Response to reviewer 1

General Comments

I noted the study's limitations as I read through it, but I realised at the end that a significant portion of the paper was devoted to them. I found this very interesting, almost as much as the results. I would keep it, but it would be helpful for the reader to outline the structure of the article at the end of the introduction. Otherwise, I find this article a little too detailed and long; it could have been shortened in places, but I don't mind keeping this format.

To sum up, my only major concern relates to the protocol, which reuses an earlier simulation by Terhaars et al. (2021). However, this simulation has already been accepted for publication. I have no problem with the use of a previous simulation in principle, but the authors could have taken this opportunity to correct a previously recognised weakness in the simulation of the previous model. Since the authors are willing to acknowledge a long list of limitations, I would unhesitatingly add (1) the very short duration of the start-up protocol, and possibly (2) the absence of permanent burial. They also use a linear relationship to compensate for sensitivity tests (i.e. instantaneous remineralisation). They claim that carrying out a new simulation is out of the question without providing any justification. Do the authors not work in modelling centres?

Although I consider this simulation to have been state-of-the-art five years ago, it would have benefited from a few updates. This is all the more true given that the authors do not present any control simulations to document the model's drift. This makes the quantitative figures regarding the contribution of terrestrial inputs rather unreliable. I understand the costs involved in running the models, and I do not wish to prevent the publication of this excellent article, particularly as it has been written by a young researcher at the start of their career. However, I believe this to be a major shortcoming of this study. The authors should, at least, acknowledge this, or, ideally, carry out a longer simulation and/or provide a model drift analysis (perhaps for the next study).

Response: We thank the reviewer for their thoughtful and constructive comments. We have extracted the individual concerns raised in the general comments and address them point-by-point below; the replies to the remaining specific comments follow afterwards.

Outline at the end of the introduction As suggested by the reviewer, we have added a paragraph to the end of Section 1 outlining the structure of the article: "The remainder of this paper is structured as follows: Section 2 describes the model and simulation design. Section 3 presents the simulated mean state of Ω_{Ar} in the Arctic Ocean and quantifies the effects of terrigenous inputs on its annual mean (Section 3.2), depth structure (Section 3.2.2), and seasonal cycle (Section 3.3). Section 4 discusses our results in the context of the existing literature, paying particular attention to how sensitive our results are to: (i) the prescribed river inputs, (ii) the recently revised estimates of coastal erosion fluxes, and (iii) the assumptions associated with how terrestrial inputs are represented in the model. Section 4.4 outlines the implications of changing terrigenous inputs for future Arctic Ocean acidification, and Section 5 summarises our conclusions".

Article length and level of detail Given that other reviewer comments encourage further ad-

ditions (e.g. additional caveats, a clearer description of the spin-up, etc.), we decided to retain the current structure rather than tighten it further, but we have streamlined individual passages where possible.

Reuse of the Terhaar et al. (2021) simulations New simulations incorporating, for example, updated erosion fluxes (Irrgang et al., 2025; Vonk et al., 2025), or a different treatment of organic matter lability, may overcome several of the limitations identified in this study. However, we were unable to perform such additional simulations for several practical reasons. First, these high-resolution simulations are computationally very demanding, and rerunning the full simulation framework would have required a substantial amount of HPC resources and machine time. Second, computing resources at IPSL, with which the initial simulations were performed, have been heavily mobilized during 2025–2026 for the IPSL contribution to the CMIP7 FastTrack exercise, including the production of a large ensemble of coupled climate simulations, which considerably limited the availability of resources for additional sensitivity experiments. Third, IPSL is currently implementing a broader policy aimed at reducing the environmental footprint associated with large-scale numerical simulations. Within this framework, the reuse and further exploitation of existing simulations, even when imperfect, is encouraged whenever scientifically justified, in order to limit unnecessary computational costs and associated carbon emissions. For these reasons, we decided to continue exploiting the Terhaar et al. (2021) simulations while explicitly acknowledging their limitations and discussing their implications for the quantitative interpretation of our results. We have expanded the relevant paragraph in Section 4.2 to clarify this rationale and to better acknowledge the limitations associated with the absence of additional control or sensitivity simulations.

