Response to reviewers by Herbert and co-authors.

We thank the three reviewers for providing comments and suggestions. There were several key comments that were consistent amongst the reviewers. We are confident that we have addressed all of these in the revised manuscript. We outline the major revision that has been made to the manuscript first then provide a point-by-point response to each comment.

Following suggestions by all reviewers we have made a major revision to the manuscript in order to better focus the manuscript and make better use of the novel elements of the experiments. The section on the global responses due to the aerosol perturbation has been removed and replaced with new analysis that uses the temporal decomposition method used for the regional analysis. We divide the globe into 15° x 15° regions and temporally decompose the response of cloud properties, precipitation, and radiation over the 30-day time series to obtain a composited diurnal response. With this method we can mitigate or reduce the role of internal variability and provide a new global-scale perspective on how anthropogenic aerosols impact clouds in our convection-permitting model (an example of the analysis is shown below in Fig. R1). In the new section we show the mean diurnal responses of LWP, precipitation, and TOA shortwave radiation on the global-scale. We also show the spatial distribution of the dominating process (ARI or ACI). This new section nicely frames the focus for the remaining results section and helps to strengthen the conclusions.

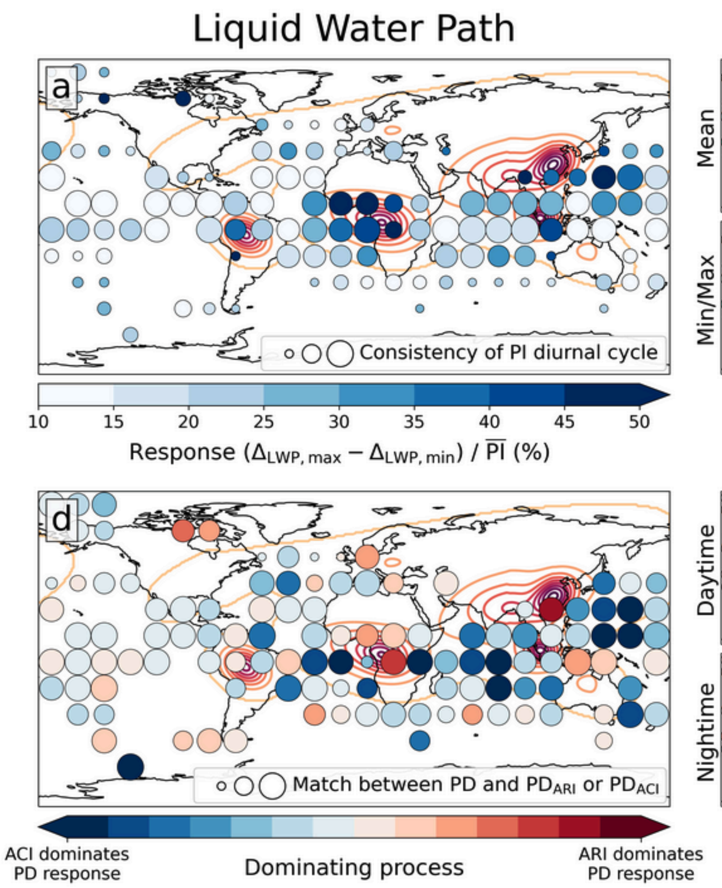

Figure R1. Mean diurnal response of LWP to the aerosol perturbation in the PD experiments from each 15° × 15° region. Panels **a** – **c** show the magnitude of the response as a percentage (**a**), absolute daily mean (**b**), and absolute daily minimum/maximum (**c**). A larger circle size in **a** – **c** represents a location with an increasingly consistent diurnal cycle throughout the PI simulation.

Panels **d** – **f** show the dominating process (ARI/ACI) driving the total LWP response during the diurnal cycle (**d**), day (**e**), and night (**f**). A larger circle size in **d** – **f** represents a better match between the individual response (PD_{ARI} or PD_{ACI}) and total response (PD). All panels show the AOD perturbation as contour lines at 0.05 increments.

Point-by-point responses

In the following pages we provide point-by-point responses to each comment. Reviewer comments are in blue. Author responses are in black and indented with new/revised text in red.

Reviewer #1

This study investigates the impacts of anthropogenic aerosols on radiation and cloud properties using a global convection-permitting model. Four 40-day simulations are conducted to isolate the aerosol impacts via the aerosol-cloud interaction and the aerosol-radiation interaction. The methodology and analysis are straightforward. However, several severe flaws in the manuscript prevent its publication in its current form.

Major comments:

Sections 2.2 and 2.3 are unclear. I don't understand what the authors changed for the three PD simulations. Only aerosol concentrations? Please provide more details.

> As suggested by the reviewer we have rewritten the two sections to provide more detail and clarification.

The authors mentioned deep convection throughout the manuscript but never provided any quantitative analysis about how much of the responses are for convection or large-scale environments. Even if the authors select several regions with frequent deep convection, I don't think convection is the only precipitating process in those regions. For example, how do you know how much precipitation is from deep convection?

> In our study we are primarily focused on the regional response, through which we can scale up to the global scale. The response of a single cloud is not necessarily representative of the regional-scale cloud field. Hence we are not trying to isolate every single deep convective core and attribute precipitation changes to it, but instead try and quantify the regional-response with the additional realism that the convection-permitting resolution provides. As we are focused on the regional scale it is difficult to quantitatively isolate the precipitation from convective cores and the larger-scale. Our model does not have an explicit distinction between convective (deep/shallow scheme) and large-scale precipitation that a typical GCM does.
>
> We use the diurnal composites to link the different responses together - through which we are able to highlight the role of deep convection in some regions, and large-scale ascent / subsidence in others. Deep convection is not the only precipitating process in the deep convective regions, but the diurnal cycle composites

clearly show a strong response coincident with the timing of deep convection, and this is supported by other variables.

In response to the reviewer's comment we have decomposed the 1-degree precipitation response for grids where there is ascent at 500 hPa. This provides some degree of separation between all grids and those that are more likely to have convection. The analysis (Fig. R2) shows strong consistency between the regional responses, consistent with our hypothesis that the precipitation response is occurring in the convective cores. For ARI in the Congo and the Amazon the magnitudes are equal, demonstrating that all the effects occur on ascending grid points. In the Maritime Continent, there is an additional impact on large-scale precipitation (persistent increase across the diurnal cycle) which is consistent with our analysis and reasoning. In Fig. R3 we additionally analyse the distribution of precipitation rates across each convective region using the 5-km resolution output. ARI and ACI impact precipitation across all intensities (Fig. R3 a – c) but the biggest impact to the total precipitation occurs at precipitation rates above 10 mm hr^{-1} (Fig. R3 g – i), which are magnitudes associated with deeper convection rather than shallow convection. We include these two plots in the supporting information and refer to them in the revised manuscript.

