Reviewer #2: Comments on

"**The Influence of solar Variability and the Quasi-Biennial Oscillation on Sea Level Pressure**"

By I. Roy and J. D. Haigh

Despite the title ("sea-level pressure"), the paper is a loose collection of three different parts, most of it on the stratosphere, and on temperature. Only the last part is on sea-level pressure. I will review each part separately.

First Part: "We investigate an apparent inconsistency between two published results concerning the temperature of the winter polar stratosphere and its dependence on the state of the Sun and the phase of the QBO. We find that the differences can be explained by the use of the authors of different pressure levels to define the phase of the QBO" (first paragraph of the Abstract).

The "inconsistency" alluded to concerns the paper of Labitzke and van Loon (1992) (LvL92) (and a more important earlier paper in 1988, LvL88, which the authors did not reference)and that of Camp and Tung (2007) (CT07). **There is actuallyno inconsistency** between these two results. The authors apparently misinterpreted LvL88's Figure 2, which was almost exactly reproduced by CT07 in their Figure 1. Both Figures used 30-hPa North Polar temperature and 45-hPa tropical QBO index to partition the phase of the QBO into easterly (eQBO) and westerly(wQBO) phase. This is plotted below.

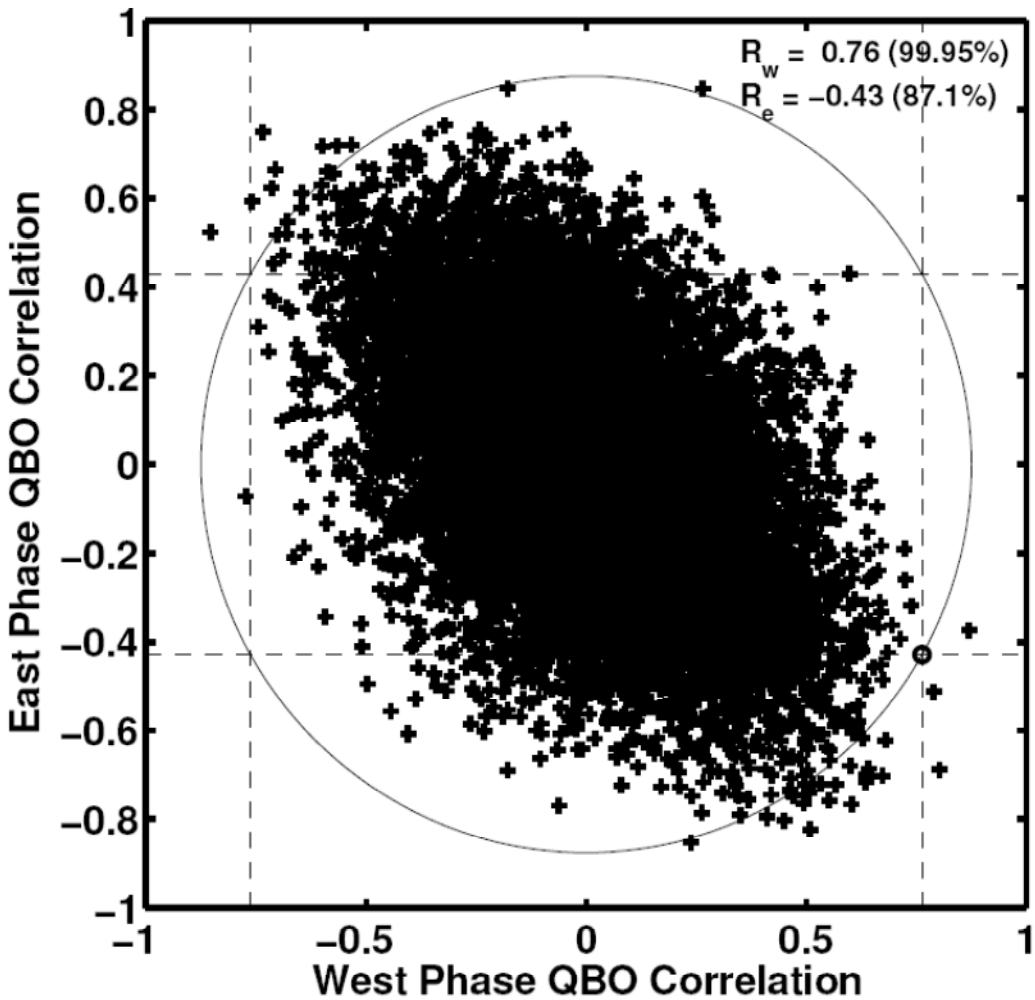

Figure 1: Reconstruction of LvL88 Figure 2. Paired correlations between the mean 30 hPa North Pole Temperature and the 10.7 cm Solar Flux when partitioned by the phase of the 45 hPa tropical QBO index. Mean values for Jan.-Feb. from 1956 to 1978 used. Statistical significances for the observed correlations determined by calculating the paired correlations from partitioning 10000 first-order autoregressive surrogate time series ($p_1 = -0.34$).

