

## ***Interactive comment on “An improved HIRS upper tropospheric water vapor dataset and its correlations with major climate indices” by L. Shi et al.***

### **Anonymous Referee #1**

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This paper presents a new dataset of the HIRS channel 12 brightness temperatures where the cloud clearing has been modified. The modified cloud clearing is meant to keep only those pixels where there are no clouds or clouds are not high enough to contaminate the channel 12 record. The dataset is compared for two months with the AMSU upper tropospheric water dataset. They then use the channel 12 dataset to explore correlations between channel 12 brightness and various indices.

I think the paper needs major revisions before it is ready for publication but is an important piece of work. I have the following concerns:

1) The cloud clearing values seem rather arbitrary. I suspect this is the nature of cloud

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clearing but I think the authors need to do two things: a) Motivate the choice of values. How does it relate to the channel 12 weighting function? b) Explore the sensitivity of their results to changes in the critical values.

2) I was worried that the comparison between AMSU 183 GHz and HIRS channel 12 had such a large bias. The authors did not seem concerned by this. I think the paper needs some quantitative calculations to show that this bias is not due to cloud contamination. Perhaps this could be done using ERA-Interim for the two months + RTTOV (or other radiation transfer codes) to compute the expected difference for clear sky cases.

3) When using the entire 33 year record some issues of data inhomogeneity need discussion and possible analysis either to correct for or to demonstrate that the issues are small enough not to be important.

a) There is a big shift in the spectral response function for channel 12 when going from HIRS/2 to HIRS/3. I suspect this has been corrected for (as Shi has done so in the past) but some discussion is needed.

b) All the NOAA platforms have orbits that drift and so will alias in the climatological diurnal cycle into the record unless corrected for. Lindfors et al, 2011 and McKenzie et al, 2012 explored this. Shi is a co-author on Lindfors et al so should be aware of the issue.

4) The correlation analysis also needs to consider statistical significance and make clear what the null hypothesis for the analysis is. I suspect for most of the indices a white noise model is adequate.

5) I think the title is somewhat misleading. Channel 12 is not a pure water vapour channel but is also sensitive to temperatures. The analysis presented in this paper only considers brightness temperatures. The authors should come up with a better title that reflects this.

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6) I think the section on indices would benefit by referring to review papers on the various indices and also by suggesting mechanisms by which the correlations happen. For example for the NAO I'd think that the primary mechanism was transport of water vapour into the upper-troposphere by mid-latitude storms. I think this section could be shorted by dropping the AO (or NAO) as they are much the same. I think the authors would help the paper by more intelligent use of seasons – I don't think for every index we need to see JJA, DJF and the annual mean. For the NAO are they using the same index for JJA and DJF? I think that Folland et al, 2008 suggested a different index for the summer NAO though given the weakness of the summer NAO I'm not convinced that the discussion adds much to the paper.

7) There are places where the language is hard to follow and a bit staccato. I suggest the authors try to improve the language.

8) The figures are small and hard to read.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 33411, 2012.

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