nature portfolio

Peer Review File

Plasma wave observations from Juno spacecraft at the Jovian bow shock

Corresponding Author: Dr Jayasri Joseph

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Please refer to my report in the attached file.

Reviewer #2

(Remarks to the Author)
This manuscript reports the first high-resolution observations of multiple plasma wave modes—ULF waves, ion-acoustic waves, electrostatic solitary waves, and whistler mode waves—upstream of Jupiter's bow shock. These observations are highly significant, as they provide new insights into energy dissipation mechanisms at outer planet bow shocks where Mach numbers are much higher than those at Earth. The results are valuable for understanding solar wind energy conversion and shock evolution in the Jovian environment.
However, I find that the paper lacks a detailed quantitative analysis of energy dissipation. Although the observed plasma waves are indeed essential for the dissipation process, the current study does not sufficiently demonstrate how these waves contribute to the energy budget or particle heating. I recommend that the authors include comprehensive analyses on particle and wave data to strengthen the physical interpretation of the dissipation processes.
My specific comments are as follows:
Lines 105–107:
Please add relevant references to support the discussion of electron beams and Langmuir waves in the foreshock region. Prior studies on similar processes near Earth and other planets should be cited.
Lines 181–182 (Figure 3e):
The magnetic field compression ratios derived from the observations appear to be less than 1 (for a background magnetic field of ~2 nT), which does not meet the definition of shocklets. The authors should verify these values and clarify the criteria used to identify shocklets.
Lines 196–197:
How can we determine whether the frequency variation is caused by the Doppler effect rather than by changes in plasma parameters?
The authors should discuss possible diagnostics for distinguishing Doppler-shifted frequencies from intrinsic plasma variations, e.g., using multi-instrument data or wave vector estimation.
Lines 207–208:
The statement "these waves have nonlinear wave behavior" requires further explanation. Why do these waves show nonlinear characteristics? The harmonic features could also result from some instabilities, which should be discussed in more details.

Figures:
the figures can be improved. While they are very comprehensive, some of the contained fonts are too small. Measures should be taken to release space. E.g., in case of Fig.1 A, frequency tick labels could be "Freq (kHz)"; Fig. 1B "Rate (10^4counts/s)"; and so on. In Fig.2, the planet "Jupiter" is very impressive and … misleading. Consider represent it as a dot. Out of the consideration of beauty, make fonts across figures/panels consistent.

Reviewer #3

(Remarks to the Author)
This article presents, for the first time, high-resolution plasma waves associated with Jupiter's bow shock. They analyze two bow shock crossings done by Juno, while the burst-mode electric waveform was active. These observations represent the first effort to understand and characterize the processes involved in energy dissipation through plasma waves at the Jovian bow shock. It also provides insights into possible shock reformation at Jupiter's bow shock. The observations are compared with typical Earth's bow shock measurements, finding several similarities, with the main difference being that non-linear behavior more frequently occurs at Jupiter.

The article fulfills the objective of reporting the first high-resolution plasma wave environment at Jupiter's bow shock. The observations are highly relevant for understanding the bow shock environment of outer planets. I believe the results presented by the authors are worth publication in Nature Communications, provided the following points regarding the validity of their interpretation and conclusions are addressed in revision.

In general, the authors mention the presence of non-linear wave activity and shock reformation (or rippling). However, the conclusion that these are the distinguishing characteristics of Jupiter's bow shock compared to Earth's is not fully supported and requires further justification, according to the following comments:

1) It is not clear that these results are consistent with previous Earth bow shock observations. Similar or higher M_A are commonly observed at Earth (Lalti2022), and significantly larger electric fields are typically measured (Wilson2014, Wilson2021). Why then are the results different? The authors must state why non-linear activity at Jupiter at this M_A is different from bow shock crossing with similar characteristics observed at Earth. I think that simply stating the activity exists is not sufficient for a strong conclusion. Especially since the conclusion relies on the comparison of the results with typical Earth's bow shock observations.

2) Reformation and rippling have been observed at Earth's bow shock, especially if by reformation you mean non-stationary behavior (Johlander2016,Liu2020,Khotyaintsev2024,Lotekar2025). With just two bow shock crossings is hard to conclude that reformation is more common at Jupiter. Especially, since there doesn't seem to be a significant difference in shock parameters between these observations and a typical Earth bow shock crossing. Additionally, without the context of typical Jovian conditions, it is difficult to assess if these bow shock crossings resemble common Jupiter bow shocks or if they are rather exceptional crossings.
Similarly, the claim that non-linear behavior occurs more frequently at the Jupiter's bow shock is a bold statement, given that the study is based on only two shock crossings, where only one of these crossings showed non-linear electrostatic activity. I suggest rephrasing the text to avoid misconceptions, indicating that in comparison with Earth, the differences are not as strong. Alternatively, you can provide better justification on why reformation and non-linear activity are more common at Jupiter's bow shock compared with Earth's.
3) I am also concerned about the Bernstein waves in Case II. The IAWs in Section 1 of Case II seemed to be highly Doppler shifted, but from the PSD of the Bernstein modes, they don't seem to suffer from the same Doppler shift. Why is this the case? For a similar Doppler shift to the one exhibited by the IAWs, one would expect less clear harmonics, unless in the electron frame. A clarification is needed to explain the difference in Doppler Shift.

I will be able to recommend this article for publication after clarification of these points and addressing some minor comments described below.

Minor comments:
Line 46-47: It is mentioned that electron heating occurs almost adiabatically, however recent studies have shown that non-adiabatic electron heating becomes relevant at High Mach Number Shocks (Lalti2024, Lindberg2025). This needs to be stated in a more accurate way in order not to mislead the reader.

Line 73-75: You can rephrase to make clear that reformation is also observed at Earth's bow shock. As it is written it gives the impression that one of the main difference is that pronounced periodic shock reformation structures are only found in Jupiter' bow shock but not at Earth. This is linked to my general comments, where the comparison between Jupiter's and Earth's bow shock doesn't seem to be entirely in agreement with Earth's bow shock observations.

Line 81: What is the typical standoff distance of Jupiter's bow shock? Is the 81.44Rj a usual radial distance for the bow shock, or does this crossing occur during special bow shock conditions?

Line 115: The fast magnetosonic Mach number Mfs is a better predictor of shock structure and dissipation mechanisms than M_A alone. Do you have any means of estimating Mfs? Is it possible that the difference between Earth and Jupiter's bow shock be related to significantly different Mfs?

Line 116: It is not very clear how exactly the theta_Bn is obtained here. You show two methods at the end of the article, maybe it is good idea to explain in the main text where which value came from. As I get it, the number outside parentheses is from coplanarity, and the number in parentheses is from MVA (which was invalid for the inbound shock). So, then how do you get the range 0.0~39° for the coplanarity on the outbound shock? Or I am reading this incorrectly? When looking at Fig.

3.D, the theta_Bn from MVA does not seem to go below 20°. Also, the rounding seems to be inaccurate.

Line 122: Is this variability on the reflected ion count rate typical for shocks of similar M_A?

Fig. 1.C: The magnetic field signal looks more noisy starting from 2024-12-11 00:00. It seems that the magnetometer is changing sampling modes. If this is the case, this needs to be mentioned, as I couldn't find any comment in the text about this. Especially, because this will mean that both shock crossings occurred at different magnetometer modes. If this is the case, how does this impact your results?

Line 194-195: It is not clear if the Bernstein waves are found in several intervals or just in the highlighted red interval in Fig. 5.E.

Line 208: I don't see the dashed lines. I see the highlighted regions, though. Is the nonlinear wave behavior only found within these two intervals?

Line 210: For consistency, use the same number of decimals in the milliseconds.

Nonlinear IAWs in Case II: Have you confirmed that the observed harmonics are not instrumental? If the response of the instrument is not linear, then you can have such type of instrumental harmonics.

Fig 5.C. I suggest adding in the caption, that the lines do not share the same Y-axis origin. It was a little confusing at first. It looks like the yellow interval has the highest PSD. I like the way you presented it, but in my opinion it will benefit from a quick explanation on how to read it.

Fig F.B-C. In the caption it says that you are displaying 5 color-coded segments of 123 ms. This seems to imply that each color-coded segment is 123 ms, which is not the case.

Line 298: I suggest adding a comment on why it is not necessary that the non-linear waves have larger amplitude than the linear waves found in different intervals.

Line 310: Is it expected that the PSD spectrum of the IAW superimposed on the ECDI to be broader than that of the other IAWs?

Line 328: A brief definition of the turnaround time is needed.

Methods Section: I suggest showing the upstream and downstream intervals used for calculating the different shock properties. For example, the windows mentioned in line 352.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
See attached file.

Reviewer #2

(Remarks to the Author)
The authors have adequately addressed all the issues raised by the reviewer. The manuscript has undergone substantial improvements, and I have no further comments or suggestions. Therefore, I recommend acceptance in its present form.

Reviewer #3

(Remarks to the Author)
The results presented in this manuscript provide detailed first insights into the bow shock environment at Jupiter. Although previously I found the results worth publishing, the conclusions and some interpretations were not fully convincing.

Now, this revised version of the manuscript is much more solid than the previous one; the authors have addressed my concern regarding their conclusions and interpretations of the results. Therefore, I can now recommend the manuscript for publication in Nature communications.

I only have some minor points that need clarification:

Line 274 - Taking the scaled amplitude rather than the absolute is now explained. However, it is still not entirely clear to me why the scaling factor is ~x10? I think providing an explanation on how you chose that value for the scaling factor will help future studies to conduct the appropriate analysis and comparisons.

Regarding IAWs, in your response, you mention that IAW part of the spectrum tends to have a unit vector direction aligned with Bo. While I agree with this, I don't understand if you have verified this in the data or if it is just the behavior you expect. If possible, you should estimate the propagation direction observationally.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
None

Reviewer #3

(Remarks to the Author)
I thank the authors for answering my remaining question. Now, all my questions and comments have been addressed. Therefore, I can recommend this manuscript for publication in Nature Communications in its current state.

Reviewer 1

We thank the reviewer for the insightful comments and constructive suggestions. These have helped us to re-examine, refine, and present our observations from a new perspective. We have made numerous corrections and added further clarification of our observations in the revised manuscript. The differences in power dissipation requirements between Earth and Jupiter are now explicitly stated. Additionally, the Discussion section has been expanded to include a comparison of dissipation features with terrestrial studies, illustrating how these requirements may be satisfied. Despite the limited instrumentation available for bow-shock studies at the outer planets, we believe that these additions significantly strengthen the manuscript and make a compelling case for publication in *Nature Communications*.

In this document, the reviewers' comments are shown in blue text, and our responses are provided in black text. In the revised manuscript (*Jupiter_bowShock_NatComm_rev1.pdf*), changes have been highlighted using different colors for each reviewer: Reviewer 1 in cyan, Reviewer 2 in yellow, and Reviewer 3 in gray.