Permanent burial We believe that there has been a misunderstanding due to a too short description of the model. The model does simulate permanent burial of alkalinity, carbon, and nutrients. However, it is not a true sediment model but a budget-balancing approach. The global burial of alkalinity is scaled to the external alkalinity input from rivers to prevent drift in the alkalinity budget. Within each bottom grid cell, the amount of alkalinity buried is proportional to the local $CaCO_3$ particle flux relative to the global total $CaCO_3$ flux across all bottom grid cells. Since the burial of $CaCO_3$ drives both alkalinity and DIC loss, and because $CaCO_3$ dissolution releases two equivalents of alkalinity per mole of DIC (following carbonate chemistry), DIC burial is half that of alkalinity on a molar basis. Organic matter is also permanently buried in the sediments, with the total burial scaled to the river input of nitrogen to keep the nitrogen budget stable. Within each bottom grid cell, the amount of organic matter buried is proportional to the combined local flux of small and large particulate organic matter relative to the global total of these fluxes across all bottom grid cells. Since PISCES uses fixed Redfield stoichiometry for organic matter, carbon and phosphorus are buried in proportion to nitrogen. We will add a clearer explanation to the Methods section and note the use of a budget-balancing approach rather than a dynamic sediment model as a caveat in the Discussion.

Spin-up The simulations used here (with more realistic carbon and nutrient inputs from rivers and coastal erosion) were initialised in 1990 from the spun-up model state of Terhaar et al. (2019), which itself comprises a 50-year spin-up from rest in ORCA05 NEMO–PISCES followed by an 1870–1957 integration with looped DRAKKAR forcing and an interpolation to ORCA025 in 1958; the full spin-up procedure is now described in the revised Section 2.3. By 1990, any drift in the seasonal mixed layer and upper halocline of the central Arctic basin (with a ventilation timescale of about 20 years; Kipp et al., 2019) should have been removed, and even the anthropogenic carbon budget in the deep Arctic Ocean had already become

substantially different from the parent ORCA05 configuration (Terhaar et al., 2019). We deliberately branched our three simulations in 1990 to allow approximately 15 years between the introduction of the new terrigenous forcing and the start of the analysis window in 2005. Because our analyses focus mostly on shelf waters with residence times of 1–6 years (Bauch et al., 2009; Schlosser et al., 1994), this 15-year transition is approximately three times the maximum shelf residence time and represents the best compromise between a sufficiently long adjustment period and computational cost. The same period has been shown to be sufficient for net primary production to reach a new equilibrium under terrigenous forcing (Terhaar et al., 2021). We will add these considerations and a careful quantification of any remaining drift from the 1990 forcing change to Section 2.3 (or alternatively to section 4.3).

Model drift and absence of control simulations Although we did not run a parallel control simulation for the period 1990–2010, we are confident that any model drift would not affect our quantitative results or conclusions for the following reasons. Two types of drift are relevant here. The first type of drift concerns the adjustment to the change in terrigenous inputs introduced in 1990. As described above, any remaining drift from that transition is negligible in the regions and variables analysed. The second type of drift concerns the underlying model drift associated with the spin-up. However, our analysis focuses on differences between three simulations that share identical initial conditions and physical forcing, with only the terrigenous forcing differing. Therefore, any model drift unrelated to terrigenous inputs would affect all three runs equally and would cancel out in the difference fields that we use to quantify the terrestrial contribution. Furthermore, we can provide evidence that this residual drift is small by using a pre-industrial DIC tracer carried in the reference simulation from which our runs were branched. This tracer evolves under a changing climate, but it is insensitive to rising atmospheric CO_2, so it tracks the natural carbon pool and serves as an independent indicator of drift. The air-sea CO_2 flux of that tracer remains constant until warming and sea ice loss accelerate around 1990, at which point it shows a slight trend towards carbon uptake, possibly due to sea ice loss. We will add a supplementary figure showing the evolution of this tracer to support the discussion of drift in Section 2.3.

Comment 1

Line 92: This article lies on the important assumption that coastal erosion does not deliver Alkalinity. I think this is a decent assumption that coastal erosion mostly delivers OM. However, could they justify this with either a short demonstration and/or backing up with literature? Permafrost also encapsulate minerals that can undergo weathering.

Response:

We thank the reviewer for raising this point. The assumption that coastal erosion supplies negligible alkalinity requires explicit justification, rather than simply being asserted. We have therefore substantially revised the relevant section of the manuscript (see the proposed text below).

