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Atmospheric Measurement Techniques Discussions

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 encouraging and constructive comments. Our point-by-point response is given as follows.

General comments: The paper describes the methodology and results of comparing a large amount of RO excess phase data with those modeled from ECMWF analyses using so-called Curved Ray Tracing (CRT). Both operational analyses (OP) and ERA-40 re-analyses (RA) are considered. The paper is generally well written, although the wording could be improved here and there. The paper starts with a review of the importance of NWP models for climate and the importance of unbiased high-quality observations - which leads to the argument for the use of RO data. I think the method-

ology is sound, and the results are very interesting. Looking at statistical differences between RO data and NWP analyses in the space of excess phase as a function of (modeled) tangent point altitude is new, and for this reason the paper should be published. There are, however, a number of issues that the authors have to address before I can recommend publication in AMT. In particular, the Authors argue that comparison in observation space avoids retrieval errors, and for this reason it is possible to reveal errors in the analyses that might be too subtle to be revealed with other approaches. I think that is a stretch. I elaborate in the specific comments below. I also find the discussion about ray tracing in section 2 questionable. This is also elaborated below. In my comments below I also suggest additional comparisons to clarify issues and an assessment to what extent the MSIS model that is used in the forward modeling has influence on the results. Below I have marked comments with page/line. Response: Our response is given below to specific comments.

Specific comments: 9484/10: Not quite a chicken-and-egg problem. With the chicken and the egg, it can be discussed which one came first and gave rise to the other. With NWP model versus observation, it cannot be the model bias that is responsible for the observation bias. The problem here is just that we don't know to what extent the observations are biased by looking at the model because some of the observation bias may also be in the model. Response: For GPS RO retrieval, observation bias should be independent of model bias. The exception is statistical optimization in which model systematic error could become a good part of observation bias. In the above-mentioned sentence, however, we are discussing a very specific problem - variational bias corrections used for satellite data assimilation. For the variational bias correction, it is indeed a chicken-and-egg problem due to the strong feedback between observation bias and model bias in the technique.

9486/4: Suggestion to expand the sentence here a bit: '... various sources of error, ranging from phase noise and ionospheric disturbance to the assumption of spherical symmetry in the retrieval of atmospheric profiles.' Reason: ionospheric disturbances seem to be one of the most important error sources on the stratosphere. Response: We have rephrased the sentence. Thanks.

9486/21: It is noted that the retrieval uncertainty in RO is avoidable by using unprocessed 'raw' data. What is meant here? Is it the phase and amplitude that is referred to? In practice someone has to interpret the 'raw' data, whether it is the data provider/processing center or some user of the data. Wouldn't such interpretation have to use a 'retrieval' in some sense? Bending angle is assimilated at many NWP centers today (assimilation is also a kind of retrieval; it is just using a priori from a NWP model instead of other a priori information) and requires a bending angle forward model (1D or 2D to various complexity). To assimilate phase and amplitude data even more sophisticated forward models are necessary. But forward models are not free of uncertainty either. In general, whether it is refractivity, bending angle or phase, forward models have their own deficiencies and simplifications to trade-off implementation complexity, accuracy, and speed. Thus, retrieval uncertainty is to some extent just replaced with forward modeling uncertainty. In fact, in the forward modeling later in the paper, the Authors use MSIS above 48 and 65 km (depending on the ECMWF model). I would guess this influences the modeled excess phases to a similar degree as the use of MSIS in statistical optimization influences retrieved atmospheric profiles (albeit at a higher altitude when the upper boundary is at 65 km). So I think it is a bit misleading to say that 'retrieval uncertainty in RO is avoidable by using unprocessed data'. Maybe just say 'can be minimized' instead of 'is avoidable'. BTW, 'unprocessed' RO data (phase measurements) are not perfect, they have uncertainties related to orbit determination and different sources of measurement errors, and are processed from even rawer data. Receiver software algorithms may contain choices that could be considered structural uncertainty, e.g., how far down occultations are tracked, at which height open loop tracking is started, etc. Response: Our analysis on the influence of MSIS is given separately as a supplement. As the reviewer mentioned, excess phase data are not truly raw and we used scare quotes to denote the fact. On the other hand, the carrier phase is one of primary observables of GPS RO and is customarily considered

