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.

We thank the reviewer for their comments. We added the specific discussions as requested by the reviewer, which are denoted as *italicized* text in our response below.

We would like to especially thank the reviewer for many insightful suggestions and an extensive list of helpful literature citations.

Based on the suggestions from both reviewers, we implemented the following major changes:

1. We have re-done our clustering analysis with four nondimensional variables (lee wave radiation parameter \chi, keel criticality J, depth ratio \eta, and Froude number Fr) and numbered the clusters in the order of decreasing area that they occupy. Therefore, the cluster numbers are different from the original draft.
2. We added seasonal panels to all figures that previously only showed distributions for annual means (Figs. 2, 3, 4, 5, 6, 9, 10).

3. We conducted three additional numerical simulations to investigate regimes where the keel depth is larger than or comparable to the pycnocline depth (e.g., Fig. 11).
4. We restructured the manuscript making a separate section describing the IW drag parameterization and the nondimensionalization (Sec. 2).
5. We restructured the results section breaking it into the discussion of the annual clusters and differences in flow characteristics depending on the nondimensional parameters from numerical simulations (Sec. 5.1) and seasonal effects and IW drag variability (Sec. 5.2).
6. We restructured the manuscript, shortening the results and conclusions sections, to add a new Discussion section with two subsections: Implications (Sec. 6.1) and Limitations (Sec. 6.2) of the study.
7. We have re-written the abstract to avoid too much specific jargon (e.g., symbology for the nondimensional numbers) and specific values.
8. We added Supplementary Information figures: (1) Fig SI1 of spatial distributions of the dimensional parameters, (2) Fig. SI2 of comparison of distributions of keel depths from the model data used in this study and observations, and (3) Fig. SI3 of comparison of spatial distributions of GMM clusters for a different number of K (number of clusters).

Specific responses to the Reviewer's comments are below and we note the line numbers where we made the corresponding changes in text.

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.

We now have a dedicated "Discussion" section (Section 6) where we address the key points raised by both reviewers and moved some of the discussion from the previous "Conclusions" section. The "Discussion" section is separated into two parts: "Implications" (Sec. 6.1) and "Limitations" (Sec. 6.2). The specifics of these points are addressed in the subsequent specific comments by the reviewer.
The "Implications" subsection covers:

1. Potential changes in the Arctic: e.g., smoothing of the ice, changes in stratification
2. Ranges of values of nondimensional numbers found in this study in relation to previous numerical studies
3. Comparison with ice keel form drag coefficients and dissipation values from observational literature

4. Seasonal effects

The "Limitation" subsection covers:

1. Absence of form and skin drag from our simulations due to choosing free-slip boundary conditions along the ice keel
2. Other physical processes omitted in the simulations, e.g., three-dimensional effects, sheltering effects, effects of the rotation, turbulent heat fluxes
3. Not considering air-ice interactions: we consider that the wind has already transferred momentum to move the ice keels and investigate the interaction only between the ice keel and the underlying ocean.
4. Our choice of the number of clusters in GMM analysis

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.

Addressing each of the points:

1. (Now on L20-40) We now have included a more detailed description of the form/skin drag (within the boundary layer) and IW drag for the ice keel (at the pycnocline). We also added a statement about the internal ice-ice stress, which would be important for consolidated pack ice, and not in the marginal ice zones. However, as this is not a review article on the ice-ocean boundary layer, we kept this portion relatively short and opted to include a number of references instead to keep the introduction section more on point.

2. GMM seasonality: although it was perhaps unclear, our GMM clustering in the paper was done separately for annually-averaged and seasonally-averaged (over summer months and winter months, separately) data. This is now made more clear in text (L232-233) and we added summer and winter panels to all of our GMM analysis figures (Revised Figures 2, 4, 5, 6, 9, 10).

3. (Now on L106-112) We included the information on the seasonality of the Arctic stratification in the introduction.

4. Seasonality: in addition to now explicitly showing the seasonality in all of the Revised Figures 2, 4, 5, 6, 9, 11 and other discussions that we outline below (seasonal differences in Ciw values, shallow mixed layer depth in the summer) we add the following discussions about seasonality of our results:

- Results (Sec. 5.2): L431-451, L483-486
- Discussion (Sec. 6.1): L496-498,

5. (Now on L533-542 & L615-619, Fig. 11) Shallow mixed layer: We would like to stress (as the reviewer also mentions later) that the absence of a surface mixed layer is not a problem from the point of view of the numerical simulations in examining the parameterizations. This just means that we would set \eta=0 and expect that the keel direct contact with the stratified layer (not buffered by the mixed layer) would increase the IW generation and IW drag. We have performed additional numerical simulations for such cases (S0 and S2, see Table 1 and Fig. 11). The limitation only arises from the dataset that we considered in this study from NEMO output that does not measure mixed layer depths shallower than 10m, meaning that such shallow (or absent) mixed layers are not present in the GMM clustering. We added this point to the "Implications" (L533-542) and "Limitations" (L615-619) subsections of the "Discussion".

6. (Now on L41-64) We added the following more thorough description of the classical flow-topography interaction problem regarding lee wave generation into the "Introduction".

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 CIW 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 CIW estimates to the "canonical/empirical" ice-ocean drag coefficient CD ≈ 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.

We thank the reviewer for these insightful suggestions. To address this Major Comment, we added the following discussion points into our "Discussion" section:

1. Comparison of the dimensional parameters (as far as we can assess them) from the Flocco model and the observations and the implication of the differences
    a. (Now on L615-619) Shallow pycnocline in the summer which is not captured by the Flocco model as the shallowest values of the pycnocline they capture in the model is 10 m (please see our response to Major Comment B)
    b. (Now on L604-614) Potentially faster ice drift speed predicted by the Flocco model in comparison to observations, which can lead to larger Froude number Fr and hence larger predicted IW drag
    c. (Now on L614-624, Fig. SI2) We also compared the distributions of sea ice keel depths with some of the available measurements from literature. As shown in the Supplementary Figure 2, the distribution from the Flocco model output overall agrees with the distribution found by Metzger et al (2021), especially as their study does not separate keel depths smaller than 6m, so the results agree well if we consider the total "less than 9m" bin. Small keel depths (less than 3m) have also been observed by other studies (e.g., Brenner et al, 2021), so we believe that the distribution that we use in our study is reasonable.

2. (Now on L502-519, Fig. 11) Comparison of Ciw values calculated from the McPhee and Kantha parameterization with skin and ice-ocean drag coefficients from observations and the canonical ice-ocean drag value – please see the text and new Figure 11.

3. (Now on L563-580, Fig. 12) Comparison of KE dissipation from our numerical simulations and observational estimates from the Arctic – please see the text and new Figure 12.

4. (Now on L498-501, L582-592, L593-602) We also appreciate the reviewer's suggestion to include some speculation regarding climate change in the Arctic. Specifically, we added three points pertinent to this discussion.

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.

Addressing each of the reviewer's main points here individually:

1. Yes, the reviewer is correct that our choice of five nondimensional variables was not ideal. Clustering algorithms, such as GMM, are less sensitive to having co-linear input variables in comparison to regression algorithms. However, it is certainly not an ideal set-up, so we have now re-done with analysis with only four nondimensional variables omitting the parameter J. We also changed the Richardson number parameter to be Froude number ($Fr = 1/\sqrt{Ri}$) as suggested by the reviewer. This way, we can focus better on the large Fr (small Ri) parameter regime. Overall, the distribution of the nondimensional numbers within each cluster does not change significantly and does not change our overall analysis as shown in the Revised Figures 5 and 6. However, we agree that this is a better way to represent the problem in terms of the nondimensional parameters, so we now have redone all of the analysis and discussion in terms of the new clusters. The mean values of the nondimensional parameters for each cluster only change by a few percent, so it would not impact the main conclusions from the numerical simulations. However, for completeness, we will re-run the numerical simulations with new values for the revised manuscript.

