Review step 1, reviewer 1

“Observations of high-frequency spectral peaks from in-situ waves in ice data: evidence for nonlinear waves in ice triad interactions?”

Authors’ answer (A): We want to thank the reviewer for their comments and suggestions. We believe that the comments raised by the reviewer are quite minor, and that we can address them in the next revision of the manuscript. A few comments are more about style of writing than about the science, and there may be different tastes on this matter - therefore, for the corresponding comments, we would like to find a middle ground, as we are happy with the manuscript as it is and we prefer to err on the side of verbosity and detailed explanations and discussions, though this may make the manuscript a bit longer, as we believe that this conveys more information and “general thoughts” that will be useful for future readers of our manuscript.

Reviewer Query (Q): The authors analyze experimental data of sea-ice waves in order to diagnose the possible existence of triad interactions. After preprocessing, analyses are based on cross-spectra, bicoherence, and triad diagrams.

The paper is interesting, although it seems a bit lengthy to me. The signal processing analysis is rigorous and instructive. From my background, I cannot evaluate the experimental side nor the possible impact on the community interested in waves in ice.

A: We want to thank the reviewer for their supportive comments. We believe that our results will be interesting to the waves in ice community, as this is to our best knowledge the first time wave triads are observed directly from in-situ measurements in the sea ice, due to the limited amount of measurements available in these conditions.

Regarding the manuscript length: we have made a conscious choice to have a manuscript that is relatively detailed, relatively verbose, and has in-depth discussions. We understand that this makes the manuscript a bit lengthy for people who are not specifically interested in the waves in ice field, but we believe that these discussions will be valuable for those people who are focusing and specializing on waves in ice. We also see that the other reviewer actually asks for even more discussions and details to make our point even stronger - meaning the next iteration of the manuscript will actually grow further in size. All in all, we would like to keep these discussions (and we will even have to add some more based on Rev 2’s feedback), though we can do our best to slightly shorten them at a few locations. Also note that some of the requests by reviewer 2 mean that we will extend some of the discussions presented in the manuscript.

Q: I would recommend publication after some revisions:

- My main concern is about the wave triad diagram. It is a key point of the paper, but it is not clear enough. A better explanation of this diagram and how to read it would help the reader. Possible ways could be a schematic diagram in section “2.1 Deployments considered”, and additional formula (at least in the appendix if needed) to better explain this diagram.

A: Thank you for pointing this out. We believed that the triad diagrams would be self-explanatory, as we believed that these are quite standard “textbook materials”-like. However, we understand that this may be a bias from our side, and that we cannot expect all readers to have a similar background as we have. Therefore, we are willing to add an Appendix explaining the way the diagrams are generated and plotted. We believe that this belongs in an Appendix, as this is “textbook-like”, though arguably niche, knowledge, rather than new knowledge.

- Q: Figures of results are referenced in section 2 and then again in section 3. Perhaps section 2 could refer only to some schematic plots or plots for the method applied to toy examples.

A: We would like to keep the manuscript as it is on this aspect; indeed i) the manuscript is already quite long as pointed out by the reviewer, and we would like to avoid adding even more “boilerplate / textbook-like” materials to it. ii) We believe that the methodologies we point to are quite standard, so we do not want to add too much “textbook-like” content that would clutter the article.

- Q: Page 7: References to Wikipedia should be replaced or at least complemented by references from scientific literature.

A: We had a dilemma when writing this paper: we find the Wikipedia articles genuinely useful and good summaries, but we agree that these are not “common” to refer to in scientific papers. At the same time, we could not find a small self contained reference to illustrate our point and be useful to the reader in the same way as the Wikipedia references are. Upon carefully weighing the pros and cons, we ended up with our current decision to refer to Wikipedia articles. We believe that this should be acceptable, especially as Wikipedia is version controlled with full history, and also archived separately (for example from the internet wayback machine). We can, however, add a reference to a standard textbook in addition to the Wikipedia references in the next iteration as asked by the reviewer, but we are quite sure that this will be much less useful than the Wikipedia articles, so we want to still keep the Wikipedia articles references that are already included.

- Q: The same figure analysis is repeated in the caption of figures and in the main text of section 3.

A: This is a conscious choice from our side. We find that we often read papers quickly at first, looking at the figures and their captions, especially for papers that are within our expertise domain. Therefore, we want to make sure that the figures + their captions can be read by someone expert in the field without the need to read the manuscript, at least in a first “quick read through” to assess the main lines of the study. This implies having detailed captions that repeat the main points discussed in the text, and we want to keep it like this. We are aware that some people recommend the exact opposite (i.e. the point of view that “captions should be minimalistic and not repeat anything written in the text” is quite common too), but we believe that choosing one or the other is a question of taste, and our taste is to have detailed caption that can be read standalone together with the figures, so we would like to keep this as it is.

