

EDITORIAL

Reviews and Responses for **Assessing Climate Effects Resulting From Airspace Closures Following the Ukrainian Crisis**

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Reviewers: Saskia Drossaart van Dusseldorp, Stefan Fluck

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1. Original paper

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2. Review - round 1

2.1 Reviewer 1

General Feedback:

This is a very interesting study and a practical approach to the topic, building on an existing model work flow - a great use-case example. I appreciate that the climate impact of non-CO₂ emissions is considered as well. With improved framing of the scientific background on non-CO₂ effects, a more comprehensive treatment of uncertainties, and clarification of methodological assumptions, the paper will make a valuable contribution to the field.

Introduction:

The historical context provided in lines 13-28 does not significantly contribute to the scientific narrative of the paper. I recommend beginning the introduction at line 29, which aligns more directly with the study's focus.

Similarly, the discussion of economic studies (lines 48-61) feels somewhat tangential. While I understand the thematic overlap in terms of rerouting and fuel flow, economic aspects are not substantively integrated into your own analysis beyond a brief mention in the discussion. Unless further integrated, I suggest omitting this content for clarity and focus. See notes on that below.

Most importantly, the introduction lacks a clear articulation of the relationship between rerouting, fuel consumption, non-CO₂ effects, and resultant climate impacts. Although the paper refers to the dependency of climate impact on the location of emissions, the theoretical underpinning is insufficiently developed. It is crucial to explain the physical basis of how non-CO₂ emissions influence the radiative balance, and why these effects vary with latitude and altitude. What qualitative or quantitative impacts can be expected from different emission locations? Furthermore, the significant uncertainties surrounding non-CO₂ climate effects merit more prominent discussion. While you mention in the conclusions that Dahlmann et al. demonstrated the relative insensitivity of model

output to uncertainty assumptions, a more holistic acknowledgment of the absolute magnitude and nature of uncertainties in this area is necessary for a balanced discussion.

Line 74: “While these results illustrate operational consequences as well as higher climate impact from CO₂ emissions following the imposed sanctions, there is a research gap regarding the change in climate impact resulting from the required re-routings.” This sentence is contradictory. I think you mean “[... research gap regarding the change in climate impact from non-CO₂ emissions resulting from the required re-routings.”

Method and Data:

Line 135: You state that emissions are “based on fuel flow values from flight performance simulations.” Please provide a more specific reference or description of these simulations.

Line 139: You reference emission indices derived from testbed measurements. A brief discussion of how representative these are of real-world in-flight emissions would enhance transparency. How might atmospheric conditions—such as pressure, temperature, altitude, or wind—alter these indices?

Line 151: The term “acceptable limits” requires clarification. How are these limits defined and applied in your modeling framework?

General question about the study design: Do you have information on how much the observed OD flight paths would deviate when observed in two different time spans, in the absence of airspace closures? Could there be a “background variability” due to standard flight planning, weather dynamics, or operational constraints that should be accounted for?

Line 200: You mention excluding effects “shortly before and after the start of war.” Could you elaborate on what these effects might be, and how you ensured that your selected dates were unaffected?

Results:

Line 277/278: “This can be explained by the shift to lower latitudes (e.g. approx. -20% in mean latitude for UAL889; see Figure 5, right) as well as lower flight altitudes due to the longer flight mission and higher take-off weights.” This is a critical result, but the explanation needs to be expanded. Why would lower latitudes and altitudes reduce climate impact from fuel burn? This ties back to the earlier need for a stronger theoretical background on the altitude and location sensitivity of non-CO₂ effects.

Lines 296 and 306: Minor typos—“Cic” should be corrected, and “form” should be “from.”

Discussion and outlook:

General remark: Your acknowledgment of uncertainties related to non-CO₂ effects is appropriate but remains somewhat superficial. As emphasized earlier, a more thorough discussion of the scientific uncertainties and limitations would be beneficial and is necessary given the centrality of these effects to your study’s novelty.

Line 367: “Furthermore, economic disadvantages can be confirmed for airlines that have to follow the sanctions and avoid Russian airspace in contrast to airlines which are still allowed to cross Russian airspace.” Because of the increased fuel burn? This statement implies a conclusion not fully supported by your analysis. If this point is to be retained, it needs to be grounded in your results or tied more explicitly to referenced economic studies. Otherwise, I suggest removing economic implications from both the introduction and discussion to maintain focus.

Line 381: “Further simplifications have been assumed in the context of this study assuming a constant load factor for all flights as well as efficient re-fueling and excluding realistic meteorological conditions especially in the context of head and tail winds.” This is an important limitation. Please

elaborate on how such simplifications might bias the results—could they lead to over- or underestimation of climate impacts?

