

Reply to Reviewer

Blue: reviewer's comments

Black: response

We thank the reviewer for his careful review.

The authors have made some important additions and modifications to the paper in response to the review, for which I thank them. Given that this is a second review, I have focused my comments on those areas which were changed (or not changed) in response to my prior assessment.

Firstly, I thank the authors for their detailed description of the changes in aircraft traffic composition during the 2020 pandemic (Tables 1 and 2 in the updated manuscript). This is valuable information and helps to support the later analysis. I also appreciate the changes made to the conclusions which discuss why the net RF was reduced by less (relatively) than either the shortwave or longwave RF, and the difficulty of drawing broader conclusions. The reference to a follow-up paper which discusses a comparison with satellite observations is also a valuable addition and helps to settle some of my prior issues.

Thank you.

I remain concerned that the paper is half focused on methodological advances in contrail modeling and half on the modeled changes in contrail cover (and RF) during 2020 compared to prior years, with no clear conceptual link. I accept that the authors have chosen not to split the paper. I also understand and agree with their assessment that the change in net RF due to an event (such as COVID) or intervention is somewhat unpredictable, due to the sensitive balance of longwave and shortwave RF. As such, I would recommend that the authors include a quantitative, comparative assessment of the specific contributions of each of their model advances to the nonlinearity in the response of the RF components with the COVID-induced change in flight distance (e.g. "after accounting for water exchange, the relative change in longwave RF between 2019 and 2020 is reduced by 10%. Contrail overlap modifies the change in longwave RF by..."). While multiple factors necessarily contribute to such changes, quantitative assessment of their contributions must at some level be possible. Such analysis would increase the scientific significance, the utility to the modeling community, and the coherence of the manuscript, by relating the model advances to the central result. This is currently only implied by data provided in single-month analyses such as in Table 8, which looks at the two years separately but do not quantify or discuss how the COVID-related reduction in RF is affected.

We thank the reviewer for his considerations. The reviewer's conclusion "that the paper is half focused on methodological advances in contrail modeling and half on the modeled changes in contrail cover (and RF) during 2020 compared to prior years", could be stated more positive: The paper addresses both, methodological advances in contrail modeling, and

modelling of changes due to the first 6 months of COVID-19. The conceptual link is stated in the Introduction, lines 64 ff and 78 ff: 1) to quantify air traffic activity, the related contrail cirrus and the radiative forcing for Europe in the months March to August 2019 and 2020; and 2) to describe the new traffic data set, its setup for 2019 and 2020, to quantify the changes in traffic, fuel consumption, soot emissions, contrail cover, RF and the related TOA irradiances, and test the sensitivity of the results to model parameters. - The reviewer asks for a more "quantitative, comparative assessment of the specific contributions of each of their model advances to the nonlinearity in the response of the RF components with the COVID-induced change in flight distance". On the other hand, the conclusions (lines 478 to 492 in the new revised version) explicitly quantify and discuss effects of changes in humidity, cloud overlap and soot. - Finally, we agree that more could be done, but this could be as well subject of future studies, possibly including longer periods, for which we are preparing right now. Therefore, we do not change the paper in this respect.

Minor comment: it would be helpful to include in the captions of Tables 5-8 that these refer only to results for July, as otherwise there is some confusion over the differing results when comparing (e.g.) Tables 3 and 8.

Response: We agree, and the time periods are now identified in all table captions.

Minor comment: there appears to be a formatting error Immediately after heading 5.2, with three lines given in boldface. See lines 432-434 in the document with tracked changes, or 422-424 of the clean manuscript.

Response: We agree, there was a formatting error. This got corrected.