Response to reviewers:

We greatly appreciate the detailed and thoughtful comments of both reviewers, and have addressed each one inline (in blue text) below. Their careful and constructive feedback identified important places to clarify and strengthen the manuscript, and we are grateful for the time and expertise they devoted to reviewing the paper.

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-Louise Timmermans

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?

Thank you for raising this point related to the significant regional differences in the dominant mixed-layer deepening mechanisms. We now discuss this explicitly with better comparison and contrast to other studies, including a more detailed comparison to Peralta-Ferriz & Woodgate (2015). Text additions are detailed in our responses below.

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.

Thank you, we agree that further discussion of this strengthens the paper. We have made the wording changes you suggest and have moved the Ekman discussion from summary to results. Surface stress curl (and associated surface Ekman convergence) accounts for an additional ~20% of winter deepening, but there are significant uncertainties in all contributing factors. Submesoscale and mesoscale variability (including entrainment induced by intra-halocline eddies) are other possibilities, but we don't feel this analysis is a simple "add-on" to this paper, and is the subject of another of our ongoing studies. We have added more in-depth discussion as documented in our responses below.

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.

We very much appreciate this suggestion and have expanded on our discussion as follows:

"Global climate model projections present contrasting future scenarios for Arctic winds and momentum input. On one hand, multi-model climate ensembles \citep[e.g., CMIP5 and CMIP6;][]{vavrus_future_2022,muilwijk_future_2024} project overall strengthening of surface Arctic winds, including potential future increases in Arctic storm frequency and intensity \citep{oh_arctic_2020}. In these simulations, increasing surface winds combine with reduced internal ice stress in a thinner, more mobile ice pack to amplify momentum flux into the ocean \citep{muilwijk_future_2024}. On the other hand, regional model studies and localized atmospheric circulation shifts suggest scenarios where surface winds over specific Arctic

sub-basins may weaken \citep[e.g.,][]{athanase_arctic_2025}. Ultimately, how future Arctic atmosphere–ice–ocean momentum transfer will evolve remains subject to considerable uncertainty, depending on the trade-off between shifting wind forcing, evolving sea ice, and regional upper-ocean stratification."

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.

Thank you, we have added further context to the Introduction as follows:

"On seasonal timescales, surface momentum stress plays an essential role in shaping upper-ocean mixed-layer depth \citep[e.g.,][]{peralta-ferriz_seasonal_2015,cole_ice_2017}. Convection driven by sea-ice growth and brine rejection also contributes to winter mixed-layer deepening. In the Eurasian and Atlantic sectors, ongoing Atlantification and a weakening halocline reduce upper-ocean stability, enabling enhanced convective mixing, storm-driven entrainment, and upward heat fluxes from Atlantic Water that accelerate sea-ice melt \citep[e.g.,][]{peralta-ferriz_seasonal_2015,fer_onedimensional_2017,meyer_mixing_2017}. Conversely, the Canada Basin and Beaufort Gyre are characterized by a much stronger halocline \citep[see][]{timmermans_understanding_2020}. Here, changes in surface forcing must overcome a strong density barrier at the mixed-layer base, suppressing penetrative brine convection and requiring strong mechanical stress to drive vertical entrainment \citep{toole_influences_2010,cole_ice_2017,zhong_weaker_2025}. Although the mean seasonal relationship between surface stress and mixed-layer depth is well established, it has not been quantified how multi-decadal increases in ocean surface stress may have directly driven the long-term winter mixed-layer deepening observed in the Canada Basin over recent decades \citep{cole_deepening_2019,liu_satellitebased_2025}."

We have made other changes as documented elsewhere in these responses.

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.

We agree and have now changed the second sentence of the abstract to:

"Here, we analyze a 20-year (2003–2023) observational record, combining satellite remote sensing data and atmospheric reanalysis with \textit{in situ} sea-ice and ocean observations, to evaluate monthly and interannual variability in ocean surface stress and mixed-layer entrainment in the BGR and the central Canada Basin (CCB)."

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.

