Response to Referee #1

This paper examines the climate effects of Near Term Climate Forcers (NTCFs) using two climate model experiments from CMIP6-AerChemMIP, “historical” and “hist-piNTCF”, which include time-varying and fixed pre-industrial NTCF forcings, respectively. Focus is given to three main climate responses to NTCFs: 1) Arctic-amplified global cooling, 2) increased Labrador Sea convection, and 3) changes in tropical precipitation, including a southward displacement of the ITCZ.

Overall, the paper is clear and well written, and the methodology is sound. However, there are a few occasions throughout where statements are made that are unclear and/or are unsupported by the authors’ results. I discuss these in my specific comments below. Once these comments are addressed, I believe that the paper should be acceptable for publication.

We thank Referee #1 for their thorough and constructive review of our manuscript. We appreciate their positive assessment of the clarity, writing quality, and methodological soundness of our work. We have carefully considered all comments and have made revisions to address the unclear statements and better support our conclusions with the presented results. Each specific comment is addressed individually below, detailing the changes made to the manuscript.

Specific comments:

1) Lines 52-54: Since this paragraph is focused on the ocean, presumably you are talking about ocean meridional circulation and ocean heat transport here?

Indeed, Cowan and Cai (2013) assesses the response of large-scale ocean circulation to aerosols. In these lines we refer to the response of the meridional overturning ocean circulation and its consequent changes in heat transport clarifies the context of their results. We have rewritten them to accurately reflect the Cowan and Cai (2013) findings:
"Cowan and Cai (2013), using a coupled atmosphere-ocean model, reported that non-Asian aerosols dominated the ocean response to global aerosol forcing during the 20th century, delaying the GHG-induced weakening of the meridional overturning circulation and, consistently, increasing the northward heat transport across the equatorial Atlantic."

2) Line 87: Should this be “key metrics”, not “key magnitudes”?

While we agree that the ITCZ latitude and precipitation indexes are better described as "metrics", ocean density and the temperature and salinity contributions to it are more appropriately described as physical magnitudes or diagnostics. To encompass both types of indicators accurately, we have revised the terminology:
“*In the following subsections we describe the selection of model data, the statistical metrics applied, and key diagnostics used to assess NTCF impacts on specific aspects of climate such as ocean density and the ITCZ.*”

3) Table A1 header row: First model should be BCC-ESM1, not BSC-ESM1.