A Note: We identified a programmatic error in the code used to calculate θ_{Bn} for the outgoing shock (case I). This error has now been corrected. The correct value is $\boldsymbol{\theta_{Bn}}$**= 16.5°** (previously reported as 44°). We have updated the manuscript accordingly. Importantly, this correction does not affect the main conclusions of the paper.

The manuscript presents particle and field observations from two successive Juno crossings of Jupiter's bow shock. The study of planetary bow shocks and their associated plasma environments beyond Earth is of fundamental importance in space physics. This is especially true for the outer planets, where in-situ measurements are relatively rare. The terrestrial bow shock and foreshock serve as key reference models against which the bow shocks and plasma environments of other planets in the solar system can be compared. The authors have described the observations clearly, making the manuscript reasonably accessible even to non-experts in the field. Additionally, the figures are of high quality, which helps the reader to better grasp the various features of the event under investigation. However, the manuscript carries several statements that are not substantiated with solid arguments and some of them appear incorrect. Given the maturity of the field and the extensive literature on the terrestrial bow shock, a critical discussion of the empirical results is essential. A deeper comparison with previous studies related to Earth's bow shock would have been both appropriate. Consequently, at this stage, I am not feeling comfortable giving my agreement for the publication of the manuscript to *Nature-Communications*. I believe the authors present a potentially valuable case; however, and as presented, it does not meet the journal's quality standards. The manuscript would benefit from moving beyond a purely descriptive approach and incorporating a more in-depth, analytical discussion. My arguments are described below. The following points are not arranged according to their importance.

Filbert et Kellog [1979] have demonstrated that strong correlation exists between the magnetic connection to the shock and the electrostatic emissions at the local plasma frequency. Near the tangent line, the emissions the most intense are narrow in frequency while the spectrum becomes progressively wider inside the foreshock.

i) Line 54: The authors claim that "… *the Mach number are typically higher than those of Earth…*". Could the authors provide references regarding this point? Typical Alfven speed at 1 AU is ~50 km/s (n_{sw} ~ 5 cm-3, B_{sw} 5 ~ nT) which gives M_A ~ 9 for v_{sw} ~ 440 km/s (1 keV), which is similar to the estimated numerical value reported in the manuscript.

Typically, the range of Mach numbers at Jupiter is higher than that at Earth. References (line 54) have now been included. However, the Mach number ranges for Earth and Jupiter overlap, meaning that **M_A~9** is relatively high for Earth but on the low end for Jupiter.

ii) Line 94-96: These lines should be reformulated. Usually, a shock geometry with an angle of Θ_{BN} less that ~30º is rather considered as quasi-parallel. Also writing "… *ions are reflected upstream in the direction of VxB electric field*" is incorrect. If reflection occurs specularly, ions gain a parallel and a perpendicular velocity. How do ions escape upstream if there is no parallel velocity since the solar wind convection pushes the particles anti-sunward? Also, the ion foreshock does not start at Θ_{BN} = 45º. There is a rich literature on the topic that the authors could consult. Regarding the location of the low frequency waves in the foreshock, I recommend Meziane & d'Uston [1998] and Andrés et al. [2015]. Finally, ions that are produced at quasi-parallel regions are not the source of free energy for the foreshock wave excitation [As an example: Meziane *et al*., JGR, 2001; Mazelle *et al*., P&SC, 2003; Thomsen *et al*., JGR, 1985]. Also, I understand from the authors that what their description of the foreshock is valid for an *electron-proton plasma* [Line 94]? This is confusing and it is not clear to me what do they mean by "*typical electron-proton plasma*"?

This section (line 93-102) has been rewritten to incorporate the requested corrections. There is some variation in the literature regarding the definition of the shock normal angle for quasi-parallel shocks (e.g. Johlander et al., 2022; Krämer et al., 2025; doi: 10.1007/s11214-024-01129-3). Accordingly, a range of 30°–45° (line 93) is adopted in this manuscript.

iii) Line 98-100: Incorrect statement and please reformulate. Particle do not have their velocity aligned with the electric field, otherwise they are [ad-infinitum] accelerated in the case of a steady solar wind. Only particles that are specularly reflected for Θ_{BN} > 45º have their guiding center directed toward the planet; these cannot escape upstream. It is not the case of foreshock fieldaligned beam produced at the quasi-perpendicular region. It is this population that provide the energy for the waves to grow [Mazelle *et al*, 2003; Meziane *et al*., 2005 or/and references above]. The authors seem to overlook that the ion foreshock extends beyond Θ_{BN} = 45º.

This section has been rewritten (line 93-102) to incorporate the requested corrections.

iv) Figure 2: This figure appears both unnecessary and potentially misleading. The foreshock topology depicted is that of the terrestrial foreshock, based on seminal studies using ISEE-1 and ISEE-2 spacecraft measurements. Significant progress has been made since the observations from Wind and Cluster missions. Furthermore, how do the authors make sure that this terrestrial-based topology applies to Jupiter foreshock? In addition, the foreshock topology shown in Figure 2 represents a cross-section of the bow shock in the ecliptic plane, with a magnetic field orientation consistent with the Parker spiral at 1 AU. However, at 5 AU, the Parker spiral angle increases to approximately 80°, making the representation less applicable to Jupiter's environment. In my opinion, this figure should be removed as it does bring any instructive element to the manuscript.

We appreciate the reviewer's observation that the original version of Fig. 2 contained inaccuracies in its depiction of the shock geometry. The figure has been redrawn to more accurately represent the bow-shock topology at Jupiter, consistent with the Parker spiral configuration and approximate shock normal angles – one quasi perpendicular and other quasiparallel. However, we emphasize that this figure remains schematic in nature. Shocks in the flank regions can exhibit Θ_{Bn} values well below 80°, including cases within the quasi-parallel regime. The accompanying wave and plasma observations suggest that Juno likely traversed both electron and ion foreshock regions. The revised figure is intended solely to aid visualization of the shock geometry relevant to the presented case studies, especially for readers from related disciplines.

v) Line 115-116: The authors should estimate the uncertainties associated with the determination of Θ_{Bn}. The shock geometry may not differ significantly between the two crossings.

The uncertainity estimates are now included (line 115, 369, 371).
As noted above, the shock normal angle for Case I, calculated using the coplanarity method, was affected by a programmatic error (original manuscript) related to the input file format, resulting in an incorrect estimate of 44° instead of 16°. No such error was present for Case II, for which the shock normal angle was correctly estimated to be ~53°. The resulting shock geometries for the two crossings are therefore substantially different. Note that, consistent with previous studies (doi:10.1029/2003JA010099), the minimum variance analysis (MVA) method is not well suited for reliable shock normal determination and has therefore been excluded from the present analysis. We only use the coplanarity method.

A rough estimate of the shock normal uncertainty using the magnetic coplanarity method is given by: $\Delta\theta_n \sim \frac{\sqrt{\Delta B_u{}^2+\Delta B_d{}^2}}{|\boldsymbol{B_u} X \boldsymbol{B_d}|} * \frac{180^0}{\Pi}$

For case-II, the standard deviations: ΔB_u ~0.1 nT; ΔB_d ~0.5 nT; $|B_u X B_d|$ ~ 3.66 nT. The resulting uncertainty is ~7.9º. The shock in Case II, with θ_{Bn}=53.5º±7.9, is therefore classified as quasi-perpendicular.
For case-I, the standard deviations: ΔB_u ~0.2 nT; ΔB_d ~1.13 nT; $|B_u X B_d|$ ~ 4.61 nT. The resulting uncertainty is ~14º. The shock in Case II, with θ_{Bn}=16.5º±14, is therefore classified as quasi-parallel.
Since the determination of θ_{Bn} is sensitive to the choice of upstream and downstream averaging intervals, plasma wave (ULF and Langmuir waves) observations upstream provide an important complementary diagnostic for distinguishing quasi-parallel and quasi-perpendicular shocks.

vi) Line 117-124: There appears to be some confusion regarding the distinction between the electron and ion foreshock. The foreshock is generally defined as the entire upstream region where interplanetary magnetic field (IMF) lines connect to the bow shock. In the terrestrial case, the concept of an "electron foreshock" arises because electrons accelerated near the IMF tangent point travel at high velocities and are only weakly influenced by solar wind convection, unlike ions. As a result, this narrow region is often referred to specifically as the electron foreshock. However, this does not imply that electrons are confined to this region alone; as illustrated in Figure 2, the yellow dots representing electrons are distributed throughout the foreshock. At this point, it is unclear how such this picture can be directly applied to the Jovian foreshock, given that the crossings of the investigated events are basically oblique in nature.

The **updated** Fig. 2 illustrates the most probable shock topologies inferred from the calculated θ_{Bn} values (53° and 17°), corresponding to **oblique and quasi-parallel** configurations. The regions indicated in the figure are based on the observed wave activity, including Langmuir and ULF waves. We do not imply that electrons are confined exclusively to the electron foreshock region; we only suggest that electron beams are the most likely drivers of the Langmuir waves detected there. Lines 117–124 (original manuscript) refer to Fig. 3, which presents Juno's wave and particle measurements. These observations are generally consistent with the Case I topology illustrated in Fig. 2.

vii) Figure-3: The observed modulation in the reflected ion count rate may instead arise from variations in the IMF connectivity to the source region [see for example Meziane *et al*., Ann. Geophys., 2004]. Additionally, the Langmuir wave spectrum appears broad—how does it compare to the solar wind plasma frequency?

This figure has been modified. Removed old panels 3D -E. Also, the section titled "Correlation of Reflected Solar Wind with Shock Normal Angle" has been removed from the study.
However, we acknowledge that the observed modulation in the reflected ion rate might arise from variations in IMF connectivity.
The Langmuir wave spectrum also exhibits a finite bandwidth. This broadening can result from multiple plasma densities being sampled along the spacecraft path or from other nonlinear processes. In this study, we use the peak of the Langmuir wave spectrum (~9.6 kHz) to represent the local plasma frequency.

viii) Figure-4: Does the Langmuir wave spectrum appear narrower compared to the previous case? Additionally, what does the narrow band near ~ 500 Hz—observed throughout the entire interval— represent?

In both cases, the Langmuir wave spectra have similar widths, as shown in the figure below; the apparent difference arised only from the use of logarithmic versus linear scales. Thank you for pointing this out—we now used a logarithmic scale in both cases for consistency.
Juno instrumental (gyro) noise is visible at approximately 500 Hz. This is now indicated in Figure-4.

