Dear Reviewer,

Thank you very much for your attention and comments on our manuscript. Please find below our detailed replies on your comments.

Reviewer #1. General Comments

Improvements could be made to better explain/quantify the effect of regularization on the time delay signal. I found it difficult to intuitively understand what the measurement fraction (Eq. 10) is saying in relation to other commonly used metrics of the effect of regularization. Since the time delay regularization approach is essential an optimal estimation style linear retrieval with K = I, would it instead be possible to calculate the equivalent averaging kernel and show rows/their sum? This would also give a sense of what effect the regularization has on the vertical resolution of the time delays.

Authors: We added a figure with examples of averaging kernels, for two occultations discussed earlier in the text: the oblique occultations R07673/S001, with strong influence of isotropic turbulence, and the vertical occultation R07588/S002, with small influence of turbulence. We added also a corresponding text discussing this figure.

A major point of the manuscript is the validation of small scale structure in the new HRTP FSP v1 data, and in particular its improvement over other processors. This is obviously a difficult task, but the validation efforts presented do show that the new data is more consistent with the radiosonde data than that of other processors. The part that I feel is missing is an explanation of why that improvement occurs. Differences in the regularization/upper limits are clearly outlined, however are these the only differences or are there others? What is the difference between the IPF and FMI v6 processing?

Other differences between IPF and FMI v6 processing are the retrieval grid, implementation of the Abel inversion and computing environment. The reason for excessive amplitude of fluctuations in HRTP produced by IPF v6 is unknown (we can only suspect that the reasons might be numeric instability or algorithm implementation), and we would prefer do not discuss this in the paper.

The manuscript would be improved by a brief overview of how the presented retrieval method is different than the IPF v6 and FMI v6 processing and how these differences can explain the observed improvement. If the primary reason for the improvement is the different regularization scheme then I think the effect of regularization needs to be better quantified, perhaps with the averaging kernel suggestion above.

Yes, the main change is the regularization. We added the figure with averaging kernel and corresponding discussion, as suggested.

Reviewer #1 Minor Comments

p.1 l.17: “The HRTP profiles are retrieved with . . . high accuracy of ~1-3 K”. As far as I can tell there is no discussion of accuracy anywhere in the discussion, this statement seems to apply to the HRTP precision. There are several other places in the abstract where accuracy should read precision.

Authors: We changed “accuracy” to “precision” and explained that “precision” means “the random uncertainty” p.2 l.16: “Furthermore, since the signal recorded by a detector is intrinsically one-dimensional, the retrieved parameter (temperature or density) is also one-dimensional.” I think this statement is slightly misleading, it should be made clear that this is an approximation being made (the next paragraph does in fact justify this approximation).

We added an explanation that it is roughly along the trajectory of the ray perigee point in the atmosphere.

p.2 l.26: “The ratio of buoyancy frequency N to the Coriolis parameter f, N/f, is typically larger than 100.” The ratio of limb horizontal path length to vertical path length is also about 100, does this mean that large scale horizontal fluctuations are expected to have an effect on GOMOS measurements?

The horizontal path length for occultation measurements in the stratosphere is ~2000 km. The vertical scale probing by the instrument depends on the sampling frequency. For GOMOS photometers with 1 kHz sampling frequency, this is ~ 3.4 m. The large-scale horizontal fluctuations do not affect the star scintillations. In our paper, we consider vertical and moderately oblique occultations (for which tan(β) is smaller than anisotropy of air density irregularities).

p.3 l.20: “... HRTP processed by the FMI scientific processor (analogous to the ESA IPF v6 algorithm) . . .” This is the only statement I could find about the relationship between the ESA and FMI v6, however from Fig. 9 there appears to be large differences between the two.

We added more details on differences between ESA and FMI v6.

p.6 l.6: “In the previous retrievals, the upper altitude, where the HRTP processing starts, depends on the strength of scintillation and value of a priori time delay”. An additional statement is needed here as to why this was changed to be fixed at 32 km. Presumably in the previous version of the retrieval altitudes where the time delay was expected to be less than the sampling rate of the photometers was not used. Was it a problem with the a priori data not being accurate? Or something else?

We rephrased the statement into: “The sampling rate of GOMOS photometers allows determination of time delay up to ~35-38 km. However, above 32 km uncertainty of retrievals is large (see also discussion below), therefore the upper limit is set to 32 km in the new retrievals”.

