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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Correlations of mesospheric winds with subtle motion of the Arctic polar vortex" *by* Y. Bhattacharya and A. J. Gerrard

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We would like to thank Anonymous Referee 2 for reviewing our paper.

Using two winters of data, the authors find a correlation between winds at these levels in the case of displaced polar vortex, and no correlation when polar vortex is not displaced. The authors suggest that further understanding of this relationship will be helpful for forecasting purposes.

A crucial aspect of this paper is that unique data – neutral winds over from the Arctic

mesopause measured almost daily over approximately 45 days, over two winters, is being analyzed at a unique time – when the polar vortex is cold and stable, without stratospheric warming events. Stable polar vortices have not been available since the mid 1990s, and the difference in the degree of distortion of the polar vortex between the two seasons has provided a valuable and rare opportunity for such analysis.
The last paragraph of section 1, on page 16552 has been modified to accommodate the above mentioned explanation explicitly.

My main concern is that the relationship between different altitudes is not clearly demonstrated. Yes, there is a correlation between mesospheric and stratospheric winds - but correlation between two datasets does not prove a cause-and-effect relationship. The authors need to demonstrate this relationship more clearly - if not with their own data, then at least through discussion of other datasets.

We agree with the referee that any direct cause-and-effect is not implied by the analysis of correlation. Therefore, we have changed the text (Section 5, first two paragraphs) as follows:

Our analysis suggests a possible (but not proven) downward propagating, synoptic, dynamical signatures in wind fields traced from the MLT region in the Arctic polar cap to the upper stratosphere. This should be interpreted as a possible indication of only downward signal propagation, since causal effect cannot be determined. To determine a causal effect would require further, detailed investigation of observations from similar situations – simultaneous measurements and underlying cause of mesospheric wind behaviour as well as causal relations between relative movements of a relatively undisrupted stratospheric polar vortex off the pole and movement back to "normal" – centered at the pole. Before a causal relationship can be deduced, the forcing(s) experienced by the polar vortex would have to be separated into those that can be *traced in the troposphere – and the residual movements that could be attributed to alternative forcing mechanisms. The observed change in the mesospheric winds, correlated with planetary wave activity associated with the Aleutian High, appears to sequentially progress towards a displacement of the polar vortex which is later identified in stratospheric analyses.*

Again, we are thankful to referee 2 for allowing us to better address this particular issue.

The figures are not visual and do not support authors' arguments as well as they should. For example, Fig.1 causes more questions than answers. The raw (20-mins) mesospheric winds show very large fluctuations for both years, 1995 and 1996, which could represent planetary wave activity or varying tidal amplitudes - or both. In addition, it is not clear if data is always taken at 85+/-5km (i.e. averaged over this altitude range) or the altitude of data varies within these limits.

We disagree with the referee that the figures are not visual – similar figures and in roughly similar contexts have been published earlier by the author and other authors in AGU publications [see, for example, Cho et al. (2004), MLT cooling during stratospheric warming events, Geophys. Res. Lett., 31, L10104, doi:10.1029/2004GL019552; and Bhattacharya et al. (2005) Variability of atmospheric winds and waves in the Arctic polar mesosphere during a stratospheric sudden warming, Geophys. Res. Lett., 31, L23101, doi:10.1029/2004GL020389.].

Observations of wind speeds (and other parameters such as volume emission rates or temperatures) using airglow are always assumed to be averaged over the nominal altitude of the emission layers – this is mentioned in the manuscript (page 16552, line 27). We have modified the text to reflect the effect of averaging over airglow altitude range:

This paper presents observations of the Meinel OH(6,2) P1(3) airglow emission line center at 843.0 nm, at a nominal altitude of 85±5 km. Measured wind velocities should be considered to be an average over this altitude range.

On data analysis: as mesospheric data has periodic 12-h gaps, variations in either amplitude or phase of tides could result in large variations in raw winds and daily averaged winds. The statement on page 16554, lines 2-4, that "we assume that any tidal bias can be treated as random geophysical noise in the Erwin data series" is not sufficient to address this issue.

The referee has not suggested an alternative to what would be sufficient to address this issue – and we suspect that there is no other alternative. Yes, the overwhelming majority of neutral wind measurements from any ground based station on earth does have periodic gaps of several hours each, and this is the case with our data as well. Our dataset represents, for this time period (calm Arctic vortex) one of a very few long continuous (with gaps) dataset for neutral mesopause winds at these Arctic latitudes, and it is worthy of the analysis. The presence of gaps is one of the reasons that it is important to show raw data. The alternative of not performing any kind of analysis unless there are datasets without gaps is not acceptable to the authors, especially for neutral winds, because there are so few sources, and that too not publicly available. We shall be happy if the referee can direct us to a better quality source of mesopause region neutral winds without daily gaps that is unaffected by aurora, and it would be a privilege to address this in a future publication.

