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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The diurnal and semidiurnal tides over Ascension Island (8 S, 14 W) and their interaction with the stratospheric QBO: studies with meteor radar, eCMAM and WACCM" *by* R. N. Davis et al.

Anonymous Referee #1

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The manuscript presents 10 years of tidal wind analyses in the MLT region from the meteor radar at Ascension Island. Diurnal and semidiurnal amplitudes and phases are compared to CMAM and WACCM simulations and the possible influence of the stratospheric QBO is discussed. The latter part is interesting because the tidal/QBO connection is not yet fully understood. Overall, it is a good, well-written paper and suitable for ACP.

However, I have three general comments that need to be addressed before I can recommend publication.

1. What is the purpose of the comparison with the models? The introduction correctly states that reproducing a single station tidal amplitude/phase is very difficult for a model, because of the superposition all the different tidal components that play a role. For low latitudes, at least DW1, DW2 and DE3 will show up in the diurnal amplitudes observed by the radar. These components come from radiative forcing, wave-wave interaction and latent heat release. A GCM would need to correctly reproduce every detail of them to get the net amplitude and phase right. And this is obviously not the case due to the large amp/phase difference presented. However, this model challenge is well known and has been studied before (e.g., in the Ward et al. 2010 paper) and I don't understand what new information comes out of the presented comparisons. The authors need to significantly scale back on the model part or omit it altogether. The whole discussion and comparison of vertical wavelengths is meaningless if the models do not capture the involved tidal components correctly. Furthermore, the models are not used for scientific interpretation, e.g., in terms of the QBO connection.

2. The QBO discussion needs to be strengthened. For example, the ENSO phase was very similar to the QBO over the analyzed time period and as such latent heat forcing variations might be miss-interpreted as QBO-induced tidal variations. I also believe that the correlation analysis suggesting a stratospheric QBO effect is somewhat questionable. The mesospheric QBO is out-of-phase with the stratospheric one and as such more likely to induce frequency Doppler shifts that lead to enhanced/reduced dissipation consistent with observed tidal amplitudes. See for example Ekanayake et al., 1997. It is, in my opinion, not clear how the correlation analysis can actually shed light on the open question where a QBO effect happens: stratosphere or mesosphere and how it is transmitted into the tides.

3. The only "error bars" shown are standard deviations of monthly averages. It is mandatory to show and discuss the true errors from instrument noise, data gaps etc. The whole discussion of inter-annual variability is otherwise speculative.

Specific comments

1. Page 4790, around lines 15-20. This was discussed in detail by Ward et al. [2010].

2. Page 4791, line 19. What causes the white circular gaps in Figure 1c?

3. Page 4793, line 15. Give reference for WACCM runs in “specified dynamics” mode.

4. Page 4794, line 14. How was the composite-year monthly-mean computed? Averaging all 1-hr data into monthly mean bins or from the least-squares tidal fits?

5. Page 4796, line 2. There are no a,b labels in Figure 6. The same comment applies to almost all other figures.

6. Page 4796, line 21. Figure 7 (and all the other line plot figures) are extremely hard to read (and thus the corresponding discussion is hard to follow) as they show six different lines with lots of variability in very small panels. These plots need to be separated into zonal and meridional components. Keeping zonal and meridional curves in the same panels is only acceptable if the model curves are omitted (see my general comment 1).

7. Page 4799, line 18. This is not an “uncertainty” because the “error bars” are standard deviations only.

8. Page 4806, line 24. I don’t understand this classical tidal theory argument solely based on the migrating tide. Clearly, DW2 and DE3 will also play a major role. See for example the TIDI analyses by Wu et al. and Oberheide et al.

9. Page 4810, line 14. Please give a reference for the “well known” WACCM problem.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 4785, 2013.