

Interactive comment on “GPS radio occultation with CHAMP: monitoring of climate change parameters” by T. Schmidt et al.

Anonymous Referee #2

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GENERAL COMMENTS:

The paper presents the relatively new radio occultation (RO) technique using CHAMP data, and aims at demonstrating its potential for monitoring climate change parameters. The subject is relevant in a variety of contexts, and the data set being used is appropriate to analyze many scientific questions that should be addressed. However, there are substantial shortcomings in the author's approach to important issues, which are detailed below under major comments. Also, some of the conclusions drawn by the authors are not supported by the presented results.

My current judgement is that the paper can not be accepted for publication in ACP in

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its present form. However, as the available data set is absolutely sufficient to address the scientific questions being raised, the manuscript can be improved significantly, and might be adequate for publication after major revision.

MAJOR SCIENTIFIC COMMENTS:

The paper aims at demonstrating the potential of GPS RO measurements to be used as a tool to monitor climate change parameters. The only such climate change parameter discussed here is the tropical tropopause height, which is indeed important and justifies a thorough analysis. However, the authors should consider to change the title by replacing "climate change parameters" by "tropical tropopause layer".

In the discussion of tropopause heights, the authors do not exploit their data set to draw valuable conclusions. No comparison is made between ECMWF and/or radiosonde derived tropopause heights and CHAMP retrievals. Instead three different definitions of tropopause height are applied to the CHAMP data and their somewhat trivial, expected differences, as well as their equally expected seasonal variations are described. No conclusions are drawn from the retrieved time series of equatorial tropical tropopause layer (TTL), although the data set could demonstrate the potential use of GPS RO to add information in global monitoring of the UTLS region (which is the paper's scope and objective).

While the scientific discussion focuses mainly on TTL retrieval, the authors (laudably) dedicate a large part of the manuscript to investigating the retrieval accuracy of GPS RO from CHAMP data. This is done by presenting retrieved temperature and water vapor profiles. Unfortunately the accuracy assessment is not done appropriately in the present version of the manuscript and needs major revision. One problem is that the authors use ECMWF model data as a "ground truth", interpreting statistical deviations from it as the accuracy limits of the retrieval. While this is inappropriate in general, it is especially critical since the ECMWF data are also used as a-priori (or background) information in the temperature and humidity retrievals.

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No connection is made between the temperature profiles presented in section 2 and the tropopause retrievals in the subsequent section 3. The title of section 2 suggests that the authors included the retrievals only to demonstrate the quality of their dataset. A relatively small deviation of temperature deduced from measured refractivity from the ECMWF a-priori information used in this deduction serves as an argument for good data quality. However, this just shows that the retrievals from CHAMP are not inconsistent with ECMWF or totally unphysical. At any rate the comparison does not allow for the quantification of the retrieval accuracy, as suggested.

This is not say that the comparisons presented are worthless, as they could shed light on the impact of the a-priori model on the retrieval result. The questions that should be (but are presently not) addressed are: - What is the benefit of introducing GPS RO measurements if ECMWF data are used in the retrieval and the result is almost the same as the ECMWF profile (which seems to be the case) ? - What is the quality of GPS RO profiles of T and H if no ECMWF data is available ? Since radiosondes are available in this study, I suggest to re-assess the error characteristics in a comparison with in-situ balloon soundings.

This approach is indeed followed for 1Dvar water vapor profile retrievals, which also require ECMWF data as background information. However, the 1Dvar water vapor results presented are in contradiction to the conclusions drawn from them by the authors. The statistical comparison with radiosondes presented in Fig. 9 shows that the GPS RO results alone are worse than the ECMWF model, and at best identical if ECMWF is used for bias correction (which is a highly problematic procedure anyway). The authors need to explain why they recommend the use of GPS RO for global water vapor monitoring on grounds of such results. Alternatively they may want to remove the section on 1Dvar retrieval of water vapor profiles from the manuscript, as this technique might not be mature enough yet.

The entire discussion of retrieval accuracy should be revised, especially since the authors do not even mention (or analyze any) error sources of the GPS RO technique,

such as multipath, ducting, orbit precision. It is attempted to assess the accuracy of meteorological profile retrieval from GPS refractivity measurements, using auxiliary data e.g. from weather models. However, providers of numerical weather prediction and climate models might not want to introduce such meteorological information that is not independent from other model results and external measurements. Therefore the authors should address (or at least mention) the possibility of directly introducing refractivity measurements in numerical models (e.g. in a 4DVAR scheme).

