Review of manuscript ”Nondimensional parameter regimes of Arctic ice keel-ocean flow interactions”

The authors Liu and Zemskova present a modeling study on the impact of under-ice keels on the underlying ocean dynamics. The study is focused on the concept of lee wave generation by flow over topographic obstacles. The authors use nondimensional parameters describing the effects of keels on the underlying ocean, and use existing modeling output to classify the ice cover and ocean conditions in the Arctic Ocean based on said parameters, applying an unsupervised Gaussian Mixture Model. They find these clusters to exhibit some spatial coherence and further use the identified regimes to inform a targeted, idealized modeling experiment. The experiment includes two-dimensional modeling of the effect of topographic obstacles on ocean flow, with the nondimensional parameters set to values representative of the derived clusters. The authors further use a nondimensional expression of internal wave drag to calculate drag for the different clusters. The authors interpret the modeling results with respect to energetics, and make several claims on the underlying dynamics explaining the energetics.

I find this manuscript is generally well written, the scientific topic is relevant, and most things are explained appropriately. From my perspective, this study has the potential for being published, but some issues and open questions need to be addressed before. More specifically, I have a few concerns about the experimental setup, the presentation of the interpretations, and I feel the study is not yet as impactful as it could be. Regarding the latter, I think relating the results stronger to parameter ranges, processes, and changes we currently *observe* in the Arctic would add to the study's relevance. I also think the presentation of the results and discussion would benefit from a restructuring.

I would like to disclose that I am no modeling expert. It is a compliment to the manuscript's quality that I feel I still understood the key aspects and approaches well enough to be able to make suggestions for revisions. I think this study offers an interesting approach to a relevant topic of current research, and its publication after the suggested revisions would be in the interest of the community.

1 General and major comments

These following aspects are my major concerns and suggestions. Some of these concerns can be addressed by referencing to additional literature, for which I made suggestions at the end of the document.

A. **The presentation of results is mixed with aspects that are more fitting to a discussion section**. I strongly suggest to use a more ”classical” paper structure by introducing a discussion section between results and conclusions, in which the authors clearly separate hard evidence from the analyses (”Results”) from interpretation and speculation (”Discussion”). The conclusion includes elements that rather belong into a discussion section, too. I also think that including new references is unusual for the conclusion, where I would rather expect a synopsis of the previous parts than new ideas and arguments.

B. **From my perspective, the introduction is missing details on mixing drivers in the ice-ocean boundary layer (other than ice keels) and the influence of seasonally changing stratification.** I suggest amending the introduction by more information on the physics in the under-ice boundary layer (IOBL), particularly the partitioning of ice-ocean drag and the effect of shallow stratification. (What are drivers/sinks of turbulence in the IOBL? What is known about the relative role of wave drag, form drag, skin drag? What are expected differences between marginal ice zone and consolidated ice pack?) While some aspects of seasonality are included in section 4.2, I am quite convinced that seasonality plays a larger role in atmosphere-ice-ocean coupling and the role of ice keels than the manuscript in its current form suggests. I recommend adding a paragraph on the seasonally varying stratification in the introduction. Ideally, the authors would repeat the GMM classification for specific seasons, but I would also be satisfied if the authors point out clearly (at the appropriate locations) that the seasonally varying stratification may affect the clustering (and perhaps speculate *how*), and this may pose a limitation to the applicability of the assumption of a pronounced surface mixed layer, which I doubt is reasonable at all times (see Randelhoff et al., 2017, for instance). It would be good to pick this up in the discussion and be clear about this limitation. Furthermore, the introduction could offer more details on lee wave generation (e.g. nonlinear vs linear regime, and particularly the $\chi > 1$ case that is omitted later). A little sketch like Fig. 1 from Shirasawa & Ingram (1991) could also be helpful in the introduction for non-specialists.

