Atmos. Meas. Tech. Discuss., 4, C1123–C1131, 2011
www.atmos-meas-tech-discuss.net/4/C1123/2011/

Atmospheric
Measurement
Techniques
Discussions

Interactive comment on "The impact of large scale ionospheric structure on radio occultation retrievals" *by* A. J. Mannucci et al.

S. Syndergaard (Referee)

ssy@dmi.dk

Received and published: 22 July 2011

General comments:

This paper sets out to investigate the impact of higher order ionospheric residuals in radio occultation measurement of the neutral atmosphere from large-scale ionospheric structures, in particular the residual caused by dispersion (or ray path separation) of L1 and L2 GPS signals. Both the International Reference Ionosphere (IRI) and the Global Assimilative Ionospheric Model (GAIM) is used in simulations. The accumulated bending angle along a single ray is shown for a few simulated occultations, and residuals after ionospheric correction are also shown. The impact on retrievals are shown for a few cases, including a case during the 2003 'Halloween' ionospheric storms. Conclu-

sions about the importance of improvement of ionospheric correction and/or monitoring of ionospheric storm events for climate use of retrievals are made.

Although I find the subject important, I am sorry that I can not be very positive about this paper. I think major revision is necessary before the paper can be published in AMT.

I have concerns about the relevance of the bending angle analysis and the correctness of the retrieval results. I elaborate in the specific comments below and provide some suggestions for improvement. This would, however, require new simulations and retrievals. The use of GAIM to investigate the residual errors is interesting because it is capable of representing the anomalous electron densities experienced during ionospheric storm events. However, I'm missing to see a case with a pronounced E-layer, which might give the largest retrieval errors in the stratosphere. Only a few single results of retrievals are shown, and besides my concern about them, I find it unwise to base general conclusions on such few examples.

In my opinion, the paper could be better written and it could be better organized. There are unnecessary repetitions and sometimes contradictions. The descriptions of methods and results are imprecise.

Specific comments:

1) I believe it is incorrect to say that the refractive index gradient depends on the Faraday rotation effect (page 2528, line 16), and that the Faraday rotation introduces f^{-3} terms (page 2530, line 23; page 2533 line 7). As far as I understand, Faraday rotation is a result of the slightly different refractive indices for the ordinary and extraordinary wave modes in an anisotropic ionized medium. A linearly polarized wave can be considered the superposition of a right-hand and a left-hand circularly polarized wave, one corresponding to the ordinary mode and the other to the extraordinary mode. These two waves have slightly different phase velocities (and in general slightly different propagation directions), which, when the two wave fields are added together, gives rise

to Faraday rotation of the plane of polarization of the linearly polarized wave. However, the GPS signals are mainly right-hand circularly polarized, which gives rise to only one of the two modes. When there is only one wave mode, there is no Faraday rotation. See, e.g., Budden (The propagation of radio waves, Cambridge University Press, Cambridge, 1985) for a description of Faraday rotation. I notice that the cited paper by Vergados and Pagiatakis (2010) mentions the Faraday rotation in relation to GPS signals, and I guess this may be why Faraday rotation is mentioned in this paper. Unfortunately, I believe the paper by Vergados and Pagiatakis (2010) contains many misunderstandings and erroneous results, and in my opinion it should not be cited.

2) The paper deals only with large-scale ionospheric structure, small-scale variations are not considered. This is understandable, since ionospheric models do not contain small-scale variations. It is, however, well recognized (I think) that the small-scale variations in the real ionosphere, and the residual ionospheric noise in the dual frequency combination, is a larger problem for individual retrievals than the possible systematic bias caused by large-scale structures. I think the text in section 2 needs to be revised with this in mind. Could the small-scale variations introduce a bias in the processing? E.g., when bending angles are extrapolated above some upper altitude? Are they assumed to be a minor problem compared to the large-scale bias? In individual profiles? On average?

3) Page 2530, line 14: "Differences... are solely due to effects of the ionosphere". Well, because of the ray path separation, the rays also go through different parts of the neutral atmosphere, and this, I suppose, could give differences not directly related to the ionosphere. Maybe skip "solely" here.

4) Page 2531, line 23: "To reduce noise...". Which noise? On the L2 signal, I suppose. Please clarify.

