Author's response to comments from Anonymous Referee #1:

Aerosol–cloud interactions influence the climate response to AMOC weakening

by Ryan Vella & Ulrike Lohmann

We thank the editor and referees for taking the time to review our manuscript and for the valuable feedback.

Here, the comments from Anonymous Referee #1 (from June 20, 2026) are reproduced in black, while our responses are presented in blue.

From Anonymous Referee #1's response:

Summary

An interesting paper that provides a relatively comprehensive analysis of the effects of AMOC weakening on aerosol cloud interactions. The study finds a positive global mean cloud radiative effect anomaly, which is due to a reduction in the cooling efficiency of existing clouds overwhelming enhanced cooling from more cloud cover. I list some concerns below.

We thank the referee for the positive assessment. Before the detailed responses, we note important changes made during revision. First, we identified and corrected a coding error in the coupling between the land surface scheme and the interactive dust emission scheme, in which regional scaling factors were passed incorrectly to the emission calculation, overestimating dust fluxes. The bug was fixed, the model was retuned, and both simulations were rerun. All quantitative values in the revised manuscript have been updated. This correction changed magnitudes only, and the sign and mechanism of the aerosol and cloud-microphysical responses are unchanged, including the hemispheric aerosol redistribution, the two-pathway microphysical response, and the regional structure of the cloud radiative anomalies.

Additionally, and independently of this correction, we replaced the CRE-based attribution with an ISCCP-simulator cloud radiative kernel decomposition (Zelinka et al., 2012a, b) and included double-call aerosol radiation diagnostics, prompted by this referee's comments on isolating the aerosol effect and by Referee #2's comments on clear-sky masking. This new analysis revises the interpretation of the results. While the cloud radiative response is strong regionally, its opposing contributions largely cancel, resulting in $+0.19\,\mathrm{W\,m^{-2}}$. The global radiative response is instead dominated by the dust direct effect and the surface albedo term, which together with the small cloud adjustment yield $-0.87\,\mathrm{W\,m^{-2}}$ and reinforce rather than offset the AMOC-induced cooling. The text has been revised accordingly, and the quantitative details are given in our responses to the relevant comments below.

Comments

First Paragraph: "Anthropogenic climate change is driving rapid Arctic warming and ice melt, increasing the influx of freshwater into the North Atlantic (Bamber et al., 2018). This additional freshwater reduces the salinity and density of the upper ocean, weakening deep water formation (Sévellec et al., 2017). As a result, the sinking branch of the AMOC becomes progressively suppressed, and the overturning circulation loses strength (Bakker et al., 2016)." This seems to suggest that freshwater input into the subpolar North Atlantic is the only way the AMOC can weaken. What about direct thermal forcing? From not only GHGs, but anthropogenic aerosols as well (e.g., https://acp.copernicus.org/articles/21/5821/2021/).

We agree that the original paragraph gave the impression that freshwater input is the only pathway to AMOC weakening, which was not our intention. We have rewritten it so that anthropogenic forcing is framed as acting on upper-ocean buoyancy through both freshwater and thermal pathways. The revision incorporates direct thermal forcing from greenhouse gases, noting that heat flux anomalies rather than freshwater fluxes have been identified as the dominant driver of the projected weakening in CMIP6 models (Couldrey et al., 2023), as well as the opposing role of anthropogenic aerosols, whose twentieth-century build-up is estimated to have offset part of the greenhouse-gas-driven weakening (Hassan et al., 2021), as suggested by the referee.

It might be worth connecting the loop somewhere (which is interesting) if possible à less anthropogenic aerosols (e.g., clean air policies) à AMOC weakening à changes in natural aerosols à positive forcing (as you find) à would potentially act to amplify the initial aerosol forcing and AMOC weakening. Although it appears you only find a dust response, and not a sea salt response (see below).

We thank the reviewer for this suggestion. On the sea-salt point, our simulations do show a clear sea-salt decrease over the subpolar North Atlantic that was masked in the original presentation by the much larger dust perturbation; we describe it, and its mechanism, in our response to the comment on L44 below.