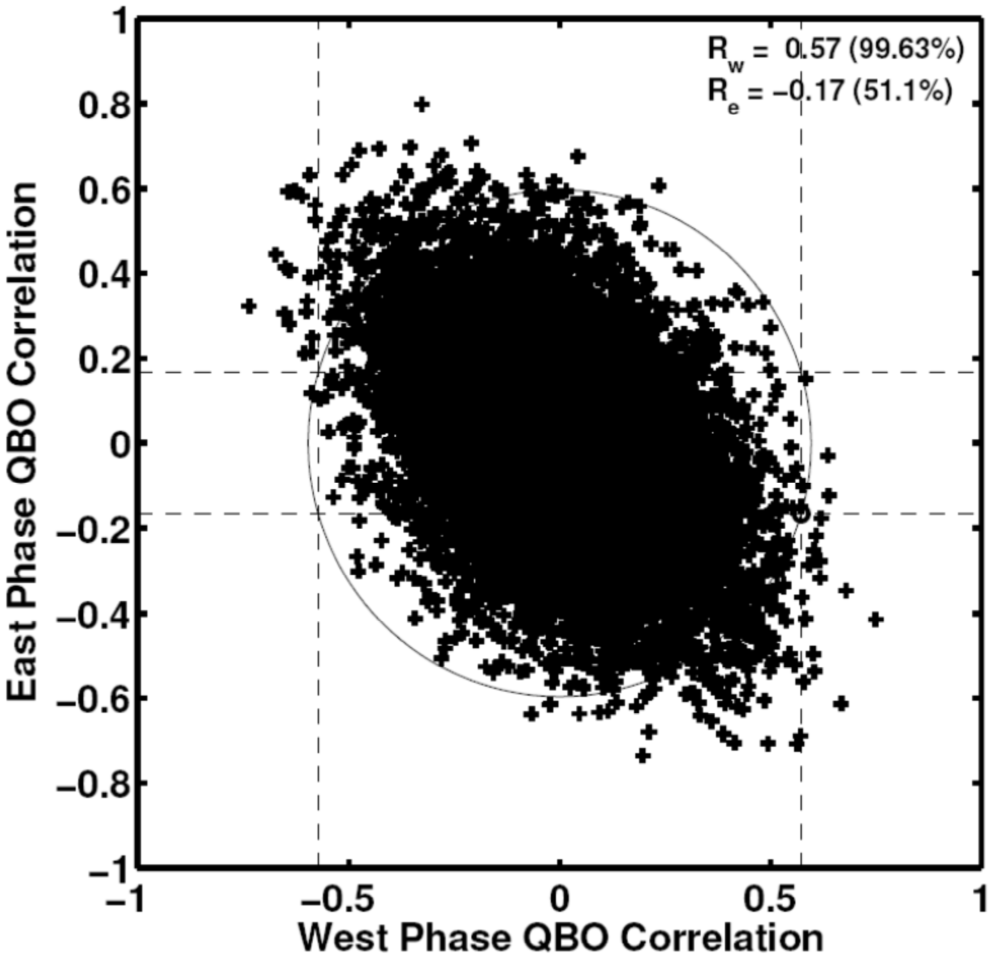

Figure 2: As Figure 1 for data from 1956 to 2001; ($p_1 = -0.33$.)

In the quiet phase of the QBO, the wQBO phase, the influence of the solar cycle is positive and unambiguous. That is, solar max warms the polar stratosphere. The correlation R_w=0.76 is positive, and unlikely to be produced by random chance (at 99.95% confidence level). In the perturbed phase of the QBO, the eQBO phase, the influence of the solar cycle is more ambiguous. The correlation coefficient is R_e= -0.43 and not statistically significant at 95% or even 90% confidence level. When more years were added in CT07's Figure 2, $R_e$becomes even smaller in magnitude and highly insignificant. **When a result does not pass the statistical test** (i.e. the null hypothesis that the correlation of this magnitude can be caused by random chance cannot be rejected), **there is no point of talking about its sign.** But the sign of R_e is what the present authors concentrated on, by varying the level used to define the phase of the QBO. **Their results on the reversal of heating in the perturbed phase (eQBO), similar to that of LvL88 and LvL92, are all statistically insignificant.**

