Geosci. Model Dev. Discuss., 7, C132–C138, 2014
www.geosci-model-dev-discuss.net/7/C132/2014/

Geoscientific Model Development Discussions

Interactive comment on "Simulations of direct and reflected waves trajectories for in situ GNSS-R experiments" *by* N. Roussel et al.

F. Nievinski (Referee)

fgnievinski@gmail.com

Received and published: 17 March 2014

GENERAL COMMENTS

The article is well written and illustrated, and tackles a topic of active development in the literature. On the other hand, I have contentions concerning the breadth versus depth in the subject coverage. The submission tries to tackle three separate issues in the simulation of GNSS reflection trajectories: - large-scale surface model - use of a detailed digital elevation model (DEM) - tropospheric refraction I believe any of these three topics individually would suffice, were it dealt with in a thorough and conclusive manner, which unfortunately does not seem to be the case.

Starting with the surface models, it fails to cover the simplest one, that of a planar hor-

izontal surface (is the curvature of the Earth significant at the lighthouse scenario?); the spherical model needs to distinguish between a geocentric sphere and an osculating sphere (also: how does the iterative procedure compares closed-form solutions reported in the literature?); the ellipsoidal model lacks further development towards a closed-form solution (after all, the ray/ellipsoid intersection has well-known solution in the computer-graphics literature); finally, there is little verification and validation reported here – authors could use the simpler models to check on the more complicated ones, forcing the latter to artificially degenerate into the former (e.g., an ellipsoid with equal major and minor axes, a sphere with near-infinite radius, etc.)

The use of the DEM consists of two main parts: visibility masking and surface slope variations. The first part seems reasonable and indeed is useful in the scenarios demonstrated; it does not seem to address, though, the issue of visibility of the satellite and of the receiver, both from the specular point (only the visibility of the satellite from the receiver). The second part is more contentious: I do not think the heuristics employed in its derivation (e.g., reflection assumed to occur along a planar cross section, Snell law being applied with no due consideration for the DEM resolution vis-a-vis the Fresnel zone area) should be trusted before they are proven correct upon comparison to a more rigorous formulation, such as geometrical-optics ray-tracing or physical-optics integration.

The treatment of tropospheric refraction must be disentangled. On the one hand, there is the angular or directional refraction, which changes the signal direction of arrival (primarily the elevation angle, secondarily also its azimuth). On the other hand, there is the refraction range or delay. It remains unclear the relative contribution of the two types with varying satellite direction – it'd seem that angular refraction is greatest near grazing incidence while ranging refraction seems greatest near normal incidence (considering reflected minus direct paths). The latter effect would need a zenith delay model, which is not normally part of a mapping as the AMF employed. Besides these main issues, there are secondary ones, such as the need for a bulky numerical weather model vs.

a leaner climatology, and whether or not azimuthal asymmetries are significant. These issues are all touched in the article though only in an inconclusive manner.

For the above-mentioned reasons, I find the coverage of the subject to be too broad at the risk of being shallow; I'd prefer to see a narrower scope and deeper treatment. May I suggest authors focus on the surface model part, as it the one requiring the least modifications to produce an acceptable article.

There is an opportunity for the authors to offer guidelines to fellow scientists concerning when it is no longer acceptable to employ the simplified models. Yet, to reap these benefits, the reporting of results also should be improved. In addition to the observation conditions (essentially satellite elevation angle and receiver height above the surface), also the reflection characteristics need clarification: instead of a combined three-dimensional position, please report vertical position separately from horizontal position (Cartesian or geodesic arc-length), as well as slant distance or propagation range. It'd be useful to emphasize whether these systematic errors translate into, e.g., over- or under-estimated reflector height, etc.

SPECIFIC COMMENTS

Here I highlight some of the intermediary-level issues; please see comments on body of the text for details.

