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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.***

**Anonymous Referee #1**

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The authors analyze a set of simulations with the WACCM CCM with respect to the possible interaction of QBO and 11-year solar cycle effects in the northern hemisphere winter stratosphere. This is a long-standing scientific issue as the observed positive correlation of stratospheric north pole temperatures and solar activity under QBO west conditions has, to my knowledge, not been reproduced convincingly in atmospheric models. The question remains open if this is due to shortcomings of the applied models or a chance occurrence in the real atmosphere. Although this manuscript cannot solve the issue, I think it is a very useful addition to the discussion. In a long-term simulation with the state-of-the-art WACCM CCM with an internally generated QBO, the

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observed correlation can be reproduced only in selected 40-year periods while during other periods correlations of opposite sign are simulated. The same inconclusiveness of model simulations is analyzed from an ensemble of simulations with a version of the WACCM model with prescribed QBO. The simulations hence show that even 40-year periods may not be sufficient to unambiguously identify signals of QBO/solar cycle interaction. Overall, the paper is well written and organized, the presentation of the model simulations and their results is clear and interesting. I recommend publication after consideration of the following minor issues:

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.

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.

25259L23 “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.

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.

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.

25160L24 “This model has been shown to agree with observations . . .” Much too posi-

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tive 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”.

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”.

25162Section2.2 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?

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

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)

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.

25164L18 What does “deterministic” mean in this context?

25164L23/24 “high latitude” or full “stratospheric response”?

25165L13 Labitzke and Kunze (2009) didn't “expect” correlations.

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

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

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

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produce a 1/200 chance exception?)

L25170L7 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"?

L25170L8 I think two further potential shortcomings needs 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.

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