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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.

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First off, we would like to thank the two anonymous reviewers for their careful reading and constructive comments they provided in the discussion phase of the manuscript. We feel that by addressing their concerns in the revised manuscript, this has helped to improve our paper significantly. Thank you.

General comments for Referees #1 and #2:

1. Please note that we have re-defined the criteria for solar maximum and minimum years. This criteria is noted in the below responses and in the revised manuscript.

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Our previous criterion was more or less by eye, and we have made the new definition more robust. As a result, there are now 45 solar maximum years and 42 solar minimum years. This means Figs. 2 and 3 have changed, along with some values in table 1. This also resulted in an improved solar cycle response in the upper stratosphere. However, the overall conclusions and results of our paper remain the same.

2. Per referee #1, we have added observations to compare with our model studies. We use geopotential height data from NCEP-NCAR reanalysis from 1953-2013 (Kalnay et al., 1996). In addition, we use the observed monthly mean equatorial zonal wind dataset at 50 hPa, comprised of radiosonde data from Canton Island (3° S/172 ° W) from January 1953 – August 1967, Gan/Maledives Islands (1° S/73 ° E) from September 1967 – December 1975, and Singapore (1° N/104 ° E) since January 1976 (updated from Naujokat, 1986). This dataset is available at http://www.geo.fu-berlin.de/en/met/ag/strat/produkte/qbo/. We use the observed radiosonde winds as reanalysis data show a much lower QBO magnitude compared to radiosonde observations.

3. Please note that we have redefined our analysis method for doing the Monte Carlo Sampling to better compare with the number of east and west years in the observational record. This means that Fig. 6 and the results have changed but the overall conclusion remains similar. This redefined method is found in the revised manuscript and responses below.

4. We have added new references and added subsections to section 4.

5. We have defined both our two WACCM4 simulations, denoted WACCM4a and WACCM4b, as well as the CCMVal-2 runs. These are described in the model description section of the revised paper.

6. We would like to respond to both reviewers concerns about whether our model is realistic and the fact that it is very difficult to separate out the responses from a combination of external forcings. We disagree that our model is unrealistic. WACCM includes

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all of the dynamical interactions that we are aware of and that can be resolved. There may be additional interactions of an unknown nature or some that have scales too small to be resolved. However, interactions between large-scale dynamics, energetics, and chemistry that occur in the atmosphere should be included in the model and so their effects, alone or in combination, are included to the extent possible. Our model results show that we can reproduce the solar-QBO correlation over selected 40 year periods, as shown in Table 2 and in our CCMVal-2 ensemble. The correlation is only significant in the west phase of WACCM4a from 1850-1890 and is not significant in WACCM4b or any of the CCMVal-2 runs. Secondly, our study aims to understand the long term time-mean of any solar-QBO signal. This time-mean cannot be determined in observations due to the short period of record. As pointed out by referee #2, it is difficult to separate out the responses from other external forcings, such as El Nino, QBO, internal variability, etc.; the observational record suffers from this same difficulty when examining the solar-QBO signal. In our results, we show that over a short period, comparable to the observational record, we do see at times a solar-QBO correlation. However, the period is short to where other forcings may contaminate the response of the polar vortex and make it hard to attribute the response to a strict set of factors, such as the solar cycle and QBO. However, over the long term, when we would assume that other external forcings would have less of an influence, the time-mean shows that the correlation is not robust. In the revised manuscript, we make the point that our results show that the solar-QBO correlation seen in observations may have occurred because of the relatively short data record and the inability to separate the impact of additional external forcings rather than a direct solar-QBO interaction.

Response to Anonymous Referee #1:

1. 25158L7 – The characterization of the simulated QBO as "realistic" seems too superficial and overly positive. As discussed in the manuscript, there are features of the observed QBO, in particular in the lower stratosphere, which are not reproduced realistically.

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a. We have removed this sentence from the abstract and also removed any other mention of the word in the manuscript.

2. 25158L26 – Besides the original publication from Holton and Tan it may also be appropriate to cite Anstey and Shepherd (2013) who provide a useful review of the high latitude influence of the QBO.

a. We have cited Anstey and Shepard (2013) in our discussion of the QBO influence on the high latitudes. We thank the referee for this suggestion to improve the literature review. We have added the following to the revised manuscript: "The waveguide shifts into the northern hemisphere (NH) during QBO east, conducive for greater wave deposition over the northern pole and favoring a polar vortex that is anomalously warm and weak (Holton and Tan, 1980; Thompson et al., 2002, Anstey and Shepherd, 2013)."

