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Peer Review File

Lightweight climate models could be useful for assessing aviation mitigation strategies and moving beyond the CO_2-equivalence metrics debate

Corresponding Author: Dr Thomas Planès

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Planès,

Your manuscript titled "Why lightweight climate models are useful tools for assessing aviation mitigation strategies and moving beyond the CO_2-equivalence metrics debate" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, please provide a thorough evaluation of the reliability of the reduced order climate model. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

Important: The response to reviewers must not include any figures, tables or graphs. If you wish to respond to the reviewer reports with additional data in one of these formats, please add them to the main article or Supplementary Information, and refer to them in the rebuttal. Due to current technical limitations, any figures, tables, or graphs embedded in your rebuttal will not be included in the peer review file, if published.

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Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Mengze Li, PhD
Editorial Board Member
Communications Earth & Environment

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

1. The paper highlights the importance of assessing mitigation strategies for aviation and the ongoing debate around the most appropriate CO2 equivalence metrics to address non-CO2 impacts.
Comment: So, from the very beginning is clear that the core of the paper is around CO2, a major component of the atmospheric greenhouse effect.
The first time the term greenhouse is written is at the Line 320: "As mentioned in the introduction, to meet its greenhouse gas emissions …". However, earlier there is no such reference to the term greenhouse.
2. The paper introduces two climate models of reduced complexity, that can be adapted to evaluate the above-mentioned strategies and go beyond the discussion of CO2 equivalence metrics. The first model uses the GWP* method to calculate equivalent warming emissions, while the second model uses the FaIR climate emulator.
Comment: Explain the motivation of using lightweight climate models. Are there any other lightweight models

3. The authors claim that these models offer a simple approach to evaluating mitigation strategies based on temperature levels, eliminating the need for CO2 equivalence measurements for strategy comparison. Furthermore, they suggest that CO2-equivalent measurements should be incorporated into policy mechanisms to promote the development of strategies, provided they are supported by climate models, as they are not ideal for assessing temperature changes resulting from aviation. The debate on the choice of CO2 equivalence metrics could then turn to methodological and ethical considerations. Furthermore, the paper shows that the selection of high-value CO2-equivalent measurements is more appropriate with increased motion to maintain consistency with temperature estimates.
Comment: The aforementioned claims may be characterized as novel and would be of interest to the community in this research field. However, an additional effort is necessary to clearly reveal the novelty and the originality of the results obtained. To reach this target the already existing background in the field must be briefly given based on the basic publications in the field. For example, it is a truism that the effects of aviation on the Earth's atmosphere are mainly focused on greenhouse gases emissions, since CO2 and CH4 emissions are an important part of climate change contribution. In the literature discussions and evaluations of this contribution have been made using coupled models of the global CO2 and CH4 cycles to determine the aviation's role in a series of scenarios of climate change (See https://doi.org/10.1016/j.apr.2020.08.033). In this way the work will become more convincing, and the results will be strengthened. The suitable revision may also enhance the paper's potential to influence thinking in this field of research

Reviewer #3 (Remarks to the Author):

Dear authors,

I have read your manuscript thoroughly and with great interest. I am certain that your manuscript will help invigorate the ongoing scientific debate around the climate metrics most appropriate in the context of aviation emissions mitigation. Combining the novel FaIR approach with your existing AeroMAPS modeling infrastructure is surely going to further improve the usefulness of this tool for researchers and policymakers alike. In addition, I am impressed by the attention to detail in the extensive literature review sections which support your main work. The detailed treatment of uncertainties originating from different non-CO2 effects is also appreciated.

In accordance with the guidelines of the journal, I can confirm that the conclusions of your work are novel, the data was generated at state-of-the-art level, a detailed treatment of uncertainties is provided and the work as a whole is important to scientists in the specific sub-field of the Earth, environmental or planetary sciences. I can therefore gladly recommend this manuscript be published with minor revisions.

Major comments:

Line 308ff.: "This shows that the FaIR approach is the most appropriate model, and that the GWP* approach with the selected settings"
Unfortunately, it is not clear to me how the choice of the FaIR approach was ultimately motivated. Is this based purely on the match between the FaIR and literature-derived temperature changes shown in Figure 1-3? The reader would appreciate a more transparent definition in this important paragraph of the manuscript.

Could the authors comment on or contextualize recent findings at the German Aerospace Center (DLR), which resulted from a multi-year study on the climate effect of contrail cirrus? The authors of this study ran a climate model to investigate the utility of ERF in the context of specific non-CO2 effects induced by aviation. They found, that:

"The conclusions of this paper are strongly suggestive of a low efficacy of contrail cirrus to force a global mean surface temperature increase, thereby confirming earlier findings for line-shaped contrails (Ponater et al. 2005; Rap et al. 2010)." doi:10.1175/JCLI-D-19-0467.1 (see also the summary presentation at doi:10.5194/dach2022-144)

It seems therefore that detailed climate models call into question at least some of the warming values for specific non-CO2 effects from the Lee et al. metastudy which your models are also using. A comment on this question would be greatly appreciated!

Minor comments:

Line 30ff.: “The debate on the choice of CO2-equivalence metrics could then focus on methodological and ethical criteria.” Since the manuscript does not mention any of these “ethical criteria” later on, it might be best to remove this mention, or back it up with references to relevant scientific discussion on the ethical implications of emissions metrics later on in the text.

Line 105: “but do not take geographical disparities into account.”
The reader might appreciate a more concise defintion of “geographical disparities” in this context.

Line 112ff.: “The choice of the climate metric is crucial and conventional CO2-equivalence metrics are for instance not appropriate in this context [37].”
It is not immediately obvious why the choice of conventional CO2 equiv. metrics is not appropriate. A short description, in addition to the provided source, should suffice.

