Review of the manuscript “The importance of an informed choice of CO_2-equivalence metrics for contrail avoidance” by A. Borella et al. (egusphere-2024-347)

Recommendation

This is an appropriate and timely contribution to the rapidly emerging discussion on how to assess the climate effect of contrail avoidance measures. It certainly contains a lot of insightful information for scientists and stakeholders alike. The strengths of the paper are to be found in the clearly presented guiding ideas that highlight and illustrate the main problems when quantifying the climate impact gain of contrail avoidance. I am especially pleased with the inclusion of surface temperature change efficacy in the metrics calculations, as this aspect has been disregarded all too often in previous respective assessments. Overall, the conceptual framework and the metrics calculations seem fully adequate to me.

However, I also perceive a number of shortcomings in the presentation of the results, both with respect to a precise description of the methods and to a coherent illustration of technical details. A particular weakness is insufficient explanation of some figures and a lack of hints to the additional material in the supplement. Transferring text from the figure captions and/or from the supplemental material to the main text may be *one* idea for amendment. As the paper deals with a topic of high socio-economic relevance, it is essential to avoid creating any kind of misunderstandings and confusion. Therefore, I encourage the authors to ensure maximum precision all through the text, also when describing the details. I will give a number of major and minor points where I see room for improvement in this respect.

I recommend the paper to be accepted for publication in ACP after those points have been addressed.

Major Concerns

1. Formulations in the abstract are generally sound and transparent, except for the final statement that “the choice [of metrics] is ultimately political”. I think that could be expressed more clearly, e.g. “the choice of metrics implies to lay a focus on climate impact reduction on shorter or longer time scales, which is ultimately a political decision”. In case that is what you meant to say. Any ascertainment should then be naturally emerging from the discussion in the conclusions section.
2. The discussion of the results presented in Figure 3 is much to brief and poor guidance is given to the reader to relate the text interpretation to certain figure features. This is emphasized by the fact that the text gives contributions in %, while the figure provides the AGWP/ATR/AGTP values in absolute units. E.g., one feature needing explanation is the CO_2 ATR independence from the time horizon, while there is a respective dependency for the end-point metric AGTP. Is there an obvious reason why the relatively large number of contrails with negative EF (line 155) contribute so little to ATR/AGTP? Please realize that these results need to be plausible also to non-experts, and adjust/extend the text accordingly.
3. In Figure 5, again, the discrepancy between the relative numbers (%) discussed in the text and the absolute numbers (thousands of flights) in the figure makes it difficult to follow the reasoning. I urgently recommend to insert a complementing scale of percentage-numbers along the right y-axis. The whole discussion, however, seems to be rather fuzzy here. The sentence “The number of “lower risk”

reroutings is 550% larger" (hardly comprehensible in itself) seems to be the only interpretation of the second row of Figure 5, which is clearly insufficient to convince the reader of anything. Please, extend the discussion and refer to exactly the columns you are interpreting.