3. 25159L23 – "These studies have confirmed…" I think it is a too strong simplification to summarize the content of the referenced studies as a confirmation of QBO and solar cycle influences on the polar vortex. Although most simulations showed some influence, the simulated influences look quite different and mostly do not reproduce the observations.

a. We have updated the revised manuscript with the following: "Although these models have not been able to reproduce all aspects, the studies indicate that there is a circulation response in the polar vortex that depends on both QBO phase and the 11 year solar cycle."

4. 25159L25 – The two following sentences seem to indicate that a solar influence on the QBO is confirmed. To my understanding this is not the case. There are also studies that question this finding.

a. We have changed this sentence and updated the revised manuscript with the following: "In addition, Salby and Callaghan (2000, 2004, 2006) and Pascoe et al. (2005) have provided some evidence from observations that there exists a decadal variation

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of both the QBO period and in the duration of the QBO west phase, suggesting that a solar modulation of the QBO may be present. These observations, however, extend over only four solar cycles. A 150 year model simulation study by McCormack et al. (2007) using a parameterized QBO and UV variations showed a shortened QBO west period by about 3 months at solar maximum compared to solar minimum. This change in the period was shorter than the observed 3-6 month variation found in Salby and Callaghan (2000), but larger than the ∼1 month variation found in an earlier model study by McCormack (2003)."

5. 25160L2 – "variability of 100% near 100 nm)..." At least radiation of 100nm is irrelevant for the stratosphere. It would rather be useful to provide a number for the Hartley band which is also referred to later in the manuscript.

a. By analyzing our solar maximum and minimum years, we determined the solar cycle irradiance variation at 255 nm shows maximum variability on the order of ∼ 3%, consistent with Lean et al. (2005). Therefore, we have edited the above-referenced sentence: "The response of the tropical stratospheric QBO to the solar cycle is thought to be caused by varying UV radiation, which causes increased heating in the upper stratosphere at solar maximum to produce anomalous easterly momentum to shorten the QBO west period (McCormack, 2003; Pascoe et al., 2005; McCormack et al., 2007). UV radiation exhibits maximum variability of ∼3% at 255 nm, the center of the Hartley band (Lean et al., 2005; Gray et al., 2010)."

6. 25160L24 – "This model has been show to agree with observations..." Much too positive and simplified. There are certainly aspects of the model simulation (also mentioned by Marsh et al. (2013) which do not agree with "the observational record".

a. We have changed this sentence to better reflect the findings of Marsh et al. (2013). We have updated the revised manuscript with the following: "In these coupled-ocean simulations, CESM1 (WACCM) reproduces many features seen in observations, such as the increase in the observed surface temperature record since 1850, the develop-

ment of the ozone hole, the frequency and distribution of stratospheric sudden warmings important for stratospheric-tropospheric coupling, and the periodicity of the El Niño Southern Oscillation (ENSO) (Marsh et al., 2013). Some differences are seen compared to observations, for example, a reduced frequency of Atlantic blocking during boreal winter and the minimum in Antarctic sea ice extent."

7. 25161L21 – It's not true, that Baldwin et al. (2001) report longer lasting westerlies. Instead, referring to their Fig. 1 they write about "stronger intensity and longer duration of the easterly phase".

a. We have removed the sentence and edited accordingly with this: "The peak amplitude of the QBO is strongest during the east phase at ∼30 m/s, while westerlies are weaker, between ∼15 and 20 m/s, also consistent with the observed QBO. Two potential shortcomings are noted of the internally generated QBO in both WACCM4 simulations, and are evident in Fig. 1: QBO westerlies are longer lasting than easterlies in the mid to upper stratosphere, a result contrary to that in observations (Baldwin et al., 2001); the QBO does not descend to 100 hPa and, in particular, the west phase does not reach 50 hPa."