Regarding the feedback loop, our kernel-corrected analysis does not support the positive-forcing step it requires. The loss of sea-salt cloud condensation nuclei reduces the subpolar CCN column burden (Fig. 4a), which in isolation would act as a positive forcing. However, the net top-of-atmosphere radiative response is a -0.87 W m^{-2} cooling, dominated by the enhanced Saharan dust direct effect (-0.23 W m^{-2}) and the surface albedo response (-0.83 W m^{-2}, sea ice and snow), while the genuine cloud radiative adjustment is small ($+0.19$ W m^{-2}). The natural-aerosol and surface response therefore reinforces, rather than offsets, the AMOC-induced cooling. Since a cooling of the North Atlantic surface raises upper-ocean density and favours deep-water formation, the mechanism by which anthropogenic aerosol cooling is thought to have offset part of the AMOC decline (Hassan et al., 2021), this response would be expected to act as a stabilising (negative) feedback on AMOC weakening rather than the amplifier the loop envisages. Our atmosphere-only design cannot quantify this, and the expanded sea ice that produces much of the cooling would itself insulate the ocean and suppress convection, so we regard the sign as indicative only (see our response to the comment on L418).

L44 "Most previous studies have focused primarily on thermodynamic and macrophysical cloud responses, leaving a critical gap in our understanding of how aerosol–cloud interactions shape the climate response to AMOC weakening. Changes in atmospheric circulation and surface wind patterns associated with a weakened AMOC inevitably alter natural aerosol emissions as well as their atmospheric transport and processing." Yes. Probably can expand this and cite a couple of papers (either here or elsewhere, since your analysis is focused on the natural aerosol emissions response to AMOC weakening). For example, this paper: https://www.nature.com/articles/s41612-024-00602-8 isolates the AMOC feedback response and finds (for AMOC weakening): subpolar North Atlantic (SPNA) cooling, increased SPNA low level cloud cover (consistent with the SST cooling), increased SLP over Greenland/Iceland (i.e., weakened Icelandic low), weakened SPNA surface winds (consistent with the former) and decreases sea salt burden over the SPNA (consistent with weaker winds and SST cooling). These SST, SLP and wind responses to the AMOC also exist in CMIP6 models, as shown here: https://acp.copernicus.org/articles/21/5821/2021/ (and presumably the sea salt response).

Although again, it appears you do not find a sea salt response (not even in the SPNA). Any ideas on why that might be? I assume it has to do with the large AMOC weakening here, and the corresponding increase in sea ice in the SPNA? Which effectively shuts of sea salt emissions here?

We thank the referee for these suggestions and the two references, which we have incorporated. The paragraph in question has been expanded to summarise the AMOC feedback fingerprint isolated by Allen et al. (2024) and the consistent CMIP6 responses documented by Hassan et al. (2021), and to state explicitly how our study builds on them, namely by resolving the microphysical pathway from emissions through CCN and INP to cloud properties rather than diagnosing the aerosol response as a feedback within coupled simulations.

Regarding sea salt, this comment prompted a regional analysis that we had not performed. It shows that our simulations do in fact produce a pronounced sea-salt response over the subpolar North Atlantic. Averaged over 50–65°N, 60–10°W, the sea-salt emission flux decreases by $63\,\%$ and the sea-salt burden by $60\,\%$, accompanied by weakened surface winds ($-0.41\,\mathrm{m\,s^{-1}}$, $-4.8\,\%$), raised sea-level pressure ($+2.8\,$hPa, consistent with a weakened Icelandic low), surface cooling of $5.2\,$K, and increased cloud cover ($+3.7\,\%$). Our response therefore reproduces every element of the AMOC weakening fingerprint described by Allen et al. (2024), including the sea-salt decrease. The signal dilutes to a $3\,\%$ decrease in the global-mean burden and is masked in the zonal-mean AOT of Fig. 3d by the much larger dust perturbation, which is why it was not visible in the original manuscript. We now document it in Sect. 3.2.

Concerning the mechanism, the referee's hypothesis of a sea-ice shut-off is reasonable, but it is not what operates in our simulations. The region over which we evaluate the sea-salt response remains ice-free in both simulations, as the sea-ice expansion under hosing (Fig. S1) lies outside it, so the open-water source is not shut off. The reduction instead arises from the surface cooling and the weaker winds acting through the temperature and wind-speed dependence of the sea-salt source function in the model. This is now stated in Sect. 3.2.

