980-2007 period for which they show decreasing temperatures (over Koror) and indeed they emphasise the possible role of changing BDC in driving recent tropical temperature changes, rather than over the longer period. You quote Johanson and Fu (2007) as providing evidence of an accelerated BDC – but I can't find any mention of the BDC in that paper. There is a statement tbere about 'an undocumented warming in the winter and spring seasons over half of southern hemisphere high latitudes' and later, a statement 'the large stratospheric warming occurring between June and November warrants further observational study'. But those statements by themselves don't seem evidence for a strengthened BDC. The Hu and Fu (2009) paper seems to be much clearer on strengthened BDC as an explanation for warming trends and I suggested that is a more appropriate reference than Johanson and Fu (2007). I don't know what is in the Lin et al (2009) paper since it is not yet generally available. I'm surprised that you don't refer to Thompson and Solomon (2009, J. Climate) here – they use their figure 7, which shows the part of the temperature trend since 1979 (the same period as you consider), which is not 'congruent' with ozone

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changes, as evidence for a strengthened BDC.

p21824 l25: There are two solid lines in Fig. 1.

p21824 l26: 'derive the contribution' seems too strong – you 'estimate' the contribution using a regression against a gross measure of ozone.

p21825 l17: 'little direct impact'? – changes in the structure of wavenumber-1 might have dynamical implications – e.g. for driving of the BDC.

p21285 l22: It is slightly misleading to say that eddy heat flux is a measure of BDC strength. I'd say that it is a measure of BDC driving, and it then follows that when the heat flux is anomalously large the BDC is anomalously strong. Andrews et al (1987) is a good basic dynamical reference, but others, e.g. Newman et al (2001), seems better references for using heat flux as a proxy for BDC strength.

p21285 l27: The broad motivation here is that the temperature depends on the previous time history of wave forcing, with the dependence reducing in backwards time on the radiative timescale. Newman et al (2001) seems a better reference on this that Hu and Tung (2002).

p21826 l3: I'm not sure that it is justifiable to say 'most of these waves break in the upper stratosphere' – the fact is that there is a significant EP flux convergence in the middle and lower stratosphere. There could be a broad correlation between wave fluxes at different levels without the requirement that most breaking occurs at upper levels.

p21826 l6: I don't see why you emphasise the upper stratosphere – it doesn't seem necessary.

p21826 l12: As noted earlier heat flux is a measure more of the driving of the BDC than of the strength of the BDC. I see the Ueyama and Wallace 'zonal wind index' as an indicator of how disturbed the circulation is – and would not be surprised if there was a reasonable correlation between actual BDC, 'zonal wind index', heat flux

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and high latitude temperatures. I'd imagine that most stratospheric dynamicists would expect this. I'm not really convinced that the Ueyama and Wallace index needs to be emphasised over any other measure of disturbed circulation.

p21826 l21: Does it trouble you that heat flux index and ozone index may be strongly correlated?

p21827 l5: Is the November derived BDC-induced trend near zero because there is little trend in October/November heat fluxes, or because there is little correlation between these heat fluxes and temperature? Why is it 'obviously incorrect' that the BDC-induced trend should be near zero? The invokation of gravity-wave drag to explain this seems to be completely speculative – I'd recommend looking for other possibilities.

p21827 l10: This is confusing – are you doing this for T_4 for all months, or only for November? Later on it sounds as though you might be doing single regression for all months – presumably you mean correlating T_4 with each month with ozone for each month (not just November for the latter).

p21827 l22: I don't see how you can tell from Figure 3 that ozone cooling plays a dominant role in December-May.

p21828 l9: 'NH summer' would be clearer.

p21828 l21: The Yulaeva et al (1994) argument seems to focus on the global mean radiative effect of ozone. Can you provide evidence (perhaps in previously published papers) that this statement would also hold for the radiative effect of high-latitude ozone on high latitude temperatures.

p21828 l24: 'the radiatively induced cooling over SH latitudes' – where has this statement come from? From earlier reasoning in this paper or from results elsewhere?

p21828 l27: 'should not be larger' – why not? It's not that I don't believe what you see but it would be good to have brief justification.

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[How do we disentangle radiative effects of ozone and BDC effects of ozone?]

p21831 l2: Presumably Yulaeva et al (2009) should be Yulaeva et al (1994).

p21831 l13-15: This seasonal variation is not unexpected if you take the simplistic view that DJFMAM is the dynamically active winter-spring season in the NH and JJASON is the corresponding in the SH. (Though that view might have been too simplistic.) You might add here that the implication is that the change in the annual average BDC is caused primarily by changes in the SH (since changes in the NH tend to cancel).

p21831 l25 and following paragraph: This discussion is rather long and speculative. The important point seems to be that the implication of your study is that the strengthening of the BDC is not only manifested in tropical upwelling but also with high-latitude descent (and hence with meridional flow from tropics to high latitudes), requiring changes in extratropical wave forcing. Evidence, e.g from Rosenlof and Reid (2008) that this might result from warmer tropical sea surfaces temperatures and hence enhanced meridional temperature gradients seems weak.

p21832 l29: I don't see an obvious 'violation of the downward contrl principle' – the key question is whether the change in wave forcing is consistent with a change in the BDC that is broad in latitude (even if the relation between wave forcing and circulation is local in latitude).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21819, 2009.

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