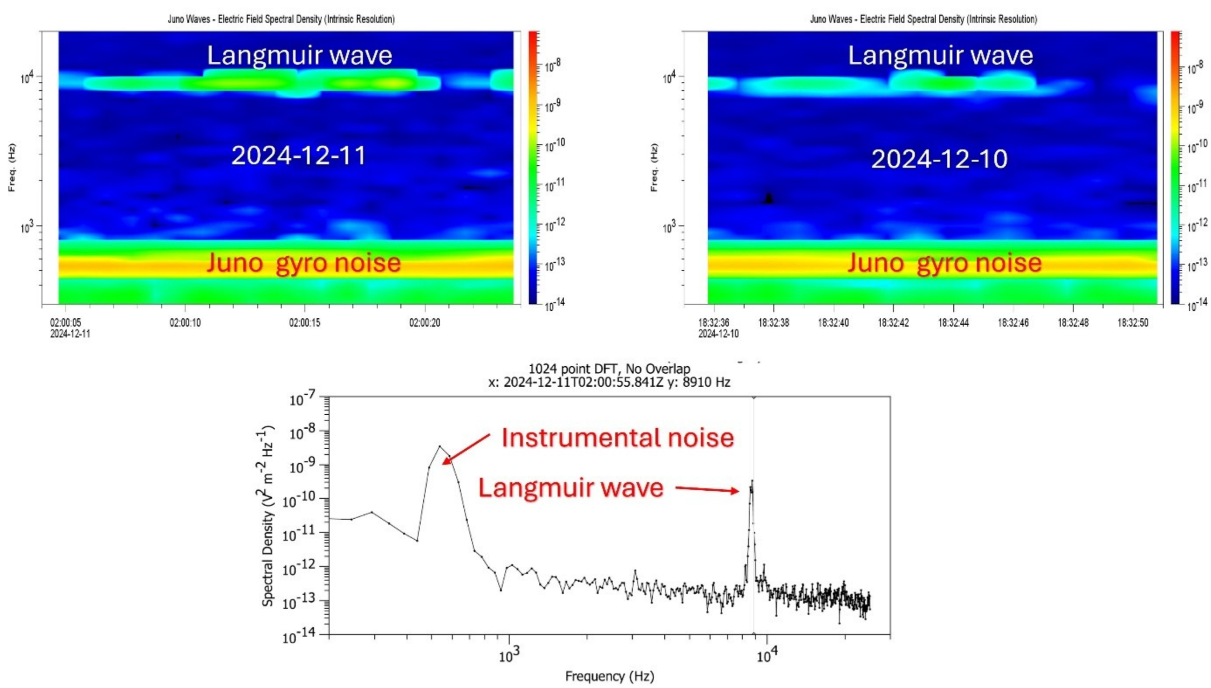

Fig. Power spectral density of Upstream Langmuir waves and instrumental noise (top). Line spectrum (bottom).

ix) Line 201: The narrow frequency band near ~9.6 kHz appears to correspond to a plasma density of approximately 10 cm^{-3}. However, this value is typical of solar wind densities near 1 AU and is not expected at 5 AU?

$$f_{pe} = \frac{1}{2\pi}\sqrt{\frac{n_e e^2}{\epsilon_0 m_e}}$$

or, f_{pe}(kHz) ~ $9\sqrt{n_e(cm^{-3})}$ So, plasma density in our case is **~1 cm^{-3}**

x) Figure 6: The figure should be constructed based on the actual direction of the interplanetary magnetic field.

This figure has been modified.

xi) *Correlation of reflected solar wind with shock normal angle Section:* Studies by Fuselier *et al*. [JGR, 91, p4163, 1986] and Meziane *et al*. [Ann. Geophys., 2004] have quantitatively addressed how shock geometry can inhibit the reflection mechanism. Cite these 2 papers if the authors want to keep this Section. Furthermore, it is unclear to me how the authors calculate the upstream ΘBn angle, given that the spacecraft lost contact with the shock at the 17:25:00 UT crossing. This crossing may not necessarily result from spacecraft motion alone but could also be influenced by the motion of the shock itself. The only reliable way to determine the shock normal remotely in such a case is by assuming a model for the shock surface shape.

This section has been removed.

xii) *Shock reformation Section*: The authors provide no clear evidence that shock reformation occurs during this crossing. They merely describe variations in the magnetic field magnitude, which alone is insufficient to support such a conclusion. As presented, the analysis lacks the depth required to substantiate the claim.

We agree that this section lacked sufficient detail and merely describing the variations in the magnetic field magnitude is insufficient to conclude shock reformation. It has now been revised (lines 245–255) to provide additional clarification. In addition, the Discussion section has been expanded to include a comparative overview of similar shock reformation observations at Earth, Jupiter, and Saturn.

xiii) *Discussion Section*: Are the authors suggesting that the processes they describe are uncommon at Earth? If so, this is inaccurate, as there is a substantial body of literature on this topic. One of the most recent studies could be found in Mazelle & Lembège [Ann. Geophys., 2021]. In addition, previous reports outlining results from numerical simulations on the same topic are completely ignored by the authors.

Moreover, as noted earlier, the authors do not present convincing evidence of bursty particle reflection. The observed modulation in the reflected ion count rate may instead arise from variations in the IMF connectivity to the source region.

The particle instrument on Juno has a cadence of 120 s and FGM has a cadence of 0.125 s. The reformation structures seen in the magnetic field occurred on timescales of roughly 8 s, as shown in the figure below. Consequently, Juno cannot resolve the bursty particle reflection associated with these rapid shock reformation cycles. The text has been revised to allow the underlying cause of the reformation to remain open. This drawbacks in general apply to many spacecrafts situation and as accurately pointed out by **Mazelle & Lembège [Ann. Geophys., 2021] – "***The constraint on time resolution necessitates the sole use of the magnetic field data for the present detailed analysis. Particle data will be used only to provide the necessary "contextual" plasma parameters, since their time resolution is commonly much lower than the magnetic field data***."**
This section has been revised to include a comparative discussion of shock reformation at Jupiter, Earth, and Saturn (lines 324–337). We do not suggest that such reformation is uncommon at Earth; rather, the scale of the reformation events is substantially larger at the outer planets, as illustrated in Fig. 7.

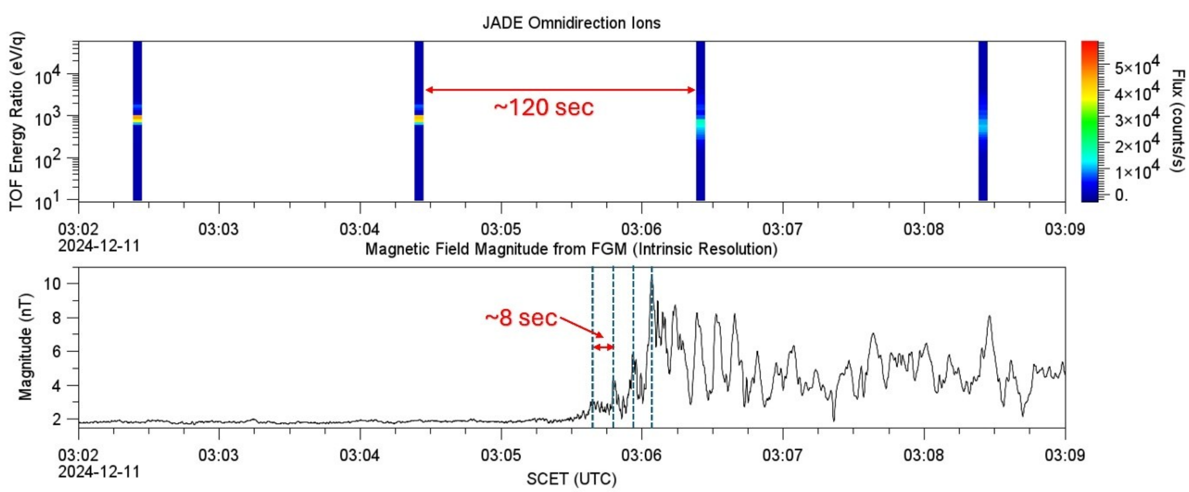

Fig. Juno's ion and magnetic field observations during shock reformation.

xiv) Line 325-327: In my view, this is a clear example where the authors fail to present any well supported evidence to substantiate their claim. Once again, previous studies—such as those by Lembège and/or Matsukiyo on shock reformation—offer a sophisticated framework within which the authors could contextualize their results. In previous studies, the shock reformation cycle has been determined in the plasma frame of reference, whereas in the present case, the estimated rate (~ tCP/3) appears to be measured in the shock frame.

This section has been revised (line 324-337) to reflect the insights provided by recent studies. We appreciate being directed to the extensive body of relevant literature.

In our analysis, the shock reformation cycle is determined in the spacecraft frame, which typically closely approximates the upstream plasma frame because the spacecraft velocity is small compared with the solar wind. Consequently, our observations are consistent with previous studies.

xv) Line 341: How the authors can claim that nonlinear effects *occur more frequently* based on a single event?

We agree that, in general, it is not possible to comment on the frequency of occurrences without conducting a statistical study. This sentence has been rephrased (line 347).

Reviewer 2

We thank the reviewer for the insightful comments and constructive suggestions, which prompted a careful re-examination of our observations in light of the limitations of the available instrumentation. We now explicitly state the measurements required, as well as the limitations of Juno in providing the data needed to obtain a quantitative estimate of energy dissipation. This, in turn, motivated us to substantially strengthen the manuscript by incorporating comparative studies with Earth, leveraging our understanding of the candidate waves responsible for the majority of the power dissipation. Even with the challenges of bow-shock measurements at the outer planets, these additions clearly enhance the manuscript and reinforce its suitability for publication in *Nature Communications*.

In this document, the reviewers' comments are shown in blue text, and our responses are provided in black text. In the revised manuscript (*Jupiter_bowShock_NatComm_rev1.pdf*), changes have been highlighted using different colors for each reviewer: Reviewer 1 in cyan, Reviewer 2 in yellow, and Reviewer 3 in gray.

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This manuscript reports the first high-resolution observations of multiple plasma wave modes—ULF waves, ion-acoustic waves, electrostatic solitary waves, and whistler mode waves—upstream of Jupiter's bow shock. These observations are highly significant, as they provide new insights into energy dissipation mechanisms at outer planet bow shocks where Mach numbers are much higher than those at Earth. The results are valuable for understanding solar wind energy conversion and shock evolution in the Jovian environment. However, I find that the paper lacks a detailed quantitative analysis of energy dissipation. Although the observed plasma waves are indeed essential for the dissipation process, the current study does not sufficiently demonstrate how these waves contribute to the energy budget or particle heating. I recommend that the authors include comprehensive analyses on particle and wave data to strengthen the physical interpretation of the dissipation processes.

Juno can measure only one component of the power spectral density (PSD) of electric field (in the spin plane) and one component of the PSD of search-coil magnetic field (along the spin axis), both at a cadence of one second. Additionally, 1-axis burst-mode electric-field waveform (@50 kHz) —essential for observing the high-frequency waves that dominate heating at the bow shock—is available with a duty cycle of 1/8. Consequently, we obtain only a partial view of the plasma-wave environment, making quantitative wave-power estimates impossible. However, extensive observational and theoretical studies of terrestrial plasma waves provide a framework for interpreting Juno's limited measurements: by focusing on specific wave characteristics known to contribute to plasma heating, we can qualitatively infer the most likely energy-dissipation mechanisms. Although Juno's primary objective was not to measure the solar wind, we can make some approximations when performing our shock analysis.

