

Interactive comment on “Net influence of an internally-generated QBO on modelled stratospheric climate and chemistry” by M. M. Hurwitz et al.

Anonymous Referee #1

Received and published: 17 July 2013

Review of "Net influence of an internally generated QBO on modeled stratospheric climate and chemistry" by Hurwitz et al.

The authors examine the impact of including an internally generated QBO in GEOSCCM. They do this by comparing the zonal mean climatologies of two multi-year time slice simulations, one with a QBO, the other without. They focus primarily on the annual mean. The paper is mainly descriptive, with little or no discussion of the reasons for the differences between the two simulations.

There are a number of important issues that must be addressed before I would recom-

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mend publication of the paper. These are discussed in my "major comments", with my first point being the most important.

Recommendation: major revisions

Major points:

1) The paper is really only a description of the impacts of an internally generated QBO in a CCM, with no attempt made to get at the root causes of those impacts. The authors state that the QBO "warms the Arctic lower stratosphere", "cools the Antarctic stratosphere" (p.13501), and "slows the overturning circulation" (p.13502), but provide no explanation why. Since the stratospheric overturning circulation is wave driven, it would seem only natural to examine the changes in resolved wave drag to try to understand the causes, but this was not done. The authors therefore need to investigate the causal effects of these changes in the residual circulation, using for example downward control calculations to help interpret the differences between the two simulations. They need to look at the seasonality and spatial pattern of the wave drag changes that are responsible for the changes.

The author's second point in the conclusions that "the net impact of the QBO depends on both the baseline zonal winds fields and the relative changes in the tropical winds" also requires further explanation and diagnostic analysis. Why in the simulation with the QBO do the time mean zonal winds in the tropical lower stratosphere increase? What is the role of the parameterized GWs? For a paper dealing with a QBO generated by increasing the tropical GWD I found it strange that not a single figure showing the parameterized GWD was shown.

2) It also seems strange that there is no discussion of the non-orographic GWD parameterization, given its central role in this study. It needs to be described in Section 2, along with the appropriate references.

3) It is unclear to me what the authors mean by "net impact" of the QBO. At first I

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thought is was the time mean impact, but then realized that it is more general since they also discuss changes in variability. It needs to be made clear in the introduction what is meant by "net impact".

4) The figures are still way too small, and as a result are very difficult to read. The authors need to provide larger figures (e.g., with fewer panels or without long labels like in Fig. 2 that take up a lot of space) in their revised version.

Specific Points:

abstract, l.6: "quasi-realistic tropical GWD" – The word quasi-realistic must be deleted since there are no observations of GWD to confirm this statement.

abstract, l.7-8: "the modeled QBO improves the simulation of tropical zonal winds" seems a bit nonsensical since it is obvious that the tropical zonal winds in a model with a QBO will agree better with observations. It may be better to reword this sentence something like "The annual mean zonal winds in the tropics in the simulation with a QBO agree better with obs" if that is what is intended.

abstract, l.15-17: "Extra-tropical differences ... reflect a bias toward the westerly phase of the QBO" is unclear. A bias in what? And how do the differences in the two simulations reflect a bias?

P.13496, l.24: Reed et al 1961 did not discuss the driving mechanisms of the QBO, as implied by this reference. I therefore suggest adding references to Lindzen and Holton and Holton and Lindzen.

P.13497, l.28-29: "QBO amplitude were negligible, typical of CCMs that lack a nudged or internal QBO signal". This is so obvious it doesn't need stating.

P.13498, l.21-22: "As non-oro GWs often accompany precipitation" is a misleading statement. What about non-oro GWS in the mesosphere? It would be clearer I think to say "The generation of non-oro GWS often accompany precip"

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P.13499, l.7 & 9: "Q" and "N" – I don't much like these single-letter identifiers of the two simulations – they make for strange sounding sentences in my opinion, e.g., "zonal winds is well simulated in Q." How about "Run Q" and "Run N" or append "simulation" before the letters?

P.13499, l.8: "quasi-realistic peak in tropical GWD" – since there are no observations of GWD, and only limited observations of GWs in the tropics, there is no justification for saying that the peak in GWD in the tropics is realistic or even quasi-realistic. Modellers often increase the parameterized non-oro GW momentum flux source because they believe there are more GWS there.

P.13501, l.8: I presume the "observations" shown here are from the MERRA reanalysis. It would therefore be better to say "agreement with the MERRA reanalysis".

P.13501, l.18-19: The sentence "the QBO warms the tropical stratosphere and Arctic lower stratosphere by ~1K but cools the Antarctic stratosphere by ~1K" either needs to be reworded or an explanation given as to how the high latitude temperature changes are caused by the QBO. See my first major point.

P.13501-3: It is impossible to read the labels on the color bars in Figs 2-4, so I cannot tell if a difference is positive or negative. See my major point about the figures.

P.13504, l.17: add "winter" before "polar stratosphere" since the Holton Tan relation is to do with the NH winter polar stratosphere. As written, a positive correlation between zonal winds in the tropics and polar regions could be in summer.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13495, 2013.

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