nature portfolio

Peer Review File

Direct observation of ion cyclotron damping of turbulence in Earth's magnetosheath plasma

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REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

This paper provides one of the first direct measurements of ion cyclotron damping in space, a potentially important dissipation mechanism for the plasma turbulence ubiquitous in the universe. This is achieved via correlations between the electric field and the velocity-space distribution of the ions. It should be noted that a related measurement was made using Parker Solar Probe measurements in the near-Sun solar wind by Vech et al. A&A650, A10 (2021), but the current paper uses the full ion velocity distribution function and so is more convincing and complete.

This is undoubtedly a highly significant result not just in the field of space plasma physics but throughout astrophysics - as the authors point out, turbulent dissipation is a grand challenge problem and understanding these issues is of relevance for our understanding of many different astrophysical phenomena.

The work presented in this paper supports the conclusions of the authors that the turbulent dissipation in this interval is due to ion cyclotron damping; for example, Fig. 3a,b and fig. 4 in my mind establish this beyond reasonable doubt. The basic methodology of the FPC technique is very sound and well-established, and the authors have explained their use of the data from MMS well. There is ample information for other researchers in the field to reproduce this work. However, I have a couple of issues with the manuscript as it stands; I'm confident these can be fully dealt with by the authors with some relatively minor revisions.

Major comments:

1. I am not yet convinced with the “analytic model” presented by the authors, and presented in (e.g.) figs 5c,d, figs 6c,d. I may be misunderstanding the technique, in which case I would be very happy to be corrected: from the methods section, it seems like what is done is as follows

- the electric field and bulk velocity from the data are fitted to the form in eqs.6-7

- the vdf is taken to be drifting maxwellian with thermal velocity fitted from the data

- the FPC is calculated for this fitted model

Is it not then somewhat inevitable that the "analytic model" fits the data, since it is in fact just a simplified version of the real data? I thought that this part of the paper was somewhat unconvincing and in fact unnecessary to the main point. Moreover, the fact that the signatures in the simulations and in the data look quite different could also be interpreted as a non-universality in the signatures identified.

2. Another related question I had was regarding the quadrupolar signatures in C_Eperp1, C_Eperp2 when plotted in the (vperp1,vperp2) space. The authors seem to be saying that this is a universal signature of ion cyclotron damping; why is this always quadrupolar? Is there are physical interpretation of this? If so, it would be good if it was mentioned or explained, especially since I was less convinced by the analytic model. It would also help make this work accessible to researchers less familiar with the basic mechanism. Perhaps a diagram of the mechanism by which this quadrupolar signature appears in vperp space would be useful.

Minor comments:

3. From Fig. 2b, it looks like the period with significant LH waves extends slightly before the interval studied. Why wasn't the whole patch of ICW studied, was the burst mode data not available there? Do we know that the ICW here was self-generated by the turbulence or could it be from some instability slightly upstream?

4. line 146, the energization coincides with the disappearance of the ICW. Can you use the EM fields to estimate the energy lost from the waves and compare with the energy gained by the ions?

5. line 188, the turbulence simulation generates linearly polarized KAW - is there a reason for this? Is it the tendency for turbulence to produce sheets, basically? If this is the case, why does the magnetosheath turbulence not?

6. Have you examined any of the other intervals in fig 7 where the cascade rate didn't match the particle energization rate from Landau damping, and could ion cyclotron damping be active in those?

Extremely minor comments:

- line 44: this sentence largely repeats information in the first paragraph just above.
- line 95: one can't see the -5/3 from the plot shown
- line 98: "excess power over 0.2Hz<f<0.5Hz": how can this and a -5/3 spectrum both be true?
- line 116: "correlation interval": I would suggest rephrasing this, since to some turbulence people this could have another meaning (it sounds in my view too similar to correlation length).

In summary, I think that with a little revision this should certainly be published, as it is a highly important result with relevance across a broad range of disciplines.

Alfred Mallet

Reviewer #2 (Remarks to the Author):

Below is a review of NCOMMS-23-03741 ("Direct observation of ion cyclotron damping of turbulence in Earth's magnetosheath plasma" by Afshari, Howes, Shuster et al.). This paper uses analytical arguments and numerical simulations as guides to interpret correlations between electromagnetic fields and ion velocity distributions measured by the Magnetospheric Multiscale (MMS) mission. In particular, the authors claim to find direct evidence for ion-cyclotron damping in Earth's turbulent magnetosheath, by examining correlations in phase space indicative of high-frequency energization of ions at suprathermal perpendicular velocities by perpendicular electric fields. The paper is interesting and potentially of broad interest, and I am supportive of the article eventually being published in Nature Communications. That being said, I have a number of comments, critiques, and questions that can ultimately be traced to an opinion that the analysis is too incomplete to support some of the stronger claims made in the manuscript, and that the citations to the published literature could be improved. These are enumerated below, with minor comments on the text itself relegated to a separate list. Despite these concerns, I would like to emphasize to the editor that the work reported in this paper is likely to be impactful and highly cited in the heliophysics community.

1. It would be beneficial to show a plot of the ion distribution function, or perhaps of the distribution function after subtracting off a Maxwellian fit (to highlight "delta-f"). This would be not only for completeness -- because the distribution function is an ingredient used to computing the field-particle correlation -- but also because a number of additional things can potentially be learned from such information. First, published (hybrid-)kinetic simulations of strong Alfvénic turbulence with a focus on ion energization have identified a number of features in the ion distribution function that have been associated with particle-energization mechanisms, e.g., resonant features and quasi-linear flattening near v_prl ~ v_{t,i} and/or ~v_A, non-thermal wings, flattened perpendicular cores, etc. (e.g., Arzamasskiy et al. 2019; Cerri et al. 2021). If the measured energization is indeed due almost entirely to cyclotron heating, it would be useful to see the impact of that energization on the distribution function itself. Or, if stochastic ion heating is not relevant during the interval studied, perhaps that is because the perpendicular distribution function has already been flattened by an earlier episode of stochastic heating at radii closer to the Sun, begging the question, what does f(v_prp) look like? Second, there are predictions for the perpendicular-energy diffusion coefficients associated with stochastic heating and with cyclotron heating (e.g., papers by Kennel & Engelmann, Isenberg & Vasquez, Klein & Chandran, Cerri et al., and others), and knowledge of the distribution function would allow one to compute it: D^E_{prp,prp} = - < dQ_prp/de_prp > / < df(e_prp)/de_prp >, where Q_prp is the perpendicular energization of the particles, e_prp = 1/2 v_prp^2 is the perpendicular energy, and the brackets indicate some appropriate space-time average. It would be interesting to see how the computed diffusion coefficient scales with e_prp.

2. It would also be beneficial to show a more quantitative plot of the electric and magnetic energy spectra (I find fig ure2a difficult to parse quantitatively). Cyclotron heating by high-frequency KAWs has been shown to steepen the magnetic spectrum near the value of k_prp rho_i where the KAWs attain near-cyclotron frequencies (see, e.g., fig 1 of Cerri et al. 2021). There is also evidence in the solar wind that the ion-kinetic-range spectral index correlates with the amount of inferred energy dissipation, with more dissipation correlated with steeper spectra (e.g., Smith et al. 2006). Is there a steepening of the spectrum in the sub-ion-Larmor range near the cyclotron frequency? Another data point on this topic is from Podesta (2009), who associated a rapid decrease in power anisotropy measured in high-speed solar-wind streams near 2 Hz with strong linear dissipation of KAWs occurring at k_prp rho_i ~ 4; in a 2012 article, he argued that KAWs can couple to ion-Bernstein waves in this wavenumber range, which are strongly damped through a combination of ion-cyclotron and electron-Landau resonances (but see #4 below).

3. I think the claim that "all significant channels of turbulent dissipation in this interval are identified" is not yet adequately justified. Figure 7 indicates that there's room within the errors for additional significant heating mechanisms; the black diamond is a factor of ~2 below the solid line. Line 228 in the manuscript begins a paragraph that acknowledges a possible contribution from stochastic heating, but this contribution is not constrained by the authors. The contribution to particle energization from stochastic heating could be estimated following the method used by Bourouaine & Chandran (2013), Vech et al. (2017), and Martinovic et al. (2019, 2020). I appreciate that stochastic heating at beta ~= 1 would require larger-than-typical fluctuation amplitudes on ion-Larmor scales to be relevant, but the magnetosheath is different than the bulk solar wind, and it's not obvious to me from what the paper presents that stochastic heating is inconsequential. Similarly, line 262 in the manuscript states that parallel energization of ions measured via < jprl_i Eprl >_tau is found to be negligible; what about a possible contribution from Barnes damping, which is parallel energization of ions through E_prp? Finally, the authors write (line 273) that the "calculation of C_{Eprp} and C_{Eprl} for both ions and electrons captures all possible channels of energy transfer to the particles in a weakly collisional plasma". Technically, this is not correct -- there can also be viscous heating, e.g., from the gyrotropic piece of the pressure tensor being correlated with the rate of strain of the plasma motions, viz., Pi:grad(u). There are some in the solar-wind community (though not this referee) who would rather focus on this particular diagnostic at beta ~< 1 than on field-particle correlations to assess dissipation. Although, even setting those proponents aside, it has been

demonstrated that viscous heating is important in weakly collisional, turbulent plasmas at higher values of beta (Arzamasskiy et al. 2022), and so the claim that C_{Eprp} and C_{Eprl} capture "all possible channels of energy transfer to the particles in a weakly collisional plasma" isn't strictly true (at least without further qualifiers).

4. It should be noted somewhere that the simulations referenced in support of interpreting the observations all adopted Ti/Te = 1 as their initial conditions. A reader may wonder whether the measured ratio Ti/Te ~= 13 affects any of the interpretation. One valid response is as follows. The linear frequency of oblique kinetic Alfvén waves at beta = Ti/Te = 1 is omega_KAW ~= 0.7 k_prl v_A k_prp rho_i. Taking into consideration the measured values of T_{prp,i}, T_{prl,i}, T_e, and beta_i, the linear frequency of a KAW would instead be just slightly smaller, at omega_KAW ~= 0.6 k_prl v_A k_prp rho_i (using equation (3.38) of Kunz et al. 2018). Evidentally, the disparate conditions don't affect much the properties of KAWs; this may be worth mentioning in the supplementary material. Of potentially more serious consequence, though, is...

5. The ion temperature anisotropy implied by the reported numbers satisfies (T_prp/T_prl - 1) ~= 1.43. At beta_prp ~= 0.93, this is beyond both the mirror instability threshold and the ion-cyclotron instability threshold. Perhaps the latter is the source of the ion-cyclotron waves responsible for the particle energization? If true, then the reference to the hybrid simulations of Arzamasskiy et al. (2019), Klein et al. (2020), and Cerri et al. (2021) is somewhat dodgy -- it's not obvious at all that the high-frequency fluctuations are in fact part of a turbulent cascade. Indeed, the authors note that "the turbulent cascade [in the HVM simulation] self-consistently generates not left-handed polarized ICWs as in the MMS observations, but rather linearly polarized kinetic Alfvén waves." This referee is wondering whether all this talk of turbulent cascades is a red herring -- ICWs can be generated purely from the unstable temperature anisotropy of the background. This, of course, doesn't take away from the novel measurement of ion-cyclotron heating, which I support being reported, but it would change the narrative greatly, from the first two words of the Abstract to the last sentence of the Discussion section. At the very least, some discussion of where these left-handed ICWs are coming from would be useful.

6. The velocity-space resolution of the MMS data in Fig 3(b) -- with dv/v_{t,i} = 0.2 -- looks poorer than the resolution implied by figure 1 of Chen, Klein & Howes (2019) for the electron distribution function. Why is that? Also, Chen, Klein & Howes (2019) showed the alternative field-particle correlation C', in addition to C. I'd like to encourage the authors to show C'_{E_prp} alongside C_{E_prp}, if not in the main text then perhaps in the supplementary material. The noise would be reduced, since no derivatives need to be computed, and it would also be a useful data point for those theorists who prefer C' over C because calculating derivatives of distribution functions obtained from PIC simulations can be a noisy affair.