as the starting point of atmospheric retrieval process. The term “raw” is used on purpose because we think that describing the data as ‘raw’ instead of ‘less or minimally processed’ helps readers to catch the main point of our study. We understand the reviewer’s concern and have reworded the sentence. Higher-level data are convenient for comparison; however, interpretation of the results is never easy and can be easily misled by retrieval errors. As we explained in our manuscript, our main concerns of using higher-level data are vertical error propagation and subsequent boost of observation error correlation as the result of continued data processing. For instance, when RO refractivity looks faulty compared to independent observations at an altitude level, it is impossible to track down origin of the error because the error is not local and highly correlated in vertical. In other words, a spike in phase measurement could result in a highly dispersed refractivity error. The comparison in retrieved parameters is thus nontrivial because of the retrieval error and its widened correlation. On the contrary, NWP errors are inherently correlated and thus using NWP data for observation modeling does not make the problems (associated with the error correlation) substantially worse. We agree with reviewer on that sophisticated forward model generally increases modeling error. In particular, modeling of excess phase for data assimilation could be complicated because doing so requires additional information (such as satellite orbits) and is also constrained by the geometry of model grid (e.g., top height). However, this is not a problem for observation modeling. It is though important to note that these practical complications must be distinguished from mathematical complexity of the observation operator. As can be inferred from equation (1) in our previous manuscript, the observation operator for excess phase consists of spatiotemporal interpolation, Smith-Weintraub equation (which relates atmospheric parameters to refractivity), and spatial summation of refractive index along the ray path. We acknowledge that computation of the ray path is a bit complicated. However, well-implemented ray tracers are able to model the ray paths accurately, within the accuracy limit of NWP refractivity field. In addition, ray shooting substantially reduces the mismodeling of ray paths. Meanwhile, due to horizontal inhomogeneity and measurement error, the estimated spatial locations of bending angle or refractivity (or dry temperature) are never correct. When NWP data are compared to RO data at those estimated locations, the positional error introduces additional uncertainty to the comparison. We don't find any convincing evidence that the error due to mismodeling of ray path is bigger than the uncertainty caused by the positional error of tangent points. We strongly believe that the potential increase of NWP error due to observation modeling is negligibly small compared to the increase of retrieval error due to continued data processing. Spherical symmetry assumption, inverse Abel transform, and hydrostatic equation are major causes to blame. We agree with reviewer that at some point evaluation must be performed. However, doing it in the model space is not the best option and we instead suggest that the comparison should be done in the measurement space. It is well known that the limb-viewing geometry of GPS RO allows a high vertical resolution for its data. This is due to the fact that a relatively thin atmospheric layer above a tangent point contributes most of ray's bending and phase delay to the measurement point; and, the contribution drops rapidly with increasing distance. The same applies to forward modeling. Please see the supplement we have provided for a detailed analysis. We don't claim that excess phase data are perfect or error free. However, the error structure is considerably simple compared to those of retrieved parameters. Because the phase error is largely random, spatial averaging (or filtering), for instance, considerably reduces random error component. This allows us to separate systematic observation error from random noises. We don't attempt to exaggerate the importance of this property. Indeed, the simple error structure of phase data allows us to estimate the observation error even without the aid of other independent data sets (via so-called dynamic error estimation). This in turn greatly increases the confidence of comparison with other data sets. On the contrary, the comparison of high-level RO data with other data perceives combined errors of two data sets. It is challenging in that case to determine which of the two is responsible for any discrepancies without the knowledge of precise observation error. Comparison in retrieved parameters is convenient, but it lacks theoretic basis. Error estimation for retrieved parameters is of higher uncertainty and imprecise, and doing so

is likely to inflate the actual error GPS RO measurements. We rephrased the sentence for clarification.

9486/22-23: Suggestion: 'quality' instead of 'worth'. Skip 'negating the processing-induced uncertainties'. Response: Rewording has been done.

9486/23-25: 'modeling ... and compare them with NWP analyses'. Where are the observations in this context? I think it needs to be reformulated. Response: We have rephrased the sentence.