We have rewritten the discussion section to highlight these instrumental limitations (line 258-265) and to incorporate detailed comparisons with terrestrial observations (Table 1 and Figure 7), thereby illustrating how plasma-heating mechanisms operate at Jupiter's bow shock.

Still, one can have a feel for the fraction of the total power that is dissipated due to high frequency waves, even though the uncertainties of the estimates are very large. Here we show some calculations –

Solar wind power input at bow shock = kinetic energy / time ~ $0.5mv^2$/T. For unit volume, we get $0.5\rho v^2$/T, where ρ is the plasma density. Time (T) can be approximated from shock-width:

$T = L_{sh}/v$).

So solar wind kinetic-energy deposition rate per unit volume: $\frac{0.5\rho v^3}{L_{sh}}$.

We measured $v = 4E5$ m/s (solar wind velocity) and plasma frequency upstream ω_{pe}= 9.6 kHz.

n~1/cm^3 (number density) = 10E5 /m^3, m_p=1.6E-27 kg

ρ ~ n*m_p = 1.6E-21 kg/m^3

$0.5\rho v^3$ = 0.5* 1.6E-21*$(4E5)^3$ = 51.2E-6 ~ 5E-5 W/m^2

L_{sh} ~ ion inertial length ~ $\frac{c}{\omega_{pi}}$, = 1.3E6 m (c=3E8 ω_{pi}= 9600/43 = 223 Hz)

Therefore, solar wind power input is,

$\frac{0.5\rho v^3}{L_{sh}}$ = 5E-5/1.3E6 =3.8E-11 W/m^3 = 3.8E-5 μW/m^3

Now, we approximate an example power dissipation for an instance (marked by a red dashed line in the figure below) of observed ECDI during one of the burst captures 3:05:47 – 3:06:20 (case II). As the high frequency waves are generated from current (db/dt) driven instabilities, we need the burst waveform (E) data at the location of large db/dt (j_0) to approximate the power (j_0.E).

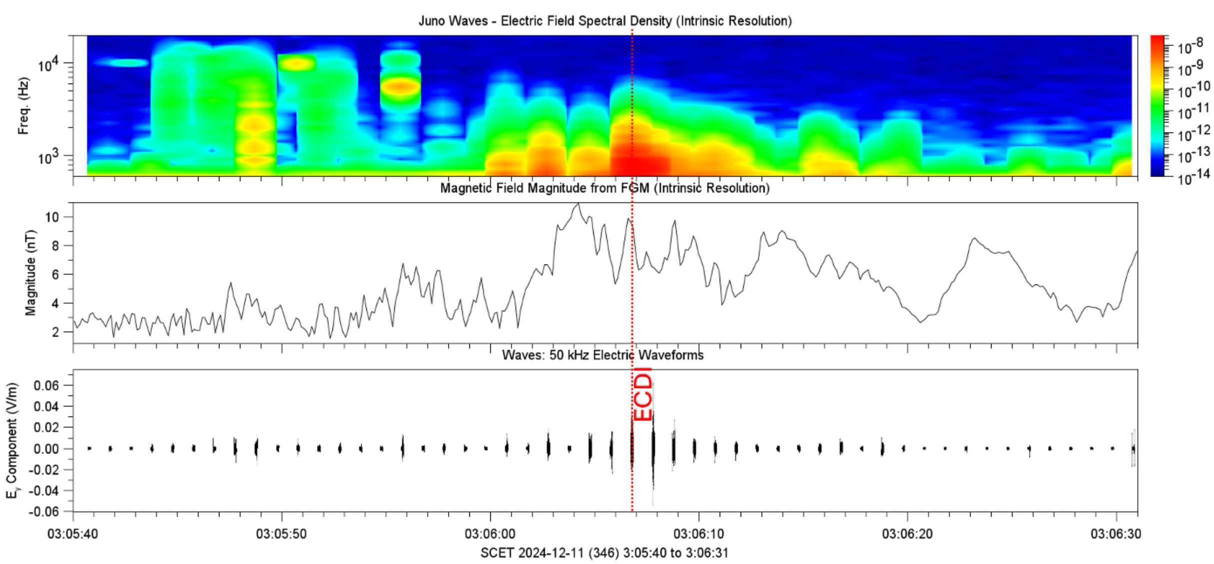


Fig. The ECDI instance is used to provide an approximate example of the power dissipation estimate.

We approximated the power dissipation at a point, marked – ECDI in the above figure, where we have burst samples at 50 KHz. The approximate current from the change in magnetic field just ahead of the ECDI observation:

Scale length, L_{sh}~c/ ω_{pe} = 1300 km and current $j_0 \sim \frac{\Delta Bo}{\mu_0 L_{sh}}$

ECDI case |B| changed *5.45 – 9.96 nT,* ΔB*o = 4.51 nT,* j_0 *~ 0.00276* μA

E is approximated from burst mode Ey electric field waveform.

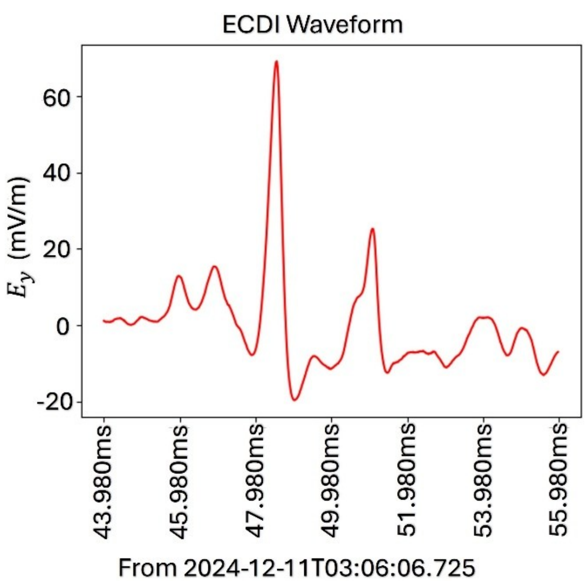
A cumulative sum of j_0.E represents the effective power transfer between waves and particles (Wilson+, 2014). Figure below show a difference in cumulative sum of ~0.0023 $\mu W/m^3$ across the ECDI observation period.

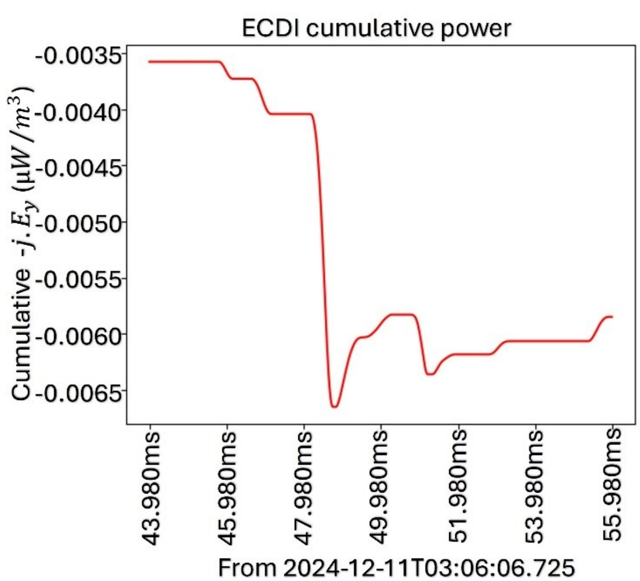
So, our estimate shows, over the ECDI interval:

(ECDI power ~ 60 times the input solar wind power)

230E-5 $\mu W/m^3$ ~ 60*3.8E-5 $\mu W/m^3$

However, the waves likely have a fractional effect on plasma heating as it will not interact with 100% of the incident particles. The above estimates demonstrate the importance of high frequency waves at Jupiter's bow shock.





Lines 105–107: Please add relevant references to support the discussion of electron beams and Langmuir waves in the foreshock region. Prior studies on similar processes near Earth and other planets should be cited.

Citation included (line 106).

Lines 181–182 (Figure 3e): The magnetic field compression ratios derived from the observations appear to be less than 1 (for a background magnetic field of ~2 nT), which does not meet the definition of shocklets. The authors should verify these values and clarify the criteria used to identify shocklets.

The section **"Foreshock shocklets and ULF waves"** has been removed from the manuscript.

Lines 196–197: How can we determine whether the frequency variation is caused by the Doppler effect rather than by changes in plasma parameters? The authors should discuss possible diagnostics for distinguishing Doppler-shifted frequencies from intrinsic plasma variations, e.g., using multi-instrument data or wave vector estimation.

IAW propagate mainly parallel to magnetic field and solar wind is often within 30-45^0 of B. In solar wind, IAW frequency observed by spacecraft $f_{sc} = f_{iaw} + \frac{k_{iaw} v_{sw}}{2\Pi}$, where k_{iaw} is the wave number of IAW, f_{iaw} is the ion plasma frequency and v_{sw} is the solar wind speed. So, we expect an inherent Doppler shift of IAW in solar wind.

We do not have measurement of plasma moments in solar wind by JADE. The low cadence of JADE (1-2 min) precludes us from seeing fine changes in the plasma parameters.

Wave vector directions cannot be determined by Juno due to lack of 3 axis measurement, as we only have one electric field antenna.

However, the frequency peak drift in time for IAW as seen in Figure 5C appears to be due to velocity dispersion from impulsively accelerated ions (DOI: 10.3847/1538-4357/ad71cf; DOI 10.3847/1538-4357/ad4b12).

Updated the manuscript text (lines 180-181).

Lines 207–208: The statement “these waves have nonlinear wave behavior” requires further explanation. Why do these waves show nonlinear characteristics? The harmonic features could also result from some instabilities, which should be discussed in more details.

Typically, nonlinearity is associated with wave Steepening. However, plasma waves can become nonlinear when they significantly modify particle motion, particularly through particle trapping in the electrostatic wave potential, a behavior associated with beam–plasma instabilities. This effect is commonly observed for foreshock Langmuir waves in the presence of strong shocks but has not previously been reported for ion acoustic waves at the bow shock. That said, harmonic structures may also be intrinsic to certain instabilities, such as the electron cyclotron drift instability (ECDI).

Added text (Lines 191-193).

Figures: the figures can be improved. While they are very comprehensive, some of the contained fonts are too small. Measures should be taken to release space. E.g., in case of Fig.1 A, frequency tick labels could be “Freq (kHz)”; Fig. 1B “Rate (10^4counts/s)”; and so on. In Fig.2, the planet “Jupiter” is very impressive and … misleading. Consider represent it as a dot. Out of the consideration of beauty, make fonts across figures/panels consistent.

