Review step 1, reviewer 2

“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 detailed comments about our manuscript. As visible below, we either see differently or even disagree with several of the comments from the reviewer, and we have data evidence that confirm our claims and rebut the reviewer’s comments. We are willing to work on the manuscript to make it clearer and more pedagogical, and to add some additional analysis (such as the biphase and considerations on the energy transfer equations) and discussions that provide better interpretation of our data, so that the points raised by the reviewer are answered in the next iteration of our manuscript.

Review of manuscript
Reviewer Query (Q) : The manuscript analyzes several sets of observations of waves in the Arctic, notes the occurrence of secondary high-frequency peaks in the power density spectrum, and investigates their relationship to 3-wave nonlinear interactions.

To follow TC review criteria:

Originality (novelty: does the manuscript represent substantial progress beyond current scientific understanding (new insight, concepts, methods, or data?).
Good.
I cannot judge whether the study represents "substantial progress" in understanding wave dynamics in the cold regions. The concept of hierarchical nonlinear wave interactions is quite old (mid-20th century) and rather well understood in the context of the non-frozen ocean. It is natural to ask this question in the frozen ocean environment.

A: We want to thank the reviewer for their kind words. Indeed, we are making it clear in the introduction that the core concepts of nonlinear wave interactions are well established (we cite, among others, the seminal work of Phillips 1966), and we also make it clear that these have been discussed in the specific context of waves in ice (see the references Marchenko, 1999). So, we are open about the fact that these aspects have been discussed for a long time.

However, we believe that this does not remove novelty to our work and manuscript. Indeed, in the context of waves in ice, wave triad interactions as we present them here have been discussed in the past (e.g. Marchenko 1999), but, to the best of our knowledge, never observed in real world data. Therefore, we believe that our work is the first confirmation, from field data and direct in-situ observations, that wave triads are indeed a relevant phenomenon in sea ice, which we believe is a significant contribution - essentially, confirming from in-situ measurements that phenomena that have been discussed / speculated theoretically for several decades do exist in the real world.

We believe that we are the firsts to present such observations because obtaining field measurements of waves in ice is difficult, datasets are limited, and time series are rare since most buoys are not recovered and only averaged under-sampled spectra are usually transmitted over satellite communications (so far: this may change in the future if satellite communication costs and power budgets are drastically improved), and that, therefore, our study is a "first" in actually presenting real world proof of the existence of this phenomenon. This may also push in the future further study of the phenomenon we report here.

Scientific quality (rigor; purpose clearly articulated, adequate methodology, compellingly underpinned by the evidence presented; methods and techniques valid and suitable? discussion appropriate and balanced way).
Fair.

1. Q: The goal of the analysis is not clearly formulated. If the question is whether there is evidence of 3-wave interactions, in my opinion, for the dataset that has some bicoherence analysis, the bicoherence maps give a clear answer: "yes." They exhibit a typical 3-wave phase coupling of the peak–second harmonic kind embedded in an overall linear wave field. The other datasets show perhaps similar peaks, and it is tempting to call these nonlinear interactions (the 3–4 peaks in Figs. 8–9 are intriguing). But without a phase coupling measure, these could just be independent wave fields. However, the authors seem to be looking for more, i.e., resonant 3-wave interactions. In other words, an exact match of frequencies and wavenumbers (Eqs. 3).

   Resonances are always of interest because, in the long time limit, the effect of non-resonant interactions vanishes. But the long time limit is an abstraction; unless waves are propagating on an infinite homogeneous ocean, every interaction has its scales of transience. Analyzing 3-wave interaction based on the resonant model begs the question: what are the scales of inhomogeneity relevant to waves in icy waters? I could not find much discussion about this question. Wave periods (spectral peaks) shown vary from 6 s to more than 15 s (Figs. 1 and 9). Are the conditions homogeneous for large enough spatial scales to justify resonances? Strong enough dissipation is enough to detune resonances.