C. **The results and implications can be embedded more effectively into the related literature.** This comment strongly refers to the repeated statement in the manuscript that the study provides/sweeps a "relevant" parameter space (e.g. L456). While I do not doubt this in general at this point, this statement needs to be substantiated better. This comment also relates to the presentation of the C_{IW} estimates, see my specific comments to Figure 11, for instance. I am aware that this is a modeling study and I do not request/suggest inclusion of analyses of observational data. What I would strongly recommend, however, is introducing and discussing more observational *findings*. These aspects include keel depth observations (which I understand are commonly rather on the order of few meters, with *tens* of meters, as the authors state, being more on the extreme end), drag coefficient estimates, and upper-ocean stratification (and turbulence), see also my comment to L69. In my perspective, these aspects would add a lot of value to the authors' work, and make it more accessible and relevant for a broader audience. The authors may also compare their C_{IW} estimates to the "canonical/empirical" ice-ocean drag coefficient $C_D \approx 0.0055$, which however implicitly bundles effects of skin, form and wave drag, one may argue. I made literature suggestions on ice-ocean drag (and more) at the end of the review, whose inclusion I leave to the authors' discretion. I further suggest amending the (to be included) discussion section by reviewing the results in the light of the ongoing changes in the Arctic. One line of thought in the discussion could be: If one nondimensional parameter changes due to climate change – say, for instance, η increases everywhere – would that make areas that are currently part of Cluster X to being Cluster-Y-dominated, and what would that mean for lee wave generation? This is just a suggestion.

D. **The number of selected nondimensional parameters and derived clusters appears not optimal.** Here I need to stress that I am no expert on these clustering methods and nondimensionalization. However, a few things stood out to me, which I believe should also be presented more clearly to the non-expert audience. It seems odd that one of the nondimensional parameters is expressed as the combination of two others. Please explain why this is necessary and how it provides additional information.

Also, the number of free variables is only reduced by 1 in your approach. Is the Pi theorem helpful in this context? I understand that nondimensional keel steepness is generally an important variable, but perhaps it is redundant for the clustering and can be diagnosed later? Please correct me if I am missing something here. I understand that the choice of the nondimensional parameters is also motivated by the subsequent modeling experiment in mind, where ice keels are treated as seafloor in a flipped-buoyancy ocean. In the context of atmosphere-ice-ocean interaction, the situation is a bit more complex, as the ice can move between ocean and atmosphere, which is arguably quite different (or not?) from the idealized case the authors present. From my understanding, a big difference between (A) topographic lee wave generation at the seafloor and (B) lee wave generation below drifting sea ice is that there is not only transfer of momentum between ocean and obstacle (case A), but also between sea ice and atmosphere (case B), and the ratio of drag coefficients between ice-ocean and ice-air plays an important role in atmosphere(-ice)-ocean coupling. This cannot be resolved by the simulations. I do not consider this to be overly problematic for the presented study, but it is a shortcoming that should be discussed. One nondimensional parameter capturing some of the atmosphere-ice-ocean dynamics is the Nansen number, which includes ice-ocean and ice-atmosphere drag coefficients (and some densities). I think it would be very interesting to derive this parameter from the Flocco-simulation. But if I understand correctly, these dynamics cannot be assessed with the Oceananigans-simulations. Overall, I kindly ask the authors to clarify (i) if there are other parameters that could have been interesting but were omitted and (ii) why certain options (such as the Nansen number describing parts of wind-driven ice drift) were discarded. Also, I think the number of clusters may be reduced – but I am open to counterarguments. Please be referred to my specific comments to L261 and Figure 5 below.

2 Specific comments

Abstract and title

L2 "Sea ice keels modulate upper-ocean momentum and mixing through internal wave (IW) generation" — If I understand correctly, keels have more effects on upper ocean dynamics/mixing than "only" lee wave generation, it would be good to dial this statement down a bit. I also suggest to finish the sentence after "generation" and start a new one after.

L11 "open-ocean" is not a good descriptor here, as this can easily be misunderstood as "open water"

L12 "boundaries" can be misunderstood, perhaps "boundaries/perimeters of the ice pack" or another alternative would be more appropriate

L20 If Ri is supposed to be a Richardson number, which the nomenclature strongly suggests at this point, it is likely not so important whether Ri is 100 or 150, but it should make a difference whether it is 0.2 or 2. See also my comment on Figure 6 below.

L22 "physically relevant parameter space" and "parameterizations are credible" — here I think the authors need to be more careful, which is why I emphasized the comparison of the chosen/derived parameter ranges with actual observations, be it the keels, stratification, or observed drag (see major comment C above).

Introduction

L26, L67 ”typically on the order of tens of meters” — from Metzger et al’s 68000 profiles, approximately 49000 are smaller than 10 m, so I believe this statement to be a bit misleading, since the bulk of the keels is smaller than tens of meters. It would be good to clarify this better.