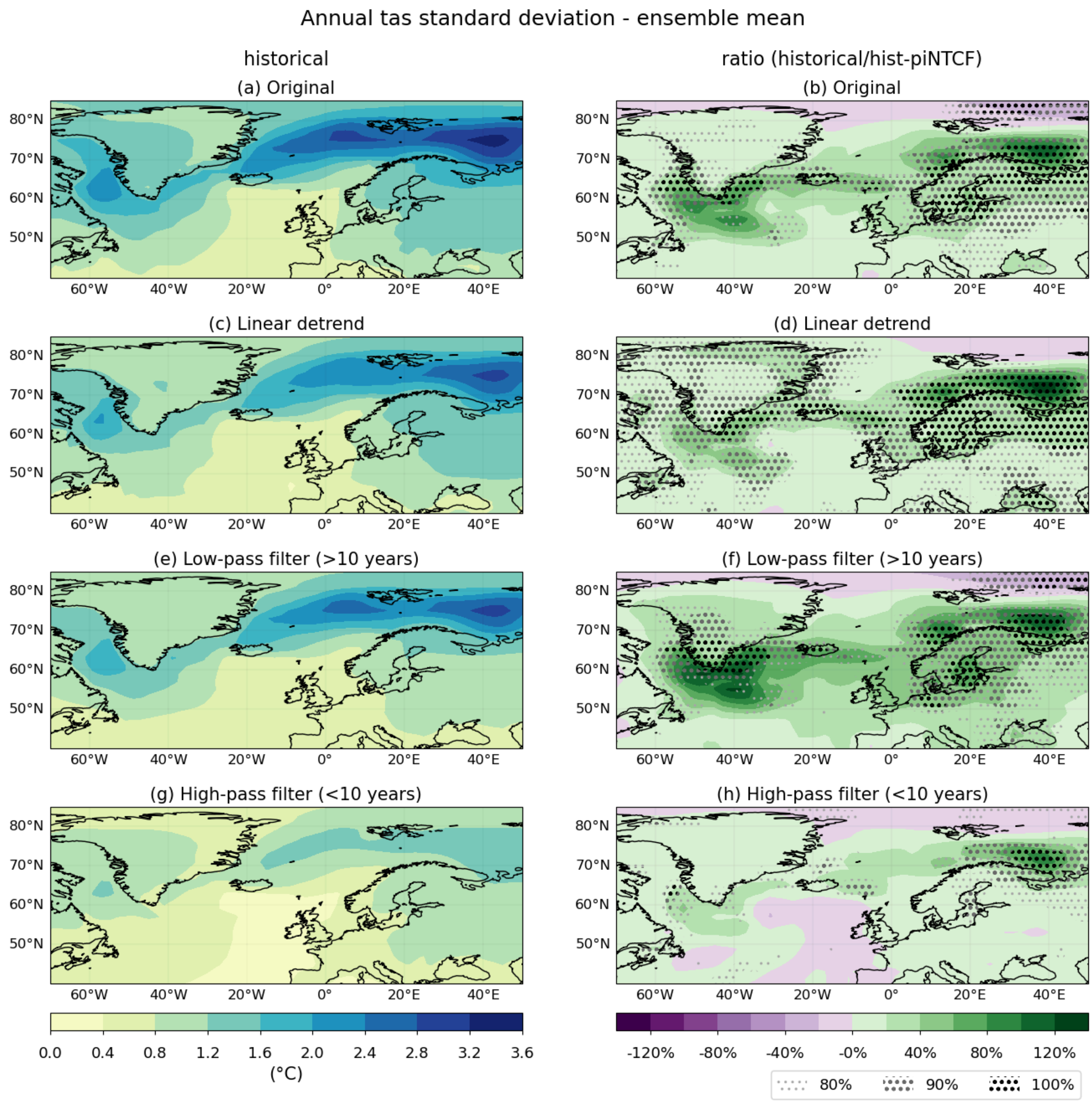
Corrected.

4) Lines 191-193 and Fig. 1c,d,g,h: It might be interesting to quantify how much of the variance change between historical and hist-piNTCF is due to different multidecadal trends in these two experiments versus different interannual variability. The impact of different trends on the variance change could be quantified by comparing Fig. 1d,h (which presumably include the effects of trend differences) with the analogous figures computed using detrended time series. Generally speaking, the effects of anthropogenic aerosols (which tend to dominate the NTCF response) counteract the effects of greenhouse gases, contributing to smaller trends in *historical* compared to *hist-piNTCF*. This is consistent with the overall decrease in variance shown in Fig. 1d,h.

We thank the reviewer for this suggestion. Following their recommendation, we performed an additional analysis using linearly detrended time series to compare with the original standard deviation results (Sup.Fig. 1a-d). This detrending is meaningful for the *hist-piNTCF* experiment, where aerosol forcing is fixed to pre-industrial and temperature continuously increases throughout the study period. However, it is less effective for the *historical* experiment, where aerosol forcing is relatively stable until the 1980s and then increases, producing a distinctly non-linear effect on surface temperatures (see Fig 3a,c of the manuscript). A simple linear detrend may therefore misrepresent the variability signal.

To better separate contributions from different timescales, we also applied a 10-year Butterworth low-pass (Sup.Fig 1e,f) and high-pass filter (Sup.Fig. 1g,h). The low-pass results capture the long-term NTCF forcing and multidecadal variability, while the high-pass results highlight interannual-to-decadal changes. We find that the greatest variance changes are associated with long-term and multidecadal variability, supporting our conclusion that the atmospheric signal is connected to ocean circulation and convection changes (multidecadal timescales; Grossmann and Klotzbach, 2009). Although weaker, we also find variability increases in the high-pass analysis, particularly in the Barents Sea and inner Labrador Sea. This may be linked to sea ice extent changes (see Fig B2 of the manuscript), where reduced sea ice cover exposes the ocean to stronger interannual fluctuations.