A: Our goal is to investigate in detail observations of harmonics in waves in ice spectra - it is essentially an observations-driven study: we have obtained data over the years that contain interesting features, and we want to document and explain these features. Our analysis concludes that these observation features are compatible, and likely caused by, triad wave interactions in the sea ice. We can make this point clearer.

Regarding the point about the nature of the harmonics observed, and the distinction that the reviewer makes between "peak-second harmonic" and "resonant 3-wave interactions". We are probably using a different "nomenclature" compared to the reviewer: we believe that what the reviewer calls "peak-second harmonic" is what we call bound waves (essentially, nonlinear "deformations" of the first order linear wave theory, similar to the flattening of troughs and steepening of crests caused by 3rd order Stokes theory; these do not follow the linear dispersion relation as they are bound, i.e. disturbances, of the low frequency "carrier" wave), and what the reviewer calls “resonant 3-wave interactions” we call free waves (that is, “true” independently propagating waves, that have an existence on their own and follow the linear dispersion relation). We believe that this nomenclature that we use is well established in the literature: see for example 1) “Effects of nonlinearity and spectral bandwidth on the dispersion relation and component phase speeds of surface gravity waves”, Crawford et. al. 2006, 2) “On the dispersion relation of random gravity waves. Part 1. Theoretical framework”, Masuda et al 1977, 3) “On the dispersion relation of random gravity waves. Part 2. An experiment”, Masuda et al 1977, and many more works since these both in the field and in the laboratory that use the “bound wave” and “free wave” nomenclature.

Given this nomenclature, we believe that our manuscript is making it clear that in the T15 case, which is the one for which we have several buoys close to each others and we can estimate the dispersion relation, the estimated dispersion relation (fig. 3) shows that the harmonic follows the dispersion relation, i.e. that it is a free resonant independently propagating wave, and not a bound wave. In order to investigate this further, we have also, following the reviewer’s comment, computed not just the bispectrum but also the biphase of the harmonic. A bound wave with Stokes-like characteristics would correspond to a biphase of around 0 degrees, and a free wave could correspond to anything else depending on “relative phases” between the carrier and resonant waves, which determines how the energy transfer is taking place, and changes in space (see discussion below). We have done such a biphase analysis following the reviewer’s comment, and we find that the biphase is clearly different from 0 degrees, and closer in T15 for the IMU VN1002 to 90 to 140 degrees. This corresponds to an actively forced, growing resonant wave, as can be derived by considering the eqns. 2.5 of “Stability of flexural-gravity waves and quadratic interactions, Marchenko, 1999, or eqns. 3.4 of “Sum-frequency triad interactions among surface waves propagating through an ice sheet”, Pierce et al, 2024, which are essentially the same equations across both papers. We believe that this can be an additional proof that the harmonic part of the signal, at least in the case of the T15 dataset, is clearly coming from free waves that are propagating independently of the “main carrier waves” and are being, at the location considered, forced (i.e., receiving energy) from the main waves (the forced aspect explains for the high bicoherence value observed).

We agree that in the other cases, less information is available, so it is more difficult to come to hard conclusions on the nature of the harmonic waves. We believe that this is already made clear, but we can make this even clearer - we have much harder evidence for the T15 case than for the other cases, and we are well aware about this and believe that this should be clear already.

We are willing to add all these additional analysis and explanations and derivations, and make this discussion more prominent in the manuscript, in the next iteration. This will add a significant amount of text (several paragraphs, a few equations derivations, and at least 1 figure) to the manuscript.