SPECIFIC COMMENTS

p. 7838 abstract, line 4-5. This sentence is somewhat misleading, since the only physical quantity observable by the GPS occultation technique is refractivity. Temperature and humidity profiles can only be derived from refractivity profiles by adding additional information from other source or a-priori assumptions. At least in the lower troposphere with significant amounts of water vapor, one of the two quantities has to be known to derive the other from the measured refractivity, since $N = k_1 * p/T + k_2 * e/T + k_3 * e/T^2$. For more clarity, the authors should indicate that meteorological profiles are secondary products derived from the measurement of refractivity. This is necessary especially in the context of the subsequent sentence, which is true only for the measured refractivity.

p. 7838, abstract, line 13. "The temperature bias", remove "temperature" (repetition)

p. 7838, Introduction, line 26: The millimeter accuracy of the phase measurement enables high precision refractivity retrieval, but not the high vertical resolution. The latter depends on the temporal sampling (50 Hz). Add a sentence to clarify and define (quantify) what is meant by "high accuracy" of the refractivity measurement.

p. 7839, Introduction, line 13: replace "with focus to" by "focusing on"

p. 7839, Introduction, line 24: "implemented to" -> "implemented in"

p. 7839, Introduction, line 25: explain acronym GFZ (preferably in English)

p. 7840, section 2, line 7: "...description to..." -> "...description of..." and "...from

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CHAMP..." -> "...for CHAMP..." The authors might want to add a reference to the orbit product used (presumably IGS)

p. 7840, section 2, line 15, 16: for clarity about the ionosphere-free linear combination add "...and the known dispersion relation for microwave frequencies in the ionosphere"

p. 7840, line 20, Equation 3 A term linear in reciprocal temperature is missing in the equation for refractivity : $k_2 * e/T$ Also, better use the more common variable e for water vapor partial pressure). This term is usually combined with the first to give the "hydrostatic" (not wet) delay

p. 7840, line 22: "derived with" -> "derived from" or better "determined by"

p. 7841, line 1-2: "new dimension of data" seems overstated (and misformulated) - "new kind" is more to the point. Also the independence from "any kind of background information" could be argued, since the retrieval of bending angles from measured phase differences requires orbit information, which incorporates external measurements (e.g. from reference GPS satellites, SLR, etc.) and models (orbit integration).

p. 7841, line 2-4: I assume this approach (dry air assumption) only works in the stratosphere. Please add a sentence to clarify. What about the ambiguity between pressure and temperature ?

p. 7841, line 6-7: Clarify how this initialization is done. Are both T and P (or just P, as possible) taken from ECMWF, N calculated for 43 km and then all measurements of N referenced to that value ?

p. 7841, line 10-12: Are the bias and standard deviation determined over the entire profile (down to zero) or just over the UTLS ? I doubt this confirms excellent data quality. With the hydrostatic equation being used and initialized with ECMWF data, this could only show consistency of GPS RO data with the model

p. 7850, Figure 2:

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add axis title on abscissa of the plot on the left (Temperature [K]) and on ordinate (Pressure (not only unit))

p. 7841, line 15: replace "However" by "Therefore"

p. 7841, line 19: In the context of the Optimal Estimation method, better use the more commonly used term "a-priori information" instead of "background information"

p. 7841, line 23: How is the vertical resolution determined ? Is it only determined by the sampling rate or have averaging kernels of the retrieval been calculated ?

p. 7842, line 4,5: Better: "The mass exchange of water and other chemical species..."

p. 7842, line 16: replace "...has not a..." by "... does not have a..."

p. 7842, line 19,20: use singular "...the interaction of...determineS..." or remove "interaction of" and use plural "...determine..."

p. 7842, line 25 - p. 7843, line 18 and p. 7852, Fig. 4 It was said that the vertical resolution of the CHAMP temperature profiles varies between 0.1 and 1 km. What is the vertical resolution in the plotted region (16-18 km) ? Should be specified in the text. Why are no ECMWF tropopause heights plotted in Fig. 4 ? This would be far more interesting than just plotting three different TTL versions (definitions) from CHAMP and comparing those.

