Response to Reviewer 1:
We would like to thank the reviewer for the thoughtful comments and constructive feedback. Below, reviewer comments (RC) are followed by our responses (AC).

RC: In general the manuscript is nicely written and describes the nudging, however I could wish for a consideration of which data, experiments etc. should be in the manuscript. For instance in figure 4 and 5 is it necessary to show all different experiments with different relaxation? Not all are in focus of the manuscript. I think that it would make the points more clear.
AC: The purpose of presenting all relaxation experiments was to demonstrate how the system responds to varying relaxation time scales and, consequently, to verify the correct implementation of the relaxation algorithm in SIS2. A physically and numerically well-behaved relaxation scheme should exhibit a smooth and monotonic dependence of the solution on the relaxation time scale. As the relaxation becomes weaker (i.e., as the relaxation time scale, τ, increases), the solution should gradually approach the free-running model state without abrupt transitions or unexpected changes in sea ice conditions within the relaxation region. We believed that showing the sea ice thickness and concentration fields for all relaxation experiments helped illustrate this behavior.
Nevertheless, we agree with the reviewer's suggestion and have removed two intermediate relaxation experiments from Figures 4, 5, 9, and 10 to reduce redundancy. Results from all relaxation experiments are still retained in the remaining figures, where they provide a more compact quantitative assessment of model sensitivity to the relaxation strength. We have also revised the text to better describe the behavior of the omitted experiments, which was insufficiently discussed in the previous version.

RC: Redistribution. I am unsure why the model becomes unstable if all sea ice is put in the target category. Yes if the concentrations becomes very low as mentioned.
AC: SIS2 is technically able to start from a single-category ice thickness distribution (ITD), in which all ice area and volume are assigned to the primary thickness category, followed by redistribution within the model. In our tests, this approach generally performs adequately and does not lead to issues in most cases. However, we have occasionally encountered instances where a single-category initialization led to numerical failure during the early-stage redistribution step. We have not diagnosed the precise cause of these rare instabilities, but they appear to occur during the initial adjustment of the ITD when the model attempts to redistribute ice area and volume from a highly singular distribution. By contrast, initializing the ice state with a non-degenerate, category-resolved ITD (distributing ice area and thickness across multiple categories with small but non-zero values) has not exhibited such behavior in our experiments. For this reason, a category-resolved initialization was adopted as a practical robustness measure. The manuscript text has been revised to avoid implying that a single-category initialization is inherently unstable and instead reflects the observed behavior in our experiments.

RC: Secondly (step iv). How can one conserve volume and area when moving ice from the target category to thinner categories? This means that the ice thickness within each thickness category is reduced

AC: The goal of the redistribution algorithm is to conserve the aggregated ice partial area (eq. (7)) and the aggregated ice volume per unit grid-cell area (eq. (8)), i.e., to preserve the target ice concentration and mean ice thickness provided by the observational or reanalysis data (PIOMAS in this study). The redistribution is performed only within the ice thickness distribution and therefore does not alter these bulk quantities.

It is correct that the ice volume associated with individual thickness categories changes during the redistribution. When ice area and volume are transferred from the primary target category to neighboring categories, the category-wise ice concentrations and mean thicknesses are modified. However, the redistribution is constructed such that the sum of ice area over all categories and the sum of ice volume over all categories remain unchanged. Thus, conservation applies to the aggregated ice concentration and volume, not to the ice volume within each individual category. The manuscript text has been clarified.

RC: The introduction describes mismatch between ocean, sea ice and atmosphere and yet the authors have chosen to use two different products for the ocean and the sea ice. With the choice of the Glory's product for the ocean, ERA5 for the atmosphere and PIOMAS for the sea ice it seems as if the experiments have been unnecessary complicated as Glory's is forced by ERA5, and it contains a sea ice model which could have been used as the reference data instead of PIOMAS. Maybe the authors can elaborate on this? Rerunning the experiments seems to be beyond the scope.

AC: We agree that a more internally consistent experimental design could have been achieved by using sea ice fields from GLORYS together with ERA5 atmospheric forcing. The discussion in the Introduction primarily concerns challenges associated with lateral boundary conditions in regional sea ice models, including potential inconsistencies between prescribed boundary conditions and the evolving interior ice state. Similar considerations also apply to relaxation experiments, where inconsistencies between the target sea ice state and the atmospheric or oceanic forcing may affect the model response.

In the experiments presented here, the atmospheric forcing was prescribed independently of the target sea ice fields. The NEP10k experiments were forced with ERA5 atmospheric fields, while the ARC10k experiments used JRA-55 forcing. The target sea ice state was obtained from PIOMAS, which is forced by NCEP/NCAR Reanalysis 1. For the NEP10k hindcast experiments, GLORYS ocean fields were used, whereas the ARC10k experiments did not employ GLORYS forcing.