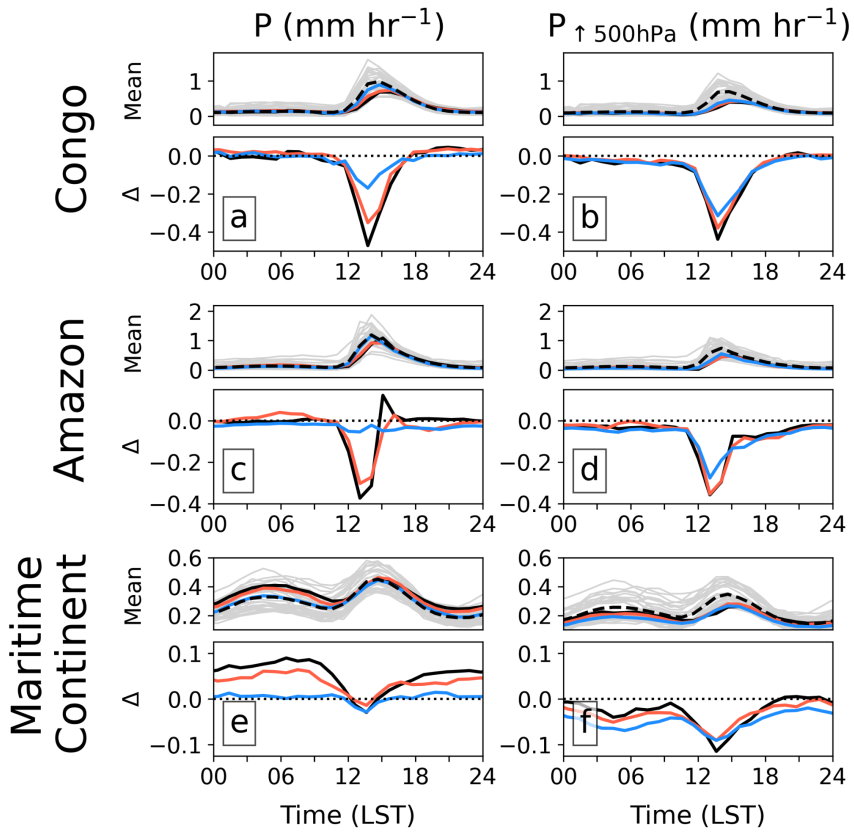

Figure R2. Decomposed diurnal response of 1-degree precipitation in the convective regions for all grids (left column), and those with ascent at 500 hPa (right column).

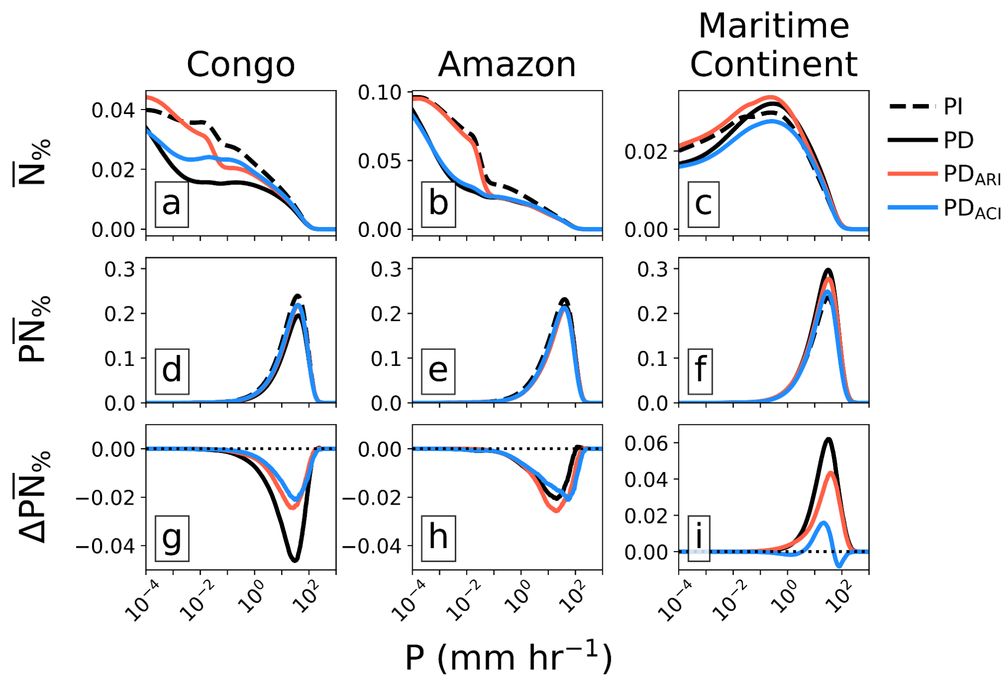

Figure R3. Distribution of time series mean precipitation rates in each of the three convective regions. Rows show the percentage of grid cells ($\overline{N}_{\%}$) in each bin dlog$_{10}$P (**a** – **c**), the total precipitation rate ($P\overline{N}_{\%}$) per bin (**d** – **f**), and the change in total precipitation rate ($\Delta P\overline{N}_{\%}$) per bin (**g** – **i**). Total precipitation rates ($P\overline{N}_{\%}$ and $\Delta P\overline{N}_{\%}$) for the Congo and Amazon are shown on the same scale to aid comparison.

The revised text is as follows:

The Congo and Amazon are strongly perturbed by anthropogenic aerosol from biomass burning sources. The primary driver is a localized modification to the convective environment that reduces daily accumulated precipitation by 1 mm day^{-1} in both regions (~15 % and 10 % of PI values for Congo and Amazon). The precipitation response is associated with ascending regions and precipitation rates greater than ~10 mm hr^{-1} (Figs. S6 and S7), linking the changes to convection.

The discussion of the results is verbose, while the reasoning is oversimplified throughout the manuscript. The latter severely degrades the quality of the study. Please explain your results more logically, not just provide some simple assumptions. Please find further details below.

We address this comment in several ways. We have removed the global-response section which, as pointed out by this and other reviewers, did not fit the focus of the study. A new section using the temporal decomposition tool (Section 3.1) expands the regional analysis to the full global coverage, which helps to refocus our study and improve the quality of the analysis and conclusions. We have gone through the manuscript and removed unnecessary passages of text and made the analysis more succinct. Where necessary we have provided additional reasoning or support to explanations and hypotheses.

Minor comments:

Line 7: Please provide the full name of "ICON" for its first appearance.

That has now been included.

Line 39: What do you mean by lack of memory?

'Lack of memory' refers to the tendency for convection on the regional scale to exhibit a memory effect (Colin et al., 2019). Past instances of convection can influence the initiation and development of future convection. We have changed 'lack of memory' to 'lack of convective memory' and cited Colin et al. (2019).

Line 43: "Limited area" to "Regional"?

This has been updated as suggested.

Line 44-45: What do you mean by large-scale controls on the availability of energy and water vapor?

The reviewer is referring to the lines:

"Limited area high-resolution models provide useful process insights (Marinescu et al., 2021), but their global representativeness and ability to respond to regional drivers remain unclear, and they do not satisfy large-scale controls on the availability of energy and water vapour (Dagan et al., 2022)."

Dagan et al. (2022) showed that the response of clouds to aerosol perturbations in regional-scale simulations is highly sensitive to the representation of the larger-scale thermodynamic environment (boundary conditions in a regional configuration). In some instances the idealized nature of the applied boundary conditions (e.g., time-independent forcings or a relaxation towards domain-mean properties) may end up controlling the regional-response, rather than the perturbation.

In response to the comment we have rewritten the sentence as:

..but their global representativeness and ability to respond to regional drivers remain unclear, and may not sufficiently represent the interaction between the large-scale thermodynamic environment and the regional scale (Dagan et al., 2022).

Line 62: What is the resolution of NICAM?

The NICAM resolution used in Sato et al. (2018) was 14 km. This has been included and the full name of NICAM has also been provided.

Line 66: "or" to "nor"?

Rewritten as suggested.

Lines 74 and 75: The full names of ICON and MACv2-SP?

Rewritten as suggested.

Line 112: add “separately” after “cloud water.”

Rewritten as suggested.

Line 115: What is SLEVE?

SLEVE is the acronym given to a terrain-following coordinate formulation called the smooth level vertical (SLEVE) coordinate.

Whilst making the manuscript less verbose we have removed reference to this acronym and refer to the model description paper (Hohenegger et al., 2023) instead.

Lines 140-141: Any references for such an adjustment?

