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Atmospheric Measurement Techniques Discussions

Interactive comment on "High-resolution temperature profiles (HRTP) retrieved from bi-chromatic stellar scintillation measurements by GOMOS/Envisat" *by* Viktoria F. Sofieva et al.

Anonymous Referee #1

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The manuscript presents the inversion algorithm for the processing of high resolution temperature profiles from GOMOS measurements. The algorithm exploits the time delay of scintillations observed from photometers at two different wavelengths to obtain vertical refractive index profiles, which are then transformed to temperature using hydrostatic balance. The retrieved profiles are compared to colocated radiosonde data and are shown to have improved spectral features relative to other GOMOS temperature products. The full GOMOS mission has been processed with the new algorithm and the dataset is in open access.

The manuscript is well organized and the logical progression of the retrieval is easy

to follow. I believe the manuscript is well suited for AMT and is suitable for publication following taking into account my comments below.

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.

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? 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.

Minor Comments

p.1 l.17: "The HRTP profiles are retrieved with . . . high accuracy of $\sim$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.

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).

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 fluctations are expected to have an effect on GOMOS measurements?

p.3 l.20: "... HRTP processed by the FMI scientific processor (analagous to the ESA IPF v6 algorithm) . . . "
This is 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.

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 τ_a"
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?

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 intrepretation 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.

p.11 l.6: "If both matrices C_a and C_{meas} 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?

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 negliglble compared to uncertainty in the time delay?

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?

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?

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.

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

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.

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.

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.

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

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.

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