2. (Now on L636-644) We agree with the reviewer that one of the important physics omitted in our simulations is the momentum transfer from the atmosphere (wind) to sea ice. Since our objective is to determine the internal wave drag, we can assume that the atmosphere has already transferred momentum to the sea ice to make it move at a certain velocity. However, it is important that these momentum transfer terms are

considered holistically, so we added the following statement to our discussion. Unfortunately, estimating the Nansen number from the Flocco simulation would be just in terms of model-parameterized drag coefficients, which would introduce their own biases. Therefore, in order to avoid any uncertainty and confusion, we do not add this analysis to the current study, but do cite Heorton et al (2019), who provided estimates of the Nansen number for the Arctic.

3. (Now on L291-294) As pointed out by the Reviewer 2, the thickness of the pycnocline can be another parameter to vary. Here, we have taken it to be a fixed small finite value to simulate a sharp pycnocline (yet finite for the stability of numerical simulations). However, this can be another parameter to adjust, which would yield a fifth nondimensional parameter for the problem. We added a statement to reflect this in Section 3.1 (Numerical Simulation Set-up):

4. (Now on L657-674, Figures 3, 4, SI3) With regard to the number of clusters, when applying GMM, having too few clusters would typically lead to more muddled results. So, for example, given our discussion and the reviewer's suggestions, it would be perhaps ok to have 3 main clusters (keeping the former cluster names: Cluster 0 with small keels, large depth, Cluster 2 with intermediate \eta and Ri that sustains IW propagation but not much turbulence within the mixed layer, and Clusters 3-5 with shallower ML/deeper keels and more turbulence). However, giving GMM only 3 clusters would not yield these clean results. This is why when performing GMM clustering, one produces the BIC score curve (Fig. 3) that balances having too many clusters (overfitting) and having enough clusters so that certain characteristics are captured. Based on our BIC score, we would argue that even 6 clusters is too few from the statistical point of view, but we chose this number to balance the fit of the GMM clustering and interpretability (i.e., not having too many clusters). In practice, once the GMM or other clustering is performed, clusters are often combined in the discussion (e.g., Sonnewald et al, 2019) as we have done here. So, the clustering with many clusters is the first step in letting the machine learning identify statistical patterns that are then interpreted by a researcher into a more coherent narrative. To that end, we agree with the reviewer that it is important to note that maybe there are fewer than 6 main "categories" of Arctic sea-ice behaviour with respect to internal wave generation, and we now explicitly discuss that (in addition to the peculiarities of GMM clustering) in the "Limitations" subsection of "Discussion". Please refer to the text below and Revised Figures 4 and 5 and the Supplementary Figure 3.

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.

We have re-written the abstract such that this sentence is no longer present.

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

We have re-written the abstract such that this sentence is no longer present, but we keep this in mind to avoid this confusing terminology throughout the paper.

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

We have re-written the abstract such that this sentence is no longer present, but we keep this in mind to avoid this confusing terminology throughout the paper.

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.
Everything is now in terms of Froude number Fr, as suggested by the reviewer.

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).
While we overall agree with the reviewer, the abstract is too short for comparison/caveats. We rephrased this sentence to better reflect the aim of our paper, but we leave the comparisons to the "Discussion" section. We also would like to emphasize again that the aim is to improve the parameterizations of the internal wave drag on ice keels in climate models. Therefore, the relevant values here are the values that the climate models see, which may not agree with the observation values. Therefore, in the absence of pan-Arctic observational datasets, we have to rely on the climate model output as our input. Comparison with actual observations would be to validate the climate model, not our results, as our goal is to identify what ranges of values need to be examined using high-resolution numerical simulations to improve climate model performance. Therefore, we add in the Discussion section specifically some of the climate model caveats, but detailed comparisons with observations for the input parameters (e.g., keel sizes, stratification) are beyond the scope of this study.

In the "Introduction", we added the following statement (L13-14)

"These results provide a framework for constraining physically relevant parameter regimes for ice keel drag and inform future model development and observational studies of ice–ocean coupling."

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.
Amended to "several meters" instead of "tens of meters" for L16.
In L90, we amended it to "typically at most tens of meters" (the main point here to emphasize that they are much smaller than 10km horizontal resolution of climate models).

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.
We have now taken out this portion and added more information about internal wave radiation both beneath the ice keels and for the classical lee wave theory. Please see our response to Major Comment B.

L31 ”IW […] enhance momentum transfer” — transfer of momentum between what? What exactly is enhanced, compared to what?
We now specified this as "transferring additional momentum from the moving ice keels into the ocean" (Now on L34)

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.
This reference has been added (now on L36-37): please see our response to Major Comment B.

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.

1. Please see our response to Major Comments B: we now have a more thorough discussion of the IW generation at the seafloor (now on L41-64)

2. Viewing the problem in the frame of reference of the ice keel, the major difference between the bottom topography and ice keel modelling lies in the treatment of stratification (linear stratification vs. presence of mixed layer/pycnocline). This leads to two additional nondimensional numbers, which was hinted in the Introduction, but is now explicitly stated (now on L65-68).

3. We also added the discussion into the "Limitations" subsection about (1) transfer of momentum from the atmosphere to sea ice that is unique to the ice keel problem (now on L636-644) and (2) importance of rotation and near-inertial waves, which may be relatively more important for the seafloor problem as the time scales are longer in comparison to the ice keel problem (now on L653-656).

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.
We have now addressed this: please see our response to Major Comment B (now on L106-112).

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

- We believe that this is precisely part of the nonlinear effect of the size of the ice keels and flow characteristics on the resulting heat and momentum fluxes. That is, depending on the keel and flow characteristics, the overall effects on the sea ice could be different. For instance, in Flocco et al, sea ice thickness is decreased in certain parts of the Arctic and during certain seasons by including IW drag into the model. To clarify we re-wrote this part on L73-76.

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

We believe that from the word description here it is clear the internal wave phase speed is

$$\sqrt{z_0 \Delta b}$$

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

We now added clarified this as "... subcritical (Fr_z < 1, relatively slow keel or relatively strong stratification) to supercritical (Fr_z > 1, relatively fast keel or relatively weak stratification) regimes..." on L82-83.

L51 please explain "secondary internal waves"

This is an important point for that paper to separate the forcing internal solitary wave from the smaller-scale (secondary) internal waves, but it is not an important point for this paper, so we have taken this mention out of our text.

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

We have taken the "careful" out of text as it was not necessary for our message. But ice keels, being smaller than climate model resolution, need parameterization of any of its influences on the coupling between ice and the flow, so it is difficult to be specific here. Therefore, we rephrased it more generically as on L91-92:

"...parameterization of the ice keel's effects on the coupling between the sea ice and the underlying ocean flow, e.g., through form and IW drag."

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 u0, N0 and the like) to actual observations (referring to major comment C again).

- To clarify (as stated above), the goal of this study is to identify the ranges of parameter values to be used in high-resolution numerical simulations that can be used to improve parameterizations of ice-ocean coupling variables (e.g., IW drag) used in climate models. Therefore, the goal is *not* to compare the observations for these parameters to the climate model parameters, but rather to identify the ranges of parameters that climate models see to focus the high-resolution numerical study efforts. Therefore, we refrain from comparing the dimensional parameter ranges to the observations extensively, as this is beyond the scope of this study and is better left for actual climate model studies to conduct more carefully.

- To address the reviewer's other comment, we clarify this through rephrasing on L102-105:

"Therefore, a key goal of this study is to determine the nondimensional parameters that are relevant to the parameterization of IW drag and identify the ranges of values of these parameters pertinent to climate model simulations in order to efficiently constrain this space, guiding targeted numerical simulations and laboratory experiments required to improve existing parameterizations."