5) Page 2532, lines 5-11: I am not sure it is correct that non-linearity in the relationship between bending angle and phase delay creates an ionospheric residual error

not accounted for in eq. (1) or (2). Are you sure of this? Does this come out of any theoretical work? In my own work (Syndergaard 2000), I did not find any residual in the formula for the bending angle correction that was not already present in the phase correction. When performing the dual frequency correction on bending angles at the same impact parameter, the main part of the ray path separation is accounted for, but a minor part still exists (see Syndergaard 2000, section 5). These two residual parts (which in Syndergaard (2000) are called the major and the minor dispersion terms in the phase correction) are both proportional to f^{-4}. A second order dispersion term in the phase correction is proportional to f^{-6}, but absent in the bending angle correction. So I agree that the non-linearity between the two correction approaches gives rise to different residuals, but it seems to be all in favor of the bending angle correction at a common impact parameter, where both the major dispersion term and the second order dispersion term are absent; Only the minor dispersion term is left.

6) Because of the above, the simulations in section 4.1-4.3 do not give a correct assessment of the residual bias, because in those sections the dual frequency combination is performed at the same time, whereas in the retrieval (section 4.4) it is performed at a common impact parameter. I am not convinced that the qualitative conclusions of the study does not depend on this difference (page 2532, lines 25-29). On page 2538, line 29, and page 2539, line 26, the residual bending at the receiver in the simulations in section 4.1 is taken as being relevant for the retrieval error. I don't think that can be concluded because the correction is made at equal times (although Fig. 8 caption refers to eq. (1), which contradicts the statement on page 2532 – I assume this is a mistake in the figure caption). Would it be possible to show bending angle residuals obtained using the ionospheric correction at a common impact parameter as it is calculated in the retrievals, and compare with the end-points of the curves in Figs. 6, 8, and 10?

7) The simulations in Syndergaard (2000) included an E-layer which seems to give larger residuals than from the F-layer. This is also the finding of a more recent paper

by Hoque and Jakowski (Higher order ionospheric propagation effects on GPS radio occultation signals, Adv. Space Res. 46, 162-173, 2010). Does GAIM represent E-layers? I only see a very small hint of an E-layer in the IRI profile in Fig. 15, and none in the GAIM profile. Would it be possible to include cases with a pronounced E-layer in the simulations? I would think an E-layer could give a significantly larger residual bias at 60 km when using the dual-frequency correction at equal times, but it might be less of an issue when using the correction at a common impact parameter. On page 2536, lines 2-3, it says that "We show in detail which part of the ionospheric electron density profile is cause for greatest raypath separation". To show that, a pronounced E-layer should be included.

8) The sentence "A dual frequency correction of the form Eq. (1) cannot account for this raypath separation. . . " (page 2533, line 5) is not quite true (cf my comments above), and seems to contradict the text on page 2531, line 13, where it says that the ". . . bending angle approach largely compensates for the separation of L1 and L2 raypaths. . . ".

9) The full Appleton-Hartree formula for the refractive index is used in the simulations (page 2533, lines 8-10). However, eq. (3) is only valid for an isotropic medium. When the Earth's magnetic field is present, the medium becomes anisotropic, and the ray equations are actually more complicated. The refractive index is no longer only a function of location, but also a function of the angle, θ, between the wave-normal and the magnetic field direction; one essentially has to take into account this dependence in the ray path equations (see, e.g., Budden (1985), Chapter 15). So there is a mismatch between the use of the full Appleton-Hartree formula and the use of eq. (3). This should be discussed and justified.

10) On page 2533, line 7, it says that "In this paper, we focus exclusively on ray path separation." Then why do the ray tracing simulations use the full Appleton-Hartree formula, including a model of the geomagnetic field? When performing the dual frequency combination, the residual error then stems partly from ray path separation and partly

from the uncorrected f^{-3} term. This needs to be clarified. The text on page 2538, line 20, indicates that there is no residual from the geomagnetic field (the f^{-3} term) in the simulations. This seems inconsistent with the statement that the full Appleton-Hartree formula is used. Please clarify.

11) A solution to both of the two issues above could be to not use the full Appleton-Hartree formula for the refractive index, but only the series expansion to first order.

12) I did not find any evidence in the results that symmetric horizontal structure about the tangent point leads to smaller errors than highly asymmetric structures (page 2534, line 7-9).

13) Figure 1 seems trivial and could be omitted.

14) Page 2536, line 12: "We use a representative ray-path from an actual occultation. . . ". I suppose the exact ray path depends on the electron density of the model. Do you mean ". . . representative satellite positions. . . "? Please clarify. A similar sentence appears on page 2537, line 13.

