Author's Response to Referee #1

We would like to thank referee #1 for the thorough evaluation of our manuscript. We have answered all comments below (for easier comparison the referee comments are included in *italic*).

General comments:

#1: In the abstract:

Page 1, Line 7:

'Above that altitude some background information for the Abel integral is still necessary.' Is this a conclusion drawn from this present study? If it is, why it is not explained or discussed in the manuscript at all. The only relevant paragraph is

'The basic idea of the API approach is that averaging of the data in bending angle space suppresses the noise in the data, so that the observed bending angle can be used up to 80 km and the SO step becomes largely obsolete. Above 80km some kind of background information is still necessary. ' in Page 3. If it is not a conclusion of this study, it is not appropriate mention it in the abstract.

#1:We tried to keep the abstract as concise as possible, but we agree with the referee that some additional information is needed for context. Therefore, we added at the top of the abstract:

Global Navigation Satellite System (GNSS) Radio Occultation (RO) data allow for the retrieval of near vertical profiles of atmospheric parameters like bending angle, refractivity, pressure and temperature. The retrieval step from bending angle to refractivity, however, involves an Abel integral, whose upper limit is infinity. RO data are practically limited to altitudes below about 80 km and the observed bending angle profiles show decreasing signal-to-noise ratio with increasing altitude. Some kind of high-altitude background data are therefore needed, in order to perform this retrieval step (this approach is known as "high-altitude initialization"). Any bias in the background data will affect all RO data products beyond bending angle. A reduction of the influence of the background is therefore desirable – in particular for climate applications. Recently, …

Furthermore we will add on p.3, line 21/22:

Above 80 km the bending angle still needs to be extended, since the Abel integral is over infinity and the bending angle is not zero above 80 km. Different extensions of the bending angle are tested in this study, see description in Sect. 2.1 and Sect. 2.2

We will also add the following citation on p. 3, line 27:

(for details see Gleisner and Healy, 2013; Danzer et al. 2014).

#2: The authors compare results with multiple data, namely the reanalysis data , and satellite data MIPAS and SABER

I have some questions here:

1) If there is a very good agreement between the MIPAS and SABER temperature data, as you mentioned in Page 7 Line 8, what is the point to compare your results with both of them?

2) Each satellite instrument has its own sensitive altitude range and accuracy. Have you consider the accuracy of the satellite data themselves?

3) You may also need to talk about the horizontal resolution of these data and its potential influence on the comparison.

#2**:** MIPAS and SABER provide independent measurements of the atmosphere, using different retrievals. Therefore we are convinced that it is useful to compare RO data with both data sets. Even if data sets are in good agreement it is valuable to see if the new data set is in line with the reference data sets. MIPAS and SABER data sets show high accuracy results in the stratosphere, and are hence interesting for a dry atmosphere comparison study. The second paragraph on p.7, lines 10-15 gives an overview of how MIPAS and SABER temperature data sets compare relative to WEGC RO data using a standard processing (Innerkofler, 2015). We think it is interesting to see if similar results are achieved relative to those reference data sets when comparing RO API data sets with different high altitude expansions, instead of RO IPI data sets. However, we see that this has to be put in a better context.

We will extend the discussion and include another paragraph in Sect. 6, p.18, after line 28:

The temperature comparison study of RO API data sets relative to ECMWF analysis, MIPAS, and SABER data sets shows for both, the WEGC and the DMI exptop case, similar temperature biases as Innerkofler (2015) found in his study analyzing global RO IPI temperature data sets. RO API, ECMWF analysis data, and MIPAS data agree within +-1K, up to 40 km. Above 40 km they begin to show larger differences than when analyzing global

RO IPI data. Furthermore, the 3K temperature bias of SABER data could also clearly be illustrated relative to RO API data.

Concerning the horizontal resolution:

It is true that the underlying observational data sets have different horizontal resolutions. However, these data are not directly compared. Comparisons are made between data that have been averaged in monthly latitude bins, which are identical for the RO, ECMWF, MIPAS, and SABER data sets. The different horizontal resolutions of the underlying data is not a major problem for these heavily averaged data sets.

#3: Clearly your inversion results vary with latitudes, but does the accuracy of your inversion result vary with seasons? And will your inversion results influenced by humidity? Although it is the 'dry temperature' you are studying, water vapor in the atmosphere may significant influence the excess phase, right?

