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.

Furthermore, we have re-done our clustering analysis with four nondimensional variables and
numbered the clusters in the order of decreasing area that they occupy. Therefore, the cluster
numbers are different from the original draft. We ask the reviewer to reference the newly revised
figures at the end of our response.

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 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" and "Limitations". 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. 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.

"The total ice-ocean stress can be thought to comprise of three main components: (1) the skin drag, (2) form drag, (3) IW drag, and (4) internal ice-ice stress (McPhee and Kantha, 1989; Brenner et al., 2021). The internal ice-ice stress is typically only important for the consolidated pack ice. When sea ice is in free-drift, i.e., either the ice concentration is sufficiently low (e.g., typical for the marginal sea ice zone) or the ratio of ice speed to wind speed is sufficiently large, then the internal stress can be neglected (Brenner et al., 2021). Skin drag is due to the small-scale roughnesses of the sea ice, and form drag is due to ice keels presenting as discrete obstacles to the flow. Both of these processes play an important role within the turbulent ice-ocean boundary layer (Shirasawa and Ingram, 1991). The transfer of momentum from the moving sea ice to the ocean through these drag forces is typically represented using a quadratic drag law with parameterized drag coefficients. Skin and form drags have been subject to parameterizations in terms of ice keel geometry in many previous modeling (e.g., Lu et al., 2011; Tsamados et al., 2014; Wang et al., 2025) and observational studies (e.g., Cole et al., 2017; Brenner et al., 2021; Kawaguchi et al., 2024; Reifenberg et al., 2025). While there are still many open questions in our understanding of these stress terms, e.g., seasonal and spatial variability and mismatch between geometry-based drag parameterizations and direct measurements (Brenner et al.,2021), in this study, we specifically focus on the IW drag.
When drifting over a stratified ocean, keels act as moving topographic features that perturb density interfaces, generating IWs that radiate away from the ice–ocean boundary and enhance momentum transfer (Flocco et al., 2024). This downward momentum flux by the radiating IWs modifies the force balance on the ice, creating drag or resistance (McPhee and Kantha, 1989). Theory of IW generation by "towing" or moving an object, such as a ship or ice, along the surface of a stratified ocean dates back to the "dead water" effect (Ekman, 1904; Morison, 1986). Roughness on the underside of the sea ice, i.e., ice keels, further promotes IW generation. Reframing the problem to be in the frame of reference of the moving ice

keel (Rigby, 1974; McPhee and Kantha, 1989; Pite et al., 1995), this mechanism can be modeled akin to the flow-topography interaction theory."

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 and we added summer and winter panels to all of our GMM analysis figures (Revised Figures 2, 4, 5, 6, 9, 11). As the numerical simulations are for illustrative purposes only, we kept them to be only for the annually-averaged clusters.

3. Information on the seasonality of the Arctic stratification in the introduction: we included the following statement

"This problem needs to be studied taking into account seasonal variability of the governing parameters in the Arctic, especially the stratification. In the summer, the sea ice melt creates a shallow halocline separate from the permanent pycnocline, strengthening the upper ocean stratification (Brown et al., 2020). This process can lead even to the absence of the mixed layer near the sea ice keels (Randelhoff et al., 2017). In contrast, in the winter, brine released from the refreezing of sea ice promotes convection, deepening the mixed layer (Brenner et al., 2021). Given that IW generation is dependent upon ocean stratification, such seasonal variability likely plays a role in the IW drag coefficient values. Therefore, in this study, we perform our analysis both using the data averaged over the summer and winter months separately."

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:

*   *Distinct seasonal differences in the magnitude of lee wave radiation parameter χ (Fig. 2(a-c)) and Froude number Fr (Fig. 2(j-l)): both are larger during the winter. The parameterization predicts larger values of χ to translate to larger values of IW drag (see Fig. 9(d, h, l) and W3 in Fig. 6(c) and Fig. 11(c,f)). This is consistent with enhanced IW generation rate and dissipation rates in the case of larger χ in flow-topography simulations (Nikurashin et al., 2014).*

*   *The spatial distribution of the other two nondimensional parameters J and η do not show as much seasonality. However, the values of keel criticality J are generally larger in the summer than in the winter (Fig. 6(e-f)), possibly reflecting stronger stratification (larger N) in the summer. Larger values of keel criticality typically yield larger dissipation rates as we find from our numerical simulations and as predicted from previous numerical studies of flow-topography interactions (Nikurashin and Ferrari, 2010; Zemskova and Grisouard, 2021). However, this is not necessarily reflected in larger predicted CIW values during the summer (Fig. 11). However, as discussed below, the current parameterization for CIW is perhaps not accurate for large values of $J \gtrsim 1$.*

*   *We also find that GMM clustering generally performs well to separate the spatial patterns of perennial multi-year ice (A0, S0, and W0) and of seasonal first-year ice zones (A2−5, S1, S3−5, W2−5) identified based on maps from*

Serreze and Meier (2019). It also spatially separates the Eurasian (S2, W1) and the Amerasian (S0, W0) for both seasons. This agrees with the current understanding of the noticeable differences between the two basins, for example, in terms of the seasonal stratification (Brown et al., 2020).

5. 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. 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 "Limitations" subsections of the "Discussion".

"During the summer, the canonical mixed layer can be absent in in parts of the Arctic, such that even the near-surface ocean layers are stratified (Randelhoff et al., 2017). For the idealized numerical simulations conducted to assess the parameterization of CIW , the absence of the mixed layer does not pose a problem, as it would be just the limiting case of setting the nondimensional parameter η to zero (as the mixed layer depth z0 = 0). However, as noted in Flocco et al. (2024), the pycnocline or mixed layer depths shallower than 10 m cannot be accurately quantified in the NEMO model, hence, they are not present in our clustering results. This means that in addition to the ranges of values of η presented in this study, smaller values of η → 0 would have to be considered when conducting numerical simulations to evaluate the parameterization IW drag induced the ice keels to capture the full range of parameter variability. Based on our preliminary numerical simulations, we would expect that smaller η would enhance the IW generation and the IW drag as the ice keel would be in a more direct contact with the stratified layer, potentially without the buffer of the mixed layer. Therefore, omitting these smaller η values would lead to an underestimate of the effects of the ice keels on the ocean IWs. This could be one of the reasons that we find relatively small CIW values during the summer months (Fig. 11(b,e))."

