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Interactive Comment

Interactive comment on "A perspective on the fundamental quality of GPS radio occultation data" *by* T.-K. Wee and Y.-H. Kuo

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We thank the reviewer for many useful and careful comments. Our point-by-point response is given as follows.

The authors use radio occultation (RO) excess phase data to determine the quality of RO data with respect to operational analysis and reanalysis data. The use of excess phase data for this purpose is a new approach, and the paper deserves to be published after revision. General comments: (1) The structure of the paper is somewhat odd. The introduction is quite long, and the general part is literally loaded with references (there are, e.g. not less than ten references backing the argument that reanalyses are susceptible to deficiencies of the observations (page 9484)). The reference section is therefore almost as long as the paper itself. Compared to the introduction the results (and discussion) section is surprisingly short and the results are just illustrated with three figures. Response: We wanted to make our manuscript as concise as possible because technical details of our approach are already given in our previous publication (Wee et al., 2010) and we did not want to repeat the same verbose description. We still think that three figures are enough to summarize our key findings. In order not to make readers feel we are making groundless claims though, we needed to provide a rather extensive list of references on which our approach is based.

(2) In contrast to the wealth of citations in the general part of the introduction there are comparatively few references to previous work on the climate quality of RO data (some examples can be found in my specific comments). Response: Thank you for the suggestion. More references on the topic have been added.

(3) I agree with the comments of referee # 2 and repeat (for the sake of brevity) only those points, which I regard as particularly important. Response: Answers can be found in our reply to the comments of referee # 2.

(4) The discussion and the conclusions are based on a comparatively short data period: May – August 2002. This is fine for a demonstration of the new approach, but the conclusions are way too general. It should also be mentioned that the quality of operational analyses has considerably increased since 2002 – in particular in the Southern Hemisphere. Response: The quality improvement of operational analyses is now mentioned. We have revised the conclusions adding a few points we missed to make.

Specific comments: (1) Page 9842, line 5/6: “This study assesses the fundamental quality of RO data, by modeling the “raw” measurements “ This formulation is somewhat misleading, since it suggests that you (just) used modeled data – in fact you compared measured phase data with modeled ones (Later on it becomes clear what you mean). Response: We have reworded the sentence. Thank you for pointing that out.

(2) Page 9842, line 7: “Center” should be “Centre” (British English). Response: Done. Thanks.

(3) Page 9842, line 8: Instead of “that the RO measurement is ..” I would suggest using “that RO measurements are ..” Response: Done.

(4) Page 9842, line 15: “.. showed a close agreement in the standard deviation. This confirms the high accuracy ..” Why? High accuracy is usually understood as “small systematic error”. Response: Agreed. Rewording has been done.

(5) Page 9483, line 4: “.. in the data records ..”. Please specify which records you mean – some of the cited references refer to radiosonde data, some to (A)MSU data. Response: Correct. As data types differ, we did not mention them specifically. We have revised the sentence. Appreciated.

(6) Page 9485, line 29: “The data close to raw measurement ..” should read “Data close to raw measurements ..” Response: Done. Thank you.

(7) Page 9485: This paragraph leaves the impression that “raw” data are essentially free of systematic errors, but excess phases are not really “raw” – they have already received quite some processing. Any systematic error in the orbit determination will lead to systematic errors in the excess phase data. Furthermore, the applied ionospheric correction does not remove the entire influence of the ionosphere and leaves a (small) ionospheric residual that increases with increasing electron density (see Danzer et al., 2013). Response: We understand the reviewer’s concern. We intended to provide an uncomplicated view on our approach for readers; however, it may sound too simplistic. We now explained what we meant by the “raw”.

(8) Page 9486, line 4/5: “.. assumption of spherically symmetric atmosphere.” should be “.. assumption of spherical symmetry.” or “.. assumption of a spherically symmetric atmosphere.” Response: Thank you for pointing that out.

(9) Page 9486, line 9: Here you should also cite the paper by von Engeln (2006), who

first reported on structural uncertainty in RO data. Response: The paper is now cited.

(10) Page 9486, line 21: "the retrieval uncertainty in RO is avoidable by using unprocessed "raw" data". This is not entirely true, since excess phase data are not really "raw" (see specific comment 7). Response: We now have explained what is meant by "raw".

(11) Page 9486, line 21: " by modeling L1 and L2 phase measurements directly and compare them with NWP analyses in the observation space." Is this really correct? I understood that you compared measured excess phases (after ionospheric correction) with modelled excess phase data based on ECMWF/ERA fields. Response: Thank you for pointing it out. We have corrected the sentence.

(12) Page 9487, line 4: Later on you will explain it in detail, but at this point of the paper the reader wonders why you chose the time period May-August 2002. At some point you should also mention that solar activity was pretty high during this period. Response: Reviewer's concerns are well taken.

(13) Page 9487, equation 1: You should definitely indicate the values of the coefficients (k1 – k3) you employed, and you should mention the uncertainty of the coefficients – in addition to the reference proposed by referee # 2 (Aparicio and Laroche, 2001) I would suggest citing the paper by Healy (2011). Furthermore you need to write that there are also higher ionospheric terms. Response: The paper is now cited in the revised manuscript. We have described higher ionospheric terms. Thanks.

(14) Page 9487, line 23: "source error" should be "error source" Response: Done. Thank you for point it out.

(15) Page 9487, line 25: Here you should consider citing the ray tracing study by Foelsche et al. (2011). Response: Thank you the suggestion.

(16) Page 9489, line 1: If you specifically mention the Frenet-Serret formula(s) you could consider including it/them in the manuscript. Response: We decided not to

mention the formula.

(17) Page 9489, line 14: Why did you use ECMWF analyses just on 26 pressure levels? In 2002 they have already be available on 60 vertical levels. Response: Not being a member state of ECMWF, we had to use the best data set available to us for the period.