Figure 1 caption: The main text details the differences between the climate models. Since these are important for a correct interpretation of the figure (ie. the differences between the lines), I suggest at least an explicit acknowledgment of the differences in the caption itself – which of course can refer to the main text for further details.

Figure 3: The temperature change curve for the “updated settings” model appear greatly discontinous, exhibiting the least degree of temporal smoothing. The reader would surely appreciate an explicit acknowledgement and interpretation of this observation.

Line 575ff.: “Lastly, the forcing from aerosol-cloud interactions caused by aviation aerosols has no best estimate given in [1], while very recent works such as [44] are giving a significant cooling estimate.”
The transparency regarding the large uncertainties originating from this effect are appreciated. However, it might be usefor for the reader to briefly mention this limitation earlier in the text as well.

Reviewer #4 (Remarks to the Author):

The authors define a set of rapid, low-complexity, aviation-focused climate emulators and assess their usefulness in evaluating aviation impact mitigation strategies, and to advance the debate around climate impact metrics in this space. Using these simplified models, they first find that the impact of historical aviation emissions on surface temperatures as calculated with the reference AirClim model can be broadly reproduced. They then compare the results of the different low-order models for a suite of different mitigation scenarios, showing that the choice of modelling approach significantly affects the estimated temperature change. They also explore the role of different climate metrics. The authors conclude that low-order climate models can provide valuable insights, but that the current debate around CO2e metrics may be overstated.

The central question posed by the authors is interesting and important. Being able to rapidly assess the effect that different aviation scenarios will have on the environment is important in order to allow technologists and decision makers to act decisively, but most models currently available are either too costly/complex to be used in this fashion or are so heavily simplified that they do not provide useful information. Furthermore, there is a debate raging regarding how to compare strategies even when the outcomes can be quantified. However, the reduced order models (ROMs) proposed and demonstrated by the authors cannot be properly assessed due to the lack of a high-fidelity control. Furthermore although I agree that the debate around emissions metrics is not particularly helpful, the proposed solution – to instead try and use ROMs to calculate temperature change directly – is not a solution because it just results in another equivalency metric.

Overall, while I agree with the goal of this paper I cannot recommend it for publication without a very significant amount of new work being added and broad structural changes. I have elaborated on these needs below, along with a short list of less significant issues.

Major comments

The paper makes two major claims: that reduced order climate models are possible for evaluating aviation sustainability strategies, and that these models can be used to bypass the debate regarding CO2 equivalence. On the first point I generally agree but do not believe that this paper achieves that goal. Two general approaches are suggested (lines 136-151): either to use a GWP*-based quantification of warming with a TCRE translation into warming, or to use open-source climate emulators with adjustments for aviation-specific contributions. As stated on line 153 the authors seek to validate these models; however the only comparisons made (see figures 2 and 3) are to other reduced order models (lines 177 to 186). The Grewe et al. AirClim model is itself a linearization, while the Klower et al. approach uses fixed factors for emitted species. Neither approach is appropriate for validation of a reduced order model. I would strongly recommend that, if the goal is to produce accurate reduced order models, the authors seek data from climate modelers who have run multiple aviation scenarios using a current-generation climate model with multiple ensemble members. Without a real reference case, the argument that these ROMs bring value is actually reversed. Figures 3, 4, and 5 show that the relative benefit of different mitigation scenarios is quite different depending on the choice of ROM, but there’s no clear evidence that this disagreement reflects real uncertainty. It seems instead that the bottom line of this paper is that the current suite of ROMs is not fit for purpose; a relatively minor tweaking of parameters results in different rank-ordering of mitigation strategies, even though nothing is being physically changed in the underlying understanding of the physics. The conclusion I draw is that more work is needed to develop ROMs which can be shown to provide results consistent with larger climate models, and that until that is the case, lightweight models of aviation climate impact could cause the drawing of incorrect conclusions and are not fit for purpose.

The second claim in the paper is also problematic. A great deal of discussion (lines 492 to 541 in particular) is spent on showing that conventional metrics of global warming do not necessarily translate consistently to changes in surface temperature. However, the authors do not demonstrate why this matters, and show only that if you use one metric (e.g. GWP20), you get different results than if you use an ROM to calculate temperature change. This seems to fundamentally misunderstand the meanings of the metrics. It is well understood that GWP20, 50, and 100 are not themselves comparable metrics of warming, but rather can be used to understand the different horizons over which additional radiative forcing is expected to take place. The authors seem to assume that "warming equivalent metrics" are widely accepted as being the goal, but if this was the case then we would not be currently debating whether GWP, GWP*, GTP, ATR, or any other CO2 equivalence metric is preferable. The fact is that different stakeholders have different perspectives (based on e.g. vulnerability to different elements of climate change, expected growth in economic output affecting their discount rates, near-term resiliency, and so on) and therefore will always need different metrics.

Furthermore the closing statements of the paper suggest that we should set a target for aviation of zero warming after a given date, and use simple models to evaluate mitigation strategies. The problem with this approach – and indeed with any GWP*-style approach – is that it sets impossible goals for the industry. As discussed in this paper, the amount of warming from aviation (as calculated by high-fidelity models) is itself dependent on the background scenario, meaning that there is no way for aviation to guarantee no further warming. The core consideration of this which I believe is missing from this paper is the question of how to set robust metrics of aviation's contribution to climate change. I recommend that the authors consider significantly restructuring the paper to focus more on the question of how to provide decision makers in industry and policy with the tools to produce robust recommendations for the aviation industry which are not overly sensitive to changes in non-aviation factors.

