We thank the two anonymous reviews for their careful attention in reviewing this manuscript, which has undoubtedly improved the quality of the paper. Our responses to specific points are detailed below in red font. New figures in this document are labelled as Figure RXX (R for review) in order to distinguish them from the figure numbers in the main manuscript. Line numbers in this document refer to the updated manuscript, unless indicated otherwise.

REVIEW 1

Review for: ‘Isolating the influence of aerosols on Arctic cloud radiative effects during a polluted warm air mass intrusion’ by Price et al. 2026

Price et al. have presented a modelling case study of a warm air mass intrusion observed in the Arctic by the MOSAiC campaign. They have performed simulations with and without anthropogenic pollution to investigate the role of aerosol transport within these warm air mass intrusions into the Arctic. They find that the aerosol do have an impact on the cloud droplet number and liquid water, but the effects on the radiative balance are limited, in part due to the sea ice state. The article is well written and provides an interesting insight into how human activity further impacts the Arctic, which provides important context for the region in a changing climate. All my comments are minor, and once addressed I recommend publication of this article.

Minor comments:

Line 12: I fully appreciate that the seasonality of the sea ice state makes this a reasonable conclusion, but I would rephrase this as a ‘suggestion’ or ‘implication’ rather than a proven result.

We have changed “these results show that…” to “these results suggest that…” on this line.

Line 24: Add ‘WAI’ between ‘Arctic events’ to be specific.

Added “WAI”

Line 25: Every year has a sea ice minima, so I would rephrase this to say ‘years in which the sea ice is below average’ or something along those lines.

Changed to “years in which the Arctic sea ice minimum is below average”

Figure 1 Caption: Could you make the domain boundary lines a different colour/style to the inset boundary line, as when I first looked at the plot (and read the caption) I thought there might have been a nested model domain shown by the inset. Also could you make the inset smaller in area, as it is pretty hard to see the ship track/time because it is very small within the domain.

We’ve changed the domain boundary to a thicker, dashed line and we have now zoomed in on the inset. We hope the figure is now clearer in these respects.

Line 94: Is it necessary to take the 9 nearest grid points when your resolution is 100km? I guess you are doing it to provide a level of uncertainty, but at that resolution you could easily be taking in other unrelated airmasses? Could this by why you did not see much impact from your aerosol changes? Did you compare the 9 grid mean to the nearest grid, or an interpolated point location?

We chose to take the 9 nearest grid points to account any possible deviation of the event footprint in the model relative to reality/observations. This choice was partially informed by similar analysis in Ahmed et al., (2023), which features a MOSAiC case study using a similar model configuration to ours (e.g. see their figure 5), in which they found that 9x9 grid cells around the ship captured the observations of ozone and mercury more accurately for this same time period. The spatial variability over these grid points can be seen in Figure 4 of the manuscript, where the red shading shows the range in each variable over the 3x3 cells.

In Figure R1 of this document (below) we have also added the value in the nearest grid point (i.e. the centre of the 3x3 grid) as a red dashed line. Using the nearest point instead of the mean of the 3x3 nearest points does not make a qualitative difference to the comparison to observations. We have also remade Figure 6 of the manuscript using only the nearest grid point instead of the average of the 3x3 nearest points. This figure is shown below as Figure R2. Although there are some differences in the individual distributions, the main takeaways of the figure are unaffected.

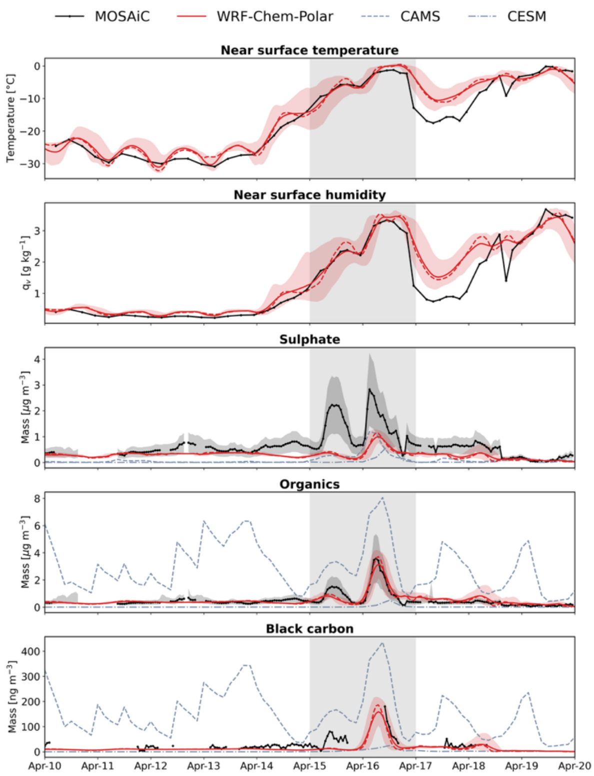

Figure R1: As Figure 4 in the manuscript, but including model output from only the nearest grid point to the ship (red dashed line), in addition to the average over the 9 nearest grid points (red solid line). Times series of hourly mean (a) near-surface temperature, (b) near-surface humidity, and (c-e) PM1 aerosol mass concentration from MOSAiC measurements (black with markers) and WRF-Chem-Polar output (red) for April 2020. Collocated aerosol mass concentration from CAMS reanalysis and CESM output are also shown on panels (c-e)