7. Finally, I would like to see the authors engage more with the recent PRL by Bowen et al. entitled "The In Situ Signature of Cyclotron Resonant Heating". While that paper doesn't compute a field-particle correlation, it does present evidence for flattening in the phase-space distribution in a specific way predicted by resonant quasilinear diffusion in ion-cyclotron waves, as well as steepening in the turbulent spectra at the ion-cyclotron-resonant scale (cf. points #1 and #2 above). In light of the authors' statement (line 214) that their FPC-driven "lines of evidence consitute [sic] the first direct measurement of ion cyclotron damping in a turbulent space plasma", a comparison of these two papers has me wondering what is indeed more "direct" evidence: field-particle correlations without an analysis of the distribution function and electromagnetic spectra, or a demonstration that the distribution function flattens along contours in a predicted way at the same time that the field spectra steepen. If the authors could provide an analysis of the electromagnetic spectrum and the distribution function to accompany their novel FPC analysis, the paper would be much more complete and notable.

Minor points:

A. For the benefit of the reader, and as is appropriate for a journal like Nature Communications, it would be useful to define the field-particle correlation in simple descriptive words within the main part of the manuscript. The authors point to the supplementary Methods section for "the detailed analysis procedure", but at the moment there isn't even a not-detailed statement of what the FPC is -- as far as I can tell, C' and C appear on page 7 without explanation.

B. The greyish edge on the left-hand side of Fig 3 suggests that this figure was grabbed from a screen shot with a shadow from a neighboring window overcast. I think this panel could be better prepared. Also in this figure, panel (c) has v_prl/v_{t,i} ranging from -4 to +4, but panel (b) has v_prl/v_{t,i} ranging from -2 to +2. This doesn't seem like a fair comparison.

C. Line 119: Unless I'm mistaken, the argument of C'_{E_j} should be \tau=0 rather than just \tau. Also, on line 121, no "s" is needed after $\tau=0$, since 0=0 in any units (and it's not immediately clear that an italic s means seconds here).

D. The sub-panels in Fig 3 are in a different order than they are introduced in the text. Likewise with Fig 4. Please reorder the figure panels or the text so that the narrative is consistent.

E. Lines 129-130: It's not clear yet at this point in the manuscript that the energization highlighted in Fig 3(a) is consistent with ion-cyclotron damping, as claimed, because the predicted signal for cyclotron damping has not yet been given, and there could also be a contribution from stochastic heating near v_prp/v_{t,i} ~ 1. Perhaps some re-ordering of the text would help the logical flow here.

Reviewer #3 (Remarks to the Author):

This paper presents measurements and analysis of collisionless plasma heating via wave-particle interactions in the Earth's magnetosheath. It uses the so-called Field Particle Correlation (FPC) technique developed by some of the authors to diagnose the heating in one interval as resulting from ion-cyclotron waves. The evidence for this involves the shape of the FPC distribution, as well as comparisons to a simple model and numerical simulations. They then measure the dissipation rate, comparing this to a previous measurement of the electron damping rate during the same interval, as well as the ion Landau damping rate. This shows that the ICW damping dominates, with a heating rate that broadly matches the inferred cascade rate over this interval. The implication is that ions are heated perpendicularly and preferentially over electrons.

The paper is well written and the basic point that ICW damping dominates in this interval is convincing. I also believe that if a broader analysis was done including more intervals, the general result would be exciting and important to the wider community, and therefore warrant publication in a high-profile journal such as nature communications. However, I found a number of aspects of analysis somewhat unconvincing, and I believe these issues to be sufficiently serious that further analysis, or at least reworking, is necessary before publication. The authors are welcome to offer a rebuttal if they believe I am mistaken on some of the more technical points.

- A fair amount of the paper concerns the comparison with the "model" (figures 5-6). This is used as evidence that it is indeed ICW damping that is being observed. I do not believe that this model provides the evidence claimed. In particular, as I understood it, the model takes the measured waveforms of U_prp and E_prp (or at least an approximation thereof), assumes a Maxwellian VDF with the measured density and temperature, then plugs these into the FPC formula. They then make a map of the FPC and compare it to the data. But, since the FPC formula is linear in f_s and E_prp, this effectively amounts to replacing the Eprp and Uprp with sinusoidal fits, and the f_s with a Maxwellian. So, as I can understand it, the comparisons in figures 5-6 are only showing that f_s is of a relatively similar shape to a shifted Maxwellian, because the Eprp and Uprp are taken from an approximate fit to the data anyway. So, by itself, how can this constitute proof that ion cyclotron waves are involved? It simply approximates the two pieces of the FPC separately by fitting a time-varying Eprp and Uprp.

I would think that to prove ICW involvement, it is necessary to solve for the plasma's response, as mentioned in the methods. This would simply add a fixed phase relationship between Uprp and Eprp, which can presumably be assessed without looking at the distribution function anyway.

- In a similar vein, I found the comparison to the simulations to be unconvincing. They show the opposite phase relationship, and as explained in the previous point, I do not believe this necessarily to be evidence of ion cyclotron damping (certainly it's a different form of IC damping). Again, real evidence of the claim would have to make reference to the plasma's susceptibility in some form, and would presumably indicate the presence of IC damped KAWs, as stated in the article. But, as it stands, does it not also just show that f_s doesn't deviate dramatically from a Maxwellian?

- More generally, what is the purpose of a comparison to the numerical simulation, since it's shown explicitly to be quite different to what is observed? I did not understand what we were supposed to learn, aside from the fact that the two damping mechanisms (in MMS and the simulation) showed a similar FPC signature in figure 3, despite being physically different (as shown in figs 5-6). This may be an important point, since it shows that sometimes 3D velocity space is needed for distinguishing power in the FPC, but this aspect didn't seem to be explicitly emphasized and is subsidiary to the main purpose of the paper.

- I'm a bit skeptical of the claims in the conclusion that this is the "first direction measurement of ICW damping of turbulence". What about the Bowen et al. 2022 paper discussed in the introduction. They show that the plasma is on average damping ICWs, that the ICWs exist (presumably arising from the turbulence), and they measure a heating rate – in what sense is this more direct?

- Overall, especially given that the application of the FPC to MMS data has already been presented by these authors in nature comms, I would find the paper a lot more compelling if many intervals were analysed, as in Afshari et al 2021. Presumably in a single interval many things can be found, but the statistical behavior is of much more general interest.

- A minor point, but I am confused by how the ion Landau damping can be so small, less than 100 times that of the electron Landau damping. Since this interval has beta~1, this seems quite unexpected considering e.g., the Howes+ 2008 heating model. Certainly it is reasonable that ICW damping and electron LD could be larger, but shouldn't we still expect modest ion LD of KAWs at such parameters, given that the KAWs must proceed through sub-rhoi scales (where they are modestly damped), in order to reach electron scales? Some comment on this surprising result would be helpful.

Response to Reviews of Manuscript NCOMMS-23-03741

"Direct observation of ion cyclotron damping of turbulence in Earth's magnetosheath plasma"

We thank the three referees for taking the time to review our paper and provide us with feedback in order to improve our paper. Below are our responses (in blue text) to each of the reviewer comments (in black). Separately, the revised manuscript has changes highlighted in blue. We hope that the paper in its revised form is acceptable for publication.

We have followed the directive to take the reviewers' comments into consideration and make major revisions of the manuscript to address their comments. The two major issue raised collectively by the referees are:
(i) That the analytical model is simply reproducing the observations rather than being self-consistently predicted from the kinetic plasma physics through the plasma response to an ion cyclotron wave.
(ii) That the numerical simulations using the Hybrid Vlasov-Maxwell (HVM) code, because of the rather different properties of the cyclotron–damped wave modes in the simulation (linearly polarized in the simulations rather than circularly polarized as in the observations) was not very relevant to the ion cyclotron waves observed by MMS.

In response, we have created a new "model" prediction that directly uses the self-consistent phases and amplitudes of an ion cyclotron wave undergoing ion cyclotron damping, showing that indeed the qualitative and quantitative features of the velocity-space signature of ion cyclotron damping are reproduced based on the prediction by the linear Vlasov-Maxwell dispersion relation. And, given this self-consistent confirmation of the velocity-space signature from linear kinetic theory, we find it unnecessary to include the HVM simulation results, so we have removed them from the manuscript. We will refer to these changes below in response to specific comments by the reviewers.

In addition, in response to a number of the many other points raised by the reviewers, we have created a Supplementary Information (SI) document to go along with the published manuscript to explore in more detail some of their comments. We refer to the relevant sections of the SI document in our response below.

1 Review #1

`Reviewer #1 (Remarks to the Author):`

This paper provides one of the first direct measurements of ion cyclotron damping in space, a potentially important dissipation mechanism for the plasma turbulence ubiquitous in the universe. This is achieved via correlations between the electric field and the velocity-space distribution of the ions. It should be noted that a related measurement was made using Parker Solar Probe measurements in the near-Sun solar wind by Vech et al. A&A650, A10 (2021), but the current paper uses the full ion velocity distribution function and so is more convincing and complete.

We thank this reviewer for pointing out this relevant reference, which we have now added along with a short discussion.

This is undoubtedly a highly significant result not just in the field of space plasma physics but throughout astrophysics - as the authors point out, turbulent dissipation is a grand challenge problem and understanding these issues is of relevance for our understanding of many different astrophysical phenomena.

The work presented in this paper supports the conclusions of the authors that the turbulent dissipation in this interval is due to ion cyclotron damping; for example, Fig. 3a,b and fig. 4 in my mind establish this beyond reasonable doubt. The basic methodology of the FPC technique is very sound and well-established, and the authors have explained their use of the data from MMS well. There is ample information for other researchers in the field to reproduce this work. However, I have a couple of issues with the manuscript as it stands; I'm confident these can be fully dealt with by the authors with some relatively minor revisions.

We thank you for the accurate summary of our work, recognizing the significance of the results, as well as the positive endorsement. With our discussions below, we hope to address the issues you have found with the manuscript.

Major comments:

1. I am not yet convinced with the "analytic model" presented by the authors, and presented in (e.g.) figs 5c,d, figs 6c,d. I may be misunderstanding the technique, in which case I would be very happy to be corrected: from the methods

section, it seems like what is done is as follows
- the electric field and bulk velocity from the data are fitted to the form in eqs.6-7
- the vdf is taken to be drifting maxwellian with thermal velocity fitted from the data
- the FPC is calculated for this fitted model

Is it not then somewhat inevitable that the "analytic model" fits the data, since it is in fact just a simplified version of the real data? I thought that this part of the paper was somewhat unconvincing and in fact unnecessary to the main point. Moreover, the fact that the signatures in the simulations and in the data look quite different could also be interpreted as a non-universality in the signatures identified.

In response to this major comment, we have improved the "analytical model" in the paper to use the self-consistent plasma response from the numerical solution for the Vlasov-Maxwell linear dispersion relation using the PLUME solver to predict the perpendicular velocity-space signature of ion cyclotron damping. Note that we use the PLUME solver to determine the eigenfunction for ion cyclotron waves undergoing ion cyclotron damping for plasma parameters relevant to the observed MMS interval, estimating the wavevector of the ion cyclotron wave based on observational constraints (as detailed in Supplementary Information (SI) Fig. S6. When using this linear eigenfunction-based method to predict the perpendicular velocity-space signature of ion cyclotron damping, it is important to note that the only free parameter in the new analytical model is the overall amplitude of the wave; once the amplitude of one of the components of the electric field (here $E_{\perp 1}$) is specified, the relative phase and amplitude relationships among the perpendicular components of the electric field and ion bulk velocity are fixed, determining the qualitative and quantitative appearance of the velocity-space signature. The result of this model is shown in Fig. 5c and d, showing excellent agreement with the observed signatures computed from the MMS measurements.

In addition, we have removed the results from the numerical simulations from the paper as they are unnecessary now that our analytical model generates self-consistent predictions of the velocity-space signatures from the Vlasov-Maxwell linear dispersion relation solutions.

2. Another related question I had was regarding the quadrupolar signatures in C_Eperp1, C_Eperp2 when plotted in the (vperp1,vperp2) space. The authors seem to be saying that this is a universal signature of ion cyclotron damping; why is this always quadrupolar? Is there are physical interpretation of this? If so, it would be good if it was mentioned or explained, especially since I was less convinced by the analytic model. It would also help make this work accessible to researchers less familiar with the basic mechanism. Perhaps a diagram of the mechanism by which this quadrupolar signature appears in vperp space would be useful.

As now stated in the main manuscript, "the quadrupolar appearance is a consequence of the self-consistently determined phase and amplitude relationships among the perpendicular electric field and ion bulk velocity components" (lines 249 - 251). To further demonstrate this, we have created a plot, presented in SI Fig. S3, that illustrates how the energization averaged over the wave period yields the resulting quadrupolar signature. We have referenced Fig. S3 in the main manuscript in support of this point.