6. We added the following more thorough description of the classical flow-topography interaction problem regarding lee wave generation into the "Introduction":

"In the classical flow-topography interaction problem, lee waves, which is the category of IWs of interest here, are generated when a steady flow has to go over a topographic obstacle, such as a seamount, in the presence of constant stratification (Bretherton, 1969, Bell Jr, 1975). Topography is typically represented as horizontally periodic sinusoidal bumps. The generated lee waves radiate upward away from the topography and the problem can be characterized in terms of two nondimensional parameters. In order for these waves to be freely propagating (i.e., have a real vertical wavenumber), if their intrinsic frequency (which is the product of the wavenumber of the topographic obstacle k0 and the flow speed u0) is within the IW frequency range between the local Coriolis frequency f and buoyancy frequency N0. This defines the first nondimensional parameter of the problem, the lee wave radiation parameter χ = u0k0/N0 , which needs to be within the [f/N0 , 1] range in order for lee waves to be freely propagating (Nikurashin and Ferrari, 2010) Outside of this range, lee waves are evanescent and their

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. *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. *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*
"Other differences in the values of the underlying dimensional parameters between the model output the real ocean can also affect the implications of our findings. For example, Flocco

et al. (2024) showed that their model in general overestimates the sea ice drift speed over most of the Arctic and across seasons in comparison to the observational data from the National Snow and Ice Data Center Polar Pathfinder dataset. This is consistent with climate models typically overestimating sea ice drift (Wang et al., 2023). From Flocco et al. (2024), the overestimation of the sea ice drift by the model is largest during the winter in the marginal sea ice areas. These regions are part of our clusters W2 and W3 (Fig. 4(i, l)) that have relatively large CIW predicted by the parameterization (Fig. 11(c,f)). For cluster W3, this is in part because of the larger Froude number Fr (Fig. 6(f)) that is proportional to the relative sea ice velocity u0. Therefore, an overestimate of the ice drift by the model could, in turn, overestimate the IW drag CIW.”

   c. 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.

"We also assess the distribution of ice keel depths h0 with observed values. SI Figure 2 shows the distribution of h0 from the overall Flocco et al. (2024) model output (not just our filtered data within the lee wave radiation regime) with the distribution from a large keel dataset by Metzger et al. (2021). The distributions are in overall good agreement, especially if we consider h0 < 9 m as one bin, considering that data for h0 < 6 m was not presented in Metzger et al. (2021). However, other observational studies (e.g., Cole et al., 2017; Brenner et al., 2021) have found such smaller keel depths (h0 < 6 m). These comparisons suggest that the distribution of the h0 values used in this study is in a reasonable agreement with the observations."

   2. 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 below and Revised Figure 11.

"We can estimate the relative importance of the IW drag by comparing the CIW values from parameterizations with skin Cs and ice-ocean drag coefficients Cio estimated from observations (Fig.11(a-c)). We take the skin drag coefficient value of $Cs = 7 \times 10^{-4}$ from the measurements under unridged summer ice by Reifenberg et al. (2025). We show the range of values for Cio estimated from various observational studies: $1.3 - 12.3 \times 10^{-3}$ (Beaufort Sea, annual cycle by Brenner et al., 2021), $4-6\times10^{-3}$ (Amudsen and Nansen Basins, summer by Fer et al., 2022), $1-10\times10^{-3}$ (Canada Basin, annual cycle by Cole et al., 2017), and $1 - 10 \times 10^{-3}$ (average value of 3.4×10^{-3}, Nansen Basin in July by Randelhoff et al., 2014). However, notably, Kawaguchi et al. (2024) measured that Cio can be as large as 0.13 at times. We also make comparisons to the canonical ice-ocean drag coefficient value of $CD = 5.5 \times 10^{-3}$, which is approximately in the middle the Cio range (Fig.11(d-f)). We find that the CIW values are typically smaller than the measured Cs and Cio values. In the regions of perennial sea ice (e.g., cluster A0, A1, S0, S2, W0), CIW is much smaller in comparison to the form and skin drag coefficients; we find it to be negligible in comparison to CD (Fig. 11(d-f)). However, in some marginal ice zones (e.g., A2, S4, W2), CIW can be $10 - 20\%$ of CD . The values of CIW are in general larger in the winter. In particular, the CIW values for cluster W3 (along Greenland

and in the Chukchi Sea) can be as large as or exceeding CD . So, even though the IW drag may be relatively not as important in the pack ice regions in the center Arctic, it could be important in the marginal zones, especially in the winter."

> 3. Comparison of KE dissipation from our numerical simulations and observational estimates from the Arctic – please see the text below and new Figure 12.

"Many observational studies estimate internal wave dissipation rates in the Arctic to be within the $10-10 - 10^{-9}$ $m^2 s^{-3}$ range (Scheifele et al., 2018; Kawaguchi et al., 2019; Fine and Cole, 2022; Fer et al., 2022). These values are smaller than observational measurements of up to 10^{-8} $m^2 s^{-3}$ in the upper parts in other global ocean regions (Waterhouse et al., 2014). In our idealized numerical simulations, we find the dissipation rates below the pycnocline to be generally on the order of 10^{-9} $m^2 s^{-3}$, which is within the range of observed values (Fig.12(b)). However, larger values of ϵ have been found close to the sea ice, in particular when the mixed layer is thin (Fer et al., 2022; Reifenberg et al., 2025). We also find larger values of kinetic energy dissipation within the pycnocline region and above the pycnocline in the case of numerical simulations with smaller η (shallower pycnocline) (Fig. 12(d)). These results suggest that depending on the sea ice and flow characteristics, there could be a spatio-temporal variability in the dissipation and subsequently diapycnal mixing rates in the Arctic. Of course, there are many real-ocean processes that are missing in our idealized numerical simulations, e.g., propagation of internal waves originated elsewhere and near-inertial waves that can also be generated by the wind stress at the ocean surface. Therefore, we caution against overinterpreting the numerical values of ϵ in our numerical simulations."

> 4. We also appreciate the reviewer's suggestion to include some speculation regarding climate change in the Arctic. Specifically, we added the following three points pertinent to this discussion:

"Measured from aircraft, above-sea surface ice ridges have significantly decreased in height and ridge density over the last 30 years. This loss is especially prominent in regions of the Arctic that are experiencing loss of multi-year ice like the Beaufort Sea and the Last Ice Area (Krumpen et al., 2025). Such smoother ice leads to reduction of atmospheric surface drag coefficient on ice ridges. While this is not direct evidence for changes in the under-sea surface keel height and density, it is plausible that there is some correlation between the above- and under-sea surface ice properties. This implies that understanding the role of ice keels on ocean mixing is important, as these effects could be reduced if the ice ridges become smoother. A large of these areas that Krumpen et al. (2025) found to have sea ice ridge smoothing are within Cluster A0 (and seasonally S0 and W0) in our dataset. This cluster is characterized by relatively large ζ and small η that enhance kinetic energy dissipation, thus, smoothing of the ice keels (a reduction in h0, so a reduction in ζ and η) can substantially change the ice-ocean dynamics in this region."

"Arctic stratification has changed in the last several decades. For example, through analysis of water column observations, Polyakov et al. (2018) found changes in both the pycnocline depth, approximated as the mixed-layer depth z0 in this study, and the change in buoyancy across the pycnocline (Δb in this study). Specifically, they found a pan-Arctic increase z0, possibly due to surface-layer freshening or deepening of the mixed-layer due to intensification of wind-driven mixing (Polyakov et al., 2020). Combined with potentially smaller h0 from sea-ice smoothing, this would result in a reduction in η and less ice keel-induced turbulent dissipation and mixing and reduced IW drag in the upper ocean. Polyakov et al. (2018) also found an increase in Δb in

the Amerasian Basin (Cluster A0 and seasonally S0 and W0), which would correspond to an increase in Fr. Based on our numerical simulations, this increase in Fr would reduce KE and dissipation rates. However, the relationship between the IW drag and Fr, at least in the current parameterization, is nonlinear (Fig. 9(right)), so further analysis is needed to assess the ocean's response to these changes."