Minor comments

The quantification of contrail uncertainty is somewhat biased by the use of results from Teoh et al.; it is likely premature to "bake in" the assumption that SAF will reduce contrail warming (line 373). This is a contentious issue and has only been shown in modelling, mostly using a single contrail model (CoCiP); to date this result mostly relies on an assumption that a reduction in emitted number of particles will result in shorter-lived contrails. I recommend that the authors survey the literature thoroughly and attempt to assess the degree to which this can be considered a consensus result.

Several of the figures are extremely difficult to parse. For example, without referring to the text it is impossible to determine what the meaning of Figure 9 is, as each panel is simply labeled "Trend scenario" without indicating that the difference lies in the amount of growth.

While I actually agree with the statement on line 630 that "quantifying SLCFs [sic] agents, including aviation non-CO2, separately in emission reporting is a much better choice than aggregating those in usual CO2-equivalence metrics", I do not agree that GWP* or FaIR are better than CO2 equivalence for comparison of those non-CO2 emissions. The citations provided only reflect one view of the aviation impacts research community. I would recommend expanding the cited works to reflect the fact that, while emissions reporting is useful, there is no consensus that approaches such as GWP* or FaIR are the appropriate mechanism to then compare them side by side.

There are small typos throughout the manuscript (mostly grammatical); I recommend the authors sweep the document once more to try and eliminate these.

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Version 1:

Decision Letter:

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Dear Dr Planès,

Your revised manuscript titled "Why lightweight climate models are useful tools for assessing aviation mitigation strategies and moving beyond the CO_2-equivalence metrics debate" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you include a robust discussion of why a timeseries of temperature change is the best metric to use for your study, along the lines recommended by our reviewer 4.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewer 4. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

Mengze Li, PhD
Editorial Board Member

Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

I want to thank the authors for successfully revising their manuscript.
Now the paper reads well and it is persuasive.
I, therefore, believe the revised version deserves publication

Reviewer #4 (Remarks to the Author):

I appreciate the thoughtful and well-reasoned response which the authors have given to my review, and the changes made to the manuscript. I have focused my comments on the changes made in response to my prior concerns, and on the authors' rebuttals.

Despite the efforts of the authors, my most significant concern remains. I interpret the results of this paper as being in direct opposition with the title. As discussed during the review, neither high-fidelity models nor the ligthweight ROMs which attempt to emulate them are consistent with regards to the effects of aviation; as the authors stated in their response, "even the use of high-fidelity models to adequately inform policymakers may be premature at this stage". This begs the question then of why the title states "lightweight climate models are useful tools for assessing aviation mitigation strategies".

Secondly, I think it is necessary to insist that global surface temperature is itself a metric, albeit one which takes the form of a timeseries rather than a single number. As such, I do not agree that using a ROM-based estimate of temperature change bypasses the need for CO2-equivalence metrics, as is currently stated in the abstract; it merely introduces another, more complex and model-specific metric to be listed alongside all the existing ones. It is also not clear that the timeseries of temperature change is the end goal as a metric. I believe it is important to the interpretation of this paper to understand whether the authors are arguing that the ultimate metrics should be a globally consistent quantity such as mean temperature (i.e. arguing that we should now focus on refining estimates of temperature change), or whether instead metrics should continue to advance in complexity with the capabilities of ROMs so that individual stakeholders can get more and more information on their specific priorities.

Nonetheless I appreciate that the authors have made a sincere and thorough effort to address my concerns, and that the manuscript now better discusses the limitations of the arguments made. With this in mind, and recognizing that there is unlikely to be a major revision of the paper at this time, I would suggest instead two changes. First, and in light of the fact that the authors agree that these models are not yet appropriate for policy decision making, I would propose that the title be changed to (perhaps) "Why simplified climate modes could become useful tools for assessing...". Second, I would ask that the authors discuss why a timeseries of temperature change is the best metric to use, rather than - say - pattern-scaled maps of surface temperature, region-specific temperature change estimators, or even impact-specific metrics.

Changes Required by Reviewer 2

Dear reviewer, we would like to thank you for the comments that helped improve the paper. The answers to the various remarks are given as follows.

> 1. The paper highlights the importance of assessing mitigation strategies for aviation and the ongoing debate around the most appropriate CO2 equivalence metrics to address non-CO2 impacts. Comment: So, from the very beginning is clear that the core of the paper is around CO2, a major component of the atmospheric greenhouse effect. The first time the term greenhouse is written is at the Line 320: "As mentioned in the introduction, to meet its greenhouse gas emissions ...". However, earlier there is no such reference to the term greenhouse.

Indeed, we inadvertently omitted to include a proper reference to greenhouse gases. We added the reference to this term, with a link with aviation non-CO_2 effects, in the first paragraph of the paper.

> 2. The paper introduces two climate models of reduced complexity, that can be adapted to evaluate the above-mentioned strategies and go beyond the discussion of CO2 equivalence metrics. The first model uses the GWP* method to calculate equivalent warming emissions, while the second model uses the FaIR climate emulator. Comment: Explain the motivation of using lightweight climate models. Are there any other lightweight models

A paragraph is devoted to describing the motivation in the introduction section (l.130). We have updated it to highlight the motivations, restructuring the last few paragraphs to make them easier to understand. Some elements are also mentioned in the Discussions section ("relevance of aviation lightweight climate models"). To summarize some examples, our approach is novel in that we adapt a global method (GWP*) and an existing lightweight climate model (FaIR) to properly account for the non-CO_2 effects of aviation. This is a first step towards making aviation climate scenario modelling more widely accessible. These models are also easy to use and can be used to assess and compare mitigation strategies.