These insights have been included in the revised version of the paper. Lines 191-193 now read:
"*Secondly, we detect an increase in* tas *variability over the Labrador and Norwegian Seas, key regions of deep water formation (Fig. 1d). This variance increase concentrates on multidecadal scales (not shown), consistent with the characteristic timescales of North Atlantic ocean circulation and convection. The detected signal aligns with changes in ocean convection due to NTCFs (Delworth and Dixon, 2006; Iwi et al., 2012), explored in Subsection 3.3.*"



Supporting Figure 1. Standard deviation in time for the multi-model historical ensemble mean (a, c, e, f) and temporal variance ratio between the *historical* ensemble mean and its *hist-piNTCF* counterpart (expressed as percentage change; b, d, f, h). (a, b) Original annual surface air temperature (tas) data, (c, d) linearly detrended data, (e, f) 10-year low-pass filtered data (g, h) and 10-year high-pass filtered data. The filtering is applied with a Butterworth approach. The *historical* and *hist-piNTCF* ensembles analysed are comprised of 4 models (BCC-ESM1, MRI-ESM2-0, UKESM1-0-LL and EC-Earth3-AerChem) with 3 members each. Stippling is applied to different percentages of ensemble members coinciding in the sign of the response (b, d, f, h).

5) Fig. B2: Should probably say something in the figure caption about why you don't show the siconc from the EC-Earth model.

We agree that this omission should be explained in the figure caption, which now includes: "*Note that EC-Earth3-AerChem is not shown as* siconc *data were not available for the* hist-piNTCF *experiment.*"

6) Lines 224-225: I would change "sea ice-albedo feedback" to "sea ice-related feedbacks" here. The albedo feedback over the Arctic mainly operates in summer, but you're showing

autumn siconc here. In the autumn, it is mainly the sea ice-insulation feedback that is acting to amplify temperature changes.

We agree with the reviewer that a more general term is appropriate given the complexity of Arctic climate processes, and have changed it as suggested. Certainly, albedo is most relevant during maximum insolation periods, controlling ocean energy absorption up until early autumn. This has direct implications to sea ice formation and ocean energy release during the analysed autumn period while also being deeply interconnected with other surface processes such as sea ice-insulation (as the reviewer notes), surface heat flux, and lapse rate feedbacks.

7) Line 230: “our results suggest” appears twice.

Corrected.

8) Lines 232-233: It’s unclear what is meant here by “regional radiative changes”.

We agree that this phrase was too vague and potentially confusing in the context of our analysis. Lines 232-233 now read:
“*The observed increase in sea ice extent spatially aligns with the temperature response, which could suggest the operation of sea ice–related feedbacks, well-known mediators of Arctic amplification (Previdi et al., 2021). Additional large-scale processes, such as variations in atmospheric (Needham and Randall, 2023) and oceanic (Iwi et al., 2012; Robson et al., 2022) energy transports, may also contribute to the observed temperature changes. However, targeted experiments would be required to quantify their relative roles*”

9) Line 236: Should be “formation”.

Corrected.

10) Fig. 4: Figure title indicates that the period of focus is 1980-2014, while the caption indicates 1950-2014.

Corrected.

11) Lines 245-246: First of all, should say Fig. 4d, not 4f. Corrected.
Secondly, is it certain that these episodes of collapsed convection are purely stochastic? Could there be a state (and thus forcing) dependence to them? If so, the results in Fig. 4d might not change much if you had more ensemble members. All this is to say that it might be good to soften the language a bit here, e.g., say that the response to NTCFs “may be” underestimated, rather than “is likely” underestimated.