Updated.

Reviewer 3

We thank the reviewer for the insightful comments and constructive suggestions, which prompted us to add several important clarifications. We now support our conclusions by incorporating a comparative study of Jovian bow shock plasma waves with those at Earth. In addition, through comparison, we demonstrate that the importance of shock reformation for power dessipation increases with heliocentric distance. Despite the challenges associated with bow-shock measurements at the outer planets, these additions significantly enhance the manuscript and further reinforce its suitability for publication in *Nature Communications*.

In this document, the reviewers' comments are shown in blue text, and our responses are provided in black text. In the revised manuscript (*Jupiter_bowShock_NatComm_rev1.pdf*), changes have been highlighted using different colors for each reviewer: Reviewer 1 in cyan, Reviewer 2 in yellow, and Reviewer 3 in gray.

A Note: We identified a programmatic error in the code used to calculate θ_{Bn} for the outgoing shock (case I). This error has now been corrected. The correct value is θ_{Bn}= 16.5° (previously reported as 44°). We have updated the manuscript accordingly. Importantly, this correction does not affect the main conclusions of the paper.

This article presents, for the first time, high-resolution plasma waves associated with Jupiter's bow shock. They analyze two bow shock crossings done by Juno, while the burst-mode electric waveform was active. These observations represent the first effort to understand and characterize the processes involved in energy dissipation through plasma waves at the Jovian bow shock. It also provides insights into possible shock reformation at Jupiter's bow shock. The observations are compared with typical Earth's bow shock measurements, finding several similarities, with the main difference being that non-linear behavior more frequently occurs at Jupiter. The article fulfills the objective of reporting the first high-resolution plasma wave environment at Jupiter's bow shock. The observations are highly relevant for understanding the bow shock environment of outer planets. I believe the results presented by the authors are worth publication in Nature Communications, provided the following points regarding the validity of their interpretation and conclusions are addressed in revision. In general, the authors mention the presence of non-linear wave activity and shock reformation (or rippling). However, the conclusion that these are the distinguishing characteristics of Jupiter's bow shock compared to Earth's is not fully supported and Requires further justification, according to the following comments:

1) It is not clear that these results are consistent with previous Earth bow shock observations. Similar or higher M_A are commonly observed at Earth (Lalti2022), and significantly larger electric fields are typically measured (Wilson2014, Wilson2021). Why then are the results different? The authors must state why non-linear activity at Jupiter at this M_A is different from bow shock crossing with similar characteristics observed at Earth. I think that simply stating the activity exists is not sufficient for a strong conclusion. Especially since the conclusion relies on the comparison of the results with typical Earth's bow shock observations.

Clarifications are included in the updated manuscript. (line 266-274)
To compare Earth with Jupiter, it is necessary to use the **normalized wave amplitude**, because the raw magnetic or electric perturbations (δB or δE) do not represent the same physical conditions in each environment. For example - solar wind ion thermal energy is $U_i = \frac{3}{2} n_i k_B T_i$ and wave energy at bow shock is $U_w = \frac{\delta B^2}{2\mu_0} + \frac{1}{2}\varepsilon_0 \delta E^2$. As heliocentric distance increases, both solar wind ion number density (n_i) and temperature (T_i) decreases. Therefore, when comparing different planets, the appropriate quantity is the **normalized wave energy**, $\frac{U_w}{U_i}$. Without this normalization, one would incorrectly infer that Earth exhibits much larger electric field amplitudes for similar M_A (e.g., Wilson 2014; Wilson 2021) compared with Jupiter. **This clarification is now included in the discussion section** (line 271-274).

The need for stronger wave nonlinearity at Jupiter arises because the **temperature increase across the bow shock** is much larger there. Observations show a downstream-to-upstream typical ion temperature ratio of $\frac{T_i^{downstream}}{T_i^{solarwind}} \sim 80$ and electron temperature ratio of $\frac{T_e^{downstream}}{T_e^{solarwind}} \sim 10$, are much larger at Jupiter compared to ~18 and ~5 , respectively, at Earth. Consequently, waves at Jupiter's bow shock must be more efficient at heating both ions and electrons.
Explanation added (line 266-268).

To clarify the concern: "Similar or higher M_A are commonly observed at Earth (Lalti2022), and significantly larger electric fields are typically measured (Wilson2014, Wilson2021).**"** - we show some comparative calculation. Solar wind (E & B) power approximately decreases as $1/r^2$, where r is the distance from Sun. So, roughly speaking, waves generated due to bow shock at Jupiter (5 AU), should be ~25 times (5^2) less energetic compared to Earth (1AU). In our case, a observed wave (ECDI) amplitude of 60 mV/m at Jupiter is equivalent to ~ 1500 mV (60*25) at Earth – much larger than what is reported at Earth.
This clarification is now included in the discussion section (line 266-274).

"Why then are the results different**?**"- M_A (Alfvén Mach number) at the shock does not tell the whole story. Both terrestrial and Cassini observations show that as M_f (fast-mode Mach number) increases, shocks tend to become more turbulent (i.e., exhibiting more and larger gradients in B_0). We could not measure M_f in our case due to instrumental limitations. However, outer planets such as Saturn and Jupiter have larger fast magnetosonic Mach numbers due to decreases in both the Alfvén and sound speeds, and thus in the fast-mode speed. Given that previous work has shown an increase in electrostatic wave amplitudes with increasing M_f, we expect larger-amplitude waves at Jupiter, which is confirmed by our observations. Ultimately, this is necessary to accommodate the larger plasma heating requirements at Jupiter, as stated above. It should be noted, however, that the source of free energy is the change in kinetic energy flux of the plasma across the shock. At a heliocentric distance of 5 AU, there is less overall plasma, so the absolute amplitudes may be lower than at Earth; hence the need to use normalized amplitudes.

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2) Reformation and rippling have been observed at Earth's bow shock, especially if by reformation you mean non-stationary behavior (Johlander2016, Liu2020, Khotyaintsev2024, Lotekar2025). With just two bow shock crossings is hard to conclude that reformation is more common at Jupiter. Especially, since there doesn't seem to be a significant difference in shock parameters between these observations and a typical Earth bow shock crossing. Additionally, without the context of typical Jovian conditions, it is difficult to assess if these bow shock crossings resemble common Jupiter bow shocks or if they are rather exceptional crossings. Similarly, the claim that non-linear behavior occurs more frequently at the Jupiter's bow shock is a bold statement, given that the study is based on only two shock crossings, where only one of these crossings showed non-linear electrostatic activity. I suggest rephrasing the text to avoid misconceptions, indicating that in comparison with Earth, the differences are not as strong. Alternatively, you can provide better justification on why reformation and non-linear activity are more common at Jupiter's bow shock compared with Earth's.

We **revised** the result and discussion section to better distinguish between the different types of foreshock magnetic turbulence. The nearly constant repetition period of the perturbations (~0.3 $\mathcal{T}_{cp}$) observed both upstream and downstream of the shock in Case II likely rules out many shock-front phenomena that typically appear as multiple, irregular magnetic fluctuations in single-spacecraft measurements. The magnetic microstructures observed in Case II probably arise from the mechanisms described by **Mazelle and Lembège (2021, Ann. Geophys.)**, including: (a) accumulation of reflected ions in the shock foot at some distance from the ramp; (b) shock self-reformation driven by the modified two-stream instability (MTSI). Shock self-reformation is expected to be more prevalent, stronger, and easier to observe at the outer planets than at Earth as **demonstrated in Fig 7**, owing to higher Mach numbers, lower plasma β, and larger ion kinetic scales in the outer heliosphere. Each microstructure within the shock front has a characteristic width on the order of the ion inertial length, which increases with heliocentric distance. Wave–particle interactions within these microstructures contribute to plasma heating. The reformation observed in Case II closely resembles the self-reformation processes expected at the outer planets and is similar to those reported at Saturn. Nevertheless, irrespective of the underlying mechanism, wave activity at the locations of magnetic perturbations—whose spatial scales are approximately an order of magnitude larger than those at Earth—contributes to enhanced plasma heating. That said, we **agree** that, in the absence of a dedicated statistical study—which is currently not feasible given the limited number of available bow-shock crossings—it would be **inappropriate** to claim that "**reformation is more common at Jupiter**" or that "**nonlinear behavior occurs more frequently at Jupiter's bow shock.".** Though, such behavior is **expected** to occur more often, given the need to achieve approximately three to four times stronger ion heating at Jupiter compared to Earth. We have rephrased (line 347) the text. In addition, both the **Case I and Case II crossings are classified as typical bow shock crossings** according to the **model** of Rutala et al. (2025). These events therefore do not represent exceptional or unusual shock encounters.

3) I am also concerned about the Bernstein waves in Case II. The IAWs in Section 1 of Case II seemed to be highly Doppler shifted, but from the PSD of the Bernstein modes, they don't seem to suffer from the same Doppler shift. Why is this the case? For a similar Doppler shift to the one exhibited by the IAWs, one would expect less clear harmonics, unless in the electron frame. A clarification is needed to explain the difference in Doppler Shift.

The Bernstein part of the fluctuation has a different wave vector than the IAW part, both in magnitude and unit vector direction. The IAW part of the spectrum tends to have the unit vector direction roughly aligned with Bo. While the Bernstein side tends to have the unit vector direction more oblique to Bo. Thus, depending on the shock geometry, they would experience a different Doppler shift.

Minor comments:

Line 46-47: It is mentioned that electron heating occurs almost adiabatically, however recent studies have shown that non-adiabatic electron heating becomes relevant at High Mach Number Shocks (Lalti2024, Lindberg2025). This needs to be stated in a more accurate way in order not to mislead the reader.
Corrected in **line 46-47**.

Line 73-75: You can rephrase to make clear that reformation is also observed at Earth's bow shock. As it is written it gives the impression that one of the main difference is that pronounced periodic shock reformation structures are only found in Jupiter' bow shock but not at Earth. This is linked to my general comments, where the comparison between Jupiter's and Earth's bow shock doesn't seem to be entirely in agreement with Earth's bow shock observations.

Shock self-reformation structures are very distinct in case of Jupiter compared to Earth. **Figure 7** now shows the actual observational data for Earth, Jupiter, and Saturn to highlight this point. Rephrased the sentence (line 73).

Line 81: What is the typical standoff distance of Jupiter's bow shock? Is the 81.44Rj a usual radial distance for the bow shock, or does this crossing occur during special bow shock conditions?

According to bow shock model (Rutala et al., 2025; e2025JA033842), 81.44Rj **is a typical** bow shock crossing distance at that location.

Line 115: The fast magnetosonic Mach number Mfs is a better predictor of shock structure and dissipation mechanisms than M_A alone. Do you have any means of estimating Mfs? Is it possible that the difference between Earth and Jupiter's bow shock be related to significantly different Mfs?