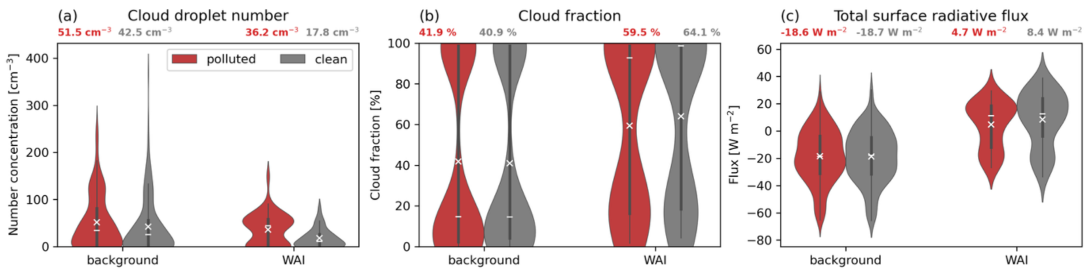

Figure R2: As Figure 6 in the manuscript but using model output from only the nearest grid point to the ship position, as opposed to the average over the 3x3 nearest grid points. Simulated distributions at ship position of (a) mean cloud droplet number concentration between 0 and 2 km altitude (in-cloud values only), (b) mean cloud fraction between 0 and 2 km altitude, *and (c) total surface radiative flux. Distributions are calculated for the background (1-13 April) and the WAI (15-16 April), and from polluted (red) and clean (grey) simulations. Mean (white cross) and median (white line) values are shown for each distribution, mean value is shown above each distribution in text.*

Figure 2: Could you overlay the MSLP to give an idea of the synoptic settings that transported the pollution?

We have updated Figure 2 with isobars from ERA5 daily means. The plume of air shown by the AOD anomaly follows the isobars between two regions of high and low pressure. More detail on the synoptic conditions of the event are given in Kirbus et al., (2023).

Line 105: Did Dada et al. do back trajectory analysis? Could you show it too? (adding the MSLP would give similar info though – so I understand if not).

We have added MSLP to Figure 2, as noted above. We agree with the reviewer that adding the FLEXPART back trajectories would give similar information as already presented in the present study and published in the literature, therefore it was not included.

Line 114: Do you have any ideas as to why the model overestimated in the earlier period? Is it a different airmass with different aerosol sources?

It is difficult to identify the exact origin of the overestimation of PM2.5 earlier in the run compared to the EMEP observations. It is worth noting that in general the model agrees well and is within 50% of the observed aerosol mass.

Line 167: I would clarify that only the Thompson results are described above/in Figure 4. I first read this to assume that the differences between the Thompson/Morrison were described above/in Figure 4.

We have added the following sentence to the beginning of the paragraph: "Model-observation comparisons shown in the previous sections were performed for simulations using the Thompson cloud microphysics scheme (Figures 3 and 4)."

We have also removed the reference to Figure 4 that was originally on line 167: "(described above, Figure 4)". We hope this makes the distinction clearer.

Section 3.3: I was a little bit confused here. My understanding of the Morrison microphysics (and I assume the Thompson) is that it just handles the cloud microphysics and not aerosol microphysics (e.g. sources, growth, which is handled by MOSAIC). So why are there differences in the aerosol between the two microphysical schemes? Is it because of wet deposition? Do these schemes handle detrainment of aerosol when clouds evaporate? Or is the Morrison microphys not coupled to an aerosol scheme (if so you should state as that would not really be a fair comparison)?

Yes, this is correct, both simulations use the MOSAIC aerosol scheme which calculates aerosol sources, growth, removal etc. Both Morrison and Thompson only calculate cloud microphysical properties. However, due to the coupling between the aerosol and cloud microphysical schemes, differences in the cloud microphysics can affect the calculation of aerosol processes, especially wet deposition rates as suggested. Also, the cloud microphysics can have impacts on stability and mixing in the boundary layer (again, even though these properties are not directly calculated by the cloud microphysics scheme), and therefore the vertical mixing and transport of aerosols can also be affected.

To try and better explain this concept in the text, we have added the following sentence in this paragraph: "Although the cloud microphysics scheme does not directly calculate any aerosol process rates, aerosol properties can still be indirectly affected due to coupling between aerosol and clouds, for example via changes to aerosol wet deposition rates or the vertical structure of the boundary layer."

Section 3.3/Figure 4/Figure A1: Could you not just put the Morrison results onto the same Figure so we can directly compare? I would make it a lot easier to see/reduce your figure count.