"Our estimates of KE dissipation rates from the idealized numerical simulations and the estimates of CIW from the current parameterization both indicate a differences between the regions of perennial sea ice (Cluster A0) and seasonal sea ice (Clusters A1 and A2). In particular, we find that these seasonal sea ice regions are predicted to have larger CIW (Fig. 11(a)), but possibly smaller KE dissipation rates (Fig. 12). This can be important as the proportion of perennial sea ice in the Arctic has been decreasing in the past three decades (Serreze and Meier, 2019)."

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. 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.

"In this study, we assume that the wind has already transferred momentum to the ice keel to move it, and hence neglect modelling the atmosphere-ice stress. This is a common assumption for an idealized process study (e.g., Zu et al., 2021; De Abreu et al., 2024; Wang et al., 2026) such as the current one aimed to isolate the dynamics of internal wave generation by the ice keels, However, in the ocean, momentum transfer within the atmosphere-ocean-ice coupled system can be simultaneous and is a more complicated process (Brenner et al., 2021). Another nondimensional parameter, the Nansen number, which measures the ratio between the atmosphere-ice and ice-ocean drag coefficients scaled by the ratio of air and sea water densities, can be useful to characterize this coupling. Better-constrained Nansen number, possibly through further observational campaigns of measuring atmosphere-ice and ice-ocean drag coefficients (e.g., Brenner et al., 2021; Fer et al., 2022; Kawaguchi et al., 2024; Reifenberg et al., 2025), would be important for more accurately capturing the three-way coupling in climate models."

3. 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):

*"The width μ with taken to be a small finite value to represent a sharp pycnocline yet
to maintain numerical stability of the simulations. However, it can also be varied after
examining observational measurements of the stratification profiles in the Arctic, thus creating a
fifth nondimensional parameter."*

4. 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 1.

*"In this study, we made a particular choice of six GMM clusters based on the statistical
information from the BIC score and by considering the interpretability of our results. For the
purpose of the discussion here, we focus on the annually-averaged data, though similar
conclusions can be made for winter and summer clusters. Based the BIC curve (Fig. 3), we
should have chosen a much larger number of clusters (K ≈ 20 clusters) in order to improve the
GMM's ability to capture the variability in the data. Having more clusters would have allowed
us have better-constrained clusters, i.e., reduce the standard deviation of the nondimensional
variable ranges within each cluster, especially for Cluster A5, and less overlap between clusters
(Fig. 9). However, this would have been too many clusters to interpret in terms of physical
regimes. On the opposite end of the spectrum, we can also divide the Arctic broadly into three
regimes: (1) central Arctic, i.e., Clusters A0-1, (2) marginal ice regions with larger η (smaller
keel depth, deeper pycnocline) and without substantial IW generation, i.e., Clusters A3-5, and
(3) marginal ice regions with intermediate η that support IW generation, i.e., Cluster A2.
However, such broad characterization would return a wide range of nondimensional parameter
values for the central Arctic, which perhaps an insightful result. In order to examine the tradeoff
between the accuracy of representing the statistical distribution of the nondimensional
parameters (i.e., large K) and ease of interpretation (i.e., small K), we show the spatial*

distribution of GMM clusters for K = 4, 5, and 7 in SI Fig. 1 for comparison with K = 6 in Fig. 5(a). Too few clusters (e.g., K = 4) leaves the entire central Arctic region as a single cluster. However, for K = 5 − 7, the GMM algorithm returns a relatively consistently clustered centered Arctic, separating the Amerasian and Eurasian basins, and the outer eastern Eurasian seas. Increasing the number of clusters K in this range (5 − 7) seems to predominantly break marginal regions into even smaller clusters. Based on this analysis, we choose K = 6, though realizing that this choice is rather subjective."

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 amended the statement to:
"One of the mechanisms through which sea ice keels modulate upper-ocean momentum and mixing internal wave (IW) generation."

L11 "open-ocean" is not a good descriptor here, as this can easily be misunderstood as "open water"
Changed to "the central Arctic"

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

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.

"Overall, we identify ranges of nondimensional parameter values that characterize the IW generation by ice keels in the Arctic that can be serve as target ranges of nondimensional values to use in numerical simulations for calibration of the parameterizations of IW drag imposed in climate models."

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 L26.
In L67, 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"

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: 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.

   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

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.

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 as:

*"Flocco et al. (2024) applied this parameterization to demonstrate via a coupled ice–ocean
model that the resulting IW drag can reduce ice drift by up to 10%, enhance sea ice thickness by
as much as 15% in regions such as the Canadian Arctic, and suppress bottom melt rates.
However, their simulations also show that the effects of the IW drag on sea ice thickness
is spatio-temporally dependent, as they found decrease in sea ice thickness over some parts of
the Arctic by including IW drag."*

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..."

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:

"...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:

"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:

"GMM is an unsupervised clustering method that attempts to represent the data as a linear combination of K M -dimensional Gaussian distributions (Reynolds et al., 2009). K is the number of clusters that needs to be specified, and M is the number of input variables. Unlike other clustering algorithms, such as k-means, that are deterministic, GMM is a probabilistic

approach to clustering. For each data point, it assigns the probability of belonging to one of the K distributions; hence, one can use these probabilities to assess the model performance."

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.

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:

"The details of model set-up, implementation, and validation are in Flocco et al. (2024), Storkey et al. (2018), and Stroeve et al. (2018), which we summarize here. Specifically, we use the reference run from their study, in which the ice–ocean drag coefficient only includes form and skin drag contributions calculated using the parameterization from Tsamados et al. (2014) without the parameterized IW drag. The model has 1° degree tripolar grid (approximately 40km grid resolution). It has 75 unevenly-spaced layers in the vertical: 1m spacing near the surface increasing to 2m at 10m depth and towards 200m near the bottom, with 31 depth levels within the top 200m aimed to resolved the near-surface processes. The timestep is 2700 seconds. Unresolved subgrid motions of the flow, i.e., the vertical mixing of tracers and momentum, are parameterized using turbulent kinetic energy (TKE) scheme (Storkey et al., 2018). The sea ice model CICE accounts for the deformation of the sea ice cover using the an elastic anisotropic-plastic rheology model (Tsamados et al., 2013) and for thermodynamical processes through an energy-conserving thermodynamic model of sea ice (Bitz and Lipscomb, 1999) and a melt pond model (Flocco et al., 2010). The sea ice and ocean models are coupled through the parameterized quadratic form drag (Tsamados et al., 2014)."