We will add references as suggested. The sentence now reads:

These figures are uncertain (Hamilton et al., 2018) and may substantially underestimate the anthropogenic contribution (Lauk and Erb, 2009). In our simulations, we enhance the anthropogenic component to 75%, which is consistent with higher estimates (Lauk and Erb, 2009, and references therein) and should provide a stronger signal in response to our perturbation

Line 152: Figure 1 shows an overestimation of Nd. Do you have any statistical calculations validating the changes of aN and bN?

The line in question is “This relationship provides more sensitivity than the original used in Stevens et al. (2017), but as we show in Fig. 1 results in a present day distribution of Nd consistent with observations.”

The purpose of the experiment design is to examine and isolate the processes through which the aerosols impact the regional-scale climate. We have used a prescribed and idealized global aerosol perturbation in order to achieve this. The values of aN and bN were taken from a previous study that used in-situ values to make an empirical fit between AOD and in-cloud Nd. The authors used the parametrization to provide a robust perturbation in a study designed to focus on ARI and ACI-related processes. We follow a similar approach.

The spatial distribution and magnitude of AOD and Nd in the PD experiment are consistent with the ocean-limited coverage of observations (as shown in Figure 1) and provide a plausible perturbation. We also focused on the month of September to gain the largest global-mean perturbation. The idealized setup is therefore not designed to provide a quantitative estimate of the anthropogenic aerosol forcing as in AeroCom style experiments, and we do not provide this.

A direct evaluation results in global means of 105 cm^{-3} (observations) and 103 cm^{-3} (model) and a correlation coefficient of 0.34. The poor correlation is primarily due to widespread overestimation of Nd over the SE Atlantic Ocean and the Indian Ocean. These are the regions that are directly influenced by the biomass burning aerosol and thus unsurprising that the comparison is weak. We also overestimate in the Pacific Ocean and underestimate close to the coastal regions of Europe and North America. This does demonstrate some weakness in the method, but is still reasonable given our focus on impacts to aerosol processes, rather than providing robust estimates of anthropogenic aerosol forcing.

In the revised manuscript we include the following text in Sect 2.3

A comparison between the simulated and observed N_d (limited to the ocean) yields similar means (105 cm^{-3} and 103 cm^{-3}) but a low correlation coefficient of 0.34. This is in part due to high simulated values over the biomass burning regions that are not reflected in the annual mean observations, but also due to regional variability that MACv2-SP does not capture. Despite the poor correlation, our idealized representation of aerosol provides appropriate perturbations to the radiative fluxes and bulk cloud properties that are spatially consistent with the dominant sources of global anthropogenic aerosol forcing.

The response to the following four comments has been combined

Section 2.2: The description of aerosols in ICON needs more detailed clarification. Which type of aerosol variables does ICON need? How do you consider pre-industrial and present-day aerosol conditions? The current description is confusing. Did you only add fN in the model? Where are Nd,cld and Nd,sfc from? Does ICON contain any aerosol microphysical processes (processes converting emissions to aerosol concentrations), or are aerosols entirely prescribed in the model (input aerosol concentrations, AOD, etc.)?

Lines 172-178: I am confused with what MACv2-SP provided to ICON. Anthropogenic aerosol concentrations?

Lines 179-180: Please rewrite this sentence.

Lines 179-185: Both PI and PD simulations contain biomass-burning sources. What are their differences? In Line 132, you mentioned that in MACv2-SP, biomass-burning emissions are anthropogenic sources. Could you please provide more details about that?

All of these comments refer to the aerosol representation (section 2.2) and coupling to the microphysics/radiation schemes (section 2.3). As suggested by the reviewer we have rewritten and expanded both sections to provide clarification / additional detail.

Lines 212-213: Can internal variability be eliminated by only focusing on regional responses?

The lines in question are

"The limited length of our simulations poses some issues as it makes it difficult to disentangle this internal variability from the global-scale responses to aerosol effects. Due to this, we will only briefly discuss the global scale responses, and instead focus on the regional responses due to aerosol, allowing us to mitigate or reduce the impact of the internal variability by compositing over multiple diurnal cycles."

The most prominent internal variability we observe is in the Southern Ocean (see Figure 3), where frontal features are often separated by 1000s km (e.g., Southeast Pacific) by the end of the simulation. However, over much of the subtropics and tropics, where our aerosol perturbations primarily are, the spatial heterogeneity occurs on much smaller scales (e.g., Southeast Atlantic Ocean). By focusing on regional domains there is a reasonable chance that we mitigate or reduce the impact of internal variability. Additionally, we composite the response onto single diurnal cycle, which increases the number of data points by a factor 30, further helping to reduce the 'noise' of internal variability (Noise $\approx 1/\sqrt{N}$).

In the current text we acknowledge that this method allows us to mitigate or reduce the impact, rather than eliminate it. This is demonstrated in a new figure (Figure R4; introduced in a comment below), which will be included in the supporting information. We also point out that the lines in question have been deleted along with the section.

Line 229-230: How did you know it is due to internal variability but not actual model differences? Internal variability refers to variability due to natural internal processes within the climate system. But here, you talked about the differences between the two simulations.

Line 234: Again, how did you know it is due to internal variability?

We believe these comments refer to lines 219-220 and 224. These lines have been removed (as part of Section 3) but as we discuss internal variability throughout the manuscript this comment is still valid and requires a full response.

The aerosol perturbation (the difference between the two simulations) will result in a redistribution of energy. Some of this will be coherent, e.g., increased cloud liquid water, but some will be random (e.g., a slightly misaligned front), and help to drive the spatial heterogeneity we observe. This redistribution may occur over seasonal/decadal timescales but in our case this is occurring over days. In both cases we would argue that it is reasonable to describe the redistribution as internal variability of the system, and is consistent with Schwarzwald et al. (2022):

> "Internal variability consists of the naturally occurring variations in climate on timescales from daily weather to multidecadal processes due to interactions between various components of the Earth system. Internal variability is made

up of components that are predictable, such as the El Niño–Southern Oscillation, as well as irreducible uncertainty due to the chaotic nature of the system."

However, in the instances where the misaligned front or fractionally strengthened tropical cyclone is due to changes in the aerosol perturbation rather than internal variability our methodology will not be able to account for this. Hence, we only suggest internal variability as a likely candidate for the unaccounted responses in the temporal decomposition.

In response to the reviewer's comment we have clarified what we refer to as internal variability with the additional text below.

"The limited length of our simulations poses some issues as it is difficult to disentangle internal variability from the global-scale responses to aerosol effects. By internal variability, we refer to the chaotic nature of the atmosphere, whereby small fluctuations grow rapidly in time. For example, Fig. 2 shows that as the simulation progresses the changes in aerosol concentration have large-scale impacts on the precise timing and location of atmospheric fronts, which appear as a regional change when differencing simulations, but are not usefully considered as a robust 'aerosol effect'. This behavior is similar to initial condition sensitivity where small-scale perturbations at the beginning of the simulation can quickly develop into pronounced changes (Keshtgar et al., 2023; Lorenz, 1963)"

Line 253: "internal variability" to "spatial heterogeneity." Please check the whole manuscript to ensure the correct terms are used.

See response above.

Line 235: add "outgoing" before "shortwave radiation."

The section and line in question has been removed, but we have clarified in further instances where appropriate.

Lines 269-271: Would applying the tool to Section 3 (original model data but not differences) be better also to remove your so-called "internal variability"?

We believe this is an excellent suggestion and we thank the reviewer. Following similar comments from the other two reviewers we chose to make major revisions to the manuscript. This is outlined below, with more detail provided at the beginning of the document.

We have replaced Section 3 (Global response) with a new section where we apply the decomposition tool globally, as the reviewer suggests. To achieve this we subset the globe into 15x15 degree regions, and applied the decomposition tool to each region. We apply thresholds to the response in each region to focus on regions where a robust response is observed, and we use the remaining data to examine the role of ARI and ACI on the regional-scale, but for all regions of the globe simultaneously.