8. 25162 Section 2.2 – Information is missing on the criterion to define solar max and min years. Fig. 2 should use identical ranges for the y-axis. Then it would become clearer some max years during weak solar cycles have irradiance very similar to some min years. Does that affect the results?

a. We have added the criterion to define solar max and minimum years. Also, we adjusted Fig. 2 to use the identical ranges for the y-axis. In responding on whether some max years during weak solar cycles have irradiance very similar to some min years, this is true. Regarding whether this would alter the results, we examined the results by examining only the high solar max years and low solar min years. The results were slightly different, primarily regarding the upper stratospheric temperature change. Our criterion to define solar max and min years is in the revised manuscript

as follows: "In stratifying solar maximum and minimum years, we focus on the peaks and troughs in the solar cycle. We first calculate a 3 year running mean of the annually averaged 255 nm spectral irradiance. From this time series, we select the 3 years that make up a local maximum and minimum for each cycle in the running mean."

9. Fig. 4 – Information is missing on the pressure level for which the power spectrum is calculated.

a. The power spectrum was calculated at the 30 hPa level. We have added this to the figure description and also edited accordingly: "In addition, to further examine the possible modulation of the QBO period by the solar cycle, we compute a Morlet wavelet power spectrum (Torrence and Compo, 1998) of the equatorial monthly zonal mean zonal winds at 30 hPa in WACCM4a (shown in Fig. 4a)."

10. 25163L27 – "...QBO period varies over 20 months..." I can't identify this from Fig. 4. Is the maximum power interpreted as period? (see also below)

a. The power in the wavelet plot is indicative of the variation in the whole QBO period. The maximum power is indicative of the variability of the QBO over time. The way our sentence was written was probably too vague and we have edited it accordingly as follows: "Fig. 4a clearly shows that the variation in the spectral power of the QBO is present at timescales between 23 and 38 months. For each month, we can determine the QBO period as the period for which the power spectrum shown in Fig. 4a reaches its maximum. Averaging those monthly periods over a year gives the average QBO period for the year. Fig. 4b shows the time series of the QBO period in WACCM4a along with the yearly 255 nm spectral irradiance. Averaged over the entire model simulation, the average QBO period is 27.2 months, again consistent with the observed period (Baldwin et al., 2001). The correlation between the yearly 255 nm spectral irradiance time series and QBO period is -0.24. When dividing the period up into three segments, the correlation is positive (0.12) from 1850-1899, switches sign (-0.41) from 1900-1949, and is -0.03 from 1950-2005. The varying sign of the correlations indicate

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that the QBO period is not modulated directly by the solar cycle. There may be other external forcings or internal variability at play in impacting the QBO period."

11. 25163L28 – Periods where "the QBO varies in lock step (in phase?) with the solar irradiance are difficult to identify from Fig. 4. Maybe it would help to add a curve with the QBO period.

a. We have included the variation in the QBO period overlaid with the yearly spectral irradiance in Fig. 4b. We also overlay the mean QBO period over the entire simulation. The updated figure is included in the revised manuscript. We have also edited the manuscript to include an updated discussion of this figure.

12. 25164L18 – What does "deterministic" mean in this context?

a. By "deterministic", we are referring to the fact that since we are parameterizing inertial gravity waves, the source of the waves is largely pre-determined since WACCM cannot generate these waves internally.

13. 25164L23/24 – "high latitude" or full "stratospheric response"?

a. We are describing and referring to the high latitude stratospheric response rather than the full stratospheric response. We have changed section 4 to be: "High latitude stratospheric response to solar and QBO forcing" b. We have also changed the beginning of this section to read: "In section 2.1, we described the high latitude response to the QBO winds that was similar in magnitude and timing to the observed Holton-Tan variations. In this section, we present the high latitude stratospheric response to both the solar cycle and QBO and compare to past observational and modeling studies."

14. 25165L13 – Labitzke and Kunze (2009) didn't "expect" correlations.

a. We have edited this sentence as follows: "In addition, the signs of the correlations during QBO east (0.24) and west (-0.10) are the opposite of what was found by Labitzke and Kunze (2009)."

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15. 25167L5 – I do not see March very exceptional. Plus, the formulation seems to indicate that the insignificance is the exception. Is that intended?

a. We have addressed this comment in the revised manuscript. The comment was intended to point out the shift in mean correlation, but, because the signal is not significant, we agree that it is clearer to remove that part of the sentence.

