Response to the reviewer:

I thank the authors for the very detailed response to all of my comments and for the very extensive rewriting of their manuscript. I sincerely was actually not expecting that. I just suggested some small clarifications to make the manuscript more understandable for a wider audience. In addition, I did not consider that they were many fundamental clarifications other than the unclear writing of some sentences (sorry if I was too pedantic with that). Other readers may have misunderstood the point of this paper in the same way as I. So, I sincerely apologize for all the additional work that my suggestions involuntarily caused to the authors.

Thanks, however there would be no reason to apologize. The responses were very useful and, as the opinion of the reviewer reflects, contributed essentially to a clearer presentation. Thanks again.

In my opinion, the revised version of the manuscript now is more clear than before.
The organization of the different sections into subsections makes everything easier to follow.
In particular, the last paragraph of page 4 (part of the introduction): I think this is one of the most important addition of the revised version of this manuscript, which explains its main point and purpose, what makes this manuscript different from other approaches explained in the previous paragraph (in page 3). The last paragraph in page 3 also motivates very well the purpose of this paper in explaining the observed bumps in the turbulent spectra. The description written in the second paragraph in page 4 is quite helpful to understand the motivation behind the idea presented in this paper. The same goes for the very helpful first paragraph in page 17 (section 5, discussion).

I have only a few small comments and possible typos to be corrected, actions items that it would be good to address before publication, please. All of them are only related with the new additions/corrections of this version of the manuscript and should not require to change more than a few words/one sentence each. This is just to try to make the manuscript more precise, please.

We appreciate this and the following comment very much indeed.

REMARKS:

1) page 4 "If kinetic Alfvén waves are unambiguously confirmed, the inner solar wind ... must be subject to the continuous presence of small scale collisionless shocks" ---> This is a strong statement, which requires more evidence, otherwise it would be misleading. Why the "must"? I am

more evidence, otherwise it would be misleading. Why the "must"? I am not sure if that is the only possible choice, the presence of KAWs could be associated to shocks, but I think it is not strictly required.

Right. This is too strong an expression. Replaced by “could”.

However, the whole story is more complicated. As expressed in the former replies and briefly noted at another place in the MS, thinking of solar wind turbulence in terms of homogeneous and stationary turbulence is incorrect. If one does so, then one has in mind the local state of turbulence (i.e. at the location of the spacecraft which measures), but in this case the system is open. On the larger scale radial dependence and thus evolution cannot be neglected. The source of the turbulence is somewhere in or close to the solar corona. The turbulence therefore evolves when streaming away. In the ion-inertial range one would expect that in the source region KAWs will become excigted by the presence of some free energy which causes the turbulence on all scales. Excitation is certainly much faster in time than Kolmogorov’s cross-spectrum flow which hold just for stationary turbulence. The KAWs will grow, usually to small amplitudes, they possibly saturate quasilinearly. However being waves in collisionless conditions they are nonlinear kind of simple waves. This implies that during transport radially downstream some of them will become damped, other with grow and steepen on a short scale. So, if they have been exited in the corona and evolve nonlinearly they will necessarily form steepened wave fronts, i.e. small-scale shock waves. This is practically unavoidable. And if one attributes any bump or flattened region in the ion-inertial range to KAWs, as is frequently done, the these KAWs are neither linear waves (which would be a completely wrong assertion) nor quasilinearly stabilized waves (which one would not detect because the quasilinear saturation level is miniscule). So the physically reasonable remaining state is that they are small-scale (ion inertial scale) shock waves with all effects which are related to them: ion heating, reflection of electrons, acceleration of electron, excitation of kinetic electron waves like Langmuir, ion-acoustic etc., electron beams along the magnetic field and even first and second harmonic electromagnetic radiation. All kind of dissipative effects in turbulence. this means that the probability of ion-scale shock waves is quite high in a radially expanding (i.e. inhomogeneous) and thus non-stationary solar wind turbulence, processses which have barely yet been considered in formal investigation of solar wind turbulence.

This is to clarify that the probability of the presence of small-scale (ion-inertial scale) shocks superimposed on the turbulent background is relatively high, at least at times.

2) (possible typo) page 5: "particles "loose" their magnetic property" ---> shouldn't be "lose" instead of "loose"?

Thanks, of course.

3) page 9, Figure 2: The region below the solid line "Te/Ti=1" should be generally forbidden" In what sense is "forbidden"? (it could happen anyway, see also remark 3)

Thanks. Deleted.

4) page 9, I have some concerns about the sentence "However, the temperature ratio Te /Ti is variable and usually large, varying between a few and a few tens". And in Fig 2: "In the solar wind the temperature ratio is usually between the solid and dashed lines but mostly closer to the dashed, depending on the exact value of beta_e" (dashed line is Te/Ti=10).
I am not sure about the validity of that statement.