#3: The influence of humidity is important in the troposphere and has also been studied by Danzer et al. (2014). For the present study, where we analyze the influence of the high altitude initialization - which is important in the stratosphere, the influence of humidity is negligible. We also do not look at seasonal dependence of the API method. However, a long term study of the API method has already been performed in a previous work of us with CHAMP data (Danzer et al., 2014), where data sets from September 2002 until September 2008 have been analyzed. The study did not indicate that the accuracy of the inversion itself does depend on season. However, the study showed that differences relative to reference data sets increase towards higher latitudes, for both, the API and IPI inversions. We focus in this study on the three COSMIC test months January to March 2011, since this work is a follow-up investigation of Gleisner and Healy (2013), who also tested the same three months at the DMI .

#4: Please try to explain why the largest differences are around 35 km in fig. 5-7, 9-10.

#4: We are not completely sure if this remark refers to the general increase in differences beyond 35 km altitude (Fig. 5, 6) or to the larger differences relative to ECMWF analysis at high northern latitudes (Fig. 7,9), therefore we tried to answer both:

1) The "core region" of RO data is between 5 km to 35 km, hence the dashed line in the figures is always plotted at 35 km. The high accuracy in this region has been shown in previous studies, such as Steiner et al. (2013), who showed that consistency between different sets from different processing centers is highest in the UTLS. Hence, regarding the API approach, it is not surprising that differences also start to increase above that respective

altitude. We emphasize that the RO – ECMWF biases above 35 km that we see here are not related to the API method. They are generally seen in all RO - ECMWF comparisons (see also Figures 5,6,7 in Gleisner and Healy (2013) which compares API and IPI relative to ECMWF analysis).

However, we see that it is necessary to emphasize this more strongly in the manuscript and we will discuss this in Sect. 6 Summary and discussion. According to your suggestion, we will rewrite this section and extend the discussion part.

On page 2, line 4 we will add:

The altitude range from 5 km to 35 km is therefore commonly regarded as the “core region” of the RO technique.

Furthermore, on p. 18, line 9.

… The observed RO – ECMWF biases above 35 km are not related to the API method. They are generally seen in all RO - ECMWF comparisons when applying the standard processing (see comparison of API and IPI relative to ECMWF analysis in Figures 5,6,7 in Gleisner and Healy (2013)). In that context it is interesting to see that different handling of the top value above 80 km also propagates down to that respective altitude. ...

On p. 18, after line 22 the following paragraph:

Steiner et al. (2013) showed in a comparison study of climate data products from six international processing centers that different high altitude initialization approaches affect uncertainties in CHAMP RO data from about 25 km upwards. Largest differences between processing centers are found towards increasing altitudes and at high latitudes. This has also been demonstrated for the API approach in a prior study analyzing CHAMP data (Danzer et al., 2014), where differences relative to ECMWF analysis also increased towards high altitudes and latitudes. Also the API approach shows an increasing sensitivity above 35 km altitude when comparing different high altitude expansions for the bending angle, as well as, comparing WEGC and DMI processing centers. The illustrated propagation of uncertainties downwards through the API retrieval chain to about 20 km in dry temperature has also been observed in prior studies for standard retrievals from different processing centers (Foelsche et al., 2011; Ho et al., 2012; and Steiner et al., 2013).

2) On p.14, lines 3-5, we will add another paragraph, including two reference:

Differences relative to ECMWF analyses are lager at northern high latitudes, which could be related to different sampling of the upper stratosphere lower mesosphere (USLM) disturbance in January 2011 (Greer et al., 2013). Related to that, the Arctic winter 2010/2011 has been notified as one of the coldest stratospheric winters on record (Sinnhuber et al., 2011).

#5: Why there are large differences in tropics and mid-latitudes near surface in fig. 5-7,9 and how does the inversion from negative to positive differences formed, e.g. at ~2-3km in the tropics in fig.5

#5: The focus of the study is the stratosphere, where the API method has decisive advantages in comparison with the IPI method. The main purpose of figures 5-7 and 9 are to show the impact on the stratospheric refractivity retrievals by different factors, such as, DMI/WEGC differences and different high-altitude expansions. The refractivity bias structure in the low- and mid-latitude troposphere in the lowest few kilometers seen in figures 5-7 and 9 is not caused by the API method. The bias structure is well-known and is also seen in the IPI method relative to ECMWF analysis. Please see Figures 5,6,7 in Gleisner and Healy (2013). However, the error at the lowest ~2 km is probably due to the use of a mean radius of curvature. This error can also seen in the comparison of API to IPI in Figure 4 of Gleisner and Healy (2013).