Regarding other lightweight models, several aviation climate models have already been mentioned in the introduction. These include some lightweight models (including several global climate emulators) as well as more complex climate models (such as AirClim, which is a linearisation of the E39/C climate-chemistry model). To the best of our knowledge, no 4D ESMs that include specific modelling of aviation non-CO_2 effects are available in open access at the time of this writing. We added this information and we have separated the dedicated paragraph to make it more prominent (l.101). We have also added some information about GWP* lightweight model in this paragraph (based on comments from other reviewers), as well as a justification of the choice of FaIR at the end of the introduction.

> 3. The authors claim that these models offer a simple approach to evaluating mitigation strategies based on temperature levels, eliminating the need for CO2 equivalence measurements for strategy comparison. Furthermore, they suggest that CO2-equivalent measurements should be incorporated into policy mechanisms to promote the development of strategies, provided they are supported by climate models, as they are not ideal for assessing temperature changes resulting from aviation. The debate on the choice of CO2 equivalence metrics could then turn to methodological and ethical considerations. Furthermore, the paper shows that the selection of high-value CO2-equivalent measurements is more appropriate with increased motion to maintain consistency with temperature estimates. Comment: The aforementioned claims may be characterised as novel and would be of interest to the community in this research field. However, an additional effort is necessary to clearly reveal the novelty and the originality of the results obtained. To reach this target the already existing background in the field must be briefly given based on the basic publications in the field. For example, it is a truism that the effects of aviation on the Earth's atmosphere are mainly focused on greenhouse gases emissions, since CO2 and CH4 emissions are an important part of climate change contribution. In the literature discussions and evaluations of this contribution have been made using coupled models of the global CO2 and CH4 cycles to determine the aviation's role in a series of scenarios of climate change (See https://doi.org/10.1016/j.apr.2020.08.033). In this way the work will become more convincing, and the results will be strengthened. The suitable revision may also enhance the paper's potential to influence thinking in this field of research

Thank you for the reference to this publication that allows us to make our literature review more exhaustive. In our approach, we have a pre-existing modelling of the CO_2 and CH_4 cycle in FaIR, on top of which we added via our simplified lightweight modelling of the CH_4 decrease from aviation NO_x. The novel results obtained with the coupled model developed in Varotsos *et al.* (2021) could be retrofitted in our parametrisation by trying to calibrate against its temperature results, this is definitely a way forward to benchmark or improve our approach. In this spirit, we added a reference to Varotsos et al. (2021) in the literature review, and as well a reference to the methane and carbon cycle, in order to illustrate the important dependence of aviation NO_x to atmospheric CH_4 cycle and associated feedbacks.

Changes Required by Reviewer 3

General comments

Dear authors, I have read your manuscript thoroughly and with great interest. I am certain that your manuscript will help invigorate the ongoing scientific debate around the climate metrics most appropriate in the context of aviation emissions mitigation. Combining the novel FaIR approach with your existing AeroMAPS modeling infrastructure is surely going to further improve the usefulness of this tool for researchers and policymakers alike. In addition, I am impressed by the attention to detail in the extensive literature review sections which support your main work. The detailed treatment of uncertainties originating from different non-CO2 effects is also appreciated. In accordance with the guidelines of the journal, I can confirm that the conclusions of your work are novel, the data was generated at state-of-the-art level, a detailed treatment of uncertainties is provided and the work as a whole is important to scientists in the specific sub-field of the Earth, environmental or planetary sciences. I can therefore gladly recommend this manuscript be published with minor revisions.

Dear reviewer, we would like to thank you for the comments that helped improve the paper. The answers to the various remarks are given as follows.

Major comments

Line 308ff.: "This shows that the FaIR approach is the most appropriate model, and that the GWP* approach with the selected settings". Unfortunately, it is not clear to me how the choice of the FaIR approach was ultimately motivated. Is this based purely on the match between the FaIR and literature-derived temperature changes shown in Figure 1-3? The reader would appreciate a more transparent definition in this important paragraph of the manuscript.

FaIR was chosen due to its open-source availability in Python, its widespread use (for instance in IPCC AR6) and the presence of a model for contrails in particular. Justifications have been added in the introduction. Nevertheless, the results obtained with FaIR would most likely be relatively similar to other climate emulators such as MAGICC or OSCAR mentioned in the introduction. Elements for generalising the results to climate emulators have been added in the paragraph you mentioned, and in the discussions.

Could the authors comment on or contextualize recent findings at the German Aerospace Center (DLR), which resulted from a multi-year study on the climate effect of contrail cirrus? The authors of this study ran a climate model to investigate the utility of ERF in the context of specific non-CO2 effects induced by aviation. They found, that: “The conclusions of this paper are strongly suggestive of a low efficacy of contrail cirrus to force a global mean surface temperature increase, thereby confirming earlier findings for line-shaped contrails (Ponater et al. 2005; Rap et al. 2010).” doi:10.1175/JCLI-D-19-0467.1 (see also the summary presentation at doi:10.5194/dach2022-144). It seems therefore that detailed climate models call into question at least some of the warming values for specific non-CO2 effects from the Lee et al. metastudy which your models are also using. A comment on this question would be greatly appreciated!

Thank you for highlighting this Bickel *et al.* 2020 DLR paper, which indeed goes in the direction of a lower contrail efficacy, so lower overall ERF and temperature impact for a given year than in Lee *et al.* meta-study. In this context, you may be interested to know that the same authors are pursuing their work on the topic and have shown at EGU2024 an abstract (doi:10.5194/egusphere-egu24-18183) which proposes a quantification which appears to be (without being able to read the full paper yet) lower than in Lee *et al.* as well.

