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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

# *Interactive comment on* "Examining the stratospheric response to the solar cycle in a coupled WACCM simulation with an internally generated QBO" *by* A. C. Kren et al.

**Anonymous Referee #2**

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General comments: This paper studies the northern stratospheric polar vortex response to the QBO modulated 11-year solar cycle signal. In doing so, they made use of the WACCM-4, a fully coupled model with interactive ocean, chemistry, greenhouse gases, volcanic eruptions, and an internally generated QBO. By analyzing their extended transient run for the period of 1850-2005, the authors have found that the QBO modulated 11-yr solar cycle signature was detected only for a 40yr periods and then opposite signed correlation appeared in other periods.

This paper is well structured and easy to follow. The tables and figures are mostly of publishable quality. The results are generally interesting and worth reporting. How-



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ever, my key concern is that their modelling results are not strong enough to support their conclusion; that is: the observed polar QBO-solar signature might be a chance occurrence.

First of all, it is worth noting that many types of solar forcing may vary over the 11-yr solar cycle and it remains largely unknown which solar forcing underpins the observed Labitzke-van Loon relationship. The conclusion, therefore, cannot be reached given that solar irradiance was the only solar forcing considered by their model runs. There are other potentially unknown or not well researched solar forcing out there that may also affect the earth's climate. For instance, it is known that energetic particle precipitation (EPP) also affects the polar stratosphere by generating NOx, which may cause ozone depletion [e.g. Randal et al., 2005]. Until we have a fair amount of confidence in that no other type of solar forcing can contribute to the solar-QBO effect, the Labitzke-van Loon relationship remains as an open research question.

Secondly, the authors failed to discuss the large uncertainties associated with the solar irradiance spectral applied to the model. More specially, what are the potential uncertainties or the error bar associated with modelling the solar spectral irradiance (SSI) distribution. How much confidence do we have in terms of determining the temporal variability of the SSI based on the TSI [Ermolli et al., 2013]?

Thirdly, the polar vortex is sensitive to many factors in both the troposphere and stratosphere. A combination of concurrent forcings cannot be added linearly because the vortex strength can be dominated by one factor and becomes less sensitive to others [Camp and Tung, 2007; Calvo et al., 2009]. During one period, the stratospheric polar vortex can be sensitive only to the dominant forcing and insensitive to other forcings. During another period, when two cyclic forcings reinforce each other, the signal of the both forcings might be artificially amplified. The switch signed correlation can be observed in real world too if other low frequency, cyclic forcings have an opposite effect to that of the solar-QBO effect on either the wave drag or the meanflow. Like the real atmosphere, this model included all sorts of forcings. It becomes very hard to sepa-



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rate the responses to different forcings. When those forcings interact with each other nonlinearly, the solar-QBO signal may be reinforce in one period and/or contaminate in another period. Also, the ocean can behave like a massive low frequency modulator/amplifier. Because the "variation/contamination" may behave differently in the real atmosphere and in the models, the "wax and wane" behaviour seen in the model cannot prove that the observed solar-QBO signal was by chance.

Another general comment of mine that the Analysis Methods section is too short in its current form while the Results section contains a lot of detailed descriptions of the analysis method used for each figure. To help the readers better focus on the results, I suggest the authors to: 1) include the method how did they test the statistical significant levels in the Analysis Methods section; 2) move the description of wavelet analysis from page 25163 to Analysis Methods section; 3) move the description of ensemble of four WACCM3.5-CCMVal-2 runs from pages 25165-25166 to Analysis Methods section; 4) move the description of Monte Carlo sampling from pages 25166-25167to Analysis Methods section.

I also entirely agree with reviewer #1 in many ways, especially the authors appear to be overly positive about the model's ability and how realistic the modelled results are in comparison to the observations.

Specific comments: 25162line2, Note that, during NH winter, both HT-effect and Labitzke-van Loon relationship are sensitive to the QBO at 45-50hPa [Garfinkel et al., 2012]. Discuss the possible implication for the disagreement between the modelled results and observational QBO-solar response if the 30hPa QBO is used instead?

25162Line20, ~50yr -> ~150yr.

25162Lines16-19, Removing the linear trends in temperature and other variables cannot remove the GHG effect on winter stratospheric circulation entirely. For instance, Shepherd McLandress [2010] showed that a strengthening of the Brewer-Dobson circulation (BDC) is robustly associated with the increase of greenhouse gases. Can the



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authors state whether or not a strengthening or a weakened BDC affect the solar-QBO signal given the bifurcation response of the stratospheric polar vortex to wave forcing?

25163Line12-13. I cannot follow the sentence “Although the lower stratospheric temperature change is slightly higher than the ozone response, ...”. Which ozone response because no ozone results are shown at ~70hPa? How can you compare the temperature change with ozone response?

25163Line25. “. . . the internally generated QBO period is in excellent agreement with observations.” -> “. . . the AVERAGE period of the internally generated QBO is in excellent agreement with observations.” Comparing to the observations, it appears to me that the modelled period of the eQBO phase is too regular and the period of wQBO at 20hPa is too long, fig. 1.

25165Lines 7-9. “while the WACCM simulation produces differences in the high latitudes as a function of the QBO that are similar in magnitude and timing to the observed HT-variations”. This is very interesting as many CMIP5 model runs tend to produce rather weak or insignificant HT-effect. Also note that the HT-effect was substantially weaken or even revised in the reanalysis during the late winters of 1977-1997 [Lu et al., 2008]. Can the authors be more specific about the” magnitude” and the” timing”?

25166Lines4-10. This is a very long sentence, thus hard to digest all the information at once, consider shortening.

25166Lines17-18, “These results suggest that the solar-QBO interaction found by Labitzke (1987) may have occurred by chance.” The results do suggest that the fully coupled WACCM cannot reproduce the solar-QBO effect. The results do however not support this statement. Please note the points I made above and also Labitzke used the 45hPa QBO not the 30hPa QBO.

25167Lines16-17, “The probability of finding a correlation of -0.3 in QBO east is 5.8% . . .”. This sentence does not prove the correlations are not statistically significant.



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Simply, it is quite possible that “the probability of finding a correlation of 0.5 is 90%”, or “the probability of finding a correlation smaller than -0.3 in QBO east is 95%”.

25167Line20, please explain why the author chose to use 90N. In general, the data quality at the very pole is poor. It is at the boundary of the model anyway.

Table 2. include p-values in bracket for each correlation coefficient.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25157, 2013.