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

o   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" sections:

"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 ($\approx$ 2 m or so)

•   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.
•   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 "Implications" subsection, as this strengthens the need for revisiting the McPhee and Kantha parameterization:

"It is important to note that the IW drag parameterization by McPhee and Kantha (1989) was developed for a two-dimensional model assuming small keel height h0, i.e., small topographic criticality parameter J for a fixed stratification N0 and relative keel speed u0. Recent study that assessed the parameterizations of form drag for flow over seamounts (Johnston et al., 2025) found that the disagreement between numerical simulations and the two-dimensional

parameterization also derived for small mount heights (Bell Jr., 1975) increased as J increased. Specifically, they found that the parameterization underestimated the form drag more in comparison to the numerical simulations for larger values of J: at J = 1, the parameterized drag was only about one third of the value estimated from the simulations. This result suggests that the current parameterization for CIW by McPhee and Kantha (1989) might also be underestimating the drag at larger values of J. In this study, we find many points in across the Arctic with J ≳ 1: 67% of all grid points in the annual average, 91% during the summer months, and 7% during the winter months. Our findings indicated that this supercritical regime J ≳ 1 could be an important parameter regime, especially during the summer, that is not well-represented by the current parameterization and needs to be re-evaluated through future numerical studies."

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

"Outside of this range, lee waves are evanescent and their amplitude exponentially decreases away from the topographic obstacle, though they could be important for localized mixing and energy dissipation (Legg, 2021)."

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:
"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. This would potentially limit the keel's ability to generate disturbance at the pycnocline and radiate IWs into the stratified interior."

L145 The choice for the variable symbol for the nondimensional parameter $\Delta b/(k_0 u_2 0)$ 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 defined in Eqns. 6-7).

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. 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 for this dataset are already plotted in Flocco et al (2024). However, we now made figures for the distribution of the nondimensional parameters broken down by the cluster for each of the (a) annual, (b) summer, and (c) winter clusters. Please see our Revised Figure 4.
- 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.

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?
- We have now added the median values to the Table.
- 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 Cluster 1: 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 (Table 1, also apparent from the large circles for (former) Cluster 1 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 a restructured "Methods" section, where we discuss the nondimensional parameters.
With regards to the derivation of the equation, 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 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.

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.

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:

"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 (u_0, k_0, h_0, z_0, Δb, N_0) 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 will address 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. A_{pyc} instead of A_{pyc}, which would also apply for "above"/"below" etc
Changed
Table 2 the units should be formatted like m^x 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

