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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Atmospheric QBO and ENSO indices with high vertical resolution from GNSS radio occultation temperature measurements" *by* Hallgeir Wilhelmsen et al.

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AC1

We thank the reviewer for the positive comments and the constructive questions. Please find our response below.

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Major comments

Comment 1a: "The authors stress the high vertical resolution of their data (line 2 in abstract and line 17 in section 5). However, I don't think it is demonstrated anywhere in the paper that this is important. Actually, in section 4.1 it is mentioned that the patterns from M1 are not sensitive to the vertical resolution."

Response 1a: The main idea of the presented study is based on introducing and demonstrating that M2 is able to exploit the vertical resolution of RO. This is mentioned several times in the manuscript, e.g., page 1, line 7 – line 9, or page 9, line 24 – line 27.

It is true that the patterns from M1 are not sensitive to the vertical resolution of the input data as stated in Sect. 4.1.

With M2, however, we extract the main atmospheric variability modes at each altitude level. Therefore, variability modes have the same vertical resolution as the input data set.

We get atmospheric variability indices with high vertical resolution *because* we use input data with high vertical resolution. For the method M2 itself, the high vertical resolution is not important.

Comment 1b: "Would the same results be obtained if the analyses were performed with re-analysis data (e.g., NCEP or ERA)? If this is the case – and I think the authors should check – then the importance of the GNSS data may not be so high."

Response 1b: We expect that both M1 and M2 would yield similar results when using reanalysis data sets instead of RO as input field. However, differences between reanalysis and the RO observational data set can be substantial, especially in the tropopause region and above. In Fig. 1 in this response we show differences between the monthly mean temperature fields from RO and ERA-I, which assimilates RO.

The distinct differences in the tropopause region and the stratosphere as well as the QBO-like signatures in the difference, stem from a known bias of ERA-I (Poli et al. 2010, https://doi.org/10.1002/qj.722, and S. Healy, personal communication).

Also, e.g. Long et al., (2017, https://doi.org/10.5194/acp-2017-289) states that all existing reanalysis data sets have difficulties describing the QBO winds.

We therefore expect that our description of the variability, especially in regions where RO is known to be of best quality, is more accurate.

Comment 2: “In the principal component analysis the authors include the latitude information. Normally when the QBO is studied from the winds latitudinal means are used. What is the reason for not using latitudinal means in this study? Does it make any difference?”

Response 2: We assume that the reviewer’s question refers to using zonal means in the analysis.

As can be seen in Fig. 4 in the manuscript, the EOF patterns reveal both latitudinal and longitudinal patterns, depending on the altitude. There is only minor longitudinal variation in the stratosphere, where the QBO is the dominant variability pattern. However, in the tropopause region and below, there is a distinct longitudinal variation.

To analyze the impact of the longitudinal variability on our results, we repeated the analysis for M2, using only zonal means from RO temperature. In Fig. 2 in this response we show the difference of the resulting PCs from using zonal means to the PCs including the latitudinal information as shown in the manuscript (Fig. 5). We find only minor differences in the stratosphere but distinct differences near the tropopause and in the troposphere.

The main goal of the methods is to capture the atmospheric variability at the respective altitude levels, and not only the variability originating from the QBO. This is the reason why we do not use latitudinal bands in our analysis, but include the whole field.

Comment 3: “Can anything be said about the coupling of the ENSO and the QBO? The method M1 includes all levels both in the troposphere and in the stratosphere so I wonder if it would be possible to gain insight into the proposed connection between these two parts of the atmosphere.”

Response 3: We thank the reviewer for pointing to this interesting topic. In the revised version of the manuscript we cite several additional studies where the coupling of ENSO and QBO and teleconnections are discussed.

As the reviewer suggests, method M1 might be useful to investigate connections between the troposphere and the stratosphere.

However, the main focus of this paper is to describe a method to detect the atmospheric variability in the QBO and ENSO regions. Investigating coupling effects is beyond the scope of this work and would require a dedicated study.

We added the following paragraph in the introduction:

“The interaction between ENSO and QBO has been investigated in many studies (Taguchi, 2010; Schirber, 2015; Christiansen et al., 2016; Geller et al., 2016; Hansen et al., 2016). For further literature on the relationship between ENSO, QBO, and teleconnections, see e.g. the introduction in Dunkerton (2017) and references within.”

Minor comments

Comment 4: Page 2, line 5: “The new paper by Dunkerton (10.1002/2017JD026542) could be included here.”

Response 4: Added to page 2, line 5.

Comment 5: Page 2, line 18: “The paper by Christiansen et al. (10.1002/2016GL070751) suggesting a coupling between ENSO and QBO could be cited here.”