There is a history of different authors trying to get different results on the correlation of polar temperature with the solar flux in different phases of the QBO by varying the pressure level used to define the phase of the QBO.Labitzke (1987) first used 50-hPa equatorial wind to define the QBO while analyzing the 30-hPa polar temperature and found R_w=0.78 at 99.9% confidence level, but R_e= -0.32 and correctly concluded that the latter negative correlation, which would have implied a cooling of the polar stratosphere in solar max, is statistically insignificant. One year later, LvL88 used 45-hPa to define the QBO and obtained a slightly more negative correlation in the perturbed phase: R_e= -0.45. The conclusion should still be the same, that there is no statistically significant evidence for the reversal of solar heating during eQBO. LvL88 however misinterpreted their own Monte-Carlo test and incorrectly concluded that this negative correlation is statistically significant. The mistake in interpretation arises when they used the radius of the circle instead of the rectangle in Figure 1 above, as explained by CT07. This misinterpretation is carried forward to their later papers.

The main result of the present authors on varying the pressure levels in defining the QBO is presented in their Figure 2.

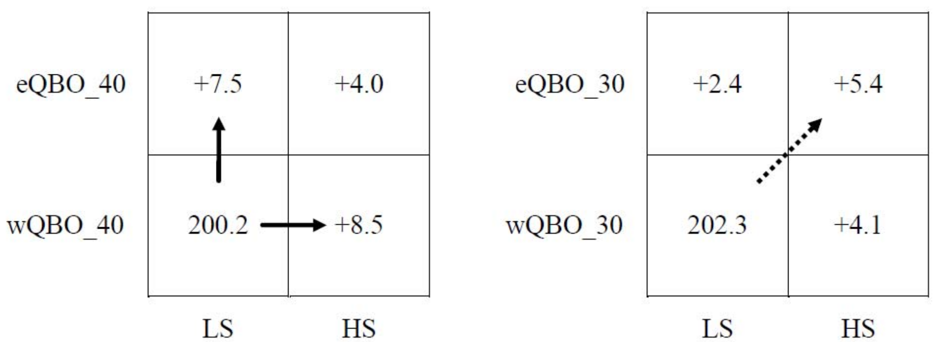

Figure 2 Average JF temperature at 30hPa over the North Pole as a function of the phase of the QBO and state of the Sun. The QBO phase is defined by the sign of the zonal wind over the equator at (in the left hand panel) 40hPa as used by LvL92 and (in the right hand panel) 30hPa as used by CT07. Separation of solar activity into low/high by F10.7 value of less than/greater than 155 units. The lower left box gives the mean temperature (K) for the LS/wQBO combination and the other 3 boxes give differences in temperature (K) from that state. The number of datapoints in each box ranges between 6 and 16 and the standard error on the mean ranges between 1.3 and 2.6K; a solid[dotted] arrow indicates a difference which is statistically significant at the 99[90]% level.

The left panel used 40-hPa tropical wind to define the QBO, as in LvL92, and the right panel used 30-hPa QBO wind, as in CT07. These results, obtained using a different methodology,have even less statistically significant arrows between the four quadrants compared to Figure 9 of CT07.

