Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-348

Anonymous Referee #2

Referee comment on "Transport patterns of global aviation NO_x and their short-term O_3 radiative forcing – a machine learning approach" by Jin Maruhashi et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-348-RC2, 2022

Overview

The study in this manuscript investigates the impact of aircraft nitrogen oxides (NOx) emissions on short-term ozone (O3) changes and related radiative forcings. The source-receptor analysis is investigated using clustering of trajectories using the QuickBundles utility which has originally been developed for neuroscience. The paper is organised in a logical fashion, and generally well written. The description of the methodology is fairly clear and seems appropriate; it brings in a few aspects a novel approach to the community for whom this manuscript is likely to be of interest. The study produces a wide range of results which have significant complexity. The authors have attempted in their analysis to present the most important results with lucidity. I have various general and specific comments which I feel would help further improve the manuscript, and once these have all been addressed, I recommend this manuscript to be accepted for publication.

General Suggestions

In the introduction, where the chemical processes are explained, the writing style can feel at times a bit like a textbook. It would be good if the authors could show their awareness of the existing body of literature by citing a handful of papers which discuss the individual aviation-NOx related chemical processes.

When the methodology is explained it would help if the authors could add more information about the rationale behind the choices they have made for the design of their study. My specific comments below highlight the relevant parts in the manuscript. While many papers will build on work from previous publications (all are duly referenced in this manuscript, however many of them are often linked to the same affiliation) the methods need to be sufficiently explained in a stand-alone way, so that the reader (or the reviewer) do not need to consult a chain of previously published papers in order to be able to follow.

Some of the methods used in this study appear quite novel and the application of Machine Learning (ML) techniques is becoming increasingly prevalent. Given the novelty it would be good if the authors could again help the reader understand why the use of ML

techniques brings advantages to this particular study.

The impact of the applied NOx perturbations, released at aircraft cruise altitudes, will be highly dependent on the modelled background atmosphere, especially the NOx mixing ratios. The abilities of global models to simulate the NOx background accurately still remains a challenge. Further, the fact that the meteorology of only one year has been used for this study can provide a limitation. This latter point is not a criticism (given the computational expense of the experiments) however it would be good if the authors could acknowledge these limitations in the discussion and in the conclusions.

Specific comments

Lines 40—54:
This paragraph would benefit from more citations that will provide more details on the individual mechanisms through which aviation emissions affect the climate system

Line 48:
Does aviation NOx release into the stratosphere result everywhere (in the stratosphere) in catalytic O3 loss? My understanding is that this is particularly relevant at altitudes above the lower stratosphere (and above typical subsonic aircraft cruise altitudes). For emissions into the LS I would expect O3 production to occur. Again, a citation here would be helpful that explores what happens to aviation NOx released into the stratosphere.

Line 56:
Both ERF and RF are used in this paragraph but not well explained. The authors should either explain the difference between these concepts or cite a publication where the reader can learn about it.

Line 64:
"Here…" I believe the authors are referring to Stevenson & Derwent (2009) so this should be "Therein…"?

Line 80:
Just to clarify: this sentence seems to imply that O3 reaches only at the end of its lifetime (after 3 months) its location of largest impact. Are the authors suggesting that there is less radiative impact prior to this time?

Line 88:
The way the process is described here seems to assume that the content of the air parcels with their chemical evolution remain completely isolated from the environment, in reality

however some kind of mixing would occur.

Line 89:
This confused me a bit. Surely, it is not the Lagrangian methodology per se that results in terabytes of data but the selected number of trajectories/parcels and, in connection with that, the number of simulated processes?

Line 90:
Could the authors add some text explaining why they think ML techniques like clustering offer an advantage to process the data? I am not disputing this, but it will not seem obvious to all readers. Why will ML techniques be helpful for this?

Line 138:
How were the chemical fields for these 10 simulations initialised? Were the initial conditions for these simulations specific to 2014?

Line 144 & 145:
It would be good if the authors could explain some of their choices for timing and magnitude of the NOx perturbations. Why 0600 UTC? Why 5x10^5 kg NO, how does this relate to average aircraft NOx emissions released over, say, the United States and what is a typical NOx background? Is this a doubling of emissions or does it represent a massive spike? That probably depends on the location. It would be good to have some context or explanation where this number comes from.

Figure 1:
Obviously the authors chose a more conceptual approach by shaping their regions in a way that roughly covers a continent (without overlap) without exhibiting any preferential treatment. This will not reflect actual distributions of aircraft emissions such as the North Atlantic Flight Corridor, major aviation hubs in East Asian cities or any flights across the Pacific. Shaping the regions to match more realistically the existing flight routing would have been an alternative to the chosen approach. Given the focus of this manuscript on assessing aviation climate impacts, as opposed to focusing conceptually on atmospheric processes (independent of the aviation application), is there a specific reason why the authors chose this particular distribution?