We separated the Morrison results into a separate figure for two reasons: (1) there are already several different lines in Figure 4, so we felt that including the Morrison results as well would hinder interpretation, and (2) most of the analysis in the main body of the manuscript focuses on the Thompson results, so we were concerned that including the Morrison results outside of the Appendix would dilute the key results of the manuscript.

Line 219: add 'during the warm air intrusion event' at the end of sentence here to be specific.

Added as suggested.

Line 249: Where you say the 'surface radiative impacts…. strongly dictated by seasonal conditions…' I think you need to need to say 'are likely' as your study does not show that exactly. You also say 'and background aerosols'. Do you mean the seasonality of background aerosol here too? I was thinking about that – if there is no sea ice, then there might be more sea spray/sulfate aerosol - so that would also change the background state and might also change your results?

We have added "likely to be" in between "therefore" and "strongly".

Background aerosols: yes, we agree completely with this interpretation. We tried to highlight this remaining uncertainty in the final paragraph of the original manuscript (which remains at lines 283-291 of the revised version: "during present-day Arctic spring, when background aerosol levels are already raised…and future transport of anthropogenic aerosol and extreme events").

My other thought here: Do warm air intrusions occur though the warmer months too (sorry – I am a bit ignorant about Arctic meteorology!)? If they don't then the seasonality of sea ice doesn't matter as much?

We consider that the possibility of a warm air event in warmer months is a relevant question for future research, for example a study of ERA5 data by You et al., (2021) found a total of 234 events in summer months over a 40 year period.

Figure A2: it seems the violin plots for the CDN background conditions are missing.

The simulation using Morrison microphysics only simulates ice clouds in the background period, resulting in a cloud droplet number concentration value of 0 throughout the background and therefore no distributions to show. We have edited the figure to make this clearer.

REVIEW 2

The topic of this manuscript is the role of aerosols during a warm air mass intrusion (WAI) event over the Arctic and the associated impacts on clouds and surface radiation. It addresses an interesting and timely topic, and the study is well aligned with this journal's scope.

Overall, I liked this paper, and think it will be a useful addition to the literature. However, I do think it has some important weaknesses that should be addressed prior to publication.

Most importantly, the current analysis puts a lot of emphasis on aerosol impacts on cloud and radiative impacts of the event, but the evaluation and validation in the study focuses mostly on aerosols, T, and Qv at the surface. Arctic clouds and boundary-layer structure are known to be very challenging to simulate, and matter a lot to cloud radiative effects. Thus, the paper would be strengthened by a more explicit assessment of whether the model captures the WAI event itself (timing, location, intensity) and whether key meteorological and cloud properties (e.g., cloud fraction, height) are realistically represented. If such evaluation has already been conducted in prior work for this event and model configuration, it should be clearly cited and summarized.

A few other points:

1) The manuscript notes the limitations of in situ observations for spatial context, which is why they say they need the model output. But as mentioned previously, there are known model challenges with correctly simulating clouds in the Arctic, and this will matter a lot to their cloud radiative effect results. That, plus the lack of cloud/WAI event model validation in the paper, pushes me to recommend that the authors add some remote sensing analysis, which I feel could substantially strengthen the paper. Even a qualitative comparison of event footprint, timing, etc. would add credibility (e.g., see https://go.nasa.gov/4f7JWb6). Similarly, cloud base height could be better constrained using lidar/cielometer from MOSAiC rather than relying only on surface T and Qv proxies.

Remote sensing:

As suggested, we have made a qualitative comparison of the event with MODIS remote sensing products. We have compared MODIS Aqua corrected reflectance and model output of total cloud water path (liquid + ice). We also add the model output for sea ice since sea ice also appears white on MODIS corrected reflectance maps. MODIS also picks up areas of high snow cover as white but we do not have the appropriate model output to represent this, so this should be taken into account when comparing the maps. The maps are shown below in Figure R3.

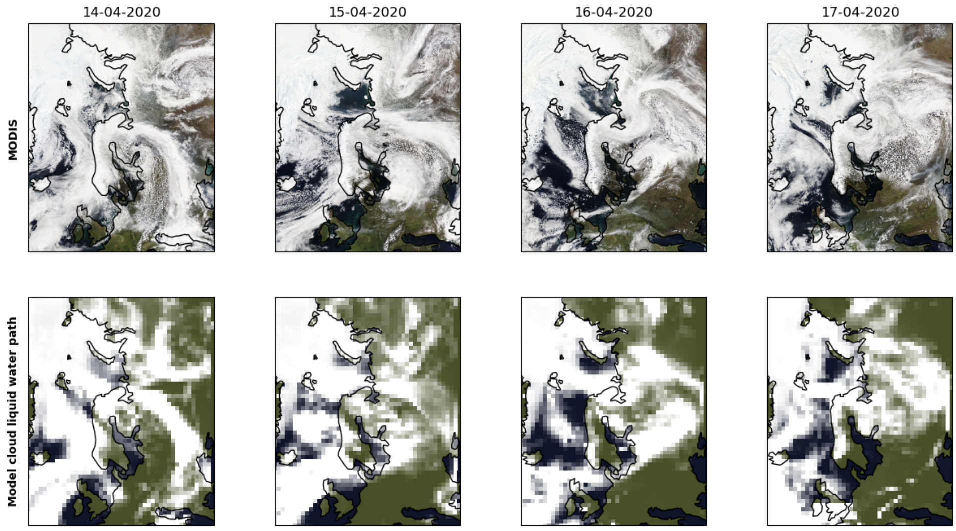

Figure R3: daily maps of MODIS corrected reflectance (top row) and model total cloud water path (bottom row).

