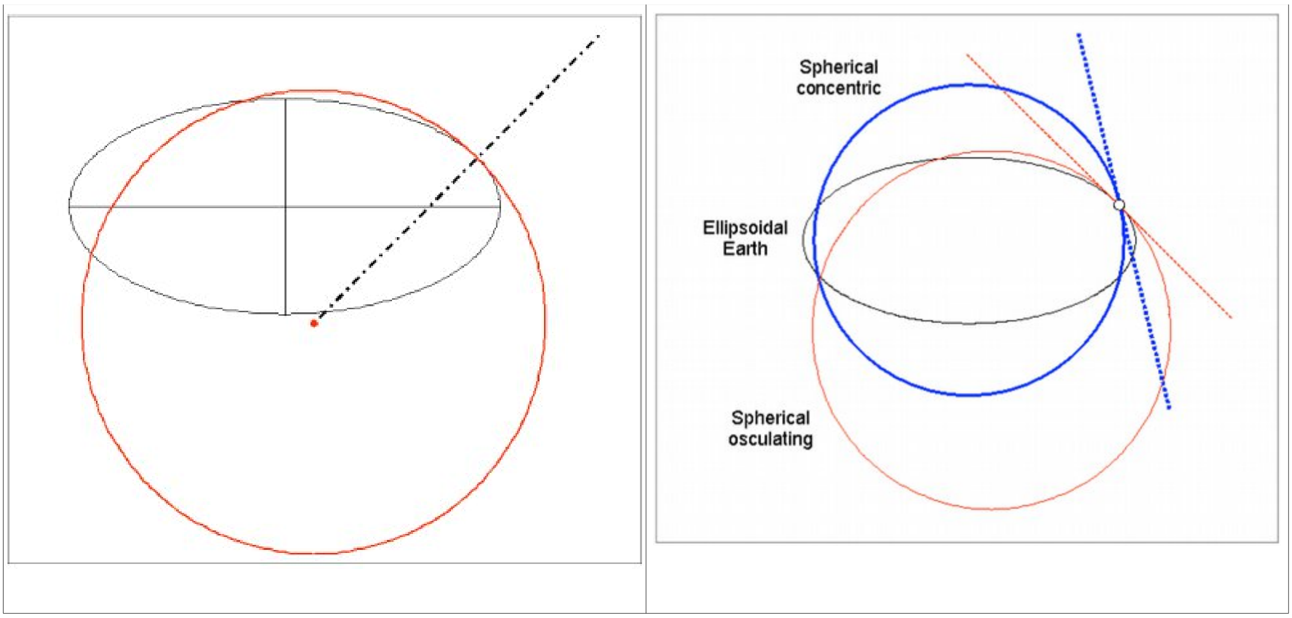
MAIN ISSUES

The effort demonstrated by the authors in preparing this revised version is highly appreciated. More specifically, the inclusion of the planar horizontal surface model greatly enriched the manuscript. Yet many of the problems indicated in the original review persist, thus major revisions remain necessary.

The main problem lies in their usage of the spherical model, which serves as the basis for comparison of all other surface models. I'm afraid it is not the ideal osculating type, as it is claimed. Evidence for the error can be found in numerous places: in sec. 3.2, Local sphere reflection approximation, it is stated that:

> *Let us consider the vertical plane formed by the transmitter (GNSS) satellite (T), the receiver (R) and O, the centre of the Earth (figure 4). We assume that the specular reflection point (S) will be included in that plane.*

Furthermore, Fig. 2 places the sphere centered at the origin of the WGS84 Cartesian system; Tab. 2-6 show a non-zero minimum difference between spherical and ellipsoidal surface models – between the two models, reflection points should coincide exactly for a satellite at zenith (equivalently, the height of the antenna above the surface is supposed to be exactly the same at nadir); Tab. 2-6 also show difference between planar and spherical surfaces greater than that between sphere and ellipsoid (this is unexpected, given the very small Earth eccentricity). As for the correction, it must be kept in mind that it is not enough to adopt the ellipsoid Gaussian radius of curvature for the radius of a geocentric sphere – the sphere center must also be displaced with respect to the ellipsoid center; the spherical radial direction shall coincide with the ellipsoidal normal; the center of the osculating sphere is to be inserted at an ellipsoidal height equal to the negative value of the Gaussian radius of curvature.



Another persisting problem is the incomplete treatment of tropospheric refraction. Between angular refraction and ranging refraction, authors dismiss the latter on the basis that:

> *As the baseline between the two receivers is short (a few centimeters to a few tenth of centimeters), and in the case of low altitude of the receivers, both tropospheric and ionospheric effects are neglected due to the spatial resolution of the current atmospheric and ionospheric models.*

The two arguments are unconvincing. First, the relevant baseline difference is not between up and down receivers, but between direct and reflected paths, where the latter includes up to 600 m two-way propagation through high-pressure air, in the case given of a receiver 300 m above the surface; a simple calculation yields a zenith hydrostatic delay (ZHD) of ~ 2.3 m and a ZHD difference of

15 cm. Second, even the simplest atmospheric model, based on a single vertical profile with no horizontal grid, would already show the significance of this error; in fact, several authors have found it necessary and accounted for it, see Anderson (2000), Treuhaft et al. (2001), and references in DOI:10.1007/s10291-014-0370-z Instead of removing the angular refraction or including ranging refraction, the issue could be settled if authors carefully delineate the scope of their study, by mentioning that they are interested in the reflection point position, and that the reflected-minus-direct range (which includes both geometrical distance and the ranging refraction) is left as future work. And that the occurrences of “tropospheric” (as in effects/corrections) be qualified with or replaced for “angular refraction”.