Regarding the stochastic nature of convection collapse episodes: Meccia et al. (2023) demonstrate that EC-Earth3 models exhibit multi-centennial AMOC oscillations triggered by the accumulation of salinity anomalies in the Arctic that, when released into the North Atlantic, affect water column stability and therefore convection. They state sea ice plays a driving role in the development of the salinity anomalies and find that in future scenarios with warmer conditions there is not enough sea ice to trigger the collapsing mechanism. Thus, there is indeed a state-dependency related to the background forcing. However, evidence from larger ensembles indicates under historical forcings the background-state does not imply the occurrence of collapsed convection. From the 15 historical members produced at the BSC with the General Circulation Model (GCM) version of EC-Earth3, only 5 members show a spontaneous collapse of the Labrador Sea mixing up to 2005, which leads to a consistently lower AMOC state (as shown in Figs. 6a and 7a of Bilbao et al., 2021). Notably, the periods of collapse differ across these members, underscoring their stochastic nature even under identical external forcing.

Nevertheless, in the simulations used in our study, collapsed convection occurs only in *historical* members, never in *hist-piNTCF* ones. This suggests that the relatively warmer climate due to the reduced presence of NTCFs (and associated sea ice changes) may prevent the collapse mechanism from operating. In this sense, NTCFs forcing does play a role in the occurrence of collapsed convection.

With respect to the chosen wording, we should clarify that our statement about underestimation refers to an implication of the used methodology rather than the nature of convection collapse itself. When a *historical* simulation exhibits collapsed convection (as seen in Figure B4a), the presence or absence of NTCFs has minimal impact on the already collapsed convection state. Comparing such a *historical* member to a *hist-piNTCF* member with active convection would erroneously suggest that NTCFs decrease convection, when in fact the difference would reflect the collapsed convection state (strongly dependent on internal variability) rather than exclusively the NTCF forcing effect. In essence, the methodology used to isolate the NTCF forcing presents itself lacking in this case.

Despite having one collapsed historical member for the whole study period and another for approximately half the period, the EC-Earth3-AerChem ensemble means still show that NTCFs enhance Labrador Sea convection (Figure 4d). Thus supporting our "likely underestimated" phrasing and our decision to discard the collapsed historical members in the following analyses.

All in all, we thank the reviewer's input and deem important a clarification. Lines 243-245 now read:
"*This behaviour is consistent with known Labrador Sea convection shutdowns in EC-Earth3-models that can persist for extended periods (Bilbao et al., 2021, Doscher et al., 2022). Meccia et al., 2023 attributes these episodes to a multi-centennial oscillation triggered by the accumulation of salinity anomalies in the Arctic that, when released into the North Atlantic, affect water column stability and therefore convection. Importantly, they find that future scenarios with warmer climates lack sufficient sea ice to trigger the collapsing mechanism, potentially explaining its absence on* hist-piNTCF *members. Due to the strong dependency of the collapse episodes on internal variability, the specific response of convection to anthropogenic NTCFs is not correctly reflected in Fig. 4f and is likely underestimated.*"

12) Line 251: Could be worth noting here that this model only shows a decline after ~1980, at which point global aerosol concentrations had stabilized.

We thank the reviewer for this observation. The timing of the convection decline in MRI-ESM2-0 after ~1980 indeed aligns with the stabilisation of global aerosol concentrations, which provides strong support for our central conclusion that NTCFs counteracted the GHG-driven convection decline.

The fact that MRI-ESM2-0 is the only model showing this clear temporal transition may reflect a higher sensitivity to GHG forcing. In fact, according to Bryden et al. 2024, MRI-ESM2-0 shows the strongest AMOC weakening (-67%) under SSP5-8.5 forcing (the ensemble includes EC-Earth3 and UKESM1-0-LL models, -34% and -50% respectively). However, other factors particular to each model, such as differences in the aerosol chemistry schemes or NTCF atmospheric lifetimes could be at play.

We think it is worth to include this information in the discussion, which now reads:
"*The* hist-piNTCF *experiments show a decrease in convection, in line with the expected response to rising GHG concentrations. In contrast, all* historical *experiments show stable or increasing* mlotst *values except for MRI-ESM2-0 (Fig. 5a). This model reports increasing convection until the 1980s after which convection declines, aligning with a first period of increasing global aerosol concentrations followed by a second period with stabilised aerosol concentrations and stronger GHG forcing. This suggests NTCFs counteracted, or at least mitigated, the GHG-driven decline in convection.*"

13) Fig. 6 caption: The variable name for salinity seems to have been entered incorrectly, i.e., (b, e) salinity (textitso).