To our knowledge, no spatially resolved, pan-Arctic estimate of the input of alkalinity from coastal erosion currently exists. The prevailing view in Arctic land–ocean carbon literature treats erosion as primarily a flux of organic matter (Lantuit et al., 2012; Wegner et al., 2015; Vonk et al., 2012; Fritz et al., 2017; Nielsen et al., 2022), with the inorganic component generally neglected. We acknowledge that this prevailing view may be partly a consequence of research priorities (Lantuit et al., 2012), in which case the assumption that erosion supplies little alkalinity is more of a provisional assumption than an evidenced result.

That said, recent work has begun to challenge this approximation. Scholz et al. (2025) present geochemical evidence from the southwestern Baltic Sea showing that fine-grained glacial till mobilised by coastal erosion undergoes rapid seafloor weathering, releasing approximately 4.89 meq of alkalinity per gram of till eroded. This mechanism is primarily driven by carbonate dissolution at the seafloor and could, in principle, operate on any till-rich coastline. To provide an approximate estimate of the maximum potential magnitude of this process across the pan-Arctic, we have conducted a scoping calculation.

To do so, we combined the Arctic Coastal Dynamics Database (ACD; Lantuit et al., 2012) with the Global Unconsolidated Sediments Map Database v1.0 (GUM; Börker et al., 2018) to identify Arctic coastal segments underlain by glacial till and compute the corresponding mineral mass flux from coastal erosion. Both datasets distribute their data as polygons. To extract the till-associated coastline, we converted the ACD polygons into boundary lines and intersected these with the GUM-Gt (= glacial till) polygons, buffered by 1 km. We then computed the flux as follows: flux = retreat rate $\times$ cliff height $\times$ (1 $-$ ice fraction) $\times$ bulk density $\times$ clipped boundary length. The 1 km buffer is small enough to capture only the landward-facing boundary of each ACD polygon within a till area, meaning no perimeter-doubling correction is necessary. This procedure yields a pan-Arctic, till-derived bulk sediment flux of $\sim$55 Tg yr^{-1}.

We assessed the methodological uncertainty of this calculation using two alternative approaches. A 5 km buffer intersection (with a perimeter-doubling correction to account for the buffer reaching across ACD polygons to both sides) yields $\sim$103 Tg yr^{-1}. However, the 5 km buffer extends well beyond the till polygon edges, into adjacent non-till coastlines, which biases the result upwards. A naïve spatial join, attributing the full flux of any ACD polygon whose boundary intersects a GUM–Gt polygon, yields $\sim$146 Tg yr^{-1} as a loose upper bound, ignoring the patchiness of till coverage along long ACD segments. Therefore, the 1-km buffer intersection-based estimate is taken as our primary value, with the 103 and 146 Tg yr^{-1} estimates serving as sensitivity bounds.

Of this flux, the silt+clay fraction (i.e. the size class relevant to the seafloor weathering mechanism

of Scholz et al., 2025), is approximately 30–70% (Cao et al., 2015; Haldorsen, 1981; Vorren, 1977), giving ∼16–38 Tg yr^{-1} of fine-grained till. Applying the yield of 4.89 meq g^{-1} from Scholz et al. (2025) gives an estimated pan-Arctic alkalinity flux of ∼0.08–0.19 Teq yr^{-1}, or ∼5–11% of the pan-Arctic riverine alkalinity flux of 1.76 Teq yr^{-1}.

This estimate has significant uncertainty, largely due to regional variability in till mineralogy. Tills derived from carbonate-rich sources may yield alkalinity at rates comparable to those of Scholz et al. (2025)'s Baltic till. In contrast, tills from purely crystalline sources are likely to yield substantially less, as aluminosilicate weathering is largely cancelled out by reverse weathering (Scholz et al., 2025). Additional shelf-process variables, including rates of organic matter degradation, sediment resuspension and seasonal hypoxia, further modulate the realised yield in ways that cannot be constrained by the Kiel Bight observations at the pan-Arctic scale (Scholz et al., 2025). We therefore consider the range of ∼0.08–0.19 Teq yr^{-1} to be an upper bound on the alkalinity flux from till-derived coastal erosion.