In the first draft of our paper, in the ”Contrails uncertainties” subsection within Results, we discussed uncertainties and chose to use the Teoh *et al.* (2023, doi:10.5194/egusphere-2023-1859) paper as a reference for an alternate quantification, with its confidence interval. This paper, from a different team and with a different model than DLR, shows a contrails forcing ”44 % lower than current best estimates for 2018” (quote), e.g. than Lee *et al.*. Using this quantification, we showed a significantly reduced temperature impact than with the reference parameterisation (see Fig. 6b).

We cannot say at this point if this result, in addition to the to-be-published work from Bickel *et al.* (2024) and others like Quaas *et al.* (2021), will move the consensus to lower contrail warming than Lee et al. However, these recent publications are strengthening our view that the field needs lightweight models that openly publish their underlying climate sensitivities with their associated uncertainties, and allow those to be changed easily, like we are doing here. This is needed to be able to illustrate the very dynamic state of science on this question, and its possible policy implications.

For an additional highlighting of these discussions in the paper, we have restructured the paragraph devoted to climate uncertainties on contrails by separating the state of the art from our results. we have also added the two Bickel *et al.* references to discuss efficacy values.

Minor comments

> Line 30ff.: "The debate on the choice of CO2-equivalence metrics could then focus on methodological and ethical criteria.". Since the manuscript does not mention any of these "ethical criteria" later on, it might be best to remove this mention, or back it up with references to relevant scientific discussion on the ethical implications of emissions metrics later on in the text.

We believe that the mention of the ethical issues associated with the choice of metrics is indeed relevant. However, we indeed only briefly mentioned the paper by Meinshausen and Nicholls (2022) in the Discussions section of the initial submission. We modified this paragraph adding a direct link with the initial reference, an example with a reference to del Prado *et al.* (2023), and a general comment indicating that discussions concerning ethical criteria are out of the scope of the paper.

> Line 105: "but do not take geographical disparities into account.". The reader might appreciate a more concise definition of "geographical disparities" in this context.

The formulation was ambiguous. We modified the sentence by "but cannot model the geographical dependence of the radiative impact of some species". We also modified a similar term in the discussions.

> Line 112ff.: "The choice of the climate metric is crucial and conventional CO2-equivalence metrics are for instance not appropriate in this context [37]." It is not immediately obvious why the choice of conventional CO2 equiv. metrics is not appropriate. A short description, in addition to the provided source, should suffice.

Thanks for the remark, it needs indeed to be explained to the non-expert reader. The following sentence has been added in addition to the reference: "Indeed, the latter, used to compare a species with CO_2 emissions over a certain time horizon, treat SLCFs, such as aviation non-CO_2 effects, as 'stock' pollutants, whereas the change in SLCFs emission rates is necessary to assess their temperature response, in particular when SLCFs emissions are stable or declining".

> Figure 1 caption: The main text details the differences between the climate models. Since these are important for a correct interpretation of the figure (ie. the differences between the lines), I suggest at least an explicit acknowledgment of the differences in the caption itself – which of course can refer to the main text for further details.

A sentence has been added in the caption for a better understanding of the figure.

> Figure 3: The temperature change curve for the "updated settings" model appear greatly discontinous, exhibiting the least degree of temporal smoothing. The reader would surely appreciate an explicit acknowledgement and interpretation of this observation.

Indeed, this behaviour is due to the calibration of this model. The following sentence has been added: "The extremely rapid change in temperature with the GWP* method with the updated settings is due to the fact that this model is calibrated with a very low value for the time parameter to represent the very short lifetimes of non-CO_2 effects, and in particular contrails (see Methods section)". The Methods section provides the model's settings and a comment on the abrupt changes in temperature.

> Line 575ff.: "Lastly, the forcing from aerosol-cloud interactions caused by aviation aerosols has no best estimate given in [1], while very recent works such as [44] are giving a significant cooling estimate.". The transparency regarding the large uncertainties originating from this effect are appreciated. However, it might be use for for the reader to briefly mention this limitation earlier in the text as well.

The aerosol-cloud interactions were briefly mentioned in the introduction. Some details have been added (l.76), and a reference to the introduction has been added in this sentence.

Changes Required by Reviewer 4

General comments

The authors define a set of rapid, low-complexity, aviation-focused climate emulators and assess their usefulness in evaluating aviation impact mitigation strategies, and to advance the debate around climate impact metrics in this space. Using these simplified models, they first find that the impact of historical aviation emissions on surface temperatures as calculated with the reference AirClim model can be broadly reproduced. They then compare the results of the different low-order models for a suite of different mitigation scenarios, showing that the choice of modelling approach significantly affects the estimated temperature change. They also explore the role of different climate metrics. The authors conclude that low-order climate models can provide valuable insights, but that the current debate around CO2e metrics may be overstated. The central question posed by the authors is interesting and important. Being able to rapidly assess the effect that different aviation scenarios will have on the environment is important in order to allow technologists and decision makers to act decisively, but most models currently available are either too costly/complex to be used in this fashion or are so heavily simplified that they do not provide useful information. Furthermore, there is a debate raging regarding how to compare strategies even when the outcomes can be quantified. However, the reduced order models (ROMs) proposed and demonstrated by the authors cannot be properly assessed due to the lack of a high-fidelity control. Furthermore although I agree that the debate around emissions metrics is not particularly helpful, the proposed solution – to instead try and use ROMs to calculate temperature change directly – is not a solution because it just results in another equivalency metric. Overall, while I agree with the goal of this paper I cannot recommend it for publication without a very significant amount of new work being added and broad structural changes. I have elaborated on these needs below, along with a short list of less significant issues.