9487/11 (eq. 1): This is a good approximation, but it is not exact. The ionospheric part of the refractive index contains higher order terms and the neutral atmospheric part is an empirical approximation, and perhaps not the best available (see e.g., Aparicio, J. M., and S. Laroche (2011), An evaluation of the expression of the atmospheric refractivity for GPS signals, J. Geophys. Res., 116, D11104, doi:10.1029/2010JD015214). This is an example of structural uncertainty that is not avoided by using 'unprocessed' data (e.g., in assimilation).âĂÍ Response: We compared different refractivity formula quite a while ago and concluded the difference among them is very small compared to the magnitude of NWP error. We concluded that it is more important for data analysis/assimilation to use same formula and constants that are used for data processing. While it is desirable to use accurate parameters, we view ensuring the consistency between data processing and forward modeling is equally important. The higher-order ionospheric terms contribute less than 0.1% [Seeber, 1993; Bassiri and Hajj, 1993; Fritsche et al., 2005] and are very small compared to the uncertainty of electron density fields provided by empirical models. Refractivity representation can be certainly improved; however, this may require a separate study. We don't think the effect is big enough to affect any conclusions of our study. We agree that it is not possible to completely eliminate structural uncertainties. However, we think that efforts to reduce the structural uncertainty are important, and must be encouraged and continued.

9487/14: Suggestion: 'contributions' instead of 'effects'.âĂÍ Response: Done. Thanks.

9487/16: The values of the constants k1, k2, and k3 should be given for completeness. Response: Done. Appreciated.

9487/23-24: Suggestion: '... which [would be] a major source [of] error in modeling the measurements. Ray tracing, on the other hand (e.g., Kirchengast ...; Wee et al. 2010) can precisely model ... atmospheric variations.'. Response: Thank you for a good suggestion.

9487/24-25: One of the first ray tracing codes for RO was developed by Hoeg et al. ("Derivation of atmospheric properties using a radio occultation technique", DMI Scientific Report 95-4 (ISBN 87-7478-331-9), 208 pp, 1995, section 3.2.4). It later became part of EGOPS, so implicitly it is cited by citing Kirchengast (1998), but I suggest to also cite Hoeg et al. (1995) because this is where the approach is described in detail. There are no details about the ray tracer developed by Zou et al. in the cited work (Zou et al. 2004). I suggest to cite Liu and Zou (JGR, 2003) instead since this is where the latest version of that code was described. I also suggest to cite the ray tracer developed by Michael Gorbunov (Gorbunov and Kornblueh, "Principles of variational assimilation of GNSS radio occultation data", Report No. 350, Max-Planck-Institute for Meteorology, Hamburg, Germany, 2003, section 2.5). Response: Thank you for the good suggestion.

9488: There is no evidence to what the Authors claim about other ray tracers and the comparison to their own CRT approach. I could not find such comparison in the Wee et al. (2010) paper, where the CRT is introduced. The Authors refer to other ray tracers as straight-line ray tracers (SLRT) and argue from there that the CRT approach is better. I think that is a misunderstanding. The ray tracers for RO simulations that are cited all solve a set of differential equations to find points along the ray and eventually the phase and/or the bending angle. There are no assumptions about straight lines in between the points; all cited works use higher order schemes. There are different approaches with various approximations to solve the differential equations and trace the rays with a minimum of computational cost. I've read the paper by Wee et

al. (2010), and the CRT is just one such approach (I admit that it is innovative and probably a good and fast approach). But it is not clear what the error is of the CRT. There is no evidence that it should be better or faster than other approaches. It may well be, but the evidence is missing. However, the Authors do not need to justify their choice of ray tracer in this study. Just say: this is the ray tracer that we used. It is described in Wee et al. (2010). The story does not have to be longer than that. In other words, the discussion on page 9488 should be removed unless the Authors can provide firm evidence of their claims (numbers with accuracy, precision, computational speed and comparison to other specific ray tracers). In my opinion, such evidence can only be obtained through a comprehensive study making sure that different ray tracers use the same atmosphere and ionosphere models. I do not suggest to make such a study in this paper, it would shift the focus of the paper, and that is probably not what the Authors want. It is better to just remove the discussion. In any case, other ray tracers, such as the ones cited, should not be referred to as straight-line ray tracers. Response: Just for clarification, what proposed in CRT is use of curved basis functions for curved elements (path segments) and we did not attempt to improve numerical integration scheme itself. Regardless of the order of numerical methods, finite difference schemes are based on piecewise linear approximation. Higher-order schemes take multiple trial steps to compute slopes (gradients) at those intermediate points. CRT replaces the slope with ray's curvature. We initially implemented a "standard" ray tracer using fourth-order Runge-Kutta method, which was a straightforward task. After some extensive tests, we decided to develop CRT in order to overcome limitations of our conventional ray tracer. In particular, we were not satisfied with the accuracy and cost of our conventional tracer around ray's tangent points. Although Runge-Kutta method can be adopted for CRT as well, we found that the CRT with first-order Euler's method already produces results far superior to the solution of our conventional ray tracer. Although our comparison with radiosonde data clearly showed the strength of CRT, we could not rule out the possibility that the result of comparison depends on how the ray tracers were implemented. For the past few years we were caught up in different research topics, but we plan to publish the result in the future. Reviewer's comments are well taken and we have revised the paragraph.