L4 I think Kawaguchi et al (2019) is not the appropriate reference here, I suggest picking an earlier one. Also, it would be good to introduce internal waves as a phenomenon a bit more thoroughly.

L31 ”IW [...] enhance momentum transfer” — transfer of momentum between what? What exactly is enhanced, compared to what?

L31 I also suggest also to cite an earlier/original source for this statement, the first description of internal wave drag (”dead water”) is often credited to Ekman (1906), I believe.

L33 ”this problem” — internal wave drag on sea ice or internal wave generation at the seafloor? The cited literature is rather covering the second aspect. What are main differences and similarities between those (e.g. strength of stratification, scales of obstacles, sea ice can move while the seafloor cannot)? This should be discussed here or in the discussion.

L37 Here it would be good to point out that stratification in the Arctic Ocean can vary seasonally, particularly in regions with ice formation and ice melt.

L46 How do the enhanced vertical heat flux (Skyllingstad) reconcile with the thicker ice (Flocco)?

L49 An equation for the internal wave phase speed would be insightful

L50 please explain the terms ”critical” and ”subcritical”

L51 please explain ”secondary internal waves”

L58 ”necessitating careful parameterization” — Parameterization of what? Internal wave drag, impact on mixing (e.g. eddy diffusivity or TKE production)?

L69 ”valid parameter ranges” and ”relevant nondimensional parameters” — Relevant for what? Mixing, momentum transfer, atmosphere-ice-ocean coupling? This is where I suggest the authors clearly write what they mean by ”valid” and ”relevant”. From my perspective, these statements also warrant briefly comparing the derived dimensional parameter spaces (for u_0, N_0 and the like) to actual observations (referring to major comment C again).

L73-80 Perhaps this can be condensed into one sentence, leaving room for brief information on the basic principles of GMM for non-experts

L89 ”greater IW generation” — What precisely is meant here? An energy flux from the mean current into the IW energy spectrum? Please be precise.

Data and GMM Methodology

L94-102 It would be good to provide a basic summary for the reader, e.g. model resolution, time step, and validation. How does the model handle/include ice morphology, particularly under-ice topography?

L100-102 ”identify parameter value ranges” and ”it is adequate” — Again referring to comment C, I think it would be crucial to discuss somewhere in the manuscript how *representative* the ranges based on the model output can be. I am neither questioning the merit of the presented study nor am I intending to criticize the approach, but I think the authors should assess and communicate shortcomings and limitations clearly.

L103 I admit this is arguably a bit picky, but I think the "interacts"-phrasing is not ideal here. Consider that the experimental setup only allows the keel to affect the ocean flow, but in reality the ocean also does affect the ice (melting/freezing, momentum and heat transfer). I think that "interaction" suggests rather this latter two-way relation. I do however not feel strongly about this.

L107 "sinusoidal topographic feature" — If I recall correctly, the setup from McPhee and Kantha is a sinusoidal relief with many bumps (as the authors write), how is that different from (or comparable to) a single-bump keel as in the present study? I also recall that the keel depths from their study are rather small (≈ 2 m or so)

L117 The vertical structure of the water column can be quite different in reality, particularly in summer, when stratification may extend to the ice itself. I understand that setting $z_0 = 0$ includes this case in parts, but the fixed pycnocline depth and the constant N_0 below can be an ill-suited conceptual model in some cases. I do not insist on using a different conceptual model, but I wish to see this limitation mentioned (or even its suitability assessed) in the discussion.

L122, L138 See major comment E

L127 Please briefly introduce the case of $\chi > 1$ here. Is this a case that could also lead to local energy dissipation without wave radiation? If so, it would be quite relevant for mixing! (In which case it should be discussed) See also my comment to L153 later.

L141 Figure 2 and Table 1 suggest that keels were usually shallower than the mixed layer, i.e. not protruding, at least on average. I suggest to rewrite this, something like "The larger η is, the smaller is the keel compared to the mixed layer depth, and the larger the distance from the keel to the pycnocline. "

L145 The choice for the variable symbol for the nondimensional parameter $\Delta b/(k_0 u_0^2)$ seems to be hinting to the Richardson number. If so, could you explain in the manuscript how Ri in Equation (6) is related to the gradient Richardson number $\mathrm{Ri_g} = N^2/S^2$? Furthermore, under which conditions is $\mathrm{Fr} = \frac{1}{\sqrt{\mathrm{Ri}}}$ – is this always valid?