The influence of this event on cloud structure and cover can be seen on the MODIS images in the form of large bands of cloud over eastern Europe and western Russia. The day before the event reaches the MOSAiC ship (14th April), the cloud band stretches from south east Europe to north west Russia and east Finland. On 15th and 16th April, the band has advected further east and can be seen progressing further north over the Barents Sea. On 17th April, the cloud cover is more broken over the Eurasian continent but cloud cover remains high over the Barents Sea. These structures and progression in time are broadly captured by the model (as viewed by total cloud water path fields), increasing confidence in the model's ability to capture the timing and spatial patterns of this event.

Cloud base height:

To compare cloud base height from ceilometer to model output, we first define the presence of clouds in a model grid box using a threshold of total water content that varies with altitude (10^{-3} gm^{-3} below 1 km, rising to 10^{-4} g m^{-3} at 4 km, following McCusker et al., (2023) then take the cloud base height as the lower boundary of the minimum grid box containing a cloud at each time step. The results are shown below in Figure R4.

Cloud base height from the ceilometer descends to approximately 100 m as the event arrives at the ship on 15th April. This low cloud base height is associated with low-level liquid clouds (c.f. Dada et al figure 7). This persists until a period of precipitation from approximately 18:00 on 15th April to just after midnight on 16th April, after which low-level clouds return, with base heights between 100 and 500 m.

While there are discrepancies in the base height of clouds in the background period (before 15th April 00:00), and delays in the onset of liquid clouds by the model, when the model produces low level liquid cloud during the event (dark blue colours, 15th April 12:00-18:00 and 16th April roughly 01:00 onwards), the modelled base heights agree reasonably well with the measurements.

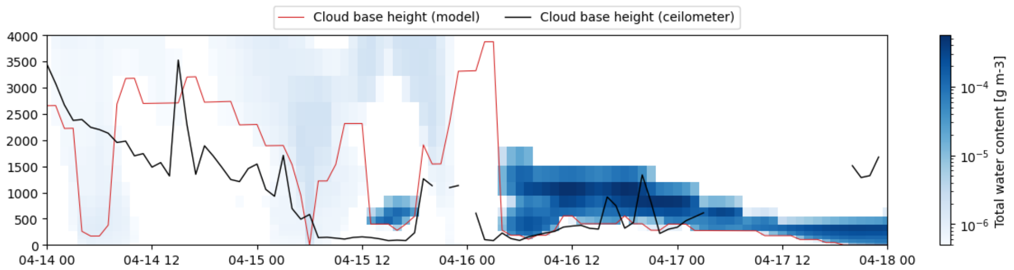

Figure R4: time series of cloud water content (blue), cloud base height from ceilometer (black line) and calculated model cloud base height (red line)

Overall, these additional comparisons to MODIS and ceilometer data increase our confidence in the ability of the model to capture the clouds during the event to a degree sufficient to study the cloud radiative effects of the aerosol perturbation.

2) I also felt that the aerosol validation was weaker than it should be. The manuscript validates aerosols primarily using surface measurements, but the aerosols most relevant for cloud interactions are those at cloud level. Over sea ice, strong temperature inversions can decouple surface and cloud-layer aerosol populations, meaning surface aerosol may not be representative of in-cloud aerosol. At minimum, the authors should discuss this limitation explicitly, but I feel just doing that alone would leave this a weaker paper. Thus, I recommend that the authors take advantage of the MOSAiC vertical aerosol constraints from balloon and lidar. I also recommend that they plot in Fig. 4 the values at cloud altitude from WRF-Chem-Polar. That will help demonstrate if the timing is similar at the altitudes that matter most.

We agree validation of aerosol at cloud level during these types of WAI events would be ideal. Unfortunately, the data coverage of the balloon and lidar profiles make this extremely difficult. Balloon measurements cannot be made during the stormy conditions associated with the WAI, and were only undertaken during MOSAiC during the summer months (Pilz et al., 2022). These measurements would be the most useful for model validation as they provide aerosol mass, number and some indications of the composition of the aerosols which are needed to understand how they impact cloud properties. Lidar profiles of extinction and backscatter provide some information on the vertical distribution of aerosol mass, although without the detailed information about number and composition that balloon measurements would provide. However, the lidar retrievals (black dots on time series showing AOD from the model and derived from the lidar, Figure R5 below) were also very limited during the WAI event and are not available for the main period when the WAI arrived at the R/V Polarstern. The timing of the lidar profiles confirms we would not be able to validate the aerosols using the lidar profiles during the largest pollution peak that arrives at the Polarstern during the WAI event.

