**Response to Anonymous review of the paper "Improved model for correcting the ionospheric impact on bending angle in radio occultation measurements" by M. Angling, S. Elvidge and S. Healy.**

Responses are included in line below in red.

The authors consider residual error of the standard ionospheric correction, linear combination of L1 and L2 bending angles, related to ray separation at L1 and L2 GPS frequencies. In previous publications, this second order effect was approximated by squared L1-L2 bending angle with the coefficient. In the reviewed paper, the authors come up with the global model of the fitting coefficient and demonstrate that such model results in more effective reduction of the residual ionospheric error than constant coefficient. The results may be useful for climate applications of the GPS RO. I recommend publishing the paper after revision with account for comments below.

The authors thank the reviewer for her helpful comments and her recommendation to publish.

In this study, the authors: (i) assume local spherical symmetry of electron density; (ii) neglect higher order terms in the Appleton-Hartree equation. It may be useful to introduce these approximations at the beginning of the paper (currently (i) is mentioned in the last sentence of conclusions, while (ii) is not mentioned). Also, it may be useful to include reference to the paper by Hardy et al. (this paper may be available from different sources, see information at the end of the review). The paper by Hardy et al. also includes references to earlier publications on the second order ionospheric effects.

Agreed.
Reference to Hardy has been included. The following sentences have also been added to section 2:

"Horizontal gradients will result in residual errors in the inversion. However, it is expected that these errors are random; therefore, they should not affect monthly or seasonal climatologies."

"The first order approximation neglects terms involving higher powers of the frequency and the earth's magnetic field; however these have little effect on the residual bending angle errors (Syndergaard 2000)."

p.1, lines 13-14; p.5, lines 13-14: "*The main area of interest for k estimation is between 40 and 80 km. It is in this region where the residual error from the ionospheric correction is likely to be a major contributor to the overall error budget of neutral atmosphere retrievals*."

First, why 40-80 km is the region of interest? I believe that for weather and climate applications, GPS RO may be somewhat useful at 40 km but it is totally useless at 80 km. An explanation or reference is needed. Second, "likely" means the authors are not sure that large-scale ionospheric residual is the major error contributor. An explanation or reference would be helpful.

A more detailed description of the useful vertical limits of k has been included in section 3.2

p.1, line 13: "*As expected, the residual bending angle is well correlated (negatively) with the vertical TEC. However, k is more strongly dependent on the solar zenith angle*." In the context of the first sentence, the second sentence is not clear. In the approximation used by the authors, k depends only on electron density. The electron density, in turn, depends on the solar zenith angle. Thus k depends on the solar zenith angle through the electron density, and the expression "more strongly dependent" is not clear, unless it is explained "more strongly than what (?)". Also, see comment to p.5, line 24.

We agree that this was unclear. The point is that whilst the residual bending angle error is strongly related to the TEC, the k is more strongly related to the solar zenith angle. This indicates that the TEC dependent component of the residual error is largely modelled effectively by the squared L1/L2 bending angle difference term in the correction. Thus, the k term is capturing other features such as the hmF2 variability. We have updated the text to make this clearer.

p.1, lines 16-17: "*The global mean error (i.e. bias) and the standard deviation of the residual errors are reduced to -2.2*x*10*$^{-10}$ *rad and 2.0*x*10*$^{-9}$ *rad respectively*." First, the number to which something is reduced requires the number from which that something is reduced. Second, it is not clear from the context, whether the reduction is relative to k=0 or k=const? The abstract should be self-explanatory. Also, see comment to p.7, line 12.

Agreed. Text has been amended to compare results of the uncorrected case to the model k case

p.2, line 17: "... *simple of implement* ..." It
should be "simple to implement".

Agreed. Text amended

p.2, line 22: "*Examples of now k varies with height* ..." It
should be "how" instead of "now".

Agreed. Text amended

p.3, line 1: "$r_t$" is introduced but never used. Is it needed?

This is the lower limit in the equation 1 integral, so should be defined.

p.3, lines 14-15: "... *bending angle error ... which increases as a function of the electron density squared, integrated over the vertical profile*." This sentence is not clear in several respects.
First, it is said "increases", but not said with what parameter? "Increases as a function" does not make sense (function may increase or decrease).
Second, "integration over" is commonly used with respect to domain (e.g., over height interval). Integration "over the profile" is not a common expression.
Third, if the authors mean equation (22) from VK94, it is more complicated than just integrated squared electron density; it includes derivative and kernel. This sentence should be made clear and reference provided.