Thanks for the suggestion. We have added the following sentences where we introduce the drag coefficient:

“We adopt a constant ice-ocean drag coefficient ($C_{Di} = 5.5 \times 10^{-3}$) in our bulk surface stress formulation, following McPhee (2008) and Cole et al. (2017). However, observational and modeling studies demonstrate that C_{Di} varies temporally and spatially by up to a factor of 2–3 (1×10^{-3} to 10×10^{-3}) depending on under-ice keel geometry, floe size distribution, ice concentration, and surface melt (Cole et al., 2017; Brenner et al., 2021; Fer et al., 2022; Reifenberg et al., 2025). Over the 2003–2023 study period, sea-ice thinning and loss of multiyear floes have changed under-ice topography. While a constant C_{Di} captures first-order seasonal and decadal stress trends, variations in under-ice roughness introduce an estimated ~20–30\% uncertainty in stress magnitudes given that regional and seasonal mean C_{Di} values in the Beaufort Sea typically fluctuate within $\pm20\text{--}30\%$ of 5.5×10^{-3} (Cole et al., 2017; Brenner et al., 2021)."

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 iceocean 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?

Thank you, we have added the following text to our introduction for more context:

"In a consolidated ice pack, the 'ice–ocean stress governor' acts as a negative feedback mechanism on gyre spin up \citep{meneghello_observations_2018}. However, as ice mobility increases, the dampening effectiveness of the governor is reduced, amplifying both local mixed-layer entrainment and large-scale Ekman downwelling \citep{meneghello_observations_2018,muilwijk_future_2024,liu_satellitebased_2025}.”

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Minor comments:

L20 “Recent” changes in the…

Done.

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).

We have added the following:

"Specifically, following decades of mixed-layer freshening and shoaling driven by Ekman freshwater convergence during the spin-up of the Beaufort Gyre (Peralta-Ferriz and Woodgate, 2015; Proshutinsky et al., 2009), the system transitioned post-2012 into a period of gyre relaxation (Timmermans and Toole, 2023; Zhong et al., 2025). During this phase, reduced surface freshwater convergence led to an increase in mixed-layer salinity of $\sim 1\text{ g kg}^{-1}$ between 2006–2012 and 2013–2017 (Cole and Stadler, 2019). This surface salinification reduced the potential energy barrier to vertical mixing (Zhong et al., 2025) and enabled mechanical winter mixed layer deepening by $\sim 9\text{ m}$ over the same period (Cole and Stadler, 2019)."

L30 Strong “haline” stratification.

Done.

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.

We have now added this, along with discussion of the regional differences in mixed-layer forcing as noted in our response above.

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.

Thanks for the suggestion and pointing out this reference. We have partitioned this as suggested and cited Sterlin et al. so it now reads as follows:

"Quantifying ocean surface stress in the Arctic is particularly complicated due to sea ice modulating momentum transfer to the surface ocean, and sea ice losses significantly influence surface stresses. Ocean surface stress has been observed to increase over recent decades (e.g., Martin et al., 2014; Sterlin et al., 2023; Liu and Yu, 2025), and is projected to continue increasing as sea ice continues to thin and retreat (e.g., Muilwijk et al., 2024). Liu and Yu (2025) found wintertime (i.e., mostly under sea ice) average increases in ocean surface stress of approximately 0.005 N m^{-2} (around 50\%) when comparing the 2003–2012 and 2013–2022 periods in the BGR."

L40 Perhaps this is a good place to mention why the observed (Liu and Yu) and projected (Muilwijk 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).

We have updated in the text in the introduction to address this comment:

"Quantifying ocean surface stress in the Arctic is particularly complicated due to sea ice modulating momentum transfer to the surface ocean, and sea ice losses significantly influence surface stresses. Ocean surface stress has been observed to increase over recent decades (e.g., Martin et al., 2014; Sterlin et al., 2023; Liu and Yu, 2025), and is projected to continue increasing as sea ice continues to thin and retreat (e.g., Muilwijk et al., 2024).

Focusing on the ice-covered season, Liu and Yu (2025) found wintertime average increases in ocean surface stress of approximately 0.005 N m^{-2} (around 50%) when comparing the 2003–2012 and 2013–2022 periods in the Beaufort Gyre region. These winter increases occur because reduced internal ice stress in a thinner, more mobile winter ice pack allows greater momentum transfer into the water column, while a more compact ice cover dissipates more momentum through internal ice deformation (Martin et al., 2014; Muilwijk et al., 2024). In a consolidated ice pack, the 'ice–ocean stress governor' acts as a negative feedback mechanism on gyre spin up (Meneghello et al., 2018). However, as ice mobility increases, the dampening effectiveness of the governor is reduced (Meneghello et al., 2018; Muilwijk et al., 2024; Liu and Yu, 2025)."

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.

