

AMT manuscript amtd-4-135-2011

“Exploring Earth’s atmosphere with radio occultation: contributions to weather, climate, and space weather”, by R. A. Anthes

Review comments

Overall assessment, general comments:

This paper is a nice review of the measurement characteristics and scientific achievements of GPS radio occultation (RO) since its conception and in particular since the GPS/MET experiment. It undertakes this review with a broad scope but despite of this challenge appears to keep a quite good logical thread. And, in fact, regarding its text it is written in a fairly concise manner given its comprehensive scope. The style is good and it is fluent to read so in the opinion of this reviewer the text will only need minor “polishing” here and there; see specific comments below.

What is striking is that the paper contains an unusually high number of figures (more than forty) and the question obviously arises whether this should not be streamlined somewhat, without losing much information. Assessing this more closely it appears, however, that the chosen number and type of illustrations, and the chosen interplay of text and illustrations to effectively convey the information, seems to justify the number in this specific review case. As to the quality of (some of) the figures some substantial improvements seem needed, though; mostly of a technical nature, see some specific comments below.

A further point is that referencing seems a bit unbalanced currently, somewhat biased towards the UCAR Boulder work where the home base of the author is. Given it is a review-type paper it should strive for more balance in this respect. Some suggestions to help remedy this are included in the specific comments below. These are probably leaned somewhat towards “European papers” (GFZ, Wegener Center, etc, with easily accessible publication homepages; and there was limited time to check beyond the easily accessible) – but anyway the author should consider also beyond these suggestions to see whether some other relevant work, complementary to key UCAR papers from which most figures derive, should possibly be cited.

Notwithstanding these few “grumblings” I rate this overall to be a very good review article, well written by one of the pioneering and leading experts in the RO field. It should clearly be published and my overall recommendation to accept it “subject to minor revisions” reflects this positive view. Still these “minor revisions” are not only a few small corrections but I would like the author to carefully consider all specific comments below in revising the manuscript. If properly accounted for I recommend the revised paper be published in AMT, clearly a very valuable review.

Specific comments:

Figures overall:

A range of the figures seem to be taken directly from presentation slides. This leads to large-font titles over some figures, sometimes also large-font subtitles, sometimes other legend-type text at unusual places nearby figure panels, etc.

In fewer cases there is the different problem of titles/fonts being clearly too small (in particular, Figs 4 and 20; Fig. 28, right).

These figure style weaknesses are inappropriate for paper-quality figures such as they should be included in the final published AMT paper. I thus strongly suggest that each figure is re-assessed from this “publication-quality readiness” perspective. In the majority of cases it is anyway just elimination of inappropriate presentation slide-type titles / subtitles (maybe in some cases replace by standard-font titles integrated immediately above figure panels or by suitable in-panel legends).

In other cases of “side information” to the figs (e.g., like Figs 7-9, 31, 38,...) such information might be moved to the figure caption.

References overall:

See the paragraph “A further point is that referencing seems a bit unbalanced currently...” under the overall assessment/general comments section above; please work to improve accordingly. Some specific suggestions for additional refs are included throughout the specific comments below; the related references list is attached below.

Order of how troposphere, stratosphere, and ionosphere are addressed:

A general point on the overall logic: as per title the review logic is to first treat neutral atmosphere and then ionosphere / space weather, which makes very good sense. On the other hand there is several places with using the order “ionosphere, stratosphere, and troposphere” (e.g. in Table 2 and elsewhere in the text) or “profiles of electron density in the ionosphere...and refractivity...in the stratosphere and troposphere” (e.g. abstract).

I suggest to generally use “troposphere and stratosphere” or “UTLS” etc. as first topic and “ionosphere” separately as a second topic; in my opinion this further improves logic and fluency of reading (especially readers being no RO experts will benefit from a clear topical order as they miss the intrinsic “setting occultation event” feel of why one might sometimes name ionosphere first).

Abstract:

All abbreviations to be spelled out. Maybe also make a bit more out of the current final sentence of the abstract, i.e., take the chance to point more specifically to the various applications addressed in the review (and point to them in the sequence they appear in the paper).