L153 Could you please disclose how many cases with $\chi > 1$ were excluded (a fraction of the total would be sufficient). Is this regime important?

Figure 2 It would be insightful to show these maps for only summer and winter months, respectively (see comments about seasonality above). It would also be interesting to some readers to see maps with the *dimensional* parameters, which could go into an appendix or supplement, but I leave this decision to the authors. I recommend to make the fontsize of the labels larger, and to add a more verbose variable descriptor in the subplot title and/or the figure caption (e.g. "wave radiation potential χ).

Numerical simulations

L186 Is ρ_0 constant in time, depth, or both?

L187 Could you please elaborate briefly what the term fu_0 achieves effectively?

L195 What skin drag coefficient is applied between the solid boundary and the ocean? I suppose even for a zero-sized keel there should be some boundary stress? (This is a limiting case which can be used for highlighting/benchmarking the relevance of keels for mixing, so I suggest to pick it up in the discussion.)

L196 As mentioned earlier (in the context of seasonality), I have concerns about the representativeness of this idealized buoyancy profile. This concern could be addressed by mentioning potential shortcomings in a discussion section, or by showing how well the Flocco-simulation profiles (or real-world examples) can be approximated by this parameterized expression.

Table 1 I suggest adding the median values, too, given that some of the distributions are relatively skewed (Fig 6). Are the results sensitive to whether mean or median values are plugged into the numerical simulations?

L227, L229 I kindly request some clarification on the frequency range that is included in the u' fluctuations. If I understand correctly, the deviations u' are effectively including all spectral velocity variance below 6-hours period (the simulation length), since u_0 is constant. This includes not only turbulent fluctuations, but likely also a significant part of the internal wave spectrum (N to f). The kinetic energy spectra of internal waves and the fully-developed small-scale turbulence are qualitatively and conceptually different (as is their effect on ocean mixing). Therefore, I am not so convinced that this definition for u' is appropriate for analyzing turbulence, particularly for deriving the dissipation rate. I consider it more appropriate to use a higher cut-off frequency for defining small-scale fluctuations associated with turbulence. If this has no effect on the results, that would also be worth mentioning here.

L228 Just to clarify, there is no "additional" mixing parameterization or turbulence closure in the simulations, the buoyancy flux is determined by $-\kappa\partial_z b$?

Results

L258 While I find the results section well-written in general, it was at times somewhat hard to follow. As mentioned earlier, I am not convinced by combining the interpretation/discussion with the presentation of the results. To address both these concerns, I suggest restructuring Section 4 ("Results") into two new sections, Results (4) and Discussion (5), each with clearly defined subsections (e.g. introducing additional descriptive headings for each cluster in 4.1). I also think the authors do not need to list and discuss each derived parameter for each cluster[1], as long as the relevant numbers are available in a table. The text readability may profit from some cherry-picking in this case – what are the *key* differences between the individual clusters? I also suggest focusing the (subsequent) interpretation of these findings on selected main aspects and implications for the Arctic (and future research). Given that this will demand some major modifications of the manuscript, I will comment a bit more coarsely in the following.

L261 and Figure 5 "Each cluster reflects different oceanographic and sea ice conditions as will be discussed below. These clusters coherent geographic patterns despite latitude and longitude not being used as input variables for the clustering" – I would like to challenge this statement (or rather its generality). Particularly Cluster 1 seems quite scattered inside the Arctic Ocean (excluding Nordic Seas and Barents Sea). Furthermore, the clusters are again "clustered" in the Abstract (0-1, 0-2, 3-5); this seems like a dark-orange flag that main aspects of the Arctic ice pack can be captured by fewer than six clusters. Lastly, I would argue that the areas associated with Cluster 0 are only ice-covered in certain seasons, which makes comparisons with the other clusters (and associated physics) at least difficult. I agree that there are vast areas with spatially coherent patterns, which is interesting in itself. From my perspective, it seems that fewer clusters would to the trick, but there should (ideally) be a distinction between summer and winter (or time periods centered around minimum/maximum ice extent as extreme cases). I am curious about the authors' thoughts on this.