16. 25167L16/17 – Labitzke and Kunze (2009) present correlations for February, not DJFM.

a. Instead of reporting the mean probability over DJFM, we report the month which had the highest probability to better represent the potential solar-QBO response, which may not be constant over the winter season and may change sign month to month. Please see the revised manuscript. The highest probability in QBO east was found in March with 34% and in January for QBO west with 6%. Referee #1 and #2, also note that we redid our Monte Carlo sampling results. The new method is described in more detail in section 2.3 of the revised manuscript. To do this, we perform Monte Carlo sampling using both WACCM4 simulations. To perform the random sampling, we focus on the winter season (Dec., Jan., Feb., and Mar.) and combine the two WACCM4 runs to create a total of 249 years of winter data. For each winter month, we group the years into east and west. Then for each east and west group, we randomly select 16 east and 33 west winters to match with the number of east and west years in the 1953- 2013 observational record. We then compute the correlation between the 30 hPa geopotential heights at the North Pole and the 255 nm spectral irradiance. This is performed a million times and repeated for each winter month. The result is a normalized histogram of the correlation (R) for east and west phases for each month. This sampling was changed to better compare with the observational period.

17. 25167L18 – If the correlation of finding the observed correlation in the model is 0.42%, wouldn't that indicate that the model is unrealistic? (Or that the nature managed to produce a 1/200 chance exception?)

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a. First off, the change of getting the observed correlation in QBO east changed to 34% and 6% in QBO west, as a result of changing the Monte Carlo results described above. We disagree with this comment that our model is unrealistic. Our model results show that we can reproduce the solar-QBO correlation over selected periods, but not over the longer term time-mean of 249 years from the Monte Carlo simulations. What our results point out is that when examining over the time-mean, when other external forcings do not have a large impact when compared to shorter 40-50 year periods, the solar-QBO correlation is not present. Please see our general comments at the beginning for further discussion.

18. 25170L7 – If the model is considered realistic, and if the model indicates a very little probability (see above) to produce the observed correlation, wouldn't that rather suggest that other forcings have contaminated the observed period than that they have "occurred by chance"?

a. This is a very valid point that our results may suggest that other forcings may have contaminated the observed period rather than them "occurring by chance". Certainly another potential influence over the observed period is the beating or interaction of the QBO with the annual cycle. We have edited the manuscript in this section to reflect these comments. Another phenomenon that is well known to affect the observed stratosphere is aerosols from volcanic eruptions, particularly the strong El Chichon and Pinatubo events in 1982 and 1991, respectively. The contamination of apparent solar response by volcanic aerosols will have a weaker impact in the long simulations because the superposition of high aerosol and high solar activity that occurred after those two eruptions is not repeated in the longer record.

19. 25170L8 – I think two further potential shortcomings need to be discussed: 1) The upper stratospheric solar effect on temperature seems to be at the very lower range of what is suggested from observations. The real signal could easily be twice as large. If the model underestimates this signal, the dynamical responses to this original radiative effect may be underestimated. 2) The observed correlations are usually analyzed

with respect to the QBO at 45 or 50 hPa. Due to the unrealistic QBO in the lower stratosphere, 30 hPa are chosen, here. It would be very useful to indicate if such a choice would alter the observed correlation. I would suggest adding correlations from reanalysis data for different QBO levels to Table 2.

a. These are two valid points that need to be discussed in the revised manuscript. Regarding the upper stratospheric solar effect on temperature, it is correct that our results are at the lower range of what is suggested from observations. We have added the shortcoming of this result in our discussion section. In the second shortcoming of the QBO, we have added reanalysis data from NCEP-NCAR from 1953-2005 (to compare to our CCMVal simulations) and 1953-2013 and stratospheric wind observations from Canton Island (3° S/172 ° W), Gan/Maledives Islands (1° S/73 ° E, and Singapore (1° N/104 ° E) to show the correlations between the 255 nm spectral irradiance and the geopotential heights over the northern pole as a function of QBO phase at 50 hPa. We defined the QBO in observations at 30 hPa as well, as in our WACCM4 simulations. This did not alter the results significantly. We discuss this in the revised manuscript.

Response to Anonymous Referee #2:

1. General comment #1 – 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 generated NO_x, which may cause ozone depletion [e.g. Randall 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.

a. We have included a discussion of the other solar forcing factors that may be respon-

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sible for the observed Labitzke-van Loon relationship in section 5.