By the way should’nt “earth” be “Earth”? (i.e. used as a proper name).

p. 136, line 14:

after Ware et al. (1996) suggest to insert also Kursinski et al. (1996), since complementary first paper from JPL side

[see references list attached below, i.e., all additional refs suggested here are listed there]

p. 137, line 25:

suggest to insert also Steiner et al. (2001) after Lee et al. (2000), since this was also an overall-intro/review-type paper at that time.

p. 138, line 5:

here again the situation that ionosphere is named first; suggest the other order (see comment above) as will also be in line with the sequence of terms in Eq. (1).

p. 138, line 12:

suggest to avoid the terminology “ionosphere-free” (any processed signal is never entirely free of ionosphere...), e.g., use “ionosphere-corrected”, “ionosphere-corrected neutral atmosphere”, “neutral atmosphere” or the like.

p. 138, line 24:

suggest to insert also Healy et al. (2005), Healy and Thepaut (2006) before Chen et al. (2009) [possibly also one or two others non ECWMF Met Office...]

p. 139, line 10:

suggest to better use “global maps” rather than “global profiles” here (or any other improvement of the sentence that conveys the mapping character, still the vertical profiling character can be separately mentioned if seen relevant here)

p. 140, line 5:

suggest to delete “please”

p. 140, line 17-18:

please expand this sentence to become fully true; PLL “can only be used to track setting...” is too strict, since clearly above about 8 km also rising events may well be tracked by PLL, after a proper sequence of signal acquisition modes throughout the troposphere (this is, e.g., done by MetOp but also by COSMIC right)

p. 141, line 3:

suggest to better use “suitable integral transforms” or similar instead of “the integral transforms”

p. 141, line 17:

is it “Vaisala” instead of “Viasala”?

p. 141, line 18:

together with He et al. (2009) also Gobiet et al. (2007) should be mentioned here, who included validation results indicating < 0.2 K accuracy, e.g., “A similar result was found by He et al. (2009) and the result is also consistent with the validation study of Gobiet et al. (2007)” or the like.

p. 142, line 6:

suggest after this statement on precision to also briefly address the matter of inter-satellite consistency. It is somewhere noted but not explicitly addressed currently, but it is important for climate and calibration applications addressed later in section 6. Most brief way is perhaps one additional sentence citing Foelsche et al. (2011), a paper that is also part of the same AMT special issue (currently online as AMTD paper); this paper likely holds the most up-to-date status on inter-satellite consistency, including appropriate further refs.

p. 142, line 20:

suggest to complement Tsuda et al. (2000) by Steiner and Kirchengast (2000), which together have been the two first ones confirming real gravity waves in RO (using real ensembles of events, etc).

p. 143, line 7:

suggest to cite also Schmidt et al. (2008, 2010) here, and Borsche et al. (2007), as these also had relevant work to this end, i.e., “(Borsche et al., 2007; Schmidt et al., 2008, 2010; Steiner et al., 2009)”

p. 144, line 17:

suggest “essentially unaffected by clouds” or the like instead of “unaffected by clouds” (there is some residual liquid water effects on refractivity as we know)

p. 147, line 2 and line 5:

suggest “essential lack of bias” or similar instead of “lack of bias” (there is always some residual biases, ideally negligible in certain application contexts but still...)

p. 148, line 4:

unclear what “about 3% of the total number” refers to here. Which total number exactly (?) – please clarify.

p. 148, line 7-8:

suggest to add Steiner et al. (2001) and Foelsche et al (2008a) papers here, also important part of the chain of work establishing climate monitoring, i.e., “(Goody et al., 1998; Steiner et al., 2001; Leroy et al., 2006a,b; Mannucci et al., 2006; Foelsche et al., 2008a, 2009; Steiner et al., 2009).” [There was also a Schroeder et al. of 2003 or so from JPL side I recall, a GRL one on RO and MSU Schroeder, Leroy and DMI people...; this one would also fit at this point in the paper]

p. 148, line 11:

in order to reflect more broadly accepted requirements it seems better to say here “...0.5 K accuracy and better than 0.1 K/decade stability...”. The 0.04 K/decade is not a key value considered required throughout all of UTLS and it is also only one value in Ohring et al.s 2005; more broadly valid is < 0.1 K/decade as overall requirement. Additional suitable reference might be Luntama et al. (2008). But perhaps even better GCOS (2006) as also cited in Luntama et al. (2008) related to the < 0.1 K/decade stability requirement.