Response 5: We added the reference in the introduction section.

Comment 6: Page 2, line 26: "This sentence is unclear."

Response 6: We replaced the sentence

"QBO characteristics can be exploited in the commonly used QBO indices, often derived from wind speeds, serving as proxies to describe the QBO."

with

"The QBO is often characterized by wind measurements."

Comment 7: Section 2, line 26: "How many grid-points with missing data do you have? "..boundaries..": But the data-set is global?"

Response 7: We thank the reviewer for pointing to this question.

The EOF analysis method requires that there are no missing data. We did the calculations on a 30°S to 30°N slice of the global data set. After doing the bilinear interpolation, missing numbers were still found at the boundaries of the 30° latitude limits.

In the revised manuscript, we first do the bilinear interpolation on the global grid. We then select the ±30° latitudinal band. The term "boundaries" is therefore not needed any more.

The number of missing points depends on altitude and time. In the beginning of the time series (the first 6 years), about 10 % to 30 % of the 5° x 5° latitude-longitude grid do not contain data. In the worst case (only in the first month of the time series), up to 60 % of the data is missing. Later, starting 2006, when more RO missions contribute, there are no missing data in the investigated spatial domain, 30°S to 30°N.

We replaced

"Grid points with missing data are filled horizontally using bilinear interpolation, at each time step, or filled with the nearest neighbor data at the boundaries."

with

"At each time step and each altitude level, grid points with missing data are filled horizontally using bilinear interpolation."

Comment 8: Page 3, top: "How much does this prior knowledge influence the temperature in the tropopause? Is it a large part or can the ENSO be seen in the raw GNSS data alone?"

Response 8: Up to 25 km the impact of the high-altitude initialization on the RO temperature is small. Therefore the tropopause is not influenced by the prior knowledge. See Angerer et al. (in press, 2017, https://doi.org/10.5194/amt-2017-225).

Comment 9: Section 3.1, line 4: "Does the centering matter? Is this not already included when you use the covariances?"

Response 9: Yes, the centering is already included.

We replaced the sentence, page 4, line 22:

"The resulting matrix is therefore a two dimensional matrix, in space (s) and time (t) only, $X(s_{(\phi,\theta,z)},t)$, represented by Eq. (1)."

with

"The resulting matrix is therefore two-dimensional, in space (s) and time (t) only, $X(s_{(\phi,\theta,z)},t)$, represented by Eq. (1), where each row, $\vec{x}_{s_p}(t)$, corresponds to a time series at a specific location (in ϕ,θ,z)."

and removed the whole sentence, page 5, line 4:

"Each row, $\vec{x}_{s_p}(t)$, which represents a time series at a specific location, is *centralized* by subtracting the arithmetic mean of the time series at each grid point."

Comment 10: Page 6, line 4: "Antipodal? Is this the right word?"

Response 10: We replaced "antipodal" with "with opposite sign".

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Comment 11: Page 6, line 8 – 13: “I found it hard to follow this. What does “this pattern” refer to? Is there a QBO pattern in the stratosphere with a longitudinal structure? It might be a good idea to merge section 4.6 with sections 4.1 ad 4.2. Many of the questions that arise reading sections 4.1 and 4.2 are answered in section 4.6.”

Response 11: We agree that this sentence is not clear, and changed it (see below). We considered merging Sect. 4.6 with Sect. 4.1 and Sect. 4.2, but we prefer the current structure.

We replaced

“This pattern around the tropopause is also visible in the third and the fourth EOFs (EOF3 and EOF4 respectively).”

with

“This longitudinal variability pattern around the tropopause is also visible in the third and the fourth EOF (EOF3 and EOF4 respectively).”

Comment 12: “Fig. 1: How is the tropopause calculated?”

Response 12: The tropopause height is calculated according to the WMO definition of the lapse rate tropopause (WMO, 1957) on the monthly mean temperature profiles. Further information on the algorithm we use can be found in Rieckh et al. (2014, http://dx.doi.org/10.5194/amt-7-3947-2014).

We replaced the line in the figure caption

“The gray line near 17 km indicates the tropopause height.”

with

“The gray line near 17 km indicates the tropopause height for the monthly mean temperature profiles, calculated according to the WMO definition of the lapse rate tropopause (WMO, 1957).”

Comment 13: “Fig. 2: Perhaps the standard pressures corresponding to these vertical levels could be given.”

Response 13: We added this information to Fig. 2 in the revised manuscript.

Comment 14: “Perhaps the last sentence in the abstract and the sentence in section 5 beginning with “We provide ..” should be removed. They sound as if you want to sell me a used car.”