L110. “All anthropogenic and biomass-burning emissions were held fixed at perpetual 2015 levels throughout the integration, ensuring that any changes in aerosol distributions arise solely from dynamical adjustments rather than emission changes.” à I assume this whole sentence refers to anthropogenic and biomass burning emissions, since certainly dust emissions are changing in responses to e.g., meteorology. Might want to clarify. Yes, L120 confirms, but probably good to be more precise in the above sentence.

We have revised it to state explicitly that only anthropogenic and biomass-burning emissions are held fixed, while dust and sea salt emissions are computed interactively and respond to the simulated meteorology, which is precisely the natural emission pathway this study examines.

L182. “The enhanced Saharan dust export finds strong support in the palaeoclimate record. Speleothem and marine sediment reconstructions show that periods of weakened AMOC and increased latitudinal temperature gradients coincide with elevated Saharan dust fluxes..” à Is this due to AMOC weakening specifically, or just general cooling?

This is a fair question, and the palaeoclimate record alone cannot fully separate the two, since intervals of weak AMOC in the late Pleistocene are embedded within glacial boundary conditions that independently favour enhanced dust emission. We have revised the text to acknowledge this explicitly and to present the two features of the record that point to an AMOC-specific contribution beyond the glacial background. The first is timing, as dust deposition off West Africa and in the Eastern Mediterranean peaks abruptly during Heinrich stadials and the Younger Dryas, millennial-scale intervals of AMOC disruption within otherwise similar background states, with the Younger Dryas maximum occurring against a backdrop of deglacial warming. The second is mechanism, as the proposed pathway operates through the interhemispheric temperature asymmetry, southward ITCZ displacement, and weakening of the West African monsoon (Mulitza et al., 2008; Collins et al., 2013), which is the fingerprint of reduced cross-equatorial heat transport rather than of global-mean cooling. We also note that our experimental design provides the isolation the proxy record cannot, since greenhouse forcing, vegetation, and all other boundary conditions are identical between the simulations and only the AMOC-derived SST and sea ice anomalies differ, so the simulated dust response is attributable to the AMOC perturbation by construction. The palaeo evidence therefore serves as qualitative corroboration of the mechanism rather than as the attribution itself, and the revised text now frames it accordingly.

Near L340 and Figure 8. It might be worth showåing the total CRE, to identify where it is positive versus negative? For example, this is not intuitive over the SPNA since SW effects are large and positive but LW effects oppose.

We thank the referee for this suggestion, which we have adopted. Figure 8 now includes the net cloud radiative effect as panel (c), alongside the shortwave (a) and longwave (b) components, so that the regions of positive and negative net CRE can be read directly.

I'm also a bit confused on the use of CRE, since this does not isolate the aerosol effect. Does the model not have diagnostics (e.g., aerosol free radiation calls) that allow one to isolate the aerosol radiative effect directly? Or, perhaps, one could use an offline radiative transfer model? Not only the aerosol-cloud effect, but what about the aerosol direct radiative effects? Are they not important to also quantify?

We agree, and have added this diagnostic. The aerosol direct radiative effect (DRE) is now obtained from a second, aerosol-free radiation call as the difference between the standard and aerosol-free fluxes, at the top of atmosphere and the surface and for the shortwave and longwave. Under AMOC weakening the enhanced dust loading strengthens the direct effect by $-0.23\,\mathrm{W\,m^{-2}}$ in the global mean, with the largest anomalies in the Saharan dust outflow over the tropical North Atlantic (Fig. 9c). This makes it a leading term in the global radiative response, larger than the net cloud adjustment ($+0.19\,\mathrm{W\,m^{-2}}$) and second only to the surface albedo response ($-0.83\,\mathrm{W\,m^{-2}}$). It is now reported and discussed in Sect. 3.4.

L347, "Globally, the area-weighted SW cloud radiative effect anomaly is +1.16 W m-2, indicating a net reduction in cloud reflectivity" à Can you say this here? CRE will be related to both cloud reflectivity and cloud amount? Doesn't the next paragraph isolate these components?

