Response to RC2

Dear Reviewer,

Thank you on behalf of all Coauthors for the valuable feedback and comments!

Below you will find your original comments in green and our corresponding reply in black. A revised version of the manuscript will be submitted to Copernicus at the same time as this reply is uploaded.

We appreciate the time you have taken to read and understand the manuscript!

Kind regards on behalf of all the co-authors.

General comments:

This paper considers how exhaust conditions which may be typical of future fuel cell powered aircraft affect the formation of contrails from such aircraft. Direct atmospheric measurements were undertaken through flight campaigns where a chase aircraft sampled the exhaust plume of an aircraft which was being studied. It is a scientifically robust and valuable manuscript.

There are two key novel aspects in this work. Firstly, the development and evaluation of an artificial dilution tracer system (ATReUS), which will be key to measuring the emission index (EI) from flights which do not produce CO_2. The authors use N_2O and conscientiously assess the potential climate implications of this tracer.

Secondly, the authors studied how the apparent emission index of ice (AEI) and the size of the ice crystals depends on the ambient conditions for temperatures close to the homogeneous freezing temperature (T_{hf}) in the case of high humidity and low temperature exhaust provided by their fuel cell exhaust emulator, concluding that small changes in ambient conditions near T_{hf} can lead to vastly different microphysical properties of the contrail, although experimental results around this point are limited.

My main concerns revolve around the instrumentation and particle characterisation, as well as assumptions made while assessing the microphysical properties of the contrail.

We appreciate the encouraging words and will address the concerns below.

Specific comments:

Section 2 (the development of ATReUS):

In this section, the authors do not describe the sampling system fully. As a result, the reader is left without knowledge of any of the potential losses or interactions within the system. Some more details would be worthwhile.

The initial wording to describe the sampling system setup: “For accurate dilution measurements, the sampling inlet needs to be co-located with the inlets for the relevant emission measurements, with the length of tubing between the inlet and the sensor kept to a minimum. Close proximity of the sampling inlets allows for a more precise dilution correction, by reducing radial dilution effects and uncertainty related to the aerodynamic influence of the chaser aircraft’s structure. The positioning of the sampling inlets for the Nephele flight test campaign is shown in Fig. 4.” We recognise that this wording does not adequately describe the

layout of the inlet and pluming in particular, and that potential losses are not discussed. Changed wording: “For the Nephele flight test campaign the inlet of the ATReUS sampling system was realized with 2.5m of PFA ¼’ tubing. The inlet itself was installed next to the inlets for the relevant emission measurements. All trace gas and aerosol inlets are positioned at a distance of 40cm to the CAPS ice particle probe mounted on the belly of the chaser aircraft (as shown in Fig. 4). Close proximity of the sampling inlets allows for a more precise dilution correction, by reducing radial dilution effects and uncertainty related to the aerodynamic influence of the chaser aircraft’s structure. With this setup, no losses are expected for the N2O measurements. The response time is estimated as < 0.5s”

The EI calculation is clearly laid out. As this manuscript aims to correct to H_2 burnt, it is important to lay out how the effective work performed by a unit mass of kerosene compares to that performed by a unit mass of H_2. This would to allow more direct comparison between fuel cells and current engines. In fact, the authors allude to this (l215-217), but I don’t see that such a comparison was actually made.

All AEI values in the manuscript are normalized to the hydrogen mass used. But, as it is a crucial step to correct for the fuel’s energy content for any comparisons to kerosene powered aircraft, we provide additional information and include the correction factor in the text: “This is done by dividing the EI value by the energy content per fuel mass, thereby relating the emissions metric directly to the energy released during combustion. A hydrogen-based EI value can be compared to kerosene-based EIs by applying a factor of 0.36 (ratio of the specific combustion heat of both fuels according to Schumann, 1996).”