Minor comments:

3. From Fig. 2b, it looks like the period with significant LH waves extends slightly before the interval studied. Why wasn't the whole patch of ICW studied, was the burst mode data not available there? Do we know that the ICW here was self-generated by the turbulence or could it be from some instability slightly upstream?

This is a good observation as burst-mode data is indeed available starting from 07:23:04 (which includes the whole patch of ICW shown in Fig. 2). The 77 second interval studied in this work was chosen to coincide with Interval 02 from Afshari *et al.* 2021, who previously quantified the electron energization rate due to Landua damping, as well as the theoretical cascade rate. Here, we make a direct comparison of the newly calculated ion energization rate could with the theoretical cascade rate since the exact same interval has been analyzed.

We do not know how the observed ICWs were generated. We have added some speculation in the mansucript as follows:
"The origin of these ICWs is unknown, and our direct measurements below show that they are damping in this interval; we speculate that they were generated upstream via the Alfvén/ion cyclotron instablity (Gary et al. (1967)) driven by the ion temperature anisotropy $T_{\perp i}/T_{\parallel i} > 1$ (see SI Sec. S2), perhaps due to compression within a quasiperpendicular region of the bow shock, but analysis of the conditions upstream of the bow shock is inconclusive (see SI Sec. S4)" (main manuscript lines 120 - 125)

4. line 146, the energization coincides with the disappearance of the ICW. Can you use the EM fields to estimate the energy lost from the waves and compare with the energy gained by the ions?

If our measurements were at rest with respect to the plasma and there were no turbulent transfer of energy from larger scale fluctuations to the fluctuations at the scale of the ICWs, we would be able to perform this estimation based on the decrease of amplitude of the ICWs. In this interval, since the plasma is streaming past the MMS1 spacecraft at high velocity, we are measuring different regions of the plasma, so it is not possible to estimate a damping rate without strong assumptions about spatial uniformity of the wave amplitude (an assumption that is almost certainly not well satisfied). Furthermore, since the turbulent cascade is constantly transferring energy from large to small scales, a steady state cascade will have statistically constant amplitude at a given scale, so it is not possible to estimate energy lost from the EM fields as a function of time.

5. line 188, the turbulence simulation generates linearly polarized KAW - is there a reason for this? Is it the tendency for turbulence to produce sheets, basically? If this is the case, why does the magnetosheath turbulence not?

Based on comments from all the reviewers that the simulations were not that relevant to the magnetosheath observations, we have removed our analysis of the simulations.

6. Have you examined any of the other intervals in fig 7 where the cascade rate didn't match the particle energization rate from Landau damping, and could ion cyclotron damping be active in those?

Preliminary work has been done for the other intervals in Fig. 7, and ion cyclotron damping could certainly be active in some of them, but this paper contains significant results with this single interval, and so a thorough analysis and publication of those results is left for future work.

Extremely minor comments:

- line 44: this sentence largely repeats information in the first paragraph just above.

The first paragraph (lines 29 - 44) discusses the generic heating/acceleration of plasma species, while the sentence on line 45 stresses the importance of identifying specific mechanisms of turbulent dissipation, so we respectfully chose to leave it as it is.

- line 95: one can't see the -5/3 from the plot shown

This has been elucidated by showing the trace PSD of the magnetic and electric fields in Fig. 2b, with a $-5/3$ slope plotted for comparison.

- line 98: "excess power over 0.2Hz < f < 0.5Hz": how can this and a -5/3 spectrum both be true?

The "excess power" statement was largely in reference to the interval from $\sim$ 0715 - 0738 which contains excess power with respect to the rest of the interval. This has been clarified on lines 104 - 106.

- line 116: "correlation interval": I would suggest rephrasing this, since to some turbulence people this could have another meaning (it sounds in my view too similar to correlation length).

This is a good point, but in keeping with the established terminology describing the FPC technique (c.f. Klein and Howes (2016), Klein et al. (2020), Afshari et al. (2021)), the correlation interval τ is the time interval over which the time-average for the correlation is computed. Changing terminology for this paper would likely result in even greater confusion.

In summary, I think that with a little revision this should certainly be published, as it is a highly important result with relevance across a broad range of disciplines.

Alfred Mallet

Thank you for recognizing the importance of these results.

2 Review #2

`Reviewer #2 (Remarks to the Author):`

Below is a review of NCOMMS-23-03741 ("Direct observation of ion cyclotron damping of turbulence in Earth's magnetosheath plasma" by Afshari, Howes, Shuster et al.). This paper uses analytical arguments and numerical simulations as guides to interpret correlations between electromagnetic fields and ion velocity distributions measured by the Magnetospheric Multiscale (MMS) mission. In particular, the authors claim to find direct evidence for ion-cyclotron damping in Earth's turbulent magnetosheath, by examining correlations in phase space indicative of high-frequency energization of ions at suprathermal perpendicular velocities by perpendicular electric fields. The paper is interesting and potentially of broad interest, and I am supportive of the article eventually being published in Nature Communications. That being said, I have a number of comments, critiques, and questions that can ultimately be traced to an opinion that the analysis is too incomplete to support some of the stronger claims made in the manuscript, and that the citations to the published literature could be improved. These are enumerated below, with minor comments on the text itself relegated to a separate list. Despite these concerns, I would like to emphasize to the editor that the work reported in this paper is likely to be impactful and highly cited in the heliophysics community.

We thank you for the review and the points made in improving our paper. With our comments below and the improvements made in the manuscript, we hope that we have adequately addressed your concerns.

1. It would be beneficial to show a plot of the ion distribution function, or perhaps of the distribution function after subtracting off a Maxwellian fit (to highlight "delta-f"). This would be not only for completeness – because the distribution function is an ingredient used to computing the field-particle correlation – but also because a number of additional things can potentially be learned from such information. First, published (hybrid-)kinetic simulations of strong Alfvénic turbulence with a focus on ion energization have identified a number of features in the ion distribution function that have been associated with particle-energization mechanisms, e.g., resonant features and quasi-linear flattening near v_prl ∼ v_t,i and/or v_A, non-thermal wings, flattened perpendicular cores, etc. (e.g., Arzamasskiy et al. 2019; Cerri et al. 2021). If the measured energization is indeed due almost entirely to cyclotron heating, it would be useful to see the impact of that energization on the distribution function itself. Or, if stochastic ion heating is not relevant during the interval studied, perhaps that is because the perpendicular distribution function has already been flattened by an earlier episode of stochastic heating at radii closer to the Sun, begging the question, what does f(v_prp) look like? Second, there are predictions for the perpendicular-energy diffusion coefficients associated with stochastic heating and with cyclotron heating (e.g., papers by Kennel & Engelmann, Isenberg & Vasquez, Klein & Chandran, Cerri et al., and others), and knowledge of the distribution function would allow one to compute it: $D^{E_{prp,prp}} = - < dQ\_prp/de\_prp > / < df(e\_prp)/de\_prp >$, where Q_prp is the perpendicular energization of the particles, $e_{prp} = 1/2v_{prp}^2$ is the perpendicular energy, and the brackets indicate some appropriate space-time average. It would be interesting to see how the computed diffusion coefficient scales with e_prp.

The ion distribution function, $f_{0i}(v_\parallel, v_\perp)$, is shown in Fig. 2g, and the reduced ion distribution function, $f_{0i}(v_\perp)$, is shown in Fig. 2h. Though any features (e.g. non-thermal wings, flattened core) are not immediately apparent in either plot, we have included them for completeness. Notably, there is no flattening in the core of the distribution for $v_\perp < v_{ti}$, thus appearing to rule out stochastic ion heating, as now explicitly stated in the paper (lines 133 - 135).

At the suggestion of the reviewer, we have computed the perpendicular-energy diffusion coefficient, $D^E_{\perp\perp}(v_\perp)$, in the likes of Squire et al. (2022), and shown in Figure R1. Theory suggests that $D^E_{\perp\perp}$ should scale as $v_\perp^2$ for an ion cyclotron resonance, but should remain constant for stochastic heating (Kennel and Engelmann (1966)). The results of this analysis do not appear to be particularly informative. It is likely that, because we are only using one 77 s interval with a relatively monochromatic ICW, our data does not have sufficient averaging to yield clear results from this analysis. Therefore, we have chosen to leave this analysis out of the paper.

2. It would also be beneficial to show a more quantitative plot of the electric and magnetic energy spectra (I find figure2a difficult to parse quantitatively). Cyclotron heating by high-frequency KAWs has been shown to steepen the magnetic spectrum near the value of k_prp rho_i where the KAWs attain near-cyclotron frequencies (see, e.g., fig 1 of Cerri et

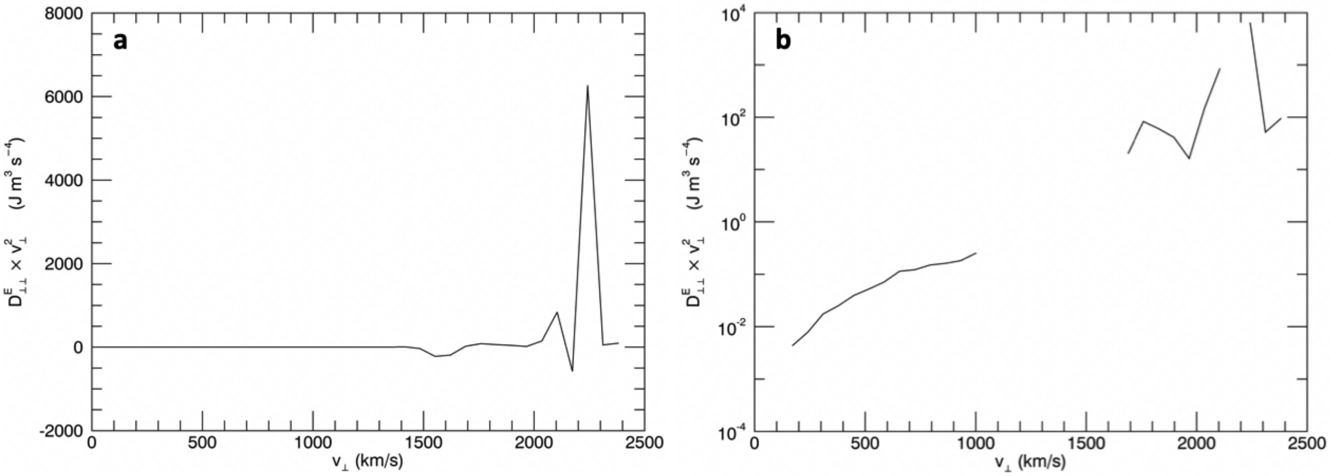

Figure R1: **a** The perpendicular-energy diffusion coefficient, $D^E_{\perp\perp}(v_\perp)$, and **b** same $D^E_{\perp\perp}(v_\perp)$ but with a log-y axis.

al. 2021). There is also evidence in the solar wind that the ion-kinetic-range spectral index correlates with the amount of inferred energy dissipation, with more dissipation correlated with steeper spectra (e.g., Smith et al. 2006). Is there a steepening of the spectrum in the sub-ion-Larmor range near the cyclotron frequency? Another data point on this topic is from Podesta (2009), who associated a rapid decrease in power anisotropy measured in high-speed solar-wind streams near 2 Hz with strong linear dissipation of KAWs occurring at $k_{prp} rho_i \sim 4$; in a 2012 article, he argued that KAWs can couple to ion-Bernstein waves in this wavenumber range, which are strongly damped through a combination of ion-cyclotron and electron-Landau resonances (but see #4 below).

Thank you for the suggestion; we have added the trace energy spectra of the magnetic and electric fields as subpanel **b** in Fig. 2. We have added a -5/3 slope for comparison in the inertial range. Indeed we do observe a slight steepening of the magnetic spectrum near the cyclotron frequency. We hope that the addition of these spectra satisfies this reviewer's request for a more complete presentation.