This major revision has greatly improved the focus and flow of the manuscript. To maintain this focus we have changed the title of he manuscript to “Variability of regional-scale aerosol impacts on clouds and radiation in global kilometre-scale simulations”

Lines 279-283: There is an assumption here: the long-term component is constant and independent of time. Is it correct? At least Figure 7f doesn’t show that!

We assume that the long-term component of the decomposition may include a persistent (time-independent) response along with internal variability (time-dependent). A persistent time-independent response may be, for example, enhanced subsidence, an overall increase in LWP, or a persistent increase in cloud fraction.

The contrasting time series of the persistent response and internal variability is well captured by Figure 7f. In the long term response there is evidence of consistently enhanced LWP in the PD_{ACI} and PD experiment, with a sharp decrease in the middle of the time series spanning 10 d. We attribute this sharp decrease to internal variability: All perturbation experiments share the same 10 d time series, strongly suggesting a synoptic feature in the PI experiment that was not present in any of the perturbation experiments. A similar feature can be seen towards the end of the long-term time series in Figure 7c (The Congo).

The introduction of the decomposition method is now in a new section (Sect. 2.4 Temporal decomposition of regional response) and has been rewritten to provide more clarity on how we isolate the persistent response from internal variability, and any assumptions and limitations. We have also included Figure R4 and associated text in the supporting information to further demonstrate the decomposition method.

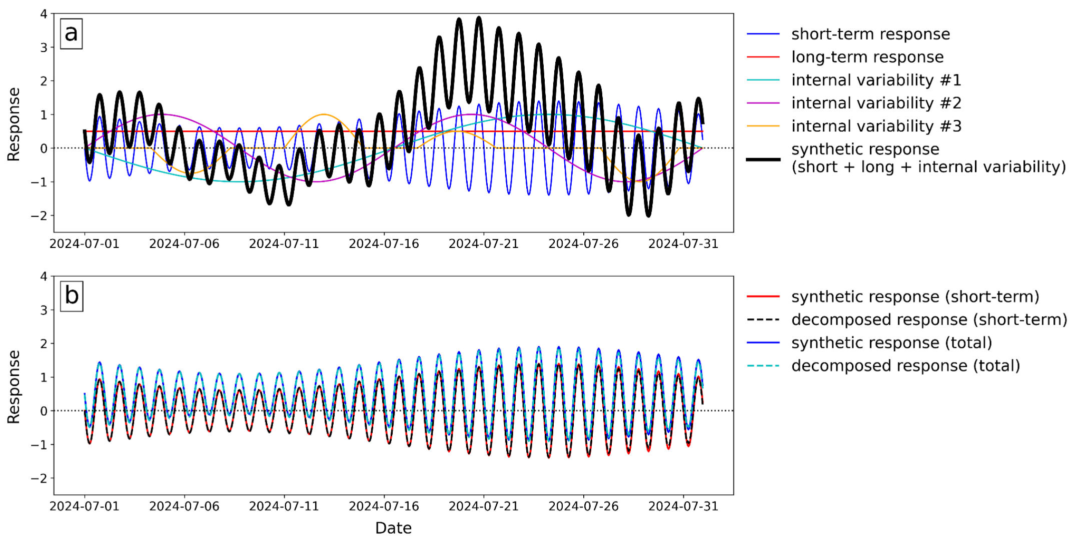

Figure R4. Figure demonstrating the capability of the decomposition method. A synthetic response in (**a**) includes a short-term diurnal component with a time-dependent amplitude, a long-term persistent time-independent component, and internal variability from multiple sources. The aim of the decomposition method is to sufficiently isolate any underlying short-term response along with a persistent response. The result of the decomposition method from the manuscript applied to the synthetic response (short-term + long-term + internal variability[1-3]) is shown in (**b**).

Associated text in the revised supporting information:

Figure S1 demonstrates the ability of the temporal decomposition method to isolate short-term (diurnal) and long-term (persistent) responses amid internal variability. Multiple sources of synthetic internal variability / noise are added to a diurnal response with day-to-day variability in amplitude and a persistent (time-independent) response. This is shown in Fig. S1a. Our method aims to isolate the responses from the internal variability. Figure S1b shows the result of applying the decomposition method (Sect. 2.4). The method successfully isolates the responses. The total decomposed response is slightly higher than the synthetic response due to an imbalance in the synthetic internal variability from the third source. This demonstrates a weakness in the method when internal variability is strongly weighted towards a single direction of response (positive/ negative).

Line 286: Delete “with the SE Atlantic region”.

This line was removed whilst making the manuscript less verbose.

Line 305: Do you have an explanation for the nighttime enhancement?

Response in question: ARI causes slight nighttime enhancement of LWP in the Southeast Atlantic.

There is a corresponding diurnal response in the liquid cloud fraction which drives the LWP response. The cloud fraction is enhanced due to an increase in latent heat flux from the surface to the atmosphere (~ -15 Wm^{-2}) which drives increased water vapour in the lowest 2 - 3 km. Additional heating due to the absorbing aerosol ‘burns off’ the cloud during the daytime, but as temperatures cool in the night the enhanced water vapour results in more cloud.

We have included the vertical profiles of LWC, IWC, W, theta, and Qv for the three non-convective regions in the supporting information. We also include additional decomposed variables of surface latent heat flux and liquid cloud cover. We refer to these in the revised manuscript.

Line 319: Does ACI increase cloud cover? Did you refer to Figure 4h?

Line in question:

“Over the Maritime Continent there is an additional enhancement of LWP throughout the diurnal cycle of +10%. This reflects the prevalence of low-level marine clouds over much of this region and may be partially driven by enhanced cloud cover.”

ACI increases liquid cloud fraction by ~2.5% over the Maritime Continent. The cloud fraction response was originally included in Figure 9 but was removed prior to submission. The cloud fraction response over the Maritime Continent (and other regions) can now be found in new figures included in the supporting information.

The revised text reads:

Over the Maritime Continent there is an additional enhancement of LWP throughout the diurnal cycle of +10 %. This reflects the prevalence of low-level marine clouds over much of this region and is associated with enhanced cloud cover (Figs. 8b and S4) and evaporation from the ocean surface (Fig. S5).

Line 345: Please provide more details about how you constrain ascending air masses. Using hourly or daily data?

We constrain two variables to ascending air masses: M_{flux} at 500 hPa and profiles of W. For M_{flux} we remove all 1° data points where W_{500hPa} is negative throughout the time series. A mean of the remaining data is taken at each timestep. For profiles of W↑ we have changed our method. Originally we constrained W to 1° grid points where W_{500hPa} was positive. However, when there is a decrease in the number of updrafts within a region (as we hypothesize with ARI) the mean profiles of W↑ from PD and PI are not directly comparable, and of less use to the analysis. In our revised method we constrain W to the PI grid points where deep convection is likely to be occurring. This is achieved by taking a time series mean W_{300hPa}, which coincides with the altitude of maximum vertical velocity. Any grids with a positive W_{300hPa} (ascent) are used to subset the PD experiments. We have renamed this variable W*.

We did not describe the remapping method previously, and so include a description of this in the revised methodology. By regridding the data we have already smoothed some of the ascending regions, so it is more appropriate to refer to this as ascending regions, rather than individual updraughts. We provide more detail on this and the constraining method as suggested by the reviewer in several locations:

In Section 2.4:

Data from the 5 km grid is regridded onto a regular 1° grid using the Climate Data Operators (CDO; http://www.idris.fr/media/ada/cdo.pdf, last access: 20 January 2025) software operator *gencon* which generates first order conservative remapping weights. As we focus on regional responses, we do not lose any information through the re-gridding process.