2.2 Reviewer 2

I would like to congratulate the authors for this well performed analysis on the impact of airspace closures following the war in the Ukraine. I believe that the modelling pipeline is appropriate and sound for the research question and the results are well presented and commented. Questions that arose during reading were all answered latest in the discussion section. The results and thus this paper is a valuable contribution to the ongoing research and legislative discourse on the topic of contrail and non-CO2 impact, highlighting that the non-CO2 effects are a multi-layered, complex topic.

I believe the paper is good as is. The only comment I have for the authors to consider is in regards to the description of AirClim and its climatological assessment approach outlined in chapter 2.3, p 5, line 172ff. I believe that a reader not familiar with AirClim, CoCip and other climate impact model approaches could benefit from some guidance on the consequences of the fact that AirClim does not consider actual meteorological data and represents a statistical average over multiple years. Especially since the analysis performed in chapter 3.1 is looking at a narrow temporal subset of two single days, compared with a year in 3.2. I believe that a sentence in chapter 2.3 in line 175 or further below in chapter 3.1 could help clarify that the underlying meteorological boundary conditions are of a more statistical nature, but in reality the non-CO2 climate impact of the individually listed flights could be varying a lot due to variations in weather on these particular days.

Last and not least, there are two typos, one in line 306 ("resulting fORm") and in the abstract (The analysis is builT on open-source...)

3. Response - round 1

3.1 Response to reviewer 1

1. The historical context provided in lines 13-28 does not significantly contribute to the scientific narrative of the paper. I recommend beginning the introduction at line 29, which aligns more directly with the study's focus.

Response

We agree and thus have shortened the introduction section with regard to the historical background to support the scientific narrative of the paper (lines 13-15).

2. Similarly, the discussion of economic studies (lines 48-61) feels somewhat tangential. While I understand the thematic overlap in terms of rerouting and fuel flow, economic aspects are not substantively integrated into your own analysis beyond a brief mention in the discussion. Unless further integrated, I suggest omitting this content for clarity and focus. See notes on that below.

Response

We agree that economic aspects are not in detail considered in our analysis. However, we find it important to mention them especially in context of the following analysis as we identify changes in fuel consumption and flight time for different airline types. As these parameters directly influence operating cost, we wanted to consider this aspect at least in a qualitative discussion. Nevertheless, we decided to rearrange the section on previous research to put more focus on ecologic aspects to address the reviewer's remark (line 47 ff.).

3. Most importantly, the introduction lacks a clear articulation of the relationship between rerouting, fuel consumption, non- CO_2 effects, and resultant climate impacts. Although the paper refers to the dependency of climate impact on the location of emissions, the theoretical underpinning is insufficiently developed. It is crucial to explain the physical basis of how non- CO_2 emissions influence the radiative balance, and why these effects vary with latitude and altitude. What qualitative or quantitative impacts can be expected from different emission locations? Furthermore, the significant uncertainties surrounding non- CO_2 climate effects merit more prominent discussion. While you mention in the conclusions that Dahlmann *et al.* demonstrated the relative insensitivity of model output to uncertainty assumptions, a more holistic acknowledgment of the absolute magnitude and nature of uncertainties in this area is necessary for a balanced discussion.

Response

From our point of view, a detailed description of the atmospheric processes is considered out of scope for this study. However, we agree that it makes sense to include additional information on the non- CO_2 effect and underlying dependencies in the introduction. To this end, we have also extended the section on aviation climate effects highlighting the influencing factors of non- CO_2 effects and how this could influence climate impact changes from re-routing (lines 21 - 37). Moreover, we have also included further information on the associated uncertainties in this area (lines 37-41), and extended the discussion of uncertainties with possible effects of overestimating non- CO_2 effects (lines 407 - 414).

4. Line 74: "While these results illustrate operational consequences as well as higher climate impact from CO_2 emissions following the imposed sanctions, there is a research gap regarding the change in climate impact resulting from the required re-routings." This sentence is contradictory. I think you mean "[... research gap regarding the change in climate impact from non- CO_2 emissions resulting from the required re-routings."

Response

We rephrased to "the change in climate impact from CO_2 and non- CO_2 emission species caused by the required re-routings." (lines 77-79).

5. Line 135: You state that emissions are "based on fuel flow values from flight performance simulations." Please provide a more specific reference or description of these simulations.

Response

Flight performance simulations to obtain fuel flow values have been described in the previous section 2.1, so we included a reference to this part of the method description (line 138).

6. Line 139: You reference emission indices derived from testbed measurements. A brief discussion of how representative these are of real-world in-flight emissions would enhance transparency. How might atmospheric conditions-such as pressure, temperature, altitude, or wind-alter these indices?

Response

The effect from atmospheric conditions (especially pressure, temperature and wind) influences the emission indices in-flight. However, this is considered in the fuel-flow correlation methods as described. We have rephrased the sentence to stress this point (lines 143-145).