4. Some confusion already exists in the (sparse) literature on the choice of numbers for certain efficacy parameters in the "radiative forcing" (RF) framework and in the "effective radiative forcing" (ERF) framework, and I feel that your paper could help to amend respective inconsistencies. The paper correctly distinguishes between a contrail "total efficacy" (line 180), which results as the product of an ERF/RF factor (originating from rapid radiative adjustments induced by the contrail forcing) and the ERF related efficacy (line 178) that originates from surface temperature induced radiative feedbacks. You have chosen a value of 0.35 for the total efficacy by multiplying the ERF/RF factor (0.35) from Bickel et al. (2020) with a value of 1 for the ERF related efficacy "for lack of a better estimate". Yet, there is an estimate of the ERF related efficacy and it's given in line 377! While there may be a good reason to choose the values as you do, any source of misunderstanding in the context should be eliminated.
   When it comes to uncertainty considerations in section 5.3, the discussion starts with the sentence "Contrail efficacy has been set so far to the best estimate of 0.35 from Bickel et al. (2020)." This wording is confusing, because the Bickel et al.'s value is actually an ERF/RF factor – as you correctly introduce in section 3. You probably meant to repeat the section 3 definition in a nutshell, but actually this combination of ERF/RF factors and efficacies emphasizes a respective simplifying approach by Lee et al. (2021), who calculated a best estimate for the contrail cirrus ERF/RF factor (value 0.42 – as referenced in line 222) from three individual model results of which two (Ponater et al., 2005, and Rap et al., 2010) had rather provided a total efficacy value. It should be easy to avoid such simplifications in the context of your paper.
   An analogous criticism applies to your choice of "contrail efficacy" values 0.23 and 0.51 (line 377) referring to the confidence interval given by Bickel et al. (2020) – but again that was for their ERF/RF factor. For choice of a lower bound for contrail total efficacy, the ERF related efficacy best estimate from Bickel (2023), i.e., 0.38, is combined with the lower estimate of the ERF/RF factor from Bickel et al. (2020). This is largely okay in a qualitative sense, but it neglects that Bickel (2023) also provides a total efficacy value in his Table 4.3., i.e. 0.21, by combining their 0.38 value for ERF related efficacy with the ERF/RF factor within their own model framework – which is more consistent. Still, given that Bickel's (2023) total efficacy value (0.21) will also have an uncertainty (which is not quantified in his thesis), your estimated parameter range for total efficacy appears quite reasonable to me.
5. The total efficacy as chosen in the paper (0.35) "is assumed to apply for all contrails" (line 182). This may well be acceptable for the conceptual targets of this paper, yet it implies that the rapid radiative adjustments (controlling ERF/RF), as well as the surface temperature induced radiative feedbacks, are independent of where and when a contrail develops. This is, obviously, a very unlikely assumption. The authors appear to be well aware of this fact, as (line 389) they write that the climate benefit from contrail avoidance, when calculated accordingly, is only valid "when considering rerouted flight together, not on a flight-to-flight basis". This overall assessment is likely correct (or, at least, tenable), but I feel that an extra warning is necessary here, in order to avoid the impression that RF (or the

energy forcing, EF) of an individual contrail (or flight) can safely be converted to ERF by using a uniform factor derived from global considerations.

6. There is a further potential source of error when introducing efficacy parameters in metrics calculations, as the CoCiP model primarily uses energy forcing (EF) to assess the climate impact of individual flights (Schumann et al., 2012; Teoh et al., 2020), which can then be used to calculate a global instantaneous radiative forcing (RF_{inst}). However, global ERF/RF, climate sensitivity and efficacy parameters (e.g. Bickel et al., 2020; Lee et al., 2021; Bickel, 2023) are usually given on the basis of the stratosphere adjusted radiative forcing (RF_{adj}). Switching between RF_{adj} and RF_{inst} makes not much of a difference for contrails (Dietmüller et al., 2016, their Table 2), but strongly matters when calculating the tradeoff with the radiative effect induced by additional CO_2 emissions. I presume that, concerning your paper, OSCAR uses additional fuel consumption (rather than a CO_2 EF provided by CoCiP) to calculate the CO_2 RF_{adj}. I deem it important that the text should not leave open any doubt on this (line 160), as otherwise a combination with the published global ERF/RF factors (and efficacies) would be inconsistent (see efficacy fluctuations for various RF definitions, as reported by Richardson et al., 2019, their Table 1).

Specific remarks/recommendations

Pg. 1, l. 17: "… defined here as nine combinations of different definitions …" sounds somewhat strange. Perhaps "… represented here as nine combinations …"?

Pg. 1, l. 19: I recommend to write "Under an idealized scenario where …" to avoid any misunderstanding.

Pg. 2, l. 33: As there is no extra definition section of radiative forcing in this paper, I recommend to insert another sentence here (before "According to"): "The climate effect of CO_2 and non-CO_2 contributions has usually been compared in terms of several radiative forcing parameters (e.g., Ramaswamy et al., 2018) or dedicated metrics (e.g., Fuglestvedt et al., 2010).

Pg. 4, l. 103: "It is impractical …", this statement demands a rationale or a reference.

Pg. 6, l. 170: "… 1726 simulations …"; Why is this selection necessary? Is this relevant for the outcome?

Pg. 6, l. 175: A personal remark at this point: You choose to label the ERF related efficacy (line 178) that originates from surface temperature induced radiative feedbacks as the "Ponater efficacy". While I feel flattered by this labelling, the term might still be misleading, as the cited reference by Ponater et al. (2021) deals with both the RF and the ERF related efficacy parameter and their interrelation, but without giving numbers. Different definitions of efficacy were introduced by Hansen et al. (2005), but the first dedicated calculation of the ERF related efficacy for contrails was given by Bickel (2023), yielding a value of 0.38, as is mentioned in your paper but only later (line 377).