In Section 3.1:

Figure 7 shows the local solar time (LST) of maximum response during the diurnal cycle for LWP, ice water path (IWP), precipitation, and the cloud condensate mass flux at 500 hPa (M_{flux}) constrained to grid points throughout times series where W_{500hPa} is positive.

In Section 3.2.2

Vertical velocity profiles are calculated on 1° grid boxes where the mean W_{300hPa} across the PI time series is positive.

Line 349: Why not decompose them given that you did it in Section 4.1?

We were unable to decompose the profiles as output on all pressure levels was 3-hourly, which is insufficient for the decomposition tool. We have clarified this in the revised manuscript:

The frequency of output on all vertical levels is insufficient (3 hr) to robustly decompose the time series following Sect. 2.4, hence the profiles include influence from internal variability. To minimise this, the responses are composited onto a single diurnal cycle. Additionally, the limited day-to-day variability evident in Fig. 10 for the regions provides confidence that the responses are primarily due to the aerosol perturbation.

Line 357: Does the profile of potential temperature changes align with the anthropogenic aerosol loading profile? Do you have any model output showing the suppression of boundary layer mixing and drying aloft? How do you define the boundary layer? 1 km? 2 km?

The aerosol profiles are consistent with the region of maximum increase in theta (the lowest 4 km). We do not define the BL nor estimate its height, and agree that this is misleading. The profile responses are consistent with suppressed mixing in the lower atmosphere, and a reduction in Qv between 3 and 5 km demonstrates drying aloft.

We have rewritten this sentence for clarity and we have included profiles of the aerosol optical thickness perturbation with each of the variables.

Line 360-362: Did you mean the daily mean or afternoon mean? If you meant afternoon mean, Figures 11a and 11d showed that Amazon has weaker w effects than Congo. If you meant daily mean, Figure 8 shows the ARI effect is the most significant in the afternoon. Please provide more reasonable descriptions and explanations for the results.

The magnitude of W is greater over the Congo than over the Amazon. Hence the same absolute decrease in W equates to a stronger perturbation in the Amazon when compared to the Congo. This should have been clearer. We have rewritten this sentence to improve clarity.

This is consistent with the findings of Herbert et al. (2021). However, the percentage response of the W* profile and LWP is greater in the Amazon and suggests the differences may be due to stronger capacity to buffer the perturbation over the Congo, which tends to exhibit more convection than the Amazon, or differences in the convective environments (Storer et al., 2010).

Line 367: How does the delayed release of CAPE increase high-altitude IWC?

The absorbing aerosol stabilises the thermodynamic profile via ARI (Wang et al., 2013), yet CAPE is still produced. Herbert et al. (2021) found that over the Amazon the CAPE was released later in the afternoon, driving some convection yet not to the full extent as without the presence of aerosols. Relative to the control simulation, IWP was suppressed during the early afternoon but enhanced later in the evening when the delayed convection occurred. This is consistent with our results: IWP is strongly suppressed during the early afternoon and weakly enhanced during the evening.

We have rewritten this statement to clearly describe our reasoning.

Changes to convection and the vertical transport of condensate strongly suppresses IWP during the afternoon, with a smaller enhancement during the evening. This is consistent with Herbert et al. (2021) who found that absorbing aerosols over the Amazon caused the accumulation of convective available potential energy (CAPE) to be released later in the afternoon, driving some convection yet not to the full extent as without the presence of aerosols.

Line 367-370: How did you know convection occurred certainly given increased CAPE aloft? More evidence is necessary to support such types of conclusions.

We have removed this sentence.

Lines 380-381: Which variables did you refer to?
Lines 381-383: Please provide more details about the reasoning.
Line 391: What do you mean by delayed CAPE? In addition, how can they explain the non-linear response?

The paragraphs that include lines 380 to 391 have been rewritten to a) refocus the points being made, and b) make the manuscript less verbose. The new paragraph reads:

The Congo and Amazon regions respond consistently to ARI and ACI individually, but the total responses are different, suggesting a degree of state dependence. In the Congo region (Fig. 11) the responses of many variables have largely additive contributions from ARI and ACI (e.g. θ, W*, IWC, LWC above 2 km, and precipitation), with ARI tending to drive stronger regional responses than ACI. In contrast, the Amazon region (Fig. 12) does not consistently show additive responses. Some variables show largely additive responses (e.g. LWP, precipitation, W* below 7 km) driven primarily by ARI but others are not clearly attributable to either ARI or ACI (e.g. IWC, W* and θ above 7km). In contrast to the Congo region, ACI plays a stronger role in the Amazon and is responsible for most of the LWP response and

LWC, but only weakly impacts precipitation. The enhanced role of ACI in the Amazon is consistent with relatively higher frequency of shallow convection than in the Congo, which is a regime known to be sensitive to ACI (Langton et al., 2021; Sheffield et al., 2015). The contrasting roles of ACI and ARI in the Congo and the Amazon suggests the response of convective environments to changes in the aerosol population is dependent on the background state, which has also been observed in remote-sensing studies of the Amazon region (Herbert and Stier, 2023; Ten Hoeve et al., 2011; Yu et al., 2007). This is consistent with Chang et al. (2015) who show that the response of deep convective clouds to aerosols is regime-dependent due to non-linearity between dynamical and microphysical processes

Line 400: Is 1 mm day-1 consistent with Figure 9?

Yes, this is consistent.

Line 415: Do you mean the red line? It is over zero at all hours.
Lines 416-417: ARI is positive in Figure 9h.

The total response includes a persistent response and a diurnal response. The diurnal response will always vary about zero, so any systematic increase or decrease is due to the persistent response. In the line referred to by the reviewer we discuss the changes “relative to the persistent response”. For the variable in question the persistent response is large enough that the total response remains above zero throughout the diurnal cycle.

We recognize that this is insufficient information for the reader. The sentence has been rewritten to improve the clarity.

Lines 417-418: The ACI effect on LWP is always positive, while its effect on Mflux is negative only between 12:00 and 15:00 LST. How can you explain it?

The mass flux response is a function of the change in vertical velocity and the condensate loading. Therefore the LWP and Mflux response will not necessarily be in the same direction. In this instance the reduction in afternoon Mflux amidst an increase in LWP suggests some afternoon suppression of W. This is consistent with the Maritime Continent region as a whole which includes convection over land and low-altitude marine clouds.

Lines 422-428: Please provide more detailed reasoning!

We have amended the sentences as suggested.

Line 440-441: ACI and ARI are comparable in the ice-phase region!

The reviewer is correct. Although ARI clearly dominates the response in the warm-phase region, neither pathway dominates in the ice-phase region, and there is a suggestion that both play a role in the total response. This has been rewritten.

Reviewer #2

The authors take a crude aerosol parameterization (MACv2) and proceeded to parameterize it further for use in km-scale aerosol sensitivity studies. It's still crude, though it seems to be more intelligently refined. Overall, the authors try to present these one-month-long km-scale simulation as a way to study generic features of aerosol radiative response. I don't think they made a convincing case. As Reviewer 1 indicated, there are "several severe flaws" in this work. However, I would go further than Reviewer 1 to say the methodology is also flawed in that it is not fit for the purpose at hand. I don't think this manuscript should be published in ACP in its current form, and I do not think it is likely any revision along the lines of the submitted manuscript will make it closer to acceptable form. Besides the points raised by Referee 1, here are additional points that will likely prevent this manuscript from proceeding further.

We thank the reviewer for their comments and criticisms, but we strongly disagree with all points raised in this summary.

