We would like to thank referee #2 for the thorough review of our paper and his/her constructive comments. We have answered all comments below (for easier comparison the referee comments are included in *italic*).

#1: Firstly, I believe there is a lot of material in the original Vorobev and Krasilnikova (1994) paper that could be related to the main results given here. They discussed the origin of the residual ionospheric errors in their approach, provided integral expression for it, and tested it in simulation. Their expression shows that the residual error is proportional to the electron density, and it would seem that the variation of with solar cycle etc. shown here follows naturally from that insight.

#1: We agree that there is a lot of material in the original article by Vorobev and Krasilnikova, 1994. Hence, we added further citations and discussions in our manuscript:

On page 1984 line 6:

"It is also possible to write an ionospheric correction as a correction of bending angles (Vorobev and Krasilnikova, 1994). This correction is not limited to a spherically symmetric ionosphere and it has the further advantage that it does not assume identical ray paths."

On page 1984 line 14:

"Nonetheless, Eqs. (4) and (5) are still approximations, which neglect higher order terms and do not address small-scale structures of the ionosphere. Vorobev and Krasilnikova (1994) performed a 1D simulation study of the ionosphere, providing an estimate for the residual error of Eq. (5). This error depends on the vertical electron concentration and its gradient and increases when the ionospheric lower boundary goes down. This captures exactly the difference of day to night time ionospheric conditions, where the electron density increases and the ionospheric boundaries expand during day time."

And on page 1991, line 16:

"Our results are consistent with Vorobev and Krasilnikova (1994). They studied the error of the atmospheric refractive angle under different ionospheric conditions, investigating the influence of the height and thickness of ionospheric layers. They found that the atmospheric refractive recovery error (i) increases with decreasing height of the electron concentration maximum, and (ii) increases with increasingly thick ionospheric layers. Our analysis confirmed their results by showing that the residual bias depends on the diurnal, solar and seasonal cycles.

#2: I would also suggest that the magnitude of the residual errors (up to ~-0.4 microradians) could be estimated from a 1D calculation assuming a Chapman layer ionosphere, with appropriate time varying peak electron density. Are the more complex simulations presented here adding more insight?

#2: In our study we use the NeUoG model. With its 3D simulation it provides a more realistic electron density distribution than the 1D simulation of Vorobev and Krasilnikova (1994), see Fig. 2 in our manuscript. Hence a more reliable estimation of the ionospheric residual is possible. The NeUoG model has been successfully used in many studies such as by Gobiet and Kirchengast, 2004 and recently by Liu et al., revised manuscript in Adv. Space Res., 2013.

#3: It would also be useful to put the magnitude of the residual ionospheric errors in some context. The largest errors at solar maximum considitions is or order ~0.4 microradians. Ringer and Healy (2008) (Monitoring twenty-first century climate us-ing GPS radio occultation bending) angles, Geophys. Res. Lett., 35, L05708, doi:10.1029/2007GL032462) suggested that climate trends in bending angle space might be ~ 0.5-1.0 microradians per year near 20 km in the tropics, where the signal is large (see their table 1). Further, the bending angles values at 20 km are typically 1700 micoradians. When viewed in this context, the residual ionospheric error does not appear particularly problematic. Please discuss this and the noise amplification in the dry retrieval, noted in the specific comments.

#3: Thank you very much for your constructive comment. We will add a paragraph in the discussion section in order to put the results in a climatological context:

We will write on page 1996, line 10:

“Thus the average day time bending angles under solar maximum conditions are approximately 0.35 μrad smaller than during low solar activity. When studying short-term atmospheric trends over an unfavorable time interval (i.e., from low to high solar activity or vice versa) the residual ionospheric error could lead to a trend, which could wrongly be interpreted as an anthropogenically induced atmospheric trend. Since absolute bending angle values decrease exponentially with altitude, the importance of this residual error increases with altitude. Ringer and Healy (2008) studied projected bending angle trends based on a climate model run. At an impact altitude of 26 km, e.g., they report a positive trend of about 4 µrad per decade. The trend decreases to about 1.2 µrad per decade, at an impact height of 30 km, see Fig. 1 in Ringer and Healy (2008). Depending on the altitude considered, the residual ionospheric error could therefore range from a few percent up to an important fraction of a short-term bending angle trend. “

#4: Page 1982, Line 23. "The second order term ... shows almost no influence to a changing solar activity." Some clarification is required here, because elsewhere in the paper ignoring higher order terms is seen as a limitation. EG, last line page 1983. Is ignoring higher order terms acceptable or not? Please be clear throughout the paper.

#4: We thank the referee for his or her comment. We see that this sentence leads to confusions. In principle the magnitude of the residual ionospheric error is small, but it can still be a considerable error in climatological applications at higher altitudes (see also comment #2).