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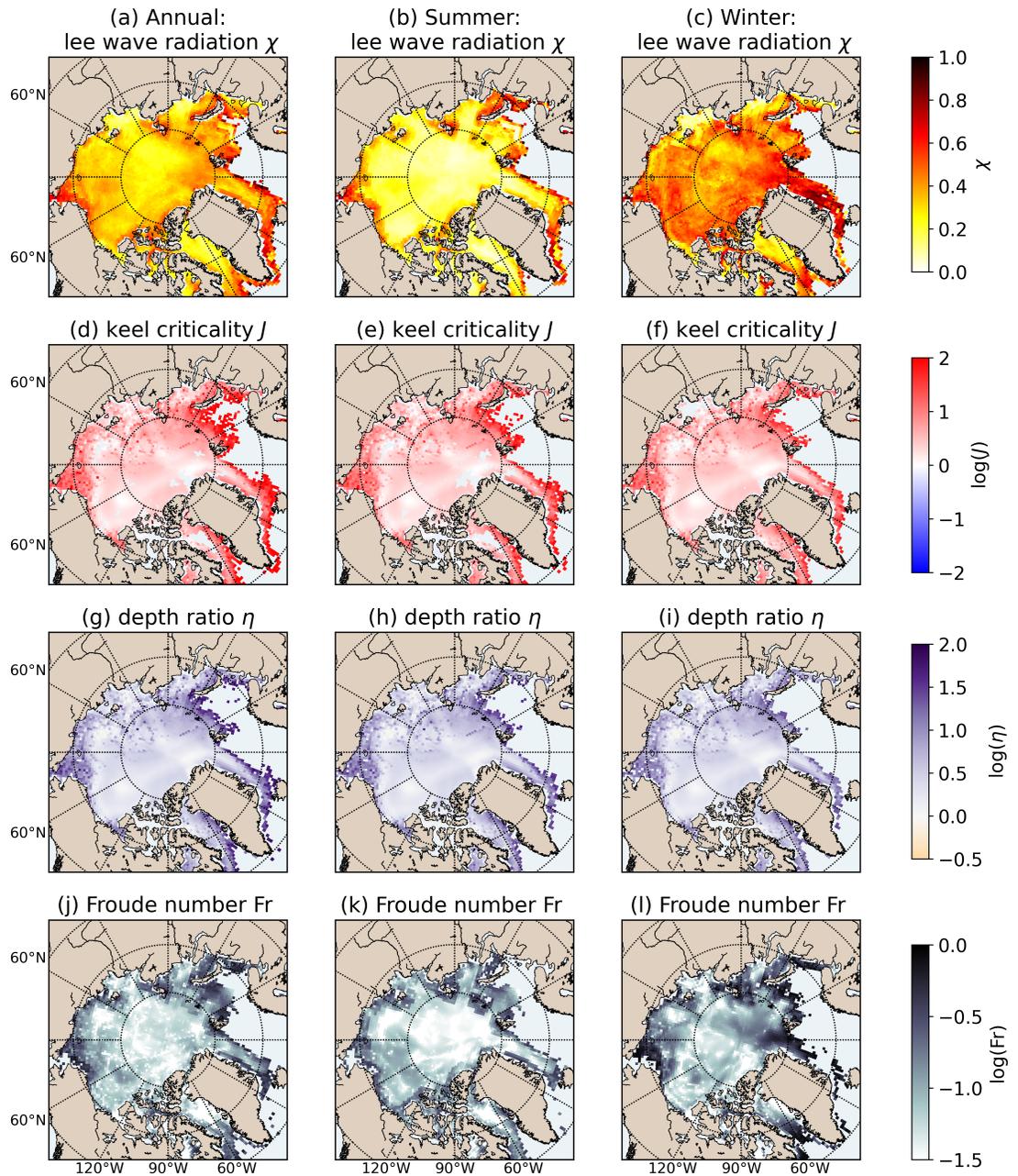
Revised Figure 2: Spatial distribution of time-averaged five nondimensional variables over the Arctic Ocean: (a-c) lee wave radiation parameter χ, (d-f) keel criticality parameter J, (g-i) depth ratio η, and (j-l) Froude number Fr. The values are based on the values averaged over different time intervals: (left) annually, (middle) over the summer months, and (right) over the winter months. Note that colorbars vary across subplots and the magnitudes of (d-f) J, (g-i) η and (j-l) Fr are shown on a logarithmic scale.
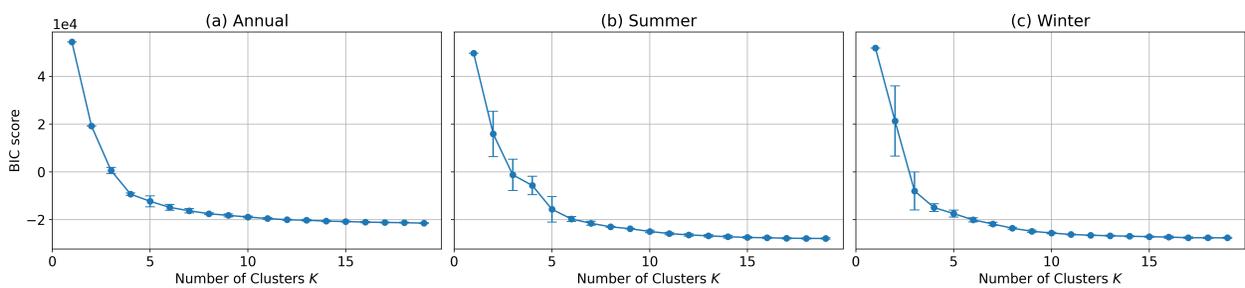
Revised Figure 3: Bayesian Information Criterion (BIC) scores for GMM fitted to the five-dimensional feature space composed of χ, J, η, and Fr. Models were fitted for cluster numbers ranging from 1 to 19. Each model fitting was repeated 20 times with different random initializations to assess variability in BIC values; error bars indicate ± 1 standard deviation. The values are based on the values averaged over different time intervals: (a) annually, (b) over the summer months, and (c) over the winter months.
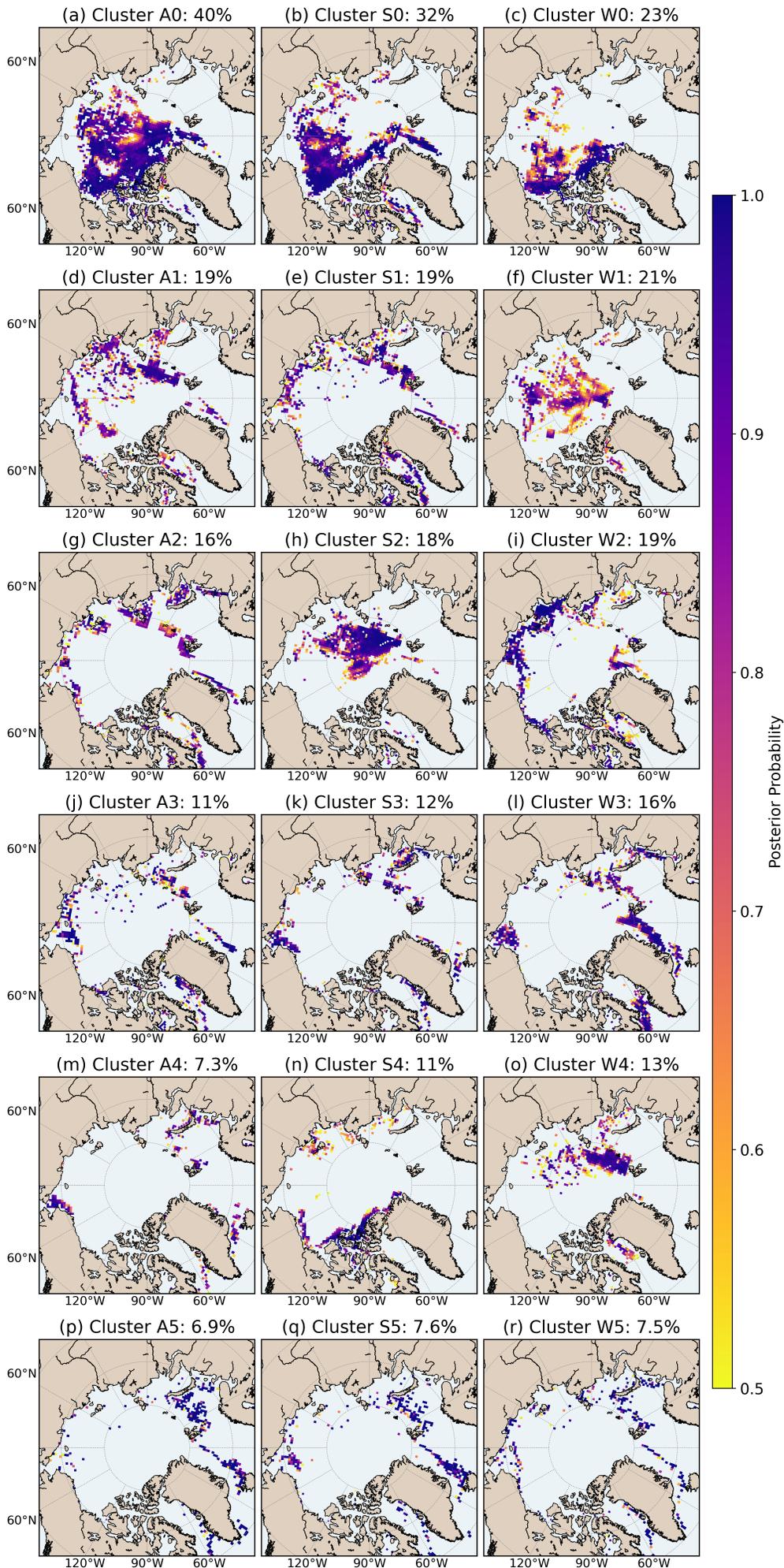