Schumann, 1996: On conditions for contrail formation from aircraft exhausts, https://elib.dlr.de/32128/1/mz-96.pdf

The environmental assessment in section 2.5 is very valuable, with the conclusion made that a CO_{2e} of 3140 kg resulted from the use of N_2O. I would like to see that put into context with an estimated amount of CO_2 from the fuel burnt during the campaign.

We agree, that putting the impact into perspective is valuable for the reader. Both aircraft involved in the campaign are highly modified and their fuel use difficult to compare outside of the context of this campaign. Therefore, we decided to compare the climate impact of the tracer system to that involved in bringing the scientific personnel from Germany to the US. The following was added: “This corresponds to an equivalent CO2 emission of 3140kg, or the CO2 emission of burning one metric ton of kerosene. For context, this is less CO2, than was emitted by three scientific personnel traveling from Germany to the US and back to conduct the flight test campaign (assuming a direct connection via a wide body aircraft 80% full).”

The authors note that N_2O, the tracer gas they have used, can be ‘produced in small quantities during combustion processes (Santoni et al 2011)’. This should come with a value so that the reader doesn’t need to delve into the paper to understand this fully. A similar criticism can be made of many comparisons throughout the manuscript which are begun but not brought to their conclusion.

We apologize for the oversight! The following value for the emission index of N2O was added: 32 mg/kg_fuel . Further comparisons without conclusion are addressed under “Technical corrections” where this issue was also brought up.

Section 3:

Instrumentation:

The description of the instrumentation in Table 1 is incomplete. For some instruments, the range and the uncertainty are quoted (e.g. CAPS PT100) whereas others do not have a range or an uncertainty at all (aMCPC). This should be completed.

We agree, that the table should be more uniform. We intended the table to only include the instrument range and to discuss further instrument characteristics in the text. Section 3.1 and Table 1 was adjusted accordingly.

In the text (l103-105), the LI-7820 is described as measuring both N2O and H2O. However, in Table 1, only its N_2O measurement ability is described. Likewise for the LI-7000 (l 314).

Thank you for catching that! For both instruments Table 1 is expanded to also include their capability for measuring H2O.

Furthermore, the SEMS is quoted as measuring the particle size distribution (PSD) between 5-350 nm, but as this must be coupled with an aMCPC to produce the PSD, the SEMS cannot measure particles below 7 nm.

The specified 7 nm corresponds to the diameter at which the aMCPC achieves 50% detection efficiency (D50), rather than a strict lower detection limit. Particles as small as 5 nm can still be detected, although with reduced efficiency. The size-dependent detection efficiency of the aMCPC was calibrated in the laboratory against a reference Faraday cup electrometer (FCE). Since the SEMS provides size-resolved measurements, the particle concentrations in the individual size bins can be corrected for the corresponding detection efficiencies. Therefore, the stated measurement range of 5–350 nm is appropriate.

The table also suggests that the aMCPC alone can segregate the nvPM counts from the vPM counts. I suggest that the thermodenuder should be included in the table for greater clarity rather than as a footnote.

The table entry was changed to: aMCPC + thermodenuder[1], the footnote was changed to [1]setpoint 230°C

There should also be additional details about the thermodenuder; if this does not include a catalytic element, it may not be removing the volatile particles in the way that is expected. In some cases, a thermodenuder could potentially even create new particles <10 nm from the evaporated volatile materials (Amanatidis et al, 2018). This needs to be addressed or discussed.

This was also remarked by the second reviewer and is therefore also addressed in the corresponding author response. Including a catalytic stripper was not an option in this case due to the high particle losses, instead the thermodenuder was used. The setup was tested in the lab in Germany and the system losses and behaviour was characterized. Behaviour as described in Amanatidis et al, 2018 was not observed. The following wording was added: The thermodenuder was operated at an evaporation temperature of 230 °C. Its evaporation performance was characterized in Neumann et al. using tetracontane, for which 100% evaporation was observed for particles up to 80 nm.