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

- We now added the following details about GMM on L115-119.

L89 "greater IW generation" — What precisely is meant here? An energy flux from the mean current into the IW energy spectrum? Please be precise.
We rephrased this is as "internal wave kinetic energy" to be more precise (now on L138)

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?

- We now provide additional details for the model configuration and additional references on L207-218.

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.

- We added a statement to the "Data and Methodology" section pointing to the "Limitations" subsection where we discuss some of the potential shortcomings of the model.
- Specifically, in the "Limitations" section, we discuss:
    - The pycnocline depth being 10m at the shallowest in the model (please see our response to Major Comment B in regards to shallow pycnoclines)
    - Ice drift speed being potentially overestimated by the Flocco model in some key clusters (e.g., W3): please see our response to Major Comment C
    - Keel depths in the Flocco model being reasonably within the distributions from observational measurements: please see our response to Major Comment C and Supplementary Figure 2.

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.

- This is a common terminology in the seafloor topography literature, so we would like to keep it the same, though we understand the semantic objection of the reviewer. We also add the point about lack of feedback from the ocean back to sea ice in terms of the turbulent heat flux and heat transfer into our "Limitations" section on L631-635:

"Because the shape of the keel in our simulations and the conceptual model is not allowed to change, we neglect the effect of turbulent heat fluxes that can melt the sea ice. This process can be complicated. For example, Skyllingstad et al. (2003) found that while ice keels enhance turbulence, their effects on melting can depend on blocking of the flow and trapping of fresh water. This can alter the shape of the ice keel, though we do not expect it to take effect on the short timescales of our simulations."

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)

- (Now on L150-151, L276-282) From the analytical solution point of view, it is easier to represent the topography as infinitely many sinusoidal bumps in order to avoid specifying horizontal boundary conditions; however, in the analytical solution, the neighboring bumps do not necessarily impact the downstream and upstream flow. In that sense, this is comparable to numerical modelling of an isolated bump in a simulation with horizontally periodic boundary conditions (again to avoid specifying the horizontal boundary conditions). As this is a relatively common technique in modelling, we do not think that this discussion adds value to the paper. However, we did include some of this information in text discussing the McPhee and Kantha parameterization (Sec. 2.1, L150-151) and discussing the numerical simulation set up (Sec. 4.1, L276-282).
- (Now on L151-152, 520-531) The reviewer is correct that McPhee and Kantha analytical model considered small keel depths. This is an important point because a recent study that assessed parameterizations of form drag for flow-topography interactions (Johnston et al, 2026) found that 2D parameterizations (like McPhee and Kantha) do not perform well when compared to numerical simulations for large topographic heights (or large J, i.e., J>1). We now added it to
    - the introduction of the parameterization in Sec. 2.1 (L151-152)
    - the "Implications" subsection (Sec. 6.1), as this strengthens the need for revisiting the McPhee and Kantha parameterization (L520-531)

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 z0 = 0 includes this case in parts, but the fixed pycnocline depth and the constant N0 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.
We now address this in the "Limitations" subsection: please see our response to Major Comment B.

L122, L138 See major comment E
We now have only 4 nondimensional parameters (and replaced Ri with Fr = 1/sqrt(Ri): please see our response to 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.

- While this is an interesting regime, this is beyond the scope of this study and the McPhee and Kantha parameterization of the IW drag. We included a sentence about this regime in the introduction and believe that it is sufficient for this study on L51-52.

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. "

- We amended the text accordingly on L189-191.

L145 The choice for the variable symbol for the nondimensional parameter Δb/(k0u20) 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 Rig = N 2/S2? Furthermore, under which conditions is Fr=1√Ri – is this always valid?

Thank you for pointing this out - we did not explicitly mention that this is indeed the Richardson number.

The gradient Richardson number is a measure of instability (or the ability of the flow to overcome stratification) that is based on the shear of the flow and is often calculated based on the local estimates of the velocity gradients. For the Richardson number representation here, we do not consider gradients, but rather it is defined based on the flow velocity.

The connection with the Froude number is valid when one considers the bulk Froude number of the flow (e.g., Baines 1982, De Abreau et al, 2024) rather than the local Froude number. We followed the nomenclature from McPhee and Kantha (1989) and Flocco et al (2024) to define this as a Richardson number.

However, after consideration and reviewer's other comments regarding the importance of the value of 1/Ri rather than Ri, we will restructure the manuscript in terms of the bulk Froude number Fr (as now defined in Eqns. 9).

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?
In the annually-averaged data, we excluded about 32% of data points. For the summer and winter months, we excluded 17% and 38%, respectively. We now added this to the text on L228-231. In this regime, the IW drag would be absent, so the primary drag on the sea ice would be from the form and skin drag. However, as the reviewer pointed out and we note in the introduction, local dissipation of evanescent IWs could be 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).

- Dimensional parameters are now plotted in Fig. SI1. We also now made subplots in Figure 2 for the spatial distribution of the nondimensional parameters broken down by the cluster for each of the (left) annual, (middle) summer, and (summer) winter clusters.
- We also added the variable descriptor for all figures.

Numerical simulations

L186 Is $\rho 0$ constant in time, depth, or both?
Constant in temporally and spatially, consistent with the Boussinesq approximation for Navier-Stokes equations (which are valid in the ocean).

L187 Could you please elaborate briefly what the term f u0 achieves effectively?
This represents the body force, i.e., steady forcing from the velocity u_0. As we are modelling the sea ice interaction with the underlying ocean in the frame of reference of the sea ice, u_0 is the relative velocity of the flow from the point of view of the sea ice (that is stationary in our representation). The "f u_0" term captures the force from the flow onto the sea ice (this is the only terms that remains if we consider perturbations about the mean state, u_0). More details are in the referenced works: Klymak et al (2018) and Zemskova and Grisouard (2021).

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.)
No skin drag coefficient is applied as we imposed a free-slip boundary condition. This is common numerical simulations but we will add the implications of this choice that this means that the skin drag coefficient is taken to be zero.

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.
Please see our response to Major Comment B. We also now added two numerical simulations (S0 and S2) in which \eta < 1 (keel depth is greater than the pycnocline

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?

- Per suggestion by Reviewer 2 that the table was too busy and that Figure 6 (boxplot distributions) was already sufficient, we have now streamlined Table 1 to show only the values pertinent to the numerical simulations rather than showing the mean and standard deviation values.
- Yes, the quantitative results would be naturally affected whether mean or median values are used in the numerical simulations. However, the numerical simulations are used for illustrative/qualitative purposes, and do not encapsulate the entire range of dynamics within each cluster. That is the reason for a follow-up numerical study with a large parameter sweep.

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 u0 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.

- Yes, the reviewer is correct and this is an unfortunate (and imprecise) nomenclature that ocean energy budget studies use. A better way would be to refer to this as the “fluctuating” component as it is the fluctuating deviation from the mean (though so many studies just refer to it as the mean-turbulent decomposition). We have rephrased it everywhere to refer to it as the “fluctuating” KE component (while this is a better terminology, it has, unfortunately, not been as much mainstream).

- The simulations results are here for illustrative purposes, and it would take a substantial amount of space to accurately explain the methodology for calculating different energy budget terms carefully. However, in the subsequent numerical study specifically focused on specifically the energy budget, we will quantify the IW and turbulent kinetic energy reservoirs and their dissipation and exchange rates more carefully.

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?

Yes, that is correct. No mixing parameterizations and turbulence closures allows one to calculate the diabatic buoyancy fluxes directly from the simulation output variables.

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 cluster1, 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.
We thank the reviewer for this (and many specific suggestions) and will restructure the manuscript accordingly.

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.