Revised Figure 4: Posterior probability maps for each of the six clusters identified by the GMM, based on time-averaged standardized nondimensional parameters (χ, J, η, and Fr. The clusters are based on the values averaged over different time intervals: (left) annually "A", (middle) over the summer months "S", and (right) over the winter months "W". Each subplot shows the posterior confidence that a given spatial grid cell belongs to the respective cluster. Clusters within each temporal averaging space are all ordered in the descending proportion of data points that belong to each cluster (i.e., most data points belong to cluster 0) and the proportion is shown in each subplot title (e.g., 40% of the annually-averaged data points below to the cluster A0).
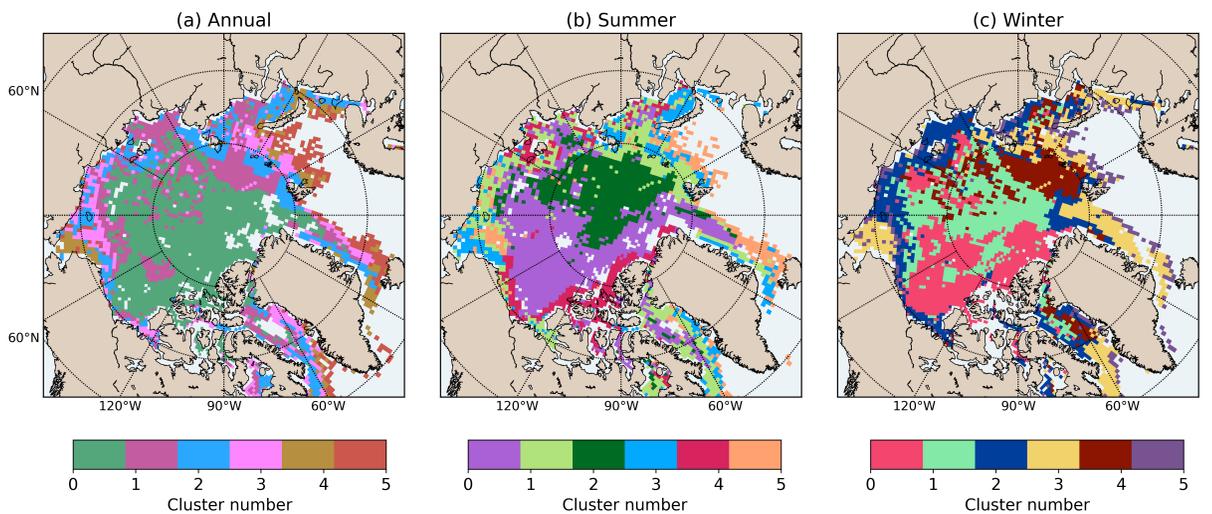
Revised Figure 5: The spatial distribution of six statistically inferred regimes ($K = 6$), each represented by a unique color, over the Arctic Ocean domain, derived from a GMM fitted to standardized time-averaged nondimensional parameter values across all spatial grid points. The values are based on the values averaged over different time intervals: (a) annually, (b) over the summer months, and (c) over the winter months. Clusters within each temporal averaging space are all ordered in the descending proportion of data points that belong to each cluster (i.e., most data points belong to cluster 0).
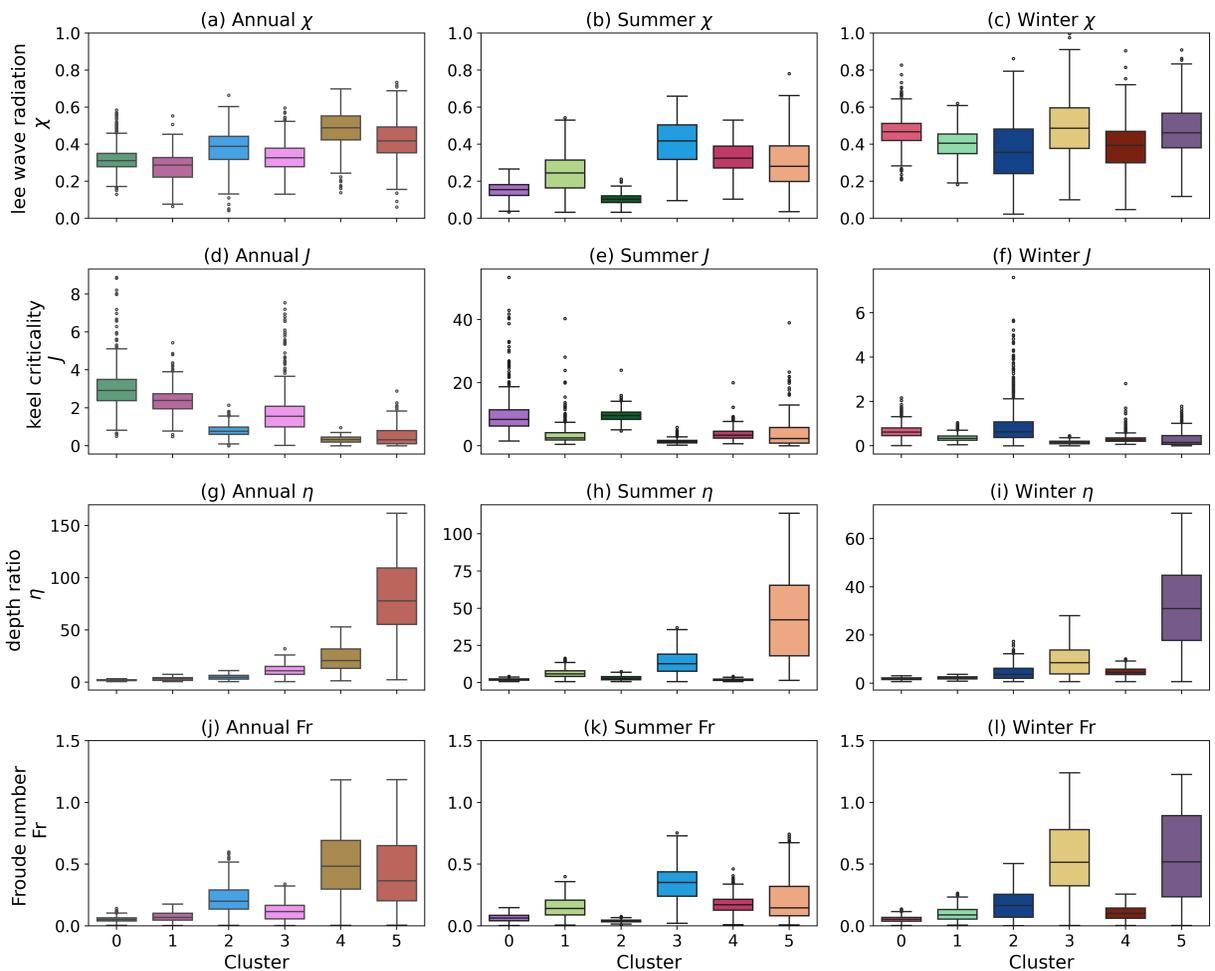