15) On page 2537, lines 22-23, positive and negative bending is mentioned, but not really well-defined. In Figs. 4 and 7, the bending is not counted with a sign, since it comes from 3D ray tracing (I suppose, but it is not clear). In line 27, the "bending angle increases rapidly again" in the bottom side of the ionosphere, which is true according to the figures, but inconsistent with the text a little earlier, where there is "negative bending in the bottom side" (meaning that the accumulated bending decreases there). On page 2538, line 7-8, it says "zero bending" and "complete cancellation". Is it complete in 3D? Please make sure there is consistency between figures and text. Be precise in descriptions. Clarify why bending is not counted with a sign in the figures.

16) Page 2540, line 11: The reference to Sydergaard (2000) (name misspelled), seems odd here. The comments about orbit altitude was very specific for the derivations in that paper.

17) Page 2542:, line 2: "…ionospheric residual decreases rapidly." Relatively to the total bending, I suppose. In absolute value it doesn't necessarily decrease. However, a rapid decrease, one way or the other, does not seem to match the results in Figs. 12, 14, and 15? See also comment 20 below.

18) The simulated retrieval results are based on an initialization of pressure at 40 km, which makes the temperature error zero (page 2542, line 7). But in real retrievals, the pressure at 40 km is unknown, and the temperature error will not be zero at 40 km. Thus, how can you be sure that the errors shown in this paper are not severely masked by the artefact that the temperature error is zero at 40 km? Surely you can't assess the influence of the residual ionospheric bias near 40 km. But how can you be sure it is okay at 30 km? Would it be possible to make the hydrostatic initialization at, say 60 km? Would it change the results at 30 km?

19) How is the initialization of the Abel transform done? How do you extrapolate bending angles above? Do you use any smoothing or statistical optimization at the highest altitudes? In my experience, a residual ionospheric bias (at least in individual profiles, but perhaps also in averages) may be masked by biases from statistical optimization and/or extrapolation of bending angles above the upper altitude of the measurements/simulations. These issues should be described and their possible influence on the results discussed.

20) Fig. 12: What causes the nearly constant bias of 0.2 K below 25 km? I would expect a nearly exponential decrease of the error with decreasing altitude if no other sources of error than the ionospheric residual bias are included in the simulations (page 2542, line 9). Same for Figs. 14 and 15. It seems that refractivity (extrapolating from Fig. 13) has a significant negative bias below 20 km (although results are not shown below 20 km, with the argument that below that altitude the ionospheric residual decreases rapidly, page 2542, lines 1-2). Is this a problem with the retrieval? How does the refractivity error look for the 'GAIM storm' case?

21) Page 2543, line 6: Numbers do not seem to match with Fig. 14.

22) It would be interesting to see the profile for 'GAIM storm' in Fig. 15. It would be relevant in connection with the sentence on page 2544, lines 4-5, referring to the anomalous gradients during a major storm.

23) Page 2544, line 16: "The analysis shows that details of the electron density distribution and orbit altitude are two major factors determining retrieval biases …". I disagree that the analysis shows that the orbit altitude is a major factor. What is shown in this paper is that the bending angle is larger and that the residual bias using the dual-frequency correction at equal times (in the few cases shown) is larger for a low orbit altitude. But in the retrieval the correction is made at a common impact parameter and it is not shown if orbit altitude matters in that case. Another issue that should not be forgotten here is that the refractive index at the orbit altitude is usually neglected in the formula relating Doppler and bending angle, because it is not generally known (e.g., eqs. (7.7)-(7.8) in Melbourne et al. (JPL Publication 94-18, 1994), and the footnote on page 42). How does this influence the residual bias in the retrieval for a low orbit? I don't disagree that a low orbit might give larger residual biases in the retrieval, I just don't think the issue is correctly addressed in this paper. Additional simulations are necessary to correctly investigate the influence of orbit altitude, e.g., simulations and retrieval of an occultation for a high orbit altitude, compared with the retrieval where the ray tracing from the same simulation is stopped at, say, 400 km. That would be an interesting comparison.

Technical corrections:

Page 2529, line 2: Discussion of results in both section 4 and 5?

Often "the" is missing in front of a noun. Please use correct syntax throughout the paper.

Equation (2): Is there a reason why the equation is written with $(C_1 - 1)$ instead of the more simple C_2?

Page 2534, line 24: “an another”? BTW, GAIM was already introduced in the previous paragraph, here it is introduced again.

Figure 1, caption: The plot of the ray starts at 2000 km, not 1600 km.

Page 2536, line 22: “...using GAIM updated...”. Something seems to be wrong in this sentence.

Page 2542, lines 23-26: Kursinski is misspelled a couple of times.

Page 2546, line 18: Typo: “ionospherc”.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 2525, 2011.