

Review of “Quantifying the Role of Sea Ice in Seasonal and Interannual Variability in Surface Stress and Mixed-Layer Entrainment in the Central Canada Basin” by Elizabeth Bailey, Annika Margevich and Mary-Lousie Timmermanns

This paper investigates surface stress, momentum transfer, and seasonal mixed-layer deepening in the Canadian sector of the Arctic Ocean. The study shows that surface stress increased over the period 2003–2023 and that this increase was primarily driven by changes in sea-ice concentration rather than changes in the wind field. The authors further investigate the impact of wind-driven stress on the ocean mixed layer, both seasonally and over time. They find that mechanical mixing dominates the seasonal evolution of the mixed layer during most months, with the exception of the melt season, when buoyancy forcing becomes more important. This is a somewhat surprising result and differs from findings in other regions of the Arctic Ocean, where convection-driven deepening associated with sea-ice growth and brine rejection is thought to be a primary driver of winter mixed-layer deepening. The authors also identify a general deepening trend in mixed-layer depth (MLD), but conclude that the observed trend in surface stress can explain only approximately 20% of this deepening. Thus, although surface stress appears to be an important driver of the climatological/seasonal MLD evolution, it does not appear to be the primary driver of the long-term MLD trend, the cause of which remains largely unresolved.

I really enjoyed reading this paper. Overall, this is a high-quality study presenting novel and carefully analyzed results. The topic is important, and the findings are both interesting and useful to the community. I believe the study will make a valuable contribution to our understanding of the rapidly evolving Arctic atmosphere–ice–ocean system. The figures are of high quality, the relevant literature is well covered, and the results are thoughtfully discussed. Overall, I consider the paper suitable for the journal and worthy of publication after some revision.

Below, I provide a relatively long list of (minor) comments, but most are simply suggestions for improving the manuscript or relatively straightforward revisions. I have only a few broader points that I think should be addressed.

One general comment concerns the structure of the manuscript. The paper is well written, but the organization is somewhat unconventional. Section 2 contains a mixture of methods and results. This works reasonably well, but it is worth considering whether a clearer separation would improve readability. Similarly, from Section 3 onward, results, interpretation, and discussion are frequently combined. Again, I generally found that this worked and that the manuscript reads well, but the structure differs from the more conventional separation of Results and Discussion. Section 4 also introduces additional methodological information

alongside the results. Overall, despite unconventional, I think the paper reads well, and thus leave it to the editor to decide whether restructuring is necessary.

Other major points:

1. I was surprised by the relatively small contribution of buoyancy forcing to MLD deepening during the sea-ice growth season. This appears quite different from conditions in the eastern Arctic, where sea-ice growth, brine rejection, and associated convection can play an important role in winter mixed-layer deepening. I think these results need to be discussed more explicitly in the context of Peralta-Ferriz & Woodgate (2015), who investigated mixed-layer variability in essentially the same region and over a substantially overlapping time period. In fact, your results appear broadly consistent with their finding that sea-ice growth/melt dominates seasonal mixed-layer variability over much of the Arctic, with the western Arctic being an important exception. At present, their results are mentioned only briefly. Given the substantial overlap in region, time period, and scientific question, I think a much more direct comparison is warranted. This regional distinction is an important result in itself. At present, I do not think it comes across clearly enough that the dominance of mechanical forcing found here may be characteristic of the western Arctic and may differ substantially from the mixed-layer dynamics of other Arctic regions. Related to this, would it be possible to also add another type of 1-D mixed-layer model, such as the approach used by Zhong et al. (2025), to independently assess the relative contributions of buoyancy forcing and mechanical mixing?
2. One of the most interesting results of the paper is also, in my view, currently somewhat understated. Your analysis indicates that wind-driven stress is the primary driver of seasonal MLD variability, yet the increase in surface stress explains only approximately 20% of the observed long-term MLD deepening. This is an important and somewhat counterintuitive result. The logical progression is essentially: a) MLD is primarily controlled by surface stress on seasonal timescales. b) Surface stress has increased over the observational period. c) MLD has also increased. d) However, the increase in surface stress explains only ~20% of the observed MLD trend. I think this contrast should be highlighted much more clearly throughout the manuscript. For example, in the abstract (around L15), wording such as “however” could help emphasize this result rather than allowing it to appear as a secondary detail.
This result also leaves the reader with an obvious question: what explains the remaining ~80% of the MLD deepening? I recognize that this is discussed to some extent in the final section, but I think this discussion needs to be substantially strengthened. It may also be worth considering whether additional analysis, for example upper-ocean

