Responses to Reviewer 1

I appreciate the thorough revisions and acknowledge the time and efforts the authors spent on the revised manuscript. I particularly enjoyed the restructured discussion. I think that the revisions made the strenghts and limitations of the study clearer. I recommend this manuscript to be accepted after the following rather minor comments are considered. Most of them are technical or aimed at improved readibility (acknowledging that the manuscript is generally well written).

L1: I suggest moving the word "stratified" into the next sentence, resulting in:

"Sea ice keels influence momentum transfer between the drifting ice cover and the upper ocean, yet their effects remain difficult to represent in large-scale models. When keels move relative to the stratified ocean, they can generate internal waves that contribute an additional drag and modify upper-ocean energetics."

My reasoning is that sea ice keels can also transfer momentum without stratification, just not in form of lee waves.

Thank you for this suggestion. We have changed these two sentences as suggested.

L18: I kindly request also to include an earlier reference than Kharitonov and Borodkin that relates mechanical forcing to morphology, perhaps Parmerter and Coon (1972), https://doi.org/10.1029/JC077i033p06565, or similar.

Thank you for this reference suggestion – it is now added.

L19: end of sentence: one comma too many

Fixed

L20: "comprise of three main components" -- four are listed, but see next comment

Fixed as suggested below

L21: The formulation "ice-ice stress" is a bit unusual, as far as I'm concerned. Perhaps this paragraph (starting in L20) can be streamlined like this:

"When ice is in free drift, i.e. internal stresses are negligible, the total ice-ocean stress can be thought to comprise of three main components: (1) the skin drag, (2) form drag, and (3) IW drag (McPhee and Kantha, 1989; 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 and floe edges presenting discrete obstacles to the flow."

Thank you for this suggested. We edited the text accordingly.

L36: consider deleting ' "towing" or '

Deleted

L50: period missing at end of line

Fixed.

L51: suggest rewriting into "Outside of this range, lee waves are evanescent and their amplitude exponentially decreases away from the topographic obstacle, a process considered to be important for localized mixing and energy dissipation (Legg, 2021)."

Grammatically and stylistically we prefer the original statement, as we believe both convey the same message.

L136: "regions of the Arctic closer land boundaries" .. do you mean coastal regions, or the shelf seas...? please rephrase

As per recommendation from Reviewer 2, we removed this sentence from text.

L137 (and other occasions): if the author's did not intentionally use "center Arctic", I suggest using the more common "Central Arctic Ocean"

Thank you for this suggestion. We replaced this term throughout text accordingly.

L195-L196: "Larger Fr (smaller Ri) corresponds to a weaker pycnocline and/or stronger forcing and has been previously found to result in more unstable, supercritical flow conditions." please add reference to this statement

Citation is now added.

L330/Eq19: I come from a rather observation-heavy background. For me and many others in the community, the term "turbulent/fluctuating energy dissipation" is rather reserved to small-scale turbulence, with "small-scale" really referring to the dissipative scales down to molecular diffusion. I understand that this is slightly different in the modeling world. I also understand (and agree) that this quantity in Eq19 is a physically meaningful and interesting parameter to analyse, and that it (if I understand correctly) is a measure for how much energy is dissipated / lost to sub-model-gridscales in the model. It may also be fine to call it turbulent kinetic energy dissipation. But can it really be compared to observed dissipation rates from measurements later in the discussion, particularly when the viscosity in the model is chosen to keep the model stable? Long story short, I do not request fundamental changes, but perhaps one or two references to studies using similar approaches and terminology would clarify this for readers with a different background, and see next comment.

L567/L568: "In our idealized [...]" --- Referring to my previous comment: Is it really justified to make a 1-1 comparison of modeled and observed dissipation rates? I don't doubt it in general, but perhaps the authors can clarify this by referencing similar studies.

Addressing both of these points together, we first would like to mention that in direct numerical simulations (such as the ones involved in this study), there are no model sub-grid scales. All scales of motion from the largest scales to the molecular scales are explicitly resolved. The main caveat is that by making the model viscosity larger than the molecular viscosity of sea water, we reduce the range of possible length scales of motion, i.e., reduce the scale separation, which can result in different interactions from the “real world.”

Acknowledging this caveat, we added the following statements to the manuscript:

- We added references to other modeling studies using such definitions for perturbation KE on L335-337:
  "This is a common way to define perturbations and compute fluctuating (or turbulent) kinetic energy budget terms in numerical simulations involving internal waves (e.g., Nikurashin and Ferrari, 2010b; Shakespeare and McC. Hogg, 2018; De Abreu and Timmermans, 2026). Note that in this definition of fluctuating kinetic energy budget terms, we include both the internal waves and smaller scale motions."

- We added the following statement reminding the readers of how we define TKE and cautioning against direct comparison with observations on L579-583:

"Note that our estimates of the dissipation rates from the numerical simulations include energy loss from all motions that are deviations from the background flow (predominantly the generated IWs) and the model viscosity is larger than the molecular viscosity of sea water. Although some previous studies have made comparison of flow energetics and in particular dissipation rates with observations (e.g., Nikurashin and Ferrari, 2010a; De Abreu and Timmermans, 2026), any direct comparisons with observational values should be cautious."