The aMCPC has a cutoff diameter of 7 nm. Yet, with the close chase distances, volatile particles may not have grown large enough to be observed at 7 nm, so there should be some discussion of the growth time of the volatiles, particularly as it seems that the authors suspect that many of the particles present could be oil. That being said, this attribution to oil assessment does not seem satisfactory as the nvPM concentrations are high (fig. 7).

We indeed expect the majority of the particles inside the FCEE to be formed from oil or oil vapors. We checked the FCEE tubes after the flights and found traces of oil. Contamination of the FCEE's hot air supply with oil coming from the C131's engine is very likely. In this context the growth time of the volatiles is less relevant, as they can already form quite early inside of the FCEE, due to the low temperatures upstream of the exhaust. We expect the bulk of emitted volatiles were already in a detectable size range with our setup as soon as they were emitted.

Although it is less likely, we cannot completely rule out the possibility that nvPM was also present in the hot air supply duct. Given the layout of the FCEE, contamination with high concentrations of nvPM is less likely. And it has to be kept in mind, that at the point of measurement the exhaust air emitted by the gasoline engines is mixed into the contrail. Subsequently the nvPM we see in the measurement might originate from the C131's engines.

Note: Due to the high supersaturations behind the exhaust of the FCEE all particles emitted by it are activated (both nvPM and vPM)! The total PM is therefore the relevant quantity for the contrail analysis (also what we measured after the contrail flights directly inside the FCEE).

Additionally, it seems as though the particle concentrations in the FCEE were not measured during the relevant flights, but only later (l480). The assumption that the concentration was the same during this later flight and the earlier flights is not necessarily correct. Overall, a discussion of the particle concentrations and their nature should be frontloaded and condensed into one area rather than requiring the reader to piece it together.

We agree that a continuous measurement of the particle concentration would have been valuable throughout the campaign. This is definitely considered for upcoming campaigns. However, for this campaign the CPC installation measuring the particle concentration directly inside the hot air supply of the FCEE was only possible after the chase flights. As this measurement is the only direct indication of the concentrations inside the FCEE we use it as a lower limit of particles in the FCEE. During this test, the FCEE was operated at the same setpoints as during the other flights. Therefore, we think the assumption that the particle emissions were similar is valid. Also, the measured value only acts as a rough estimate that particles of high concentrations were present. We note here, that we do not use the measured concentration for the data evaluation but rather for the interpretation of the measurements.

We updated the third paragraph in section 3.2 to discuss the particle contamination further and to condense the issue for the reader before the results chapter:

"It should be noted here that the system was only deployed during a two-week campaign for a total of 5 test flights carried out in relevant atmospheric conditions. A contamination in the hot air supplying the emulator was identified, either coming from unfiltered air or oil contamination in the system. Total particulate matter (tPM) number concentrations greater than $3 \cdot 10^6$ cm−3 were measured inside the FCEE. During inspection of the FCEE's ducts, traces of oil were found on the inner surfaces, which are therefore the most likely source of the particles. The oil contamination most likely originates from hot, oil-coated surfaces in the C131's engines, close to the hot-air intake for the FCEE. Model simulations by Zink et al. (2025) show that small amounts of oil (in the order of ml h−1) can cause high number concentrations of oil particles that act as precursors for contrail ice crystals (Ponsonby et al., 2024). In our experiment, we determined the number of aerosol particles in the hot supply air but not the type or size. While the original design targets for the FCEE regarding exhaust humidity and temperature were met, the goal of producing a particle-free exhaust was not achieved. Consequently, it is not comparable with an actual fuel cell system. Nevertheless, we use the data to explore the high

humidity - low temperature conditions with classical formation pathways involving exhaust type particles to explore the droplet freezing near the homogenous freezing threshold."

Zink, J., Unterstrasser, S., & Jurkat-Witschas, T. (2025). On the potential role of lubrication oil particles in contrail formation for kerosene and hydrogen combustion. *Journal of Geophysical Research: Atmospheres*, 130, e2025JD043487. https://doi.org/10.1029/2025JD043487

Ponsonby, J., King, L., Murray, B. J., and Stettler, M. E. J.: Jet aircraft lubrication oil droplets as contrail ice-forming particles, Atmos. Chem. Phys., 24, 2045–2058, https://doi.org/10.5194/acp-24-2045-2024, 2024.

