Many thanks to both reviewers for their thorough and helpful reviews on our submission. We have addressed each point in turn below in blue text.

We wish to highlight that, in the creation of the original manuscript, the future ozone impacts under the novel scenarios were incorrectly plotted in Figures 3 and S5; the Tables and other Figures showed the correct values. We have corrected these figures; this did not change any of the analysis of the results.

This file contains responses to both reviewers; responses to reviewer 2's comments begin on page 7.

Reviewer 1

We thank the reviewer for their kind words on the submission as a whole, and for their detailed and useful comments on improvements to the manuscript. We have implemented changes based on each comment and detailed this below.

General comments:

Congratulations to the authors for this important study and their work to expand health and air quality research into this understudied region. I appreciate your thorough reasoning for the various decisions made to best address inherent study limitations and the clarification that these results are intended for relative comparison between themselves. Your figures are fantastic as well, and I would suggest working with the editors to make sure they are all large enough to be read clearly.

While overall design and potential impact of this study are powerful and intriguing, the text is unpolished and thus diminishes these strengths. There were a number of repeated discussion points between and within sections that should be merged to improve readability. This paper also needs some work so that the methods (throughout the paper) are presented beginning with a general outline and working into the details. As is, specific details/jargon are introduced before a general overlay is presented that is necessary to give the reader context (e.g., see comment about lines 302–304 below). Additionally, there were several cases of inconsistent formatting that should be addressed (e.g., of confidence intervals; "stippling" vs. "dashed" in figure captions).

We have substantially re-ordered the different sections in order to more coherently present them. Specifically, in addressing this and the below comments, we have moved the last Introduction paragraph to the methods, created a new subsection in the methods to describe the experiments, shifted points of discussion from the results to the discussion section, and rearranged parts of the sections to improve legibility. Many thanks for highlighting this; we feel the paper flows much better after making these changes.

I'm also concerned about the amount of technical jargon and lack of high-level summaries that may prevent this study from entering more interdisciplinary spaces. For instance, could certain repeated labels like "AerNonBB" and "SSP119" be replaced with something more meaningful and intuitive? I understand, however, that some of these came from previous studies, so perhaps instead a glossary of important acronyms/terms with clear definitions would be a useful quick reference for the reader. Please also provide a longer-form definition of these terms (e.g., "aerosol non-biomass burning, termed AerNonBB…").

We appreciate the focus on interdisciplinarity and agree on the importance of minimising jargon to enable this. The Shared Socioeconomic Pathways are used extensively within climate policy, modelling, and impacts work; while we share the concern that terms such as "SSP119" may be new

to readers from even broader disciplines, we feel that substituting for other terms would harm the legibility for readers within the general climate research community. At the other end of the spectrum, the AerNonBB, AerBB, and AerAll experiments were created from the SSPs purely for this project; they are new to any reader and we have constructed these names to condense their meaning and allow us to refer to them throughout with brevity. We were unable to find more suitable concise names to use for these experiments. We also used these names in a prior paper (Wells et al 2023) exploring the climate impacts of the experiments; changing them for this paper could create confusion for readers who engage with both studies.

To aid in the interpretability of the study for broader readers, we have added a new subsection in the methods solely describing the experiments themselves. Also, as suggested above, we have combined the information on these experiments into this part of the paper. As suggested, we have also added short concise explanations of the experiments when they are introduced, which the reader can refer back to quickly.

These changes, with greater detail offered below, should create a final paper that will reach wider audiences and have the impact it deserves.

Specific comments:

Abstract:

The abstract lacks several important aspects about the study. More background information and overall study objectives should be included at the beginning of the abstract. I suggest tying in the novelty of this study, how it progresses global research equity by focusing on the most understudied continent in terms of health and air quality research. There is also no mention of the individual scenarios run in this study (i.e., strong and weak mitigation scenarios, examined by fuel type).

We have modified the abstract to better present the focus of the study, and also to introduce the type of experiments used in our analysis.

Line 14: Should include the full name of the model in the abstract as well as the text: “UK Earth System Model 1 (UKESM1)”

Done

Line 15: Please clarify that these are global annual deaths.