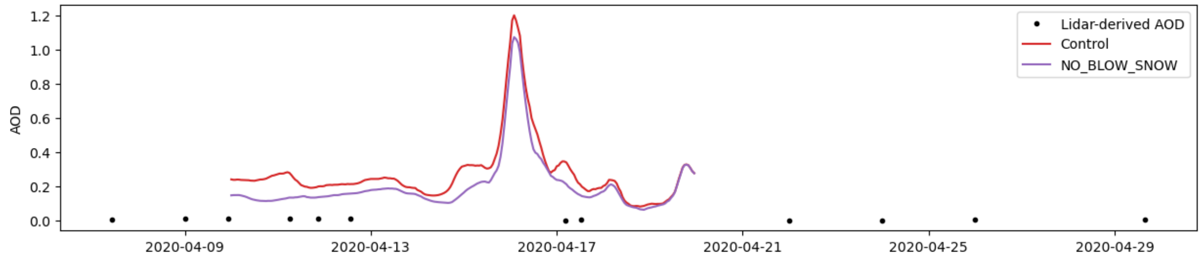

Figure R5: time series of AOD calculated by integrating lidar extinction coefficient over height (black dots) and from model output (solid lines). Difference between simulations is explained further in the text.

Figure R5 compares the model with AOD derived from the lidar observations, showing that the control simulation presented in the paper (red line) simulates a higher AOD than that derived from the lidar during background (non-WAI) conditions. We hypothesised that the reason for this overestimation is uncertainties in the amount, mass, and size of sea salt aerosols originating from blowing snow. We therefore ran a sensitivity study with no blowing snow emissions (purple line). However, removing blowing snow sea salt, while improving the agreement with background AOD conditions, does not change the main conclusions of the paper (see figure R6, which shows surface cloud radiative effect anomalies from the simulations with no blowing snow).

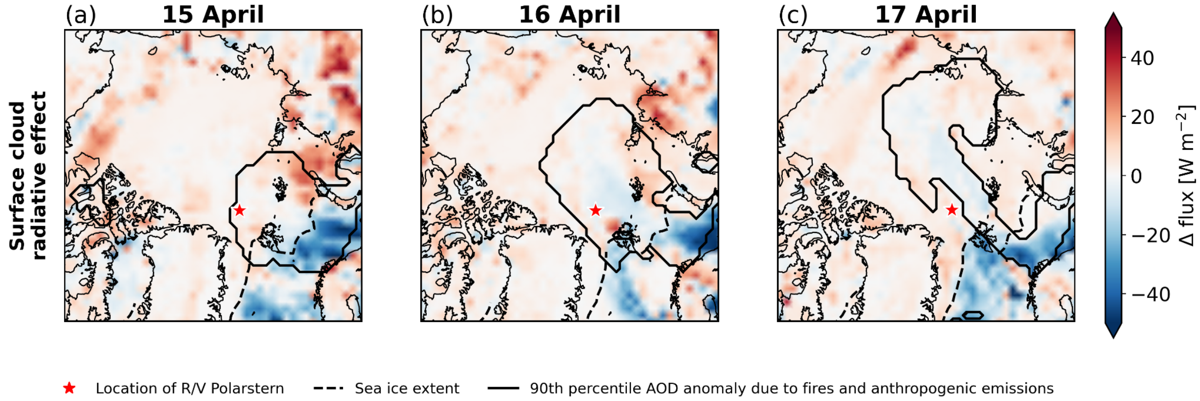

Figure R6: maps of daily mean anomaly in total (longwave + shortwave) surface cloud radiative effect due to pollution in simulations with no blowing snow (c.f. Figures 5g-i of main manuscript).

Given these limitations of balloon and lidar measurements for this event, and especially given that the main conclusions of the paper are not sensitive to the lower AOD from the change to the natural aerosol burden, we do not go further with this analysis in this present work.

3) The two-scheme approach is very commendable, but the manuscript's current framing suggests that agreement in radiative conclusions across schemes implies robustness in their results that meteorology is more important than aerosols. I don't disagree with the meteorology being more important. However, Figure A1 appears to show near-zero aerosols in the Morrison configuration, which would naturally minimize aerosol–cloud effects. In that case, similarity in radiative response just reflects the fact that aerosol perturbations are effectively absent, rather than a meaningful confirmation of aerosol insensitivity. The authors may want to more explicitly state that the Morrison run may not provide an informative aerosol sensitivity test given that aerosols are unrealistically low. I'd honestly just take that extra analysis out, but I leave that decision up to the authors and editor.

It is correct that the Morrison simulation has less aerosol at the surface than the Thompson simulation, but it has a similar aerosol loading aloft at cloud level (as shown by the aerosol vertical profiles at 12:00 UTC on 16 April 2020, shown Figure R7). We have added this to the supplement as Figure A3. We therefore feel that the Morrison simulations still provide a reasonable sensitivity test and have chosen to keep them in the paper.