freshwater content could help constrain the likely cause of the MLD trend. If additional analysis can identify the dominant contribution to the remaining MLD trend, I think this would significantly strengthen the manuscript. As currently written, the paper develops a convincing story about the importance of changing surface stress, but this story becomes less clear at the end when it is shown that surface stress explains only a relatively small fraction of the long-term MLD trend. Without a clearer explanation of the remaining trend, the reader may also be left wondering whether the decomposition and attribution adequately capture all of the relevant processes. At a minimum, I therefore recommend expanding the discussion of other potential drivers of MLD deepening and bringing in additional literature on BG freshwater content.

3. In the abstract you write: “We demonstrate that the shifting sea-ice regime, characterized by greater mobility and increased responsiveness to wind forcing, is the primary driver of increased surface-ocean stress, as opposed to changes in the wind field itself.” This is a strong and well-supported result. I think it would be useful to state more explicitly, both earlier in the manuscript and in the conclusions, that this result is consistent with previous modeling studies (e.g., Martin et al., 2014; Muilwijk et al., 2024). Near the end of the manuscript, you also briefly discuss possible future changes in winds. Here, the discussion mainly suggests that Arctic winds are not expected to increase substantially. However, other studies (e.g., Vavrus et al., 2022; Muilwijk et al., 2024) indicate that wind speeds and/or surface stress may increase in parts of the Arctic in a warming climate. I think this discussion could be expanded and made more nuanced. This is particularly relevant because it increases the broader significance of your results. Your study demonstrates that changes in the sea-ice cover alone can substantially modify momentum transfer even without a corresponding increase in wind speed. If future changes include both a more mobile/reduced sea-ice cover and stronger winds, these effects could act together to further increase ocean surface stress, as suggested by modeling studies.
4. I think the Introduction and Discussion would benefit from greater consideration of changes in upper-ocean stratification. Given the contrasting evolution of Arctic stratification, with Atlantification and weakening stratification in the eastern Arctic versus strengthening haline stratification in the western Arctic, these changes may either reinforce or counteract the effects of changing surface forcing.
5. I strongly suggest mentioning the primary data sources (e.g., ITPs and moorings) in the abstract. One of the major strengths of this paper is that the conclusions are based on long-term observational records rather than model output. However, this is not

immediately obvious from either the title or the abstract.

6. I suggest adding an additional sentence or two discussing the uncertainty associated with the assumption of a constant drag coefficient. This is a very important source of uncertainty particularly given the substantial changes in sea-ice concentration, morphology, and mobility over the study period. You mention it, but I believe its potential uncertainty needs to be emphasized more strongly.
7. I think the ice-ocean governor effect deserves somewhat more attention throughout the manuscript. In particular, do your results have implications for how effective the ice-ocean governor may be under future sea-ice conditions? For example, could increasingly mobile and reduced sea ice weaken the governor effect and thereby allow a larger fraction of atmospheric momentum to be transferred to the ocean, as suggested by modeling studies?

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Minor comments:

L20 “Recent” changes in the...

L26–28 It would be useful to also include the mechanisms responsible for these ML salinity changes and MLD trends (as identified by these references).

L30 Strong “haline” stratification.

L28–30 Here you only explain the one-way link between stratification and sea ice (heat → melt). Perhaps also explicitly mention how sea ice contributes to shaping MLD/stratification through brine-rejection-driven convection. You could potentially include references from the Eurasian sector to illustrate how this link differs in regions with weaker salinity stratification/halocline.

L37–40 Perhaps divide this paragraph and the references into two parts: a) stress has been observed to increase, and b) it is projected to continue increasing. Perhaps also add Sterlin et al. (2023) to this list of references.

