Interactive comment on "Information content in reflected signals during GPS Radio Occultation observations" by Josep M. Aparicio et al.

Anonymous Referee #2

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The paper presents an overview of the detection of reflected signals in radio occultation events and discusses the potential use of additional information that could be derived from such data. It is well readable, also covers some basics and appears complete, although the authors could sometimes be more concise. Some statements and claims are made that should be clarified or corrected, see my comments below.

To summarize, the paper is acceptable for publication after addressing the following issues.
- Page 2, line 20: "Also, at this very low elevation, and unlike at higher incidence angles, a reflection does not lead to a reversal of the polarization."
I think the description of reflection off an infinite surface by use of Maxwell's equations does not entail a dependence of the polarization from the incidence angle as claimed above. The authors need to either prove this claim for the geometry at hand or give some reference.

At normal incidence, reflection of a circularly polarized signal switches handedness (the EM field keeps rotating in the same orientation, but propagation reverts). Since GPS is right-hand circularly polarized, it will switch to left (again, at normal incidence). We add a reference to a section of Born & Wolf and Ulaby for reflection and polarization as a function of the incidence angle. We add later another (Zavorotny & Voronovich, 2000)

- Page 4, line 28: "GPS satellites carry atomic clocks, which produce pure sinusoidal tones. Before emission, these tones are modulated by characteristic bitstreams. ..." Unless I misunderstand things, the GPS carrier signal is primarily generated by some (highly stable) oscillators for the Lx frequencies, whose signal is modulated with information derived from the atomic clocks, plus ancillary data. There exist different technologies for atomic clocks (e.g. Cs or Rb based), whose details are of little interest as far as the emission, reception, and further processing of the carrier signal is concerned. Since this is also probably not relevant for this work, I recommend that the authors correct or remove this claim there and in other places of the manuscript.

Since the details are irrelevant, we simplify to "GPS satellites produce very pure sinusoidal tones. Before emission, these tones are modulated by characteristic bitstreams. ...". We still mention the bitstream, as the receiver cannot listen directly to the pure L-band tone. The received "tone" is built by demodulation.

- Page 6, line 27: "... radio occultation events with SVM value greater than 0.25 are identified as reflections. " The use of the term "SVM value" is probably some slang. A couple lines above the term "scalar" was used. Further down, "... SVM threshold." I would recommend to use a more coherent term for the output variable of the SVM.

Ok. We have parsed the manuscript to verify accuracy of the expressions. SVM (Support Vector Machine) is an algorithm. We have changed as appropriate to "value" when we refer to the output of this algorithm, and "threshold" for the value where we assume that a reflection has been observed.

- Page 8, line 35 to page 9, line 3: "These patterns, consistently associated with geographic and seasonal features, do not suggest a direct relationship with instrumental problems or performance. They may, at most, be linked to instrumental performance issues if these arise under certain geophysical conditions related to seasons or geography."
I am not sure what the authors wanted to tell here. Is there evidence for variations of instrumental performance, for fundamental instrumental limitations, or other? Please rephrase.

We have rephrased. We do not suspect that instruments vary in performance. Since there is a clear geographic and seasonal association (as opposed to random), of the presence/absence of a reflection signature in individual profiles, we state that it is unlikely that this presence/absence is due to some kind of unidentified instrumental issue. Therefore we attribute it to properties of the atmosphere and ocean.

- Page 9, line 7: "... The fields from the ECMWF ERA Interim analysis are used here to determine the weather state. The correlations presented in the following are all computed using monthly averages evaluated on cells of 10_x10_ over the oceans, with the reflection fraction compared against several environmental variables."
How useful is the use of monthly averages for determining the correlation of the fraction of reflections with several atmospheric variables, or quantities derived from the weather situation? For "slow" variables such as SST this is certainly fine. But given the large cross-correlation for some of the quantities given in table 3, one might be asking if using the actual situation instead of some average might lead to different results. Did
the authors investigate this?

Yes. With this volume of data, correlating the individual data (several million profiles) or clustered subsets (several thousand clusters) leads to very similar results. We comment now that.

- Page 9, line 32: "... must not be directly linked ..." Should "must not" rather read "cannot"?

Ok.

- Page 10, line 6: "As mentioned above, the significant sea wave height is nearly uncorrelated (r=0.04), although wind speed over sea has a moderate positive correlation (0.43). This was somewhat unexpected, as stronger winds correspond to rougher sea surfaces, which could seem to link to less chances of coherent reflections."
Not sure if it is relevant, but for measurements of wind speed over sea (e.g. scatterometry and altimetry), instead of SWH the correlation with smaller scale features of the sea surface such as capillary waves is exploited. These are important to the description of reflection and damping of (Ku/C band) signals at larger incidence angle.

Theoretical considerations (Page 9, L11) already indicated that at this incidence angle and in the L band, the sea surface is specular even for large waves (even more for capillary waves). That low correlation with SWH was not unexpected.
The interesting part with the wind is that it is a positive correlation (better specular reflection with wind), whereas standard scatterometry (closer to normal incidence!) is based on the opposite: a decrease in specular reflection and increase in diffuse scattering with wind. This must be a different physical link with wind (indeed, we later point to atmospheric mixing as the link).

- Page 11, line 15: "that the reflection flag, either a qualitative present/absent, or the quantitative SVM value, stems from the observation, and that the knowledge that an occultation is expected a priori to show lower OMB difference than an average occultation is already a supplemental NWP value to the standard profiles of direct non-reflected signals."
Did anybody already see positive impact from using that information? The paper by Healy says that this appears to be difficult in practice. So why is it "already a supplemental NWP value"?