The choice of PIOMAS was motivated primarily by its extensive validation and widespread use in studies of Arctic sea ice thickness and volume. In addition, these experiments were conducted as part of a broader system-development effort, and the experimental design was intended to remain consistent with existing hindcast and retrospective forecast configurations, particularly

with respect to atmospheric forcing. This limited the flexibility to redesign all components of the experimental setup.
We agree that using GLORYS sea ice fields would provide a more consistent combination of oceanic, atmospheric, and sea ice forcing for the NEP10k experiments. However, the primary objective of this study is to describe and evaluate the implementation of the sea ice relaxation algorithm in SIS2 rather than to assess the sensitivity of the results to a particular choice of forcing or reference product. The application of the relaxation procedure with independently derived target fields suggests that the method is sufficiently robust to accommodate realistic inconsistencies among data sources. Future hindcast applications of the NEP10k system will use GLORYS sea ice fields to ensure greater consistency between the prescribed oceanic and sea ice states.
Clarification on the choice of forcing fields in the experiments has been added to section 5 (Numerical simulations) and Section 9 (Discussion).

RC: Another topic that could be elaborated a bit more on is the limitations of using observations or forcing that do not contain all the fields that are required to constrain e.g the dynamics as this is often the case when applying nudging or other methods to assimilate/add lateral boundary conditions into the model.
AC: A discussion of the limitations associated with constraining only a subset of the sea ice state variables has been added to Section 9 (Discussion).

RC: Line 40: Duarte et al implemented this in CICE V5. Strictly speaking it is not available in the main branch of CICE6 however a nesting module should soon be included in this. Different implementations do exist but they are outside the main branch.
AC: Thank you for noting this. Corrected.

RC: Line 44 (and the section around) describes the challenges of nudging/boundaries if all parameters are properly described. I think that it is worthwhile to mention that often it is not possible to get for instance stresses for the nudging unless the large-scale model used for the forcing of the regional model originates from the same group (see also comment above).
AC: A sentence has been added to this paragraph noting that only a limited set of sea ice variables is typically available for prescribing boundary conditions, which can hinder the specification of a fully dynamically consistent sea ice state.

RC: Line 108: either list the barotropic and baroclinic timestep or skip the sentence with a baroclinic….
AC: A reference to Table 1 has been added, where the time-step values and definitions are listed.

RC: Line 257: How is salinity and enthalpy changed? Often these parameters are not known within the target values. If they are kept constant they should only be “unrealistic”if ice was not present before? A comment on this would be good.
AC: This is described in section 4.2.3. A clarification describing the “no-ice to ice” case has been added.

RC: Line 420: I would use a different word than error. In general most ice thickness products are associated with a relatively large bias/uncertainty. I would use bias instead.
AC: Here and in other similar places, the term has been corrected to “differences” for consistency with the figure captions.

RC: Line 485: The high impact is likely due to the slow ice thickness evolution. There should be a comment on why
AC: This is discussed in Discussion section.

RC: Line 535: Can you point to a reason. The ocean relaxation?
AC: The unconstrained ARC10k simulation is compared against PIOMAS in this section. The ice edge position in this experiment is determined by the response of the sea ice model to the simulated ocean state (MOM6) and the prescribed atmospheric forcing (JRA-55). The causes of the ice concentration bias along the East Greenland Current (EGC) and in the Barents Sea MIZ were not investigated in this study. Both regions are strongly influenced by ocean heat transport and air-sea interactions. The positive ice concentration bias may therefore suggest that the simulated upper ocean is too cold, either because oceanic heat transport into the ice-edge region is insufficient or because ocean heat loss to the atmosphere is too strong. In the EGC region, sea ice conditions are additionally influenced by ocean dynamical processes, including the Greenland Gyre, the EGC itself, and frontal eddies. The positive sea ice bias in this region may therefore also indicate excessive lateral export of Polar Water or sea ice across the EGC front in ARC10k into the Greenland Sea. A detailed investigation of these biases is beyond the scope of the present study. To maintain focus on the implementation and evaluation of the sea ice relaxation algorithm in SIS2, this speculative discussion has not been added to the manuscript.