The reviewer is picking up on a few features of GMM clustering algorithm that we indeed glossed over somewhat and that are perhaps worth discussing in the manuscript as well.

- Addressing the comment regarding old Cluster 1 (now Cluster A4): the way GMM clusters data is by calculating the probability of each data point (in this case, every grid point) belonging to one of the 0-N clusters (N is the number of clusters that we pick, in this case N=6). Then, it assigns the data point to the cluster with maximum probability. However, because it *has to* classify every data point, some of the data points fall in-between multiple clusters. For geophysical and environmental data, this often happens along the spatial edges between clusters. This is largely what is happening to (the former) Cluster 1 – you can think of it as the “left-over” cluster for data points that are in-between several clusters. This is in part why the standard deviation for many of the nondimensional parameters is actually quite large for (former) Cluster 1 (apparent from the large circles for former Cluster 1 and new Cluster A4 in the left column of Fig. 9). The way to minimize this kind of “leftover” is actually to increase the number of clusters N so that data points have more categories to fit in, but that decreases the interpretability of the clustering, as the reviewer suggested.

- Addressing the comment regarding fewer clusters: please see our response (point #4) to reviewer's Major Comment D

Figure 6 The authors could consider showing Ri−1 instead, since – if Ri is to be interpreted as Richardson 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.
We have addressed this comment by restructuring the narrative to now be in terms of the bulk Froude number Fr = 1/sqrt(Ri).

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.
Yes, the reviewer is absolutely right, and we will remove any references to the NIW and lee wave interactions (as suggested, the simulation time is too short for us to observe these effects).

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.
We appreciate this comment and we think this would be an interesting point to add to our future study, in which we will perform a thorough parameter sweep and investigate the numerical simulations and the energetics in more detail.

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 CIW can evidently also do with four nondim. parameters.
We have addressed this comment by now re-doing the clustering with only four nondimensional parameters. However, we still choose to show the values of J in other figures, as it is often an important parameter set in numerical studies.

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.
Please refer to our response to Major Comment C, where we compare certain observation values to the Flocco model output values.

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.
To address this comment, we will move the referenced part into the “Internal wave drag parameterization and nondimensional parameters” section (Section 2), where we discuss the McPhee and Kantha parameterization (Sec. 2.1) and nondimensional parameters (Sec. 2.2). With regards to the derivation of the equation, Eqns 10-11 (former Eqn. 21) itself follows from plugging in nondimensional variables for the dimensional counterparts (Eqns. 2-3 in Flocco et al, 2024). We now provide these dimensional equations first (Eqns. 5-6) so that one can derive the nondimensional versions if necessary. However, for the derivation of the actual \Gamma from the physics principles, we would refer the reader to the original study (McPhee and Kantha, 1989) as it is too lengthy to summarize in this study. We choose to just use their end derivation result though we do discuss their set-up and present a brief background for their analytical derivations in Sec. 2.1.

L393 please reference more than one ice-ocean model here
Flocco et al (2024) was the first study to include ice–ocean IW drag into a climate model set up. Therefore, we cannot reference other ice-ocean models here, but we will remove “e.g.” as that is misleading (now on L173).

L398, Fig 10, Fig 11 The values for CIW 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 CIW into perspective.
We now have incorporated this into our Revised Figure 11 and added discussion to the “Implications” section. Please see our response to Major Comment C.

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.
We have now added these figures.

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?

- We appreciate this suggestion by the reviewer, so we added the following statement to our conclusion on L696-701:

“Numerical simulations are a powerful tool to study a particular phenomenon and perform controlled parameter sweeps. However, in order to groundtruth the parameterizations derived from numerical simulations, observational measurements are necessary. In particular, we would need simultaneous measurements for the values of the input dimensional variables (u0, k0, h0, z0, Δb, N0) and the values that we would like to parameterize, e.g., IW drag and KE dissipation. As we find significant spatial and seasonal variability, especially differences between perennial and seasonal ice and pack and free-drift ice, long-term observational measurements in different parts of the Arctic would be helpful to validate the parameterizations.”

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.
Yes – however, the key is that our results point to there being substantial spatially coherent patterns in the input dimensional parameters AND the nondimensional parameters that we construct from these input dimensional parameters. This could be necessarily true, especially when considering nondimensional parameters that could introduce more spatial spottiness.

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.
Please see our response to Major Comment C

3 Minor technical comments

We have addressed these comments in the revised manuscript accordingly
L12 Ri should not be italic
Changed
L235 and later many style guides recommend setting subscripts that are words or abbreviations in non-italic font, i.e. Apyc instead of Apyc, which would also apply for ”above”/”below” etc
Changed
Table 2 the units should be formatted like mx s−y
This has now been adjusted
Figure 3 It would help the non-expert readers if BIC was written out once in the caption or figure title
It has now been added to the figure caption.
L287 typo? ”keel”/”keep”
Changed
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?
The label has been adjusted. As the purpose of this study is to focus on the internal wave generation below the pycnocline, we would like to keep the figures as they are. The numerical simulations are for illustrative purposes only, so we would like to now include too many relatively redundant figures.
Figure 9 Please make the labels larger. Again, the authors may consider to plot inverse Ri instead.
This has been adjusted, and all of our results are now in terms of Fr number.

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.
We sincerely thank the reviewer for this thorough list of suggested studies. We have now incorporated them into our “Introduction” and “Discussion” sections.

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 C0 D = 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 CIW (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 aspect 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.

References
Anhaus, P., Katlein, C., Arndt, S., Krampe, D., Lange, B. A., Matero, I., Salganik, E., & Nicolaus, M. (2025). Under-ice environment observations from a remotely operated vehicle during the MOSAiC expedition. Scientific Data, 12(1), 944. https://doi.org/10.1038/s41597-025-05223-1

Brenner, S., Rainville, L., Thomson, J., Cole, S., & Lee, C. (2021). Comparing Observations and Parameterizations of Ice-Ocean Drag

Through an Annual Cycle Across the Beaufort Sea. Journal of Geophysical Research: Oceans, 126(4), e2020JC016977. https://doi.
org/10.1029/2020JC016977
Cole, S. T., Timmermans, M.-L., Toole, J. M., Krishfield, R. A., & Thwaites, F. T. (2014). Ekman Veering, Internal Waves, and Turbulence
Observed under Arctic Sea Ice. Journal of Physical Oceanography, 44(5), 1306–1328. https://doi.org/10.1175/JPO-D-12-0191.1
Cole, S. T., Toole, J. M., Lele, R., Timmermans, M.-L., Gallaher, S. G., Stanton, T. P., Shaw, W. J., Hwang, B., Maksym, T., Wilkinson,
J. P., Ortiz, M., Graber, H., Rainville, L., Petty, A. A., Farrell, S. L., Richter-Menge, J. A., & Haas, C. (2017). Ice and Ocean Velocity
in the Arctic Marginal Ice Zone: Ice Roughness and Momentum Transfer. Elementa: Science of the Anthropocene, 5, 55. https:
//doi.org/10.1525/elementa.241
Cole, S. T., Toole, J. M., Rainville, L., & Lee, C. M. (2018). Internal Waves in the Arctic: Influence of Ice Concentration, Ice Roughness,
and Surface Layer Stratification. Journal of Geophysical Research: Oceans, 123(8), 5571–5586. https://doi.org/10.1029/2018JC014096
Fer, I., Baumann, T. M., Koenig, Z., Muilwijk, M., & Tippenhauer, S. (2022). Upper-Ocean Turbulence Structure and Ocean-Ice Drag
Coefficient Estimates Using an Ascending Microstructure Profiler During the MOSAiC Drift. Journal of Geophysical Research:
Oceans, 127(9). https://doi.org/10.1029/2022JC018751
Fine, E. C. & Cole, S. T. (2022). Decadal Observations of Internal Wave Energy, Shear, and Mixing in the Western Arctic Ocean. Journal
of Geophysical Research: Oceans, 127(5), e2021JC018056. https://doi.org/10.1029/2021JC018056
Kawaguchi, Y., Hoppmann, M., Shirasawa, K., Rabe, B., & Kuznetsov, I. (2024). Dependency of the Drag Coefficient on Boundary
Layer Stability beneath Drifting Sea Ice in the Central Arctic Ocean. Scientific Reports, 14(1), 15446. https://doi.org/10.1038/
s41598-024-66124-8
Krumpen, T., von Albedyll, L., Bünger, H. J., Castellani, G., Hartmann, J., Helm, V., Hendricks, S., Hutter, N., Landy, J. C., Lisovski,
S., Lüpkes, C., Rohde, J., Suhrhoff, M., & Haas, C. (2025). Smoother sea ice with fewer pressure ridges in a more dynamic Arctic.
Nature Climate Change, 1–7. https://doi.org/10.1038/s41558-024-02199-5
Lu, P., Li, Z., Cheng, B., & Leppäranta, M. (2011). A parameterization of the ice-ocean drag coefficient. Journal of Geophysical Research:
Oceans, 116(C7). https://doi.org/10.1029/2010JC006878
Martin, T., Tsamados, M., Schroeder, D., & Feltham, D. L. (2016). The Impact of Variable Sea Ice Roughness on Changes in Arctic Ocean
Surface Stress: A Model Study. Journal of Geophysical Research: Oceans, 121(3), 1931–1952. https://doi.org/10.1002/2015JC011186
Randelhoff, A., Fer, I., & Sundfjord, A. (2017). Turbulent Upper-Ocean Mixing Affected by Meltwater Layers during Arctic Summer.