Pg. 6, l. 180: Please, add references to Hansen et al. (2005) and Richardson et al. (2019).

Pg. 6, l. 182: "… the same factor is assumed for all contrails." Does this mean that each contrail EF is multiplied with the assumed contrail efficacy? Is this relevant for OSCAR or does that model only receive global mean contrail RF as an input? – See major concerns.

Pg. 6, l. 187: Recommended modification: "… are illustrated in an idealized but representative way by Figure 1, …"

Pg. 7, l. 202-204: This sounds as if the ocean surface layer absorbs more or less instantaneously the whole energy provided by any forcing and then returns the temperature signal slowly back to the atmosphere, but such a notion would be inconsistent with the known large ocean heat capacity. However, I presume that's not what you mean. Please make it plain that the time limit for the heat transfer from the atmosphere to the ocean is prescribed in your scenario through the length of the "pulse" for which the contrail forcing lasts.

Pg. 8, l. 229: Lee et al. (2021), Appendix E, explicitly states: "When calculating the contrail cirrus ERF, the error range given refers to the error range of contrail cirrus RF and not ERF." Hence, I recommend to modify your formulation to "… confidence level in contrail cirrus ERF, as given by Lee et al. (2021) on the basis of corresponding RF uncertainty considerations."

Pg. 9, l. 244: Choice of the headlines in this section 5 seems a bit incoherent to me. One idea could be to title the subsection here as "5.1 Assessment of an idealized contrail avoidance scenario"; then replace the heading of what is now sub-section "5.1 Sensitivity to additional CO_2" by "5.2 Sensitivity to the amount of additional CO_2 emissions"; and then replace the heading of what is now "5.2 Imperfect contrail avoidance" by "5.3 Sensitivity to imperfect contrail avoidance". The 5.3 heading could remain as it is (but then as 5.4). Please, consider this suggestion as a potential improvement.

Pg. 11, l. 290: "Figure 5 shows …", seems bad wording to me as Figure 5 is in fact dealt with only in the following sub-section. "As will be shown in the next sub-section" may be an improvement. But see major concerns (above) with respect to Figure 5.

Pg. 12, l. 305: "…into the climate damage category, as defined in the previous sub-section"

Pg. 12, l. 310: "550% larger"; I fail to relate this statement to any feature in Figure 5, nor do I understand what the statement actually means (larger than what?). But see major concerns above.

Pg. 14, l. 335: "There is no simple relationship between addition CO_2 and contrail EF": True indeed! The statement could be moved to the begin of the sub-section to motivate why "The results presented in Figure 5 are idealised" (line 332), despite the fact that the additional fuel increment is varied in that figure.

Pg. 14, l. 342: "… the rerouting fails to reduce the energy forcing …" Completely? Why? I fail to perceive the logic in this assumption.

Pg. 14, l 344: "… the average rerouting efficiency is 0.71 …"; How is this number calculated? And how is it, then, used to modify the original results presented in Figure 4? Is this addressed in the following paragraph?

p. 14, l. 351: "In contrast, … temperature rise in 100 years … by only 3%". Which line in Table 1 are you referring to here, which numbers are you comparing? Is "warming in 100 years" (l. 342) referring to something different than "temperature rise in 100 years" (l. 352)? Please, be precise and refer to the actual metric you are addressing.

p. 14, l. 356: "… the absolute benefit comes from flights for which the impact of the additional emitted CO_2 is much greater than the impact of the avoided contrail"; the statement strikes me as illogical. What do you mean by this?

p. 15, l. 371: "Contrail efficacy has been set to the best estimate of 0.35 from Bickel et al. (2020). This statement is confusing as Bickel et al. provide an ERF/RF factor. Please, consider the efficacy related part of the major concerns above.

p. 16, l. 396: In some contrast to the results section, formulations in the conclusions section are generally sound and precise.

p. 16, l. 401: Some more references to previous work would be fine here. Immediately to my mind come Deuber et al. (2013) and Irvine et al. (2014).

References:

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