Figure 6 The authors could consider showing Ri^{-1} instead, since – if Ri is to be interpreted as Richardson

[1]Which would also be easier for four or five clusters only.

number, which L287 suggests – large numbers of Ri are less "interesting" for shear instability than the very small numbers, which are hard to assess in the plot as it is. Using Ri^{-1} is not uncommon in turbulence literature.

L306 (e.g.) I find the suggested role of the interaction of near-inertial waves and lee waves (linked to Nikurashin and Ferrari 2010) in several occasions too speculative, particularly so given that this is presented within the results section (i.e. not the discussion, where speculations are more appropriate) and the simulations cannot possibly resolve this effect due to the simulation length. For instance, "rotation is likely to not significantly influence the simulations as the total length of the simulation time is less than one inertial period" (L207) seems to be at odds with "enhanced dissipation below the pycnocline, possibly due to the interaction between lee-waves and near-inertial waves" (L305). I think these dynamics need to be either explained better and at the right locations (introduction and discussion, I suggest), or dialed down strongly.

Figure 7 and 8 Particularly for Cluster 2, 4, and 5, one can directly observe wave propagation in the ocean interior, it appears. It would be quite interesting and useful for further studies to quantify the key characteristics of these waves (horizontal and vertical wavenumber, wave frequency) and subsequently compare them with predictions from lee wave theory.

L363 Here the authors omit the presentation of J based on the grounds that it is similar to ζ, which – to me, at least – again raises the question as to whether it makes sense to have one of the nondimensional parameters being composed of two of the others. The nondimensional expression for C_{IW} can evidently also do with four nondim. parameters.

L380 "This finding can help limit the number of numerical simulations that need to be conducted." – From my perspective, this statement would have much more impact if it were shown before that these parameter ranges have some resemblance with actual observations, and not "just" an (albeit very interesting) numerical simulation. Particularly my comments on L57 and L100-102 are relevant in this context. I will not adamantly request that the authors add a comprehensive comparison with observations, but I strongly suggest to pick up this topic in the discussion. I agree that one can learn many things from the present study – but some things we can not, which should also be presented more clearly than it is currently done.

L383 and following The analysis of the internal wave drag coefficient is a very interesting aspect of this study. This is one reason why I suggested to include more information on the partitioning of ice-ocean drag in the introduction (see above). At this point, the text (L383 to L296) reads rather like an introduction or methods part, and I think it would be better suited in one of these sections. Also, I would appreciate a few derivation steps of Eq. 21, perhaps in an appendix or supplement.

L393 please reference more than one ice-ocean model here

L398, Fig 10, Fig 11 The values for C_{IW} vary *substantially* in magnitude, which suggests that there are situations where wave drag is important and some where it is not. Particularly for this reason I strongly suggest to reference these derived values to some "benchmarks". These *could* be (i) the skin drag coefficient in the 2D model from the study (e.g. assuming there is no keel at all), (ii) ice-ocean drag coefficients in common ocean models (if drag is not parameterized), (iii) skin drag for relatively smooth ice (see Reifenberg et al., 2025; Shirasawa & Ingram, 1991, and recommendations below) and observed ranges of drag coefficients under different boundary layer regimes and ice conditions (Fer et al., 2022; Kawaguchi et al., 2024; Cole et al., 2017, 2018, and recommentations below), or (iv) other studies of drag, its partitioning, and its impact on the coupled system (Martin et al., 2016; Lu et al., 2011; Brenner et al., 2021). The motivation behind this suggestion is that it would enable the authors to clearly put the derived C_{IW} into perspective.

Figure 11 It would likely be more insightful to add winter-vs-summer composites here (or an appendix) rather than referencing Figure 9 in the text, the same holds for Figure 10.

Conclusions

L429 The conclusions are also well written and easy to follow. Similar to the results section, the conclusions feature some aspects that I rather consider to be an ongoing discussion of the results and their limitations. I suggest to move some of the content to a dedicated discussion section. It could also be insightful to provide suggestions on future observational studies. What kind of observations (e.g. turbulence in the wake of keels) would be useful for putting your results into more context?

L442 "The GMM fit used only nondimensional parameter values at each grid point (no geographic predictors), so the geographic coherence in our results reflects underlying mechanics rather than explicit location features" – I do not quite follow. Is the spatial coherence of the nondimensional parameters not rather a consequence of the input fields also being rather coherent in the model? I kindly ask the authors to explain this statement a bit better.