Regarding the commentary about homogeneous vs. non-homogeneous ocean: we agree that the resonant triad interactions, and the associated free waves they create, will be associated with significant spatial inhomogeneity for the distribution of harmonic wave energy. This is intrinsic to the shape of the equations for energy transfer to and from the harmonic wave (see above), and this is also easily “visible intuitively”: due to the fact that the free harmonic wave propagates at a different phase and group velocity compared to the initial “fundamental” waves, in some areas the phases of the 3 waves involved will be in relative phases such that the harmonic is being forced and receives energy (which is the case for VN1002 given the value of the biphase we observe, as will be added in the next iteration of the manuscript), while due to the difference in the phase velocity, the relative phases and, hence, energy transfer, can be modulated and become null or even negative further down, before changing further to a relative phase where the harmonic is forced, etc. This means that the pattern in space (for sure in space, and maybe also in time) will be non trivial, with “node- and antinode-like” features. We agree this is not made clear in the manuscript at the moment, and we can discuss this in more detail and make this clear to the reader.

However, we do not agree with / understand the comment the reviewer makes about inhomogeneity: the ice cover is large enough (typically in T15 homogeneous on several km, which is several 10s to 100s of wavelengths) that the triad mechanism can force waves, as visible in the actual data we present, and we do not claim that the situation is fully homogeneous in space - we do not believe either that the situation has to be homogeneous in space for such triads to develop.

We do not see the relevance of the comment on the spread of wave periods either - indeed, different wave periods are observed, but this should not have much to say for the possibility of existence of the fundamental mechanism at stake.

2. Q: Data description is inadequate. This is supposed to be an analysis of wave fields that propagate in a very peculiar environment. Where were these measurements taken? Please give maps, locations of instruments, and types of instruments. Using cross-spectra and phase lags to estimate wavenumbers might be fine where wavenumbers are well defined (homogeneous conditions), but it’s a crude estimate otherwise. The spectral, cross-spectral, and bispectral analyses are superficial: no information about error estimates, bandwidths, etc. The authors seem intrigued by the bicoherence “blob.” Were they expecting delta function phase correlations?

A: We discuss at several locations in the manuscript that the data we analyze here have been previously discussed, and we refer to our corresponding articles (all of which are accessible on preprint servers or publisher websites or similar without paywall or limitations). We believed that, in the interest of brevity and to avoid repeating ourselves across our works, the best solution would be to refer the reader to these works if looking for more information. If the reviewer disagrees with that, we are fine to add Appendixes that repeat the information presented in our previous works (as long as the article proceeding fees will not be increased by this, we will check with the publisher), and this can be added to the next iteration of our manuscript. The same is true for the instrumentation used. We can add more information about these, and / but if doing so, we want to keep these as appendixes, to not clutter the flow of the reading for readers familiar with this specific field from before.

We are fine adding more discussions about error estimates and bandwidths in the next iteration of the manuscript. We do, indeed, perform cross spectra and phase lag analysis, and we are aware of the fact that this is not as good as e.g. taking a 2D FFT based on many synchronized wave measurement positions on a regular grid as can be done for example in the laboratory. However, gathering data from the real world in harsh conditions is difficult, and, given the data we have, and the field measurements that are commonly gathered on sea ice, this is the best we can do. We can make these limitations even clearer, though the good agreement with the theory in e.g. Fig. 3 shows that our analysis provides clear results despite the inherent noise. We discuss in the end of the manuscript that one could envision making much more measurements, but this would be a new and separate project far beyond the scope of this manuscript.

We are not really "intrigued" by the bicoherence blob - we just want to make it clear to the reader that, since real world conditions include a spectrum of waves with finite frequency spectral width, the harmonic energy is not concentrated on a single frequency (that would be the result of the interaction of 2 discrete frequency components), but spread over a blob (that results from many pairs of frequencies interacting). So we want to make it clear that the blob is the result of many wave triad interactions, owing to the spectral width of the main peak, and that we do not (and should not) expect discrete / "sharp" bicoherence maps. We can make this clearer in the next iteration of the manuscript.

3. Q: The discussion of the effect of the interactions suggests an underlying "energy cascade" idea, e.g., "These may be a signature of the energy that is being 'stolen' from the main peak and transferred into the high-frequency peak by the triad interactions." This kind of description makes sense in the infinite, homogeneous, non-dissipative ocean, which has a well-defined inertial range. Is there such a thing here? If there is, where is the evidence?