We appreciate these suggestions. We have added text around this place as follows:

“On seasonal timescales, surface momentum stress plays an essential role in shaping upper-ocean mixed-layer depth (e.g., Peralta-Ferriz & Woodgate, 2015; Cole et al., 2017). Convection driven by sea-ice growth and brine rejection also contributes to winter mixed-layer deepening. In the Eurasian and Atlantic sectors, ongoing Atlantification and a weakening halocline reduce upper-ocean stability, enabling enhanced convective mixing, storm-driven

entrainment, and upward heat fluxes from Atlantic Water that accelerate sea-ice melt (e.g., Peralta-Ferriz & Woodgate, 2015; Fer et al., 2017; Meyer et al., 2017). Conversely, the Canada Basin and Beaufort Gyre are characterized by a much stronger halocline (see Timmermans and Marshall, 2020). Here, changes in surface forcing must overcome a strong density barrier at the mixed-layer base, suppressing penetrative brine convection and requiring strong mechanical stress to drive vertical entrainment (Toole et al., 2010; Cole et al., 2017; Zhong et al., 2025). Although the mean seasonal relationship between surface stress and mixed-layer depth is well established, it has not been quantified how multi-decadal increases in ocean surface stress may have directly driven the long-term winter mixed-layer deepening observed in the Canada Basin over recent decades (Cole & Stadler, 2019; Liu & Yu, 2025)."

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

Thanks for the suggestion and citation, and we have now added mention of this, including the citation, where we describe the possibility of future changes in wind speed (see our response to your earlier comment).

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).

Thank you for this suggestion. The revised text in the Introduction now reads as follows:

“A challenge in linking storms to upper-ocean mixing is that sea-ice cover moderates the transfer of wind momentum into the ocean; consequently, intense atmospheric storms or synoptic cyclones may not produce proportional momentum transfer if a consolidated ice pack resists wind forcing (see e.g., Meyer et al., 2017; Fer et al., 2017; Graham et al., 2019; Martin et al., 2014). While studies in the Eurasian Arctic have shown that storm-driven ice deformation and mechanical mixing accelerate under-ice heat and momentum transfer, it remains unquantified how decadal shifts in sea-ice mobility and surface stress influence long-term mixed-layer deepening in the strongly stratified Central Canada Basin.

Here, we use a 20-year (2003–2023) record of ocean surface stress, wind forcing, and sea-ice drift derived from atmospheric reanalysis (ERA5) and satellite remote-sensing observations, alongside in situ ocean temperature and salinity profiles from Ice-Tethered Profilers (ITPs; Krishfield et al., 2008; Toole et al., 2011), together with a one-dimensional bulk mixed-layer model, to isolate and quantify the mechanical stress-driven versus buoyancy-driven mechanisms controlling mixed-layer evolution.”

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.

Thank you. We have updated the text as follows and included citations:

“Here, we use a 20-year (2003–2023) record of ocean surface stress, wind forcing, and sea-ice drift derived from atmospheric reanalysis (ERA5) and satellite remote-sensing observations, alongside in situ ocean temperature and salinity profiles from Ice-Tethered Profilers (ITPs; Krishfield et al., 2008; Toole et al., 2011), together with a one-dimensional bulk mixed-layer model, to isolate and quantify the mechanical stress-driven versus buoyancy-driven mechanisms controlling mixed-layer evolution.”

We have added to the caption of Figure 1: “Data sources are outlined in Section 2.1.” Adding specifics detracted from the readability of the caption.

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?

We have revised the sentence in Section 2 as suggested:

"In order to calculate ocean surface stresses, we require simultaneous observations of atmospheric wind forcing, sea-ice drift velocity, upper-ocean geostrophic currents, and sea-ice concentration. Specifically, we compile 10~m wind velocities ($\mathbf{u}_{10}$) from ERA5 atmospheric reanalysis (Hersbach et al., 2020), sea-ice drift velocities ($\mathbf{u}_i$) and sea-ice concentration from NSIDC satellite products (Tschudi et al., 2019, 2020), and surface ocean geostrophic velocities ($\mathbf{u}_g$) from satellite Dynamic Ocean Topography (Armitage et al., 2016, 2017; Lin et al., 2023)."

With respect to interpolation onto the EASE grid, we have clarified as follows:

"All atmospheric reanalysis, sea-ice, and dynamic ocean topography datasets were interpolated onto a common 25~km Equal-Area Scalable Earth Grid 2.0 (EASE-Grid 2.0; Brodzik et al., 2014; Meneghello et al., 2018). This grid matches the native grid of the NSIDC satellite sea-ice motion products, ensuring exact spatial alignment across all vector inputs ($\mathbf{u}_{10}, \mathbf{u}_i, \mathbf{u}_g$) for point-wise surface stress calculations while preserving equal grid-cell areas for spatial averaging across the Canada Basin."