Revised Figure 6: Distribution of nondimensional variables across six GMM clusters based on annual-averaged nondimensional variable values shown in Fig. 4. Box plots summarize the spread of each computed nondimensional parameter (χ, J, J, η, and Fr) across the six clusters identified by the GMM. Each subplot corresponds to a single parameter, with individual boxes showing the interquartile range, median, and outliers for each cluster. The values are based on the values averaged over different time intervals: (left) annually, (middle) over the summer months, and (right) over the winter months.

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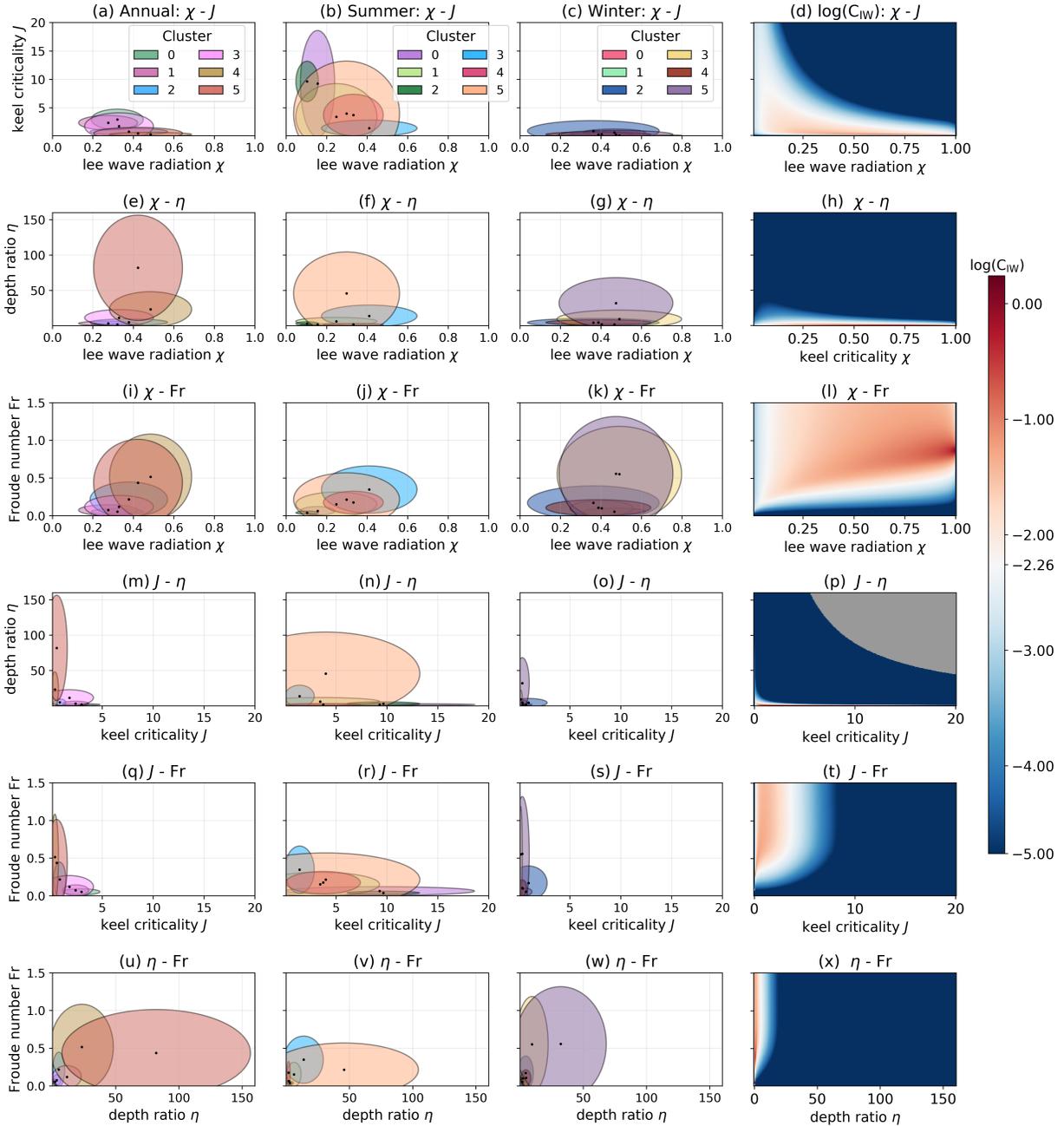
Revised Figure 9: (left three columns) Pairwise ellipse plots showing the cluster-mean values and associated variability across six GMM-identified regimes (columns from left to right: Annual, Summer, Winter). Each colored ellipse is centered at the cluster mean for the variable pair shown and spans two standard deviations along each axis, capturing the internal spread of that cluster. (right column) internal wave drag C_{IW} induced by the ice keel calculated from the parameterization expression over the joint pairwise parameter range. The values of C_{IW} are plotted on a logarithmic scale and the white values (center of the colorbar) corresponds to the the canonical ice-ocean drag coefficient value of $C_D = 5.5 \times 10^{-3}$ ($\log(C_D) = -2.26$). Subplots on each row correspond to the following pairs: (a-d) χ–J, (b-h) χ–η, (i-l) χ–Fr, (m-p) J–η, (q-t) J–Fr, and (u-x) η–Fr. Note that the grey shaded region in (p) represents undefined values in the parameterization due to the large values of J and η.
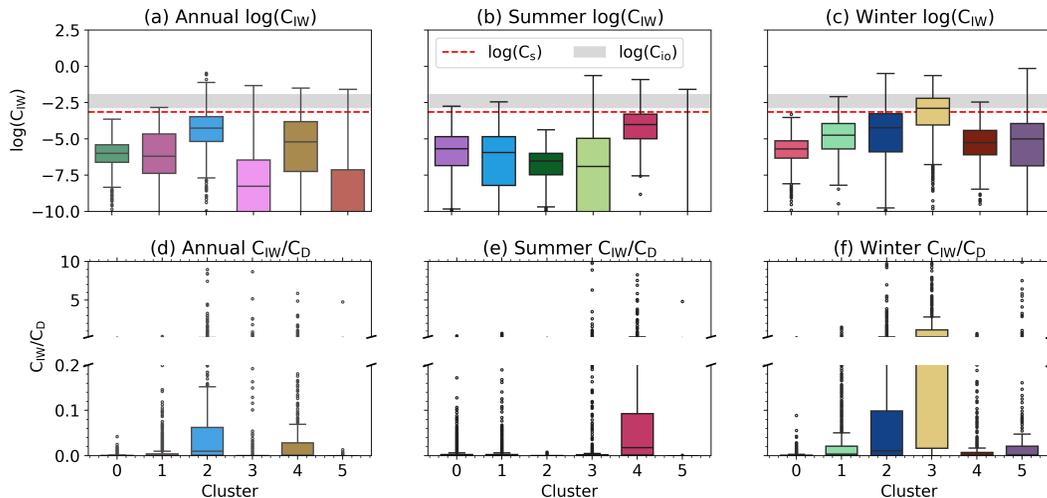
Revised Figure 11: Distribution of internal wave drag C_{IW} induced by the ice keel calculated from the parameterization expression for each of the clusters based on data averaged over: (a, d) annually, (b, e) over the summer months, and (c, f) over the winter months. Panels (a-c) show the values of C_{IW}, with grey shaded region representing the