A significant criticism is in the methodology of our study. Regional-scale simulations have been prolifically used in the community to focus on the impact of aerosols on clouds/precipitation/radiation in order to improve our process-level understanding. These studies frequently use prescribed aerosol fields in order to focus the study and remove a source of considerable uncertainty due to aerosol-related processes. They are typically simulated on timescales of a single day to weeks.

Our study is well aligned with these studies. In our study we perform the same type of simulation but across the entire globe for 40 days, which in itself is an uncommonly long timescale. With this design we can explore the role of ARI and ACI, directly akin to a regional convection-permitting simulation, but simultaneously across the entire globe, encompassing different convective environments, thermodynamic environments, large-scale drivers, aerosol-types, cloud distributions etc. This provides an unparalleled opportunity to identify consistent, generic, responses in different environments. This can be achieved to a certain extent by collating regional simulation studies from across the literature, but will inherently include significant uncertainty due to the different models, configurations, assumptions, microphysics etc used in each study. The regional simulation studies may also include deficiencies from predefined boundary conditions that may erroneously influence the response (e.g., Dagan et al., 2022) - something that our methodology largely overcomes.

Alternative studies have been designed to provide quantitative estimates on the radiative forcing of anthropogenic aerosols, or the long-term response of the hydrological cycle. In these simulations, such as those in the AeroCom initiative, global aerosol microphysics schemes are coupled to the atmospheric component and run for decadal timescales. This type of experiment is required to robustly estimate the global radiative forcing of anthropogenic aerosol, but is not well-suited for exploring process-level understanding (ARI/ACI). An AeroCom style experiment is not what we are attempting to do in this study, though the reviewer's comments suggest this is the direction they believe we are aiming.

This misunderstanding may partly arise from the Global response section, in which we discuss global-mean responses of the aerosol during the time series. This analysis was primarily used to demonstrate that the idealized nature of the experiment design and the simulated responses were consistent with what we may expect. However, it is likely that this has detracted from the core of the study.

In response to this potential misunderstanding, and comments from the other reviewers, we have removed the global response section and instead we present additional analysis that keeps the focus of the study on the novel aspect of the study. We have also changed the title to "Variability of regional-scale aerosol impacts on clouds and radiation in global kilometre-scale simulations". Our results are consistent with other regional-scale studies with similar focus but we are able to extend to the whole globe. We describe the changes in more detail at the beginning of this document.

The authors should release the code for the model itself, the code edits required for their specific MACv2 setup, and the resulting MACv2 files (Nd, optics, etc.) as part of this manuscript. The analysis code released is not sufficient for reproducibility (anyone can make up a few netcdf files). I understand there may be limitation at some European centers regarding data/code transparency, but there are workarounds. If not possible, detailed explanation must be given why it is not possible. Note that this is not a request to release model outputs (though that can be nice; and the authors should definitely consider releasing those too), it is simply the underlying code/files that should be made public (as much as possible).

As requested by the reviewer we will provide all necessary information.

The model we used is publicly available and released under the permissive BSD-3C licence and can be downloaded here: https://gitlab.dkrz.de/icon/icon-model. This includes the implementation of MACv2-SP, which is also publicly available as supplementary information in Stevens et al. (2017). Full access to the data can be granted (and processing resources) for anybody interested via the DYAMOND initiative website (https://www.gewex.org/dyamond/).

We include this information in the Code and data availability section:

The ICON model is available under an open source (BSD-3C) licence (https://www.icon-model.org) and publicly available at https://gitlab.dkrz.de/icon/icon-model. The MACv2-SP software is implemented in the ICON model source code, and is publicly available in the supplementary material of Stevens et al. (2017). Full access to the simulation output data and necessary processing resources is available upon request via the DYAMOND initiative website (https://www.gewex.org/dyamond/).

The design of the simulations (namely 40-day DYAMOND cases) is not really appropriate for the aerosol response being targeted here. The authors make note of this serious issue several times in the manuscript (see below), but they somehow overcome it without much explanation, or maybe I missed it. Why do you think you say so much about ARI and ACI in 30-day runs? That doesn't seem quite right to me. There's simply too much noise (internal variability, etc.) in this for any result to be meaningful.

This comment is echoed in the reviewer's summary and was addressed in our response.

More clarity about the one-way coupling here will be beneficial, and in so doing, it is important to highlight how limiting the setup is. By one-way coupling, I mean that MACv2 prescription affects the optics, radiation droplet number, and cloud droplet number, but nothing in the model affects the MACv2 prescription. Is that correct? If so, I think it should be highlighted more prominently — sections where we cannot say much definitively about what's going on should be deleted (e.g., S 3). As a corollary, do you think the one-way coupling will make the response overestimated or underestimated?

We have expanded Section 2 to provide more detail and clarity on the manner in which the aerosol is coupled to the model. Section 3 has now been deleted and we include a discussion of this limitation using previous studies to hypothesise how an interactive representation of aerosol would impact our results.

Potentially, the authors can consider submitting aspects of this work to GMD (e.g., focusing on the modifications to MACv2 and maybe some aspects of remapping, etc.). For ACP, I think a refocused, less verbose manuscript can potentially be useful for the community. However, the manuscript should narrowly tackle what is possible and avoid less certain topics.

The manuscript is not well-suited for GMD as we are not presenting a model development, but instead using an existing model with minor additions to study aerosol-cloud-radiation interactions. This study is well aligned with the scope of ACP.

Changes to Section 2 will help clarify the modifications that have been made to the existing model framework (ICON coupled to MACv2-SP), and the major revisions we have made (see beginning of document) have ensured the manuscript has a clear focus.

Below are some comments I wrote down while (re)reading the manuscript:

L 23 and elsewhere: I would talk about ARI before ACI (because that's the logical progression, think direct vs indirect).

Where appropriate we have done as suggested.

L 29–24: Hmm, is that really “a primary” one?

We have rephrased this sentence.

L 73: What’s "well defined" here? And why do you feel the need to say so? Are you trying to say people before this study used poorly defined treatments?

We have rephrased this sentence.

L 76–80: the manuscript is already tortuously long; these types of meaningless lines can be deleted.

As suggested by the reviewer this has been deleted. We have also gone through the manuscript and attempted to minimise similarly redundant paragraphs etc.

L 86: This is not a classic “AMIP” experiment by any stretch of definition, or am I confused? (Cf. L 121 and thereabouts.)

It is similar to AMIP experiment protocols in that the sea surface temperature and ice content are fixed to climatologies, and therefore it is an atmosphere only configuration. We refer to “AMIP” on L86 in reference to the SST/SIC climatologies that we use.

Fig 3 and associated text: What’s the deal with the “upscaled” panel? Are you simply remapping the middle panel to 2-degree resolution? If so, and if you really want to discuss this, you will have to give more details about the remapping algorithm and all sorts of things associated with this. I don’t quite see the point of all of this though, so more motivation may be needed to begin with…

We have removed the third panel as suggested.

L 210–215: I don’t think this is enough to circumvent these serious issues. Can you give more reasoning why you think you’d adequately address these challenges by doing something different?

In response to this comment and others we have removed this section and analysis of the global scale mean response.

L 234–235: Yeah, or at least, we simply don’t know…

In response to this comment and others we have removed this section and analysis of the global scale mean response.

S 4: Why do we need a global model to study regional responses? Maybe some regionally refined setup will be more useful (much cheaper) than we have here?

Regional simulations are cheaper to run, but only provide information on one region. Hence we can only quantify the role of aerosols on the regional-scale climate in one location. These may not be applicable to other regions due to different meteorological conditions and non-linearity, therefore the regional studies need to be performed for all regions where we anticipate a role of aerosols.

