

Interactive comment on “Multidecadal Variations of the Effects of the Quasi-Biennial Oscillation on the Climate System” by Stefan Brönnimann et al.

Stefan Brönnimann et al.

stefan.broennimann@giub.unibe.ch

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Reply to the reviewers' comments

Reviewer 1: Many thanks for the helpful review and suggestions. We have improved the manuscript along the lines indicated. Please find below the replies.

Comment: Page 7, line 27: Discuss clearly the method 'heteroscedastic t-testing to assess statistical significance'. Also page 8, line 1: Explain standardized differences. As those are the main methods to capture signal QBO east to that from QBO west, describe those methods in details and give the formula.

Reply: We have added more detail on the method, including a formula and references for the testing.

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Comment: Page 10, line 20: "predominantly negative, as expected from the Holton-Tan (H-T) effect." Also in page 9, line 7: there are some remarks about H-T effect. It is better to mention some discussions in this context relating to change in the behaviour of H-T effect during period 1977-1997. Lu et al. (2008) discussed that H-T effect weakened and even reversed around that time. Though it was statistically significant during solar min and 1959-1976 and 1998-2006 for extended winter (Nov-April).

Reply: We have rephrased the text and have added the reference.

Comment: Page 7, line 29: It is good as the different period is used '1908-1957 and 1958-2012'. Considering longer period, Roy (2014) also showed that some tropospheric signatures were different before and after 1958. I liked the idea of Table 1. Give little discussions with a possible explanation for the change in the northern hemisphere polar vortex and Berlin surface air temperature during that periods. One explanation could be that there was a change in mean state of tropospheric circulation during 1958 (Vecchi and Soden (2007)). Change in tropospheric circulation can directly impact Berlin surface temperature. Also, the circulation change can modulate upward propagating planetary wave activity and subsequently can influence Polar Vortex. Page 1, line 29: In the abstract it is mentioned that 'In the model simulations, likewise, both links tend to appear alternately, suggesting a more systematic modulation.' Discuss possible mechanism in the text, in line with the earlier paragraph, to make that argument strong.

Reply: We have added the references and also some text explaining the possible dependence on the background. We have no results to offer here, however. We tested several measures of the base state (e.g. circulation indices), but did not find agreement between models and observations. Moreover, given the time window used, the signals in the observations must be enormous to be significant. Therefore, although we found for instance a very strong correlation between the non-stationarity of the signal and the AMO in the observations, it is not significant in the observations alone and the same is not found in the model. It therefore remains a hypothesis. We added a little bit of text,

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but no results.

Comment: It is hard to distinguish the colours in f, g and h.

Reply: Done

Minor comments. All minor comments were incorporated into the manuscript.

Reviewer 2:

Thank you very much for the review and the suggestion. We have improved the manuscript along the lines indicated. Specifically, we have improved significance testing (and its description), as outlined below and in the replies to the other reviewer. The use of data is now illustrated in a schematic, and the historical background is shortened. We have tried to straighten the argumentation at times.

Comment 1: Sec. 2: Although detailed information on data used in QBO reconstruction and tropospheric climate is described, it is very difficult to follow which one goes to which period and which exact period is used for the study. A table or schematics showing types of observations and period used in reconstruction will be certainly helpful to readers.

Reply: We have added a schematic figure that illustrates the data sets used, time periods, and analysis windows.

Comment 2: Model: Indicate the vertical resolution between 300hPa - 50 hPa, which would be important for the QBO to influence the tropical convection.

Reply: The vertical resolution around the tropopause is indicated.

Comment 3: The composite is done for QBO phases. The phases are defined by the sign of the wind without the information on the amplitude. Has the amplitude been considered? For instance, sampling only when QBO index exceeds certain threshold may reduce discrepancy between the sub-periods and between the datasets.

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Reply: We require the winds to be above 5 m/s, otherwise QBO phase is classified as neutral. In this sense, the amplitude of the QBO is included to a small extent. This is made clear in the revised manuscript.

Comment 4: It is puzzling that the paragraph starts that the impact of QBO on the troposphere is weak, but the main context of the paragraph supports the tropospheric signal of the QBO.

Reply: We have rephrased the beginning of the paragraph (and in the abstract).

Comment 5: Figure 2 must be for 1979 and onward, and one of the purposes of the plot is to show the pattern of the H-T effect. But the results from other datasets often do not support the H-T mechanism. To better visualize why this is the case, I recommend to examine the patterns in other datasets as well, such as REC+NNR, 20CRv2c, and the model simulations. This can tell us about the following: Z100 does not tell us about tropical response, which should be consistent with ERA-interim by construction. Extratropical Z100 response of REC+NNR for 1957-2014 is consistent with ERA-interim and the H-T effect. Some model simulations also support this picture. However, it is not the case for 20CRv2c.

Reply: Yes, the analysis is since 1979, and this is now clearly stated in the revised manuscript. It is also true that 20CRv2c behaves differently, and this is also mentioned and a column is added to the Table for 20CRv2c for the same time period. We have also prepared a figure where we show the same for 20CRv2c and ERA-20C for the exact same period. A sentence is added to the text.

Comment 6: Please indicate pressure levels in Fig. 2. Absence of this hinders comparison with Table 1. Statistical significance will be very useful. Reply: We added the pressure levels as well as statistical significance. (We also added the same to Fig. 5)

Comment 7: Table 1: Please define REC+NNR. Also add period 1979-2014 and ERA-interim result; this period is shown in Figs. 2 and 5. Lastly, it is difficult to picture what

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is going on based on the mean pictures of the two sub-periods. Temporal variation would be certainly useful to understand each data. For example, running correlations between QBO index and Z100 may be able to tell us about the discrepancy between data. At least all the data are expected to be consistent for the latest decades, say after 1979 and onward. One may be able to trace back where the discrepancy is started.

Reply: We added ERA-Interim results. We also added an additional supplementary figure on the moving window composites between the QBO and Z100 for all data sets (for consistency with the rest of the paper, we did not use correlations). Results show that in recent years, differences between the data sets are small, while they increase further back in time.

Comment 8: Figure 3: Another row using REC+NNR can be a useful reference as REC+NNR better compares with the simulations based on Table 1. Reply: We performed the same analysis also for REC-NNR and prepared a supplementary figure. Results are consistent with Table 1. Some text is added.

Comment 9: Model simulations in Fig. 4 provide a nice visualization that the impact of QBO is weak. However, some metrics can be useful to summarize the result. For example, what are the correlations among models? Also, the authors probably have tested different windows for the moving average. What is the reason of choosing 30 years? It should be tested to indicate robustness of their result to the choice of the window.

Reply: Concerning the correlation among models, the indices are nearly uncorrelated as correlation can only come from the forcings or the QBO. We added a sentence on that. Concerning the window length, a period of 30 years was chosen as this represents the typical time period of data availability in many studies. However, as the effects could be aliased by variability modes of a similar periodicity, we also tested other window lengths (20 to 50 years) and found the same results. A sentence on that is added (there is no need to show a figure).

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Comment 10: Figure 5: Statistical significance test especially for the vertical motion is required.

Reply: Significance is added.

Comment 11: Figure 6: Fig. S10 shows just one row. Besides it appears there is almost no significant areas, which is not clear in the text. I feel that Figure 6 is misleading as if there is strong QBO signal.

Reply: Yes, almost nothing is significant, and we added a sentence to the text on that. The fact that this relation is weak and unstable becomes even more clear now that we add another column to Table 1, which show significantly negative values for the PWC. This is pointed out in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-502, 2016.

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