range of ice-ocean drag coefficients C_{io} and skin drag coefficient C_{S} estimated from observations. Panels (d-f) show the ratio between the parameterized values of C_{IW} for each cluster and the canonical ice-ocean drag coefficient value of $C_{\mathrm{D}} = 5.5 \times 10^{-3}$. Note that in (a-c), in order to better see the differences across clusters with larger internal wave drag, the $y-$axis is cropped; so values for some clusters that fall below $\log(C_{\mathrm{IW}}) = -10$ and are too small to be shown. Also, note that in (d-f), the vertical y-axis is broken into two intervals $[0, 0.2]$ and $[0.2, 10]$ in order to show the distributions for clusters with both small and large values (e.g., cluster W3).
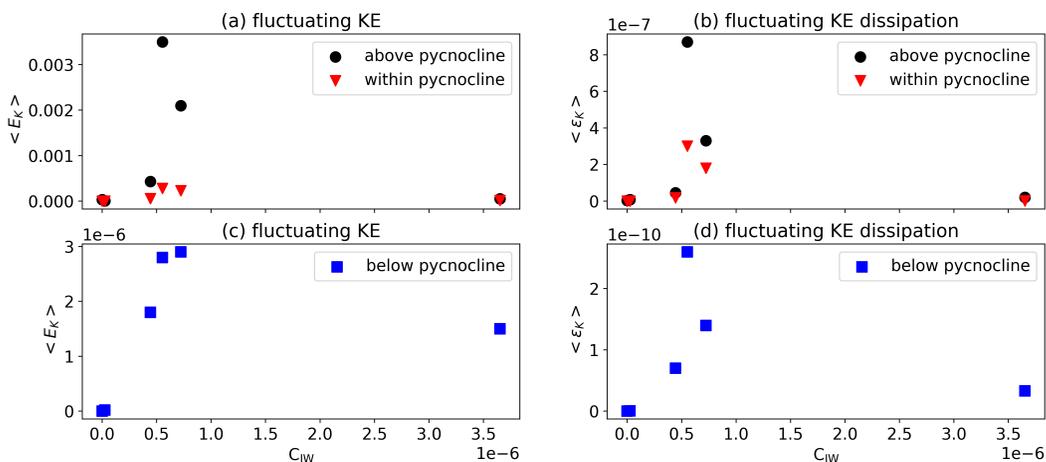
New Figure 12: (a, c) fluctuating KE and (b, d) fluctuating KE dissipation from the idealized 2D numerical simulations plotted against the IW drag coefficients C_{IW} calculated based on the current parameterization for the same values of nondimensional parameters as used to initialize the numerical simulations. Panels (a, b) show the energy terms averaged in the regions above the pycnocline (within the mixed layer) and within the pycnocline, and (c, d) averaged below the pycnocline (within the stratified portion of the domain).
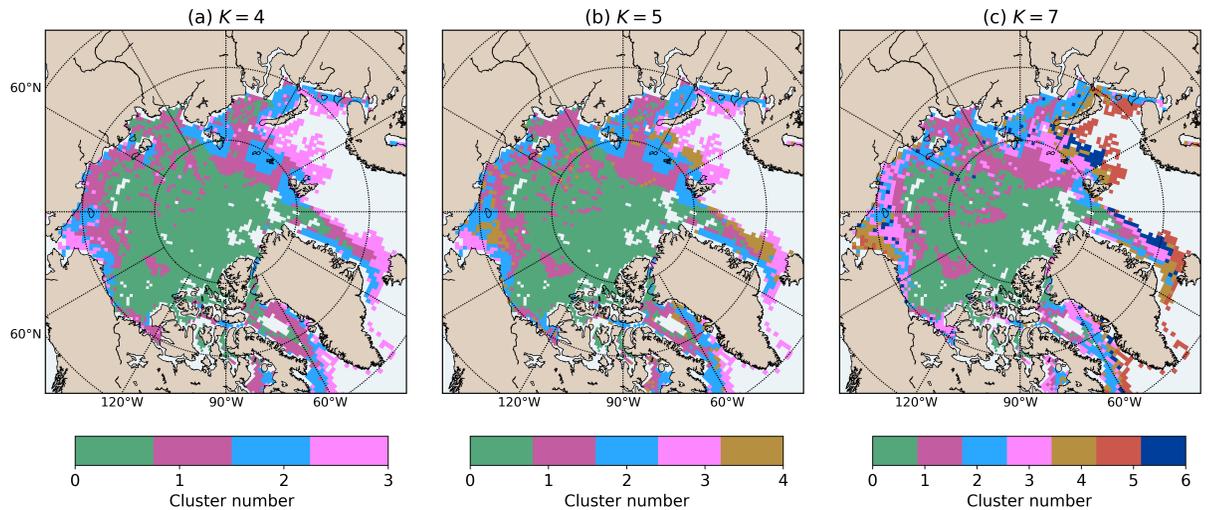
Supplementary Figure 1: The spatial distribution of GMM clusters for the annually-averaged data changing input values of the number of clusters K: (a) $K = 4$, (b) $K = 5$, and (c) $K = 7$. The case with $K = 6$ is shown in main text Fig. 6(a). Clusters are all ordered in the descending proportion of data points that belong to each cluster (i.e., most data points belong to cluster 0).
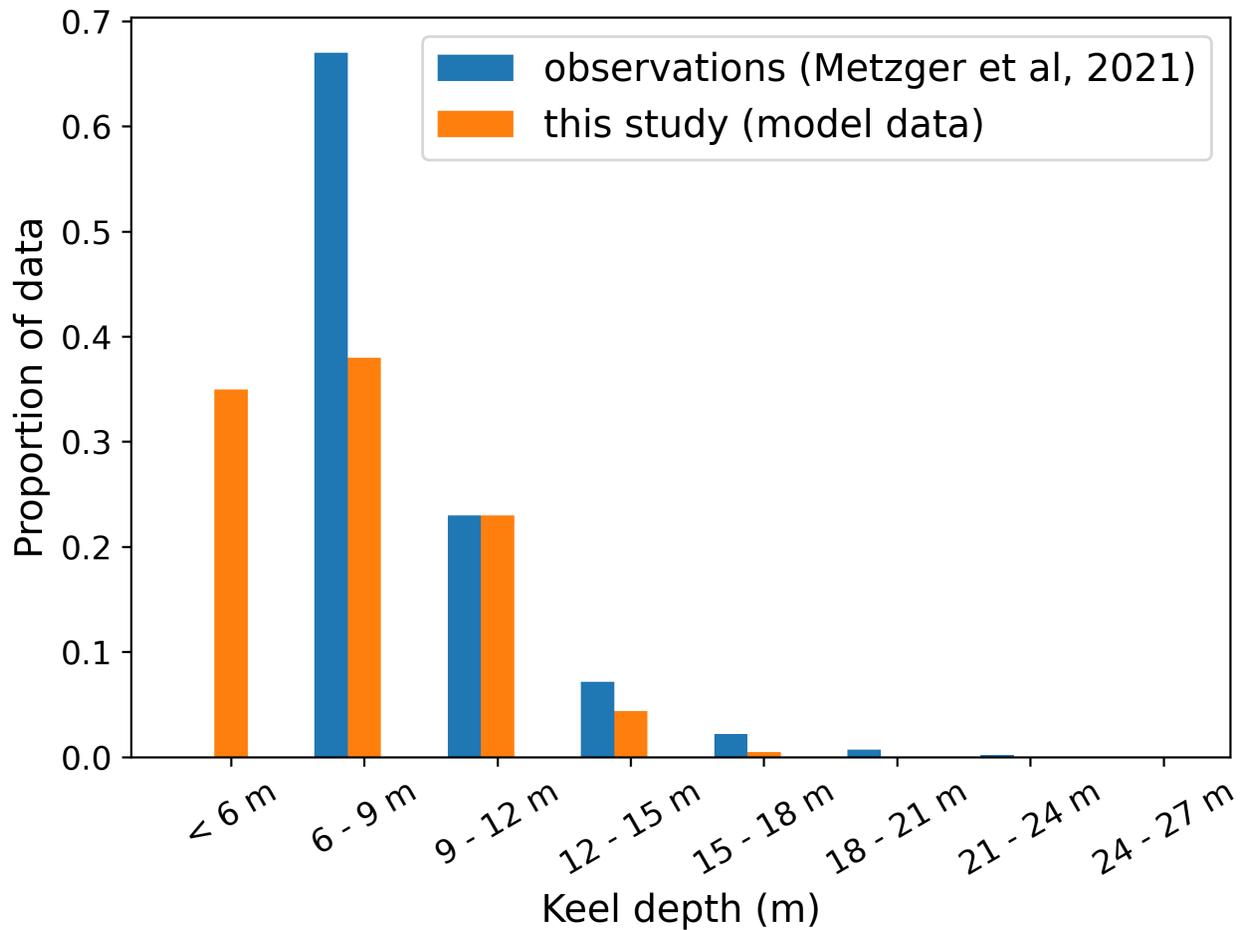
Supplementary Figure 2: Distribution of keel depths based on (blue) the observational study by Metzger et al (2021) and (orange) model output from Flocco et al (2024) that is used as inputs in this study. For the model output, this distribution reflects all data points, not only the ones within the lee wave radiation range ($\chi < 1$).