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.

Done.

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…”

Thank you for pointing out this contradictory phrasing.

We have rephrased this sentence to clearly state that across the Central Canada Basin and the majority of the Beaufort Gyre Region (BGR), 10~m wind speeds show no statistically significant trend over the 20-year record, and we explicitly delineate the localized areas where weak trends do occur.

The revised text reads as follows:

"Across the CCB and most sectors of the BGR, mean 10~m wind speeds show no statistically significant trend over 2003–2023. Weak, localized wind speed increases ($\sim 0.1\text{--}0.2\text{ m s}^{-1}\text{ decade}^{-1}$, $p < 0.05$) are isolated primarily to the southern and southwestern margins of the Beaufort Sea near the continental slope (Figure S1a--c)."

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

We have quantified the agreement between datasets during the overlapping period by reporting the correlation coefficient ($r > 0.98$) and root-mean-square difference in Section 2, and we cite Margevich et al. (2026) for detailed evaluations.

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

We have added “surface” in front of “geostrophic currents”.

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.

Thank you for this feedback. Our choice of groupings were to capture the distinct melt/growth seasons within the CCB. Steady sea ice growth typically takes place between January and April, while steady sea ice melt occurs from May through August (see Figure 2 in Timmermans, 2015) . We have replaced the abbreviations (MJJA, SOND) with explicit seasonal descriptions

('melt season' and 'growth season') throughout the text. We further point out that our analysis based on 3-month groupings did not make an appreciable difference in our results.

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

We have added the citation as follows: “A fundamental challenge is that sea ice cover moderates the transfer of wind momentum into the ocean such that an intense cyclone may not lead to significant momentum transfer if extensive sea ice is not responsive to wind forcing, and direct evaluation of the surface stress magnitude is necessary (see e.g., Martin et al., 2014; Graham et al., 2019; Muilwijk et al., 2024).”

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.

Thank you, we have expanded the text slightly in the main manuscript as follows:

"The observations reveal a distinct non-linear relationship between ocean surface stress magnitude normalized by wind speed and SIC (Figure S5). In the fall (when SIC spans a full range in our data), normalized surface stress increases significantly with SIC as the presence of sea ice introduces additional drag, reaching a distinct peak at $\sim 80\%$ SIC before declining with further increases in SIC. This is consistent with the pan-Arctic analysis of Martin et al. (2014) using the Pan-Arctic Ice-Ocean Modeling and Assimilation System (PIOMAS). These authors demonstrated that momentum transfer into the upper ocean is optimized at intermediate ice concentrations (~80–90\%). At higher concentrations (>90\%), internal ice stress within the consolidated pack absorbs atmospheric momentum and inhibits ice drift."

We add text in the summary and discussion as follows:

"A notable finding of this study is the observational consistency in the CCB with the pan-Arctic finding of Martin et al. (2014) of an optimal ice concentration for momentum transfer. Instead of surface stress increasing monotonically with ice loss or wind speed, momentum transfer peaks at intermediate ice cover (~80\% SIC) where under-ice drag is significant while internal ice stresses are not sufficiently large. As multiyear ice continues to transition into a thinner, more fractured seasonal pack, the Canada Basin will be more often characterized by ice concentrations within this optimal stress window."

There was not an obvious place to point to this peripheral finding in the abstract without detracting from the main results.

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 (Muilwijk et al 2024).

Thank you for this suggestion. Our revised text is as follows:

“Our observation that the largest surface stress increases occur during late fall and winter (e.g., $\sim 0.013 \pm 0.009\text{ N m}^{-2}\text{ decade}^{-1}$ in November–December) aligns directly with both regional modeling and global climate projections. Regionally, Martin et al. (2014) demonstrated that simulated Arctic surface stress exhibits its strongest decadal increases during October–December ($0.007\text{ N m}^{-2}\text{ decade}^{-1}$ over 1979–2012) due to a declining internal ice interaction force. On a pan-Arctic scale, CMIP6 climate projections (Muilwijk et al., 2024) similarly show that surface stress increases most rapidly during winter (+5.1\% per decade), where reduced internal ice stress amplifies wind-driven momentum transfer into the ocean. Our 20-year observational record in the CCB confirms that this fall/winter momentum transfer amplification is already occurring.”

