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Interactive Comment

Interactive comment on "Change in turbopause altitude at 52 and 70 N" *by* C. M. Hall et al.

C. M. Hall et al.

chris.hall@uit.no

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The very specific criticisms of the manuscript are much appreciated, even though the referee recommends rejection. Our views on the respective comments are as follows.

Major comments:

1. That the manuscript looks like and updated version of an earlier paper. This is true, but the data have been extended by over half a solar cycle. Contrary to the referee's view, we insist there are new aspects to this study: a. The temperature analysis of meteor radar data for higher latitude published elsewhere had been applied to the meteor radar dataset for the same latitude as the 70°N medium frequency radar used to estimate turbulence. The turbulence calculation has been performed for a number of hypothetical temperature trends encompassing the measured temperature trend.

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Interactive Discussion

Discussion Paper

We establish any trend in temperature is incapable of altering the turbulence temporal evolutions over the observation periods. b. Furthermore, we have noted that atomic oxygen concentration trends (Oliver et al., 2014) can be explained by turbopause altitude variation; we invert this calculation to predict an atomic oxygen concentration change that would be induced by our determined turbopause change.

2. The group retardation of the 2.78MHz radio wave. It is true that ionization up to the echo altitude slows down the radio wave rendering the echo altitude to be "apparent". As in earlier studies, here we use data from the entire day, and over a $\sim$ 14-year period. The auroral activity capable of ionising the ionospheric D-region is intermittent and does not last for more than perhaps 10-20% of the day, and for that matter not even daily. Statistically, we do not believe (although admittedly do not prove) that the echo heights are not significantly incorrect. Apart from the fact that riometer data are not available for the period in question (observations were discontinued at the Ramfjordmoen site long ago), the paper by Hall earlier demonstrated a technique that could be employed during observational campaigns designed specifically for auroral conditions. We admit, however, that given a suitable riometer time-series it would be theoretically possible to attempt to correct altitudes for the group delay.

3. We acknowledge the recommendation to use SABER data for temperature and pressure. As the reviewer states, this would involve assimilation of SABER data appropriate to the turbulence observation and then a major re-analysis. This is beyond our resources in the framework of the current study, and if the referee still feels that this is a ground for rejecting the paper, we accept and respect the decision. A follow-up would then be turbulence determination using SABER (or similar) data and trends in turbulence intensity at various altitudes including those low enough for us to be able to ignore the group-delay aspect.

4. We accept the criticism that the time-series is too short to investigate the influence of the solar cycle. Indeed if we are to assert there is a long-term trend (note that we use the term "change" rather than "trend" in the title), several solar cycles are needed.

On the other hand, the change in turbopause height we present is just that – whether the change or lack of it is affected by the solar flux or is anthropogenically forced is not a subject of the paper. Regarding the electron density, apart from the group delay aspect, fluctuations in neutral air are simply made visible to the radar by the presence of weak ionisation, i.e. the electrons are a passive tracer.

Minor comments:

1. (Turbulent) energy dissipation can indeed be explained better in a revision of the manuscript.

2. Explanations of determination of turbulence and turbopause altitude can indeed be moved (for example) to the "data and method" section, and more information on the two prime instruments should of course have been included.

3. The observations are not zonally representative and this must be stated more clearly.

4. Figure-quality can be improved in any subsequent version of the manuscript.

5. References can be added as the referee recommends. While Wayne Hocking does warn of potential hazards in determining turbulence (group delay, "beam-broadening" etc.), the method most discussed in his papers is determination of spectral width using VHF radar and fundamentally different from the method used here. Nonetheless, references to his work can still be usefully included by us.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20287, 2015.

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