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Interactive Comment

Interactive comment on "Interannual variability and long term changes in planetary wave activity in the middle atmosphere observed by lidar" *by* A. Hauchecorne et al.

Anonymous Referee #3

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The article 'Interannual variability and long term changes in planetary wave activity in the middle atmosphere observed by lidar' by A. Hauchecorne, P. Keckhut, and M.L. Chanin presents a long time series (1981-2000) of Rayleigh lidar temperature measurements in the middle and upper atmosphere at a mid-latitude station (Haute-Provence Observatory, France). In the context of detecting climate change in the middle atmosphere, continuous monitoring is of high scientific relevance, and the OHP data are a valuable contribution to a global network of long-term measurements for trend analyses. From this viewpoint the manuscript is worth to be published. The scientific analysis and interpretation of the data could however be more substantiated.

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Interactive Discussion

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In section 2.2 the authors derive the energy spectrum of the temperature disturbances and separate it according to the phase of the QBO. They find a difference at 40 km with a broad frequency peak (18-20 days) for QBO West and two sharper peaks (18 days and 13 days) for QBO East. When looking at Figure 2, I come to the opposite conclusion.

Neither in the Introduction nor in Section 2.2 the authors explain, why a correlation of planetary wave activity with the phase of the QBO is expected, and why in particular the 12 day wave activity is anticorrelated with the QBO. It would be helpful for the readers to include some of the basic theory on the interaction between planetary waves and the zonal mean circulation together with the relevant references (e.g., Holton and Tan, 1980, 1982), earlier than in the Discussion.

Section 2.3 (Trend in planetary wave activity), consisting of 5 sentences, is not acceptable as a discussion of the delicate topic of trends in the northern winter stratosphere. In Figure 6 the authors show trends in planetary wave amplitude between 1982 and 2000 at 40 km. They find a decrease in autumn and early winter (Oct-Jan) and an increase in Mar and April. These results should be discussed in more depth in view of other publications that find changes in northern hemisphere (total and transient) planetary wave activity of opposite sign. E.g. Randel et al. (2002) find an increase in planetary wave activity in November and December. Coy et al. (1997) and Waugh et al. (1999) find a decrease in northern hemisphere eddy heat flux at 100 hPa in February and March for a comparable time period. Langematz and Kunze (2006) find an increase in the eddy heat flux at 100 hPa in December. If the differences arise because of the fact that trends in transient wave amplitudes over a single station at 40 km do not have to be identical to trends in zonal mean total heat flux at the tropopause, this should be elaborated.

When deriving trends it is essential to consider the impact of the length of the time-series. I am wondering why the authors did not extend their time series to the recent winters which were characterized by enhanced dynamical variability. Adding Arctic win-

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ters since 2000 would modify the trends significantly. If measurements from OHP are available, their inclusion in the trend analysis would yield more reliable trend estimates.

Finally, could the authors please comment on how reliable a derived trend of 0.1 K/year is, given the instrumental uncertainties of the measurements?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11299, 2006.