We have clarified this point; these impacts are in fact for Africa itself; please see the discussion below RE Table 2.

Lines 15–16: Confidence intervals should be provided for the results here.

We have added confidence intervals as suggested.

Introduction:

You begin with a general overview of aerosol impacts on climate/health, but a much better way to highlight the novelty of this particular study would be to begin by covering the state of emissions in Africa. You cover this a bit in lines 65–74, but since this is really the selling point of your study it would be best to introduce these ideas earlier. I would suggest you also go into greater detail on

what we do know (major or unique emissions sources, historical trends, etc.) and especially how much we don't know due to a lack of research in this continent compared to other areas in the world. I've listed some potential reviews you may wish to reference below. These and/or others should be used to highlight the importance of studies like yours in improving the equity of existing global air quality and health research. At least some of these ideas should be introduced towards the start of the paper, and could also work well to strengthen the ideas in the Discussion in lines 445–449.

Many thanks for bringing those informative studies to our attention! We have incorporated those sources with our prior discussion on the regional context, and moved this earlier in the introduction as you suggest. We feel this certainly situates it better within the existing literature.

Abera A, Friberg J, Isaxon C, Jerrett M, Malmqvist E, Sjöström C, Taj T, Vargas AM. Air Quality in Africa: Public Health Implications. Annu Rev Public Health. 2021 Apr 1;42:193-210. doi: 10.1146/annurev-publhealth-100119-113802. Epub 2021 Dec 21. PMID: 33348996.

Coker E, Kizito S. A Narrative Review on the Human Health Effects of Ambient Air Pollution in Sub-Saharan Africa: An Urgent Need for Health Effects Studies. Int J Environ Res Public Health. 2018 Mar 1;15(3):427. doi: 10.3390/ijerph15030427. PMID: 29494501; PMCID: PMC5876972.

Katoto PDMC, Byamungu L, Brand AS, Mokaya J, Strijdom H, Goswami N, De Boever P, Nawrot TS, Nemery B. Ambient air pollution and health in Sub-Saharan Africa: Current evidence, perspectives and a call to action. Environ Res. 2019 Jun;173:174-188. doi: 10.1016/j.envres.2019.03.029. Epub 2019 Mar 16. PMID: 30913485.

Pinder RW, Klopp JM, Kleiman G, Hagler GSW, Awe Y, Terry S. Opportunities and Challenges for Filling the Air Quality Data Gap in Low- and Middle-Income Countries. Atmos Environ (1994). 2019 Oct 15;215:116794. doi: 10.1016/j.atmosenv.2019.06.032. Epub 2019 Jun 18. PMID: 33603562; PMCID: PMC7887702.

Line 31: Please check the references and clarify throughout this paragraph whether the PM2.5 concentrations are annual/long-term or daily/short-term averages. These cannot be directly compared.

We have clarified this paragraph to note that these values are annual average concentrations.

Line 45: Same comment for ozone; please confirm/specify averaging times for these levels.

Many thanks for highlighting this; the two studies cited in fact use different definitions of concentrations to each other, and different again to the definition we use (which is constrained to be the appropriate definition for use with the concentration-response functions). We have clarified this; we don't make direct comparisons between the studies – as one focuses on population-weighted values – or with our own results so it doesn't affect the overall analysis, but it is essential to be clear on the definitions.

Lines 75–93: This paragraph feels too technical for the introduction. I suggest generalizing this section in the Introduction and saving the details for the Methods.

We have applied this change, leaving a shorter explanation in this section and incorporating the relevant details within the new section 2.2 presenting the experiments.

Methods:

Lines 151–153: No need to separate these labels out from the paragraph unless you also include definitions alongside them; please see notes above about updating these terms or at least providing clear, easy-to-reference definitions.

In addressing the earlier note on easy-to-reference definitions, we have added these alongside these labels.

Results:

Lines 302–304: This sentence does a great job explaining your methods in clear, simple terms, and should be presented this way much earlier in the manuscript.

Many thanks; we have incorporated a similar-framed sentence into the experiment section 2.2.

Lines 330–344: This comparison to similar studies is more appropriate as discussion section material. It should be integrated with similar ideas repeated in lines 411–427.