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.

We have tried to emphasize this point in the revised text (described here) and have also highlighted it in the first paragraph of the summary as follows:

"Because wind speeds exhibit no statistically significant trend over 2003–2023, the observed decadal increase in surface ocean stress is primarily attributed to enhanced sea-ice drift speeds and reduced internal ice stress within a thinner, less consolidated ice pack. That is, we see increased sea-ice mobility because sea ice has become more responsive to largely unchanged wind forcing …"

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).

We have removed this text.

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.

Thank you for this comment. We chose the maximum buoyancy frequency (N^2_m) method because this avoids the need to select a density threshold, and we use this metric N^2_m directly in our calculations. The two methods yield indistinguishable results in this region of strong stratification. We have added:

“Because the vertical density step across the base of the mixed layer is so pronounced, the calculated mixed-layer depth is insensitive to whether it is defined by the peak density gradient or a density difference threshold (see e.g., Timmermans et al., 2012).”

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.

We have updated the text to clarify that this rate considers all seasons annually:

“From 2003 through 2019, considering profiles across all seasons annually, the CCB mixed layer has deepened at an overall rate of $1.1 \pm 0.1\text{ m year}^{-1}$ (Figure 8e). If we partition this into individual monthly trends, there are statistically significant linear deepening trends in April through December (ranging from $\sim 0.5\text{--}2.0\text{ m year}^{-1}$ depending on the month; Figure S9a), while January through March exhibit no significant trend.”

L283 I am not sure I completely understand why large N2 values coinciding with small Δρ 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.

To clarify this point, we note that there is an apparent contradiction because a large density difference ($\Delta\rho$) coincides with a relatively small peak buoyancy frequency (N^2_m). This is because N^2_m measures the local vertical gradient ($N^2 \approx \frac{g}{\rho_0} \frac{\Delta \rho}{\Delta z}$) rather than just the total density step ($\Delta\rho$).

We have revised the text as follows:

"However, there is insufficient mechanical energy to sharpen the mixed-layer base via scouring, allowing the density transition zone to broaden vertically. This thickened transition layer reduces the local vertical density gradient, resulting in relatively modest N^2_m values despite the large overall $\Delta\rho$."

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).

Thank you for the comment. We have added two additional sentences to this paragraph as follows:

This indicates that surface ocean stresses dominate mixed-layer deepening during this time, as buoyancy forcing (i.e., sea-ice growth and brine rejection) would be expected to result in a more diffuse (thicker) density interface. From June--August, $\Delta\rho$ increases with the onset of the melt season and the mixed layer shoals (Figure \ref{ccb_ml_seasonal}a,c). **However, there is insufficient mechanical energy to sharpen the mixed-layer base via scouring, allowing the density transition zone to broaden vertically. This thickened transition layer reduces the local vertical density gradient, resulting in relatively modest N^2_m values despite the large overall $\Delta\rho$.**

We found it too cluttered to add additional information to Figure 11, but hope that it is clear how the grey bars relate to the sum of the buoyancy and stress contributions from the parameterization.

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.

Thanks for the suggestion. After testing this, we prefer to retain the monthly climatology format in Figure 11 rather than adding a full time series. It is significantly more cluttered, and the interannual variability and trends are covered elsewhere.

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

We have added text to contrast our results with the pan-Arctic study of Peralta-Ferriz & Woodgate (2015), and others:

“Our results confirm previous observational and modeling studies in the Canada Basin (e.g., Toole et al., 2010; Peralta-Ferriz & Woodgate, 2015; Cole et al., 2017; Zhong et al., 2025) demonstrating that surface momentum transfer plays a larger role in seasonal mixed-layer deepening than convective brine rejection. However, our finding that wind-driven entrainment dominates mixed-layer deepening may be more applicable in general to the strongly-stratified Canada Basin. Consistent with this interpretation, Peralta-Ferriz and Woodgate (2015) identified the Beaufort Gyre and Western Arctic as exceptions to the convective deepening characteristic of the Eurasian Arctic. In the Eurasian sector, weaker halocline stratification allows for convection from sea-ice growth and brine rejection to drive a larger fraction of winter mixed-layer deepening (e.g., Peralta-Ferriz & Woodgate, 2015; Fer et al., 2017; Meyer et al., 2017). However, it remains the case that sea-ice growth plays a small role in seasonal mixed-layer salinification in both basins; Fer et al. (2017) demonstrated in the Eurasian sector that entrainment from below accounts for ~90\% of mixed-layer salinity increases during winter, with sea-ice freezing contributing only ~10\%.”