Additional technical comments:

Fig1b: better use m s^-2 than m/s^2 (also applies to L300 and L308)

This has now been fixed in all figures and throughout text.

Fig2: period at the end of caption, also parameters instead of parameter in first sentence

Fixed.

L210-L215: I think the journal expects numbers and units to be separated by whitespace, e.g. 2 m instead 2m

Fixed

In the references:

There are some typos like "https://doi.org/https://doi.org/10.[...]", i.e. one "https://doi.org/" too many

Thank you for noticing this problem – we have fixed the reference entries accordingly.

Lastly, I thank the authors for their thorough revisions and the work put into the whole study, I enjoyed reviewing this manuscript. I find Fig. 2 and Fig. 4 particularly insightful, and believe the findings from idealized simulations can inform future related studies.

Thank you very much! We would also like to thank the Reviewer for many insightful comments and recommendations that undoubtedly significantly improved our manuscript.

Responses to Reviewer 2

This manuscript is much improved, and clearly explains the problem, its importance, and why the Arctic requires additional considerations. While there is a significant amount of methods, the organization and separation of this from the results is logical and easy to read. The results overall are well supported and interesting. I have only a few minor suggestions below.

Thank you very much for all of your constructive comments helped improve our study and our manuscript.

Minor comments

This is only a suggestion. Consider adding 'and internal wave drag' at the end of the title

Thank you for this suggestion. We added it to our title as we agree that it better reflects the content of our manuscript.

Lines 128-142: I suggest shortening this description. In my opinion, results should not be mentioned here.

We have taken out the sentence ("Specifically, we find regions of the Arctic closer land boundaries to be within parameter regimes of less internal wave generation and kinetic energy dissipation due to ice keel effects, whereas regions of the center Arctic to be in the regimes in which the flow is more affected by the moving ice keels, with greater internal kinetic energy and turbulence.") that explicitly mentions the results from this section.

Line 227: 'u0 is taken just below the pycnocline'. In summer, when mixed layer depths are shallow, is this the value just below the shallow summer mixed layer base, or still at the deeper base of the surface layer? I am curious about this because of the result at line 435: I am surprised that there are larger u0 values in winter months. Ice speeds are generally larger in summer, and interior ocean speeds are not that seasonal. Does this result from where the pycnocline is identified during summer? An example summertime profile of stratification and velocity from the model might help.

Unfortunately, the published model data from Flocco et al (2024) does not provide the full-depth density (or stratification) and velocity profiles (i.e., not the full 3D fields for the model outputs), only the pertinent dimensional variables that go into the IW drag estimate and that we use in formulating the nondimensional parameters. Therefore, we are unable to look at sample profiles.

However, we did examine the ice and ocean velocities (using the values that we have from the model data output) to examine their seasonality, as shown in the figure below. Indeed, we find faster ice velocity in the summer months compared to the winter months (this is also consistent with results from Flocco et al, 2024). We also find that there are seasonal differences in ocean speed: ocean currents are faster in the summer than in the winter, in particular in the central Arctic. This has been observed in previous studies (e.g., Polyakov et al, GRL, 2020). Cumulatively, these seasonal differences in ice and ocean current speeds result in higher u_0 in the winter. This is an interesting point that certainly should be investigated further, especially in terms of the sensitivity of the choice of depth to measure ice and current speeds, but we will leave it to another study.

Also, note that in the figure below, we compute the ice speed as $\sqrt{u_{ice}^2 + v_{ice}^2}$ in panels (a) and (b) and ocean speed as $u_{ocean}^2 + v_{ocean}^2$ in panels (d) and (f). Therefore, the difference between them is not exactly $u_0 = \sqrt{(u_{ice} - u_{ocean})^2 + (v_{ice} - v_{ocean})^2}$, but the individual components can be used to examine the justification behind the seasonal differences that we find for u_0.