Agreed; we have moved these paragraphs from the results to the discussion, and merged it with the existing related discussion there.

Line 338: Be clear about what "smaller CRF" means here ("used an earlier CRF reflecting weaker associations between O3 and health outcomes" or similar).

Done

Line 342: The studies that these different CRFs came from need to be specified here as well as in Table 1, with more detail so the reader can look them up for themselves in those references (i.e., many epidemiological studies report RRs by age group, location, etc.).

To be clear: our study uses a single source for the PM2.5 CRFs (the Global Burden of Disease 2019). This study supplies an uncertainty range on these CRFs – it is this 5-95 percentile range that we present here to sample the CRF uncertainty. Similarly, we use the 5-95 percentile range in the O3 CRF from Turner et al 2016 to explore that uncertainty. We have clarified this in this sentence, and in the health methodology (now section 2.3).

Lines 363–367: These ideas are very similar to those presented in the Discussion on lines 440–443, including identical wording in certain places. Please choose one place for this topic (Discussion seems best) and unify the two sections.

We agree that these two sections are too similar. However, we feel we should present this interesting dust effect in the results and add further, broader discussion in the discussion & conclusions section, so we are hesitant to remove it from the results entirely. We have instead re-phrased the section in the results, and pointed the reader to the analysis in Figure S4.

Discussion and Conclusions:

As noted above, a number of ideas presented in this section were also discussed in earlier sections. I suggest unifying these ideas in the discussion instead of spreading them out, since it feels a bit redundant by the time you reach the discussion.

We agree that the flow of the paper is greatly aided by focusing these ideas in this section. In addressing the above comments on the Methods section, we have moved these features into the discussion.

Lines 445–449: This is a key idea that could potentially be expanded on; see notes and suggested references in Introduction section above.

We have expanded this briefly, with reference to those sources added to and discussed in the introduction.

Lines 450–456: This paragraph may work better if moved up, following the paragraph with a similar idea mentioned in lines 434–435.

We agree that greater clarity is achieved by presenting these paragraphs simultaneously, and have made this change.

Figures & Tables:

Figure S3: I suggest updating the global O3 plot to also include its upper boundary of the LCT, like is done for PM2.5. This would give more context/clarification that O3 remains above this boundary in all experiments.

We agree this is a useful clarification and have made this change.

Figure 2: Great maps, but a little difficult to see the colors beneath the stippling with how small they are. It may be worth putting this figure across multiple pages so these can be enlarged, cutting out latitudes below -60 (Antarctic region) to allow figure to be larger on the page; and/or reducing the density of the stippling so the colors come through better.

We agree the colours were hard to see on these plots. We have removed the Antarctic, and used a Robinson projection to allow easier viewing of the tropics. We think this has greatly improved the legibility of this figure.

Table 1: What epidemiological studies were used for “mean”, “low RR” and “high RR”? This should be stated explicitly in the table caption and in the main text. Also, “mean” is confusing here – language like “middle RR” would be more appropriate. The table title needs to be updated to explain that this is comparing different CRFs, as is mentioned in the text.

As we note above, this study uses the Global Burden of Disease 2019 for all PM2.5 CRFs, and Turner et al 2016 for O3, with the CRF uncertainty presented within those studies used for the CRF uncertainty here. We have edited the caption to clarify this, and also changed the language to “middle RR” as you suggest.

Table 2: Please clarify in the table labels, legend, and text that the “Global” numbers exclude the “Africa” numbers – as is, it’s confusing that Africa has higher mortality counts than the global totals. Perhaps replacing “Global” with “Other Global Regions” or similar. (Please ensure this clarification is updated in other relevant places in the paper). This table also needs more details so it can stand alone (i.e., note somewhere that these numbers represent deaths, ideally noting this is 2015–2090; add a label over the first column noting these are different (weaker) mitigation scenarios.)