Journal of Physical Oceanography, 47(4), 835–853. https://doi.org/10.1175/JPO-D-16-0200.1
Randelhoff, A., Sundfjord, A., & Renner, A. H. H. (2014). Effects of a Shallow Pycnocline and Surface Meltwater on Sea Ice–Ocean
Drag and Turbulent Heat Flux. Journal of Physical Oceanography, 44(8), 2176–2190. https://doi.org/10.1175/JPO-D-13-0231.1
Reifenberg, S. F., Fer, I., Kanzow, T., von Appen, W.-J., Hoppmann, M., Krumpen, T., Neudert, M., Preußer, A., & Haas, C. (2025).
Turbulence Observations below Drifting Sea Ice: TKE Production and Dissipation in the Meltwater-Influenced Boundary Layer.
Journal of Physical Oceanography, 55(4), 451 – 470. https://doi.org/10.1175/JPO-D-24-0102.1
Shirasawa, K. & Ingram, R. (1991). Characteristics of the turbulent oceanic boundary layer under sea ice. Part 1: A review of the
ice-ocean boundary layer. Journal of Marine Systems, 2(1-2), 153–160. https://doi.org/10.1016/0924-7963(91)90021-L

General comments
This manuscript is concerned with ice-ocean drag and ocean dissipation that results from moving sea ice keels, a topic worthy of study. I find that the main novel conclusions are quantification of the parameter space that exists in the Arctic. The behavior of lee waves in various parameter regimes appears to be known from previous work (but correct me if I am wrong on this point). This conclusion regarding what parameter space is applicable for the Arctic could be better supported in a number of ways, including providing details of how the NEMO model is validated against the parameters of interest (keel geometry, mixed layer depth, and ice-ocean shear in particular). This conclusion could also be expanded to more robustly consider how that parameter space is related to ice conditions, ocean conditions, or both ice and ocean conditions (see comments below).
It is not particularly clear what new conclusions result from other portions of the analysis. 1) The paper claims to conclude that existing parameterizations are not sufficient and should be re-examined. I do not find this to be a robust conclusion, it is more of a suggestion or possibility to consider. 2) This paper additionally uses 2D numerical simulations, and it is not clear what new conclusions are derived from these simulations. They are useful illustrative examples, and can be retained in the paper, but should be presented as illustrative examples instead of new conclusions.
Overall, I think the novel and most useful aspects of this paper are the quantification of the five parameters for the Arctic environment. I think this paper could be shortened significantly to focus on this narrower set of novel conclusions.

We thank the Reviewer for their comments. We added the specific points and clarifications as requested by the reviewer, which are denoted as *italicized* text in our response below.

We appreciate that Reviewer's suggestion to shorten the paper – however, we received many suggestions to add new information to the paper by Reviewer 1. Striking some balance, we streamlined some portions of the text (e.g., Table 1, removing metrics for changes in buoyancy frequency N for the numerical simulations that were not very informative, and shortening the discussion of the numerical simulations in Results section 5.1).

Based on the suggestions from both reviewers, we implemented the following major changes:

1. We have re-done our clustering analysis with four nondimensional variables (lee wave radiation parameter \chi, keel criticality J, depth ratio \eta, and Froude number Fr) and numbered the clusters in the order of decreasing area that they occupy. Therefore, the cluster numbers are different from the original draft.
2. We added seasonal panels to all figures that previously only showed distributions for annual means (Figs. 2, 3, 4, 5, 6, 9, 10).
3. We conducted three additional numerical simulations to investigate regimes where the keel depth is larger than or comparable to the pycnocline depth (e.g., Fig. 11).
4. We restructured the manuscript making a separate section describing the IW drag parameterization and the nondimensionalization (Sec. 2).
5. We restructured the results section breaking it into the discussion of the annual clusters and differences in flow characteristics depending on the nondimensional parameters from numerical simulations (Sec. 5.1) and seasonal effects and IW drag variability (Sec. 5.2).

6. We restructured the manuscript, shortening the results and conclusions sections, to add a new Discussion section with two subsections: Implications (Sec. 6.1) and Limitations (Sec. 6.2) of the study.
7. We have re-written the abstract to avoid too much specific jargon (e.g., symbology for the nondimensional numbers) and specific values.
8. We added Supplementary Information figures: (1) Fig SI1 of spatial distributions of the dimensional parameters, (2) Fig. SI2 of comparison of distributions of keel depths from the model data used in this study and observations, and (3) Fig. SI3 of comparison of spatial distributions of GMM clusters for a different number of K (number of clusters).

Specific responses to the Reviewer's comments are below and we note the line numbers where we made the corresponding changes in text.

Specific comments
It is difficult to follow this paper because of its use of variables, e.g, chi, J, eta, instead of their more dynamical names / descriptions, along with the use of cluster numbers, instead of more physical descriptions of these regions. For example, cluster 3 could be referred to as Cluster 3 (central arctic) or Cluster 3 (perennial sea ice zone) as the authors see fit. Lines 125-130: parameters like chi and J that have names should have their names stated here as well as in the abstract. In general, use of these names, e.g., internal wave nonlinearity, in addition to their symbols throughout the text would make this paper easier to read and understand.

While we appreciate the reviewer's comment regarding the variable names, stating the variable names every single time would be unnecessarily confusing (in terms of sentence structure). These nondimensional variables are fairly common in modelling studies (for both sea ice and bottom topography studies), so we would like to keep them in text as they are. However, acknowledging that this might be confusing to readers who are unfamiliar with such studies we will:

- add the variable names where the variables are defined in terms of the dimensional parameters (Eqns (1-2), (8-9)): "lee wave radiation parameter" (\chi), "keel criticality parameter" (J), "depth ratio" (\eta), "Froude number" (Fr)
- add the variable names to all figures (subplot titles or axes labels) as appropriate
- where the flow in text allows without making the sentence too cumbersome, specify the physical meaning of the variable in addition the variable symbol.