We compare this with the co-delivered organic carbon flux. Carbonate dissolution adds 1 eq alkalinity and 0.5 mol DIC per equivalent, resulting in a net [TA-DIC] gain of ∼0.5 mol per equivalent of alkalinity. In contrast, aerobic OC degradation adds 1 mol DIC per mol C without compensating alkalinity. The upper-bound alkalinity flux therefore corresponds to an increase in [TA-DIC] of ∼0.04–0.09 Tmol yr^{-1}, while the OC degradation term (1.28 Tmol C yr^{-1}) corresponds to a decrease in [TA-DIC] of up to ∼1.28 Tmol yr^{-1}, approximately 14–30 times larger in magnitude (and of opposite sign). As the effect of organic carbon thus dominates, and as any prescribed alkalinity flux would be rather arbitrary given the large uncertainties, we believe that simplifying coastal erosion to deliver no alkalinity is justifiable.

We now provide explicit justification in the main text: "Coastal erosion of glacial deposits can release alkalinity into seawater through the rapid weathering of fine-grained glacial till on the seafloor, primarily via carbonate dissolution (Scholz et al., 2025). This process has recently been quantified by Scholz et al. (2025) in the Kiel Bight (Baltic Sea), but it has yet to be measured in the Arctic Ocean. To place an upper bound on the potential alkalinity flux derived from coastal erosion on a pan-Arctic scale, we applied the alkalinity yield of 4.89 meq g^{-1} obtained in the Kiel Bight (Scholz et al., 2025) to an estimate of the pan-Arctic fine-grained glacial till flux. We obtained this till flux estimate by combining the Arctic Coastal Dynamics Database (ACD; Lantuit et al., 2012) with the Global Unconsolidated Sediments Map Database (GUM; Börker et al., 2018) through spatial intersection, yielding an erosion-derived pan-Arctic alkalinity flux of about 0.08–0.19 Teq yr^{-1}. This flux corresponds to approximately 5–11% of the riverine alkalinity flux (1.76 Teq yr^{-1}; Table 1) and would have an effect on [TA-DIC] that is 14–30 times smaller than that produced by the co-delivered organic carbon flux from erosion upon degradation (1.28 Tmol C yr^{-1}; Table 1), disregarding CO_2 outgassing. The range of 0.08–0.19 Teq yr^{-1} is likely an upper bound. The Kiel Bight yield reflects a particular sedimentary and biogeochemical setting that may not be representative of the Arctic as a whole. The carbonate content of glacial tills depends on the type of bedrock crossed by the parent ice sheet, while the realised weathering yield depends on local rates of organic matter degradation, sediment resuspension and seasonal hypoxia. Therefore, the pan-Arctic per-gram alkalinity yield cannot simply be constrained by observations from the Baltic Sea (Scholz et al., 2025). Given the substantial uncertainties associated with the alkalinity flux and its modest magnitude relative to the co-delivered organic carbon term, we neglect any potential erosion-derived alkalinity flux in our simulations".

Comment 2

Line 144: When a terrigenous DOC with specific lability is not available in a model, I understand that one should come with some assumption. And I totally agree with the claim that riverine matter contains an important share of recalcitrant DOC. However, I do not understand why the other share is considered labile (sent to DIC directly) and not to (semi-labile) DOC. I could eventually understand the instant remineralization of fresh OM from coastal erosion permafrost, but not quite this one. NOTE: I see that the authors discuss this issue later in the discussion and are constrained by the fixed stoichiometry of the model. At least, a sensitivity test would have been appreciated. Maybe next time.

Response:

The reviewer is correct that adding the bioavailable riverine DOC fraction to the semi-labile DOM pool would have been a more realistic representation. As the reviewer notes, this was not possible in our PISCES configuration because the semi-labile DOM pool has a fixed marine C:N:P stoichiometry that differs substantially from that of terrigenous DOM; routing the bioavailable fraction there would have distorted the terrigenous nutrient ratios. We agree that a sensitivity test on this partitioning would have been valuable and should be a target for future work using a model framework with multiple organic-matter pools and variable stoichiometry.

Comment 3

Line 181: "aligns well" seems exaggerated. Since most of the values are exceeding 2 in glodap (Fig. 1 & S2), it is hard to know what are the bias between the model and the observations, Although I acknowledge the general pattern is here. I would eventually start by acknowledging the strong biases in the inflowing Atlantic and Pacific waters.