The number of figures remains excessive: it was 20, only 4 have been removed; ideally it should be aimed at 10. The tables also could be summarized for the benefit of the reader. See below for suggestions.

Finally, at closer scrutiny, I find the assessment of the ocean tide influence to distract from the surface model comparisons, the latter being a truly scientific contribution, whereas the former is more of a software usage illustration. In fact, the original version of the manuscript was slightly misleading in that it called the usage of tide gauge data as a validation of the simulations, as if it involved an independent comparison against external measurements, which is not the case. Considering that the article already contains more than sufficient material for publication in terms of algorithm comparisons, and that it could benefit from a greater focus, I'd recommend discarding that section. Similar for the more technical section 2.3 Simulator outputs and Calculation time (unnumbered) which would soon become out-of-date.

SECONDARY ISSUES

- Figure suggestions:
  Fig 1 and Fig. 5 are mostly redundant given the presence of Fig. 3
  Fig 4 contains too much information for the reader to grasp
  Fig 9 is irrelevant in my opinion
  Fig 10 and 11b could be combined
  Fig 11a can be shown as a small inset in Fig 11b
  Fig 12, 13, 14 need to be cropped at the top as the title is supposed to be in the textual caption not overlaid on the image
  Fig 15 and 15b could be combined showing a shorter time space (fewer reflections)
  Fig 15b could show fewer rays; currently it more impresses than instructs
  Fig 16 waste 80% of the horizontal axis space; either or both use a vertical log scale or a horizontal scale linear in sin(e)
  Fig. 7 can be replaced by a formula relating output bending angle to input elevation angle, which incidentally would make it easier for readers to reuse this result
  Fig 14a: red and orange are too similar; pick red and green, or red and blue, or even different marker symbols

- One of the main contributions is the quantification of how the reflection point differs with varying elevation angle for each pair of Earth surface models, so this result would deserve to be shown in a figure of its own, a hybrid of Fig. 12 and Fig. 16, which are good examples.

- Fig 14 contains superfluous information: I'd remove the mean elevation angle line and respective right-hand axis as it is distracting from the main message and it can be adequately summarized in the caption by saying that it has negligible changes. In contrast, the green line which is the most important information is almost invisible behind the numerous dots; I'd make it much thicker and placed in the foreground. Finally, would you please explain why the distance is sometimes correlated with the tides and other times it is anti-correlated (their arithmetic signs are not always the same). I'd crop the horizontal axis to 0-24 h.

- Tables should be more succinct:

The first three rows (Vertical visibility mask, Horizontal visibility mask, Receiver height) should be discarded and their information incorporated into the caption.

The row "distance with respect to receiver" (incline or planimetric distance?) has half of its columns duplicated and, in fact, doesn't vary much, so it could be replaced for a single column.

The row titled "propagation difference" could be removed entirely, as it does not seem to be commented in the body of the article, is less informative than its decomposition in planimetric and altimetric components already given in the same table, and actually refers to just the geometric distance (not the full propagation range, given the neglect of ranging refraction).

The planimetric/altimetric separation is very informative, but that row needs not to report two sets of results based on separate calculation methods (cartesian WGS84 and geodesic arc-length) – I'd find that planimetric geodesic arc-length and altimetric ellipsoidal height differences would be ideal (also, I do not understand how the "altimetric geodesic arc-length" can even be defined.)

The mean and standard deviation are hard to interpret, because they depend on how the satellite elevation angle is sampled – it can be intentionally or inadvertently distorted by sampling more densely near zenith or near the horizon. These statistics would become unnecessary assuming the proposed new figure (hybrid of Fig. 12 and Fig. 16) is prepared.

Finally, as the maximum value is the most relevant number in these tables, hopefully authors can find a way of presenting them in a single or a couple of unified tables, in a way that the reader can compare and contrast results without having to sort through half a dozen tables.

- The comparison should be incremental: plane vs. sphere, sphere vs. ellipsoid, ellipsoid vs. DEM – not sphere vs. DEM.

MINOR ISSUES

- Eq.(5) needs correction; it has units of m^2; it should yield units of m.

- Trigonometric functions should be typed as \sin and \cos so that the font is upright.

- The following two sentences seem in conflict:

> *So it is absolutely mandatory to convert the altitudes of the DEM grid points into ellipsoidal heights by adding the geoid undulation. To do so, a global grid from the EGM96 geoid undulation model with respect to the WGS84 ellipsoid was removed from SRTM DEM grid points.*

To achieve $h = H + N$, one would have to restore, rather than remove, the geoidal undulation N.

- Please clarify the adjustment in "*ellipsoid adjusted to the position of the receiver*".

- the reference Nievinski (2009) should be replaced for Nievinski and Santos (2010):

> Nievinski, F. G. and M. C. Santos (2010) "Ray-tracing options to mitigate the neutral atmosphere delay in GPS." *Geomatica*, Vol. 64, No. 2, pp. 191-207. Available at: <http://bit.ly/1jh4sas>

Also, a reference cannot be listed without a citation in the body of the text (near osculating sphere preferably).

- section titled “Comparison between algorithms” should mention surface shape or models

- sec. 4.3, “Simulator outputs” should be “Type of simulator outputs” as no actual outputs are presented there.

- suggestion for future work: for reflections off of the ocean surface, is the difference between the geoid and and ellipsoid significant?