Yes, you are right - the fast magnetosonic Mach number is a better indicator at the bow shocks of outer planet. But unfortunately, JADE does not have reliable observation of plasma parameters in the solar wind. Hence, we **could not estimate** the Mf.

Line 116: It is not very clear how exactly the theta_Bn is obtained here. You show two methods at the end of the article, maybe it is good idea to explain in the main text where which value came from. As I get it, the number outside parentheses is from coplanarity, and the number in parentheses is from MVA (which was invalid for the inbound shock). So, then how do you get the range 0.0~39° for the coplanarity on the outbound shock? Or I am reading this incorrectly? When looking at Fig. 3.D, the theta_Bn from MVA does not seem to go below 20°. Also, the rounding seems to be inaccurate.

Yes. We see the source of confusion. We **modified** the manuscript. As stated above (Note), an error in the input data file format for the outgoing case (Case I) affected the coplanarity-based calculation of Θ_{BN}, resulting in an incorrect shock normal value of 44^{o} in the original manuscript. The correct value for the outgoing shock (Case I) is 16^{o}. In the revised manuscript, we no longer use the minimum variance analysis (MVA) method to estimate Θ_{BN}, as previous studies of Earth's bow shock have shown this method to be highly unreliable for shock normal determination.

Old Figure 3D and 3E are removed as "**Foreshock shocklets and ULF waves"** section is **removed** from the manuscript.

Line 122: Is this variability on the reflected ion count rate typical for shocks of similar M_A? Fig. 1.C: The magnetic field signal looks more noisy starting from 2024-12-11 00:00. It seems that the magnetometer is changing sampling modes. If this is the case, this needs to be mentioned, as I couldn't find any comment in the text about this. Especially, because this will mean that both shock crossings occurred at different magnetometer modes. If this is the case, how does this impact your results?

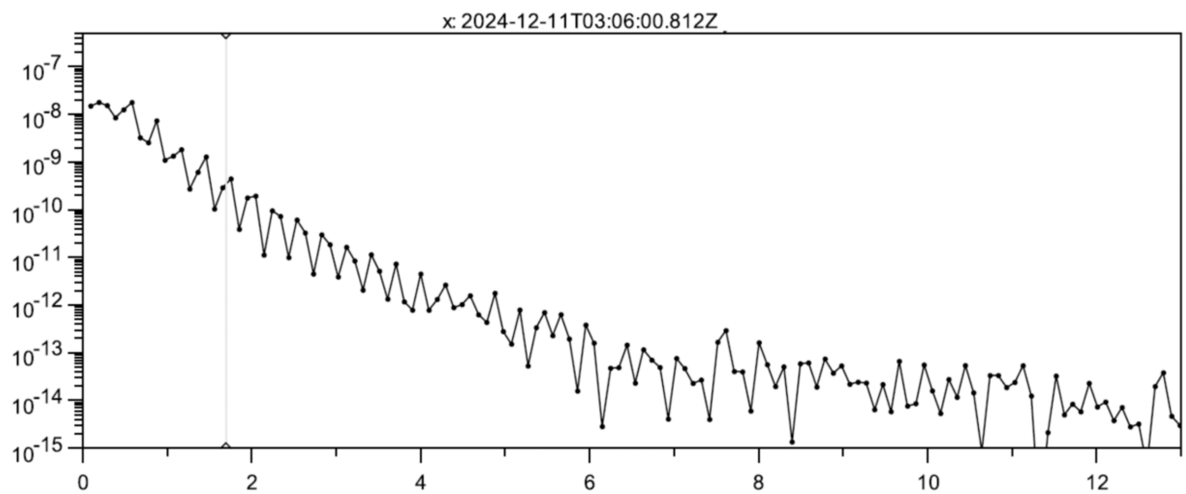
Yes, the reflected count rate variability is considered typical.

Last line of Fig. 1. caption says - "In panel C, the thickness of the plot varies as a result of changes in the magnetic field sampling rate". Both the shock crossing occurred at high (8 Hz) sampling rate of FGM. The nominal sampling rate of FGM is 1 Hz. For 2024-12-11 crossing, FGM was in high sampling mode well ahead of the shock crossing.

Line 194-195: It is not clear if the Bernstein waves are found in several intervals or just in the highlighted red interval in Fig. 5.E. Line 208: I don't see the dashed lines. I see the highlighted regions, though. Is the nonlinear wave behavior only found within these two intervals?

There are many regions with multiple harmonics of ECDI including the self-reforming fore shock region (example figure shown below). However, harmonic structure of IAW was only seen in one instance (Fig 5C). Added a sentence (**line 209-210**) to indicate this.

Thank you for pointing out the mistake (Fig 5) – the regions are indicated by solid (not dashed) blue/red lines. This has been corrected now (**line 195**).



Example of an upstream ECDI on 2024-12-11

Line 210: For consistency, use the same number of decimals in the milliseconds. Nonlinear IAWs in Case II: Have you confirmed that the observed harmonics are not instrumental? If the response of the instrument is not linear, then you can have such type of instrumental harmonics. Fig 5.C. I suggest adding in the caption, that the lines do not share the same Y-axis origin. It was a little confusing at first. It looks like the yellow interval has the highest PSD. I like the way you presented it, but in my opinion it will benefit from a quick explanation on how to read it. Fig F.B-C. In the caption it says that you are displaying 5 color-coded segments of 123 ms. This seems to imply that each color-coded segment is 123 ms, which is not the case.

Corrected the text for same number of decimals.

We have confirmed that the harmonics are not due to instrumental artifact. It is mentioned in Line 295-297 (old manuscript) or line 288-290 (new manuscript).

Thank you for pointing the error in the caption of Fig 5. Caption should read 20.8 ms (not 123 ms) (line 217). Figure caption also has explanation of how to read Fig 5C now (line 218).

Line 298: I suggest adding a comment on why it is not necessary that the non-linear waves have larger amplitude than the linear waves found in different intervals.

Comment added (**line 296**).

Line 310: Is it expected that the PSD spectrum of the IAW superimposed on the ECDI to be broader than that of the other IAWs?

We do not have a definitive answer. At this point, we are not aware of any systematic studies examining the width of IAW spectrum in the presence of cyclotron harmonics versus those without.

Line 328: A brief definition of the turnaround time is needed.

We rephrased the sentence – removed the use of ‘turnaround time’.

Methods Section: I suggest showing the upstream and downstream intervals used for calculating the different shock properties. For example, the windows mentioned in line 352.

Included (**line 374**).

Response to referees (Revision-2: Manuscript#NCOMMS-25-64264)

In this document, the reviewers' comments are shown in blue text, and our responses are provided in black text. In the revised manuscript (*Jupiter_bowShock_NatComm_rev2.pdf*), changes have been highlighted using different colors for each reviewer: Reviewer 1 in light green, and Reviewer 3 in dark green.

Reviewer #1

In their revised version, the authors made substantial changes to the manuscript, particularly by removing or reformulating several incorrect or unsubstantiated statements. Although they addressed most of the comments I raised in a satisfactory manner, some statements remain to me still unjustified. Finally, the authors analyzed Jovian bow shock crossings with characteristics like those of the terrestrial bow shock and found that comparable underlying processes are at work. In this stage, and I sincerely regret to I am not convinced that the work carried by the authors represent *an advance in understanding likely to influence thinking in the field* [Nature Portfolio Criteria for Publication]. In this respect, I am not persuaded that the last paragraph (Lines 344-349) – which summarize the main findings – well grounded.

We thank the reviewer for carefully reassessing the revised manuscript and for acknowledging the substantial revisions made in response to the previous comments.

The main contribution of this study is the very first high-resolution plasma wave observations at Jupiter's bow shock, enabling direct investigation of shock microphysical processes. This represents a crucial step toward determining how energy is dissipated at Jupiter's bow shock, where the required dissipation is substantially greater than at Earth's bow shock. Prior to this work, no observational data were available to constrain how energy dissipation operates in the outer-planetary environment. Our observations demonstrate that while dissipation mechanisms similar to those at Earth can persist at Jupiter, they also exhibit systematic differences in spectral characteristics and scaling behavior. We therefore view the significance of this work not as the identification of entirely new shock physics, but as the first direct observational extension of collisionless shock microphysics into a previously unexplored outer-planetary parameter regime.

We appreciate the reviewer's concern regarding the wording of the concluding paragraph. To address this point, we revised the final paragraph (Lines 356–359) to more clearly distinguish the observational results from their broader interpretation, avoid claims of universality that cannot be conclusively established from the limited number of events analyzed here, and emphasize the need for future studies to further establish the generality of these processes under outer-planetary plasma conditions.

i) The authors should emphasize that their analysis on two case events and does not have the pretention to yield a comparison with the terrestrial bow shock. Although, the Alfvenic Mach number (M_A) increasing with distance from the Sun, the present case study cannot not be considered as a differential indicator since the measured M_A compares well with that of Earth. Also, the β values are unknown. In this respect, Lines 72 and 73 (and 325) appear obscure.

Removed reference to high Mach number in lines 72-73. Line 325 is a general expectation at Jupiter's bow shock – not the particular cases described in this study.

ii) Lines 97-99: Partly incorrect. Again, as I explained in my previous report, the field-aligned beam ion population produced at quasi-perpendicular shock are backstreaming. Based on their statement, the authors mean the specularly reflected ions [these, indeed, re-encounter the shock].

This sentence is modified. We explicitly mention 'specularly reflected ions'.

iii) Line 102: In my view, Case II does not convincingly support the authors' claim.

This sentence is modified.

iv) I still think that Figure 2 is misleading, unnecessary and as I explain possibly incorrect. The authors modified the figure in re-establishing the nominal direction of the IMF at Jupiter. But does it correspond to the direction of the IMF during the time of interest (at the time of the Juno crossing)? This Figure also indicates that the IMF direction at the bow shock crossings is the same!

We reiterate that Figure 2 is intended solely to provide a conceptual and contextual visualization of the shock geometry relevant to the presented case studies, particularly for readers from related disciplines. The figure qualitatively summarizes the observations shown in Fig. 1 and illustrates the approximate trajectory of Juno relative to the Jovian bow shock, together with the associated regions of distinct wave and particle behavior.
The IMF direction shown in the figure is intended to represent the nominal IMF orientation at 5 AU and not the instantaneous IMF direction during the individual Juno crossings. We agree that the previous version of the figure caption did not sufficiently clarify this point and may have implied identical IMF orientations for the different crossings. To avoid this ambiguity, we have revised the figure caption to explicitly state: "This illustration is a conceptual rendering based on the interpreted spacecraft trajectory and plasma wave regions," and that the IMF direction shown is representative of the nominal IMF at 5 AU.
Should the reviewer and editor nevertheless consider the figure unnecessary, we would defer to their judgment and remove it from the manuscript.

v) To the best of my knowledge, the Jovian foreshock remains still unknow and I am not convinced that characterizations such as "electron foreshock" or "ion foreshock" is properly justified. These characterizations are not necessary to make the point; moreover, directly juxtaposing the terrestrial and the Jovian foreshocks is not necessarily valid.