Response:

Following the reviewers suggestions, we have rewritten the opening of this paragraph to (i) describe the model–observation comparison as "broadly consistent" rather than "aligning well," (ii) acknowledge the inflow bias up front, and (iii) restructure the subsequent regional comparison accordingly. The revised paragraph reads as follows: "The simulated spatial pattern of $\Omega_{\mathrm{Ar}}^{\mathrm{Surf}}$ is broadly consistent with observations from GLODAPv2.2023 (Fig. 1a-b). Both show the highest values in the inflowing waters from the Pacific and Atlantic Oceans and the lowest values in the LESS and Kara Sea (Fig. 2a). Yet the model underestimates $\Omega_{\mathrm{Ar}}^{\mathrm{Surf}}$ in the Pacific and Atlantic inflow regions. Observed values frequently exceed 2, whereas simulated values tend to remain closer to 1.5–2. This bias likely originates in the inflowing North Pacific and Atlantic water masses themselves and propagates into the Arctic. A second discrepancy appears in the Beaufort Sea, where observations show high spatial variability in Ω_{Ar}, with no apparent spatial pattern (Fig. 2); a feature that the model fails to replicate [...]".

Comment 4

Figure 2: concept is excellent and extensive but very hard to read and understand rapidly and is also under-exploited to synthetize the outputs of the article. Design should be improved for better readability. Subjective suggestions: One inlet map for summer sea-ice may be enough. Representation of the continental shelf must be more explicit. Increase compacity & remove blank/empty areas. Is the gray circle arrow needed ? Stating the exact months necessary (move to caption) ? Try not to repeat what is not necessary (e.g. seasons in the title for lower panels, etc). Increase fontsize.

Response:

We have updated Figure 2 to address the concerns that were raised.

Comment 5

Line 241: It is nice to see that different models come to the same conclusion (e.g. Polimene et al., Oziel et al.). This article has the nice added value and steps one step further into the complexity of the carbonate system.

Response:

We agree and have added a reference to Polimene et al. (2022), alongside the existing Oziel et al. (2025) citation, to acknowledge the consistent findings across modelling frameworks.

Comment 6

Line 370: Shocking, but transparency really appreciated.

Response:

We thank the reviewer for this comment.

Comment 7

Line 382: I doubt the omega response is linear... is it ? could you justify your approach?

Response:

We have now explicitly tested the carbonate-chemistry component of the linearity assumption using mocsy 2.0 at three representative Arctic background states: the Arctic shelf surface (avg.); a Laptev Sea surface point; and a Laptev Sea subsurface point at ∼24 metres. At each location, we imposed a series of DIC perturbations ranging from 0 to the full simulated, erosion-driven anomaly. The relationship between ΔDIC and $\Delta\Omega_{Ar}$ is approximately linear, with minimal departures from linearity at the surface and less than 7% at depth at the revised-flux scaling factor of ×0.25 (Fig. 1). The carbonate-system response is mildly mildly nonlinear, such that linear downscaling slightly overestimates the magnitude of the residual erosion anomaly (i.e., our rescaled values are conservative with respect to the strength of the remaining acidification signal).

As stated in the main text, this rescaling is intended only as a first-order approximation. Given the substantial remaining uncertainty in the revised erosion flux estimates themselves (Vonk et al., 2025), which likely exceeds the error introduced by the linearity assumption, we consider linear scaling an appropriate approach for a first-order assessment of how a lower erosion flux would affect our results, under the assumption that a proportional change in the prescribed erosion flux leads to a proportional change in the local concentrations of DIC and nutrients within the model. The latter assumption is supported by Terhaar et al. (2021), who demonstrated the approximate linearity of net primary production relative to terrigenous nutrient inputs in the same simulations.