Corrected.

14) Lines 262-263: Missing parenthesis, i.e., (as observed in Fig. 4).

Corrected.

15) Lines 265-269: This part seems too speculative to me. Can you present any evidence that this recirculation of saltier subsurface water is actually happening in the models? Or at least some citation from the literature supporting the existence of this positive feedback in the Labrador Sea?

A limitation of using free running coupled model simulations is that it is not possible to cleanly separate the driving signals of deep ocean convection from the ocean response to the associated vertical mixing. Therefore, our interpretation is unavoidably speculative to some degree.

Conceptually, NTCFs radiative forcing directly affects surface temperature whereas surface salinity anomalies can only emerge indirectly, e.g., via changes in the vertical mixing or the ocean circulation. We propose the presence of a convection-driven salinity feedback as an explanation of the maintained salinity increase, as such a mechanism has been identified in more idealised setups. In particular, Lenderink and Haarsma (1994) using both a one-box model and a conceptual ocean model, show that when convection is triggered in a region with cold and fresh surface waters above saltier and warmer subsurface waters, it produces a maintained convection state, with vertical mixing resulting in a saltier and warmer surface. Because the surface heat anomaly is rapidly lost to the atmosphere, while the salinity anomaly is conserved, the resulting positive density anomaly sustains convection. This feedback regime aligns with the properties of the Labrador Sea water column, and is consistent with the response to NTCFs seen in our results (Figs. 6 and B5, see also answer to comment 17). However, given the limitations of our methodology and the lack of idealised experiments, we agree that the strength of our claim should be moderated in the revised text.

Lines 265-269 now read:

"*The saltier surface conditions may result from a positive feedback: stronger convection, initially driven by surface cooling, brings saltier subsurface waters to the surface, further increasing surface density and reinforcing deep convection. Although our analysis based on monthly model outputs does not allow us to clearly separate the driving signals of deep convection from the resulting response, a similar feedback mechanism has been identified in idealised frameworks (Lenderink and Haarsma, 1994), suggesting that this process is plausible in regions such as the Labrador Sea where subsurface waters are climatologically saltier (Fig. 6b). This mechanism could also explain the steady increase in* mlotst *seen in Fig. 5b, despite aerosol reductions after the 1980s.*"