The particle measurement payload may not be well adapted to future measurements for hydrogen engines where it is likely that small volatile particles play the largest role in contrail formation due to this 7 nm cutoff.

We are aware of the limitations of the described setup and tried to combine the measurements with size-resolved measurements from the SEMS (Neumann et al. 2025). At present there are no information on the particle sizes from hydrogen engines. For aircraft measurements, compromises have to be made, especially due to the limited space available on the aircraft.

Neumann, G., Marsing, A., Harlass, T., Sauer, D., Braun, S., Pühl, M., Heckl, C., Stock, P., De La Torre Castro, E., Hahn, V., Roiger, A., Voigt, C., Unterstraßer, S., Cammas, J., Renard, C., Vasenden, R., Vasenden, A., and Jurkat-Witschas, T.: In-flight emission measurements with an autonomous payload behind a turboprop aircraft, Atmos. Meas. Tech., 18, 6795–6816, https://doi.org/10.5194/amt-18-6795-2025, 2025.

Fig 6, right: both ends of the temperature scale are impossible to resolve. Why is it not resolved? If this is impossible, would it not be more beneficial to have this as >241 K, <230 K? In the text you say you account for uncertainties by relaxing the RHi criterion to 90%. Does this mean that the uncertainty is typically biased in a certain direction? This merits a clarifying sentence or two.

How long must a contrail survive for you to qualify it as persistent? I think this should be more clearly defined as this can mean different things depending on the precise research field.

The idea was to only resolve the temperature range relevant for the flight planning. We adapted the suggestion to change it to >241K and <230K.

Defining the criteria for persistence depends on the context of the research. In general, ambient RHi has to be >=100% (as defined, for example, by Megill and Grewe 2025). However, the time were this condition is met for a contrail to be classified as persistent depends on the research field. For our measurements we define a contrail to be persistent, when up to the point of the measurement, the ambient air was above 100% RHi. This rules out the sublimation of contrail ice crystals in the time from formation to measurement. For the prediction used for the flight planning, we relaxed the condition to 90%, otherwise the potential regions were very small. Also, for near field measurements with large particles as performed here, 90% ambient RHi would only probably lead to a decrease in the contrail ice particle size but not deplete the ice particle number significantly. The uncertainty of the predicted RHi is not biased, but the lower threshold is the relevant value here.

Instead of: "For our purposes, we defined ISSRs with RHi > 90% to account for uncertainties in the forecast.", we write: "Taking into account the uncertainty of the forecast the criterion of ISSRs was relaxed to RHi > 90%. This resulted in lager contiguous ISSRs and simplified flight

planning, while the probability of actually encountering ice supersaturated air masses without significant particle loss through sublimation remained within acceptable limits."

Megill, L. and Grewe, V.: Investigating the limiting aircraft-design-dependent and environmental factors of persistent contrail formation, Atmos. Chem. Phys., 25, 4131–4149, https://doi.org/10.5194/acp-25-4131-2025, 2025.

Fuel cell exhaust emulator:

The FCEE is innovative and a good tool to study the effect of future propulsion technologies on contrail formation, provided issues can be addressed. Most important is the high concentration of particles in the exhaust which is unlikely to be present in a true fuel cell experiment. The fact that these particles were not fully characterised or identified serves to exacerbate the problem. As it stands, this is inadequately discussed.

As stated before, we updated the third paragraph in section 3.2 to discuss the origin of the particle contamination further and to condense the issue for the reader. However, as this is not a study on aerosol-droplet formation which leads to contrail formation we do not see a drawback to the data interpretation. As all particles in the FCEE plume will activate due to the high exhaust humidity, the question is less about the nature (size and composition) of aerosols. We therefore focus on the droplet freezing. This is sensitively dependent on ambient temperature as our measurement show.