RC: Line 560: It is likely that both the atmosphere and the Glorys nudging leads to similar cold biases as Glorys is forced with ERA5. The assimilated sea ice product within ERA5 and GLORYS are the same (OSISAF) whereas PIOMAS uses NSIDC (if I remember correct).
AC: We would like to clarify that the simulations discussed here are forced with JRA-55 atmospheric reanalysis, whereas PIOMAS is based on a different atmospheric forcing (NCEP/NCAR Reanalysis 1) and a different assimilation framework for sea ice and ocean conditions. No GLORYS forcing or relaxation is applied in this experiment. We agree that part of the differences between the ARC10k simulation and PIOMAS may reflect inconsistencies among reanalysis products rather than purely model deficiencies. In particular, differences in

atmospheric forcing characteristics between JRA-55 and NCEP/NCAR may lead to systematic differences in surface heat fluxes and, consequently, sea ice evolution. A colder atmospheric forcing in JRA-55 relative to the NCEP/NCAR Reanalysis 1 forced PIOMAS could contribute to the seasonal ice concentration differences presented in the manuscript. The text has been revised to clarify this point and to avoid over-attribution to a single mechanism.

RC: 645 Year-end. Not sure. Would end of the year be better? If year end is used. The version should be decided. It is year-end here and year end in line 658.
AC: Changed to "by the end of the year".

RC: Line 647 remove s from figures
AC: Corrected

RC: Line 694 The problem often arise when adding "new" ice is to water above the freezing point. I think that the thickness is less important for instability as long as it is not very small but it may or may not influence the freshwater balance.
AC: We agree that imposing ice over ocean water above the freezing point is a common source of thermodynamic inconsistency. More generally, inconsistencies may arise whenever the relaxed ice state is not in equilibrium with the underlying oceanic or atmospheric conditions, including cases where ice is removed under conditions favorable for ice growth. The discussion has been revised accordingly.

RC: Table 1+Table 2: These are nice to know but I would put them in an appendix as most of the information here is not directly necessary for the point of the manuscript.
AC: We appreciate the reviewer's suggestion. While moving Table 1, which summarizes the MOM6 configuration, to an appendix could be considered because the ocean model is not the primary focus of this study, we believe that Table 2 should remain in the main text. The sea ice relaxation algorithm is implemented and evaluated within SIS2, and the sea ice model configuration is therefore directly relevant to the methodology and interpretation of the results. The parameters listed in Table 2 may also serve as a useful reference for readers seeking to reproduce or adapt the experiments. Because the study presents simulations of a coupled MOM6–SIS2 system, we prefer to retain both tables in the main text for completeness and ease of reference. The tables are concise and provide key information about the model configurations used in the experiments without substantially increasing the length of the manuscript.

RC: Table 3: Are all these experiments addressed? I am not entirely sure where relaxation domain and time scale belongs to. Maybe it is the split of the table on two pages but it is a bit difficult to link the headers of each column with content. I think that this can be improved. What is COBALT?

AC: Yes, all these experiments are discussed in the manuscript. A detailed description of all simulations is provided in section 5. In the copy of the manuscript, the table heading got separated and ended up on different pages, which makes it confusing to relate the header with the table columns. This will be avoided in the revised version. COBALT is a biogeochemical component coupled to MOM6-SIS2 used in one of the multidecadal hindcast simulations for assessing the model drift (mentioned in section 2).

RC: Figure 3: For the numbers underneath the colorbar I would reference an equation from the figure text of figure 3c.
AC: We are not certain which equation the reviewer refers to. The colorbar values correspond directly to the relaxation rate, defined as $1/(3600 \times \tau)$, where τ is the prescribed maximum relaxation timescale in hours. Since the figure shows multiple experiments with different prescribed τ values, the colorbar is intentionally annotated with the corresponding numerical rates for clarity. The figure caption has been revised to improve the explanation of this representation.

RC: Figure 4. Last statement, where are the hours shown?
AC: We agree that this may not be immediately obvious from the figure. The relaxation timescale is indicated in the experiment names shown on the left-hand side of the panel. The figure and caption have been revised accordingly, following a previous reviewer's suggestion, to improve clarity.

RC: Figure 4+5: I think that it would be useful to show the PIOMAS as reference.
AC: PIOMAS seasonal ice concentration and ice thickness fields have been added to figures 4,5, 9, and 10.

RC: The Reference run for NEP10k (e.g row 5a) is more an artifact of a setup that is difficult to use for sea ice predictions without boundaries.
AC: We agree with the reviewer. The NEP10k control simulation employs closed sea ice boundaries and therefore is not expected to provide an optimal configuration for sea ice prediction in the Arctic region. It should be noted, however, that the primary forecast region of the NEP10k system lies south of the Bering Strait. Although the closed lateral sea ice boundaries introduce unrealistic ice conditions in the Arctic portion of the domain, their impact on sea ice conditions in the Bering Sea is comparatively limited. The purpose of including this simulation is to provide a baseline against which the boundary and domain relaxation approaches can be evaluated. One of the primary motivations of this study is to investigate whether sea ice relaxation can mitigate the limitations imposed by the closed sea ice boundaries in the NEP10k configuration.