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Comment on acp-2022-584

Anonymous Referee #1

Referee comment on "Long-term upper-troposphere climatology of potential contrail occurrence over the Paris area derived from radiosonde observations" by Kevin Wolf et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-584-RC1, 2022

This paper uses radiosonde observations and flight Automatic Dependent Surveillance-Broadcast (ADS-B) telemetry to estimate the potential contrail occurrence by altitude and seasonality. The paper is very well written, the methodology is appropriate, and the results are reasonable. It is relevant to the scope of Atmospheric Chemistry and Physics, and I recommend it for publication with minor revisions.

Major Comments

- [Sections 4.1 and 4.4] Are these results (i.e., ISSR properties, potential contrail formation, and their respective seasonal variabilities) consistent with existing publications that used radiosonde measurements and in-situ water-vapour measurements from aircraft[1–3]? It would be great if the authors can compare and quantify the differences in their results relative to existing studies and include a short discussion on the potential reasons causing these discrepancies.
- [Section 4.6] The authors estimate the change in the non-persistent and persistent contrail formation from different fuel types. However, as pointed out, these fuels have differences in the water vapour emissions index and nvPM number emissions index, both of which are expected to change the various contrail properties such as the lifetime and radiative forcing[4–6]. While I understand that the changes in contrail properties are beyond the scope of this study, it would still be appropriate to add a short discussion on the potential changes in contrail properties due to the use of different fuel types.
- [Section 4.7] The results in this section were very well described. However, the authors can improve it further by highlighting the potential factors that contribute to the seasonal difference in flight altitude changes in minimising persistent contrail formation. For example, it would be great if these results can be related to the seasonal cycle of the thermal tropopause height, as illustrated in Figure 5.

Minor Comments

- [Line 43] It would be great to add a short sentence describing the second-order effects of contrails in affecting the natural cirrus properties[7,8].
- [Line 53] The terms "terrestrial RF" and "solar RF" that were used in this paper are not commonly used in the literature. The authors can consider renaming them to "shortwave (SW)" and "longwave (LW)" RF here and in other parts of the paper to conform to the terminology that is used in the literature.
- [Line 272] There is a typo where "R1-NOC" should be "R1-NPC".
- [Lines 430 - 432] This statement conflicts with Figure 5c. The stratosphere is generally dry and identified where the background H2O is below 10 ppm. Ice supersaturation in the stratosphere is a rare event and Figure 5c shows that persistent contrails generally form below the troposphere.
- [Lines 477 – 485] It would be a great for the authors to highlight that the contrail climate forcing from NPC are generally negligible, and that PC's are the significant contributor to contrail climate forcing.

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