Some aspects of the FCEE could be expanded further, particularly how it is ensured that 100% of the water droplets evaporate. If any droplets remain at the exhaust plane, these could strongly influence contrail formation.

Simulations and ground tests were carried out to assess the droplet formation behaviour within the FCEE's evaporation chamber, and the droplet spray and water flow were adapted accordingly. The design also allowed for the controlled, direct emission of droplets in the exhaust in order to investigate their effect on contrail formation. During our experiments, we observed that the ice crystals formed from these droplets are much larger (diameter of 30-80 µm) than those formed from the water vapor. We use the number of these very large ice crystals to determine whether droplets were emitted directly. Their number were two orders of magnitude lower than the small ice crystals formed form the vapor phase. The model simulations ran with this prescribed number of droplets did not show a significant influence on the contrail ice crystals from the vapor phase. Nevertheless, for the analysis described in sections 4.3 to 4.5, only those flight sequences were analysed where we knew that the FCEE was not supposed to have emitted any droplets and where we additionally had not measured a high number of large droplets.
For future experiments using a new setup without particle emissions, this effect will, however, be a key research question!

The statement on deployment is confusing. L359 refers to the system only being deployed once, however the manuscript later refers to 3 test flights where the FCEE appears to be deployed.

"We note here that the system was deployed only one time during a two-week campaign on the aircraft in relevant atmospheric conditions." changed to: "It should be noted here that the system was only deployed during a two-week campaign for a total of 5 test flights carried out in relevant atmospheric conditions."

Values of the humidity in the fuel cell exhaust are only given much later in the manuscript. It would be clearer to have them here as well.

The target RH is given in Figure 5, but not in the text. We added this wording to clarify the matter:

“For this purpose, liquid water is evaporated in a hot air stream to generate high humidities at the exhaust. The exhaust humidity can be adjusted, by changing the amount of injected water, with a targeted exhaust gas composition of 100% RHw at 34°C.”

Note: The RH given later in the manuscript are always measured values.

Similarly, to improve clarity, the authors could consider the units and standards they use, for example at lines 146-147, gas flows are quoted in both kg/s and slpm. Shortly after, the reader must compare a nitrogen equivalent mass flow to an N_2O mass flow, which leads to some of the values initially seeming inconsistent. The description of the variables in Eq. 6 and 7 (l 239 – 245) is unclear as the order in the text does not appear to be the same as the order in which the variables appear in the equation.

The order is actually very confusing! We have adjusted it so that it matches Eq. 6 and 7 as closely as possible. The mass flow through the FCEE is measured in kg/s. The N2O flow is measured in slpm. We added the N2O flow values also in kg/s.

Section 4:

The values quoted l 409 cannot be found in fig. 7. I believe this is because they were measured in different flight, but if so, this should be specified to prevent confusion.

We added the remark, that the mentioned values were measured earlier during the flight shown in Fig 7 to prevent confusion:

“…up to 700ppbv for N2O (at 20:11UTC during FT5) and 160 ppmv (at 20:10 UTC during FT5) for CO2 can be identified”

L412, 90% of the maximum N_2O flow rate is referred to with a different value to that previously (9.585 slpm v 9.33 slpm on l 156). Again, this removes clarity and confidence.

The value of 9.33 is the calibrated and therefor correct value, updated for all occurrences. The value of 9.585 is the initial estimate, using the manufacturers calibration. I forgot to update the value for all occurrences. Sorry for that and thank you for spotting this mistake!

The dilution and mixing of the plumes are very valuable measurements to have come from this work, as they give an idea of how exhaust plumes spread out behind an aircraft. This warrants more discussion.

We added Appendix section C into the main text, to further discuss plume evolution and cross contamination.

The final sections in fig. 7 show high nvPM, but low ice. I think this is because the gasoline engine was sampled, with lower humidity and therefore less favourable conditions for contrail formation, but this should be laid out explicitly, perhaps even within the figure.