3. I think the claim that "all significant channels of turbulent dissipation in this interval are identified" is not yet adequately justified. Figure 7 indicates that there's room within the errors for additional significant heating mechanisms; the black diamond is a factor of 2 below the solid line. Line 228 in the manuscript begins a paragraph that acknowledges a possible contribution from stochastic heating, but this contribution is not constrained by the authors. The contribution to particle energization from stochastic heating could be estimated following the method used by Bourouaine & Chandran (2013), Vech et al. (2017), and Martinovic et al. (2019, 2020). I appreciate that stochastic heating at beta $= 1$ would require larger-than-typical fluctuation amplitudes on ion-Larmor scales to be relevant, but the magnetosheath is different than the bulk solar wind, and it's not obvious to me from what the paper presents that stochastic heating is inconsequential. Similarly, line 262 in the manuscript states that parallel energization of ions measured via $< jprl_i Eprl >_{tau}$ is found to be negligible; what about a possible contribution from Barnes damping, which is parallel energization of ions through E_prp? Finally, the authors write (line 273) that the "calculation of C_Eprp and C_Eprl for both ions and electrons captures all possible channels of energy transfer to the particles in a weakly collisional plasma". Technically, this is not correct – there can also be viscous heating, e.g., from the gyrotropic piece of the pressure tensor being correlated with the rate of strain of the plasma motions, viz., Pi:grad(u). There are some in the solar-wind community (though not this referee) who would rather focus on this particular diagnostic at beta $\sim<$ 1 than on field-particle correlations to assess dissipation. Although, even setting those proponents aside, it has been demonstrated that viscous heating is important in weakly collisional, turbulent plasmas at higher values of beta (Arzamasskiy et al. 2022), and so the claim that C_Eprp and C_Eprl capture "all possible channels of energy transfer to the particles in a weakly collisional plasma" isn't strictly true (at least without further qualifiers).

We have made numerous changes to the manuscript to address the comments here. First, we have qualified the "all significant channels" comment to be limited to "all significant channels *at small scales*" (line 76). The turbulent cascade estimate is only accurate to order of magnitude, so one cannot be more precise than a factor of 3 or so, as stated in the manuscript. We have made a significant extension to the Discussion section to assess the possibility of stochastic ion heating: (i) comparison of the velocity-space signatures finds negligible ion energization at $v_\perp < v_{ti}$; (ii) The reduced $f_i(v_\perp)$ shows no evidence of the flattening of the core as is expected for stochastic ion heating; and (iii) We have computed the stochastic ion heating rate using eq. (3) of Bourouaine & Chandran (2013) and found it to be negligible for the parameters of this interval. Ion Landau damping and transit-time damping (TTD) rates are expected to be very small (see Fig 6c), and we also do not see any evidence for energization at $v_\parallel \sim v_A$ in $C_{E_\perp}$ that would be associated with ion TTD. Finally, we have addressed the possibility of anisotropic viscous damping, but find it to be unimportant in this interval, and we have removed the statement that "C_Eprp and C_Eprl capture all possible channels of energy transfer to the particles in a weakly collisional plasma."

4. It should be noted somewhere that the simulations referenced in support of interpreting the observations all adopted Ti/Te = 1 as their initial conditions. A reader may wonder whether the measured ratio Ti/Te = 13 affects any of the interpretation. One valid response is as follows. The linear frequency of oblique kinetic Alfvén waves at beta = Ti/Te = 1 is omega_KAW ~= 0.7 k_prl v_A k_prp rho_i. Taking into consideration the measured values of T_prp,i, T_prl,i, T_e, and beta_i, the linear frequency of a KAW would instead be just slightly smaller, at omega_KAW ~= 0.6 k_prl v_A k_prp rho_i (using equation (3.38) of Kunz et al. 2018). Evidentally, the disparate conditions don't affect much the properties of KAWs; this may be worth mentioning in the supplementary material. Of potentially more serious consequence, though, is...

We have removed the presentation of simulation results in this paper, as the new self-consistent analytical prediction serves as a more relevant confirmation that we observe ion cyclotron damping.

5. The ion temperature anisotropy implied by the reported numbers satisfies (T_prp/T_prl - 1) ~= 1.43. At beta_prp ~= 0.93, this is beyond both the mirror instability threshold and the ion-cyclotron instability threshold. Perhaps the latter is the source of the ion-cyclotron waves responsible for the particle energization? If true, then the reference to the hybrid simulations of Arzamasskiy et al. (2019), Klein et al. (2020), and Cerri et al. (2021) is somewhat dodgy – it's not obvious at all that the high-frequency fluctuations are in fact part of a turbulent cascade. Indeed, the authors note that "the turbulent cascade [in the HVM simulation] self-consistently generates not left-handed polarized ICWs as in the MMS observations, but rather linearly polarized kinetic Alfvén waves." This referee is wondering whether all this talk of turbulent cascades is a red herring – ICWs can be generated purely from the unstable temperature anisotropy of the background. This, of course, doesn't take away from the novel measurement of ion-cyclotron heating, which I support being reported, but it would change the narrative greatly, from the first two words of the Abstract to the last sentence of the Discussion section. At the very least, some discussion of where these left-handed ICWs are coming from would be useful.

Although we are unable to determine the origin of the observed ICWs, we do observe that they are damping in this interval. We have added speculation about their origin in the paper (lines 120 - 125), and have gone into detail analyzing the linear dispersion relation and damping/growth rates using a Vlasov-Maxwell linear dispersion relation solver in the newly created Supplementary Information (SI), with specific discussion of issues related to this in SI Sec. S1–S4.

6. The velocity-space resolution of the MMS data in Fig 3(b) – with dv/v_t,i = 0.2 – looks poorer than the resolution implied by figure 1 of Chen, Klein & Howes (2019) for the electron distribution function. Why is that? Also, Chen, Klein & Howes (2019) showed the alternative field-particle correlation C', in addition to C. I'd like to encourage the authors to show C'_E_prp alongside C_E_prp, if not in the main text then perhaps in the supplementary material. The noise would be reduced, since no derivatives need to be computed, and it would also be a useful data point for those theorists who prefer C' over C because calculating derivatives of distribution functions obtained from PIC simulations can be a noisy affair.

First, Chen et al. (2019) analyzes electrons and we are analyzing ions here, so that certainly leads to some unavoidable changes in the velocity resolution of the measurements relative to each species thermal velocity. Fig. 1 of Chen et al. (2019) has a resolution of dv/v_t,e = 0.1. We have used a lower resolution, with dv/v_t,i = 0.2, due to the step-wise increase in energy coverage of FPI DIS producing large data gaps if a finer resolution is chosen. A resolution of dv/v_t,i = 0.2 is adequate to resolve the physics we seek.

We have added the alternative field-particle correlation $C'_{E_\perp}(v_\parallel, v_\perp)$ side-by-side with the field particle correlation $C_{E_\perp}(v_\parallel, v_\perp)$ in SI Fig. S7, as requested. As you have mentioned, there is less noise in $C'_{E_\perp}(v_\parallel, v_\perp)$ than in $C_{E_\perp}(v_\parallel, v_\perp)$.

7. Finally, I would like to see the authors engage more with the recent PRL by Bowen et al. entitled "The In Situ Signature of Cyclotron Resonant Heating". While that paper doesn't compute a field-particle correlation, it does present

evidence for flattening in the phase-space distribution in a specific way predicted by resonant quasilinear diffusion in ion-cyclotron waves, as well as steepening in the turbulent spectra at the ion-cyclotron-resonant scale (cf. points #1 and #2 above). In light of the authors' statement (line 214) that their FPC-driven "lines of evidence consitute [sic] the first direct measurement of ion cyclotron damping in a turbulent space plasma", a comparison of these two papers has me wondering what is indeed more "direct" evidence: field-particle correlations without an analysis of the distribution function and electromagnetic spectra, or a demonstration that the distribution function flattens along contours in a predicted way at the same time that the field spectra steepen. If the authors could provide an analysis of the electromagnetic spectrum and the distribution function to accompany their novel FPC analysis, the paper would be much more complete and notable.

As we have stated earlier in response to this reviewer's comments, we have now added electromagnetic spectra and the ion distribution function plots to provide all of the relevant supporting data. We have elucidated in more detail the approach used by Bowen et al (2022) to analyze ion cyclotron damping in the introductory literature review (lines 65 - 70). Our approach, by contrast, has been explained more precisely by the statement"Together, these lines of evidence constitute the first measurement of ion cyclotron damping in a turbulent space plasma through a direct determination of the work done by the perpendicular electric field on the ions" (lines 294 - 296). By "direct", we mean that we are directly measuring the work done on the ions by the perpendicular electric field, without any of the assumptions required by quasilinear theory (e.g., a (possibly narrow band) spectrum of randomly phased fluctuations shaping the velocity distribution on a much longer timescale than the wave period.

Minor points:

A. For the benefit of the reader, and as is appropriate for a journal like Nature Communications, it would be useful to define the field-particle correlation in simple descriptive words within the main part of the manuscript. The authors point to the supplementary Methods section for "the detailed analysis procedure", but at the moment there isn't even a not-detailed statement of what the FPC is – as far as I can tell, C' and C appear on page 7 without explanation.

We have added descriptive words to define the FPC technique (lines 140 - 143), and have added the equation for C' (line 148).

B. The greyish edge on the left-hand side of Fig 3 suggests that this figure was grabbed from a screen shot with a shadow from a neighboring window overcast. I think this panel could be better prepared. Also in this figure, panel (c) has v_prl/v_t,i ranging from -4 to +4, but panel (b) has v_prl/v_t,i ranging from -2 to +2. This doesn't seem like a fair comparison.

Figure 3 has been better prepared: no greyish edge on the left-hand side, the parallel and perpendicular axes in panel (b) and the perpendicular axis in panel (c) have been normalized by their respective thermal speeds and now match panel (a).

C. Line 119: Unless I'm mistaken, the argument of C'_E_j should be $\tau = 0$ rather than just τ. Also, on line 121, no "s" is needed after $\tau = 0$, since 0=0 in any units (and it's not immediately clear that an italic s means seconds here).

Thank you for pointing this out. It has been corrected.

D. The sub-panels in Fig 3 are in a different order than they are introduced in the text. Likewise with Fig 4. Please reorder the figure panels or the text so that the narrative is consistent.

The sub-panels in Fig. 3 have been re-ordered and the corresponding text (lines 156 - 165) has been appropriately updated to reflect this change. The text for Fig. 4 has been re-ordered so that the narrative is consistent (lines 166 - 184).

E. Lines 129-130: It's not clear yet at this point in the manuscript that the energization highlighted in Fig 3(a) is consistent with ion-cyclotron damping, as claimed, because the predicted signal for cyclotron damping has not yet been given, and there could also be a contribution from stochastic heating near v_prp/v_t,i $\sim$ 1. Perhaps some re-ordering of the text would help the logical flow here.

Fig. 3 has been re-ordered to first introduce the 2V gyrotropic velocity-space signature of ion cyclotron damping, and the text has been updated (156 - 165). Stochastic heating would be present as the flattening of the core of the distribution, which is not seen in the newly added subpanel Fig. 2h of the reduced ion distribution, and mentioned on lines 133 - 135.

3 Review #3

Reviewer #3 (Remarks to the Author):

This paper presents measurements and analysis of collisionless plasma heating via wave-particle interactions in the Earth's magnetosheath. It uses the so-called Field Particle Correlation (FPC) technique developed by some of the authors to diagnose the heating in one interval as resulting from ion-cyclotron waves. The evidence for this involves the shape of the FPC distribution, as well as comparisons to a simple model and numerical simulations. They then measure the dissipation rate, comparing this to a previous measurement of the electron damping rate during the same interval, as well as the ion Landau damping rate. This shows that the ICW damping dominates, with a heating rate that broadly matches the inferred cascade rate over this interval. The implication is that ions are heated perpendicularly and preferentially over electrons.

The paper is well written and the basic point that ICW damping dominates in this interval is convincing. I also believe that if a broader analysis was done including more intervals, the general result would be exciting and important to the wider community, and therefore warrant publication in a high-profile journal such as nature communications. However, I found a number of aspects of analysis somewhat unconvincing, and I believe these issues to be sufficiently serious that further analysis, or at least reworking, is necessary before publication. The authors are welcome to offer a rebuttal if they believe I am mistaken on some of the more technical points.

Thank you for the careful review and criticisms of our manuscript. We hope our discussions below will be sufficiently convincing and will warrant your approval for publication.

