Atmos. Chem. Phys. Discuss., 15, C10252–C10254, 2015
www.atmos-chem-phys-discuss.net/15/C10252/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observation of a tidal effect on the Polar Jet Stream" *by* C. H. Best and R. Madrigali

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Received and published: 7 December 2015

The authors of this very brief paper describes a mechanism by which lunar tides in the atmosphere could affect the Arctic Oscillation. They then present time series observations to support their conclusion that the tide is in fact affecting the AO. The authors acknowledge that the magnitude of the forcing is exceedingly small but claim that the evidence is convincing for a causal relationship.

The paper is fundamentally flawed. I recommend that it not be accepted. Below I list the primary problems.

1. It was not possible for me to tell exactly what their strong evidence is. The conclusion seems to be based on graphs showing time series of the AO index and the derived tidal forcing. The reader is supposed to see a relationship between the two variables on the graphs shown for six NH winters and for shorter periods during a few other

years. This relationship was not apparent to me. The text also mentions that correlation coefficients were computed and are "small". Are these what are shown in Figure 3, with maximum magnitudes of less than .015? This is indeed small and I would consider it indistinguishable from zero. It would not be statistically significant during winter because the atmosphere is highly variable. How can a correlation coefficient of almost zero be considered to be "statistically overwhelming" as stated on p. 22705?

2. A further note on statistical significance. It is not clear how the authors determined probability but their conclusion that the odds are less than 10e-6 for the observed relationship to occur by chance based on 46 events does not conform to modern statistical methods. The natural variability of the atmosphere must also be accounted for. Something like the Student's t-test, at the minimum, should be applied.

3. It is very risky to make the jump from correlation to causation without some evidence from basic physics to support the attribution. The authors appear to acknowledge that their hypothesized forcing is very weak. Instead, they rely on the timeseries plots to convince the reader. However, it cannot to emphasized too strongly: correlation is not causation.

4. The large amount of low frequency variability in the extratropical atmosphere has power at a wide range of periods, overlapping with 28 days. This makes it difficult to identify any but the most robust external forcing with such a time frame. In addition, the 28-day periodicity in Northern Hemisphere winter has been identified as an atmospheric normal mode (also called a free Rossby wave); see the review paper by Madden (2007). There is both theoretical and observational support for the existence of this wave. Through periodic interference with the quasi-stationary planetary wave, this normal mode can cause variations in the large-scale dynamical fields with a 28-day time scale.

Reference: Madden, R. a. (2007) Large-scale, free Rossby waves in the atmosphereâĂŤan update, Tellus, 59A, 571-590.