As demonstrated by Marinescu et al. (2021) and others, the response of clouds to aerosol is sensitive to the model and the microphysics. Hence, relying on singular regional simulations (from different models) to build up our understanding of global responses to anthropogenic aerosol will inherently include similar uncertainty. Using one model to simultaneously focus on aerosol-cloud-radiation interactions across all regions of Earth removes a lot of this uncertainty and allows us to identify consistent processes/drivers/pathways across the globe. Ideally this would be repeated using other models, and is the focus of future DYAMOND style experiments, which we discuss in the conclusions.

Aerosols have local and non-local effects. A limited-area regional simulation cannot provide information on the non-local response, whereas a global model can. We need the spatial-scale of a global simulation but the ability to sufficiently represent the key processes, including those at the cloud-scale. This configuration is able to satisfy both. Though we cannot fully exploit this in our simulations, we hope that ours can inform the configuration and design of future experiments.

L 450: I cannot find this reference; looks like it has not been published yet? If so, maybe this is an improper citation…

We apologize for this. We can confirm that this study is now published and the reference is legitimate and proper.

L 454: I don't think we can say that ("link"), due to all sorts of challenges (internal variability, etc.) related to the design.

The model design allows us to focus on the underlying pathways and processes (links) through which the aerosol impacts the cloud droplet number concentration and the radiative properties of clouds and the atmosphere.

This has been rewritten as

This allows us to identify pathways and key processes through which the aerosol perturbations impact radiative fluxes and cloud droplet number concentration.

L 466: Future direction for…?

This references "Upscaling the analysis to a coarser resolution of 2 degree highlights the recovery of some features (Fig. 3) and a possible future direction"

This has been removed, along with the panel as suggested by the reviewer in a previous comment.

L 466–467: I am not following — perhaps elaborate further here and elsewhere?

This paragraph that contained these lines has been removed.

Reviewer #3

Review of egusphere-2024-1689: Isolating aerosol-climate interactions in global kilometre-scale simulations

Summary: In this study, the authors utilized month-long, global simulations with 5 km horizontal grid spacings to assess the aerosol-radiative impact, the aerosol-cloud impact, and their combined effects, using current day and pre-industrial aerosol estimates. Aerosols are introduced into their model by affecting the number of cloud droplets, the cloud droplet effective radius, and through AOD impacts on radiation. While I commend the authors on running these computationally expensive, global simulations, I question whether their framework and model can be used to come to the process-level conclusions that are being drawn in this study. Furthermore, some of the methods and model set-up can be made clearer. Throughout their results, the authors provide plausible explanations for their results, but they often seemed to be speculative, as opposed to rooted in analysis from the model data. As such, while global, long-term, simulations of aerosol effects do provide unique science opportunities (such as impacts on the larger-scale features that can be better resolved with 5 km grid spacing), this manuscript seems to focus on more uncertain and less justifiable aspects of their simulations.

We thank the reviewer for their comments. We have made revisions to the manuscript to remove the more speculative elements of our manuscript. This includes the global-mean response section (Section 3) which has now been replaced with a global analysis of the responses using regional-scale decompositions as suggested by this reviewer in a later comment. In the remaining results sections we have revised the manuscript to make sure our explanations are supported by either the figures in the manuscript, additional figures in the supporting information, or from the analysis/conclusions of previous studies. Any explanations that are not fully supported have either been removed or noted as such.

Major Comments and Concerns:

The authors state that “the novel aspect of this study is the globally resolved deep convection” (L262). However, 5 km grid spacing does not resolve most deep convection (i.e., isolated and scattered deep convective clouds), so the authors should reconsider framing their study in this way. This is particularly concerning given that the authors choose their regions of analysis due to having deep convection. This may require a shift to better resolved features in their simulations.

A numerical model of the atmosphere will resolve convection on the scales that are represented in the model, if not, parameterization for the unresolved convection is active. At very coarse resolutions convection will be under-resolved and some aspects of convection will not be perfectly represented. We agree that a horizontal resolution of 1 km would be preferable, but at 5 km resolution many aspects of convection are already better represented than in a model with parameterized convection. Among those better represented aspects are for example the diurnal cycle of convection and the coupling of convection with its environment (important for this study), which is especially true in tropical regions dominated by large convective clusters. There is a large body of literature demonstrating this, which goes back at least two decades, especially in the area of limited area modelling. We try to avoid making "the perfect the enemy of the good" here. A recent study describing the simulation of convection with ICON in a comparable configuration to our setup was conducted by Segura et al. (2022).

We have added the following text to the methods section (Sect. 2.1)

In our experiments, we use a horizontal resolution of approximately 5 km with 90 levels from the surface to 75 km corresponding to a vertical resolution of about 25 to 400 m (Hohenegger et al., 2023, G_AO_5km setting). This configuration of ICON does not explicitly resolve the smallest scales of convection (< 5 km) but has been shown to reproduce observed diurnal and seasonal cycles of tropical precipitation Segura et al. (2022). Given that ESMs tend to use spatial resolutions of tens to hundreds of kilometres, this makes a marked improvement in our ability to resolve many aspects of convection (Done et al., 2004; Prein et al., 2013) and is well-suited to our study.

And the following to the conclusions section:

Additional sources of uncertainty arise from the cloud microphysics scheme and unresolved convection. The choice of the cloud microphysics scheme and representation of cold-phase processes has been shown to impact the response of convective clouds to aerosol (Heikenfeld et al., 2019; White et al., 2017; Sullivan and Voigt, 2021; Marinescu et al., 2021). Archer-Nicholls et al. (2016) and Possner et al. (2016) have shown that the magnitude of ACI and ARI responses may be sensitive to unresolved convection at 5 km resolution, potentially requiring a finer global resolution (e.g. Wedi et al. (2020)).

Many studies have shown that aerosols have a clear diurnal cycle. Given the focus of this study's analysis on the diurnal cycle of aerosol effects, can the authors comment on and justify their use of a time-independent aerosol perturbation that does not vary diurnally? This simplification seems especially concerning, given the focus on the diurnal cycle of aerosol effects in this study.

We agree that studies demonstrate a diurnal cycle of aerosols, but in many instances there is no consistent diurnal cycle. For instance, Yu et al. (2021) demonstrate that the diurnal cycle of dust from five globally-important source regions is not spatially nor seasonally consistent. This occurs due to the regional-dependent drivers of dust emissions, including convection, windspeed, and wildfires. Variability in the diurnal nature of aerosols from other species is also demonstrated by Torres and Ahn (2024). A lot of the diurnal features of aerosol emissions and concentrations are driven by small-scale features of the atmosphere and surface, which will themselves be prone to considerable uncertainty and likely poorly represented in aerosol microphysics models. This is the uncertainty we try to remove from our study by using prescribed aerosol fields, however, we recognise the feedbacks between in-situ aerosols and their emissions would be a particularly important response. To achieve this we need interactive aerosols in global high-resolution models, which will be the focus of future studies (e.g, Weiss et al., 2024).

For these reasons we disagree with the comment that the simplification is especially concerning. However, we agree that the impact of, and feedbacks to, the diurnal cycle of aerosols is an important consideration. We have acknowledged this limitation in the conclusions section as follows:

The use of non-interactive and spatially invariable aerosol may be masking important feedbacks and processes. This includes the strong impact of clouds and precipitation on the spatio-temporal distribution of aerosols, changes to the surface properties, fluxes and turbulence that would influence emissions and aerosol removal processes. Aerosol emissions also exhibit diurnal cycles (Yu et al., 2021; Torres et al., 2024) that we do not account for. This gap will be closed by future aerosol perturbation experiments in global kilometre-scale models with an interactive treatment of aerosols and clouds.