- A fair amount of the paper concerns the comparison with the "model" (figures 5-6). This is used as evidence that it is indeed ICW damping that is being observed. I do not believe that this model provides the evidence claimed. In particular, as I understood it, the model takes the measured waveforms of U_prp and E_prp (or at least an approximation thereof), assumes a Maxwellian VDF with the measured density and temperature, then plugs these into the FPC formula. They then make a map of the FPC and compare it to the data. But, since the FPC formula is linear in f_s and E_prp, this effectively amounts to replacing the Eprp and Uprp with sinusoidal fits, and the f_s with a Maxwellian. So, as I can understand it, the comparisons in figures 5-6 are only showing that f_s is of a relatively similar shape to a shifted Maxwellian, because the Eprp and Uprp are taken from an approximate fit to the data anyway. So, by itself, how can this constitute proof that ion cyclotron waves are involved? It simply approximates the two pieces of the FPC separately by fitting a time-varying Eprp and Uprp. I would think that to prove ICW involvement, it is necessary to solve for the plasma's response, as mentioned in the methods. This would simply add a fixed phase relationship between Uprp and Eprp, which can presumably be assessed without looking at the distribution function anyway.

We appreciate this comment, and are now using the self-consistently determined plasma response from a Vlasov-Maxwell linear dispersion relation solver to predict the perpendicular velocity-space signature of ion cyclotron damping using an improved version of the analytical model. Once the plasma parameters and wave vector (which is estimated from observational constraints in the newly added Supplementary Information (SI) Sec. S3) is specified, the only free parameter in this model is the overall amplitude of the wave, which we choose to best match the observations. All of the amplitude and phase relationships between the perpendicular electric field and perpendicular ion fluid velocity are specified by the eigenfunction computed from the linear dispersion relation, so this provides much more convincing proof that the observed perpendicular velocity-space signature identifies the mechanism as ion cyclotron damping.

- In a similar vein, I found the comparison to the simulations to be unconvincing. They show the opposite phase relationship, and as explained in the previous point, I do not believe this necessarily to be evidence of ion cyclotron damping (certainly it's a different form of IC damping). Again, real evidence of the claim would have to make reference to the plasma's susceptibility in some form, and would presumably indicate the presence of IC damped KAWs, as stated in the article. But, as it stands, does it not also just show that f_s doesn't deviate dramatically from a Maxwellian?

As pointed out by all reviewers, the simualation results were not very relevant to understand the MMS observations, so we have removed the discussion of the numerical simulations from the paper, especially since the analytical prediction of the perpendicular velocity-space signature now provides strong evidence that we are observing ion cyclotron damping in the MMS interval.

- More generally, what is the purpose of a comparison to the numerical simulation, since it's shown explicitly to be quite different to what is observed? I did not understand what we were supposed to learn, aside from the fact that the two damping mechanisms (in MMS and the simulation) showed a similar FPC signature in figure 3, despite being physically different (as shown in figs 5-6). This may be an important point, since it shows that sometimes 3D velocity space is needed for distinguishing power in the FPC, but this aspect didn't seem to be explicitly emphasized and is subsidiary to the main purpose of the paper.

As stated above, we have removed the numerical simulations.

- I'm a bit skeptical of the claims in the conclusion that this is the "first direction measurement of ICW damping of turbulence". What about the Bowen et al. 2022 paper discussed in the introduction. They show that the plasma is on average damping ICWs, that the ICWs exist (presumably arising from the turbulence), and they measure a heating rate – in what sense is this more direct?

In response to this comment and that of another reviewer, we have clarified precisely what we mean by more "direct" in the manuscript. By "direct", we mean that we are directly measuring the work done on the ions by the perpendicular electric field, without any of the assumptions required by quasilinear theory (e.g., a (possibly narrow band) spectrum of randomly phased fluctuations shaping the velocity distribution on a much longer timescale than the wave period. Furthermore, we have qualified that statement in question to be more precise, "Together, these lines of evidence constitute the first measurement of ion cyclotron damping in a turbulent space plasma through a direct determination of the work done by the perpendicular electric field on the ions" (lines 294 - 296).

In addition, in their work, Bowen *et al.* (2022) used ion distribution data from Parker Solar Probe's (PSP) SPANi instrument which provides relatively sparse measurements in the $v_\perp - v_\parallel$ plane when compared to ion distribution data from MMS' FPI DIS instrument. Their results use interpolations of ion distribution measurements in the $v_\perp - v_\parallel$ plane, where-as our results use the full ion distribution measurements which need not be interpolated. We make use of the true measured distribution values without any interpolation, and in this sense our results are more direct.

- Overall, especially given that the application of the FPC to MMS data has already been presented by these authors in nature comms, I would find the paper a lot more compelling if many intervals were analysed, as in Afshari et al 2021. Presumably in a single interval many things can be found, but the statistical behavior is of much more general interest.

Our current work provides new insight into the nature of dissipation of turbulent energy through ion cyclotron damping, so it is distinct from Chen *et al.* (2019), who also analyzed only one interval of MMS data by applying the FPC technique to identify electron Landau damping. Even with the single interval analyzed, this paper contains a number a very significant results: (i) the first identification of ion cyclotron damping in a space plasma by computing the work done on the ions by the perpendicular electric field; (ii) an observational determination of the gyrotropic velocity-space signature of ion cyclotron damping, $C_{E_\perp,s}(v_\parallel, v_\perp; \tau)$, confirming earlier results from numerical simulations by Klein et al. (2020); (iii) the discovery of a new distinctive perpendicular velocity-space signatures $C_{E_{\perp 1}}(v_{\perp 1}, v_{\perp 2})$ and $C_{E_{\perp 2}}(v_{\perp 1}, v_{\perp 2})$ that can be further used to confirm ion cyclotron damping; (iv) the first observational study the identifies two separate channels of turbulent dissipation in the same interval, with the sum of the dissipation rates agreeing to order of magnitude with the estimated turbulent cascade rate, suggesting we have captured all significant channels of turbulent dissipation at small scales. A statistical analysis in the likes of Afshari *et al.* (2021) is in store, but to include all of the fundamental information describing the analysis technique for a new, observationally identified dissipation mechanism (ion cyclotron damping) along with the information required to present a moderate sized statistically study would be much too long for a single Nature Communications paper. Thus, we leave a thorough analysis of many intervals for future work.

- A minor point, but I am confused by how the ion Landau damping can be so small, less than 100 times that of the electron Landau damping. Since this interval has beta 1, this seems quite unexpected considering e.g., the Howes+ 2008 heating model. Certainly it is reasonable that ICW damping and electron LD could be larger, but shouldn't we still expect modest ion LD of KAWs at such parameters, given that the KAWs must proceed through sub-rhoi scales (where they are modestly damped), in order to reach electron scales? Some comment on this surprising result would be helpful.

As shown in the new Fig 6c, the ion Landau damping rate for these parameters happens to be much smaller than the ion cyclotron damping rate, so in fact our observational findings are completely consistent with expectations from the Vlasov-Maxwell linear dispersion relation.

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REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

First, I would like to apologize for the lateness of my review.

The changes made by the authors have fully satisfied my previous questions and comments: I especially prefer the new model to the one that appeared in the previous version. I only have two minor additional comments

1) Given the link to turbulent dissipation, it could be worth mentioning the recent "helicity barrier" model (Meyrand et al. 2021, Squire et al. 2022) as a potential mechanism for generation of ICW (if only to perhaps rule it out in this case: is the turbulence relatively balanced here?)

2) I don't think it is stated anywhere how precisely epsilon in fig. 7 is being calculated. To make this paper stand alone better, it could be worth doing this explicitly (e.g. Eq 10 in Afshari et al. 2021).

I think these two changes would improve the paper marginally for little effort, but the authors should feel free to ignore these suggestions. I am happy with the paper as it stands!

Reviewer #2 (Remarks to the Author):

I believe the manuscript has been significantly improved following all three referee reports. Aside from some split infinitives, which the highly skilled copy editors will surely correct, I see just three very minor issues whose fixing might improve the presentation but aren't necessary for the editor to accept the paper.

(1) The authors find that, given the measured parameters during the interval in question and assuming a bi-Maxwellian distribution, the ICWs should be unstable. Instead, the authors present evidence that these ICWs are damped. To get the linear solver to comply with this result, they artificially set Tprp/Tprl = 1 in PLUME so that the distribution is linearly stable and so that they can calculate a decay rate. This seems like a rather dodgy dodge; the measured parameters are what they are… why change them? I think it's clear that the reason the waves aren't unstable in the measured interval is because they've already grown and changed the form of the distribution to be flattened along the resonance contours, i.e., the VDF isn't bi-Maxwellian. Indeed, the authors write "These [cyclotron-resonant] contours serve as a qualitative guide along with the iVDF appears to be flattened, which is an indication of ICWs pitch-angle scattering the iVDF through cyclotron resonance." The authors do acknowledge that adopting a Tprp/Tprl = 1 Maxwellian in PLUME for the sake of achieving a desired result is artificial, but this is really only admitted in the supplemental material: "Therefore, it is possible that the ion velocity distributions in the magnetosheath plasma are not well approximated by the idealized bi-Maxwellian form assumed in the PLUME solver, leading to a difference in the resulting collisionless damping or growth rates." I think this should be admitted in the main text; it's obvious from Fig 2g that the iVDF isn't an isotropic Maxwellian, and so using "probable" or perhaps even "certain" would be more accurate than "possible" in the above quote. [On this note… why not use co-author Klein's ALPS code and feed it the measured iVDF, smoothed as needed? It seems this would be better than adopting an irrelevant isotropic Maxwellian and then decomposing the resulting growth/decay curve into various damping contributions (Fig 6) that may or may not be representative of the real interval. Is ALPS not working well with the measured iVDF? Perhaps the authors didn't attempt using it on this iVDF? Anyway, not a big deal… I'm just curious. Such additional work isn't necessary for the current manuscript to be accepted for publication.]

(2) The authors overlay in Fig 2h a "fit" Maxwellian distribution to the measured iVDF and state that, because the core of the iVDF isn't significantly different from the "fit", then stochastic heating (which would've flattened the core) must not have been operational. This seems a bit misleading, though. Fig 2h looks like the authors prioritized fitting the core with a Maxwellian, so that the biggest difference between the two curves happens not at those data points in the core but rather around $v_{\perp,ti} \approx 343$ km/s, where the Maxwellian "fit" over-predicts f. An arguably more informative fit would be to the $v_\perp >= v_{\perp,ti}$ part of the distribution function, which I'm guessing would show that the core

measured iVDF is flatter than what a core fit would give. I agree that the measured iVDF doesn't look particularly flattened in the core, but I do think the more fair comparison would fit the iVDF near the thermal speed rather than at those two core points.

(3) This is extremely, extremely minor... In fig 3(a), v_{ti} is used to normalize the ordinate and abscissa. In fig 3(b) and 3(c), v_{\perp ti} and v_{\parallel ti} are used. The reason for fig 3(a), I've gathered, is that the simulation involved artificially exciting ion-cyclotron fluctuations in a plasma whose iVDF was initially Maxwellian, rather than self-consistently exciting the ion-cyclotron waves through an instability driven by having v_{\perp ti} > v_{\parallel ti}. Not a big deal, but v_{ti}/v_{\parallel ti} in the measured interval is \simeq 1.4, which could explain why the observed C_Eprp in panel (b) is noticeably thinner than the simulated C_Eprp in panel (a). For the purposes of comparing panels, might it be better to normalize v_\perp in panels (b) and (c) to the isotropic temperature? Maybe, maybe not. I'm just raising this issue because I didn't notice at first that the different panels use different normalizations on their axes, then I wondered if there might be a typo.

Reviewer #3 (Remarks to the Author):

Thanks to the authors for taking my concerns, and those of the other referees, seriously. The analysis is more convincing now with the new analytical model, and narrative is improved by the removal of the numerical simulations. I have a few lingering concerns about aspects of the manuscript, including a couple of the changes, but I believe these will be easily addressed.

- Presumably, given the clear wave-signature of the ICWs and the polarization signature in figure 2c, the ICW population is quite imbalanced, i.e., dominated by waves propagating in one direction? There is some discussion of this in the supplementary material, but it would be nice to emphasise it more in the manuscript. The main reason for interest is that it could give a useful hint as to the origin of the ICWs, which is discussed extensively in the manuscript; so it is useful information for the reader. Presumably an instability creates a balanced ICW population, although not if they propagate over from elsewhere (though they would have to do so without damping). There is a lot of discussion of the wavevector and bow shock in the SI, but I couldn’t immediately see whether their direction of propagation was consistent with them having propagated from the bow shock (i.e., from a possibly unstable regio; since va>U, the propagation should be more important than the advection). Another possibility is that they are driven indirectly by the turbulence and oblique ICWs through the mechanism of Chandran et al. ApJ 722 710 (2010), which would mean their direction should be correlated with any imbalance of the large-scale Alfvenic turbulence (as seen in Squire et al. 2022). Is this the case? A little discussion would be helpful along with the numerical value of the imbalance. It would also be important to mention the form of the ion VDF in figure 2G, which seems to indicate that both wave directions have shaped the VDF.