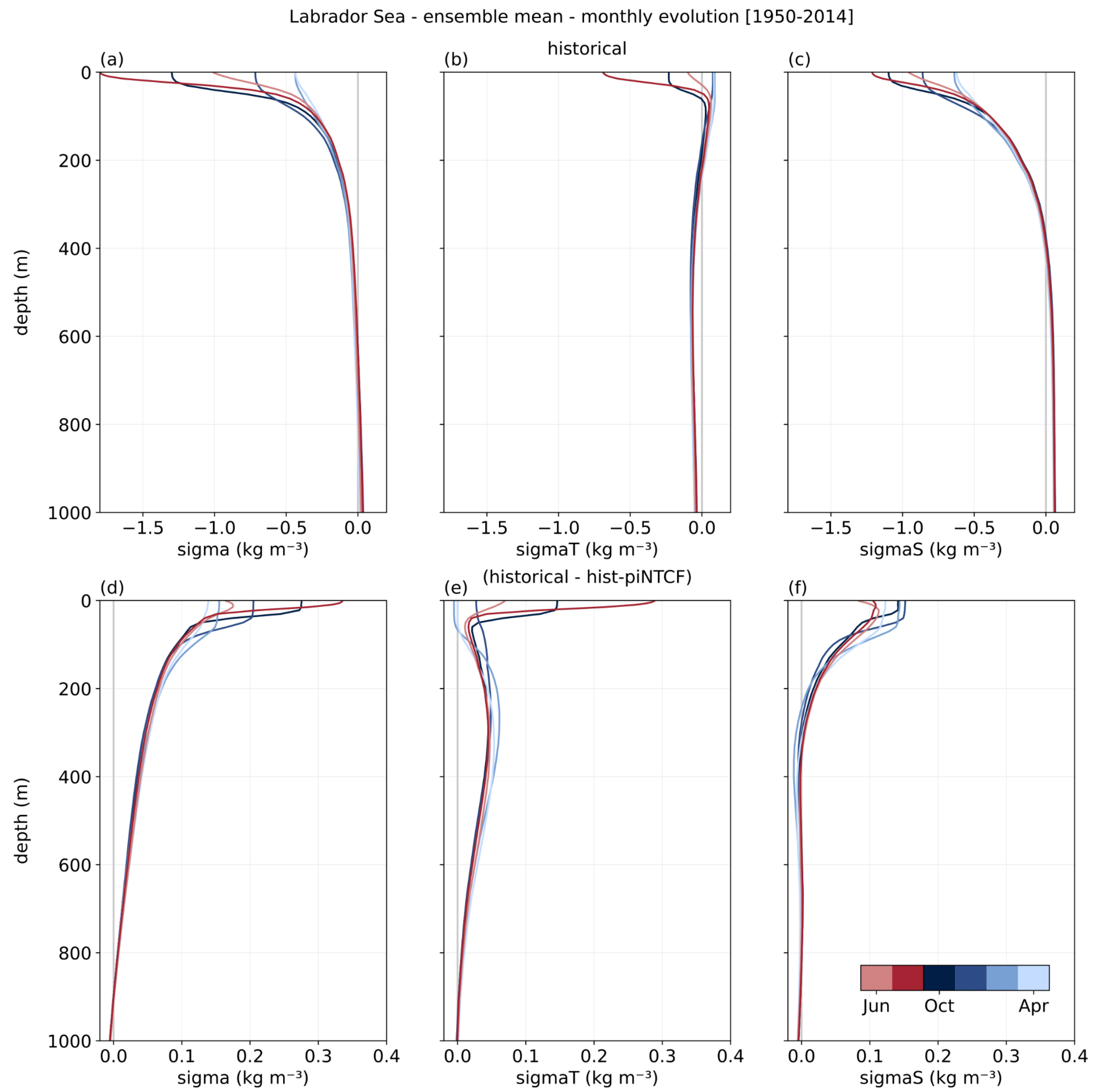
16) Fig. B5 caption: Should be "temperature's contribution to density (sigmaT)".

Corrected.

17) Lines 270-275: I think that Fig. B5 is useful for understanding the contributions of temperature and salinity to the simulated density anomalies. However, I don't agree with the authors' interpretation of this figure. Specifically, it is stated that "temperature initially triggers surface density increases, which are subsequently reinforced by a salinity-driven feedback" (echoing a similar statement on lines 265-269). However, in October, temperature and salinity contribute about equally to the surface density anomalies. So, I don't see how Fig. B5 can be used to argue that temperature anomalies are the initial trigger of the density anomalies, and that salinity anomalies are a subsequent feedback. I think this section (and lines 265-269, which make similar statements) needs to be reworded a bit.

We thank the reviewer for this careful reading of Fig. B5, which led us to refine both our analysis and its interpretation. To provide a clearer view of the evolution of density anomalies, we modified Fig. B5 to include not only the convective months (October–May, in blue) but also the non-convective months (June–September, in red), following the climatological cycle shown in Fig. B3.

In the revised figure, the response of *sigmaT* to NTCFs peaks in August, far exceeding that of *sigmaS* (new Fig. B5e,f). This is consistent with the seasonality of irradiance and therefore with the seasonality of NTCF radiative forcing, which is strongest in summer in the Northern Hemisphere. During this season, warmer surface temperatures maintain water-column stratification. Therefore, stronger surface cooling due to NTCF forcing in the *historical* ensemble would increase surface density and erode the stratification, favouring convection in the following months. Once convection is active in October, the thermal contribution decreases while the haline contribution increases, consistent with enhanced vertical mixing bringing warmer and saltier subsurface waters to the surface.