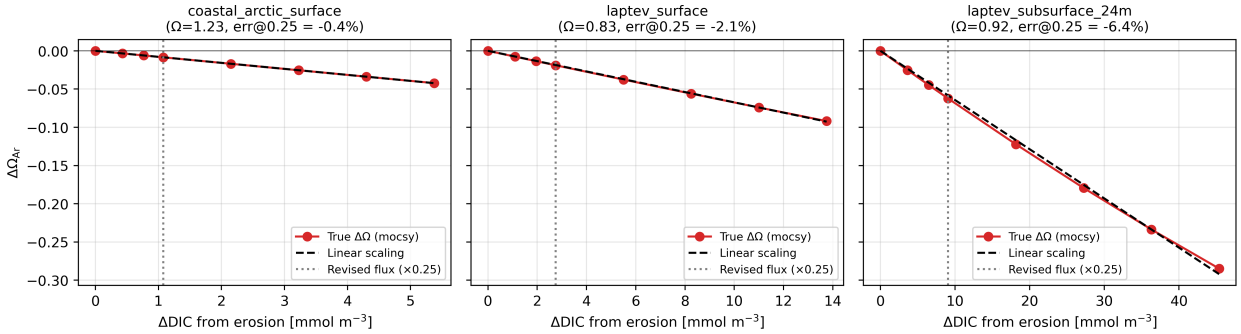

Figure 1: Test of the carbonate-chemistry component of the linearity assumption underlying the rescaling of erosion-driven $\Delta\Omega_{Ar}$. Each panel shows the simulated relationship between an imposed DIC perturbation (ΔDIC) and the resulting change in $\Delta\Omega_{Ar}$, computed with mocsy 2.0 at three representative Arctic background states: the area-averaged Arctic shelf surface, a Laptev Sea surface point (74.94°N, 119.95°E) and the corresponding subsurface point at ∼24 m. Background state values (T, S, TA, DIC, PO_4, Si) are taken from the no-erosion scenario averaged over 2005–2010. Red circles show the true $\Delta\Omega_{Ar}$ response from mocsy under a closed-system assumption (no air–sea gas exchange), with TA held constant and DIC increased to mimic the DIC perturbation due to erosion inputs. The percentage error reported in each panel quantifies the departure of the linear extrapolation from the true mocsy response at the revised-flux scaling factor of ×0.25.

Comment 8

Line 392: why aren't you able to do new runs? Don't you have HPC resources in your institutions (e.g. IPSL)? I find it a bit easy.

Response:

We addressed this concern in our response to the reviewer's general comments above.

Comment 9

Line 497: Soil DOC is labile yes, but not necessarily riverine DOC which experienced a lot of degradation before reaching the ocean and is expected to be less labile than oceanic DOC.

Response:

The bioavailable fraction of 50 ± 30% for riverine DOC that we cite is supported by multiple independent observation-based methodologies. Holmes et al. (2008) found that 20–40% of riverine DOC was lost during three-month incubations of samples collected during the spring freshet on three Alaskan rivers, when most of the annual DOC flux occurs. Kaiser et al. (2017) estimated from lignin phenol and CDOM measurements along the Eurasian shelves that approximately 50% of the annual riverine tDOC discharge is mineralised in estuaries and on Eurasian shelves. Thibodeau et al. (2017) inferred from nitrogen isotopes that 62–76% of the riverine dissolved organic nitrogen is removed within the Laptev Sea shelf. Modelling studies further support this: Fransner et al. (2016) demonstrated in the Baltic Sea that treating riverine DOC as fully refractory yields modelled concentrations up to five times higher than observed, and that observed concentrations can only be reproduced by partitioning riverine DOC into a labile pool (80%) with a remineralisation timescale of order one year and a refractory pool (20%); Fransner et al. (2019) confirmed this partitioning is required to reproduce observed CO_2 supersaturation in coastal waters. The studies therefore converge on a substantial labile fraction in riverine terrigenous organic matter, supporting our 50 ± 30% estimate.

We have taken the opportunity to clarify a potential ambiguity in the original wording. Our reference to "aquatic DOC" was intended to mean riverine DOC. We have rewritten this sentence to avoid potential misunderstandings: "Observation-based estimates further suggest that the bioavailability of organic nutrients differs between sources (Terhaar et al., 2021): $80 \pm 10\%$ for coastal erosion inputs (Sanchez Garcia et al., 2011; Bruchert et al., 2018) and $50 \pm 30\%$ for riverine inputs (Holmes et al., 2008; Kaiser et al., 2017; Thibodeau et al., 2017); consistent with the finding that soil DOC is generally more labile than riverine DOC, which has typically undergone substantial in-transit degradation (Vonk et al., 2015).

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