- Related to the previous comment, at the end there's a nice comparison of the damping rate to the cascade rate. Why is this relevant if the ICWs were sourced by upstream instabilities? Why would we expect a relationship between the turbulent cascade and the ICW damping in this case, since they arise from separate processes? It seems a bit logically inconsistent to claim both (i) the importance of the agreement of the cascade rate with the ion energization rate and (ii) that the ICWs likely arise from upstream instabilities. More discussion would be helpful.

- As far as I noticed, the directions vprp1 and vprp2 are introduced in the main text without being defined. It would help to add (see Methods) or a little more explanation.

- In the manuscript and in the response to my first report, it's stated that the small ion-Landau damping FPC contribution is consistent with the small predicted damping rates and expectations based on figure 6c. Either I'm missing something or this is not a meaningful comparison. Figure 6 plots the damping rate for Alfven waves at a specific kprp at kprp*rhoi<<1 (kprp*di=0.016). Such waves are always undamped by Landau resonances, including at beta>>1. But, in the Howes+ 2008 model, the cascade takes power to kprp*rhoi~1, where Alfvenic modes are strongly damped at beta~1 or high beta, thus damping significantly into ion heat. The FPC diagnostic would pick up this damping, not that of large-scale modes, thus (at higher beta) registering ion Landau damping into heat. So figure 6 doesn't seem relevant to this discussion about the damping rates of the cascade.

As for the question of whether the small ion Landau damping is actually surprising, certainly the high Ti/Te should be expected to push down Qi/Qe (from Landau) somewhat, so maybe it isn’t. But still, the claim it’s at least two orders of magnitude higher seems extreme and worth mentioning.

- Is there a reason to choose the particular kprp*di = 0.016 for all of the linear analysis? Maybe I just missed it, but if not, it would be nice to discuss a bit why this was chosen and/or how the results depend on the choice.

- Minor points about normalization: in SI, figure S2 uses units of kprl*rhoi, whereas earlier discussion is framed with kprl*di. The latter seems more sensible to me, but either way it would be better to be consistent unless there’s a good reason I’m missing. Likewise, in the main text it uses kprp*di and krpl*di units, while kprp*rhoi and kprl*di units seem more sensible based on the physics of linear waves.

- line 226 of SI “The we”

Response to Reviews of Manuscript NCOMMS-23-03741B

"Direct observation of ion cyclotron damping of turbulence in Earth's magnetosheath plasma"

We once again thank the three referees for taking the time to review our paper and provide feedback in order to improve our paper. Below are our responses (in blue text) to each of the reviewer comments (in black). Separately, the revised manuscript has changes highlighted in blue.

We have addressed all of the reviewers comments and improved our paper through their suggestions. The paper now includes all elements for a standalone study, and the information contained in the supplementary information is pointed to at appropriate times in the text. As per Reviewer #2's request, we have completed substantial work in applying the ALPS analysis to the measured iVDFs (a heretofore unprecedented work specially done for this study).

We hope that these major revisions now render this paper acceptable for publication.

1 Review #1

`Reviewer #1 (Remarks to the Author):`

First, I would like to apologize for the lateness of my review.

The changes made by the authors have fully satisfied my previous questions and comments: I especially prefer the new model to the one that appeared in the previous version. I only have two minor additional comments

Thank you for your review.

1) Given the link to turbulent dissipation, it could be worth mentioning the recent "helicity barrier" model (Meyrand et al. 2021, Squire et al. 2022) as a potential mechanism for generation of ICW (if only to perhaps rule it out in this case: is the turbulence relatively balanced here?)

The large-scale turbulence here is not substantially imbalanced, although with our specific focus on the ion cyclotron damping we have not included that Poynting flux analysis here. In order to keep focused on the physics of ion cyclotron damping of turbulent fluctuations and not the generation of these ICWs, we have chosen not to include these references. Though this interval contains many interesting phenomena, they are beyond the scope of this manuscript.

2) I don't think it is stated anywhere how precisely epsilon in fig. 7 is being calculated. To make this paper stand alone better, it could be worth doing this explicitly (e.g. Eq 10 in Afshari et al. 2021).

Great suggestion. We have added a note on lines 326 - 327 to find the explicit equation for ϵ in the Methods section, under the newly added Turbulent Energy Cascade Model subsection.

I think these two changes would improve the paper marginally for little effort, but the authors should feel free to ignore these suggestions. I am happy with the paper as it stands!

Thank you again for your review and suggestions. We trust that our newly revised manuscript is acceptable for publication.

2 Review #2

`Reviewer #2 (Remarks to the Author):`

I believe the manuscript has been significantly improved following all three referee reports. Aside from some split infinitives, which the highly skilled copy editors will surely correct, I see just three very minor issues whose fixing might improve the presentation but aren't necessary for the editor to accept the paper.

Thank you for your review. We have corrected the split infinitives. We hope that with the following improvements, the paper is now acceptable for publication.

(1) The authors find that, given the measured parameters during the interval in question and assuming a bi-Maxwellian distribution, the ICWs should be unstable. Instead, the authors present evidence that these ICWs are damped. To get the linear solver to comply with this result, they artificially set Tprp/Tprl = 1 in PLUME so that the distribution is linearly stable and so that they can calculate a decay rate. This seems like a rather dodgy dodge; the measured parameters are what they are... why change them? I think it's clear that the reason the waves aren't unstable in the measured interval is because they've already grown and changed the form of the distribution to be flattened along the resonance contours, i.e., the VDF isn't bi-Maxwellian. Indeed, the authors write "These [cyclotron-resonant] contours serve as a qualitative guide along with the iVDF appears to be flattened, which is an indication of ICWs pitch-angle scattering the iVDF through cyclotron resonance." The authors do acknowledge that adopting a Tprp/Tprl = 1 Maxwellian in PLUME for the sake of achieving a desired result is artificial, but this is really only admitted in the supplemental material: "Therefore, it is possible that the ion velocity distributions in the magnetosheath plasma are not well approximated by the idealized bi-Maxwellian form assumed in the PLUME solver, leading to a difference in the resulting collisionless damping or growth rates." I think this should be admitted in the main text; it's obvious from Fig 2g that the iVDF isn't an isotropic Maxwellian, and so using "probable" or perhaps even "certain" would be more accurate than "possible" in the above quote. [On this note... why not use co-author Klein's ALPS code and feed it the measured iVDF, smoothed as needed? It seems this would be better than adopting an irrelevant isotropic Maxwellian and then decomposing the resulting growth/decay curve into various damping contributions (Fig 6) that may or may not be representative of the real interval. Is ALPS not working well with the measured iVDF? Perhaps the authors didn't attempt using it on this iVDF? Anyway, not a big deal... I'm just curious. Such additional work isn't necessary for the current manuscript to be accepted for publication.]

In performing additional analysis in response to this comment, we have come to recognize that "the measured parameters are what they are" may simply not be true, due to instrumental effects on the measurements. The particular issue is that of "apparent temperature," first addressed in a publication by Verscharen and Marsch (2011). The issue is simply that, if there is significant wave activity with motion of the plasma perpendicular to the magnetic field, as is certainly the case for both Alfvén and ion cyclotron waves, that motion can lead to an artificial spreading out of the measured perpendicular velocity distribution over the interval of time over which the velocity distribution is measured, yielding a higher perpendicular temperature than actually exists in the plasma. Following the reviewer's suggestion, we have used the ALPS dispersion relation solver to model the instability growth rate. By decreasing the time interval over which the ion VDF measurement is constructed, we find that the growth rate indeed decreases, suggesting that the "apparent temperature" over longer timescales leads to an apparently more unstable plasma. We have added this ALPS analysis to Section S2 of the Supplementary Information along with the discussion of apparent temperature in the main text on lines 212 - 216.

An additional issue, now also mentioned in the Supplementary Information, is how the instability growth rates compare to the wave periods in the plasma. If temperature anisotropy instabilities are driven by large-scale motions (such as compressions), the growth rates of the unstable modes, which are typically on ion cyclotron timescales, are usually much faster than the timescales of the large-scale motions, so that unstable waves can grow under relatively static conditions. In this observed interval, however, the measured periods of the ion cyclotron waves are actually faster than the unstable wave growth rates, so the growth rates calculated from the linear dispersion relation, which assume a static background, may be quantitatively altered in the presence of waves that oscillate more rapidly than the unstable wave growth rate. Thus, the growth or damping rates in a turbulent plasma may not be the same as those as calculated by the Vlasov-Maxwell linear dispersion relation (calculated by PLUME or ALPS), which assumes static conditions.

We have added a brief listing of these discussion points in the main text in the *Analytical model of ion cyclotron damping* section on lines 212 - 219 with a reference to Section S2 of the Supplementary Information.

(2) The authors overlay in Fig 2h a "fit" Maxwellian distribution to the measured iVDF and state that, because the core of the iVDF isn't significantly different from the "fit", then stochastic heating (which would've flattened the core) must not have been operational. This seems a bit misleading, though. Fig 2h looks like the authors prioritized fitting the core with a Maxwellian, so that the biggest difference between the two curves happens not at those data points in the core but rather around $v_{\perp,ti} \approx 343$ km/s, where the Maxwellian "fit" over-predicts f. An arguably more informative fit would be to the $v_\perp >= v_{\perp,ti}$ part of the distribution function, which I'm guessing would show that the core measured iVDF is flatter than what a core fit would give. I agree that the measured iVDF doesn't look particularly flattened in the core, but I do think the more fair comparison would fit the iVDF near the thermal speed rather than at those two core points.

We have updated Fig. 2h with a curve fit function that minimizes the sum of the squared difference between a Gaussian fit and the distribution data. We trust that this is a more fair comparison between the fit and the data, especially since at no point have we prioritized fitting the Gaussian to a region of velocity space (e.g. neither in the core nor in the tail). Now, the Gaussian fit marginally underpredicts the measured distribution where $v_\perp >= v_{\perp,ti}$, yet we see no flattening in the core. As we stated previously and our esteemed reviewer has stated "the measured iVDF doesn't look particularly flattened in the core".

(3) This is extremely, extremely minor... In fig 3(a), v_{ti} is used to normalize the ordinate and abscissa. In fig 3(b) and 3(c), $v_{\perp ti}$ and $v_{\parallel ti}$ are used. The reason for fig 3(a), I've gathered, is that the simulation involved artificially exciting ion-cyclotron fluctuations in a plasma whose iVDF was initially Maxwellian, rather than self-consistently exciting the ion-cyclotron waves through an instability driven by having $v_{\perp ti} > v_{\parallel ti}$. Not a big deal, but $v_{ti}/v_{\parallel ti}$ in the measured interval is $\simeq 1.4$, which could explain why the observed C_Eprp in panel (b) is noticeably thinner than the simulated C_Eprp in panel (a). For the purposes of comparing panels, might it be better to normalize $v_\perp$ in panels (b) and (c) to the isotropic temperature? Maybe, maybe not. I'm just raising this issue because I didn't notice at first that the different panels use different normalizations on their axes, then I wondered if there might be a typo.

It is indeed not a typo that we have normalized the abscissa and ordinate by their respective thermal speeds. As you have mentioned, the simulation is Maxwellian with $v_{\perp ti} = v_{\parallel ti}$, thus it is appropriate to normalize the axes by the isotropic thermal speed. In the magnetosheath interval we have analyzed, $v_{\perp ti} \neq v_{\parallel ti}$. While having anisotropic thermal speeds does change the scaling of the axes, it is representative of the data.

3 Review #3

`Reviewer #3 (Remarks to the Author):`

Thanks to the authors for taking my concerns, and those of the other referees, seriously. The analysis is more convincing now with the new analytical model, and narrative is improved by the removal of the numerical simulations. I have a few lingering concerns about aspects of the manuscript, including a couple of the changes, but I believe these will be easily addressed.

Thank you once again for your review. Below we have addressed your concerns and hope that the updated manuscript is acceptable for publication.