Note: During this flight only the FCEE generated a contrail, therefore Nice is exclusively from the FCEE.
It is true, the effect you describe is due to sampling of air with a higher ratio of the exhaust gases of the engines of the C131 mixed in. This is also indicated by the Delta CO2 and Delta N2O measurements (comparatively low N2O and high CO2). However, at no point during this flight was any attempt made to measure only the gasoline engine’s exhaust gases. The variation of the ratios between the trace gases and aerosol/ice was due to changes in the distance between the

aircraft and/or due to the radial distance between the center of the FCEE plume and the point of sampling. We changed the y-axis scaling of the nvPM measurement in Fig 7 to a linear scale to improve the readability of the figure. By displaying both Nice and nvPM on a linear scale, they are easier to compare.

The models are used before they are introduced and therefore the reader does not really understand them when it is relevant. For example, it is not clear what the water droplets are forming on when they are allowed to form, or if they result from homogeneous nucleation (l459). How the peak supersaturation is actually obtained (why does it start decreasing when it is shown to in fig. 9?) is also unclear. A more in depth and earlier discussion of the model, including details on the dependence of T_{hf} on particle size and solubility and any quenching of the supersaturation is needed.

We rearranged the sections and put the model description in the methods section. This should improve readability. We also extended the model description and included more references about model details. We also refer to the publication of Hillenbrand and Unterstrasser, 2026 showing the HDN parametrization and the behaviour of supersaturation during contrail formation in the model.

Hillenbrand, D. and Unterstrasser, S.: A new ice crystal formation pathway in contrails generated by fuel cell propelled aircraft, J. Env. Com. Air Transp. Sys. Discuss. [preprint], https://doi.org/10.5194/jecats-2026-16, in review, 2026.

The discussion of the peak supersaturation in these models is also unsatisfactory; are these values likely to truly be reached when particles are present, activating and thus quenching the supersaturation?

In Figure 9 we try to show the hypothetical supersaturation that can be reached without particle formation, based on the initial temperature and humidity. This is a purely thermodynamic analysis based on the isobaric mixing line in the temperature/water vapor partial pressure diagram. In our case these supersaturations of > 1000% were never reached due to the aerosol particles in the exhaust. These particles act as precursors of droplets and reduce the saturation through nucleation and condensation. The model of course, also accounts for these changes in available water vapor. We want to illustrate the potential maximum supersaturation the system would be capable of (without contamination of course) allowing for HDN to occur.

The calculation of the fraction of frozen droplets relies on an assumption that the maximum number of ice crystals corresponds to 100% frozen fraction. This assumption should be more clearly validated.

The measured maximum ice crystal number as the reference for fully glaciated particles (100% frozen fraction) is somewhat arbitrary but seemed the most relevant. The maximum values of measured AEI compare quite well to the modelled AEI with a prescribed number concentration of 10^7 cm^{-3} aerosol particles in the exhaust which also seems very reasonable. Therefore, the value is constraint from various perspectives. We will add these thoughts in the manuscript.

In addition to number concentration and size distribution, the CAPS ice crystal probe also measures the asphericity of the particles. By looking at the asphericity of the particles, we can distinguish between water droplets (highly spherical) and ice. During the sequences, where we analysed the AEI, the particles are clearly aspherical and therefor frozen. In addition, droplets would not survive in these atmospheric conditions (<100%RHw), given their small size shortly after activation.

I would also like this treatment to be performed with respect to the number of particles sampled in the exhaust rather than just water droplets to understand how the activation conditions required for aircraft emissions to activate. I think it a shortcoming of this paper that needs to be addressed for future studies that you have so little knowledge about the particulate emissions from the FCEE.