9489/9: Suggestion: 'was modeled' instead of 'was made available'. Response: Done. Thanks.

9489/29: 23563 plus 18846 exceeds 36512 by 5897. Is this the number of occultations discarded in the quality control? If so, it is more than just a few. Anyways I would expect the numbers to add up the way the sentence is written. Response: Corrected.

9490/9-10: The Authors write that '... modeled and measured phases were consistent and very similar to each other (Wee et al., 2010)'. However, Wee et al. show only one nice example. I doubt that all are as nice as that one. Please do not generalize from just one (or a few) examples. I assume the Authors have looked at many examples, but then they should say so, and not just refer to their own paper that shows only one example. Are all the examples that the Authors have looked at as nice as the one in Wee et al. (2010)? Response: We think that the extensive comparison the reviewer has asked is presented in this study. We are pretty confident that ALL soundings (of sufficiently high SNR) used in this study are as NICE as the two cases we showed in our previous paper. We are proud of the stability and computational efficiency of CRT.

9490/10: It should be mentioned here if the observed excess phases are adjusted by a constant offset based on the modeled excess phases (as in Wee et al., 2010). Otherwise one will not understand how percentage differences can make sense at high altitudes. It should also be mentioned how the height/altitude in the following figures is calculated. Response: The phase ambiguity exists only when ionospheric effects are retained. Atmospheric phase delay diminishes to zero at the top of neutral atmosphere. It is therefore sufficient to set the observed excess phase to zero at TOA (or the same value with modeled phase if the top of occultation is too low). There is no ambiguity for atmospheric excess phase. We missed mentioning it and will clarify this. Thanks for the good suggestion.

9491/18: I suggest: 'as opposed to' instead of 'differently from'. Response: Agreed.

9491/19: I suggest: '... there is no reason why the RO measurements should be responsible for such ... differences'. Skip the next sentence starting with 'Otherwise ...' since it is not a necessary (and I don't think it is strictly correct; e.g., there could in principle be significant errors in both RO and OP and one would then not expect OP and RA to be similar in M-O). However, the forward modeling is different for OP and RA since the two models have different upper boundaries (48 and 65 km), which means that part of the difference could in principle stem from the use of MSIS above 48 km in the OP forward modeling/ray tracing. Maybe it could be assessed if this is a problem by forcing the use of MSIS above 48 km in the RA as well, perhaps just as a test on a limited number of occultations at southern hemisphere high latitudes. I think such assessment would be good to rule out that the difference between OP and RA in the southern hemisphere high latitudes is due to the use of MSIS. It would also give an idea of the reliability of the modeled excess phase. Response: We think answers to this comment are given earlier (9486/21).

9491/Discussion of Figure 1: How should the results in this figure be interpreted? It obviously tells that the OP is different from the RA, and that the two analyses may be significantly biased in opposite directions at the southern hemisphere high latitudes. This is an interesting result. But because this is a comparison in excess phase it is not straightforward to interpret the bias in well-known atmospheric parameters. E.g., What does it mean for the temperature of the RA at 25 km? Would it be cold-biased or warm-biased? The notion on page 9491/24-25 that excess phase relates inversely to temperature is in principle correct, but it is not obvious what a positive (or negative) bias in excess phase means in temperature because the pressure is also involved in eq. 1. And the whole thing is integrated along the ray paths. How does retrieved dry temperature (CDAAC product) look compared to OP and RA at 25 km (e.g., similar to Figure 1, but for dry temperature)? Is it obscured by retrieval uncertainty? Could such temperature biases be confirmed by previous studies? It could actually be very interesting to see how the difference to OP and RA varies with altitude at these southern hemisphere high latitudes (e.g., above 70S), both in excess phase and dry temperature. Having both OP and RA with such distinct differences, and looking at both excess phase and dry temperature, could give an indication of how far up in the stratosphere the retrieved dry temperature is reliable. Response: Considering the uncertainty of dry temperature, we do not find any good reason to believe that the comparison in temperature could strengthen our study. Quite the opposite, we propose to use underived measurements to deal successfully with retrieval issues. The limitation of using dry temperature is discussed in detail in our recent publication (Wee, T.-K., and Y.-H. 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). Briefly speaking, it is shown that statistical optimization can lead to 4-8 K error in the stratospheric dry temperature (Wee et al., 2013).