We agree that the Jovian foreshock environment is not characterized as comprehensively as Earth's, and that a direct one-to-one mapping of terrestrial foreshock classifications to Jupiter may not necessarily be justified. However, we note that Gurnett et al. (1981; doi: 10.1029/JA086iA10p08833) in their Figure 2 present a classic schematic of the electron foreshock at Jupiter, just months after the Tsurutani & Rodriquez illustration was published. While the term 'electron foreshock' is not used, the physics of this region is unmistakable. Further, similar terminology has been used by Hoppe & Russell (1982; Nature, doi:10.1038/295041a0) for Jupiter and by Pisa et al. (2016; doi:10.1002/2016JA022912) for Saturn, where upstream wave populations are discussed in analogy with terrestrial foreshock regimes. Szego et al. (2001; doi:10.1016/j.pss.2005.10.011) support the existence of distinct foreshock regions at Jupiter. We submit that these regimes are recognized at Jupiter in long-standing literature.

But, to address this concern, we have revised the manuscript to avoid the direct use of the terms "electron foreshock" and "ion foreshock," and instead use "electron foreshock-like" and "ion foreshock-like" to emphasize that these labels are employed in a descriptive, morphological sense rather than as strict physical classifications equivalent to the terrestrial case. We now clarify in the revised manuscript (Lines 117–120) that this terminology is intended only to describe observed upstream wave activity with similar characteristics, and not to imply full physical equivalence with Earth's foreshock regions. We consider this revision sufficient to avoid direct juxtaposition of terrestrial and Jovian foreshocks while preserving a purely observational description of the upstream region.

vi) The Mach number determined by the authors is only correct at the nose of the shock (in other words, it corresponds to the solar wind Mach number and not the shock). Rigorously, one should consider the shock normal direction and the correct definition is:

$$M_A = \frac{\boldsymbol{V}_{SW} \cdot \hat{\boldsymbol{n}}}{V_A}$$

Jade could not measure the Solar wind velocity vector reasonably. So, we could not estimate the local M_A. The text has been corrected to reflect that we are only estimating M_A at the nose (lines 390-392).

vii) Line 331: For highly perpendicular shock, the micro-structures on the shock ramp are rather comparable to electron inertial length (*see* Mazelle & Lembège, 2021).

Thank you for pointing it out. It has now been corrected.

viii) Given the numerical value of θ_{Bn}, I would characterize Case II as an oblique shock rather a quasi-perpendicular on. In this respect, I would recommend that the authors examine the work of Lefebvre *et al*. (2009), Reformation of an oblique shock observed by Cluster, *J. Geophys. Res*., 114, A11107.

We acknowledge the comparison with Lefebvre et al. (2009). Based on the measured value of θBn = 54° ± 8°, Case II may lie within the oblique shock regime in a purely geometric classification.

However, in borderline cases, shock behavior cannot be inferred from θBn alone, as kinetic processes control the relevant dynamical regime. In this context, we examined the shock reformation behavior in Case II and compared it with previously reported cases. In Lefebvre et al. (2009), the reformation periodicity associated with upstream ULF wave activity is approximately 6.0 Tcp (|B| ∼ 8 nT, repetition period ~50 s), whereas quasi-perpendicular shock self-reformation typically occurs on significantly shorter timescales of ~0.3 Tcp (Mazelle and Lembège, 2021). Note that no ULF waves were observed in Case II.

The reformation periodicity observed in Case II is consistent with the shorter-timescale behavior characteristic of quasi-perpendicular shock self-reformation rather than the longer-period ULF-driven reformation described in Lefebvre et al. (2009). We therefore interpret Case II as exhibiting quasi-perpendicular-like reformation dynamics, despite θBn not being close to 90°, indicating that its microphysical behavior is closer to the quasi-perpendicular regime.

ix) The response given by the authors to my point # *xiv*) of my previous report on the initial manuscript is incorrect. The upstream (or downstream) rest frame is the frame where the solar wind (magnetosheath) is at rest, and it has nothing to do with the spacecraft speed [which is basically the shock frame]. Caution must be taken when comparing measurements with the theory and numerical simulations.

We agree with the reviewer's previous comment that, in simulation studies, the shock reformation cycle is generally determined in the plasma/rest frame of the upstream flow, whereas our estimate (~Tcp/3) is derived from spacecraft observations and therefore corresponds to the spacecraft frame. There was some confusion in our initial response.
Although the observational manifestation of reformation depends on the reference frame, the characteristic reformation scale is still governed by ion gyromotion and remains associated with the proton cyclotron timescale. In the plasma frame, shock reformation is commonly interpreted as the cyclic generation and advection of shock structures, while in the spacecraft frame it appears as a temporal repetition of magnetic structures during the shock crossing. Unlike multi-spacecraft studies (e.g., Cluster/MMS), our single-spacecraft event does not permit a reliable transformation from the spacecraft/shock frame to the plasma frame. Nevertheless, the observed repetition period (~Tcp/3) is comparable to previous observational reports of shock nonstationarity/reformation, including for example the Cluster-1 event at 17:38 UT on 31 March 2001, MMS-2 event at 05:38 UT on 11 January 2016, etc., which are regarded as representative shock reformation events in the terrestrial context.
Therefore, although we cannot unambiguously determine the reformation cycle in the plasma frame, the observed periodicity remains consistent with ion-scale shock reformation dynamics.

Reviewer #3

Line 274 - Taking the scaled amplitude rather than the absolute is now explained. However, it is still not entirely clear to me why the scaling factor is ~x10? I think providing an explanation on how you chose that value for the scaling factor will help future studies to conduct the appropriate analysis and comparisons.

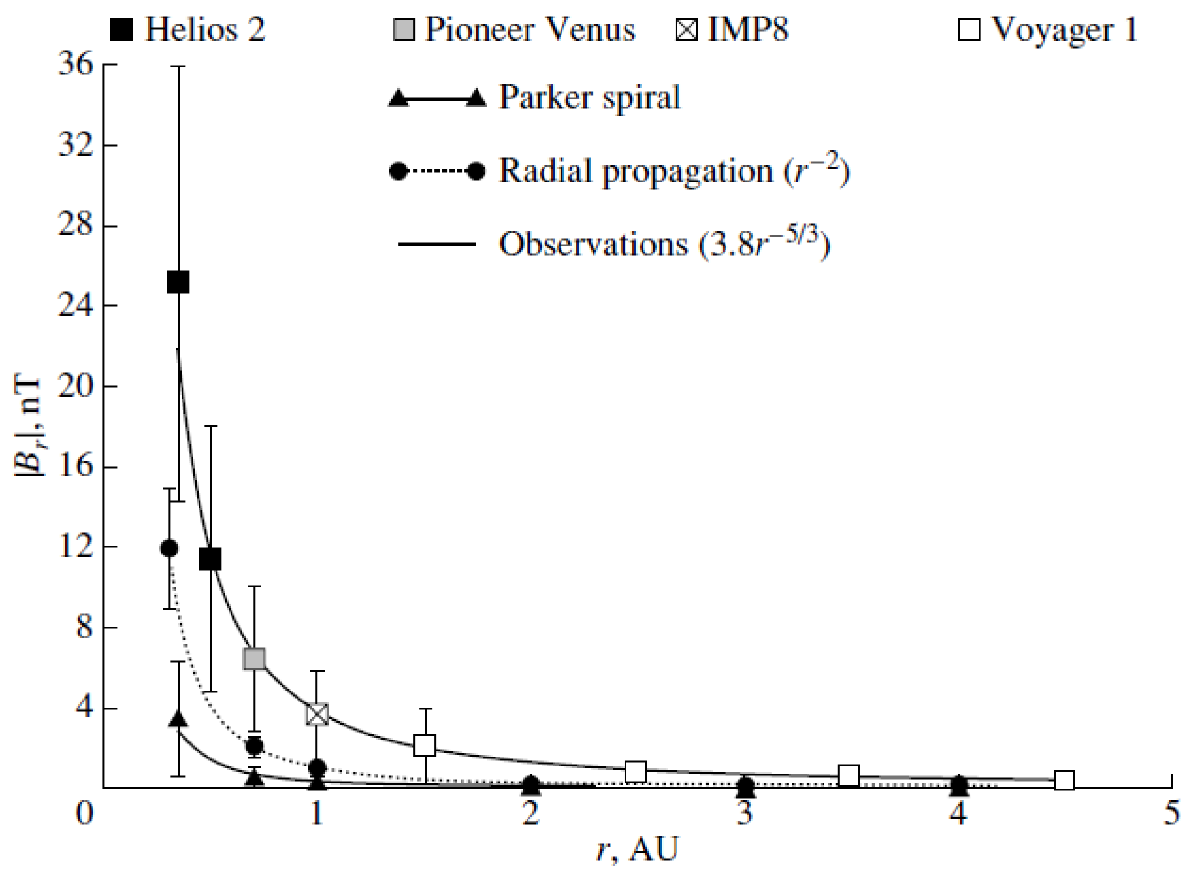
This section (lines 280–283) has been **revised to clarify** the origin of the ∼10 scaling factor. A detailed explanation is provided below.

The radial IMF component decreases with heliocentric distance as $r^{-1.66}$, as shown in the figure below, while the tangential component scales as r^{-1}, where r is the distance from the Sun (Khabarova, 2013). Consequently, the overall IMF magnitude varies approximately as $r^{-1.4}$ (Khabarova, 2013). Assuming similar relative wave amplitudes at Earth and Jupiter,

$\frac{\delta B_E}{B_E} = \frac{\delta B_J}{B_J}$,

where subscripts E and J denote Earth and Jupiter, respectively. The corresponding scaling factor is then given by:

$\frac{\delta B_E}{\delta B_J} = \frac{B_E}{B_J} = \frac{1}{5^{-1.4}} = \sim 10.$



Regarding IAWs, in your response, you mention that IAW part of the spectrum tends to have a unit vector direction aligned with Bo. While I agree with this, I don't understand if you have verified this in the data or if it is just the behavior you expect. If possible, you should estimate the propagation direction observationally.

Juno carries only a single electric field antenna located in the spin plane, which prevents us from determining the wave propagation direction. As a result, we **could not confirm** the propagation characteristics of the observed waves. This is just the expected behavior.

