

***Interactive comment on “A QBO-signal in mesospheric water vapor measurements at ALOMAR (69.29° N, 16.03° E) and in model calculations by LIMA over a solar cycle” by G. R. Sonnemann et al.***

**Anonymous Referee #3**

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This paper reports on a 2-year periodicity in observed mesospheric water vapor at high northern latitudes, which, by comparisons to model results, the authors attribute to the QBO in the tropical stratosphere. I am entirely unconvinced by their results and the interpretations they draw from them. The paper is also very poorly written, contains many sentences that make little sense, as well as many statements that are unsubstantiated. For these reasons I am compelled to reject this paper. Below are my reasons why.

1. I do not believe the observed water vapor spectra in Figure 3: The authors have 11

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years of data containing at least two large gaps. The authors provide no information whatsoever about the level of uncertainty in these spectra, and discuss each spectral peak (including, of course, the 2-year peak) as if it were real. On page 886 (lines 20–22), for example, when discussing the instrument they state that the “uppermost panel [80 km] is the most uncertain one”, yet show results at this height. The authors also do not discuss how they filled the data gaps, or discuss how those gaps impact on the spectra. The bottom line is that with so very few years, I cannot see how the very weak peak at 2 years is statistically significant. The entire paper rests on these spectra being meaningful, and the authors have not shown this.

The model spectra that are shown in Figs 3 (bottom), 4 and 5 also don't have statistical uncertainties associated with them. For instance, I am skeptical of the 2-year peak in the model vertical wind spectra since it is a very noisy field especially in the mesosphere.

2. I am completely unconvinced by the model results and the physical picture the authors' draw from them. They are arguing because there is a 2-yr peak in simulated mesospheric water vapor and because the model has a QBO in the tropical lower stratosphere that the tropical QBO is responsible. (They should have used the model to diagnose the causal mechanism since after all that is what models are for!) That logic is flawed since there may be other possible explanations that are unrelated to the QBO. I can think of at least one: Imagine that during this 11 year period there were 5 major SSWs with one winter with one SSW, the next without, etc. Since the authors claim that SSWs modify mesospheric water vapor (page 888, l.24), a spectral analysis would reveal a 2-year peak in water vapor that would have absolutely nothing to do with the QBO! This explanation would also explain the 2-yr periodicity in the vertical wind (Fig 5) since SSWs are associated with mesospheric coolings, i.e., anomalous upwelling.

They argue that transport must be the cause of the 2-yr peak in mesospheric H<sub>2</sub>O, but provide no convincing evidence for this. They show model spectra of the vertical wind

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in the mesosphere (presumably at high latitudes), but how does that connect to the tropical stratosphere? As I said before, I am also skeptical of these spectra.

The authors make no attempt to diagnose from the model results what could be causing the 2-yr peak in the simulated H<sub>2</sub>O in the mesosphere. They simply mention a number of possible causes like the Holton-Tan mechanism, which in fact appears not to be very robust when longer datasets are used. They repeatedly refer to SSWs as being important for mesospheric H<sub>2</sub>O, but never verify if this is the case in the model. They could have examined the SSWs in the model to see if there was a correlation with the mesospheric H<sub>2</sub>O, but didn't. They argue that a possible reason for the stronger response in the 2-yr peak in mesospheric H<sub>2</sub>O in the observations than in the model is due to the Brewer-Dobson circulation (page 891). Given that they have not demonstrated that transport from the tropics to the polar mesosphere is responsible, I am skeptical of this explanation. Couldn't it simply be due to the characteristics of the model, i.e, transport differences due to model numerics?

In the abstract they state that the 2-yr period in mesospheric H<sub>2</sub>O is due to “planetary wave activity triggered by the QBO”, which is nonsensical since the PWs they are talking about are forced in the troposphere by heating or topography, not by the QBO!

They are also not justified in using “A QBO-signal” in the title. All they have shown is that there is a spectral peak near 2 years, and have not demonstrated that it is caused by the QBO in the tropical lower stratosphere.

3. Contrary to reviewer #1 who said the paper was well written, I find the paper very poorly written. Here are just a few examples of badly written sentences or phrases: “changes its direction in a rhythm of quasi two years” (page 884, l.20); “LIMA of the GCM ...” (page 887, l.4); “Fig 4 displays results of the FFT analysis of LIMA data if using absolute amounts of the zonal wind component. If not using the absolute amounts but the calculated values ...” (page 889, l.18); “summery maximum”, “wintry downward transport”, “exhibits the same state of affairs”, and so on. A native English speaker,

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or one with much better written English, should have edited the paper before it was submitted.

The paper also contains several important but unsubstantiated statements: 1) Regarding Fig 1, the authors state that “the impact of SSWs enhancing the water vapor mixing ratio” can be seen. I don’t see how the authors can make this statement without any justification. 2) Regarding Fig. 3 they state on page 889 (line 10-11) that “the statistics of the influence of the SSWs in winter” can be seen without any reason why this is so.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 883, 2009.

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