- Presumably, given the clear wave-signature of the ICWs and the polarization signature in figure 2c, the ICW population is quite imbalanced, i.e., dominated by waves propagating in one direction? There is some discussion of this in the supplementary material, but it would be nice to emphasise it more in the manuscript. The main reason for interest is that it could give a useful hint as to the origin of the ICWs, which is discussed extensively in the manuscript; so it is useful information for the reader. Presumably an instability creates a balanced ICW population, although not if they propagate over from elsewhere (though they would have to do so without damping). There is a lot of discussion of the wavevector and bow shock in the SI, but I couldn't immediately see whether their direction of propagation was consistent with them having propagated from the bow shock (i.e., from a possibly unstable region; since va>U, the propagation should be more important than the advection). Another possibility is that they are driven indirectly by the turbulence and oblique ICWs through the mechanism of Chandran et al. ApJ 722 710 (2010), which would mean their direction should be correlated with any imbalance of the large-scale Alfvenic turbulence (as seen in Squire et al. 2022). Is this the case? A little discussion would be helpful along with the numerical value of the imbalance. It would also be important to mention the form of the ion VDF in figure 2G, which seems to indicate that both wave directions have shaped the VDF.

Although the measured ICWs themselves in our interval are rather unidirectional (as the polarization signature in figure 2c suggests), the large-scale turbulent fluctuations are quite balanced, so the physics of the helicity barrier (Squire et al. 2022) is not relevant here. Furthermore, the model of Chandran et al. ApJ 722 710 (2010) applies only to asymptotically low beta turbulence (the focus of that paper was the physics in solar corona), and our parallel plasma beta here has a value of $\beta_{\parallel i} = 0.38$, not satisfying the limit needed for that application of that model. Given that the focus of this paper is the direct measurement of the damping of turbulent fluctuations by the ion cyclotron resonance, we do not feel that a more expanded discussion of the source of the ICWs will contribute substantially to the manuscript (especially since the ICWs are likely to have been generated elsewhere and arrived at the measurement location through a combination of propagation and advection, so the measurements may not directly reflect the conditions where the ICWs were generated). We have placed some of these considerations (such as our analysis of the upstream bow shock, which turned out to be inconclusive) into the Supplementary Information, but further speculation on the source of the ICWs does not add significant value to a paper that focuses in the local and direct observations of the damping of these ICWs.

- Related to the previous comment, at the end there's a nice comparison of the damping rate to the cascade rate. Why is this relevant if the ICWs were sourced by upstream instabilities? Why would we expect a relationship between the turbulent cascade and the ICW damping in this case, since they arise from separate processes? It seems a bit logically inconsistent to claim both (i) the importance of the agreement of the cascade rate with the ion energization rate and (ii) that the ICWs likely arise from upstream instabilities. More discussion would be helpful.

We appreciate that there are indeed subtle aspects to assessing the turbulent cascade rate and energy dissipation rate when kinetic instabilities mediate nonlocal energy transfer from large to small scales. We have addressed this issue at the end of the first paragraph of the *Channels of turbulent energy dissipation* section (lines 314 - 322) with the following clarifying text:
Here we propose a working definition of the turbulence as *all* of the physical mechanisms that serve to mediate the conversion of the energy of large-scale plasma flows and electromagnetic fields into heat of the plasma species, including both the local energy transfer by the turbulent cascade and any nonlocal energy transfer via kinetic instabilities. We adopt this definition because, in a practical sense, it is not generally possible to separate observationally whether turbulent fluctuations were driven by local or nonlocal energy transfer. Our measure of the cascade rate ϵ based on the turbulent amplitudes includes fluctuations from both sources, so the observed dissipation rate may be compared to this turbulent cascade rate.

- As far as I noticed, the directions vprp1 and vprp2 are introduced in the main text without being defined. It would help to add (see Methods) or a little more explanation.

On line 191 we have added the note "see Methods for definition of $(v_{\perp 1}, v_{\perp 2})$".

- In the manuscript and in the response to my first report, it's stated that the small ion-Landau damping FPC contribution is consistent with the small predicted damping rates and expectations based on figure 6c. Either I'm missing something or this is not a meaningful comparison. Figure 6 plots the damping rate for Alfven waves at a specific kprp at kprp*rhoi<<1 (kprp*di=0.016). Such waves are always undamped by Landau resonances, including at beta>>1. But, in the Howes+ 2008 model, the cascade takes power to kprp*rhoi∼1, where Alfvenic modes are strongly damped at beta∼1 or high beta, thus damping significantly into ion heat. The FPC diagnostic would pick up this damping, not that of large-scale modes, thus (at higher beta) registering ion Landau damping into heat. So figure 6 doesn't seem relevant to this discussion about the damping rates of the cascade. As for the question of whether the small ion Landau damping is actually surprising, certainly the high Ti/Te should be expected to push down Qi/Qe (from Landau) somewhat, so maybe it isn't. But still, the claim it's at least two orders of magnitude higher seems extreme and worth mentioning.

We regret that we were not as careful as needed in specifying that Fig. 6 indicates that the observed *ion cyclotron wave* will not be strongly damped by the Landau resonance (Landau and transit-time damping); we have added a phrase in the text to make this clear (line 227). As well, it is indeed true that the ion damping of the anisotropic fluctuations of the large-scale turbulent cascade at $k_\perp \rho_i \sim 1$, which is expected to have wave vectors $k_\perp \gg k_\parallel$, is very weak for the plasma parameters of this interval, as shown here in Figure R1. The reason is that the low $\beta_i = 0.38$ leads to a parallel phase velocity that falls in the tail of the ion velocity distribution, so ion damping is very weak. We have added a sentence to the text to clarify this point (lines 230 - 232).

- Is there a reason to choose the particular kprp*di = 0.016 for all of the linear analysis? Maybe I just missed it, but if not, it would be nice to discuss a bit why this was chosen and/or how the results depend on the choice.

As can be seen in Fig. S5 (Left) of the Supplementary Information, the growth rate of the unstable ion cyclotron wave mode is not strongly dependent on the perpendicular wavenumber, since the contours of that figure are generally horizontal in the regime $k_\perp \ll k_\parallel$ expected for ion cyclotron waves. The specific value of $k_\perp d_i = 0.016$ happens to correspond to $k_\perp \rho_i = 0.01$ (the natural units used by the PLUME dispersion relation solver) for the plasma parameters of the observed interval. Generally for any value of $k_\perp d_i \lesssim 0.5$, the key result of Fig 6 in the manuscript holds: that the ion cyclotron wave mode is only strongly damped by the ion cyclotron resonance and that other physical mechanisms of collisionless damping (ion and electron Landau and Transit-time damping) remain weak. To illustrate this point,

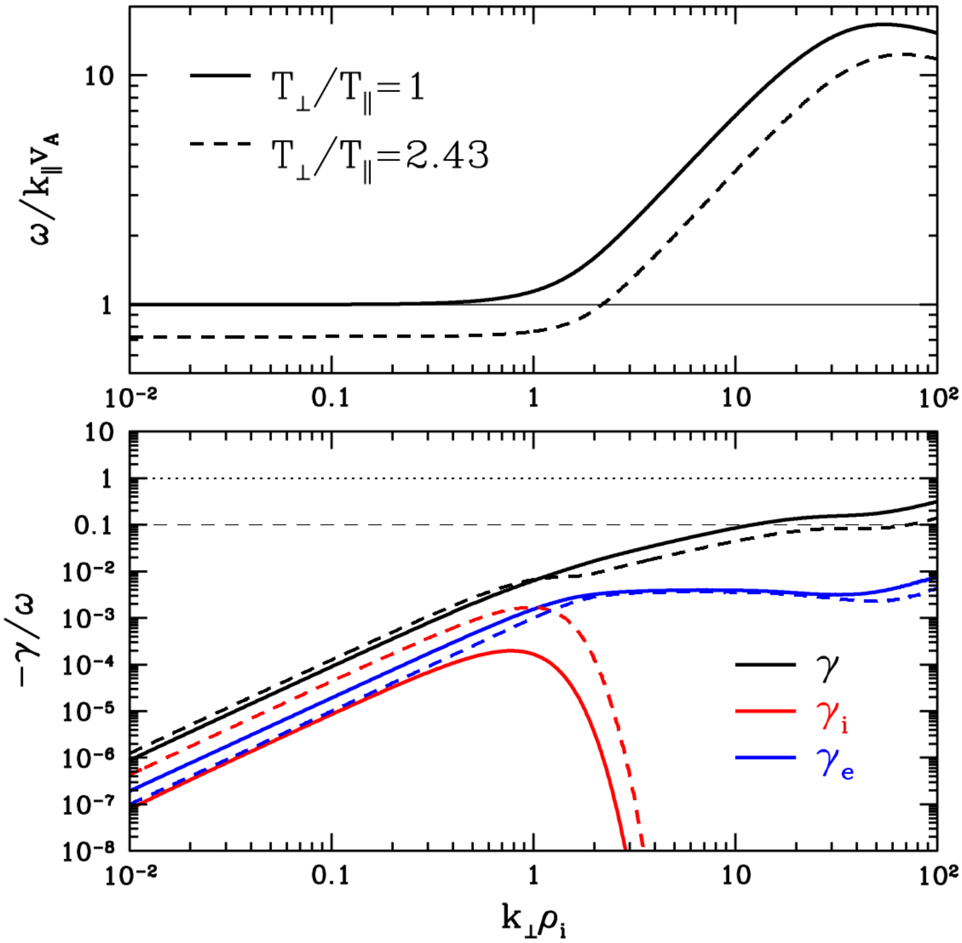

Figure R1: **A plot of collisionless damping rates for the parameters of the large-scale cascade with a typical wavevector anisotropy $k_\perp \gg k_\parallel$ for the same plasma parameters as Fig. 6 of the manuscript. In this limit, ion damping is dominated by the Landau resonance (Landau and transit-time damping), and the ion contribution to the normalized damping rate γ_i/ω is given by the red curves (solid for $T_{\perp i}/T_{\parallel i} = 1$, dashed for $T_{\perp i}/T_{\parallel i} = 2.43$. Significant collisionless damping occurs when $\gamma_i/\omega \gtrsim 0.1$, but here the maximum damping rates for ions have $\gamma_i/\omega \sim 10^{-3}$, predicting very weak ion damping.**

we include two figures here: Figure R2 for $k_\perp d_i = 0.16$ and Figure R3 for $k_\perp d_i = 0.48$, both of which show that all other collisionless damping mechanisms remain weak (with $|\gamma|/\omega < 0.1$) while the ion cyclotron damping rate is largely unchanged. We have added a parenthetical statement on lines 207 - 208 of the main manuscript to address this point that the ion cyclotron damping remains dominant and unchanged for $k_\perp d_i \lesssim 0.5$.

- line 226 of SI "The we"

This has been corrected. Thank you.

References

Verscharen, D. and Marsch, E. (2011). Apparent temperature anisotropies due to wave activity in the solar wind. *Annales Geophysicae*, 29:909–917.

