

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.

A. Hauchecorne et al.

Received and published: 10 February 2007

Referee comment: The scientific analysis and interpretation of the data could however be more substantiated. 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.

Author answer: We apologize but the QBO East and QBO West have been inverted in Figure 2.

Referee comment: 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.

Author answer: We thank the referee for its good remark. In the new version of the paper we shall add some explanation of the basic theory on the interaction between Planetary waves (PWs) and the zonal wind.

Referee comment: The scientific analysis and interpretation of the data could however be more substantiated. 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.

Author answer: We apologize but the QBO East and QBO West have been inverted in Figure 2.

Referee comment: 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.

Author answer: We thank the referee for its good remark. In the new version of the paper we shall add some explanation of the basic theory on the interaction between Planetary waves (PWs) and the zonal wind.

Referee comment: Section 2.3 (Trend in planetary wave activity), consisting of 5 sen-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tences, 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.

With a fixed lidar it is only possible to sample the travelling component of planetary waves (PWs) and global data are needed to study the stationary component. This may be a reason why the trend detected from lidar data differs from other studies taking into account the total PW energy. We intend refocus the paper on the characteristics on travelling waves and to remove the section on trends which may be questionable. Lidar data can provide a unique source of information concerning the characteristics of travelling PWs in an altitude range (30 to 70 km) not well covered by other methods (satellites, meteorological analyses), especially for the mesosphere and it is worth to concentrate the paper on these aspects.

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 winters 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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

is, given the instrumental uncertainties of the measurements?

Author answer: Data have been limited to 2001 because for some recent winters the data time coverage was not sufficient to estimate correctly the PW characteristics. As we plan to refocus the paper on PW characteristics, we don't consider that adding new years will change significantly the conclusions. 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.

With a fixed lidar it is only possible to sample the travelling component of planetary waves (PWs) and global data are needed to study the stationary component. This may be a reason why the trend detected from lidar data differs from other studies taking into account the total PW energy. We intend refocus the paper on the characteristics on travelling waves and to remove the section on trends which may be questionable. Lidar data can provide a unique source of information concerning the characteristics of travelling PWs in an altitude range (30 to 70 km) not well covered by other methods (satellites, meteorological analyses), especially for the mesosphere and it is worth to concentrate the paper on these aspects.

When deriving trends it is essential to consider the impact of the length of the time-

[Interactive
Comment](#)

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 winters 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?

Author answer: Data have been limited to 2001 because for some recent winters the data time coverage was not sufficient to estimate correctly the PW characteristics. As we plan to refocus the paper on PW characteristics, we don't consider that adding new years will change significantly the conclusions.

[Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11299, 2006.](#)

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)