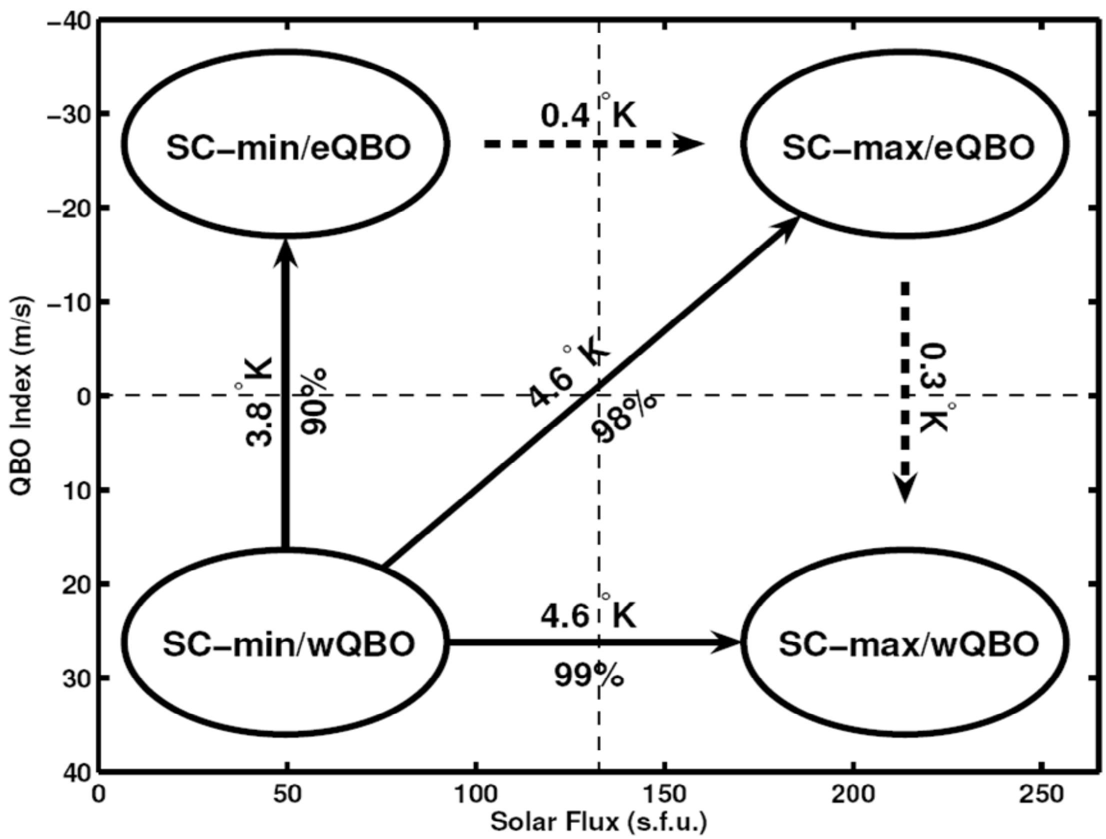

Figure 9: Summary schematic: The state of the SC-min/wQBO is the least-perturbed state. Solid arrows indicate the mean warming of the pole for the perturbed states relative to this state. Confidence levels for the associated LDA's are also shown. Dashed arrows indicate the small difference found between perturbed states, which are not statistically significant. All results are for the Feb.-Mar. average of 10-50 hPa layer mean temperature.

The present authors then drew some very misleading conclusions based on the change between quadrants which they themselves deem to be statistically insignificant. CT07 found that the transition between the top two quadrants is statistically insignificant, and the same insignificant transition is also found by the present authors. The difference of the top two quadrants represents the increase or decrease in polar temperature during the perturbed (eQBO) phase. The present authors' Figure 2 shows a 3.5 K cooling going from solar min to solar max in the left panel, while their right panel shows a 3.0 K warming. **Both warming and cooling are not statistically significant**, as not even a dashed arrow was shown. On this basis they drew the misleading conclusion that "we conclude that the main source of discrepancy between the results of LvL92 and CT07, and the reason for their different conclusions, is their use of different pressure levels to define the phase of the QBO". There is no different conclusion by these prior papers. The consistent conclusion should be, as stated in CT07: *Solar max should warm the polar stratosphere during the quiet phase (wQBO). There is no statistically significant warming* *or cooling associated with the solar cycle during the perturbed phase (eQBO).* The present result is not inconsistent with this conclusion, but adds nothing new to it.

Second Part: The second part of the work consists of Section 4. It is a very different from the first part. It analyzes the zonal mean temperature in NCEP data using the method of multiple linear regression, as in Haigh (2003). The results are “similar to those found previously by Haigh (2003) and Frame and Gray (2010)”. So nothing is new here. What is new is the “second approach”, which uses as the regressor the product of the solar flux and QBO index. This nonlinear regression is unorthodox and more detail and justification should have been given. There is no reference cited, and no description of what is done. Is this a simple regression or multiple regression? Are there other regressors? I assume that this is a simple regression with no other regressors. If this is the case, I would expect that the residue to contain very large deterministic signals, which would affect the noise model adopted. I simply cannot judge if this result is correct without reading more detail. The authors do not appear to pay much attention to the results either. There is one line conclusion saying “Thus using the compound index suggests a weak relationship between the polar lower stratosphere and mid latitude troposphere in the northern hemisphere and a stronger one between the extra-tropical lower stratosphere and lower troposphere high latitude temperatures in the southern hemisphere”. The “weak” result on the northern hemisphere is not reconciled with the first part of this paper.

Third Part: This third part is contained in a short section, Section 5. This is the only part of the paper that deals with sea-level pressure, which is the only quantity mentioned in the title. This is very similar to a recent paper of Roy and Haigh (2010). The part that is new is the addition of a regressor that is the product of the solar and QBO indices. My comment on this unorthodox method is the same as given above for Section 4. It should be justified and explained more. Furthermore, it appears that the multiple regression here uses both this product and QBO and solar flux individually as regressors. This will cause a problem with multi-colinearity of the regressors. That is, QBO and QBO*SOLAR are linearly dependent.

In summary, it appears that this paper consists of three independent parts. The first part is a misunderstanding. The second and third parts are quite similar to what the authors have previously published. The new content concerns the use of a product of two indices as a regressor, but not enough discussion is given for me to judge if such a “nonlinear” regression is justifiable. The statistically significant result in the longer data record is mainly in the southern hemisphere, where the data quality is suspect.