New Figure B5. To enhance figure clarity only data from every second month is shown. Colours represent the state of climatological convection according to Figure B3: active (Oct-May; blue) and nonactive (Jun-Sep; red).

We also note the potential role of sea ice in shaping *sigmaS* signal. As shown in Fig. B2, NTCFs increase Labrador Sea sea ice extent in at least three models. Greater sea ice cover in the *historical* ensemble compared with *hist-piNTCF* would be expected to increase surface salinity through brine rejection during ice formation (autumn and winter), and reduce it through freshwater input when the ice melts (spring and summer). This seasonality qualitatively aligns with the evolution of the *sigmaS* response to NTCFs (new Fig. B5f). While a targeted analysis of brine injection or salinity transport would be required to quantify the sea ice role, we acknowledge its potential contribution. However, the relatively small seasonal variability of the haline contribution, as well as the larger magnitude of the summer thermal signal, suggest that the salinity response mainly reflects a feedback of intensified convection rather than a driving signal external to the convection system.

We therefore expanded the discussion in the revised manuscript to include the contributions to density during the non-convective months, and softened the strength of our statements to better reflect the limitations inherent to our analysis. Lines 270-275 now read:

"*The monthly evolution of potential density and its temperature and salinity contributions (Fig. B5; see subsection 2.3) provides additional insight into the processes driving convection, despite methodological limitations. We observe that NTCFs enhance the temperature contribution to surface density increase during the non-convective months (red profiles in Fig. B5e). During this summer period, warmer surface temperatures maintain water-column stratification, therefore, greater surface cooling due to NTCFs in the* historical *ensemble would increase surface density and erode the stratification, favouring convection in subsequent months. As convection activates, the relative contribution of salinity increases, consistent with enhanced vertical mixing bringing warmer and saltier subsurface waters to the surface. The seasonality of the* sigmaS *signal (Fig. B5f) is also consistent with the seasonal salinity changes arising from sea ice formation and melting, potentially relevant as the historical presence of NTCFs results in greater sea ice extent in the Labrador Sea region (Fig. B2). Further analysis would be required to quantify the sea ice contribution. However, the relatively small seasonal variability of the haline contribution, as well as the larger magnitude of the summer thermal signal, suggest that temperature anomalies are the dominant destabilising factor, while salinity anomalies reinforce and sustain convection.*"

18) Line 310: Should be “rsut”, not “rust”.

Corrected.

19) Fig. 9 and Fig. B6 captions: I’m a bit confused here about the distinction between MRI-ESM2-0 and the other models in terms of representing the effects of major volcanic eruptions. Even if the models other than MRI-ESM2-0 don’t include interactive stratospheric chemistry, they should still prescribe the volcanic aerosols in their historical simulations, correct? If so, why isn’t this reflected in od550aer? Do these models simply exclude the stratosphere in their calculation of od550aer? Or, is there some other explanation?

We agree with the reviewer that this distinction should be made more explicit. While some models explicitly resolve stratospheric chemistry, others parameterise the effects of volcanic aerosols. Unlike the other models, MRI-ESM2-0 includes stratospheric aerosols in the *od550aer* variable. As a result, peaks in AOD following major volcanic eruptions are present in both the *historical* and *hist-piNTCF* ensembles for the *od550aer* diagnostic (Fig. B6b). Despite this model-specific characteristic, the difference between these two experiments still effectively isolates the anthropogenic emissions signal, maintaining consistency with the other models in the study.

Evidence that all models in this study account for the radiative effects of volcanic aerosols is seen in Fig. B6a where all ensembles show a decrease in the *netR_HD* index after the major eruption of Mount Agung in 1963.

To clarify this point for readers, we have updated the figure captions:

- Figure 9 caption: "*... Note that MRI-ESM2-0 (b) resolves stratospheric chemistry and therefore stratospheric aerosols are included in the* od550aer *variable. Regardless, all models in this study account for the radiative effects of volcanic aerosols either explicitly or through prescribed datasets or parameterisations.*"
- Figure B6 caption: "*... Note that MRI-ESM2-0 resolves stratospheric chemistry and its effect is included in the* od550aer*. As a result, peaks following major volcanic eruptions are present. Regardless, all models in this study account for the radiative*

effects of volcanic aerosols either explicitly or through prescribed datasets or parameterisations."