We agree that this was unclear. We have amended the text to read:
"One downside is that a systematic bending angle error remains (see equation 5 of Healy & Culverwell 2015). The bending angle error has a dependence on the electron density squared, which indicates that it will vary with the solar cycle. This has been recognised as a potential source of bias in climatology products (Danzer et al. 2013)."

p.3, line 24: "... *as a function of ... time* ..." Logically,
it should be "local time".

Agreed. Text amended

p.4, line 2: "*A month median* ..." It
should be "A monthly median".

Agreed. Text amended

p.4, line 10: "PRIME" and "COST 238" should be explained.

Agreed. Text amended.

p.4, line 12: "... *current version NeQuick* ..." It
should be "current version of NeQuick".

Agreed. Text amended

p.4, line 13: "... *Galileo GNSS system* ..."
In the "GNSS", the last "S" already stands for "system". The expression above should be corrected and "GNSS" explained.

Agreed. Text amended

p.5, line 16: "*Example height dependence*" It
should be "Example of height dependence"

Modified to “Height dependence” for consistency with the following sections

p.5, line 19: "... *k is approximately linear* ..." Linear
with what parameter?

Tangent height. Text amended

p.5, line 24: "... k *appears to be more strongly dependent* ..."
More strongly than what? Also, see comment to p.1, line 13.

We agree that this was unclear. The point is that whilst the residual bending angle error is strongly related to the TEC, the k is more strongly related to the solar zenith angle. This indicates that the TEC dependent component of the residual error is largely modelled effectively by the squared L1/L2 bending angle difference term in the correction. Thus, the k term is capturing other features such as the hmF2 variability. We have updated the text to make this clearer:
“However, $\kappa$ is more strongly dependent on the solar zenith angle, indicating that the TEC dependent component of the residual error is largely modelled by the squared L1/L2 bending angle difference term in the correction, and that $\kappa$ is modelling other features such as changes in hmF2.”

p.5, lines 30-32: What is the physical sense of the statement that dynamic range of k is smaller than of F10.7? What are practical conclusions from this statement? This should be explained, otherwise I don't see why is this statement needed.

This is similar to the previous point. The reduction in dynamic range indicates that k is only weakly dependent on the electron density changes associated with the change in f10.7 through the solar cycle. We have added an explanatory sentence referring back to the previous section.
“This, again, is indicative of the TEC dependent component of the residual error being largely modelled by the squared L1/L2 bending angle difference term in the correction.”

p.6, line 11: "*In order the build the models...*" It
should be "in order to build the models".

Agreed. Text amended.

p.6, lines 20-25: It may be better to introduce "chi" here (rather than after equation 8) because "chi" is used in discussion of figures 11-13.

We do not think the symbol $\chi$ needs to be introduced until equation 8. However, to avoid any confusion, we have included the symbol in the captions of Figures 11-13.

p.6, lines 11-13; p.7, line 7: What is the reason for using two different sets, generated with the same "random drivers", for building and testing the k-model? I assume that the sets are statistically representative and the results are statistically significant. Will the results be substantially different with the use of one set for building and testing?

The aim is simply to have a statistically similar, but independent set of data for testing the models. Given the size of the test sets and the low complexity of the proposed model, we do not expect the model parameters to vary significantly if determined from one set or the other.

p.7, line 12: "... *residual errors are reduced to* $2.2 \times 10^{-10}$ *rad and* $2.0 \times 10^{-9}$ *rad respectively*."
The number to which something is reduced requires the number from which that something is reduced. Also, see comment to p.1, lines 16-17.

Agreed. Text has been amended to compare results of the uncorrected case to the model k case

Figure 9: Axes labels on all figures, except Figure 9, have units in parentheses. Thus "k (impact parameter 60 km)" is confusing. It should be "k (1/rad)". The title above the figure can be changed to "k value at 60 km above London".

Agreed. Figure has been amended.

Figure 16: I think, "left" and "right" are mixed up in the caption. Left is full histogram, while right is zoomed to highlight tails.

Agreed. Text amended.