L211-215: The authors state that they will only discuss the global scale briefly due to internal variability but can mitigate internal variability on a regional scale by compositing over multiple diurnal cycles. It is unclear why this same method cannot be applied globally, and whether this compositing can be used to reduce internal variability in these simulations. Internal variability comes up several more times in the manuscript, so being clearer about internal variability, and its impact on this study up front would be helpful.

We agree with the reviewer and thank them for this excellent suggestion. In response to this, and similar comments from the other reviewers, we have made major revisions to the manuscript following the suggestion made here. A more detailed description can be found at the beginning of the document.

With respect to internal variability, we discuss this more in the revised manuscript, and we include a new figure in the supporting information (Figure R4 in the response to reviewer #1) that helps to demonstrate how the decomposition method helps reduce or mitigate the effects of internal variability in the response.

Aerosol-Radiation details. It seems that plumes of biomass burning emissions and industrial emissions are used. Do these aerosols have different radiative properties that are interacting with the radiative scheme? Where do the aerosol particles live in the vertical? Are they advected over the course of the simulation? The authors state that the nine plumes are configured to reproduce the AOD for the year 2005 (L134), but how are they configured? I think this section should have more details, which would improve the clarity of the experimental set-up.

We have rewritten Sections 2.2 and 2.3 to provide more detail and clarity on how the aerosol is represented in the model. We also include the resulting vertical profiles of aerosol extinction from the six regions in the supporting information.

There have been countless limited-aera modeling studies of aerosol effects, specifically ones that focus on the regions highlighted in this study. However, there were only a few limited-area studies included in the authors' introduction or through their results section. Process-level insights from additional limited area studies may help the authors disentangle and contextualize their results.

We have included additional references throughout the revised manuscript.

Minor comments:

L4: "we have been unable to explicitly simulate cloud dynamics." We have been able to in limited-area model studies, so can you please make this more explicit about global models.

This has been rewritten

Due to computational constraints, we have been unable to explicitly simulate cloud dynamics in global-scale simulations, leaving key processes, such as convective updrafts, parameterized.

L44: What does "regional drivers" mean here?

Different regions will have different drivers that influence the thermodynamic environment, or diurnal cycle. This means the role of aerosols in one region will not necessarily translate to other regions. We have rewritten this sentence for clarity.

L97: It is unclear to me why the same number concentration cannot be used for both radiation and microphysics? This seems like an unnecessary source of inconsistency.

The Nd concentrations are used in the radiation scheme and microphysics scheme but in our version of ICON the two are not coupled. Therefore the treatment of Nd in each scheme has been developed independently. In the radiation scheme Nd influences the cloud droplet effective radius, and when developed, was tuned to produce observationally-consistent cloud radiative properties. Similarly, in the microphysics scheme, Nd influences the warm-rain process via the autoconversion rate. When developed, Nd values were used to produce realistic precipitation rates. In an ideal scenario the two schemes would be coupled, and this is the focus of future development work for ICON.

For these reasons, we intended to make only minor modifications to the existing model, and maintain the treatment of Nd in each scheme. Larger modifications, specifically to the radiation scheme, may have resulted in significant changes to the global energy budget, resulting in an unrepresentative model. We have attempted to make this clearer in the revised Sections 2.2 and 2.3.

L134: Unclear how the extrapolation is done here. Can you make clearer?

This has been expanded to provide more detail.

L197-202: The authors state several important features are well-simulated in their simulation, but it is difficult to see from Figure 2 whether these features are being simulated properly.

This figure and accompanying text has been removed in order to focus on the new global-scale analysis.

L215-225: Most of this section focuses on results from the spin-up phase. Why do the authors focus so much on the spin-up phase, as this phase is used to spin up their model?

This section has been removed in order to focus on the new global-scale analysis.

L267-272: The authors separate their time series into short-term and long-term components, which reduces contributions from internal variability but introduces “non-local impacts to the region.” More details are needed here in terms of how this reduces internal variability, and what is meant by non-local impacts.

L275: The “second application of the decomposition tool with a large prescribed periodicity.” What is the prescribed periodicity, and how is this used to provide new, helpful information? More details would be helpful here.

In response to these two comments we have expanded our description and explanation of the decomposition method. We also include a new figure in the supporting information (see Figure R4 in the response to reviewer #1) that helps demonstrate how the method is able to reduce the impact of internal variability.

L349: Given the importance of removing internal variability, why aren’t Figures 10 and 11 decomposed with similar methods as the other figures?

We were unable to decompose the profiles as output on all pressure levels was 3-hourly, which is insufficient for the decomposition tool. We have clarified this in the revised manuscript:

The frequency of output on all vertical levels is insufficient (3 hr) to robustly decompose the time series following Sect. 2.4, hence the profiles include influence from internal variability. To minimise this, the responses are composited onto a single diurnal cycle. Additionally, the limited day-to-day variability evident in Fig. 10 for the regions provides confidence that the responses are primarily due to the aerosol perturbation.

L391: The authors mentioned delayed CAPE as a potential process? Did the authors look at this in the simulations or is this speculation?

This hypothesis is based on a previous study (Herbert et al., 2021) that focused on aerosol impacts to deep convection. We do not calculate values of CAPE and we do not look further into the non-linearity. This sentence has now been removed to refocus the paragraph.

Figures 8-11: Given that the author's describe the results for each region at a time, it may be easier for the reader if the authors combined these figures based on region. I found it challenging to jump around from each figure to understand how these different variables come together to tell a consistent story.

We agree that this would help the reader navigate the figures. We have combined the figures and now show all variables (and corresponding response) for each region individually. We have also included the vertical profiles of aerosol optical thickness perturbations to panels that show the response of a vertical profile.

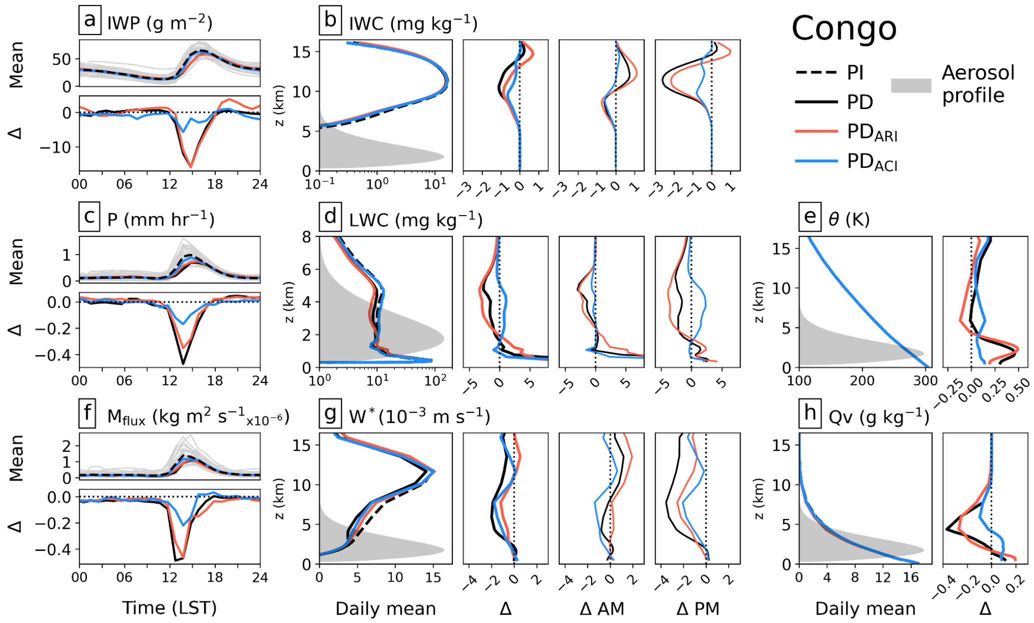

FIG R5. Example of the new 'combined' figure as suggested by Reviewer 3.

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