(18) Page 9489, line 20: Did you use SMI just for the Plasmasphere, or did you use both models as alternatives – in the latter case it would be very interesting to show how the ray tracing results differ when using the different ionospheric models. Response: The top of IRI is 2000 km and we needed SMI to provide electron density above the level. IRI does not provide electron density at plasmaspheric heights. We have clarified that in our revised manuscript.

(19) Page 9489, line 29: "36512" Shouldn't the sum of 23563 and 18846 be more like 42409? Or was this meant in a different way? Response: Corrected. The number was mixed up with other data sets that we used for a different study.

(20) Page 9490, line 3: Please provide the equation for the ionosphere-free linear combination – and a reference. Response: Done. Thank you.

(21) Page 9490, line 9: This would be another option to cite the paper by Danzer et al. (2013). Response: Good suggestion. Thanks.

(22) Page 9490, line 11/12: "..a low-pass filter, fourth-order Butterworth filter .." There is a duplicate "filter". Response: Corrected.

(23) Page 9490, line 21: Ionospheric residual errors in 2002 have been higher due to higher solar activity. Response: It is likely, but a higher solar activity does not necessarily mean a larger residual error. The higher solar activity in 2002 is now mentioned.

(24) Page 9491, line 5: You mention the degradation of ERA40 forecasts in 2002. Differences between operational ECMWF analyses and RO-derived temperatures during Jun-July-August 2002 are, however, smaller than differences in JJA 2003 (Foelsche et al., 2008), though showing a similar wavelike bias structure (as reported for JJA 2003

by Gobiet et al. (2005)). Response: ERA40 does not cover 2003 and the degradation of ERA40 is reported by ECMWF. As described in the manuscript, ERA40 differs significantly from operational data. We just have no clear idea how to relate the possible inter-annual variation of operational data to that of ERA40, which does not exist in 2003. We are also very concerned about the risk of using dry temperature, which is discussed in great detail in earlier papers (Wee and Kuo: A noise-aware combination of dual-frequency measurements from GPS radio occultation, J. Geophys. Res., 118, 12,852-12,868, 2013; Wee and Kuo: Advanced stratospheric data processing of radio occultation with a variational combination for multifrequency GNSS signals, J. Geophys. Res., 119, 11,011-11,039, 2014).

(25) Page 9491, line 23: "As can be inferred from Eq. (1), the excess phase in the stratosphere inversely relates to the temperature." This is not always true – e.g. during an SSW the thermal expansion of the stratosphere will lead to an increase in temperature and density (and therefore also refractivity) at a given altitude, thereby causing an increase in excess phase. Response: Thermal expansion reduces the air density and the refractivity. Linearized equation can be written as: N'/N = rho'/rho = p'/p – T'/T.

(26) Page 9492, line 15: "Data from CHAMP and SAC-C are largely independent from each other" This is essentially right, but it should be noted that both datasets have similar systematic residual errors due to the same (incomplete) ionospheric correction applied. Response: Those are now mentioned. Thanks.

(27) Page 9493, line 10: "..OP and RA are significantly biased, and RO data are able to quantify their systematic errors." But this does not necessarily mean that RO data are unbiased (see comment 26). Response: The reviewer is right. While the bias in RO data could increase as RO data processing continues and collects systematic error components, it would not be such a bad assumption that phase data are unbiased as long as phase cycle slips are properly detected and fixed by pre-processing algorithms. While systematic errors (e.g., higher-order ionospheric residual) stay local in phase data, they are accumulated via Abel transform and hydrostatic equation during the

process of higher-level data retrieval. The vertical accumulation of bias is our main concern of using high-level data products because higher-order ionospheric residuals, for instance, can be the same sign throughout the entire atmosphere.

(28) Page 9493, line 18/19: "Our study finds that the oscillation is pervasive without being confined in the SH." This is not new: Foelsche et al. (2008) found wavelike bias structures in dry temperature also in the Northern Hemisphere – they were just most pronounced in Antarctic winter. In Northern summer 2006 they appeared with a similar magnitude in the Arctic. Response: The paper is now cited.

(29) Page 9494, line 17: "..without the involvement of RO retrieval uncertainties". This is too optimistic – you ignore all the uncertainties in the level 1 processing. Response: Agreed. We rephrased the sentence.

There are several minor issues (use of articles ...), which can be solved in a later stage of the review process (I have just commented one some of them). References: Danzer, J., B. Scherllin-Pirscher, U. Foelsche (2013) Systematic Residual Ionospheric Errors in Radio Occultation Data and a Potential Way to Minimize them, Atmos. Meas. Tech., 6, 2169-2179, doi:10.5194/amt-6-2169-2013 von Engeln, A. (2006) A first test of climate monitoring with radio occultation instruments: Comparing two processing centers, Geophys. Res. Lett., 33, L22705, doi:10.1029/2006GL027767 Foelsche, U., S. Syndergaard, J. Fritzer, and G. Kirchengast (2011) Errors in GNSS radio occultation data: relevance of the measurement geometry and obliquity of profiles, Atmos. Meas. Tech., 4, 189–199, doi:10.5194/amt-4-189-2011 Foelsche, U., M. Borsche, A.K. Steiner, A. Gobiet, B. Pirscher, G. Kirchengast, J. Wickert, and T. Schmidt (2008) Observing Upper Troposphere–Lower Stratosphere Climate with Radio Occultation Data from the CHAMP Satellite, Climate Dynamics, 31, 49-65, doi:10.1007/s00382- 007-0337-7 Healy, S. B. (2011) Refractivity coefficients used in the assimilation of GPS radio occultation measurements, J. Geophys. Res., 116, D01106, doi:10.1029/2010JD014013.

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