Dear Reviewers,

Thank you very much for your valuable comments on our paper amt-2012-97 "Anisotropy of small-scale stratospheric irregularities retrieved from scintillations of a double star α-Cru observed by GOMOS/ENVISAT". Below we present the detailed replies to your comments. Since some of comments are similar in both reviews, we give joint replies to the comments. Please find also the revised manuscript with all changes marked.

COMMENTS

Reviewer #1
Assumptions and approximations: The section is supported by a number of citations to other papers. Nevertheless it would help to expand a bit more with respect to the justification of assumptions and approximations used for the analysis.
Reviewer#2
P4886-4887. A description of assumptions looks somewhat sketchy. There is no mentioning of the model for the anisotropic part of the medium spectrum. A reference to a relevant work would be appropriate in this place.

Authors
In the revised version, we have included a more detailed description of assumptions, and the corresponding references (pages 4-5 of the revised manuscript with tracked changes).

Reviewer#2
Can it be specified what are limitations for the phase-screen assumption? Different aspect angles for inhomogeneities away from the perigee point might lead to some averaging of the anisotropy coefficient. Can this effect be reasonably neglected?

Authors
The use of the phase screen approximation is justified by the fact that the effective length of interaction of stellar light and the atmosphere, which is a few hundreds of kilometers, is small compared to the distance from the phase screen to observer (in the considered experiment, it is ~3200 km); thus, intensity fluctuations at the exit from the atmosphere are negligibly small. The properties of the phase screen and conditions of its applicability to satellite measurements are discussed in detail in (Gurvich, 1984; Gurvich and Brekhovskikh, 2001). In particular, the effect of changing aspect angles of strongly anisotropic irregularities along the ray is discussed in detail in this paper. This effect is taken into account in our computations of the theoretical scintillation spectra.

In the revised version, we have added these clarifications (page 5).

Reviewer#1
As the method applied is limited to a weak scintillations regime and thus limits the applicable atmospheric altitude range are there any prospects to be able to overcome this in future work?
Reviewer #2
The weak-scintillation regime limits the applicability of the used approach for lower heights (<30 km). It would be interesting in the future to extend this method to a strong scintillation regime. For example, maybe it is possible to filter out the high-frequency part of the scintillations, so the rest would be described by a weak-scintillation theory?

Undoubtedly, a possibility of reconstruction of the parameters of air density irregularities in strong scintillations regime is very interesting problem for future studies. This would allow significant extension of the altitude range. However, for strong scintillations, the consideration becomes much more complicated. For strong scintillations the relationship between the 3D spectrum of air density irregularities and 2D scintillation spectrum becomes non-linear, in contrast to the perturbation method for weak scintillations. In particular, this means that for statistically independent anisotropic (Φ_W) and isotropic (Φ_K) components, the corresponding components of the scintillation spectrum are not independent anymore. The latter assumption is used essentially in the developed method, which is based on separation of anisotropic and isotropic components in the measured scintillation spectra. Strong scintillations results in changes not only in the high-frequency part of the scintillation spectrum, but also in its low-frequency part. Therefore, low-frequency part of the strong scintillation spectra cannot be adequately described by the formulae developed for weak scintillations, even after filtering high frequencies.

In the revised version, we have added:
" For strong scintillations (ray perigee altitudes less than 30 km), the consideration becomes much more complicated. Properties of strong scintillation spectra in occultation experiments and some possibilities for retrievals of the information about the structure of atmospheric irregularities are discussed, in particular, in (Gurvich et al., 2011)".

A. S. Gurvich, V. V. Vorob'ev, and O. V. Fedorova «Strong Scintillation Spectra behind the Atmosphere with Large- and Small-Scale Inhomogeneities», Atmospheric and Oceanic Optics, 2011, Vol. 24, No. 4, pp. 347–357).

Reviewer#2
P4888L1. It reads: "using the hypothesis of "frozen" field of irregularities, which is valid due to the large satellite velocity." It seems to me that the satellite velocity itself is irrelevant here. It is the speed of movement of the point of ray intersection with a limb's plane which is important for "frozen" field assumption.

Authors
We replace "large satellite velocity" with "large velocity of the line of sight".

Reviewer#1
Figure 1, please add captions to the axes or explain the meaning of z & y in the figure caption.
Reviewer#2
P4888L4-8. A description of Figure 1 needs to be expanded. Please describe the orientation of axis x and y. It looks like axis x is oriented along the phase screen. Then, why rays from stars are going through anisotropic irregularities (ellipses) very steeply?
Since this is a perigee area rays should propagate along the longer axes of the ellipses rather than across them. What is the meaning of the dashed lines connecting points 2, 4, and 3? Is notation Δc denotes a distance between points 1 and 3? Then, it would be useful to indicate this with a bracket.

Authors
We have included an additional panel into Fig.1, which show the occultation geometry. We have added also explanations, which are suggested by reviewers (please look at Fig.1 and page 6 in the revised paper).

Reviewer #1
The estimated parameters of irregularities in Table 1 exhibit quite some fluctuations, it is mentioned that the result is consistent with former work. Can this be explained as a physical phenomenon (natural variability) or is it more likely related to the used model/assumption and/or sampling?
Reviewer#2
P4894L4, 5. It reads: "The estimates of the parameters of irregularities presented in Table 1 exhibit rather large variations." Why? Is this because of a not sufficient averaging of limited numbers of scintillation samples, or due to a natural variability of the internal wave intensity, or both? Also, maybe, the model for the anisotropic part of the spectrum relies on an assumption that the spectrum is fully-developed which is not necessarily happens for all observations?

Authors
In the revised version, we replace:

«The estimates of the parameters of irregularities presented in Table 1 exhibit rather large 5 variations, a feature noticed earlier in Gurvich and Kan (2003a) and Sofieva et al. (2007a).»

by

«The estimates of the parameters of irregularities presented in Table 1 exhibit rather large variations, which exceeds significantly uncertainty of the estimated parameters. This feature has been observed also in earlier works; it is related with the natural variability (intermittency) of the parameters of gravity waves and turbulence in the atmosphere (Gurvich and Kan, 2003a; Sofieva et al., 2007a).»

Reviewer#1
4884, 10 …a single star -HAS- been analyzed …4885, 7 …double sources, -HAVE been Discussed…

Authors
4884, 10 autospectra for a single star have been analyzed (original is correct)
4885, 7 Text is corrected.

Reviewer #2
Minor corrections:
P4885L15. Typo: "scintillatioin"
P4890L8-9: It reads: "Coherency of bi-chromatic scintillations has been discussed in Gurvich et al. (2005), Kan (2004) and Kan et al. (2001)." It would be more appropriate to reverse the order of references to have them following chronologically.

P4900L11: Typo: "gravityy."
P4890L24. There is a typo in subscript of the second argument of the cross-spectrum: Va(f, λ_B,λ _B). It should be Va(f, λ_B, λ_R).
P4891L3. The same typo as above.
P4892L24. Replace '"autospectra" with "auto-spectra."
Terms "coherence" and "coherency" are used intermittently through the text. For example, Fig.3 caption contains both "coherency" and "coherence" terms. Choose one of them.

Authors.
Thank you. All corrections are introduced. In the revised version, the term "coherency" is used throughout the text.