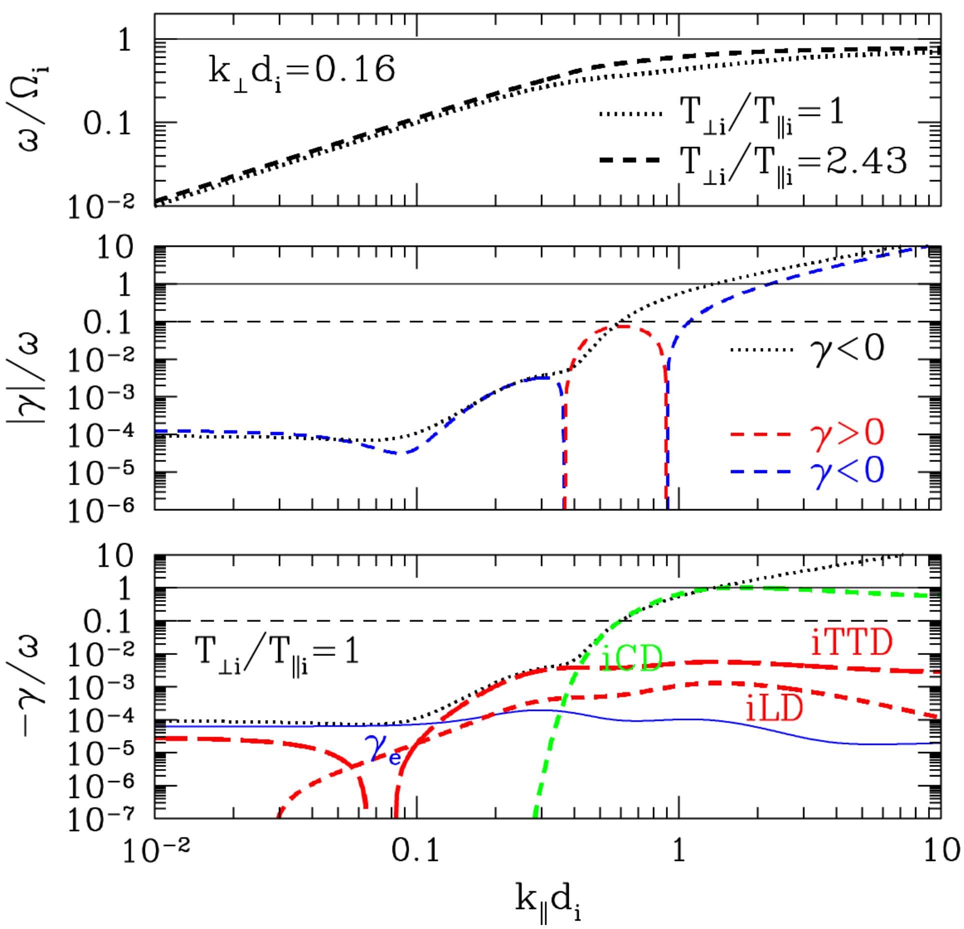

Figure R2: **The same plot as Fig. 6 of the manuscript, but using $k_\perp d_i = 0.16$ instead of $k_\perp d_i = 0.016$. Here the ion cyclotron damping rate in the lowest panel is largely unchanged from that in Fig. 6, while the damping rates of the other mechanisms [electron damping (γ_e) and ion Landau damping (iLD) and Transit-time damping (iTTD)] remain relatively weak with damping rates $|\gamma|/\omega < 0.1$.**

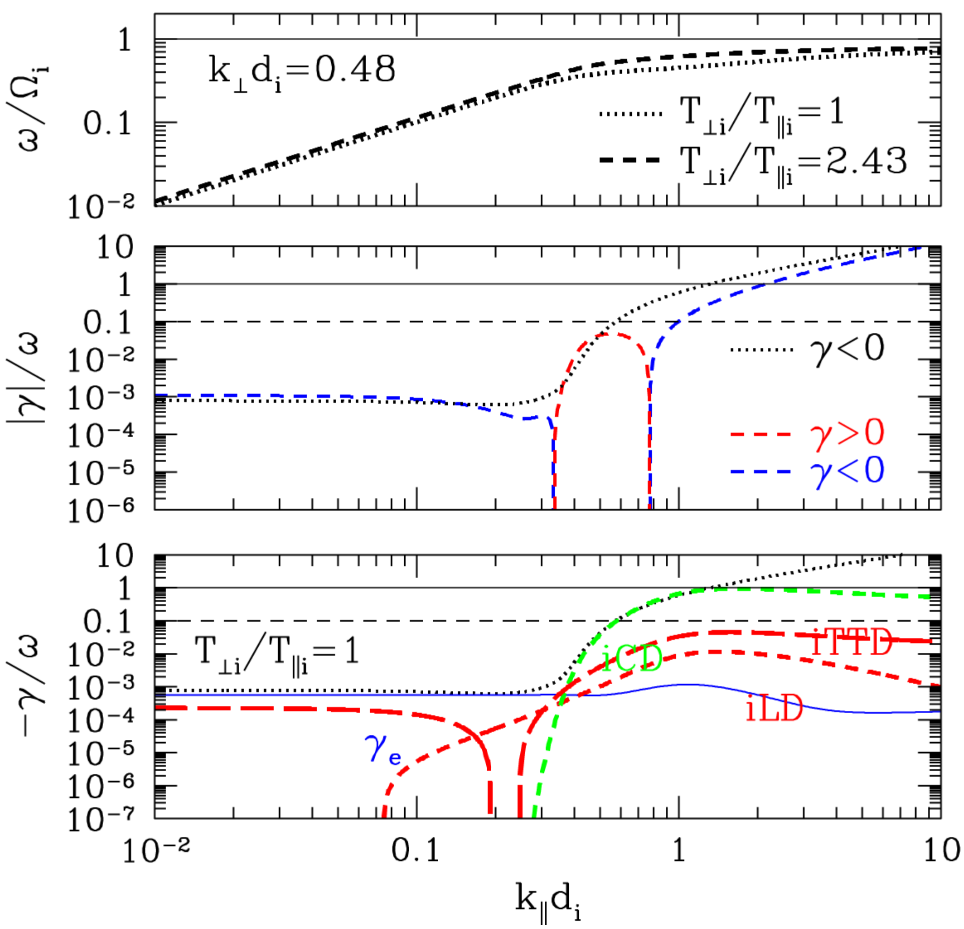

Figure R3: **The same plot as Fig. 6 of the manuscript, but using $k_\perp d_i = 0.48$ instead of $k_\perp d_i = 0.016$. Here the ion cyclotron damping rate in the lowest panel is largely unchanged from that in Fig. 6, while the damping rates of the other mechanisms [electron damping (γ_e) and ion Landau damping (iLD) and Transit-time damping (iTTD)] remain relatively weak with damping rates $|\gamma|/\omega < 0.1$.**

REVIEWER COMMENTS

Reviewer #2 (Remarks to the Author):

I am happy to recommend publication of this paper. The analysis seems as good as it can possibly be, the reviewer reports were taken seriously and addressed to the extent possible, and the result is new, novel, and interesting. Congratulations!

Reviewer #3 (Remarks to the Author):

The changes have improved the manuscript, and we are certainly converging. I just have some lingering concerns about a couple of the points:

- There still seems to be a dissonance between in the comparison of the turbulent damping rate and heating rate, even with the new definition of turbulent transfer. Specifically, early on it's stated, "we speculate that the ICWs were generated upstream via the Alfv én/ion cyclotron instablity driven by the ion temperature anisotropy $T_{\perp i}/T_{\parallel i} > 1$ (see Supplementary Information (SI) Sec. S2), perhaps due to compression within a quasiperpendicular region of the bow shock." But then the whole last section is comparing the cascade rate from the local region (i.e., the measured spectrum) to the ICW damping rate. Even with the more inclusive definition of turbulence, this seems to be comparing driving by a shock in one place to turbulent fluctuations in another – why should these be related? Perhaps you could just argue broadly that the outer scale turbulence should have similar timescales to other processes, but I don't see the justification for much more than that.

- Regarding the argument about LD of the turbulence, it is still mentioned in the discussion (l344-346) that the low ion LD rate of the turbulence is expected based on 6c. But 6c still shows the damping rate of kprp<kprl modes, which are not relevant to this turbulent ion LD expectation, even if the ICW (high kprl) LD rate happens to not much depend on the choice

of kprp. This just requires rewording or removing one sentence, but currently it seems misleading.

-l216 about the wave frequencies in the turbulence: is this meaning that the instability is growing on a time-dependent background? I found the statement confusing, perhaps it could be reworded.

-Supp 170 “as those as”

Response to Reviews of Manuscript NCOMMS-23-03741B

"Direct observation of ion cyclotron damping of turbulence in Earth's magnetosheath plasma"

We thank the two referees for taking the time to review our paper and provide feedback in order to improve our paper. Below are our responses (in blue text) to each of the reviewer comments (in black). Separately, the revised manuscript has changes highlighted in blue.

With reviewer #2 already recommending publication of the manuscript, and these final revisions completed to satisfy reviewer #3, we hope that this paper is acceptable for publication.

1 Review #2

Reviewer #2 (Remarks to the Author):

I am happy to recommend publication of this paper. The analysis seems as good as it can possibly be, the reviewer reports were taken seriously and addressed to the extent possible, and the result is new, novel, and interesting. Congratulations!

Thank you for working with us in strengthening our paper during this review process. Your time has been greatly appreciated.

2 Review #3

Reviewer #3 (Remarks to the Author):

The changes have improved the manuscript, and we are certainly converging. I just have some lingering concerns about a couple of the points:

- There still seems to be a dissonance between in the comparison of the turbulent damping rate and heating rate, even with the new definition of turbulent transfer. Specifically, early on it's stated, "we speculate that the ICWs were generated upstream via the Alfvén cyclotron instablity driven by the ion temperature anisotropy $T_{\perp,i}/T_{\parallel,i} > 1$ (see Supplementary Information (SI) Sec. S2), perhaps due to compression within a quasiperpendicular region of the bow shock." But then the whole last section is comparing the cascade rate from the local region (i.e., the measured spectrum) to the ICW damping rate. Even with the more inclusive definition of turbulence, this seems to be comparing driving by a shock in one place to turbulent fluctuations in another – why should these be related? Perhaps you could just argue broadly that the outer scale turbulence should have similar timescales to other processes, but I don't see the justification for much more than that.

The last revision of the manuscript stated (lines 314–322):

"Here we propose a working definition of the turbulence as *all* of the physical mechanisms that serve to mediate the conversion of the energy of large-scale plasma flows and electromagnetic fields into heat of the plasma species, including both the local energy transfer by the turbulent cascade and any nonlocal energy transfer via kinetic instabilities. We adopt this definition because, in a practical sense, it is not generally possible to separate observationally whether the turbulent fluctuations were driven by local or nonlocal energy transfer. Our measure of the cascade rate ϵ is based on the turbulent amplitudes that includes fluctuations from both sources, so the observed dissipation rate may be compared to this turbulent cascade rate."

The last sentence of this section is critical here: the estimated turbulence cascade rate is based on the measured turbulence amplitudes, which include both ICWs (which may have been driven by nonlocal energy transfer from kinetic instabilities) and any anisotropic fluctuations of the large-scale cascade (which is the local energy transfer associated with the typical turbulent cascade). The reviewer's statement that we are "comparing driving by a shock in one place to turbulent fluctuations in another" is not correct. Whatever the original source of the turbulent fluctuations that we measure directly, those fluctuations will interact nonlinearly, contributing to turbulent cascade rate at the location of the measurements. We estimate that rate based on the directly measured turbulent fluctuations, and we are comparing that estimated rate to the directly measured ion and electron energization rates.

This approach is consistent with the tradition, introduced by Kolmogorov (1941), of estimating the turbulent cascade rate based on the local conditions. Turbulence theory suggests that, in the inertial range, the details of the driving mechanism at large scales have been forgotten, and only the local (in scale) conditions govern the turbulent cascade rate. We are comparing the cascade rate estimated by the local-in-scale fluctuations that are directly measured to the energization rate that is directly measured locally. These calculations are essentially independent of the original source of the locally measured fluctuations. We do not believe there is any inconsistency in that approach. We have made a few minor wording changes to this section (highlighted in blue, lines 320 - 322) to clarify this point for the reader.

- Regarding the argument about LD of the turbulence, it is still mentioned in the discussion (l344-346) that the low ion LD rate of the turbulence is expected based on 6c. But 6c still shows the damping rate of kprp<kprl modes, which are not relevant to this turbulent ion LD expectation, even if the ICW (high kprl) LD rate happens to not much depend on the choice of kprp. This just requires rewording or removing one sentence, but currently it seems misleading.

We apologize that, although we made modifications to the main manuscript on lines 230-232 of the previous revision to address this point in the previous review, we inadvertently forgot to update the additional reference in the manuscript on lines 344-346. We have now made these additional corrections, including adding Figure R1 from our last referee response to the Supplemental Information as Figure S9, with the information explaining this figure in a new section S6, just to ensure that all of the supporting information about this point is available to the readers. We have also updated the wording on lines 231 - 233 and lines 346 - 349 to clarify these points.

- l 216 about the wave frequencies in the turbulence: is this meaning that the instability is growing on a time-dependent background? I found the statement confusing, perhaps it could be reworded.

As suggested, we have reworded this statement to state explicitly that the instability is growing on a time-dependent background (lines 212 - 220).

- Supp 170 “as those as”

This has been corrected. Thank you.

References

Kolmogorov, A. N. (1941). The local structure of turbulence in incompressible viscous fluid for very large reynolds numbers. *Dokl. Akad. Nauk SSSR*, 30:9–. English Translation: Proc. Roy. Soc. London A, 434, 9 (1991).

REVIEWERS' COMMENTS

Reviewer #3 (Remarks to the Author):

While I still have a slight disagreement with the authors about the turbulence discussion, they have addressed all the main concerns seriously and I'm happy to recommend the paper for publication. They are interesting results that I think will be impactful in the community.