We have rephrased this. No, it has not yet been tested in an NWP environment. The sentence is supposed to mean

1) that the SVM analysis is providing information (a modulation of the apriori error estimation for non-reflected) that is generally useful in NWP assimilation of any kind of data.
2) “already” because the paper presents a second reflection product: an extension of the bending angle profile, which is in addition to the SVM.

- Page 12, line 19: "The formal precision of the inversion ..." Can the authors please explain what they mean by "formal precision"?

We have rephrased. The inversion is a fit, and provides an error estimate of the fit (“formal precision”), which is a good measure of the precision of the result, although may not be a good estimate of the accuracy.

- Page 13, line 11: "This is a physically new phenomenon that must be included ..." I don’t think that reflection is a new phenomenon, it is just that it needs to be taken into account. Recommendation: discard "physically new".

We rephrased. It is different from what is involved in GNSS occultations (i.e. only refraction).

- Page 13, line 18: "Due to reflection, the direction of the ray suddenly changes at the surface, ..."
"suddenly"? Use a less prosaic description of trivial reflection.

Rephrased for a simpler sentence.

- Page 14, line 12: "Interestingly, the slope of the reflected profile is very sharp, when compared with the direct profile. ..."

When looking at eq.(3) and taking the derivative w.r.t. a, one has two contributions of different origin. The second term on the r.h.s is purely geometric and thus more or less trivial, while the first one involves a derivative of the kernel K of eq.(4) that would be quite interesting to see a discussion about.

We further discussed about the general behavior of the reflected bending, and this strong derivative. This derivative wrt a is largely dominated by the second term (geometric), as the kernel does not have a peak (large derivative) for reflected rays.

- Page 14, line 17: "but the fact of having reached the surface, ..."
It is actually an assumption here that reflection is off the (Earth's) surface, and not some reflecting layer, isn't it?

The observed Doppler of the "reflected" signals indicates in general that this layer is not very far from the surface. But we have added the comment, notably in the conclusion, that there is some possibility that some elevated layer causes the reflection in some cases, and that a user should verify if the apparent reflection is compatible with the surface. We consider this a case that is best handled through background check.

- Page 14, line 24: "For direct, non-reflected propagation paths, this dependency of the properties of the bending with respect to the atmosphere, presents sharp peaks at the respective tangent altitudes."
The kernel K has an (integrable) singularity, not just a "sharp peak". The following discussion on that page and also fig.11 is based on the naive assumption of some "sharp peak" and needs to be corrected.

We have rephrased for clarity.
We do not "correct" anything, nor find any problem with this integrable singularity. We mention the "peak" because the kernel of a direct path heavily concentrates the weight near the tangent point height, and is the basis for the high vertical resolution of radio occultations. This is not the case in a reflected path, where the distribution of weight vs altitude is much more uniform.

- Page 14, line 32: "The reflected kernels always include the entire atmosphere, and are always sensitive to the low troposphere. Among the direct paths, only a few are sensitive to the low troposphere, and some may be missing. The reflected kernels have therefore the ability to fill any section where direct data are not sufficiently sensitive." Is this "filling of missing sections" just some hope, or has it actually been done or shown? If not, remove or weaken the claim in the last sentence. Given the discussion in that paragraph and the first one on page 15, I recommend that strongly.

We add the word "potential". The example in Fig 12 does show that the reflected profile provides sensitivity to a ducting layer, in a simple case where the direct profile cannot. We specify in the conclusion that future work will be dedicated to practical use in a NWP context.

- Page 15, section 4.3: "Value of reflected data under superrefraction"
For the sake of completeness of the paper - and also for understanding this section – it would be good if the authors briefly explained superrefraction and ducting.

We have parsed the paper, as they had been used inaccurately. We add definitions for both.

- Page 15, line 17: "The additional refractivity is shaped as an error function, leading to a vertical gradient that may be strong. ..."
This needs to be explained better. From fig.12 I would expect that the vertical gradient is strongest near 2 km height, so the relation to surface refractivity is only through the property of the employed model (assumptions). The conclusions described there are not clear to me. What am I missing?

We have expanded this section. We explicit that this is a numeric experiment that demonstrates sensitivity of the reflected profile that fills a sensitivity gap of the direct profile.
The problem with a gap is not only a missing portion of the atmosphere. Between the gap and the surface, the direct profile may provide further information of the refractivity **gradient**, but only weak constraints on the refractivity (because a portion of the gradient is missing).
A reflected profile provides a closure with a **different** integral of the refractivity profile. This closure is not extremely good at any particular altitude, but being complete, adds the required constraint on the **refractivity below the gap.**

- Page 16, line 19: "Although the reflected section of the profile contains less independent information than the direct section, it contains a few unique capabilities. It can resolve voids in the direct profile, for instance in the event of loss of track."
Can one really resolve voids in the event of loss of track? Has this claim been demonstrated somewhere?

We have rephrased here (and in the paragraphs above).

1) Reflected data are observed at the same time as the direct scans the mid troposphere. If track is lost in the low troposphere, reflected data is a supplement. In the list of occultations, there are many events where the profile does not completely reach the surface, but a reflection is seen.
2) We are not trying to provide a refractivity profile with voids resolved. We are trying to produce useful constraints to the refractivity profile that are vertically complete and mostly involve the low troposphere, which is the weaknesses of the direct profiles.

Spelling:
- Page 18, line 11: "Marcquardt" -> Marquardt

Done

Running a spell checker over the entire text might find a couple typos.

Ok.