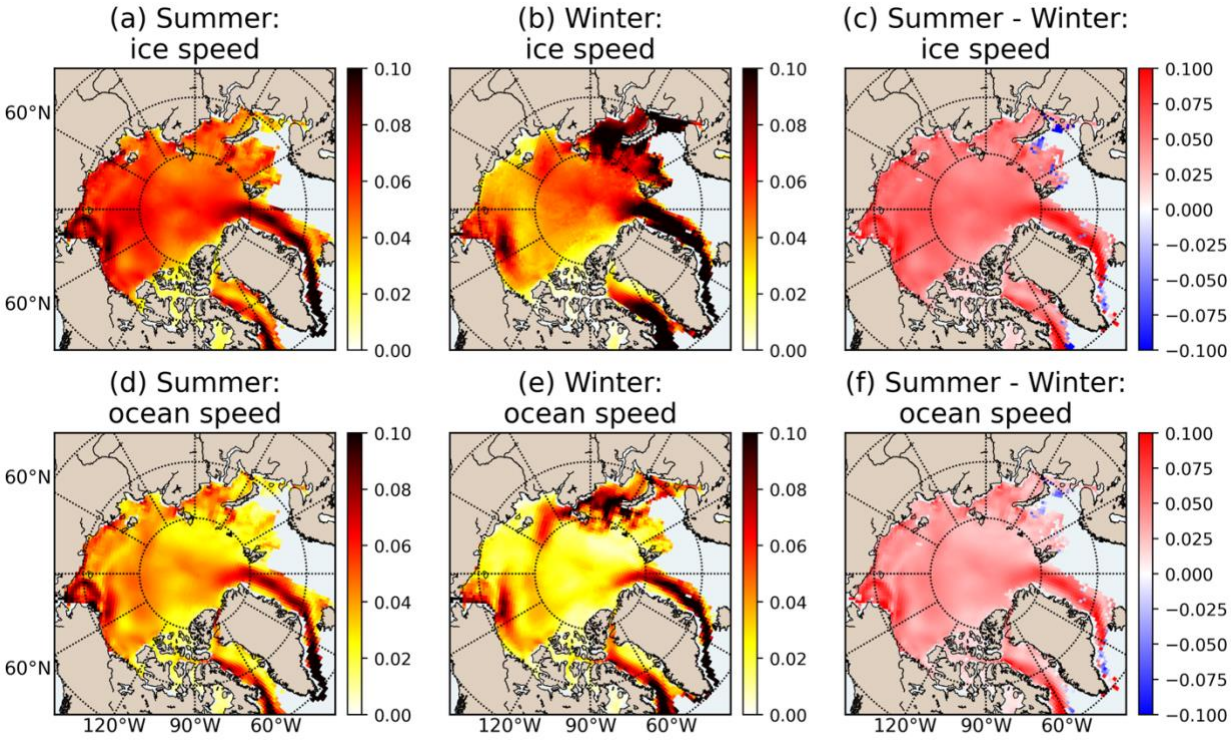

Figure 1: (a, b) ice speed and (d, e) ocean current speed from Flocco model. Seasonal differences (summer minus winter) are in (c) for ice speed and (f) for ocean current speed.

That summer is JJA and winter is DJF should be mentioned in the text as well as the figure captions. Neglecting September when mixed layers are also shallow (and shallower than June) is a choice worth commenting on.

- We now have explicitly defined "JJA" and "DJF" for summer and winter in text on L228-229).
- We added "JJA" and "DJF" to all figure captions where these were missing.
- We agree that our choice of the summer months neglects this important September regime (though as discussed in the Limitations section, the model has limited capability of capturing shallow pycnoclines). We added the following statement on L233-235 to comment on our choice of summer months: *"Notably, our definition of the summer months, while common, omits September, when the Arctic sea ice extent and thickness are typically at its minimum, so we could be omitting some shallow pycnoclines and small keel depths from the summer analysis."*

What time period does this model cover? A specific year?

As stated on L204, the model covers period over 2000-2017.

Line 313-314: rephrase as ’two summer clusters (s0 and s2) and one winter cluster (w2)’.

Done.

Figure 4: clusters 1 and 5 appear very similar in color when printed on paper

We adjusted the color of clusters A1and switched the colors of clusters A2 and A3 to improve the legibility of the map. This change has also been done to other cluster color-coded figures (Figs. 6, 9, 10, and 12).

One additional point could be added to the discussion. The keel shape when the keel extends below the mixed layer base in summer may be more blunt than the shape used here. This would result if there are warmer waters beneath the mixed layer that would preferentially melt the keel in this region.

Thank you for mentioning this point. We agree that while the timescales of the considered processes are probably too short for melting, melting itself is important to altering the shape of the keel. We added a short discussion and a recent reference (Eilers and Bradley, GRL, 2026) about different ice keel shapes (e.g., triangular, cusp, trapezoidal) on L646-651.

“The choice of shape to represent the ice keel also plays an important role. A recent study examining submarine sonar data (Eilers and Bradley, 2026) found that keels can have different shapes, e.g., triangular shapes, cusp shapes like Versoria used in this study, and trapezoidal shapes that have a flatter bottom. Their study also found that the keel shape is dependent upon the keel depth and processes that the keel undergoes throughout its lifecycle, e.g., it may form in a pointier shape but flatten along the bottom due to accelerated bottom melt in the summer. Such differences in keel shapes can affect IW generation and drag.”

Typos:

Line 20: 4 main components

We now changed this sentence in accordance with the suggestion from Reviewer 1.

Line 20: spell out IW

Changed

Line 215: possible typo, check ‘the an elastic anisotropic…’

Fixed

Line 426: ‘drage’ should be ‘drag’

Fixed

Line 550: sea ice-floe not sea ice-flow

This is correct as we mean the interaction between sea ice and ocean flow.

Figure 8 caption: for A3, A4, and A5, not clusters 3, 4, and 5.

Fixed