7. Line 151: The term "acceptable limits" requires clarification. How are these limits defined and applied in your modeling framework?

Response

We have specified this to "deviations of up to 2%" (line 156-157).

8. General question about the study design: Do you have information on how much the observed OD flight paths would deviate when observed in two different time spans, in the absence of airspace closures? Could there be a "background variability" due to standard flight planning, weather dynamics, or operational constraints that should be accounted for?

Response

Yes, this could be the case, but is considered outside the scope of this study from our point of view. Nevertheless, this is an interesting aspect that we have added to the discussion (lines 416-417).

9. Line 200: You mention excluding effects "shortly before and after the start of war." Could you elaborate on what these effects might be, and how you ensured that your selected dates were unaffected?

Response

By doing so, we wanted to make sure that stable operation pre and post invasion is in place, e.g. sanctions have been set-up and airlines have adjusted their operations to the post invasion conditions. We have added additional information for further clarification (lines 209-211).

10. Line 277/278: "This can be explained by the shift to lower latitudes (e.g. approx. -20

Response

Lower latitudes reduce the climate impact to due lower climate sensitivity at lower flight altitudes (see extended description of non-CO₂ effects in introduction, lines 25-37). The latitudinal dependency is mainly caused by the lower tropopause altitude for lower latitudes (e.g. Dahlmann et al. 2016). We have added information on the species-individual effects for further clarification (lines 291-295).

11. Lines 296 and 306: Minor typos-"Cic" should be corrected, and "form" should be "from."

Response

Typos were corrected.

12. General remark: Your acknowledgment of uncertainties related to non- CO₂ effects is appropriate but remains somewhat superficial. As emphasized earlier, a more thorough discussion of the scientific uncertainties and limitations would be beneficial and is necessary given the centrality of these effects to your study's novelty.

Response

A detailed quantitative analysis of the uncertainties of non-CO₂ effects is considered out of scope for this study as we focus on the detailed trajectory modelling of flight trajectories and an application of an existing climate impact modelling tool chain. Nevertheless, we have added some additional information how uncertainties of non-CO₂ effects could potentially influence the study results (lines 407-414).

13. Line 367: "Furthermore, economic disadvantages can be confirmed for airlines that have to follow the sanctions and avoid Russian airspace in contrast to airlines which are still allowed to

cross Russian airspace." Because of the increased fuel burn? This statement implies a conclusion not fully supported by your analysis. If this point is to be retained, it needs to be grounded in your results or tied more explicitly to referenced economic studies. Otherwise, I suggest removing economic implications from both the introduction and discussion to maintain focus.

Response

We agree that we have not in detailed modelled the economic aspects, however changes in fuel consumption and flight time directly imply changes to direct operating cost. Therefore, we rephrased the sentence (lines 384-386).

14. Line 381: "Further simplifications have been assumed in the context of this study assuming a constant load factor for all flights as well as efficient re-fueling and excluding realistic meteorological conditions especially in the context of head and tail winds." This is an important limitation. Please elaborate on how such simplifications might bias the results-could they lead to over- or underestimation of climate impacts?

Response

These effects typically lead to a slight underestimation of the fuel consumption and associated emission quantities (see Weder *et al.* 2025). Due to the relative comparison in the course of this study, we do not expect a significant impact of these inaccuracies on the study results. We have included this additional information in lines 400 - 402.

3.2 Response to reviewer 2

We thank Mr. Fluck for his very valuable feedback on our paper. We have included changes to our manuscript addressing his remarks as follows:

1. The only comment I have for the authors to consider is in regards to the description of AirClim and its climatological assessment approach outlined in chapter 2.3, p 5, line 172ff. I believe that a reader not familiar with AirClim, CoCip and other climate impact model approaches could benefit from some guidance on the consequences of the fact that AirClim does not consider actual meteorological data and represents a statistical average over multiple years. Especially since the analysis performed in chapter 3.1 is looking at a narrow temporal subset of two single days, compared with a year in 3.2. I believe that a sentence in chapter 2.3 in line 175 or further below in chapter 3.1 could help clarify that the underlying meteorological boundary conditions are of a more statistical nature, but in reality the non-CO2 climate impact of the individually listed flights could be varying a lot due to variations in weather on these particular days.

Response

We have included further information on AirClim as the applied climate response model in section 2.3 (lines 177-184). Furthermore, our study is based on the assumption that the selected days with their respective weather situation can be interpreted as representative for a larger time scale in terms of lateral and vertical shift of emissions. We have added this aspect for further clarification in chapter 2.4 (lines 221 - 223).

2. Last and not least, there are two typos, one in line 306 ("resulting fORM") and in the abstract (The analysis is built on open-source...).

Response

We have corrected the typos in the manuscript.