We appreciate the suggestion of naming the clusters, but we now present all of the clusters based on the annual, summer, and winter averaged data, yielding 18 clusters. Therefore, naming them would not be tenable and will become rather more confusing. The clusters are now all clearly plotted separately in the Revised Figure 5.

Lines 71-80 are not necessary. These are not examples of GMM relevant to this paper.

We would like to keep these as examples of previous studies that applied GMM clustering methodology to oceanographic datasets. However, we shortened this paragraph acknowledging that some of the details are superfluous. As suggested by Reviewer 1, we instead added some more details about the GMM algorithm in general (now L113-122).

Line 96-97: More details of the model validation and whether it is realistic or not need to be given in this paper. This study still relies on a model representation of keel characteristics (height and steepness). How do we know this is valid? Line 317-323: does the model accurately represent observed ocean stratification for the Arctic? Many arctic models fail to do so. Based on the information given in this paper, the claim that this model is a representative sample is not backed up. Figure 2: Can any of these parameters be compared to observational estimates? Is the geographic distribution of these parameters accurate?

We acknowledge that the input data in this study is taken from another model, which, as the reviewer has pointed out, may not be an accurate representation of the relevant Arctic dimensional parameters. However, the main objective of this study is to identify an approximate range of nondimensional parameters for the Arctic to use in future high-resolution numerical simulations. These high-resolution numerical simulations would then be used to determine how accurate the existing parameterizations (representation of unresolved climate model processes) are. One of the main reasons for this is that current high-resolution numerical studies choose the ranges of parameters somewhat haphazardly, and it is unclear what portion of the range of parameters (or dynamical regimes) has been studied.
To this end, we would argue that it is sufficient to examine the representation of ice keel characteristics from a commonly used regional/climate model that has a coupled sea-ice model. Ultimately, any new parameterizations would need to be implemented into such a climate model, so we need to see what ranges of sea ice characteristics the climate model sees (rather than what are true sea ice characteristics). So, we would want to know what high-resolution numerical simulations will need to be run to ensure that the parameterizations in climate models accurately capture the physics. These need to be based on the values of ice keel characteristics that the climate model sees, not necessarily based on the actual observed values. Therefore, this study is done in service of future studies to narrow down the parameter ranges of the nondimensional parameters to vary in future high-resolution simulations rather than pinpointing the precise distribution of sea-ice characteristics. Of course, it would be significantly better if this kind of analysis could be performed on observational data. However, there are no datasets with a full spatial coverage for all of the required variables that go into calculating the nondimensional parameters of interest.

The main point is how accurate is the actual internal wave drag (by considering small-scale physics in high-resolution numerical simulations) in comparison to the predicted drag *based on the parameter values that the climate model sees*. If we can improve this parameterization (the relationship between the nondimensional parameters and the drag), then we can make climate models more accurate and hopefully reflect the observations better.
We have made this point clearer in the abstract (L13-14), the introduction (L102-105), and the conclusion (L696-701). In addition, we discuss the potential limitations of the current parameterization from McPhee and Kantha in the new “Implications” subsection (Sec. 6.1) on L520-545).

With all of this mind, it is important to add some details about the previous studies that performed model validation for this particular model, as suggested by the reviewer.
We have added this to the "Limitations" subsection (Sec. 6.2) in the Discussion section:

b. (Now on L615-619) Shallow pycnocline in the summer which is not captured by the Flocco model as the shallowest values of the pycnocline they capture in the model is 10 m.

c. (Now on L604-614) Potentially faster ice drift speed predicted by the Flocco model in comparison to observations, which can lead to larger Froude number Fr and hence larger predicted IW drag

c. (Now on L614-624, Fig. SI2) We also compared the distributions of sea ice keel depths with some of the available measurements from literature. As shown in the Supplementary Figure 2, the distribution from the Flocco model output overall agrees with the distribution found by Metzger et al (2021), especially as their study does not separate keel depths smaller than 6m, so the results agree well if we consider the total "less than 9m" bin. Small keel depths (less than 3m) have also been observed by other studies (e.g., Brenner et al, 2021), so we believe that the distribution that we use in our study is reasonable.

Figure 2, line 230, and elsewhere: Three of these five parameters rely on u0. How is u0 estimated from the model? Is ocean velocity at a specific depth used, or is it averaged over a certain depth range? What drag coefficient formation is used in the model from which u0 derives? Is the drag coefficient constant or variable? Do the details of u0 varying with depth in the turbulent boundary layer and Ekman layer matter to this problem? How has u0 been validated? In general, I am also confused about when u0 is allowed to vary, and when a constant value of u0=0.1 m/s is used (2D simulations I think). Variability in u0 is potentially very important to this problem.

We thank the reviewer for pointing out some of the details that we omitted from the manuscript and will include in the revised one. Mainly:

- u0 is calculated as speed of the ocean current relative to the sea ice directly below the pycnocline (now on L227), and
- we use the outputs from the reference run from Flocco et al (2024), that is without parameterized IW drag included in the model simulations (now on L209-211).

To address the reviewer's other questions:

- It does not matter (for the definition of the nondimensional parameters and the derivation of the parameterization relationship) where, i.e., at which depth, u0 is measured, as long as it is defined and later applied consistently in the parameterizations.
- The specific accuracy of u0 is also not important as we are interested in the range of parameter values, not specific values at specific locations – see the detailed response to the previous comment.
- Yes, u0 does spatio-temporally vary (please see Figure SI1, for example). However, the variability of u0 is captured in the variability the nondimensional parameters. Since we have 6 dimensional parameters and 4 truly independent

nondimensional parameters, in order to vary the nondimensional parameters, we need to fix two of the dimensional parameters (we choose u0 and k0). That is the key advantage of collapsing dimensional parameters into fewer nondimensional ones.

Figure 2: How often do the parameters satisfy the lee wave radiation condition?

In the annually-averaged data, we excluded about 32% of data points. For the summer and winter months, we excluded 17% and 38%, respectively. We now added this to the text on L229-231.

Line 153-155: Is there seasonality in any of these parameters? Are these parameters only within the lee wave radiation range during winter? These are important details to convey. I realize that seasonality is addressed later in the paper, but it is important in this context.

Although it was perhaps unclear, our GMM clustering in the paper was done separately for annually-averaged and seasonally-averaged (over summer months and winter months, separately) data. This is now made more clear in text on L233-234 and L251, and we added summer and winter panels to all of our GMM analysis figures (Revised Figs. 2, 4, 5, 6, 9, 11). Specifically, Revised Figure 2 addresses this comment by the reviewer. We indeed find seasonal differences in the distribution of the nondimensional parameters. Please note (in addition to the previous response) that we find data points within lee wave radiation regime both during the winter- and the summer-averaged data (and in the annual average): Revised Fig. 2 (a-c).

Line 161-162: I'm confused about time averaging in the GMM setup. This sentence seems to state that data are NOT averaged ('at each timestep'), but also that time averages are used ('after fitlering and time-averaging').

We appreciate the confusion that this sentence introduced. GMM clustering is performed on the filtered and time-averaged data, so only on spatially-distributed (in lat-lon space) nondimensionally variables.

However, we perform three types of time-averaging: (a) annual average, so over all the data points after filtering, (b) over the summer months, and (c) over the winter months. For each of the time averaging interval, we compute separate time-averaged values for the nondimensional parameters, and then use them to perform the GMM clustering. So, the clusters for each of the three time-averaging periods are "independent" from each other in a sense that they do not know about the values of other time averages (of course, they are not truly independent because parts of the ocean might behave similarly despite the seasons).

We now have re-written text to make it more clear on L251-252.

How sensitive are the main conclusions of this paper to some of the choices and assumptions made? For example:

Figure 3: Do the conclusions of this paper change if only 5 clusters are used?