p. 148, line 17:

suggest to add a sentence here like “Similar results were found by Leroy et al. (2006a) and Foelsche et al. (2008b) for other retrieved parameters, including geopotential height of pressure levels as promising detection variable.”; so as to point also to this relevant work in the context.

p. 148, line 21:

at the recent FORMOSAT-3/COSMIC workshop in Taiwan a recent work in press was cited also, Lackner et al. (2011) [found listed on the Wegener Center publication webpage in 2011]. This evidently did a more complete detection already, especially in geopotential height. Thus suggest to add a sentence like “Lackner et al. (2011) recently found, based on the RO climate record until 2010, a climate change signal in the geopotential height of pressure levels in the UTLS, consistent with tropospheric warming underneath.”

p. 149, line 2:

suggest to add after “...demonstrating the stability of the RO observations.” also a brief sentence pointing to the fact that Foelsche et al. (2011) have meanwhile shown for a full three-year record that inter-satellite consistency of observations is within 0.03% for refractivity and 0.05 K for temperature, respectively. [see this AMTD paper submitted to the same special issue]

p. 149, line 15:

here it seems appropriate adding a sentence saying that Steiner et al. (2007) and Ladstaedter et al. (2011) [the latter also an AMT special issue paper currently found online as AMTD] have found statistically significant differences in stratospheric-channel brightness temperature anomaly trends of MSU(AMSU) and RO in 2001 to 2009. This also points to the problems of long-term stability in the radiometric record.

p. 150, line 21:

suggest to add here a sentence pointing to the advantage of RO to provide precipitable water estimates as well above land as above oceans, while the nadir-passive radiometers are strongly limited over land due to the difficulties with surface emissivities [in fact also Fig. 36 shows the AMSR-E data with voids over the continental land regions]

p. 150, line 26:

suggest to write more precisely “the structural uncertainty from GPS RO refractivity data...”, to more directly reflect what Ho et al. (2009c) worked out.

p. 151, lines 9-10:

suggest to more appropriately write “...retrieval of electron density profiles from RO data...”
As to the refs at this point, there is also an older paper by Leitinger et al. (1997) that already included electron density profile retrieval from RO data (using GPS/MET ionosphere data).

p. 152, line 14:

perhaps better write here “more than 100 km...” rather than “greater than 100 km...”

p. 155, line 28:

suggest to add here also the Gobiet and Kirchengast (2004) paper to the Kuo et al. (2004) paper as in particular the former discusses these matters in detail. And in the next sentence say “Gobiet et al. (2004), Kuo et al. (2004), and Gobiet et al. (2007) show that the inversion...” since here especially the latter paper clearly demonstrates and discusses the initialization dependence directly in a validation context, including pin-pointing the high-latitude polar winter vulnerability.

p. 156, line 23:

suggest to more informatively write something like “e.g., the series of MetOp satellites already started with MetOp-A, which is joined by the second satellite MetOp-B in 2012/13.”
Consider also to drop the term “single-satellite” in the line before (e.g., MetOp will be twin satellites from 2012/13 onwards).

p. 157, line 2:

suggest to slightly expand this final sentence to “positive impact on weather and space weather prediction, climate monitoring, and weather, climate, and ionospheric research.”

Table 2:

suggest to write in the third line “Profiles troposphere and stratosphere, and ionosphere” and a bit lower in the list “Self-calibration, no external calibration required” instead of “no calibration required”

Please consider/implement the above carefully. Overall thanks a lot for what will become an excellent review paper, ready to be cited on many occasions.

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