We agree, that knowledge of the type and number of emitted aerosol particles would be valuable. The measured value of $3*10^6$ cm^{-3} is a lower limit of the actual particulate contamination. If we assume that every particle forms an ice crystal, we end up with an AEI value in the order of $8*10^{14}$ ice crystals per kg burned H2. This is roughly a factor of two below the maximum AEI of 4-5*10^^{15} ice crystals per kg burned H2 we measured. Therefore, our values roughly (by a factor of two) agrees in terms of aerosol-ice emission indices within the described uncertainty. We will make a note in the manuscript.

The behaviour measured near T_{hf} does not closely follow the model, partly due to the large uncertainties on the measurements, but it seems as though the increase in AEI only occurs around $\Delta T_{hf} > 4.5$ K. Does this agreement get better with higher aerosol loadings in the model as the trend seems to go that direction? There are also very few points in the model showing this increase (only one or two before reaching a relative plateau). The model data should be extended to $\Delta T_{hf} = 0$ K to make this relationship clearer and could also include more aerosol loading values (e.g. the 3e6 cm^{-3} that was established as the minimum aerosol loading).

We attribute some of the discrepancies of model and measurement to fact that the box model in this case only considers the plume centreline, while the measurements are “scans” through the plume with changing distance between the aircraft and radial distance to the plume centre. The model represents a single trajectory case and the measurements rather show a mean value of many trajectories in the plume. We mention this in the manuscript. For future publications we aim to represent the plume with several trajectories as it was done in Bier et al. 2022.

What we mainly want to demonstrate here, is that the model originally set up for classical jet configurations can be adapted to our experiment by using the measurement data. We than use it to assess the assumptions we make to explain the data. Our aim here is not to show that the model perfectly represents the measurement data, but rather that it exhibits similar trends.

For higher aerosol number concentrations, the drop in AEI does occur at lower temperatures. For temperatures approaching $\Delta T_{hf} = 0$ K the AEI in the model drops to zero. This is again due to the fact, that the model only considers the plume centreline. All aerosol particles in the simulations see the same humidity and temperature. Therefore, aerosol of equal size activate at the same time and grow at the same rate. The droplets are reduced to one size distribution where all droplets evaporate if the freezing condition is not met. In the real plume individual particles/droplets see a wider range of conditions (plume edge vs centre). Some droplet may grow larger, potentially scavenge the released humidity from droplet evaporation and freeze (the droplet freezing is highly dependent on the droplet size, with larger droplets freezing faster → O and Wood, 2016).

This is also the reason why we see such a large variability in the data.

O, K.-T. and Wood, R.: Exploring an approximation for the homogeneous freezing temperature of water droplets, Atmos. Chem. Phys., 16, 7239–7249, https://doi.org/10.5194/acp-16-7239-2016, 2016.

l 618 refers to vertical colored lines in fig. 11d, but I do not see these.

For most of the displayed simulations this exhaust dependent effect on AEI is not relevant and the vertical coloured lines therefore smaller than the square scatter points (and therefore not visible). Only for the right-most blue coloured datapoint is this effect actually large enough that you can see it in the figure.

The final statement l 625 is phrased too strongly, although I agree with it. The model run with and without HDN, and in the absence of measurements directly at the exhaust plane we cannot strictly know what supersaturations were reached.

Initial wording: The model simulations also confirm, that no HDN took place during the flights, [...]
Rephrased to: The model simulations also support the assumption that no HDN took place during the flights, [...]

Appendix C:

This cross-contamination aspect was a key question in my mind throughout this manuscript. As such, I think this appendix should be in the main text. The final section of this appendix is worded confusingly, and the data do not seem to agree with the text. Fig. A1 does not show a difference in the distance at which the max Δr_{N2O} and Δr_{CO2} were reached. Perhaps the use of different color scales hides this as it makes it more difficult to compare the two plots. The authors also state that the lowest cross-contamination was measured up to 58 m behind the C131, but there are almost no data shown at closer distances, so this conclusion cannot be supported.

We rephrased this section and added it to the main result chapter. The figure grid was changed for a closer zoom on the distance range in question. This was done to show more of the data measured at distances 50 to 58m.