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The manuscript presents particle and field observations from two successive Juno crossings of Jupiter's bow shock. The study of planetary bow shocks and their associated plasma environments beyond Earth is of fundamental importance in space physics. This is especially true for the outer planets, where in-situ measurements are relatively rare. The terrestrial bow shock and foreshock serve as key reference models against which the bow shocks and plasma environments of other planets in the solar system can be compared. The authors have described the observations clearly, making the manuscript reasonably accessible even to non-experts in the field. Additionally, the figures are of high quality, which helps the reader to better grasp the various features of the event under investigation. However, the manuscript carries several statements that are not substantiated with solid arguments and some of them appear incorrect. Given the maturity of the field and the extensive literature on the terrestrial bow shock, a critical discussion of the empirical results is essential. A deeper comparison with previous studies related to Earth's bow shock would have been both appropriate. Consequently, at this stage, I am not feeling comfortable giving my agreement for the publication of the manuscript to *Nature-Communications*. I believe the authors present a potentially valuable case; however, and as presented, it does not meet the journal's quality standards. The manuscript would benefit from moving beyond a purely descriptive approach and incorporating a more in-depth, analytical discussion. My arguments are described below. The following points are not arranged according to their importance.

<u>Filbert et Kellog [1979] have demonstrated that strong correlation exists between the magnetic connection to the shock and the electrostatic emissions at the local plasma frequency. Near the tangent line, the emissions the most intense are narrow in frequency while the spectrum becomes progressively wider inside the foreshock.</u>

***i*)** Line 54: The authors claim that "… *the Mach number are typically higher than those of Earth* …". Could the authors provide references regarding this point? Typical Alfven speed at 1 AU is ~ 50 km/s (n_{SW} ~ 5 cm^{-3}, B_{SW} 5 ~ nT) which gives M_A ~ 9 for V_{SW} ~ 440 km/s (1 keV), which is similar to the estimated numerical value reported in the manuscript.

***ii*)** Line 94-96: These lines should be reformulated. Usually, a shock geometry with an angle of θ_{Bn} less that ~30^o is rather considered as quasi-parallel. Also writing "… *ions are reflected upstream in the direction of **VxB** electric field*" is incorrect. If reflection occurs specularly, ions gain a parallel and a perpendicular velocity. How do ions escape upstream if there is no parallel velocity since the solar wind convection pushes the particles anti-sunward? Also, the ion foreshock does not start at θ_{Bn} = 45^o. There is a rich literature on the topic that the authors could consult. Regarding the location of the low frequency waves in the foreshock, I recommend Meziane & d'Uston [1998] and Andrés et al. [2015]. Finally, ions that are produced at quasi-parallel regions are not the source of free energy for the foreshock wave excitation [As an example: Meziane *et al*., JGR, 2001; Mazelle *et al*., P&SC, 2003; Thomsen *et al*., JGR, 1985].
Also, I understand from the authors that what their description of the foreshock is valid for an *electron-proton plasma* [Line 94]? This is confusing and it is not clear to me what do they mean by "*typical electron-proton plasma*"?

***iii*)** Line 98-100: Incorrect statement and please reformulate. Particle do not have their velocity aligned with the electric field, otherwise they are [ad-infinitum] accelerated in the case of a steady solar wind. Only particles that are specularly reflected for $\theta_{Bn} > 45^o$ have their guiding center directed toward the planet; these cannot escape upstream. It is not the case of foreshock field-aligned beam produced at the quasi-perpendicular region. It is this population that provide the energy for the waves to grow [Mazelle *et al*, 2003; Meziane *et al*., 2005 or/and references above]. The authors seem to overlook that the ion foreshock extends beyond $\theta_{Bn} = 45^o$.

***iv*)** Figure 2: This figure appears both unnecessary and potentially misleading. The foreshock topology depicted is that of the terrestrial foreshock, based on seminal studies using ISEE-1 and ISEE-2 spacecraft measurements. Significant progress has been made since the observations from Wind and Cluster missions. Furthermore, how do the authors make sure that this terrestrial-based topology applies to Jupiter foreshock? In addition, the foreshock topology shown in Figure 2 represents a cross-section of the bow shock in the ecliptic plane, with a magnetic field orientation consistent with the Parker spiral at 1 AU. However, at 5 AU, the Parker spiral angle increases to approximately 80°, making the representation less applicable to Jupiter's environment. In my opinion, this figure should be removed as it does bring any instructive element to the manuscript.

***v*)** Line 115-116: The authors should estimate the uncertainties associated with the determination of θ_{Bn}. The shock geometry may not differ significantly between the two crossings.

***vi*)** Line 117-124: There appears to be some confusion regarding the distinction between the electron and ion foreshock. The foreshock is generally defined as the entire upstream region where interplanetary magnetic field (IMF) lines connect to the bow shock. In the terrestrial case, the concept of an "electron foreshock" arises because electrons accelerated near the IMF tangent point travel at high velocities and are only weakly influenced by solar wind convection, unlike ions. As a result, this narrow region is often referred to specifically as the electron foreshock. However, this does not imply that electrons are confined to this region alone; as illustrated in Figure 2, the yellow dots representing electrons are distributed throughout the foreshock. At this point, it is unclear how such this picture can be directly applied to the Jovian foreshock, given that the crossings of the investigated events are basically oblique in nature.

***vii*)** Figure-3: The observed modulation in the reflected ion count rate may instead arise from variations in the IMF connectivity to the source region [see for example Meziane *et al*., Ann. Geophys., 2004]. Additionally, the Langmuir wave spectrum appears broad—how does it compare to the solar wind plasma frequency?

***viii*)** Figure-4: Does the Langmuir wave spectrum appear narrower compared to the previous case? Additionally, what does the narrow band near ~ 500 Hz—observed throughout the entire interval—represent?

***ix*)** Line 201: The narrow frequency band near ~9.6 kHz appears to correspond to a plasma density of approximately 10 cm^{-3}. However, this value is typical of solar wind densities near 1 AU and is not expected at 5 AU?

***x*)** Figure 6: The figure should be constructed based on the actual direction of the interplanetary magnetic field.

***xi*)** *Correlation of reflected solar wind with shock normal angle Section:*
Studies by Fuselier *et al*. [JGR, **91**, p4163, 1986] and Meziane *et al*. [Ann. Geophys., 2004] have quantitatively addressed how shock geometry can inhibit the reflection mechanism. Cite these 2 papers if the authors want to keep this Section. Furthermore, it is unclear to me how the authors calculate the upstream θ_{Bn} angle, given that the spacecraft lost contact with the shock at the 17:25:00 UT crossing. This crossing may not necessarily result from spacecraft motion alone but could also be influenced by the motion of the shock itself. The only reliable way to determine the shock normal remotely in such a case is by assuming a model for the shock surface shape.

***xii*)** *Shock reformation Section*:
The authors provide no clear evidence that shock reformation occurs during this crossing. They merely describe variations in the magnetic field magnitude, which alone is insufficient to support such a conclusion. As presented, the analysis lacks the depth required to substantiate the claim.

***xiii*)** *Discussion Section*
Are the authors suggesting that the processes they describe are uncommon at Earth? If so, this is inaccurate, as there is a substantial body of literature on this topic. One of the most recent studies could be found in Mazelle & Lembège [Ann. Geophys., 2021]. In addition, previous reports outlining results from numerical simulations on the same topic are completely ignored by the authors. Moreover, as noted earlier, the authors do not present convincing evidence of bursty particle reflection. The observed modulation in the reflected ion count rate may instead arise from variations in the IMF connectivity to the source region.

***xiv*)** Line 325-327: In my view, this is a clear example where the authors fail to present any well-supported evidence to substantiate their claim. Once again, previous studies—such as those by Lembège and/or Matsukiyo on shock reformation—offer a sophisticated framework within which the authors could contextualize their results. In previous studies, the shock reformation cycle has been determined in the plasma frame of reference, whereas in the present case, the estimated rate (~ $t_{CP}/3$) appears to be measured in the shock frame.

***xv*)** Line 341: How the authors can claim that nonlinear effects *occur more frequently* based on a single event?

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In their revised version, the authors made substantial changes to the manuscript, particularly by removing or reformulating several incorrect or unsubstantiated statements. Although they addressed most of the comments I raised in a satisfactory manner, some statements remain to me still unjustified. Finally, the authors analyzed Jovian bow shock crossings with characteristics like those of the terrestrial bow shock and found that comparable underlying processes are at work. In this stage, and I sincerely regret to I am not convinced that the work carried by the authors represent *an advance in understanding likely to influence thinking in the field* [Nature Portfolio Criteria for Publication]. In this respect, I am not persuaded that the last paragraph (Lines 344-349) – which summarize the main findings – well grounded.

***i*)** The authors should emphasize that their analysis on two case events and does not have the pretention to yield a comparison with the terrestrial bow shock. Although, the Alfvenic Mach number (M_A) increasing with distance from the Sun, the present case study cannot not be considered as a differential indicator since the measured M_A compares well with that of Earth. Also, the β values are unknown. In this respect, Lines 72 and 73 (and 325) appear obscure.

***ii*)** Lines 97-99: Partly incorrect. Again, as I explained in my previous report, the field-aligned beam ion population produced at quasi-perpendicular shock are backstreaming. Based on their statement, the authors mean the specularly reflected ions [these, indeed, re-encounter the shock].

***iii*)** Line 102: In my view, Case II does not convincingly support the authors' claim.

***iv*)** I still think that Figure 2 is misleading, unnecessary and as I explain possibly incorrect. The authors modified the figure in re-establishing the nominal direction of the IMF at Jupiter. But does it correspond to the direction of the IMF during the time of interest (at the time of the Juno crossing)? This Figure also indicates that the IMF direction at the bow shock crossings is the same!

***v*)** To the best of my knowledge, the Jovian foreshock remains still unknow and I am not convinced that characterizations such as "electron foreshock" or "ion foreshock" is properly justified. These characterizations are not necessary to make the point; moreover, directly juxtaposing the terrestrial and the Jovian foreshocks is not necessarily valid.

***vi*)** The Mach number determined by the authors is only correct at the nose of the shock (in other words, it corresponds to the solar wind Mach number and not the shock). Rigorously, one should consider the shock normal direction and the correct definition is:

$$M_A = \frac{\boldsymbol{V}_{SW}.\boldsymbol{\hat{n}}}{V_A}$$

***vii*)** Line 331: For highly perpendicular shock, the micro-structures on the shock ramp are rather comparable to electron inertial length (*see* Mazelle & Lembège, 2021).

***viii*)** Given the numerical value of θ_{Bn}, I would characterize Case II as an oblique shock rather a quasi-perpendicular on. In this respect, I would recommend that the authors examine the work of Lefebvre *et al*. (2009), Reformation of an oblique shock observed by Cluster, *J. Geophys. Res*., 114, A11107.

***ix*)** The response given by the authors to my point # *xiv*) of my previous report on the initial manuscript is incorrect. The upstream (or downstream) rest frame is the frame where the solar wind (magnetosheath) is at rest, and it has nothing to do with the spacecraft speed [which is basically the shock frame]. Caution must be taken when comparing measurements with the theory and numerical simulations.