

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-62

Anonymous Referee #1

Referee comment on "Air traffic and contrail changes over Europe during COVID-19: a model study" by Ulrich Schumann et al., Atmos. Chem. Phys. Discuss.,
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Summary

The authors use a contrail model to quantify the change in contrail cover over Europe between 2019 and 2020, and the sensitivity of simulated contrail coverage to different modeling assumptions. They find that contrail coverage over Europe fell substantially over Europe during 2020, but that the overall change in net RF was smaller than the change in total flight distance. They gain some insight into the root cause of this through a counterfactual analysis, using mismatched weather and traffic data to quantify the role that meteorological variability had in the difference. They also find that implementation of new contrail interaction terms could reduce the simulated net radiative forcing due to contrails by up to 65% - an intriguing result.

The central question of the manuscript is the degree to which contrail coverage over Europe declined between 2019 and 2020, and whether this was proportional to the reduction in flight distance. This question is interesting, but without comparisons to observations its accuracy cannot be evaluated. The authors tease such a comparison but defer it entirely to a second paper. The authors are also unable to address the question of why this mismatch was present, beyond showing that weather variability alone is not sufficient to explain it. As such, while the data do support the conclusions, and the methods are appropriate for the relatively narrow scope of the question, the paper constitutes only an incremental advance. In addition, roughly half of the results section is taken up with discussion of methodological advances which seem to have little to do with the central question of the paper.

Overall, it is useful to have the first data point for the likely change in contrail coverage resulting from air traffic reductions in 2020, and the technical advances made in the CoCiP model are significant. However, the paper would be substantially improved by the inclusion of a comparison to observations, and by either moving the discussion of methodological advances into a separate paper or refocusing their analysis on the central question of the paper. The former in particular would raise the scientific significance of the paper and make the title more appropriate.

Major comments

The overall goal of the paper seems confused. From the abstract alone, two separate

objectives are clear: to quantify the change in contrail cover during 2020 compared to 2019 (lines 15-26), and to determine the sensitivity of the CoCiP model to certain model parameters (lines 26-32). Similarly, I count roughly four pages of discussion of the differences in simulated contrails between 2019 and 2020, and around the same number of pages of discussion of the effect of model parameters on simulated coverage. The problem is that these two disparate components do not add up to a complete study, in part because there seems to be little meaningful overlap between the two. Given that the title of the paper is specifically focused on the effect of COVID-19 on air traffic and contrail coverage, my recommendation would be to focus the discussion in section 5 on the question of whether these model advances affect the conclusions of the paper, rather than the current more abstract discussion of the effect they have on individual years. Specifically, to what degree does each of these model advances change the effect that changes in air traffic had on contrail coverage in 2020 compared to 2019? As it stands, the discussion in section 5 almost exclusively discusses what each of these advances does to the estimated contrail coverage for one year at a time without covering the implications for our understanding of the changes between 2019 and 2020.

Such an analysis would also illuminate the most important observation in the paper, which is (lines 319-321) that the reduction in the net RF was smaller than the reduction in traffic. Currently, the only explanation offered is that “[t]his is due, in part, to different changes of SW and LW RF and to the nonlinear effects from contrail-background humidity exchanges and contrail-contrail overlap”. The paper would benefit from a rearrangement of the analysis to focus on why the traffic, coverage, and RF changes were not all proportional. While this is partially addressed in the paper, the message of which factors contribute what (other than the separation of meteorological factors) is not communicated clearly.

My final major comment is that the paper seems like it would be best served by separation into two parts. The authors mention several times that they are in the process of developing a follow-on paper which will compare the model results to satellite data. It would appear much cleaner if the current manuscript were focused specifically on the model advances, rather than on the effect of COVID on contrail coverage. This would allow the aforementioned second paper to cleanly introduce both the model-based estimate of changes in contrail coverage due to COVID in a context where the results could be validated against observations. Naturally this is at the discretion of the authors, but such a division would resolve many of the concerns I have above.

Minor comments

Line 96: “Piston engine power aircraft only make a very small contribution” – a citation or quantification is needed for this.

Line 190: “The decrease of fuel consumption and flight distances are similar because the relative increase in aircraft weight (more cargo aircraft) is largely balanced by the lower load factor”. Can you provide some quantification or reference? This is an interesting observation and potentially relevant to the discussion.

Line 420: The statement “The changes appear to be larger than expected” seems incorrect. Given the larger optical depth and narrower regional scope of this paper, the two studies seem to be in broad agreement – noting Sanz-Morère’s discussion of the increase in overlap RF effects with optical depth.

There are minor typos throughout the manuscript (e.g. line 125, “was in the order” should be “was on the order”, line 153 is missing an “s” after “BADA3”).