With regard to the number of clusters, when applying GMM, having too few clusters would typically lead to more muddled results. This is why when performing GMM clustering, one produces the BIC score curve (Fig. 3) that balances having too many clusters (overfitting) and having enough clusters so that certain characteristics are captured. Based on our BIC score, we would argue that even 6 clusters is too few from the statistical point of view, but we chose this number to balance the fit of the GMM clustering and interpretability (i.e., not having too many clusters). In practice, once the GMM or other clustering is performed, clusters are often combined in the discussion (e.g., Sonnewald et al, 2019) as we have done here. So, the clustering with many clusters is the first step in letting the machine learning identify statistical patterns that are then interpreted by a researcher into a more coherent narrative. Please refer to the text on L254-262 ("Methods") and L658-675 ("Limitations") and Revised Fig. 4 and Supplementary Fig. 3.

Line 197: A pycnocline width of 0.5 m is likely fine. But do the results depend on this parameter? A comment might help.

This width is chosen to have a thin but finite-width (for the stability of numerical simulations) pycnocline. In practice, the pycnocline width could be another dimensional parameter to consider (so there would be another nondimensional parameter). However, this would complicate the number of high-resolution numerical simulations to be conducted in the parameter sweep (having to vary another parameter). Therefore, since the pycnocline width does not appear in the IW drag parameterization, we choose to keep it constant. Changing its value would, again, affect the numerical simulations, and it might be interesting in the future to consider real stratification profiles from the Arctic to get better estimates of this value (though I am not sure if measurements would have sufficiently high vertical resolution).

To reflect this discussion, we have added a sentence on L252-255 below Eqn. (16) where pycnocline width is defined.

Line 211: If u0 were a factor of 2 larger or smaller, would this affect the conclusions?

Again, we are initializing the numerical simulations by choosing the nondimensional parameter values, not the dimensional parameter values. Based on the choice of the nondimensional parameter values and the values of the two fixed dimensional parameters (u0 and k0 in our case), the other dimensional parameters are set. If u0 was chosen to be larger or smaller, the other dimensional parameters would be adjusted accordingly based on the choice of the nondimensional parameter values to be within the same dynamical regime.

Figure 8: The winter surface layer depths in the Arctic shown here are approximately 50 m depth on average, and deeper for cluster 5. There can be a range of surface layer depths across the Arctic, with wintertime values of 40 m or even shallower. How much difference would a 10 m change in MLD make to diagnosed mixing, or any of the other conclusions?

That is precisely the objective of a follow-up study (currently underway), where we will vary the relevant parameters (based on the ranges of values that we identified in this paper plus some additional runs from observational measurements that do not overlap with these ranges) and diagnose mixing, dissipation, and wave drag.

I would interpret Figure 4-5 as having Cluster 4 representative of marginal ice zone / seasonal ice zone conditions. Is it the ice keel dimensions and not the ocean conditions that distinguish this cluster from 3 and 5? This is more relevant than listing the names of the seas that clusters occupy (line 267).

Our analysis in terms of the nondimensional numbers that combine both the ocean and ice characteristics does necessarily let us distinguish this. However, looking at Figure 4 in Flocco et al (2024), the area covered by (the former) Cluster 4 is characterized by relatively deeper mixed layer depth, relatively deeper keel depth, and relatively weaker density jump across the pycnocline. So, we would not say that it is more strongly driven by sea-ice dimensions as opposed to the ocean conditions. However, it does appear to be a transitional (perhaps marginal) zone.

We also now restructured this paragraph such that we present the spatial distribution of the annual clusters in the context of perennial and seasonal ice based on the recent study by Simon et al (2025) on L359-371 instead of listing specific geographical regions (e.g., names of the marginal seas).

This paper would benefit from some context about ocean mixing in the mixed layer, pycnocline region, and interior. Is diagnosed mixing weak, average, or strong? Are lee wave processes a dominant source of mixing? The diagnosed dissipation rate ranges from order 10^-15 to 10^-7, which is a wide range of values. The largest values for the ocean interior are 10^-10, which I believe is small to average compared with other studies (e.g., Fine and Cole, 2022, Decadal observations of internal wave energy, shear, and mixing in the Western Arctic Ocean, J. Geophys. Res., https://doi.org/10.1029/2021JC018056).

We now more included a more thorough comparison of the observed KE dissipation rates in the Arctic with the rates calculated from our numerical simulations in the new "Implications" subsection (Sec. 6.1) on L564-573.

Also in terms of context, I find it hard to visualize which regions have elevated mixing due to lee wave generation from the tables and discussion of the figures. Consider making a map showing pycnocline or interior mixing across the Arctic based on the clusters (obviously involving some assumptions). Line 446-455: The regurgitation in terms of relationships between different parameters is one view, but what is missing is the practical aspects. Overall, is lee wave drag or mixing significant for the Arctic? Where and when is it significant?

In order to make such maps, we would need to conduct a parameter sweep over the ranges of values of the nondimensional parameters with high-resolution numerical simulations, as proposed for a follow-up study. Otherwise, there are no ways to make good predictions for the mixing values generalized from this study, as no appropriate parameterizations for the mixing values exist. We, in fact, use this study to point to the need for such a follow-up study based on the parameter ranges and the numerical simulation set-up that we discuss here.

For the predicted lee wave drag, Flocco et al (2024) showed the values based on McPhee and Kantha (1989) parameterization. We invite the reviewer to see their figures 4 (b,c), 5 (b,c), and 6 (b,c) and we do not reproduce these figures as they are already published. However, the central premise here is that the existing parameterizations may not be correct because they are based on assumptions to derive them analytically (or do not exist as in the case for mixing), so they need to be investigated further, as we plan to do in the follow-up study. The limitations of the current parameterization for IW drag by McPhee and Kantha are now discussed on L521-546 in the new "Implications" subsection (Sec. 6.1).

We also have done a more thorough comparison of the predicted Ciw drag parameterization values with form and skin drag coefficients from observations. Please see our response to the reviewer's other comment below.

Line 422-429: These lines relate to one of the main conclusions of the paper, that the existing parameterizations are not quite correct. These lines do not give sufficient detail to support that conclusion. I do not think that the factor of 5 difference in internal wave drag at cluster 2 is particularly robust given the overlap in the 25-75th percentiles, and the general spread in the distributions of several orders of magnitude. The simulations that these are compared to also do not account for any variability in parameters, and are constructed with mean values. There might be a suggestion that the parameterization could be further investigated, but it is presented elsewhere as more of a conclusion.

We believe that this comment stems from some misunderstanding (and perhaps lack of clarity on our part) about our argument. Our point here is that comparing across clusters, there is a discrepancy between the relative values of C_IW predicted from parameterization (Eqn. 21) and the relative values of KE dissipation that we measure in the numerical simulations. That is, from the crosses in Fig. 11, the existing parameterization predicts 5-10 times greater internal wave drag for the former Cluster 2 mean parameters (the ones that we use in the numerical simulations) in comparison to the internal wave drag for the former Clusters 3-5 mean parameters. However, based on the results from the numerical simulations, Cluster 2 has 1-2 orders of magnitude smaller KE and KE dissipation rate compared to Clusters 3-5. If the IW drag is larger, one would expect larger KE and KE dissipation rates, but we find that predicted larger IW drag values (from existing parameterization) are associated with smaller KE/KE dissipation rates (from our numerical simulations). Hence, we suggest that the existing parameterization needs to be revisited.

We will amend our writing to reflect the explanation above and soften the language regarding revising the parameterizations. To that end, there are other reasons (e.g., parameterizations only consider 2D flows, whereas 3D turbulence is most likely important) that point to the need to re-examine this parameterization as is now discussed in our new "Implications" (Sec. 6.1) and "Limitations" (Sec. 6.2) subsections.