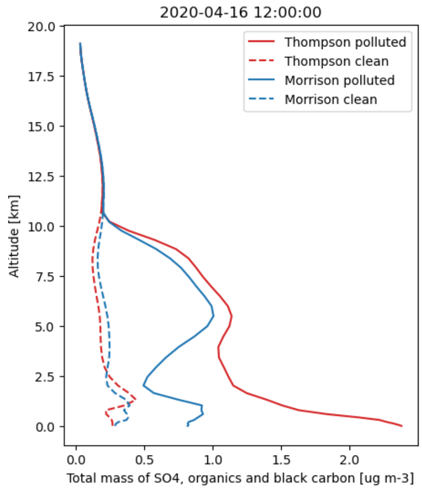

Figure R7: vertical profiles of aerosol mass (sulphate, black carbon and organics) from Morrison and Thompson simulations at 12:00 UTC on 16th April 2020.

4) The paper would benefit from more brief context (maybe a paragraph or so?) on how common WAIs are, their seasonality, and whether this event is representative of WAIs if they have that information. Additionally, the the conclusions should more clearly acknowledge that a) the study focuses on one extreme, moisture/heat-rich event, and does not explore cases where aerosols are transported without similarly strong thermodynamic forcing, and b) aerosol impacts on mixed-phase and ice-cloud processes are not fully explored, despite their potential importance in the Arctic.

Frequency and seasonality of Arctic WAIs: In general it is difficult to make robust conclusions on the frequency or seasonality of WAI events, since the results strongly depend on the dataset used and the methodology to define and identify WAI events. For example, Woods et al., (2026) (published since this manuscript was submitted) found an average of 228 events per year over 1979 - 2022, with a higher frequency of events in summer than in winter whereas Zhang et al., (2023) found an average frequency of events per year over 1980 - 2019 of between 256 to 559, and a higher frequency of events in winter than in summer, directly contradicting the Woods result. Due to the differences in methodology between the two studies it is hard to make an overall conclusion on the cause of the difference in the results. However, consistent across both of these studies is that the hundreds of WAI events occur in the Arctic every year, with tens of events in every season. We have added these references to the first paragraph of the introduction (lines 22-23):

“Studies suggest that on average hundreds of Arctic WAIs occur each year with events taking place during every season, although precise climatologies are limited by the large sensitivity to the data and methods used to define WAI events (Zhang et al., 2023; Woods et al., 2026).”

Representativeness of this event: this April 2020 event is placed into a larger context by the Kirbus et al. study. Considering surface energy budget anomalies and maximum changes in potential temperature and specific humidity, they find that the event as recorded at the ship caused very strong anomalies, at the upper end of but not exceeding the distribution of anomalies for other WAI events (e.g. matching the 90th percentile for potential temperature and humidity anomalies from a dataset of 50 Arctic winter WAIs between 1979 and 2016). Thus, this event can be considered representative of a very strong Arctic WAI. We added this text to the introduction at lines 43-45:

“The surface impacts of the event were particularly intense; Kirbus et al. (2023) showed that the potential temperature and specific humidity anomalies recorded during the event were equal to the 90th percentile of a dataset of the 50 most extreme WAIs between 1979 and 2016.”

Implications for this study: we have added the following text to the conclusions to highlight these limitations. New sentence at lines 279-281:

“Note that because this was a particularly intense WAI event in terms of the impacts on temperature, moisture, and surface energy budget, it is possible that equivalent aerosol transport during an event with a weaker thermodynamical forcing could produce a relatively stronger effect.”

and new text (bold) in an existing sentence at lines 243-245 to highlight the mixed and ice phases:

“We caution that the coarse horizontal resolution (100 km) may introduce limitations due to uncertainties in sub-grid cloud parameterisations**, including those related to mixed and ice-phase cloud processes and their interactions with aerosols, which we have not considered in this study.**”

More minor comments:

L. 16: “The Arctic is warming four times faster than the global average rate (Rantanen et al., 2022), a consequence of local climate feedbacks amplifying the effects of local radiative forcing and of warm air transported from the lower latitudes (Stuecker et al., 2018).”

I know it is probably a smaller factor, but isn’t greenhouse gas warming also a factor? That’s not necessarily a local feedback and it isn’t just from transported air.

Yes, this sentence warrants rephrasing. We have now changed it to:

“The Arctic is warming four times faster than the global average rate (Rantanen et al., 2022), driven by greenhouse gas emissions and amplified by local climate feedbacks and warm air transported from the lower latitudes (Stuecker et al., 2018).”