20) Lines 329-330: I would change this to "supports the hypothesis that aerosols, through some combination of direct effects and aerosol-cloud interactions, force…", or something similar. You haven't actually quantified the relative impacts of aerosol direct and indirect effects on the net radiation.

We agree that our original wording implied that our analysis distinguishes between aerosol direct and indirect effects, which it does not. Following the reviewer's suggestion, we have revised our discussion to reflect the combination of forcing pathways.

In addition, and following feedback from Referee #2, we expanded the analysis to include the NTCF signal on cloud radiative forcing (all-sky minus clear-sky fluxes), including the decomposition of shortwave and longwave radiation components. The results are shown in a new version of Figure 8 and discussed in the revised manuscript (see also our response to RC2 for further detail).

21) Lines 330-331: "Notably, the clt magnitude…" I don't understand this sentence. clt is the total cloud fraction/amount – how does it capture changes in other cloud properties besides that? And how can it be used to detect aerosol-cloud interactions? I would explain more clearly what you mean here, or just remove this sentence.

We thank the reviewer for raising this point. We acknowledge that our original statement was misleading: the total cloud fraction (*clt*) indeed represents only the percentage of sky covered by clouds and does not capture other relevant microphysical or radiative properties such as albedo or optical thickness. While *clt* could reflect changes in cloud lifetime, it is not an adequate or direct diagnostic of cloud properties. To avoid overinterpretation, we have removed this sentence from the revised manuscript.

22) Lines 335-339: I would remove this paragraph as it does not fit well within the rest of the discussion. First of all, you have not actually quantified aerosol-cloud interactions in your model simulations, so it's unclear how your results relate to those of Zhao and Suzuki (2021). Secondly, all of the previous discussion/analysis attempting to link aerosols to the ITCZ shift focused on the aerosol effect on the top-of-atmosphere radiation. Now, in this paragraph, you start to talk about aerosol effects on surface evaporation and the hemispheric atmospheric energy contrast. Again, I think this paragraph just doesn't fit well, adds confusion, and is unnecessary.

We agree that the paragraph in question does not align closely with our results and, as the reviewer notes, introduces unnecessary confusion. We have therefore removed this paragraph from the revised manuscript.

23) Lines 344-346: The Byrne et al. (2018) paper is a review paper that does discuss "not only changes in ITCZ location but also in its width and strength", in multiple contexts (e.g., observations, future climate projections). The role of aerosols is discussed some, but mainly (as far as I can tell) in terms of aerosol effects on ITCZ latitude. Please explain more clearly how your finding here of a negative correlation between netR_HD and equatorial rainfall amount is "consistent with" the Byrne et al. (2018) study. Or just remove this sentence.

After revisiting Byrne et al. (2018), we agree that the link we drew was not sufficiently supported in the context of our results. We have removed this sentence from the revised manuscript.

24) Lines 370-372: As discussed in previous comments, I don't believe that your results support these statements. The 38% increase in convection refers to the Feb.-Mar.-Apr. (FMA) season. During FMA, surface density (and thus convection) anomalies are driven primarily by salinity anomalies, not temperature anomalies (Fig. B5). And you've provided no evidence as far as I can tell to support the existence of the salinity feedback that is proposed here.

We agree that our conclusions regarding the salinity feedback mechanism, as currently phrased, may be too speculative given the evidence presented. The hypothesis is discussed in the context of our results as referenced in previous comments, however, we have removed it from the main conclusions of the paper. Lines 370-372 now read:

"***Increased Labrador Sea Convection:*** *Over the period 1950–2014, historical NTCFs contributed to a 38% increase in FMA Labrador Sea convection. This increase is consistent with summer surface cooling eroding water-column stratification and favouring deeper convection in the following months. Once convection is active, salinity anomalies appear to lead the density response.*"

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