L40 Perhaps this is a good place to mention why the observed (Liu and Yu) and projected (Mulwijk et al. 2024) changes are largest during winter? I think it would also be good to mention the ice-ocean governor effect here (Meneghello et al., 2018). Could these changes be related to it becoming less effective? This could perhaps instead be discussed in the Discussion, but I would at least mention the governor effect and how it fits into the story here. Since your work is largely about how the ice pack regulates momentum transfer in the Beaufort Gyre

region, I think the Brenner et al. (2021) and Cole et al. (2017) papers should also be mentioned early in the Introduction (they currently first appear in the Methods).

L41 Before the sentence “It has not been quantified how increased stresses may have played a role in the mixed-layer deepening shown by Cole and Stadler (2019) and Liu and Yu (2025),” it may be worth introducing that surface stress plays an important role in shaping seasonal MLD (and referring to Peralta-Ferriz & Woodgate, 2015). Then state that the changes have not been linked, because the mean state has. Here you will also need a sentence or two (with appropriate references) explaining that stress is not the only mechanism influencing MLD (e.g., ice-growth-driven convection) and that changes in stratification, either heat- or freshwater-driven, can counteract or reinforce stress-driven MLD trends.

L45 Perhaps worth mentioning here that models project storms to increase in frequency and intensity (Oh et al., Climate Dynamics, 2020).

L45–57 This sentence is a bit unclear and appears to make two points. Consider rephrasing and separating it. I think this sentence naturally leads to the “research gap” before you explain what you do in this study, but the gap your study aims to fill is not completely clear. Spelling this out more specifically may help readers see the novelty of your study.

L46 Could consider adding references from the Eurasian Arctic here (e.g., Meyer et al., 2017, JGR; Graham et al., 2019, Scientific Reports).

L48 Since you mention ocean mixed-layer observations in the second part of the sentence, please also mention what data the first part is based on (atmospheric reanalysis combined with remote-sensing data).

L55 ITP reference? Toole et al.?

Fig. 1 Perhaps indicate the data source in the caption too? E.g., “from ERA5...”, etc.

L67 This sentence appears somewhat incomplete and poorly connected. Perhaps write something like, “In order to calculate surface stresses, we need...” It is the calculation of the stresses that requires this information, rather than the stresses themselves.

L70 Can you explain why you interpolate this to the EASE-Grid?

L71 Perhaps write “in situ radiosonde observations” instead of “ship-based.” Even though the sondes were launched from ships, the evaluations were against balloon-based measurements.

L77 “The wind field has not changed significantly from 2003–2023, with the exception of statistically significant wind speed increases...” I find this confusing. First you say there are no significant changes, and then mention that there are in some regions. I suggest something like:

“In most sectors of the BGR there are no significant changes, except in the XX and YY regions, where...”

L84 “Datasets agree well in the overlapping period.” Perhaps show this in the Supplementary? “Agree well” is not very quantitative.

L95–100 Perhaps mention the depth range over which this is calculated?

L108 and elsewhere I am confused about the choice of seasons. Can you comment on this? For example, why is September (the ice minimum) grouped with December? In terms of surface forcing, these months are quite different, which may influence your interpretations. Additionally, using season names (e.g., spring, winter, melt/growth season) instead of abbreviations such as MJJA and SOND would improve readability.

L154–155 I think this statement needs a reference or further support.

L185 I really like Figure S5 and suggest including it in the main manuscript. The finding that you confirm the modeling results of Martin et al. regarding optimal ice concentration is very nice, and I suggest highlighting this more in the Abstract and Conclusions as a key finding.

L190 The largest increase occurring during fall/winter was also shown by models. I would highlight that the observations are consistent with both regional model results (Martin et al. 2014) and global CMIP projections (Mulwijk et al 2024).

L205 “These authors highlight the increasing mobility of the ice pack as the primary mechanism behind increasing surface stress.” Yes. So what is your conclusion regarding your findings? Do you agree, confirm, or cannot say? I think some additional interpretation of your results in relation to this statement is needed here.

L208 I don’t think it is necessary to start a section by summarizing what you did in the previous section. This sentence could be removed. Similarly, I don’t think you need to end the section by stating what you will do in the next one (L222).

L243 Very interesting. Can you explain why you chose to use N^2 for the MLD calculation instead of the more classical density-gradient criterion (e.g., Peralta-Ferriz & Woodgate, 2015)? I think this is completely fine, and it is interesting/nice that it gives the same results as the density-difference method. Could you include a figure comparing MLD from these two methods in the Supplementary? I assume you have done both since you state that the results are identical.