According to
"Newbury, J. A., Russell, C. T., Phillips, J. L., & Gary, S. P. (1998). Electron temperature in the ambient solar wind: Typical properties and a lower bound at 1 AU. Journal of Geophysical Research: Space Physics, 103(A5), 9553–9566. https://doi.org/10.1029/98JA00067"
the ratio Te/Ti tends to be mostly between 4 and 0.5.

And "Wilson III, L. B., Stevens, M. L., Kasper, J. C., Klein, K. G., Maruca, B. A., Bale, S. D., … Salem, C. S. (2018). The Statistical Properties of Solar Wind Temperature Parameters Near 1 au. The Astrophysical Journal Supplement Series, 236(2), 41. https://doi.org/10.3847/1538-4365/aab71c"
found that the typical temperature ratio in the solar wind is actually Te/Ti=1.64 , with a standard deviation of 1.27. So, values even close to Te/Ti=10 are actually rare, and not really a few tens.
Any comment or clarification, please?

Deleted. The two citations included. Thanks for the hint on the two papers.

Well, I did not look into those papers yet. My own experience from measuring electron an ion temperatures in the 80th-90th was that T_e~10 T_i. But this might have been polluted by the location being close to the bow shock as those were the tmeperatures I had access to. I may accept that generally T_e>T_i but strongly doubt the opposite case as I never saw any observation of this kind neither upstream nor downstream of the bow shock. The uncertainty in Wilson et al of being of the same value as the temperature ratio seems to me a statistical effect which I hardly believe because I do not see any efficient bulk cooling mechanism for electrons, and the corona is definitely hotter in the electrons than the ions. There are three cooling mechanisms in a collisionless plasma like the solar wind: radiation of electromagnetic collisionless plasma like the solar wind: radiation of electromagnetic waves (in a teneous plasma like the solar wind definitely < 1% in energy, because the medium is optically thin), cooling in electron holes (a very interesting process never yet investigated or discussed: this is a two step process: holes are excited by nonlinear trapping of low-energy electrons in the potential of Langmuir or Bernstein modes; this low energy electron component is heated by trapping, part of it escapes and, together with the passing energetic electron component forms an electron beam which has narrow width in momentum and energy, i.e. is cold), finally charge exchange with neutrals which happens in cometary atmospheres and in the upper atmosphere of planets but plays little role in the undisturbed solar wind. It generated hot neutrals and cold electrons (see IBEX observations). Maybe some of the events where old solar wind electrons have been observed mixes in to generated the small T_e/T_i ratios? Otherwise I do ot believe in those observations because the other processes (radiation, electron holes) are probably out in the solar wind, they play a role in the vicinity of shocks or inside shocks, however, though the radiative cooling is rather inefficient. But the cooling by holes is strong!

5) (typo) page 13: "with a factor of proportionality .... ()1/3". I think the factor (1/3) should be the exponent of the parenthesis (not multiplied as it is now).

Thanks, indeed. Corrected.

6) (possible typo) page 16, Table 1. In the second row, last column, shouldn't be -1/3 instead of -1/6?
(I noticed than the spectral indices ("a-2") are corrected compared to the previous version (I overlooked them before) and also that one table was removed, probably it was not needed. It is also clearer the last column with an explicit b/2 instead of c.)

Thanks again! Yes. True. Typo. Indeed, Table 2 partly doubled this one and was not needed. Instead I replaced it by a few words on advection in the text in order to avoid confusion.

7) page 19: "the power in the second to last expression becomes 1/3" ---> which equation this is referring to? Eq. (43)? If so, which exponent exactly?

Cleared and equation number included.

8) page 27, Caption Figure 3: "power spectrum exhibits a so-called bump at intermediate frequencies of positive slope ~\omega^{1/3}" But later, "The large scatter in the data (weight of line) inhibits distinguishing between K and IK inertial range velocity turbulence" (and similar statements in the main text). So, that sounds like a contradiction, it

should be written in the caption that the fit could be both \omega^{1/2} and \omega^{1/3} (not only the latter).
Similar for Figure 4. "The positive slope \omega^{1/6} in the deformation confirms its origin from pressure balance" I think both slopes 1/6 and 1/4 work as a fit.

True. Distinction is not possible. Expressed in both captions.

9) (typo) page 21, "Sine both evolve" --> "Since both evolve"

Thanks.

10) page 21: "Moreover, from the observations the total \beta > 1 though nothing is known about \beta_i. We expect \rho_i > \lambda_i, and also \beta_i~1" This is a contradictory sentence, either beta_i is known or not.

Thanks. Clarified.

11) page 21: please mention what V_0 and U_0 are in the first line of the summary and outlook, that will make this section more self-contained (or at least please make a cross-reference to their definitions after Eq 2 in page 6).

OK. Mentioned in summary. cross-ref is not required then.

Thanks very much for all these very useful comments!