Dear reviewer, we would like to thank you for the comments that helped improve the paper. The answers to the various remarks are given as follows, but first, we would like to quickly respond to your general comments. Regarding your first point on the validation of reduced order models, we believe that our approach, based on comparison with accessible data from published and peer-reviewed academic literature, is the best we can do without access to high-fidelity models. We believe that this approach is sufficient as the comparison models themselves have been validated, even if we agree that these reduced order models (such as the higher fidelity ones) are still limited for informing policy makers due to significant uncertainties. Regarding the use of reduced order models, these are not necessarily based on the use of equivalence metrics, such as the FaIR-based approach. It could also be argued that GWP* is more of a (micro) model than a metric. In both cases, we have revised the structure, wording and explanations to address your various points. Further details are provided below.

Major comments

The paper makes two major claims: that reduced order climate models are possible for evaluating aviation sustainability strategies, and that these models can be used to bypass the debate regarding CO2 equivalence. On the first point I generally agree but do not believe that this paper achieves that goal. Two general approaches are suggested (lines 136-151): either to use a GWP*-based quantification of warming with a TCRE translation into warming, or to use open-source climate emulators with adjustments for aviation-specific contributions. As stated on line 153 the authors seek to validate these models; however the only comparisons made (see figures 2 and 3) are to other reduced order models (lines 177 to 186). The Grewe et al. AirClim model is itself a linearization, while the Klower et al. approach uses fixed factors for emitted species. Neither approach is appropriate for validation of a reduced order model. I would strongly recommend that, if the goal is to produce accurate reduced order models, the authors seek data from climate modelers who have run multiple aviation scenarios using a current-generation climate model with multiple ensemble members. Without a real reference case, the argument that these ROMs bring value is actually reversed. Figures 3, 4, and 5 show that the relative benefit of different mitigation scenarios is quite different depending on the choice of ROM, but there's no clear evidence that this disagreement reflects real uncertainty. It seems instead that the bottom line of this paper is that the current suite of ROMs is not fit for purpose; a relatively minor tweaking of parameters results in different rank-ordering of mitigation strategies, even though nothing is being physically changed in the underlying understanding of the physics. The conclusion I draw is that more work is needed to develop ROMs which can be shown to provide results consistent with larger climate, models, and that until that is the case, lightweight models of aviation climate impact could cause the drawing of incorrect conclusions and are not fit for purpose.

The validation we have carried out is based on models and results with accessible data that we have been able to find in the literature. While we agree that validation against higher fidelity models would be ideal, we were not able to have access to any of the very few that include an aviation climate impact modelling consistent with the current state of the art. Nevertheless, we believe that the validation we have carried out remains relevant, albeit imperfect. Indeed, we have compared our results with peer-reviewed reference data. For example, AirClim, although itself a linearisation that could also be confronted to higher fidelity models, is widely adopted for assessing aviation scenarios and provides a reasonable reference given its extensive use. FaIR2.0 itself is calibrated for GHGs and aerosols against results from 28 higher fidelity models, as detailed in Table 2 from Leach *et al.* (2021, doi:10.5194/gmd-14-3007-2021). This calibration lends confidence to its ability to represent broader climate dynamics, and its use in IPCC AR6 further demonstrates its recognition in the scientific community.

We agree that we are not yet in a position to adequately influence regulations with our model, due to both its limitations and the significant remaining underlying uncertainties (which we can at least parameterise and assess with our models). However, one could argue that the latter comment also applies to high-fidelity models and recent observational studies quantifying the climate impact of aviation, given the remaining uncertainties and scatter of recent results, as highlighted for example in the response to your comments on contrails (see below). This means that even the use of high-fidelity models to adequately inform policymakers may be premature at this stage.

We also agree that such calibration by high fidelity models is urgently needed: calibrating similarly aviation species from systematic results from higher fidelity models is an objective. We are aware of a very recent initiative to make AirClim open access (see https://github.com/dlr-pa/oac), which could be a first step towards a proper calibration in the future. We are also aware of a similar initiative, that is part of the French "Climaviation" research programme, on the development of a version of the OSCAR climate emulator adapted to the climate impacts of aviation from high-fidelity modelling.

In response to your requests, we have updated the abstract and the "Limitations and future work" section to acknowledge this need for caution when using simple models. We have in particular added a full paragraph.

> The second claim in the paper is also problematic. A great deal of discussion (lines 492 to 541 in particular) is spent on showing that conventional metrics of global warming do not necessarily translate consistently to changes in surface temperature. However, the authors do not demonstrate why this matters, and show only that if you use one metric (e.g. GWP20), you get different results than if you use an ROM to calculate temperature change. This seems to fundamentally misunderstand the meanings of the metrics. It is well understood that GWP20, 50, and 100 are not themselves comparable metrics of warming, but rather can be used to understand the different horizons over which additional radiative forcing is expected to take place. The authors seem to assume that "warming equivalent metrics" are widely accepted as being the goal, but if this was the case then we would not be currently debating whether GWP, GWP*, GTP, ATR, or any other CO2 equivalence metric is preferable. The fact is that different stakeholders have different perspectives (based on e.g. vulnerability to different elements of climate change, expected growth in economic output affecting their discount rates, near-term resiliency, and so on) and therefore will always need different metrics.

We think there has been some misunderstanding of the message we were trying to get across. First of all, we are not saying that metrics are useless; they are still necessary for various purposes (optimising an aircraft design or a flight trajectory with an objective function, pricing non-CO2 effects, etc.). And in this case, of course, there are many trade-offs, depending on the stakeholders involved, such as short- and long-term goals. Nevertheless, we maintain that for certain issues, such as the evaluation of a mitigation strategy, temperature assessments (using reduced-order models) are relevant and make it possible to avoid the debate on the choice of equivalence metric. This method also allows the policy to focus on objectives rather than metrics, which is easier to explain and disseminate to a wide audience. We have reworded certain passages to emphasise this specificity to reconcile these visions, in particular in the "Co_2-equivalence metrics, temperature estimates and policy implications" subsection.