2. General comment #2 – Secondly, the authors failed to discuss the large uncertainties associated with the solar irradiance spectral applied to the model. More specifically, what are the potential uncertainties or the error bar associated with modeling 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]?

a. We have added a discussion about the uncertainties associated with the spectral irradiance variability in the Lean et al. (2005) model. This has been added to section 2.1 in the model description section. Please see the revised manuscript.

3. General comment #3 – 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 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 mean flow. Like the real atmosphere, this model included all sorts of forcings. It becomes very hard to separate the responses to different forcings. When those forcings interact with each other nonlinearly, the solar-QBO signal may be reinforced 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" behavior seen in the model cannot prove that the observed solar-QBO signal was by chance.

a. The WACCM model includes all of the dynamical interactions that we are aware of and that can be resolved. There may be additional interactions of an unknown nature

or some that have scales too small to be resolved. However, interactions between large-scale dynamics, energetics, and chemistry that occur in the atmosphere should be included in the model and so their effects, alone or in combination, are included to the extent possible. We completely agree with this comment that it is very difficult to separate out the responses from a combination of external forcings, such as ENSO, the QBO, solar, volcanic aerosols, etc. Our study aims to understand the long term time-mean of any solar-QBO signal. This time-mean cannot be determined in observations due to the short period of record. As pointed out by the reviewer, it is difficult to separate out the responses from other external forcings; the observational record suffers from this same difficulty when examining the solar-QBO signal. In our results, we show that over a short period, comparable to the observational record, we do see at times a solar-QBO correlation. However, as pointed out by the reviewer, the period is short to where other forcings may contaminate the response of the polar vortex and make it hard to attribute the response to a strict set of factors, such as the solar cycle and QBO. However, over the long term, when we would assume that other external forcings would have less of an influence, the time-mean shows that the signal is not robust. In the revised manuscript, we make the point that our results show that the solar-QBO response may have occurred do to a combination of external forcings that aligned to produce an apparent solar-QBO signal. Please see our general comments at the beginning for further discussion.

4. General comment #4 – Another general comment of mine is 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 they tested 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 25166-25167 to Analysis Methods section.

a. We have moved the analysis descriptions to section 2.3 in the manuscript. We agree that this greatly improves the flow of the manuscript and helps the readers to better focus on the results. The one exception is that we moved the description of the WACCM3.5 CCMVal-2 runs to section 2.1. We also added another section to discuss the reanalysis and observational data in section 2.2. Please see the revised manuscript.

5. General comment #5 – 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 in comparison to the observations.

a. Please see responses to reviewer #1 comments.

6. Specific comment 25162L2 – Note that, during NH winter, both HT-effect and Labitzke-van Loon relationship are sensitive to the QBO at 45-50 hPa [Garfinkel et al., 2012]. Discuss the possible implication for the disagreement between the modelled results and observational QBO-solar response if the 30 hPa QBO is used instead?

a. We have included NCEP-NCAR reanalysis data and stratospheric wind observations from 1953-2013. We defined the QBO at 30 and 50 hPa in the observations to examine whether the various defined levels of the QBO alter the observed QBO-solar correlation. When the QBO is defined at 50 hPa, we see the Labitzke and Kunze (2009) correlation in the polar vortex state, yet the response for the east phase is not significant at all levels. When defining the QBO at 30 hPa, we still see the variation in the polar vortex state from QBO and solar forcing in QBO west. These results suggest that when the QBO is defined at 30 hPa, the result is not changed significantly. These are discussed in the revised manuscript, although the various levels are not included in Table 2. We also noted in the revised manuscript that the observed response from 1953-2013 is not highly significant and the correlations are lower than originally found by Labitzke and Kunze (2009).

7. 25162L20 – ~50 yr > 150 yr.

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a. We have removed the "∼ 50 yr" comment in the manuscript and changed the sentence to better reflect the description of the figure.

8. 25162L16-19 – Removing the linear trends in temperature and other variables cannot remove the GHG effect on winter stratospheric circulation entirely. For instance, Shepard McLandress [2010] showed that a strengthening of the Brewer-Dobson circulation (BDC) is robustly associated with the increase of greenhouse gases. Can the 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?

a. Since we are looking for a cyclic response, we are simplifying the analysis to represent the removal of the noticeable trend which is due to increases in greenhouse gases, variation in ozone depleting substances, and a contribution from the circulation response to the first two.