L. 24: “some studies have indicated a connection between springtime Arctic events” it would be helpful to be more clear about what kind of events is meant here (e.g., number or intensity of WAI events)

The Kapsch paper does not go into details about whether higher number or intensity of WAI events drives this connection. They simply conclude that high cloud and water vapour anomalies produce a stronger surface downwelling longwave flux which is then linked to the sea ice loss. To make this clearer, this sentence now reads:

“some studies have indicated a connection between springtime Arctic WAI events and autumn sea ice extent during years in which the Arctic sea ice minima is below average (e.g. Kapsch et al., 2013), a connection that is driven by the high downwelling longwave flux from cloud and water vapour anomalies caused by WAI events”

l. 35: “strong aerosol transport associated with WAIs can lead to extreme deposition events of light-absorbing impurities onto ice and snow (Thomas et al., 2017)” I think I’d feel more comfortable with a different word than “extreme” here, since the authors of that Thomas et al. paper did not use that word and my reading of that paper does not support that word either. Maybe “concentrated”, “considerable” or “significant” would be more appropriate?

Yes, we have changed this to “considerable”.

l. 37: maybe mention that this is the same event that the authors will be describing in this paper, and how specifically it differs from those other papers, especially as these papers are brought up again in the conclusions.

We have expanded on this text, creating a new paragraph starting at the position of line 37 in the old version of the manuscript. We hope the new text provides more helpful context about the existing papers and how the current study complements their results. The paragraph reads:

“The WAI event that we investigate here was observed during the Multidisciplinary drifting Observatory for the Study of Arctic Climate (MOSAiC) expedition. Dada et al. (2022) presented in-situ aerosol measurements that show the event carried an extremely high aerosol burden and caused a strong perturbation to CCN concentrations. This suggests that the high aerosol burden impacted the microphysical properties of the clouds and affected the radiative budget at the location of Research Vessel (R/V) Polarstern during MOSAiC. Kirbus et al. (2023) investigated this further alongside their characterisation of the synoptic and thermodynamic drivers of the event. Using a high-resolution large eddy simulation (LES) model to test the sensitivity of the cloud and boundary layer structure to different prescribed CCN values, they characterise the impact that a CCN concentration representative of the polluted conditions has on the clouds via processes such as cloud-top longwave cooling and entrainment. However, since their model setup is necessarily limited to a few hours of the event over a spatial domain of a few kilometres around the ship, they could not provide further details about the large-scale surface radiative impacts of the event.”

Fig. A1: The caption says, "as in Fig. 1", but they use a different color scheme than Fig. 1. I recommend that they maybe use the same color scheme or mention that the blue is for the Morrison scheme.

The caption is now: “Figure A1. As Figure 4, but showing WRF-Chem-Polar model output using the Morrison cloud microphysics scheme (solid blue lines).”

Fig. 5 (g-i). Is this net (LW+SW) CRE? Please specify in the figure/caption.

Yes it is LW+SW, this information has been added to the caption.

l. 197: "Regions of increased cloud fraction do lead to surface longwave warming (not shown), however, these regions that experience longwave warming are not widespread and are typically offset by shortwave cooling."

It seems that Fig. 5 shows that there is some important regional variability, which should be mentioned here. It might also be helpful to show regions of increased cloud fraction in Fig. 5.

The primary feature of spatial variability shown in Figure 5, other than the straightforward relation to the position of the polluted air as marked with black solid lines, is the difference in cloud response over open ocean vs. sea ice, as discussed in the text. There is also some amount of noise in each variable outside of the area of the polluted air, which is generally of a smaller magnitude than the effects inside the polluted air, as expected.

Below (Figure R8) we have plotted the cloud fraction anomaly along with the individual components of the surface cloud radiative effect (longwave and shortwave). Spatially-limited areas of cloud fraction increase can be seen in the polluted air, for example on the 15th April over the ocean at the edges of the pollution plume. Comparison between panels (a) and (d) demonstrates that these areas of cloud fraction increase are associated with increased surface longwave flux, as mentioned in the text. However, as we see on panel (g) (surface shortwave cloud radiative effect) and on Figure 5(g) in the main manuscript (surface total cloud radiative effect), these longwave increases are mitigated by the concurrent negative anomalies in surface shortwave cloud radiative effect and therefore do not have a substantial overall effect on the surface fluxes. As such, we do not feel that these extra figures add much information to the discussion beyond what is already written in the text.