L456 "The results of this study also revealed the ranges of values for these five nondimensional parameters that are relevant to the Arctic sea ice." – It is particularly this statement that made me point out so frequently that the term "relevant" here should be clearly substantiated by placing the ranges into some context of existing literature.

3 Minor technical comments

L12 Ri should not be italic

L235 and later many style guides recommend setting subscripts that are words or abbreviations in non-italic font, i.e. A_{pyc} instead of A_{pyc}, which would also apply for "above"/"below" etc

Table 2 the units should be formatted like $\mathrm{m}^{x}\ \mathrm{s}^{-y}$

Figure 3 It would help the non-expert readers if BIC was written out once in the caption or figure title
L287 typo? "keel"/"keep"

Figure 7 and 8 It would be helpful to have a close-up version of the top 100 m, perhaps in the appendix. A lot of the interesting dynamics occur there. Furthermore, the axis labels are too small. Is there a typo in the colorbar label of 7k?

Figure 9 Please make the labels larger. Again, the authors may consider to plot inverse Ri instead.

4 Notes on recommended/suggested literature

Here I list a few studies that could be included by the authors. The authors do not need to address in their response whether and why they included an individual study or not, these following notes are just meant as helpful suggestions.

Krumpen et al. (2025)
The authors mention that there is no pan-Arctic dataset for under-ice keels (which I think is correct). Krumpen

et al. (2025) however present a plethora of ridge data from the ice surface. While this does not necessarily imply that the ice underside looks the same, it may be reasonable to assume that there is some correspondence of surface and under-ice properties. They also show that ice became smoother recently, which has implications for the atmosphere-ice momentum transfer, and likely implies that the underside will also become less ridged. I think this could be a good point for a discussion of the future role of ridges on upper ocean mixing. Observational efforts like from Anhaus et al. (2025) and Brenner et al. (2021) may also enable obtaining better spatial coverage of keels in the future.

Reifenberg et al. (2025)
This is an observational study about turbulence under sea ice, but other than the present work it is from a regime with relatively smooth, un-ridged ice. They estimate a skin drag coefficient of $C_D^0 = 0.0007$, which is very much on the lower end of observed drag coefficients in the Arctic. Together with the studies presented below, the authors could use this work to (i) discuss regimes where keels are not present (which may be the case in the future Arctic with younger and smoother ice) and (ii) benchmark the estimated wave drag coefficients (Fig. 11). Reifenberg et al. also discuss turbulence energetics in the stratified boundary layer. The impact of buoyancy fluxes on turbulence production and suppression is not included in the simulations of the reviewed manuscript, which I think is an entirely valid approach, but this still poses a missing source/sink of turbulence that should be taken up in a discussion on the relevance of mixing from keels.

Fer et al. (2022); Kawaguchi et al. (2024)
These observational studies also provide drag coefficients in different dynamical regimes. The authors could add these drag parameter ranges in the plots referencing C_{IW} (e.g. as horizontal lines in Fig. 11, or as markers on the colorbar in Fig. 10), and discuss how/mention that the ice-ocean momentum flux is not only controlled by wave generation from keels, but also from boundary layer stability, form drag on floe edges, or skin drag (i.e. roughness).

Randelhoff et al. (2014), Randelhoff et al. (2017)
These studies discuss the effect of summer stratification on upper ocean turbulence and drag. As mentioned before, there may be no mixed layer at all in summer. This information should be part of the introduction. Meltwater also has effects on momentum transfer other than internal wave drag through compressing the Ekman transport vertically, for instance. This also why I kindly request the authors to give seasonal aspects more consideration in the manuscript.

Brenner et al. (2021)
This is an insightful study about the effect of ice morphology that you should include in your introduction and/or discussion. Interesting note in the abstract of Brenner et al. (2021): "[...] reveal that keel drag is the primary contributor to the total ice-ocean drag coefficient". This seems a great motivation for the presented study here.

Cole et al. (2014), Cole et al. (2017), Cole et al. (2018), Fine & Cole (2022)
These studies use larger datasets to study internal waves and stratification in the upper Arctic Ocean, and how these relate to a changing sea ice cover. The authors could use these studies to better describe the influence of ice morphology on internal wave generation, and to better interpret the different clusters in a geographical and seasonal context.

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