Apologies, this Table needed additional clarity. To be clear: this table presents annual deaths in 2090 only – not 2015-2090 – and the Global numbers don’t exclude the Africa numbers. Because of the

short lifetime of PM2.5, the vast majority of its impact is typically felt within Africa itself. The effect outside Africa is modulated by internal variability, as well as any climate-related pollution feedbacks, in addition to the direct effect of the emissions change. While we averaged over ensemble members, we were unable to apply multi-year averaging, as discussed in the methods section. The very high population densities in some regions, particularly in Asia, cause the non-Africa impact to be modulated by the internal variability of the concentrations in these areas. We have added some text to the main text to clarify this, and have added further labels to the table and caption as suggested for additional clarity.

Technical corrections:

Please use consistent formatting for confidence intervals throughout the paper and clarify whether they are 95% CIs early on.

Done

There are many places where paragraphs are broken up / there are very short sections standing alone that seem to belong to the preceding paragraph; these should be checked and corrected, and single-sentence paragraphs avoided as much as possible.

Done

Line 13: Suggest changing “range in the future impacts of African” to “range of future impacts from Africa”

Done

Line 25: Remove extra space after citation.

Done

Line 57: Fix the degrees symbol.

Done

Line 64: Remove comma after “less explored”.

Done

Line 67: Remove comma after “detail”.

Done

Line 70: Suggest “range of” replace “range in”.

Done

Line 194: Suggest rewording from “cancel to drive little” to “cancel, resulting in little” or similar.]

Done

Line 306: Typo; should be “centered”

Done

Lines 365–366: Wording of this phrase is vague/confusing, please clarify: "… indicating that indirect effects of pollutant emissions on atmospheric circulation and therefore natural emissions can have a substantial influence on their overall impact."

We have re-phrased this sentence to clarify.

Lines 431–432: Typos: "SSP1" should be "SSP119", and "SSP3" should be "SSP370".

The population projections in the SSPs are specific to the narrative, with no change under mitigation – so they should just be referred to as SSP1 and SSP3; there's no difference between e.g. SSP119 and SSP126 populations.

Reviewer 2

We thank the reviewer for their comments and useful suggestions, which we have implemented and discussed with details below.

The publication presents the results obtained using the UKESM1 model to quantify the impact of air pollution on mortality in Africa by comparing contrasting SSP scenarios.

The format of the publication is pleasant to read, despite a few redundancies in the text which are not problematic.

The modeling work is serious, well explained and supported by pedagogical figures.

My main reservations, which lead me not to accept the publication as it stands, are as follows:

- It is obvious that measures to reduce pollutant emissions lead to a reduction in population exposure and to excess mortality induced by this exposure

- The added value of this study is its ability to estimate the relative impact of emissions mitigated by political measures (anthropogenic emissions) and those less dependent on such measures (emissions from vegetation fires). The DACCIWA campaign, for example, showed that during the wet season, air pollution in the coastal cities of the Gulf of Guinea was dominated by advected biomass fire emissions from the southern hemisphere (Hasslet et al, Atmos. Chem. Phys., 19, 15217–15234, https://doi.org/10.5194/acp-19-15217-2019).

- Yet, as the authors quite honestly point out, the most pessimistic SSP scenarios (SSP370) lead to lower BB emissions than the more virtuous scenario (SSP119). There is no explanation for this counter-intuitive result (for exemple, Lund et al., ACP, 2019 mention that it « can be linked to assumptions about the land-use sector in the respective integrated assessment models »).

- Applying in the modelling work a trend that we know to be false disqualifies the results obtained and calls into question the value of the mortality figures obtained. It seems to me that, as a scientific

community, we need to be able to explain the content of our modeling work, to avoird the risk of weakening our messages. This is of particular importance when there are no data to confirm or challenge the modeling results.

- Three of the publication's co-authors have presented a very interesting work (EGU 2021 -Improved estimates of future fire emissions under CMIP6 scenarios and implications for aerosol radiative forcing) to create an updated emissions and correct those proposed in the SSPs. Why not make use of this updated dataset, which would make the results more reliable ?