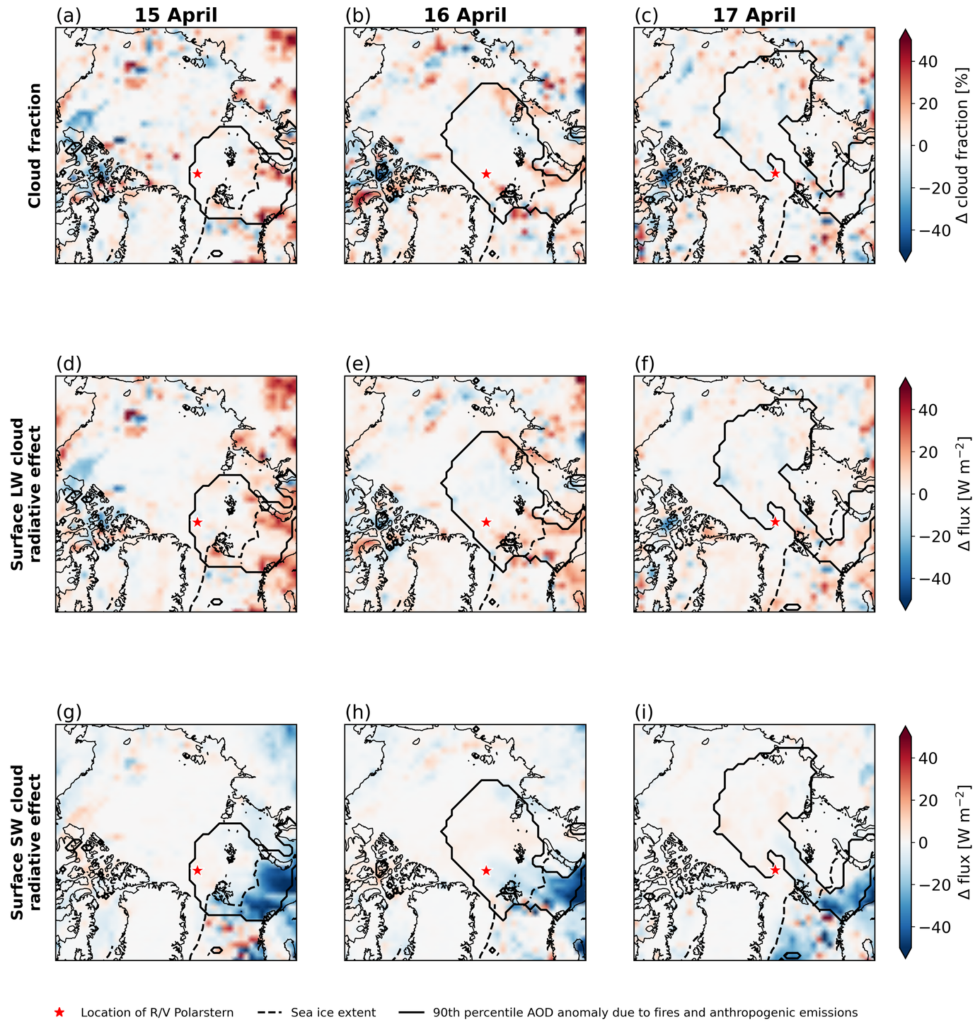

Figure R8: Same as Figure 5 in the main manuscript but for different variables: maps of daily mean anomaly of (a-c) cloud fraction, (d-f) surface longwave cloud radiative effect, and (g-i) surface shortwave cloud radiative effect.

L. 200: "Distributions of cloud droplet number concentration and cloud fraction are calculated for altitudes between 0 and 2 km, corresponding to the altitudes where clouds are simulated in the model" Is this consistent with what the cielometer/lidar indicated was the cloud base?

Yes, for low-level liquid clouds during the event. Please see Figure 4 above where we have plotted cloud total water content and cloud base height from model and ceilometer.

l. 225/ l. 249: The role of sea ice and season is important; however, lighting conditions (polar night vs daylight) also strongly modulate radiative impacts. I suggest the authors briefly discuss how results might differ during polar night and how often WAIs occur in darkness vs daylight.

The lack of a shortwave response during polar night would indeed impact the overall cloud radiative effect of an event like this. We have added the following sentence at lines 263-265 to highlight this:

“Note that this dependence of radiative effect on surface type is specific to this season, whereas in polar night this shortwave effect would not take place and the total cloud radiative effect would therefore be controlled by the longwave response (in this case a weaker effect).”

as well adding “incoming shortwave radiation” to the last sentence of the paragraph (line 270) to emphasise the role of lighting conditions.

The literature is inconsistent about the relative frequency of WAIs during polar night vs polar day, as per our response to major comment 4, but in our edits for that comment we added references to the introduction that show that despite the lack of agreement on relative frequency of events, studies consistently show that WAIs can be observed in all seasons.

L. 261: The statement appears too general given that the study focuses on a single extreme event with strong moisture and heat transport. I suggest that the authors rephrase to avoid overgeneralization and to clarify that the results may be very different if aerosols are introduced without the same moisture and heat content

We have rephrased the beginning of the sentence to emphasise the dependence of the results on the specific conditions of this event. The sentence now begins “For this extreme event that occurred during present-day Arctic spring...”

We have also added this text later in the paragraph (lines 291-294): “Moreover, the relative importance of aerosol forcing could be greater for events that are less intense than this one was in terms of the thermodynamical forcing on the surface energy budget. Thus, the radiative effects of aerosol transport also depend on trends in moisture and energy transport driven by WAI events, which are subject to large uncertainties (Zhang et al., 2023).”

L. 268: "Comprehensive modelling and observational approaches are needed to understand this evolving system, capable of capturing the interacting effects of sea ice loss, shifts in large-scale dynamics, and changing aerosol emissions." Aerosol loss processes and mixed and ice phase cloud processes and their interactions with aerosols are also important and can also change with climate.

We have changed “emissions” to “processes” to account for the fact that other factors than just emissions are important here.

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