Furthermore the closing statements of the paper suggest that we should set a target for aviation of zero warming after a given date, and use simple models to evaluate mitigation strategies. The problem with this approach – and indeed with any GWP*-style approach – is that it sets impossible goals for the industry. As discussed in this paper, the amount of warming from aviation (as calculated by high-fidelity models) is itself dependent on the background scenario, meaning that there is no way for aviation to guarantee no further warming. The core consideration of this which I believe is missing from this paper is the question of how to set robust metrics of aviation's contribution to climate change. I recommend that the authors consider significantly restructuring the paper to focus more on the question of how to provide decision makers in industry and policy with the tools to produce robust recommendations for the aviation industry which are not overly sensitive to changes in non-aviation factors.

Our point with this closing statement: "Objectives in terms of temperature (e.g. no additional warming after a given date) could be set for aviation, as discussed in..." is that, once freed for the question of "which metric", the debate could refocus more usefully on setting climate objectives for aviation and assess mitigation strategies with models. The mentioned objective of no additional warming after a given date was just intended as an example from existing literature (we used "could" and not "should"). We added another example target in our revision in order to clarify our message. We absolutely do not advocate a specific target in the paper, which is partly a political choice indeed.

We totally agree though that setting such objectives with confidence is challenging due to the heavy dependence of aviation impacts to background emissions, as discussed in our paper as you have seen. Producing metrics for the aviation industry "which are not overly sensitive to changes in non-aviation factors" seems difficult to us, especially for NO_x, possibly as well for contrails and particulates/aerosols emissions due to its interference with cloud coverage. We confess that this is a topic we have not addressed at all. We tend to think instead that a scenario-based approach relying on models (high fidelity, or properly calibrated and scenario-dependant low fidelity ones), as per the usual IPCC ways of working, is sensible, and relevant to answer questions such as: what would be a relevant target for aviation in this or that specific RCP emissions scenario? This is achievable even with a simple GWP* approach, provided that sensitivities used for ERF estimates varies with time, e.g. implicitly with the evolution of background emissions, from results of higher fidelity models. This is possible as well with FaIR since it manages the atmospheric contents and cycles. We actually tested this approach in a simplified manner in our paper, in the "NO_x uncertainties" part: we have used sensitivities to NO_x emissions from Skowron *et al.* (2021) which are only valid for a RCP 4.5 background.

With such an approach and when available, mitigation scenarios against various RCP scenarios can be tested, and possibly some strategies for aviation that would be relatively robust against the scenario (and as well problematically, robust enough against the remaining large uncertainties on the impact of specific species) could be determined, a work that remains to be achieved.

We have modified our "Limitation and future work" subsection by adding a paragraph specifically acknowledging these limitations and highlighting the required research program, as also mentioned in our previous answers.

Minor comments

> The quantification of contrail uncertainty is somewhat biased by the use of results from Teoh et al.; it is likely premature to "bake in" the assumption that SAF will reduce contrail warming (line 373). This is a contentious issue and has only been shown in modelling, mostly using a single contrail model (CoCiP); to date this result mostly relies on an assumption that a reduction in emitted number of particles will result in shorter-lived contrails. I recommend that the authors survey the literature thoroughly and attempt to assess the degree to which this can be considered a consensus result.

Regarding the first point, we agree with the reviewer that showing solely results from Teoh *et al.* as an alternate example of quantification of contrails radiative forcing is limited in scope. We chose to do so in light of two other publications that we are quoting, both posterior to the Lee *et al.* meta-study: Quaas *et al.* (2021) which gives a quantification from observations remarkably close to Teoh's, and Digby *et al.* (2021) which proposes an even lower ROM quantification, even though the authors are warning against premature conclusions to be drawn from their "highly simplified approach to a very nuanced problem" (quote). Both results, together with the papers from

Bickel et al. (including this abstract from EGU2024, doi:10.5194/egusphere-egu24-18183), seem to show a recent downward trend on the assessment of yearly aviation contrails forcing, that we have chosen to illustrate with Teoh *et al.* values, even though of course more publications are needed before building an updated consensus. We have updated the paragraph on contrails uncertainties for highlighting these results.

Similarly for the second point, we agree that the assumption that SAF are reducing contrail warming still needs to be confirmed. Still we remark that, since our first submission, the ECLIF3 results have been published (doi:10.5194/acp-24-3813-2024) and are showing a measured reduction of both particles and ice crystals with SAF. We have added a reference to this new paper in our revision (introduction and Methods section). Nevertheless, we acknowledge that these new results do not fully prove that the contrails formed are shorter lived and have less impact (forcing and temperature results in the ECLIF3 paper are sill the outcome of modelling, only the reduced number of ice crystals in measured).

So to the point, we agree that the non-CO_2 climate benefits from SAF are not fully established yet, and that we were not cautious enough in our statements, especially in the scenario analysis part, and have updated the following text: "Additionally, we notice a decrease in the impact of contrails, as the model incorporates the impact of using low-aromatic fuel on reducing contrail cloudiness, *even though the underlying benefit on contrails still needs to be confirmed*." Moreover, in the Methods section (Non-CO_2 mitigation modelling/Use of alternative fuels), we are now highlighting that the curve from Burkhardt *et al.* (2021) that we are using comes from a reduced set of publications and needs to be confirmed.

For information, we have also mentioned, in the "Relevance of aviation lightweight climate models" subsection, that the lack of proper quantification of all sulphur impacts "makes it problematic to assess the pertinence of acting on aviation fuel properties".

Lastly and regarding both points, we fully acknowledge the uncertainties and contradicting views in literature on both contrails impact quantification, and its mitigation via fuel properties. This state of the art underscores the value of publishing lightweight models that clearly and transparently disclose their underlying parametrisations, enabling easy updates as new findings emerge or consensus shifts. We believe that this flexibility makes them a valuable contribution to the field.