- Q: Page 12: “As visible there, triads are indeed possible given this dispersion relation.” Why?

A: The fact that we find at all loci curves where the triad conditions are valid, i.e the “colored, bent lines”, is what shows that triads are possible. We recognize that this assumes that the reader is familiar with triads diagrams, and we can make this clearer in the next iteration of the manuscript and explain this in more details, as part of the point above about the triads diagram explanation appendix.

- Q: The paper – the section “4. Discussion” in particular – is quite lengthy to me, with several repetitions, and overly detailed descriptions of plans for future works. I think that the draft may be more direct and synthetic.

A: Thank you for pointing this out. We agree that this section is quite long, but we would like to keep these discussions as we believe that they are useful for the wave in ice community. We believe that a reader from outside the waves in ice community could skip over lengthy parts, but we want to keep the “meat” in the discussion for our readers who will be specifically interested about these. We can, however, take an iteration on section 4 to make the different discussions more clearly separated and shorter, and avoid duplication.

- Q: Would it be possible that the bicoherence you observe is the result of a linear interaction of the wave field with its heterogeneous media of propagation (B=B(x) and/or H=H(x)), rather than the result of a nonlinear wave interaction (3 waves)?

A: Thank you for raising this question, this is an interesting point that we can add in the discussion. We feel confident that we can answer “no” to this hypothesis, based on the data we have from both the T15 and Y20 datasets:

- we do not believe that what we observe is the result of the linear interaction between 1 single wave and an heterogeneous medium, because the bicoherence precisely indicates that the harmonic is phase locked with 2 lower frequency waves at the same time (more specifically, what the bicoherence demonstrates is that the harmonic wave phase is phase-locked with a given relative phase difference between the low frequency modes). So the higher frequency harmonic we observe is, with very high confidence, the result of the interaction between 2 lower frequency waves in the T15 and Y20 cases where we can perform the bicoherence analysis. This means that the harmonic we observe is the result of a nonlinear 3-wave interaction, not the result of a linear (or nonlinear) wave interaction between a single wave and inhomogeneities in the propagation medium.
- but we agree that an heterogeneous medium would influence the transfer of energy between 3 waves that fulfil the resonance condition and modulate the amount of energy transfers; however, this will still require the resonance condition to be met, this will still be a nonlinear 3-wave resonance rather than a linear wave-medium interaction, and this will only influence the details of how and how much energy is transferred by the 3-waves interactions, which we do not discuss here. This is visible from the equations discussed in both “Marchenko, 1999” references cited in our manuscript, where gradients in ice properties would introduce additional partial derivatives of group velocity coefficients, however, this would not change the nature

and key properties of the problem. We believe that this could be investigated in follow up works, but that this is out of the scope of the present manuscript - here, we only want to focus on analyzing in-situ data to show such interactions exist, and we believe that a detailed analysis of the energy transfers, for example by using a non-linear potential flow solver, is outside of our scope and would be too much complexity and work to add to our present manuscript. In addition, doing an in-depth analysis at any meaningful accuracy level would require precisely knowing the distribution of ice thickness and elastic properties in space, which we do not have and would require additional advanced measurements in future field campaigns (for example data from ice radar / electromagnetic measurements of sea ice properties).

We will add a short paragraph to discuss this into the next version of our manuscript.

As a side note, we were initially a bit unsure of how we should interpret this comment, and we wondered if the reviewer was thinking about an effect similar to e.g. wave shoaling (where the wave group speed decreases, so that wave height increases to conserve energy fluxes, when waves go on shallower water). However in the present case we should observe the opposite: both phase and group speed increase when waves go from open water to sea ice, or from thin to thick ice (as is the case when waves propagate into a fast ice cover in the T15 case; for the actual plots, see for example Fig. 17 of Rabault et. al. 2016, available openly as a preprint at https://www.researchgate.net/profile/J-Rabault/publication/305339430_Measurements_of_Waves_in_Landfast_Ice_Using_Inertial_Motion_Units/links/605c6001299bf173676905c8/Measurements-of-Waves-in-Landfast-Ice-Using-Inertial-Motion-Units.pdf), so that one would expect that the wave amplitude gets reduced, not increased, and even more so the higher the wave frequency considered. However, upon considering again the reviewer's comment, we believe that the question asked was rather along the lines of the points discussed above. But, if the reviewer was thinking about something similar to wave shoaling, the answer would also be "no, this cannot explain the harmonic either, actually quite the opposite".

Q: Typo

- Page 25 : "contributions from from J.V. Writing"

Thank you for pointing this out, we will fix this typo.