Authors need to spell out upfront and use consistently the various vertical position and direction coordinates utilized, amongst which: receiver height (above reflecting surface), satellite elevation (angle w.r.t. horizon), and altitudes (ellipsoidal and orthometic). Unqualified usage (e.g., "elevation" by itself) is confusing. Also the grazing angle, w.r.t. the surface tangent, needs to be introduced for non-horizontal surfaces, as a generalization of the elevation angle.

The original SRTM was provided as orthometric heights w.r.t. the older EMG96 geoid. Using geoidal undulations from the newer EGM08 would be inconsistent with the way

SRTM was generated. Please check.

The Gaussian radius of curvature should be preferred over the meridional radius of curvature used.

I believe the spherical approximation is osculating rather than geocentric as stated in the text; this is a consequence of the type of elevation angle employed, whose complement is reckoned from the ellipsoidal normal direction rather than the geocentric radial direction.

Glistening zone and Fresnel zone are confounded in the text.

Formulas given for first Fresnel zone are not valid for near-surface receivers.

TECHNICAL CORRECTIONS Please see annotated PDF.

REVIEW QUESTIONS

- Does the paper address relevant scientific modelling questions within the scope of GMD? Does the paper present a model, advances in modelling science or a modelling protocol that is suitable for addressing relevant scientific questions within the scope of EGU? Yes.

- Does the paper present novel concepts, ideas, tools, or data? Partially.

- Does the paper represent a sufficiently substantial advance in modelling science? Not in present form.

- Are the methods and assumptions valid and clearly outlined? No.

- Are the results sufficient to support the interpretations and conclusions? Yes.

- Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model

development papers should be similarly reproducible. For MIP and benchmarking papers it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible. Mostly yes.

- Do the authors give proper credit to related work and clearly indicate their own new/original contribution? No; but I take their lapse on good faith that they were unaware of relevant work; see below for details.

- Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model. Mostly; the expression "in situ" needs to be replaced by "ground-based" or "near-surface"; and the models compared could be mentioned as well (e.g., "surface shape").

- Does the abstract provide a concise and complete summary? Yes, perhaps with unnecessary details.

- Is the overall presentation well structured and clear? Yes.

- Is the language fluent and precise? Only minor English mistakes, as noted in the body of the article.

- Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

- Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Figures are excessive; at least Fig. 12, 11, 5, 3b could be discarded. On the other hand, Fig 5 and 6 are clear and concise, yet could be further improved by means of a gridded image, whose bi-dimensional domain represents the independent variables and whose image color represents the dependent variable. Six tables are also too many and do not seem to be cited at all in the text. Figure captions should be made self-contained, so that it is understood without having to consult the body of the article; also information printed on top of the images ought to be moved to

the bottom caption.

- Are the number and quality of references appropriate? No; there's abundance of references not directly relevant and lack of references directly relevant.

More specifically, the bulk of the references cited in the Introduction section could be replaced by a citation to one or two recent review publications, e.g.: Jin, Cardellach, and Xie (2014) GNSS Remote Sensing - Theory, Methods and Applications. Springer. Gleason, Lowe, and Zavorotny (2009) Remote sensing using bistatic GNSS reflections, In: Gleason and Gebre-Egziabher, GNSS Applications and Methods, 399–436, 2009.

Some missing references directly relevant are: Kostelecky, KlokocnÄśk, and Wagner (2005), Geometry and accuracy of reflecting points in bistatic satellite altimetry, J Geod. Semmling (2011), Altimetric Monitoring of Disko Bay using Interferometric GNSS Observations on L1 and L2, PhD Diss. doi:10.2312/GFZ.b103-12049

Finally, the following are some of my own articles which I only cite because they provide subsidies for some of the comments made in the body of the article: Larson and Nievinski (2013), GPS snow sensing: results from the EarthScope Plate Boundary Observatory, GPS Solut. Nievinski and Santos (2010), Ray-tracing options to mitigate the neutral atmosphere delay in GPS, Geomatica.

- Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged. N/A

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C132/2014/gmdd-7-C132-2014-supplement.pdf