We also now include a more illustrative figure (please see our new Figure 12) to demonstrate the explanation above to be clearer. For example, on a log-log plot, Ciw parameterization reasonably captures the variability in KE and KE dissipation below the pycnocline (Fig. 12 (c, f)) except for certain cases (e.g., A4, S0). This is, of course, a log-log plot that hides some finer details and only captures the trends in orders of magnitude. Furthermore, the parameterization does not seem to capture the KE metrics in regions around the pycnocline and above the pycnocline (Fig. 12 (a-b, d-e)). This is perhaps expected, as the parameterization is focused on IWs below the pycnocline. However, it is still important to note as the keel movement can generate large KE dissipation in these regions. We added this discussion to text on L574-583 (Sec. 6.1).

We also now added two paragraphs (L521-546) discussing why the current IW parameterization might not be correct, in particular in the regions with large value of J or small \eta.

Line 422-429: Again, context is needed. Can these values of the internal wave drag coefficient be compared with previous studies? Typical ice-ocean drag coefficients are order 10^-3, which is not mentioned in this paper and should be. Compared to that value, in all but the most extreme examples, the drag from lee waves is negligible.

We now have redone our analysis to compare the parameterized (predicted) Ciw values with values of form and skin drag coefficients from observations. Please see our Revised Figure 10 and the added discussion on L503-520 (Sec. 6.2).

Line 440: Are these sea ice regimes, ocean regimes, or sea-ice and ocean regimes? I believe it is the latter. The results section seems to focus more on their interpretation as ocean regimes more than ice regimes. Additional interpretation in terms of perennial ice cover (cluster 3) vs. seasonal ice cover (cluster 4-5) is also needed. Can arguments be made about ice regimes? What does this imply about a future arctic environment with increasing seasonal sea ice?

Yes, it is, of course, the ice-ocean coupled regimes. We use the term "flow characteristics" rather loosely here, as we are interested in the flow processes (i.e., internal waves, turbulence), but the dynamics are influenced by both the characteristics of the sea ice and the underlying upper ocean. This is also reflected in our choice of dimensional parameters (both pertaining to the ocean and sea ice) to characterize this problem. The term "sea-ice regime" is also used rather loosely here as it is the sea ice movement that is driving the generation of IWs in the ocean interior. This portion is now taken out of the revised text.

We also thank the reviewer for the suggestion about the perennial vs. seasonal ice cover regimes. We now added the following to our text:

- Comparison of the cluster distributions with permanently ice covered regions and seasonally ice covered regions identified by Simon et al (2025) in Sec. 5.1 (L359-371)
- Discussion of successful GMM differentiation between the perennial and season ice cover regions in Sec. 6.1 (L495-502)
- Differentiating Ciw and dissipation rates in regions of perennial ice vs seasonal ice in Sec. 7 (L689-694).

Lines 446-455: Please be explicit about what new results are revealed by these 2D simulations. It is my understanding that none of the behavior in different parameter-space regions is novel, it is simply that the quantification of those parameter-spaces for the Arctic has not been previously investigated. The 2D simulations should be presented more explicitly as an illustrative example instead of as new conclusions. I will also make a second point that I do not think conclusions about mixing or kinetic energy values from these 2D simulations are particularly robust. Not enough of the variability in parameter-space has been included to draw robust conclusions. I view these 2D simulation primarily as useful illustrative examples.

We agree with the reviewer that the 2D simulations are used as illustrative examples. While that was our intention, we now emphasize it more clearly in the paper.

- Results (Sec. 5.1) L420-421
- Discussion (Sec. 6.1) L579-582
- Conclusions (Sec. 7) L696-699

However, one major point is that this numerical set-up for sea ice modelling in Oceananigans is novel because Oceananigans significantly increases the speed of numerical simulations through its GPU architecture. In this study, we develop a prototype for numerically studying this problem in a way that allows to conduct a parameter sweep within a reasonable timeframe. Therefore, using our set-up, it is possible to conduct a parameter sweep over a wide range of parameter values in a more time-efficient and systematic manner in comparison to the previous CPU-based numerical solvers. This is made explicit in text on L547-563.

Technical comments

Abstract: Too many variables for an abstract. Use of the more common Ri might be okay, but I suggest removing the symbols for others.
The abstract is now restructured such that we talk about the variables in terms of their physical meaning rather than symbols.

Lines 2-4: Run on sentence with two uses of 'because'
Thank you – we have re-written the abstract such that this sentence is no longer present.

Line 57: Be more specific than 'keel size'. Is this horizontal extent? Height?
We mean horizontal extent – it is now specified (now on L90).

Line 105: Horizontal spacing BETWEEN KEELS, L.
Now clarified.

Line 227: Equation 13 has w without a prime, u with a prime. Which is correct?
The prime represents that the background mean flow has been subtracted. The mean flow is only imposed in x-direction, hence u' represents the deviation from the mean flow. For w, there is no mean flow, so w (without the prime), represents the total deviation. This is a common notation in describing perturbations about mean state.

Line 287: 'small KEEP depth'
Changed.

Figure 2d: log10=0 corresponds to eta=1, which is an important value for this parameter. The colorbar should be changed to emphasize this range of values, and the variations between eta=10 and eta=100 are less important.
This has now been adjusted – please see the Revised Figure 2.

Figure 5: cluster 0 and cluster 5 are hard to distinguish, especially when printed out.
We now adjusted cluster colors to be more distinctive – please see the Revised Figure 4.

Tables 1-3: These are hard to understand as just numbers on a table. I suggest plotting the values as a graph where shading or symbols can also be used to show standard deviations. Table 2 may need to be a logarithmic scale, but I think it is still useful to consider presenting it this way. Figure 6 is much better than Table 1! I think table 1 could be removed, and mean values could be added to Figure 6.
We now have the values from Table 2 plotted in the new Figure 12. We also believe that having Table 1 in addition to Figure 6 is important to give the exact values (not just ones that are inferred from plots such as Fig. 6), especially for the reproducibility of numerical simulations in the future. Therefore, we chose to retain Table 1, but we have now streamlined it (e.g., taken out the standard deviation values) so that it only presents values needed to reproduce the numerical simulations.

Tables 2 and 3 should make it clear in their description that these results derive from the idealized 2D model.
Tables 2 and 3 already state that these are derived from numerical simulations.

Figure 9: Text is too small. The color-scheme should be improved for readability, especially for summer. Panel (o) is also difficult to determine which clusters are which. Different seasons do not need to have different color schemes.

- We have now adjusted the text size – please see our Revised Fig. 9.
- The overlapping of the clusters for certain variable combinations is inevitable. The main point of Figure 9 is to showcase the pairwise range of values for the nondimensional parameters and to demonstrate the parameter regimes that are important and not important to the Arctic. Instead, Figure 6 (boxplots) clearly shows the ranges of values for each cluster separately for comparison across the clusters.
- We choose to retain the different color schemes for different seasons (annual, summer, winter) because those clusters are derived from conducting the GMM clustering on the annual-, summer-, and winter-average data separately. Therefore, representing them with the same color might be confusing as it would imply that cluster 1 is the same region for annual, summer, and winter. However, that is in fact not the case – please refer to our Revised Figures 4 and 5 to see that these are spatially different clusters.

Figure 9 and 10: please adjust one figure so that the corresponding panels share the same axes. Example: Figure 9d uses a linear axis in eta and Figure 10d uses a logarithmic axes. Also consider adding dots for the mean values in each cluster and season to Figure 10.
We have now combined Figures 9 and 10 such that Figure 10 is now a fourth column. Please see our Revised Figure 9. All of the axes on the corresponding panels for each row are now the

same. We tried adding dots of the mean values to the fourth column (Ciw), but the plots became too busy. Therefore, we kept the revised configuration in hopes that one can compare across each row to interpret the parameter spaces.