Technical corrections:

Generally, many sentences in the manuscript are very long (e.g. l337-338, l 430-432), preventing clarity and marring the good science which was done. I would recommend the authors proofread the manuscript once more and try to address this.

At many points variables are referred to, but numbers are not included. Doing so would help prevent the reader from searching for those values later in the manuscript and make the discussion flow more clearly (e.g. l363 – 365, what are the number of particles, the humidity and the temperature?).

At times, abbreviations for quantities are introduced, but these are then not always used (e.g. RHw, RHi, T_{hf}) leading to longer and less clear sentence structure.

We have proofread the text again and rephrased long sentences, added missing values and updated abbreviations for every occurrence we found.

Comparisons to literature are often not brought to their conclusion, with values or the conclusion of the comparison left out, requiring the reader to finish making the comparison themselves, and potentially coming to the wrong conclusion. This should be corrected (e.g l392-393 comparison to ERA5, l. 456 does the exponent increase or decrease with jet size, l 563)

Addressed for:
l392-393: We briefly summarise the results regarding the comparison of GFS forecast, ERA5

reanalysis and measurement data presented in Mackay et al. (2025) in the manuscript.
l115-117: We include the EI value measured by Santoni et al. (2011), for the production of N2O in aircraft engines in the manuscript.
l456 We summarize the effect of jet speed and diameter on dilution speed described by Zink and Unterstrasser (2026).
l563 We added the values of the effective contrail ice crystal diameter mentioned in Voigt et al. (2021) to allow a direct comparison.

Mackay, C., Marizy, C., and Raguet, I.: The prediction of Ice Super Saturated Regions and persistent contrail formation using weather data and in-flight observations, Proceedings of the 2024 Aeronautical Meteorology Scientific Conference (AeroMetSci-2024) p208, Publication AeM SERIES No. 10 WMO e-Library, https://library.wmo.int/idurl/4/69607, 2025.

Gregory W. Santoni, Ben H. Lee, Ezra C. Wood, Scott C. Herndon, Richard C. Miake-Lye, Steven C. Wofsy, J. Barry McManus, David D. Nelson, Mark S. Zahniser; Aircraft Emissions of Methane and Nitrous Oxide during the Alternative Aviation Fuel Experiment. *Environ. Sci. Technol.* 15 August 2011; 45 (16): 7075–7082. https://doi.org/10.1021/es200897h

Zink, J. and Unterstrasser, S.: Contrail formation for aircraft with hydrogen combustion – Part 2: Engine-related aspects, Atmos. Chem. Phys., 26, 3145–3165, https://doi.org/10.5194/acp-26-3145-2026, 2026.

Voigt, C., Kleine, J., Sauer, D. *et al.* Cleaner burning aviation fuels can reduce contrail cloudiness. *Commun Earth Environ* **2**, 114 (2021). https://doi.org/10.1038/s43247-021-00174-y

The authors could consider using AEI_{ice} for greater clarity. This would also match Eq. 2 better.

Accepted.

L 75: was lead -> was led

Accepted.

L 182: did not effect -> did not affect

Accepted.

L230 done, by -> done by

Accepted.

L 258: 2023 lead by -> 2023 led by

Accepted.

L288: is utilizing -> utilizes

Accepted.

L337-338 I do not understand

Rephrased:

“The fuel cell exhaust emulator (FCEE) is designed to emulate the exhaust composition expected from future hydrogen fuel cell propulsion systems. It was designed by Airbus and built according to the design of Airbus. AV Experts LLC adapted and installed the system onboard their Convair C131 aircraft for the Nephele flight test campaign.”

L370: saturation versus liquid wate -> saturation with respect to liquid water

Accepted.

L378 Global Forcast System -> Global Forecast System

Accepted.

L397: is analyzed -> are analyzed

Accepted.

l 401: exemplary -> example

Accepted.

References

Amanatidis et al, Aerosol Science and Technology, 2018:

Citation: https://doi.org/10.5194/egusphere-2026-2887-RC2