Below we discuss that, in addition to small time delay and the influence of instrumental noise, the isotropic turbulence affects the retrievals in oblique occultations mostly in the altitude range 30 -40 km.

p.9 l.16: “These situations are handled through regularization applied to the time delay profile . . . ” I initially read this and was confused. The word regularization in standard limb retrievals is used because there is not enough information to retrieve the (essentially continuous) target quantity, but here the time delay can be found exactly. That being said, the technique applied is a standard regularization technique for limb retrievals and has a simple interpretation as ‘virtual measurements’ added to the system. I am not sure if it would be better to use a different word (nothing immediately comes to mind), but it should be made clear that this is not regularization based upon the standard mathematical definition.

From our point of view, this is rather a standard case when regularization is needed. Although some value of time delay can be computed in cases of low correlation, this value has a very large uncertainty.

p.11 l.6: "If both matrices Ca and Cmeas are chosen to be diagonal, the Bayesian approach coincides with optimal filtration". This is only the case if what is referred to as "optimal filtration" does not include the CCC < 0.7 criteria that was used in v6. In fact the effect of removing this condition might be more important than going to full covariance matrices, is there an estimate of which of these two things is the dominant difference?

Yes, you are right, the condition "CCC<0.7" has a dominating effect and therefore the choosing Ca and Cmeas matrices diagonal would not give the results identical to V6. In the revised version, we removed this statement. In addition, according to comments of Reviewer #2, we added more discussion on the differences with the V6 algorithm (In Section 3 and in the Summary).

p.12 l.5: "Since the refractive angle is proportional to the time delay, its uncertainty can be easily obtained by multiplication of the time delay uncertainty by the corresponding factor." I assume that the uncertainty in the spectra is negligible compared to uncertainty in the time delay?

Yes, this is true. In the revised version, we noted this: "The uncertainty associated with effective wavelength determination is negligible compared to time delay uncertainty".

p.12 l.13: "The error due to horizontal gradients of the refractive index at right angles to the direction of light propagation has been estimated . . . it is less than 1% for altitudes" Is this for all altitudes or the altitudes relevant for the retrieval?

Sorry, the end of the sentence was missing: " for altitudes above 10 km"

p.12 l.18: "ECMWF & MSIS data is used at altitudes above HRTP range in the processing". Is there an estimate for how much of an effect this has on the final retrieved temperature?

The upper limit initialization affects the upper part of the profiles, with error rapidly decreasing (nearly exponentially with the atmospheric scale height ~ 7 km) so that ~3-5 km range of upper altitudes is affected. This is the same uncertainty as occurred in processing of radio-occultation data. The uncertainty due to upper-limit initialization is included in our processing at the stage of computing temperature profile uncertainty (Eq. (18) of the original manuscript).

p.13 l.23: "The regularization on time delay, as well as other inversion steps from time delay to temperature profile, can slightly degrade the vertical resolution ..." The effects here that can be quantified should be, in particular the effect of regularization.

As mentioned above, the illustration of averaging kernels after the regularization is added. We added also a reference to a publication on Abel inversion.

p.14 l.3: “. . . bright stars in vertical occultations . . . ” Vertical here means in orbital plane?

Yes, they are the occultations in vertical plane; this definition of vertical occultations is given already on page 3, line 23.

p.14 l.5: “. . . from the SPARC data center (http://www.sparc.sunysb.edu/html/hres.html)” Link did not work for me when I tried it, but it might have just been temporarily down.

Thank you for noting this. The new link is https://www.sparc-climate.org/data-centre/data-access/us-radiosonde/

p.14 l.26: “The HRTP wavenumber spectra in Figures 6 and 7 have visible cut-offs corresponding to scales ~150-250 m” It is hard to see this from the figure, maybe something could be added to highlight this.

In Figures 6 and 7, we added a red vertical lines, which indicates the small-scale regions (for those spectra where this cut-off is visible), where HRTP resolution can affect the spectra.

p.17 l.18: “We have used the radiosonde data from the . . . ” Here the radiosonde data is explained in detail, however this information should be stated earlier since many of these details are relevant for the colocations presented as well.

In the revised version, we moved the description of radiosonde profiles used for validation to the beginning of Section 4.

p.22 l.4: “. . . altitude range 10-35 km . . . ” It was previous stated the high altitude was 32 km.

Corrected to “10-32 km”.

p.22 l.5: “. . . accuracy in the stratosphere of 1-2 K” Again this should be precision, also earlier and in the abstract the numbers quoted were 1-3 K.

Corrected to “1-3 K”.

p.22 l.11: “The upper limit of HRTP is defined mainly by the sampling frequency of the photometers . . . ” This seems contrary to what was stated earlier, that the upper limit was set to 32 km.

Here we discuss the principles of HRTP retrievals, not only the practical application to GOMOS. In the revised version, we added “In general, ..” in the beginning of this sentence.