A: We disagree with the reviewer that this discussion makes sense (only) in the infinite, homogeneous, non-dissipative ocean. Even if the sea ice cover is of finite length, non-homogeneous, and dissipative (which it is, all 3 aspects), and the spectral wave energy distribution varies in time and space, having energy transferred to more strongly damped higher frequency waves is still going to be an increased energy sink for the main low frequency waves (even if there would be for example some energy transfer back and forth and a 2-way heterogeneous coupling). The higher frequency wave is more strongly damped than the two lower frequency waves that inject energy into it, independently of the factors mentioned by the reviewer. Said otherwise, even if the wave energy content at different frequencies is highly heterogeneous / varies in space and time, more energy dissipation will take place for energy that is distributed at higher frequency (as is very well established in the literature, see the references provided in the manuscript), independently of any additional complexity in the real world wave field.

We agree though with the reviewer that the "back of the envelope" calculation that we do in the discussion to estimate the order of magnitude of the associated additional energy dissipation due to the higher frequency wave, makes a simplification hypothesis based on uniform conditions. If the situation is highly heterogeneous, and there are areas with more or less energy in the higher frequency harmonic, the local value of the (harmonic) wave energy dissipation will vary in time and space, and the averaged net energy damping may be slightly different - however, getting an exact value is not our point here, as our goal is to estimate the relative order of magnitude between different phenomena. If we make the assumption that there is nothing special with the measurement position that was picked, we can argue that, in the absence of better possibilities, it is reasonable to assume that the conditions we observe are not an abnormal outlier but just "quite representative and standard" ones (a sort of Copernican principle applied to our modest measurements). Said otherwise, this is a “physics-” rather than a “mathematics-inspired” discussion that only focuses on estimating the expected relative importance of two different dissipation terms. We agree that calculating an exact accurate value is not attainable given the data we have both about the wave field and the ice cover, and is neither our goal nor attainable given the complexity of sea ice covers in real world conditions. To get an accurate answer, a large scale campaign would be needed, as is discussed in the manuscript.

We do not really see what point the reviewer is making about an energy “cascade” - usually, to the best of our knowledge, this term is used in the context of the turbulent energy cascade, which continuously covers a broad range of frequencies. Here, we observe mostly energy being limited to well defined regimes - a “swell and wind wave blob”, and the harmonic we consider.

We can make these points clearer in the discussion.

Significance (impact - contribution to substantial scientific understanding; new practical applications of broad relevance?)

Fair.

Please see above; concepts are old and misapplied. Might be relevant, but the manuscript does not make this point clearly.

A: See above. To summarize:

Though we agree that we can make our discussions and presentations clearer and more detailed, we disagree with the reviewer’s comment that the concepts we use are misapplied, see our answers to previous comments.

We agree that the existence of wave triads has been discussed for a long time, and we are perfectly open about this - see the introduction and the discussion about existing works, i) going all the way back to the Phillips 1966 seminal work for wave in the ocean triads, and ii) also discussing the works from Marchenko 1999 for specific in-ice triads.

What is new in our study, is that this is the first time, as far as we know (and we observer that the reviewer does not disagree on this point / does not provide references that conflict with this view), that actual direct in-situ observations of such wave in ice triads are obtained from field measurements. As we point out, obtaining waves in ice data is challenging, data are limited and fragmentary, conditions are harsh and make instrumentation survival highly stochastic, so this makes our study unique in this regard. Until now there was only speculation that waves in ice triads could happen and theoretical works on the topic, but we believe that our manuscript provides convincing evidence that triads actually exist in real world conditions, based on our real world measurements.

Presentation quality (results and conclusions clear, concise, and well-structured; number and quality of figures/tables, appropriate use of English language).
Fair. (See above)

A: See above.