Why are the raw winds shown and how do they support the statements about the relationship between the MLT and stratosphere? To me, they just distract from *the main topic of the paper. In addition, there is not enough information about the stratospheric winds. Does the figure show stratospheric winds directly below the mesospheric observations or is it zonally averaged wind? Is it combined in the same manner as mesospheric winds, i.e., both zonal and meridional components, or just one component?*

We think it is important to show raw data for the readers to get an idea of the range of fluctuations and appreciate the nature of periodic gaps in the data. In an earlier paper [Bhattacharya et al. (2005), Variability of atmospheric winds and waves in the Arctic polar mesosphere during a stratospheric sudden warming, Geophys. Res. Lett., 31, L23101, doi:10.1029/2004GL020389] illustrating very similar data (same instrument and location - from year 2000), the referees insisted on showing the raw data in a separate figure.

Figure 1 of the manuscript does show daily assimilated stratospheric winds from the grid point closest to Resolute Bay. Stratospheric assimilations were only available once a day (noon). It is important to note that we have used averaged daily winds to compare against daily average stratospheric winds. It is also very important to note that the terms “zonal” and “meridional” are not meaningful in the often distorted or otherwise displaced polar vortex co-ordinates. This is explicitly mentioned in the manuscript (page 16553, lines 27-29):

It should be noted that traditional notion of “zonal” and “meridional” components of winds breaks down in the context of rapid polar vortex movements and distorted dynamical co-ordinates. Therefore, wind magnitudes have been considered for this analysis.

On Fig.2: as I understand, the figure is supposed to show the difference in polar vortex

location between 1995 and 1996 and illustrate distortion and displacement of polar vortex in 1996. Again, this is not very clear on these spatial and temporal scales. There are periods in 1995 when polar vortex is distorted (Nov 25, 1995), and there are periods in 1996 when polar vortex is directly below the Erwin instrument. It is not easy for the reader to grasp the sequence of events and compare it to the winds in Fig.1. I am not sure how to improve it though - maybe, just show regular and disrupted polar vortex on a larger scale, and provide time series of stratospheric parameters at selected location, with clear indication when exactly the polar vortex was not below the Erwin. This part needs more explanation/discussion.

As the referee states in comments, it is difficult to suggest an alternative figure / projection in stratospheric analyses. The size of the plot could be increased, and this is easy to do in a primarily on-line publication such as ACP. We have explained the sequence of plots in the text. The core idea is synoptic scale evolution of the polar vortex in the light of mesopause wind observations, which are available only at a single location. This is done in Figure 1, where the stratospheric winds at a single location is being compared, and is followed in Figure 2, where the winds at a single location (Resolute Bay) are seen in the general context of the polar vortex shape and location.

The discussion part needs to be extended to demonstrate relationship between the winds. For example, the authors point to the period after Dec 7, 1996, saying "the mesospheric winds start to decrease and 2-days later (09 December 1996) the stratospheric winds start to increase". This is not enough to claim the relationship - the data in figure 1 have periods when both stratospheric and mesospheric winds increase or decrease. It is possible that time series with 2-day shift will demonstrate the point better, but current figures and discussion are not too convincing.

Most of the section on analysis is spent on analysis of the correlation between the synoptic scale polar vortex, and a causal relationship is not claimed for the correlations. We agree with the referee on the broader point (that a direct cause cannot be ascertained that there is a downward forcing), and therefore this reflects in the modifications to the paragraph in Section 5 described earlier.

Minor points:
p.16553, line 1 - Accuracy of measurements is typically 1 m/s for a single observation. What integration time was used to achieve this accuracy?

A single observation is defined as a line of sight wind, assuming that the reference phase is known. This requires an integration time of 1-2 seconds. A full cycle of measurements – zonal and meridional winds (as stated in the paper) took about 20 minutes in the current instrument configuration. We agree with the referee that this needs to be included in the text, and have modified paragraph 1, page 16553 of the manuscript as follows:

Observations from the ERWIN Michelson Interferometer are of high quality. Accuracy of measurements is typically 1 m/s for a single observation, with an integration time of 1-2 seconds A full set of measurements – zonal and meriodional winds – takes about 20 minutes in the instrument configuration used for this paper. An excellent correlation was found in simultaneously measured wind velocities (corresponding to the 97 km green-line airglow emission) from ERWIN and a ground-based Fabry-Perot instrument (CLIO), also located at Resolute Bay (Fisher et al., 2000). Uninterrupted measurements are possible during darkness (polar winter) without significant cloud cover or bright moonlight within the instrument field-of-view.