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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variability of the stratospheric wave driving during northern winter" *by* A. J. Haklander et al.

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Thank you very much for your comments.

>> When the authors discuss and interpret the results (for example on top of p75 and in the introduction) they seem to be under the impression that EP-flux is a simple measure of the wave activity propagating from below. This is far from correct as wave activity can be generated by instabilities in the stratosphere and influence the EP-flux at the tropopause level. A very good example is the modelling studies of the downward propagation from the stratosphere to the troposphere. Even models without an explicit troposphere can generate large and realistic variability in the stratosphere (see Stratospheric vacillations in a general circulation model, Christiansen, J. Atmos. Sci., 56, 1858-1872, 1999). This point should be discussed in more detail in the paper. <<

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We agree that transient wave activity can be generated in the stratosphere and alter the EP flux at 100 hPa, but this seems to be a minor influence. In the paper by Christiansen (1999), it is indeed found that, for runs without any transient waves below 200 hPa, there exists a small negative transient contribution to the vertical EP flux at 100 hPa. However, the magnitude of this transient component is only about 5% of the magnitude of the stationary component (Table 1; Christiansen, 1999). Therefore, the effect on the 100 hPa EP flux of instabilities that originate from the stratosphere can be assumed to be very small. The model runs in Christiansen's study without a time-dependent troposphere still include stationary wave forcing from below. It is found that this series of experiments agrees well with the notion that the vacillations are due to an instability in the stratosphere, but that a constant wave forcing of finite strength from the troposphere is needed. Nevertheless, we do agree that this point should be discussed in more detail, which will be done in the revised paper.

>> The authors should also explain why the eddy heat flux is a good approximation for the vertical Eliassen Palm flux. Why are the other terms not important? <<

The quasigeostrophic vertical EP-flux component is directly proportional to the poleward eddy heat flux. Based on scaling arguments, the other terms may be neglected for large-scale quasigeostrophic waves, which are the focus of this paper. See, e.g., Eq. 3.5.6 in Andrews et al.: Middle atmosphere dynamics, Academic Press, 489 pp., 1987.

>> The authors give significance levels and error bars for their results. This is admirable but they should discuss the methods behind these calculations and their assumptions. <<

We agree that this should be clarified. This will be done in the revised text.

>> Something is clearly wrong in the bottom of page 72. You should not be able to find several statistical significant 20-years trends in your time-series. This suggests that your method is wrong - perhaps you overestimate the degrees of freedom. <<

ACPD
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It is true that shifting the 20-year time window one year at a time and subsequently calculating trends reduces the number of degrees of freedom below the standard N-2 = 18. We indeed have not taken this into account. We do consider each separate winter as independent: the autocorrelation at a lag of 1 year is only -0.10 for the 1958-2002 period. However, this discussion does not seem to be relevant anymore for the revised version of the paper, since we will confine our analysis to the 1979-2002 period.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 65, 2007.