Actually, in the comments of referee 1, we had a similar remark regarding this statement. We repeat the answer here: According to Melbourne et al., 1994, there is a splitting term, second-order term, third-order term and a bending term, which act as residual error sources after applying a linear combination which removes the $1/f^2$ ionospheric terms. Furthermore the authors write that for a year near solar maximum, the day time second-order term results in a propagation delay residual between 0 to 20mm and for the night time between 0 to 3 mm, while the third-order term is in the region of sub-millimeter and can be ignored. However, since we did only discuss the second-order term in our manuscript, we decided to remove the sentence completely on page 4.

#5: Page 1984, equation 5. I think it should be noted that Vorobev and Krasilnikova (1994) actually provide an integral expression for the residual error in their correction (their equation 22), and it depends on the electon density. The error arises because of the assumption that the refractive index is unity in the denominator of the bending angle integral. It will arise even in the simplest case of a spherically symmetric plasma, neglecting the earth’s magnetic field.

#5: Thank you for bringing this to our attention. We decided to add a further citation on page 1984, line 14, see answer #1.

#6: It would be useful to see how the magnitude of their error estimate compares with the residual errors presented in this paper, given similar peak electron densities. I believe their error term gives ~-0.3 microradians near 60 km for solar-max, day time conditions.

#6: Vorobev and Krasilnikova (1994) studied the dependence of the recovery error for the atmospheric refractive angle on the height of the electronic concentration maximum (*Z*) and the thickness of the ionospheric layer (*H*). Looking at their Figure 4 and comparing their curve 3 (*Z*=200 km and *H*=75 km) to curve 2 (*Z*=300 km and *H*=75 km) confirms, that the recovery error decreases when the electronic concentration maximum height increases. The latter ionospheric state (curve 2) represents night time conditions where only the F2 layer is preserved. In that case Vorobev and Krasilnikova find an error smaller than 0.05 µrad at 60 km altitude. For the lower height of the electron density maximum (curve 3) the error is larger than 0.2 µrad at the same altitude, which is comparable to daytime conditions where also E and F1 layer maxima are present. Those two values are in good agreement with our results, keeping in mind that the residual error shows a dependence on solar activity, studied time frame, geographic latitude and also orbital satellite height, which differ between Vorobev and Krasilnikova and our study.

#7: It is also worth noting that Vorobev and Krasilnikova (1994) claim that their method is adequate whether the ionosphere is spherically symmetric or not (Their paper Page 608, paragraph starting "Note also ..."

#7: We will add a comment on page 1984, line 6, as formulated in answer #1.

#8: Page 1986. Generating the simulated data. I'm not clear whether the magnetic field term in equation 2 is included when the data is simulated. Please clarify.

#8: The magnetic field term according to Eq. (2) is not included in the simulations. However, a collaboration between the Key Laboratory for Land Environment and Disaster Monitoring of SBSM in China, the SPACE Research Centre in Australia and the Wegener Center in Austria performed sensitivity tests with the NeUoG model (G. Kirchengast, Wegener Center, personal communication, 2013). They studied to which order the ionospheric refractive index needs to be considered. For that they compared a modified version of the NeUoG model which included the geomagnetic term of 2nd order to the old version without the field term (1st order approximation). Their results showed no essential effects on the bending angle residuals to whether the magnetic field term is included or not. The work by Liu et al. (2013), is submitted as a revised manuscript in Adv. Space Res. We decided to add a view sentences about it on page 1986, line 28.

#9: Page 1986 (end of). I think the three main points are saying the residual bias increase with the ionization state, but this point is also clear analytically from Vorobev and Krasilnikova (1994), equation 22. Please consider relating this study to their work here.

#9: According to your suggestion in comment #4 and comment #8 we included a further reference to Vorobev and Krasilnikova (1994) about the connection between the residual ionospheric error and the ionization state on page 1984, line 14, as formulated in answer #1.

#10: Page 1995. The dry temperature baises at 35 km are -3.9 K for Jan 2002. The temperature at 35 km is ∼240 K, so the bias in percentage terms is -1.6 %. It is interesting to note that the bending angle values at 35 km will be ∼ 130 microradians, and the largest residual bias is ∼-0.4 microradians above 60 km. It appears that the fractional error in bending angle space is likely ∼-0.3 % or lower. This appears to illustrate the noise amplification in the dry retrieval. Please discuss.

#10: Yes, that is correct. This example illustrates the systematic error amplification through the retrieval. We already mentioned this kind of error propagation in our manuscript on page 1994, line 29. However, in order to emphasize this stronger we decided to rewrite the sentence and add a citation in the following way:

“Besides being an important parameter for climate research, temperature profiles are of special interest, since they illustrate how the ionospheric error amplifies through the retrieval (Schreiner et al., 2011.)

Finally we want to thank referee 2 for pointing out some typos in our manuscript. Of course they will be corrected in the article.