9492/11: I'm not sure it makes sense to say that the difficulties are fundamental in contemporary NWPs. If they are truly fundamental, then perhaps the word 'contemporary' could be skipped. However, I suggest to say just 'the' instead of 'fundamental' since claiming that the difficulties are fundamental is too strong. Response: Following the suggestion, we have reworded.

9492/19-20: I suggest: 'Especially, [the] zonal mean values of the O-M and the envelopes of one standard deviation are not very different'. Response: Done. Thanks.

9493/10-11: 'As backed by the results so far, both OP and RA are significantly biased, and RO data are able to quantify their systematic errors.'. This is probably true, but I'm not totally convinced. Verification that the use of MSIS has minimal influence on the results, and further verifying results by looking also at dry temperature retrievals (as suggested above), would make a much stronger case. Response: As explained earlier, our study is motivated by the fact that dry temperature is unreliable. We showed earlier that MSIS virtually has no influence on the comparison at the height of 25 km.

9493/14-21: This part needs re-evaluation or should simply be removed. It is not clear if the oscillations in Figure 3 are related to those observed by, e.g., Gobiet et al., (2005), because what we see here is the excess phase whereas Gobiet et al. showed retrieved dry temperature. There is not enough evidence to say that 'Our study finds that the oscillation is pervasive without being confined to the SH.' If this is true, it is contradicting the results by Gobiet et al. (although it is for a different year). The Authors argue that it proves that the phase measurements can 'capture the artifact in the analyses that might be too subtle for other approaches'. I strongly disagree with this statement if it is based on the results in this paper. The few oscillations in Figure 3 are not sufficient evidence of pervasive oscillations, and I'm not convinced that the excess phase comparisons (when MSIS is used above 48 km in the forward modeling) can reveal anything more than what can be revealed by dry temperature comparisons (as done by Gobiet et al.). Response: We do not find any contradiction. Dry temperature is expected to be useful, but only for qualitative or visual comparison. We are certain though that dry temperature has limitations to be used for quantitative analysis. In order to quantify the observation accuracy precisely and with minimal influence of adverse error propagation, we strongly suggest use of excess phase. As dry temperature is derived from excess phase, any temperature discrepancy with can be traced back to corresponding phase difference. Even in ideal situation (e.g., if a perfect statistical optimization were available), retrieved dry parameters would have considerable phase shifts due to hydrostatic integration in the height coordinate. Comparing temperature in pressure coordinate does not resolve the issue because NWP model contains significant pressure error. By comparing measured and modeled excess phases, there is no need to worry about how to separate temperature and pressure errors from each other. We just observe the combined effect in terms of refractivity (note that phase is fundamentally nothing but spatially integrated refractivity).

9494/10-12: It is not quite clear what 'also' refers to. It is not quite clear what 'These areas' refer to. Maybe you could write something like this: 'The standard deviation also increases significantly with lead-time in the NH lower stratosphere. Thus, the SH stratosphere as a whole, as well as the NH lower stratosphere, seems to be areas where there is room for improvement in ECMWF forecasts.'. Response: We have rephrased the sentence and appreciate the suggestions.

Technical corrections: 9482/19: 'is challenging' instead of 'are challenging' (as in 'the scientist is doing this and that'). Response: Done.

9486/9: "...inter-center differences increase" Response: Thank you pointing it out.

9486/13: Skip 'the' in front of 'structural uncertainty' Response: Thanks.

9487/14-15: Put 'is' in between symbols and words, i.e., 'T [is] temperature...', etc. Response: Done.

9489/21: "[the] ionosphere and [the] plasmasphere" Response: Done.

9490/4: '... in the modeled phase [is] to be removed' Response: Thank you.

9490/19: "[The] RO technique" Response: Change has been made.

9491/7: "... could differ [by about] 20 K" Response: Thanks.

9506/Figure 1 caption: Skip 'the' in front of 'period'. Say '... least-square[s] fit and yellow curve[s] indicate the envelop[e] ...' instead of '... least-square fit and yellow curve indicates the envelop ...'. BTW, the yellow curves look green in my copy. Maybe they could be thicker. Response: Thank you for the suggestions.

9507/Figure 2 caption: 'separation of RO missions' instead of 'deviation of RO data'. Response: Done.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/7/C4814/2015/amtd-7-C4814-2015-supplement.pdf

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