Response 14: We reformulated the respective sentence in the summary section from

“We provide vertically high resolved atmospheric variability indices which can deliver improved information on the natural variability patterns such as QBO and ENSO.”

to

“Vertically high resolved atmospheric variability indices can deliver improved information on the natural variability patterns such as QBO and ENSO.”

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-226, 2017.

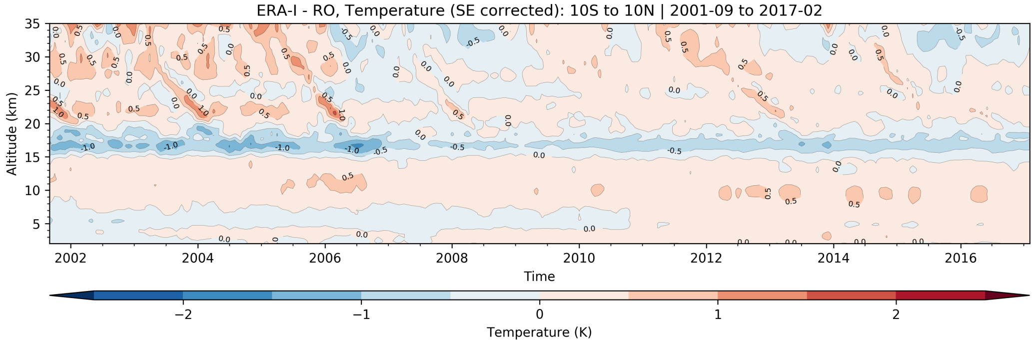

Fig. 1. Monthly mean temperature differences from RO and ERA-I.

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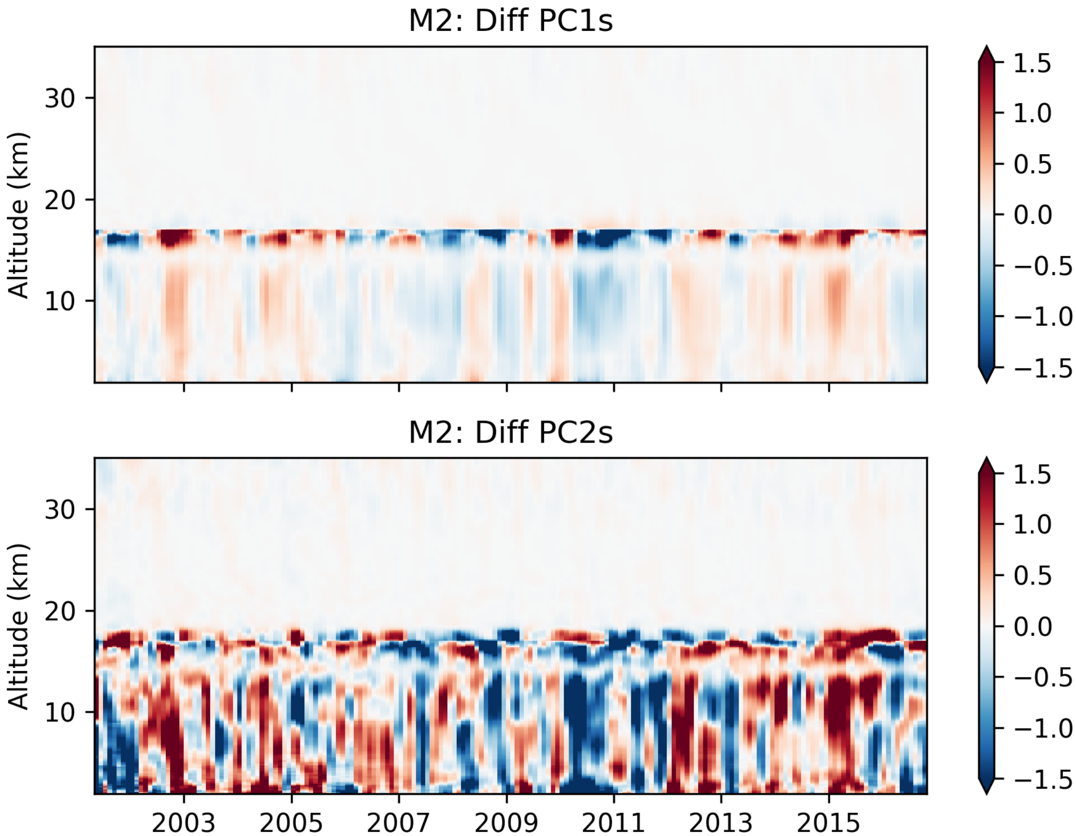

Fig. 2. Difference of PC1 and PC2 obtained from M2 using zonal means and M2 using the whole input field.

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