We will therefore add (page 10, line 15):

Please note that the focus of this study is the stratosphere and that we therefore show dry parameters, which are not fully adequate to characterize moist regions in the lower troposphere. The refractivity bias structure in the low- and mid-latitude troposphere in the lowest few kilometers relative to ECMWF is not caused by the API method. It can also be seen for the IPI method (see Figures 5,6,7 in Gleisner and Healy (2013)). However, the error at the lowest ~2 km is probably due to the use of a mean radius of curvature.

#6: All your results are based on COSMIC excess phase from Jan to Mar 2011. So I guess if your results depend on seasons, your conclusions are only valid in January to March. Please refine the way that you describe your conclusion.

#6: Please see answer #3. Furthermore, for clarifications we will include the following sentences in Sect. 6:

p.2, line 33

In this study, we test different implementations of the API approach at the Danish Meteorological Institute (DMI) and the Wegener Center for Climate and Global Change (WEGC) and validate them against independent data. We analyze three COSMIC test months from January to March 2011, following the investigations of Gleisner and Healy (2013). A long term API data set study has already been performed for the complete CHAMP period (Danzer et al., 2014), and is not part of this investigation.

#7: In Sect. 6 Summary and discussion, the authors summarized the study and talked about the outlook of the study. I would say Sect. 6 is only a summary but not a decent discussion at all. In fact, in the whole manuscript, the authors have made a very comprehensive comparison, but they focused only on the 'fact' but ignored the 'reason'. I suggest the authors add a separate section of discussion before the summary, in which all the problems and uncertainties of the present study should be discussed in a more detailed manner. And in the section of summary and/or conclusion, the authors should show readers very clear the conclusion from this present study, not from previous study or future work.

#7: According to your suggestion we will rewrite Sect. 6 and extend the discussion part. Furthermore we will rename Sect. 6 to "Summary, discussion and outlook"

Specific comments:

We do not list the complete number of specific comments. However, we thank the referee for the thorough reading of the manuscript and will perform the necessary changes according to your suggestions.

Only specific comments, which require an answer, are listed here:

#1: *Page 2, line 4*

numerical weather prediction (NWP) and climate monitoring in the upper troposphere and lower stratosphere (UTLS) (however, I believe the GPS RO data do not only valuable in the UTLS but in both troposphere and stratosphere, and one or more references are needed here.)

#1: We thank the referee for his valuable comment about the utility of RO data: You are right, but the highest quality (and the highest impact on NWP analyses) is clearly achieved in the UTLS. We will change the first sentence of the introduction to:

... Monitoring, *in particular* in the Upper Troposphere and Lower Stratosphere (UTLS).

The general goal is to expand this altitude range and to increase the utility of RO data (towards the bottom, as well as towards increasing altitude). This study attempts to increase the utility in the (upper) stratosphere.

The citations are given in the same paragraph in the next three lines (p.2, lines 5-7), first referring to NWP, then to Climate.

We will add to the introduction on p. 2, after line 31.

The advantages of the API approach are the following, a) the reduction of background in the data, b) the circumvention of the complicated statistical optimization step (a known reason for differences between processing centers), c) the API approach is much faster in computation.

Furthermore we extend the paragraph on p.2, line 33

...The aim of the API approach is to produce high quality climatologies, with well characterized errors, which might push current limits in altitude further, enabling the study of stratospheric climatologies above 35 km.

In the discussion on p. 19, line 3 we add the following sentences:

The latter result might suggests that API dry temperature climatologies can be used up to 40 km, pushing current limits of the utility of RO data in the stratosphere.

#2: Figure 1: Left panel: what does the blue dashed line indicate? Please explain.

#2: Thank you for noticing. It is the standard deviation of AvProf. We will write:

p.4, line 26

(Eq. 2, AvProf – blue line, its standard deviation - blue dashed line)

#3: I would strongly recommend that the authors find a native English speaker to check the manuscript for grammar and structural problems.

#3: We will follow your suggestion and have asked a native speaker to perform final proof-reading on the revised manuscript.