p. 7843, line 5, 6 and entire paragraph:

"...supporting the interpretation of ... for LRT and CPT altitude:" I do not see any "interpretation" supported here. Fig 4b merely shows the same TTL as in Fig. 4a just plotted on an pressure scale. The observations described in this paragraph summarize well known and rather trivial conditions (e.g. the LRT is by definition a few 100 m below CPT (corresponding to 5-10 hPa), the 100 hPa level is always at constant height, the seasonal variation is as expected). It is not clear why these common features are described here as observations in such detail, and this section should be shortened

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(e.g. one sentence for the corresponding height and pressure difference in LRT and CPT).

p. 7853, Figure 5 and p. 7854, Figure 6 The color scale could be expanded to display more than 3 colors and resolve more structure

p. 7843, line 24 - p. 7844, line 2 "Thus, already this single...where no interpolation is necessary" This is a somewhat trivial statement. The global coverage of GPS occultation measurements has been demonstrated many times before and can be calculated for any desired time interval and region from the given satellite constellation alone. And surely the shown contour plots of averaged tropopause values at 10° intervals involved some interpolation ?

p. 7844, line 6-18 Have these data already been shown in the reference given in the first sentence of the paragraph ? The first 31 months of the CHAMP mission are included in this plot...

p. 7844, line 22,23 and p. 7856 Fig. 7:

I don't agree Fig. 7 reveals significant improvement of "background" (ECMWF) humidity. The Figure shows that the 1DVar retrieval does not differ from the a-priori (ECMWF) data down to the 800 hPa level, and shows large deviations from the radiosonde. Below that level the retrieval runs lower than ECMWF and closer to the radiosonde. Given the difficulties of near surface GPS occultation measurements and the large refractivity bias wrt. to ECMWF in this area (see Fig. 3), this single profile does not provide sufficient evidence for improvement. If there is anything that can be concluded from Fig. 7, it is that humidity retrieval from GPS occultation measurement do not work well.

p. 7845, line 3-5: "Radiosonde data were quality checked..." Again, ECMWF is used as a comparison standard (ground truth), although the in-situ measurement (radiosonde) should be more suitable.

p. 7844, line 24 - p. 7845 line 11 and p. 7856 Fig 8: Figure 8 confirms that the humidity

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1Dvar retrieval from GPS RO measurements a) yields results which are worse than the ECMWF model (comparing panels a and b) b) yields (almost) identical results as the ECMWF model, if that model is used for bias correction.

Again, the conclusion (not drawn by the authors) seems to be that humidity profiles from GPS RO do not provide additional information to numerical weather models.

p. 7845 line 12,13 The statement, that 1Dvar retrievals may be used for investigation of global water vapor distributions is not supported by the data presented here (see previous comment).

p. 7845 line 12-19 and p. 7857, Fig. 9: The presented Figure shows the global water vapor distribution from GPS RO measurements, but the quantitative comparison shown in the same section casts serious doubts on the usefulness of such retrievals, even at the 700 hPa level. Fig. 9 should therefore be compared to a corresponding figure showing the H₂O distribution from ECMWF, which I expect to show (almost) the same distribution.

The authors may also consider to remove section 4 entirely and focus only on tropopause parameter retrieval.

p. 7845 line 22: "Because of accuracy..." In my opinion, the accuracy of GPS RO retrievals of temperature has not been quantified in this study, as the statistics of a comparison with a numerical model do not represent the retrieval accuracy. Therefore the statement, that accuracy makes GPS RO a suitable tool for global monitoring of UTLS seems too bold at this time, while the other factors (resolution, coverage) certainly provide the potential for it. Reformulate the phrase accordingly.

p. 7846 line 2,3 It is my understanding that FSI has been used in all the retrievals presented in this study. If this is true the reduction of the refractivity bias due to its introduction was not demonstrated here. Please add a reference or remove phrase from conclusions of this study.

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p. 7846 line 6: replace "Potentials..." by "The potential..." As indicated in my comments above, I do not agree that such a potential has been demonstrated here (but rather the opposite).

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7837, 2004.

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