9. 25163L12-13 – I cannot follow the sentence "Although the lower stratosphere temperature change is slightly higher than the ozone response ..." Which ozone response because no ozone results are shown at ∼70 hPa? How can you compare the temperature change with ozone response?

a. We have removed this sentence from the revised manuscript regarding the comparison of the temperature changes to the ozone response. What we meant to point out was that the upper stratospheric temperature change seen in our WACCM results is consistent with the observed location and that this temperature change in the upper stratosphere is in the same location as where ozone would be observed to change over the solar cycle.

10. 25163L25 – "... the internally generated QBO period is in excellent agreement with observations." And "...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 the wQBO at 20 hPa is too long, fig. 1.

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a. We have edited the manuscript to reflect better what our results show in comparison to observations. Please see the revised manuscript.

11. 25165L7-9 – "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 weaker 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"?

a. We have addressed this comment by discussing in more detail the magnitude and timing of the Holton-Tan variations. Please see the revised manuscript. What our WACCM4 simulations show is that there is a weaker polar vortex in the east QBO and vice versa in west QBO. The magnitude of the temperature and zonal mean zonal wind is on the order of $\sim$ 4 K and $\sim$ 6 m/s. This has also been stated in the revised manuscript in section 2.1. There are some differences compared to reanalysis, which are discussed in the revised manuscript.

12. 25166L4-10 – This is a very long sentence, thus hard to digest all the information at once, consider shortening.

a. We have re-organized this sentence so that it is shorter. Please see the revised manuscript.

13. 25166L17-18 – "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 45 hPa QBO not the 30 hPa QBO.

a. We have removed this sentence from the revised manuscript. Please also note that in response to referee #1, we have included the correlations from reanalysis between

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the 255 nm spectral irradiance and the 30 hPa geopotential heights in February as a function of QBO east and west when the QBO is defined at various levels. The results are discussed in the revised manuscript, but only the results when the QBO is defined at 50 hPa are included in Table 2. When we define the QBO at 50 hPa, as Labitzke and Kunze (2009) do, we do get the solar cycle-QBO correlation. When we define the QBO from observations at 30 hPa, the signal weakens only slightly but remains positive in QBO west. This is in response to the comment from referee #1 comment, namely, that the level of defining the QBO will not significantly impact the results. We also point out that by looking at the observational record (1953-2013), the results are lower than what were previously reported and are not as significant.

14. 25167L16-17 – "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. 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%".

a. We have omitted any reference of significance in the Monte Carlo section. Please see revised manuscript for further discussion. First off, we have changed our definition of probability. We determine the probability now for less than or equal to -0.18 for QBO east and 0.36 for QBO west to compare better to the 1953-2013 observational record. Furthermore, the probabilities that we are stating are not just for -0.18 and 0.36. We integrated the total probability for less than or equal to -0.18 and greater than or equal to 0.36. So, the statement mentioned in the comment is not true. The probabilities have changed due to the Monte Carlo sampling being slightly different. We now see probabilities in the east phase of 34% and 6% in the west phase. However, the overall probability is low for seeing the solar-QBO correlation.

15. 25167L20 – 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.

a. We will respond with several reasons why we chose 90N. First, to be consistent,

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we followed the method of Labitzke and Kunze (2009) and determined the correlations between the spectral irradiance and the geopotential heights as function of QBO phase at the North Pole. This was done to ensure that we were comparing the same thing to observations. The 90N polar location is also a good indicator of what is going on dynamically at the polar cap and is often used for analyses. Secondly, we did not restrict our analysis to just the polar region. As discussed in section 4, we examined the circulation response in the stratosphere by computing the difference in the geopotential heights, temperature, and zonal mean zonal wind between solar maximum and minimum for QBO east and west phases over the whole stratosphere. We did not show these results. But the results were consistent with the results when strictly focusing on 90N, that is, that the observed solar-QBO correlation could not be reproduced.

16. Table 2 – Include p-values in brackets for each correlation coefficient.

a. We have included the two-sided significance level in brackets next to each correlation coefficient, both for the model simulations and the newly added observations.

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