Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-154-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Forcing Mechanisms of the Terdiurnal Tide" *by* Friederike Lilienthal et al.

Anonymous Referee #2

Received and published: 25 July 2018

The paper "Forcing Mechanisms of the Terdiurnal Tide" by Lilienthal et al. investigates the nature of the mechanisms which cause terdiurnal tides using a on-linear mechanistic global model, the Middle and Upper Atmosphere Model (MUAM). The approach involves analysing various terms in an ensemble of model runs which use monthly zonal mean temperature fields from ERA-interim reanalysis data to nudge the lower 30 km of the model. The relative role of various terms are diagnosed by doing ensemble runs with specific wave number 3 forcing terms removed. The conceptual framework of the paper is sound but there are a number of issues with the paper that need to be addressed before it can be published in ACP. These include an examination of the solar heating in the model, inclusion of the diurnal and terdiurnal tidal amplitudes which are thought to cause the non-linear interactions and a discussion of the relative amplitudes of these components at the heights at which non-linear effects become important, some discussion of whether the forcing terms result in propagating components

Printer-friendly version

Discussion paper

or local forcing (i.e. trapped components) and clarification of the details of the model runs used in this paper. These points are discussed in more detail along with some other less significant points below.

The solar forcing in the this model (discussion on Pg 7, lines 204 to 206) has a similar form (Figure 2) to that published in Smith and Oortland (2001) and Du and Ward (2010) but is over an order of magnitude smaller. The UV heating parameterizations in these models are the same as the one used in the current paper. Furthermore, in the discussion of the terdiurnal amplitudes (pg 8-9, lines 257 to 285) the model amplitudes are generally significantly smaller than those observed. They are also significantly smaller than the amplitudes reported by Du and Ward (2010) in their model run. These smaller amplitudes are consistent with the difference in Solar heating between this model and earlier modelling papers noted above. The authors should investigate the source of these differences and confirm that the heating in the model is correct. Issues with the heating will affect the later sections of the paper so I have not commented on Sections 3.2 and 4.

Some discussion of the amplitudes of the diurnal and semi-diurnal tides in this model should be included. In particular, if non-linear interactions are indicated at a particular height it is important to know what the form of the parent waves is (i.e. the diurnal and semidiurnal tides). It is also possible that although there may be indications of non-linear forcing that this forcing might not result in a propagating tide. In addition to the relationship between periods noted in this paper, there are also relationships between the horizontal and vertical wavelength that should be met for the forcing to result in a propagating component (see Teitelbaum and Vial, JGR, 1991). Consideration of these aspects of the forcing should also be discussed.

The authors provide useful and interesting comparisons between their results and observations. Previous modelling studies are mentioned but there is no explicit comparison between the results of this paper and the previous modelling studies (i.e. latitude/height amplitudes and phases, annual variations, forcing mechanisms). This com-

parison is needed and would provide the reader with a better idea of how this paper advances the field.

The description of the model runs and the exact nature of the numerical experiment can be improved. The authors note that the lower 30 km of MUAM runs are nudged with monthly mean ERA-Interim reanalyses of zonal mean temperature. An ensemble mean is calculated with the individual runs being driven by results from 2000 to 2010. Runs consists of a 120 day spin-up with not tidal forcing followed by a 90 day run of which the last 30 days are used to investigate the terdiurnal tidal signatures. Apart from Figure 6, only results from January and April are presented. Were there any background winds imposed during the runs? Are the runs used for each month, perpetual runs for that month (i.e. no temporal evolution with the Solar elevation angle remaining constant)? Are the results for the last 30 days stable results (i.e. the model run had achieved some sort of equilibrium) and how was this determined? The authors state that there was no planetary wave forcing at the lower boundary. Was any forcing at the lower boundary included? How was the boundary between the free running part of the model and the nudged portion handled (step function or gradual change). When the tidal forcing started to be included, how was this made compatible with the nudging? Is the tidal forcing turned on gradually? How was the nudging undertaken for the 11 ensemble experiments (page 3) daily zonal means). Is the turning on of the tidal heating a step function?

Additional Comments:

Pg 4 line 113: What is meant by "... remove it in each model time step.". Does this mean that the wave number 3 signature is removed from the model run at each time step at each point in the model?

Pg 4, line 116: Include a reference to justify your decision to ignore non-migrating tides based on the source of nonmigrating tides?

Pg 4, line 123: Please comment on whether ozone is included above 50 km. Is there a

step function in the vertical profile of ozone?

Pg 4, line 128: This should be recombination of O and O3.

Pg 6, line 168: Please explain in more detail how the two gravity wave parameterizations are linked. In most parameterizations the upward flux and dissipation is accounted for. How is the linkage between the upward fluxes between these two parameterizations made? It seems from later results (Figures 2 and 3 and discussion at line 217) that the largest effects are associated with the YiËĞgit parameterization and must have come upward through the stratosphere and mesosphere. This should be discussed.

Pg 6, line 173: Please make explicit that it is the wave number 3 component that is removed in the CTRL run.

Pg 6, line 175: To clarify your procedure, explicitly note that only solstice (January) and equinox (April) conditions are being analysed in this paper apart from Figure 6.

Pg 6, lines 179 - 181: Although it is mentioned earlier in the paper, restating that the ensembles consist of runs using data from each of the years 2000-2010 would make the paper clearer.

Pg 7, lines 215 - 218: The diurnal and semi-diurnal tidal amplitudes are small in the troposphere and the background does not include any planetary waves and is nudged to zonal mean temperatures. The tendency terms here likely do not have much to do with the terdiurnal tide and unless the authors have good reason to include discussion of the dynamics of this region, I suggest it be omitted.

Pg 7, lines 227 - 231: Tidal amplitudes are compared at different heights (90 km for the observations, 100 km for the model). Is there a reason for this?

Pg 8, lines 251 - 256: In Figure 5, the phases are plotted from the ground to 130 km but the amplitude of the terdiurnal tide is only significant about $\sim$80 km as indicated in Figure 4. The vertical wavelength determinations appear to be associated with this

whole height range. They should only be associated with heights where the amplitude is significant. The wavelength determination should be discussed in more detail.

I have also attached a commented pdf of the paper with some suggestions for improvements to the writing.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-154/acp-2018-154-RC2-supplement.pdf

---

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-154, 2018.