

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-246

Anonymous Referee #2

Referee comment on "Coupling a weather model directly to GNSS orbit determination – case studies with OpenIFS" by Angel Navarro Trastoy et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-246-RC2>, 2022

This paper deals with the precise orbit determination of GPS satellites. This is impeded by atmospheric refraction. The paper describes the traditional method to account for this, using a refraction field derived from weather data and mapping functions that allow to derive the necessary data at any viewing direction from the data at zenith direction. The authors then describe a new method using ray-tracing that circumvents the mapping functions. They test the new method, and show that it is not worse than the mapping method, with one exception. This exception is traced back to a simplification in the ray-tracing method, that can in principle be avoided, which is left for future work.

To my view, this is a rather good paper and it certainly should be published. In the following I make a few suggestions that may help to make the paper clearer, in particular to a reader without expertise in GNSS topics like me.

Abstract:

On first reading my impression was that the use of openIFS is the novel thing, but on a second reading, remembering what I have read before, my opinion changed. Now I believe that it is the use of the ray-tracing instead of the mapping functions that is the new aspect here. I suggest that this should be made clearer so that the reader gets it on the first reading.

I am also a bit surprised that the whole paper deals with a-priori estimates only. Perhaps it is my ignorance of this field, but why do you compare only a-priori estimates instead of final ones?

A minor issue was the notion "midnight discontinuity" which was puzzling on first reading.

Later it became clear. Please try to find a short but simple circumscription of what is meant so that the reader does not get lost already in the abstract.

Section 2.1:

It is astonishing to me that 30 sec of data per day are sufficient to compute the orbit for a complete 24 hours period. Is this, because it is essentially celestial mechanics (i.e. almost negligible perturbations) or is there a misunderstanding?

What do you mean with "pseudostochastic pulses are estimated"? Don't repeat the quoted paper here, but a few simple words for explanation would be fine.

I understand that you are computing a-priori delays, but in lines 99 ff you compare them with actual delays and the difference or discrepancy is determined (or estimated). To me it is unclear from where you have the actual delays and whether you need an a-priori estimate at all if you have the actual delay. Or is the a priori for orbit prediction and the actual positions can be measured after the event?

Section 3.2:

I wonder why the Deltas (to my understanding, the difference between the a-priori estimate and the actual delays) are compared for the two methods instead of the a-priori parameters directly. It should make no difference, of course, since the actual delay must be the same for both methods and must thus cancel out. But it is puzzling. I would expect instead two tests here: 1) the comparison of both methods using the a-priori parameters only and 2) the distribution of the deltas in the new version, to see whether the difference of the two methods is significant in comparison to the difference between model and measurement. While the first of these steps are given, the second is missing and should be given.

Section 4:

For the VMF3 method you use the operational weather forecast, and for the new method the openIFS. Your interest is not to demonstrate the differences arising from using one set of weather data that has millions of observations assimilated with another data set without data assimilation. Instead your interest is to compare the use of the ray-tracing method with the mapping method, if I understand it correctly. But of course, the atmospheric states should be different in the two weather data sets, and the one using the actual forecast should be more realistic. So it is surprising that this does not lead to a significantly better performance of the mapping method. I would like to see your comments on this issue.