L262 “From 2003 to 2019 the CCB MLD has deepened.” Can you specify whether this trend is based on all data annually or on a specific season? I see that you give the monthly trends later, but perhaps specify here that 1.1 considers all seasons.

L283 I am not sure I completely understand why large N_2 values coinciding with small $\Delta\rho$ values suggest mixed-layer entrainment. Can you explain this a bit more? Isn't the density difference small because the ML is relatively dense during the growth season? Similarly, I understand why the melt season gives a larger density gradient, but I don't understand how this implies weaker mechanical mixing. Furthermore, I find the phrase "to thicken the density interface at the mixed-layer base (Figure 10b)" slightly awkward and unclear.

L287 I think this "hint" needs a bit more explanation.

Figure 11 Very interesting, and I really like the methodology and results. I am aware that, due to the nature of this 1-D framework, the calculation does not capture the full change in MLD, as you nicely explain in the text. ΔH in reality differs from the modeled value. However, it would be very interesting to see exactly how the two compare (e.g., how much of the observed MLD change can be explained by the simplified 1-D parameterization). I strongly suggest adding a panel showing both the monthly mean observed ΔH and monthly mean calculated ΔH (the grey bars in Figure 11).

L219 I really like this section. Very interesting and a good method. In addition to the monthly mean thickness change, it would be great to see this as a time series, with each component shown similarly to Figure 8b.

L324 Could you compare these findings in more detail with Peralta-Ferriz & Woodgate (2015)?

L327 "Hence, surface momentum transfer plays a larger role in seasonal mixed-layer deepening than convection in the CCB (Figure 11)." Yes, great point. However, as currently written, it sounds as though this is the first study to show this, whereas you are confirming previous studies that have found the same. This should be mentioned. It should also be noted that this is particularly relevant to the western Arctic and is not necessarily true for other parts of the Arctic.

L341–349 This is a very important paragraph (and a key result of the paper), but a bit short given the importance of these statements. I think you should expand on this and consider including Figure S10 in the main paper. I see that you discuss this in the final Discussion, but I am baffled by the key result here: wind mixing is, according to your results, the primary driver of MLD, but it does not seem to explain the MLD trend (only ~20%). This is a very interesting but non-intuitive result, and the discrepancy is currently understated. For example, in the Abstract (L15), I would use "however" to emphasize this somewhat surprising result: MLD is primarily driven by stress → stress increases → MLD increases too → however, the stress increase explains only ~20% of the MLD trend.

L373–375 This statement and reference are not completely correct. Peralta-Ferriz & Woodgate (2015) conclude that the classical picture of MLD deepening being primarily driven by

convection associated with ice growth/brine rejection, with occasional storm-driven mixing, applies to most of the Arctic. However, they also emphasize that this is not true for the western Arctic, particularly the BG region, which is consistent with rather than contradictory to your results. Please revise accordingly.

L377–384 The Ekman pumping discussion is more of a new result than a discussion topic. Since this appears to be additional analysis, I think it belongs in the final part of Section 4 rather than in the summary.

L395 I would start this statement with “However,”.

L395–403 This is a key result, but I think the discussion is somewhat weak and could be improved. See major comments above. Simply saying “it is complicated” feels like an easy way of saying that the cause remains unknown.

L403–408 This section could be more nuanced. For example, several models project increased future wind speeds (Vavrus et al., 2022; Muilwijk et al., 2024), so this statement should include that possible scenario. I would rather end with a statement that it remains uncertain how future Arctic atmosphere–ice–ocean momentum transfer will change under evolving wind and ice conditions.

I like the final sentence regarding the importance of enhanced observations of these processes. However, I think you also need to highlight regionality, given that the western Arctic, particularly the Canada Basin, differs substantially from other regions (e.g., the Eurasian Arctic), and that these other regions lack the long time series available here from moorings, ITPs, etc.

Finally, I also miss a little bit of the “so what?” What are the broader implications of these trends? Feedback mechanisms related to ocean–ice heat exchange are briefly mentioned, but the importance of the trends described in the paper could be highlighted further, including, for example, impacts on ecosystems and other climate-system feedbacks.