Several of the figures are extremely difficult to parse. For example, without referring to the text it is impossible to determine what the meaning of Figure 9 is, as each panel is simply labeled "Trend scenario" without indicating that the difference lies in the amount of growth.

Thank you for the comment, in particular on Figure 9. This figure has been updated. Moreover, a more detailed description has been added to the title of some figures to make them easier to understand.

While I actually agree with the statement on line 630 that "quantifying SLCFs [sic] agents, including aviation non-CO2, separately in emission reporting is a much better choice than aggregating those in usual CO2-equivalence metrics", I do not agree that GWP* or FaIR are better than CO2 equivalence for comparison of those non-CO2 emissions. The citations provided only reflect one view of the aviation impacts research community. I would recommend expanding the cited works to reflect the fact that, while emissions reporting is useful, there is no consensus that approaches such as GWP* or FaIR are the appropriate mechanism to then compare them side by side.

We recognise that our paper, by focusing on the advantages of using temperature change estimates directly via reduced order models, does not mention the advantages of conventional metrics, only their disadvantages. We have revisited the last section (CO_2-equivalence metrics, temperature estimates and policy implications) in order to highlight the advantages of using conventional metrics, which we do not deny, such as their simplicity, their wide distribution, their possible application in aircraft design or for economic schemes. Nevertheless, we believe that it is easier and more appropriate to directly estimate the temperature change due to a fleet and its emissions, rather than aggregating equivalent emissions, when comparing two mitigation strategies. We have modified the corresponding section, in particular by adding a last paragraph to discuss the advantages of using conventional metrics (and the interesting coupled use with lightweight models).

There are small typos throughout the manuscript (mostly grammatical); I recommend the authors sweep the document once more to try and eliminate these.

We have proofread the text to remove existing errors. We hope that most of them have been removed.

Changes Required by Reviewer 4 – Round 2

I appreciate the thoughtful and well-reasoned response which the authors have given to my review, and the changes made to the manuscript. I have focused my comments on the changes made in response to my prior concerns, and on the authors' rebuttals.

Despite the efforts of the authors, my most significant concern remains. I interpret the results of this paper as being in direct opposition with the title. As discussed during the review, neither high-fidelity models nor the ligthweight ROMs which attempt to emulate them are consistent with regards to the effects of aviation; as the authors stated in their response, "even the use of high-fidelity models to adequately inform policymakers may be premature at this stage". This begs the question then of why the title states "lightweight climate models are useful tools for assessing aviation mitigation strategies".

Secondly, I think it is necessary to insist that global surface temperature is itself a metric, albeit one which takes the form of a time-series rather than a single number. As such, I do not agree that using a ROM-based estimate of temperature change bypasses the need for CO2-equivalence metrics, as is currently stated in the abstract; it merely introduces another, more complex and model-specific metric to be listed alongside all the existing ones. It is also not clear that the timeseries of temperature change is the end goal as a metric. I believe it is important to the interpretation of this paper to understand whether the authors are arguing that the ultimate metrics should be a globally consistent quantity such as mean temperature (i.e. arguing that we should now focus on refining estimates of temperature change), or whether instead metrics should continue to advance in complexity with the capabilities of ROMs so that individual stakeholders can get more and more information on their specific priorities.

Nonetheless I appreciate that the authors have made a sincere and thorough effort to address my concerns, and that the manuscript now better discusses the limitations of the arguments made. With this in mind, and recognizing that there is unlikely to be a major revision of the paper at this time, I would suggest instead two changes. First, and in light of the fact that the authors agree that these models are not yet appropriate for policy decision making, I would propose that the title be changed to (perhaps) "Why simplified climate modes could become useful tools for assessing...". Second, I would ask that the authors discuss why a timeseries of temperature change is the best metric to use, rather than - say - pattern-scaled maps of surface temperature, region-specific temperature change estimators, or even impact-specific metrics.

Dear reviewer, we would like to thank you for the comments and interesting discussions. We are pleased that our changes have largely answered to your feedback, which has enabled us to significantly improve the quality of the paper. We answer to your two last suggestions in the following.

First, we have changed the title of the paper for: "Lightweight climate models could be useful for assessing aviation mitigation strategies and moving beyond the CO_2-equivalence metrics debate". We think this new title responds to your comment and keeps the number of words to a reasonable level for the editors.

Regarding the discussion on temperature time series, we have modified an existing short paragraph for adding this full paragraph in the discussion section. We think it answers to your main question.

"In other words, reasoning in terms of temperature impact remains probably the best choice for obtaining results on mitigation levers. Indeed, average temperature change is one of the ultimate indicators used to set overall climate policy goals, such as the Paris Agreement's "well below 2°C above pre-industrial levels" [UNFCC 2015]. This value is also simpler to use than region-specific temperature change. Then, temperature change expressed as a time series allows for the crucial arbitration between short-term and long-term climate impacts. For example, in a 1.5°C scenario, limiting the peak and duration of warming can help avoid long-term or irreversible damage to ecosystems or society [IPCC 2018]. Starting from such an overall climate objective linked to a clear policy goal, a specific objective in terms of temperature impact over time could then be cascaded down to aviation, inscribed within a global non-aviation emissions scenario. An example of this is a maximum induced temperature change target associated with no additional warming after a given date, as discussed in [Sacchi 2023, Brazzola 2022]. Doing this accurately as a function of time is particularly important because of the relative weight of non-CO_2 effects in aviation climate impact, and especially for operational contrail mitigation which, if proven effective, could provide a significant temperature benefit over the next few decades."