We wholeheartedly agree on the importance of accurate emissions data in order to ensure the accuracy of modelling results. Unfortunately, current pollutant emissions & levels, and future trends, are more uncertain over Africa than other regions. We discuss this in the text – and have now added further exploration of this into the introduction; we note that concentration-response functions can't be fully constrained by region-specific data at present, and that issues exist around data collection and availability. In addition, the process for the creation of the SSPs complicates the understanding of different biomass-burning emissions scenarios everywhere, with an outsized effect in Africa due to the relatively large role of these emissions there, and the differing trajectories of their socioeconomic drivers in the SSP narratives.

These issues reflect limitations of our methodology, which in turn reflects the accumulated research gaps on this issue. Rather than steering away from research in this area, we chose instead to highlight these limitations in our discussion, building on a body of research calling on further study into health impacts in Africa. We believe that research such as this, containing these limitations, can aid calls for further research to build on, and improve, these results. We have added some more text to this point into the discussion, noting again the limitations with the SSPs and also re-iterating the calls made elsewhere for further research into CRFs appropriate for Africa. We thank you for emphasising this point, as we feel our additions enhance the paper's clarity and restate the context for readers.

We are also keen to note that the SSP emissions scenarios are very widely used in climate change, mitigation and impacts studies; for this reason, regardless of their accuracy or representativeness, it is worth exploring the effects of their emissions on the climate and society, to add further context to other work using these scenarios.

In regards to the reviewer's point on updated emissions which we have contributed to, we entirely agree that this would be a useful exercise. The simulations performed here were conducted before that study was undertaken; this project partially informed that work. Unfortunately, running additional simulations within UKESM1 sits outside the scope of this project, but we entirely endorse the idea of applying updated emissions to investigate their impact. We note that, while the methodology for generating the SSPs confounds inter-model and inter-scenario distinctions, it is not the case that biomass-burning emissions should necessarily be lower in SSP119 than SSP370. Indeed, the updated emissions also find higher emissions in SSP119 as SSP370. Generally, that further study found that while high-latitude fire emissions are mostly climate-driven, those from the tropics are more sensitive to population and other socioeconomic dynamics. This does not suggest that the SSPs aren't getting the same sign difference for the same, or sensible, reasons, but indicates that the SSPs are not necessarily "incorrect" in this.

We would finally note two further points on this topic. The bulk of the effect here is attributable to the non-biomass burning changes, which, as you note, more directly reflect policy decisions than do changes in biomass burning emissions. Therefore, conclusions on the effect of policy to address non-

biomass burning emissions, somewhat independent of those from biomass burning, can be drawn by the reader. The effect of direct policy choices on health impacts – and therefore the role of non-biomass burning in particular – was the motivation for this project. Secondly and relatedly, we note in the manuscript that the lack of bias-correction points to a focus on the relative effects of these scenarios within UKESM1; it is in this comparative context that we interpret and present our results.

- The results on the reduction in desert dust emissions due to the disruption of the surface wind are very interesting. Is it corroborated by other similar studies ? The discussion on that point would benefit from references to past studies

We agree that this point needed contextualising within the broader literature. We have added references to studies which also find feedbacks from dust emissions onto broader aerosol levels in other contexts.

- Are the indirect and semi-direct effects of aerosols considered in the study of the feedbacks on atmospheric dynamics and physics ?

The indirect and semi-direct effects are both included in the schemes in UKESM1. Thank you for highlighting this; it was an oversight on our part to not specify this fact within the paper, and we have now included it explicitly in Section 2.1.

Details:

- What does the number 385 in table 2 correspond to?

This is an artefact of the line numbering formatting – we have ensured the line numbers are in the correct place in the new version.

- To facilitate the reading of the manuscript, it would be interesting to standardise the presentation of the results : Table 1 provides the figures for Central Africa, Figure S3 illustrates the evolution of concentrations for West Africa, Figure S5 illustrates the calculation of mortality for West, North and East Africa.

We tried to strike a balance between presenting a wide range of results and ensuring coherency amongst them. We agree however that the range of regions chosen was too broad, detracting from the legibility of the overall results. We have now standardised this by changing Table 1 to present the West Africa results instead; now this region is represented in each result (Table 1, Figure S3, Figure S5), with the supplementary figure S5 showcasing some additional regions for further detail. We believe this is represents a better balance between broadness and consistency in our presentation.