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.

Thanks for this important point. We have modified the sentence in question as detailed in the previous comment.

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.

Thank you for emphasizing the importance of the key result. We have modified the abstract as follows: "This is ~20\% of the observed…" to "However, this is only ~20\% of the observed…". In addition, we have added the following text immediately after where the 20\% is first mentioned:

“While multi-decadal acceleration in surface stress accounts for $\sim$20\% of the observed winter mixed-layer deepening trend over 2003–2019 ($\sim 0.15\text{ m year}^{-1}$ of the observed $\sim 0.7\text{ m yr}^{-1}$ trend in November–December; Figure 11), the remaining $\sim$80\% reflects processes that cannot be represented by a 1D model. Most notably, surface stress curl forcing drives regional convergence (ΔH^{Ek}), which accounts for $\sim 21\%$ of seasonal winter deepening (Figure S10d). The remaining trend discrepancy is attributable to processes such as lateral advection, including submesoscale flows. These results demonstrate that while local surface stress supplies the kinetic energy for seasonal entrainment, long-term decadal mixed-layer deepening in the Canada Basin is governed predominantly by wind-stress curl dynamics and lateral advection.”

We appreciate this point with respect to the paper citation, thank you, and have corrected the lines in question as follows (and added additional context as described above):

“Consistent with this interpretation, Peralta-Ferriz and Woodgate (2015) identified the Beaufort Gyre and Western Arctic as exceptions to the convective deepening characteristic of the Eurasian Arctic.”

Finally, we prefer to keep Figure S10 in the supplemental rather than including it in the main paper. This figure only serves the purpose of demonstrating that the terms in the final figure of the main paper (Figure 11) do not exhibit significant interannual variability.

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.

We have relocated this discussion to the end of section 4.3 with new text that reads:

"While multi-decadal acceleration in surface stress accounts for $\sim$20\% of the observed winter mixed-layer deepening trend over 2003--2019 (~ 0.15~\unit{m\, year^{-1}} of the observed ~ 0.7~\unit{m\, year^{-1}} trend in November--December; Figure \ref{stress_ml_deep}), the remaining $\sim$80\% reflects processes that cannot be represented by a 1D model. Most notably, surface stress curl forcing drives regional convergence (ΔH^{Ek}), which accounts for $\sim 21\%$ of seasonal winter deepening (Figure S10d). The remaining trend discrepancy is attributable to processes such as lateral advection, including submesoscale flows. These results demonstrate that while local surface stress supplies the kinetic energy for seasonal entrainment, long-term decadal mixed-layer deepening in the Canada Basin is governed predominantly by wind-stress curl dynamics and lateral advection."

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

We have edited this paragraph as noted elsewhere here, so this comment no longer applies.

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.

Thank you, we have revised emphasized this point earlier as noted above and amended the lines in the summary as follows:

“While interannual increases in surface stress account for $\sim$20\% of the observed winter mixed-layer deepening over 2003–2019, the remaining $\sim$80\% reflects non-1D dynamical processes, such as Ekman convergence. That is, while local surface stress supplies the kinetic energy for seasonal entrainment, long-term decadal mixed-layer deepening in the Canada Basin is governed predominantly by lateral processes.”

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.

We have added the following additional discussion:

“Global climate model projections present contrasting future scenarios for Arctic winds and momentum input. On one hand, multi-model climate ensembles (e.g., CMIP5 and CMIP6; Vavrus & Alkama, 2022; Muilwijk et al., 2024) project overall strengthening of surface Arctic winds. In these simulations, increasing surface winds combine with reduced internal ice stress in a thinner, more mobile ice pack to amplify momentum flux into the ocean (Muilwijk et al., 2024). On the other hand, regional model studies and localized atmospheric circulation shifts suggest scenarios where surface winds over specific Arctic sub-basins may weaken (e.g., Athanase et al., 2025). Ultimately, how future Arctic atmosphere–ice–ocean momentum transfer will evolve remains subject to considerable uncertainty, depending on the trade-off between shifting wind forcing, evolving sea ice, and regional upper-ocean stratification.”

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.

We prefer not to venture here as we have a sufficiently large number of measurements to expand the regional scope of the study.

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.

We hope that our additional text throughout the manuscript has helped to highlight the broader implications, feedbacks and remaining open questions.