Section 2.3
The methodology needs a bit more explanation. Radiative forcing from O3 is most effective at the tropopause region (see papers by Shine et al), therefore it is not obvious how linking emission region with region of largest air quality impact (at the ground) relates back to maximum RF impact

Figure 2:

The dark green lines and labels over grey background are difficult to read. Can you make them more visible (e.g. in red)?

Line 203:
"To identify characteristic patterns" is too brief an explanation and should be expanded into more detail. For readers who are not familiar with clustering it would be good if a bit more text (1-2 sentences) could be added that explains the benefit.

Section 2.4.1
This section is very technical and not within my area of expertise. I could not follow all parts of this description and, given that the target audience for ACP are atmospheric scientists I would recommend a to include a less technical summary of the methodology without formalism. The same applies to section 2.4.2 about the K- or L-method which are not explained and will likely not be general knowledge to the average atmospheric scientist.

Section 2.5
Can the authors give a little more context why we are interested in the rate of descent?

Section 3 – Intro
Can the authors please add an explanation for the connection between air parcel descent and O3 production. Further, why is the latitudinal residence time relevant?

Lines 327:
It would be good to have a sentence here providing a transition from the preceding description of Table 1 to the quickbundles results. Otherwise, this reads a bit disjointed, I felt a bit confused when I read this at first.

Line 342:
"area of lower chemical activity" This is a misleading statement. It needs to be clear that this refers to the discussed O3 processes of interest in this study. There is lots of other chemical activity here.

Figure 9:
It is easy to discern the difference in the red. The green coloured trajectories however cover a large area of the plot, and the distinction between the two panels is not so easy to see in the green. Could perhaps the age of the trajectory be expressed through different colour shading, or perhaps some other information be conveyed such that the change in structure becomes more visible?

Figures 12 and 13:
There is a lot of information in these figures. What springs to mind is that all this information is based on a single year of study. Will the authors consider the possible impact of inter-annual variability, e.g. in circulation patterns. Obviously, without carrying out a multi-year study this would be difficult to quantify. However it would be worth stressing to the reader that the findings are based on the meteorology from a specific year.

Line 524:
Here is a statement that puts the NOx perturbation in a practical context. This should have been mentioned much earlier in the discussion of the methodology. "121 700 A320 aircraft", does this mean 121,700 (one hundred and twenty one thousand and seven hundred)? The context is certainly very specific (even mentioning to a specific engine model) but what does this mean in terms of percentage increase of contemporary air traffic? Please do not misunderstand me, I welcome the fact that the authors try to put it into context, however adding a more easily accessible context (to the non-expert) would be appreciated, something along the lines of "this corresponds to a 200% increase in emissions from transatlantic flights from mid-sized airliners" or something similar.

Line 534:
Careful with general statements: this is based on NOx-O3 processes only, ignoring other aviation related non-CO2 processes. If this is not mentioned this statement could be misunderstood.

Line 555:
Could the authors also add a bit of interpretation for the link between N. America as a source region and Europe as impact region. Subsequently an interpretation is offered for the southern hemisphere, however it would be also good to have some text about this largest impact.

Section 4
The first part of this section focuses mostly on the experience gained from the methodology which is a good idea. The second part provides a fairly high-level discussion of the scientific findings. Given the complexity of the study design and the employed methodology, this work has yielded in a large number of results. While Section 3 has largely been confined to presenting the quantitative findings, not much space was dedicated to an interpretation. I would have hoped to find more of this in Section 4. If the authors do not wish to expand Section 4 then I would recommend that within Section 3 a few more sentences of interpretation are added to specific findings. In general, I am satisfied that the authors provide a discussion of the caveats and potential limitations of their work.
An important point is that the diagnosed impacts of a NOx perturbation will be strongly dependent on the simulated NOx background in the chemistry-climate model. While a general validation of the model's atmospheric background is beyond the scope of this work this needs to be mentioned. It would be ideal if the authors could point to a model validation paper which gives a fair indication of the performance of their model in this regard.

Line 620:
To what extent do the authors expect the linearisation to hold true for their perturbation? Are we well within range of a linear response for small perturbations? Has this been explored, if not by the authors then by other studies perhaps?

Line 627:
"Earlier work has shown…" It is not clear what the authors mean without reading e.g. the Grewe et al 2019 paper. Can the authors add a bit more explanation?

Line 645:
If more computational resources were available, would the authors prioritise vertical level increases over horizontal grid resolution?

Line 655:
It should be pointed out that the study was based on a single year of meteorology.