We agree. The cloud radiative effect reflects contributions from both cloud amount and cloud optical thickness, and these are separated only by the kernel decomposition presented in the following paragraph. We have removed this sentence, and the global-mean attribution is no longer made from the CRE at all. The CRE now serves only to describe the spatial structure of the response, and the separation into cloud-amount, altitude, and optical-depth contributions is provided by the kernel decomposition. That decomposition gives a net cloud radiative adjustment of only $+0.19\,\mathrm{W\,m^{-2}}$ (shortwave $+0.49\,\mathrm{W\,m^{-2}}$), dominated by a reduction in cloud optical thickness ($+0.37\,\mathrm{W\,m^{-2}}$) and a longwave-dominated cloud-altitude term ($+0.16\,\mathrm{W\,m^{-2}}$) that arises as the residual of opposing cloud-top shifts tied to the ITCZ displacement rather than a general rise of cloud tops, partly offset by the effect of increased cloud amount ($-0.22\,\mathrm{W\,m^{-2}}$) and a decomposition residual ($-0.11\,\mathrm{W\,m^{-2}}$); this attribution now follows from the decomposition rather than being asserted from the CRE alone.

L418 (and L349). "The global-mean net cloud radiative effect anomaly of +0.84 W m-2 acts as a negative feedback that partially offsets AMOC-induced surface cooling." More broadly, if this positive (global mean) forcing also exists over the North Atlantic, the implication is that it would feedback and further weaken the AMOC? I understand this cannot be quantified in this model setup, but nonetheless, it's an interesting idea.

We agree this is an interesting question, but the revised analysis reverses its premise. The quoted sentence has been removed, since the $+0.86\,\mathrm{W\,m^{-2}}$ net CRE anomaly was largely clear-sky masking by the expanded sea ice and the enhanced dust. The kernel-corrected cloud adjustment is only $+0.19\,\mathrm{W\,m^{-2}}$

globally and, over the North Atlantic, is a strong cooling of about $-3\,\mathrm{W\,m^{-2}}$ in the area mean and up to $-13\,\mathrm{W\,m^{-2}}$ locally (Fig. 9b), to which the dust direct effect and the sea-ice albedo response add further cooling. A perturbation of this size could well affect the AMOC, but with the opposite sign to that envisaged by the referee: cooling of the North Atlantic surface favours deep-water formation and would tend to stabilise rather than further weaken the overturning. Our atmosphere-only design cannot quantify this, and we now note it in the limitations paragraph of Sect. 4.

L437. "a pathway that models relying on prescribed aerosol fields cannot represent, and whose absence may contribute to the spread in projected climate responses to future AMOC change." à is this referring to natural aerosols, anthropogenic aerosols, or both? Were there any CMIP6 models that used prescribed aerosol fields? In terms of dust (and even anthropogenic aerosol) emissions, I think that nearly all had interactive emissions? I also note that this point is also made at the end of the abstract. Perhaps a more important caveat to note (one that is in common to all single-model studies, in particular when based on aerosols) is that the results presented here may be model dependent.

The referee is right that nearly all CMIP6 models carry interactive dust and, in many cases, interactive anthropogenic aerosol emissions, and the statement was not meant to refer to emissions. It refers to the aerosol–cloud microphysical coupling, in which prognostic aerosol number determines the CCN and INP concentrations and thereby the cloud droplet and ice-crystal numbers. Many CMIP-class models prescribe this step instead, for example through a fixed cloud droplet number, a simplified activation, or a prescribed aerosol climatology driving the cloud microphysics, so that a change in the natural aerosol burden cannot feed back onto cloud properties. This applies to natural and anthropogenic aerosols alike, although our simulations isolate the natural response.

We also agree with the referee that a more fundamental caveat, common to all single-model studies and particularly those centred on aerosols, is that the magnitude, and in some regimes the sign, of the aerosol–cloud response may be model dependent. We have added this explicitly to the limitations paragraph, noting that our quantitative results reflect the aerosol and cloud-microphysics parameterisations of ICON-HAM and would benefit from corroboration in a multi-model framework.

References

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