Atmos. Chem. Phys. Discuss., 12, C2496–C2504, 2012
www.atmos-chem-phys-discuss.net/12/C2496/2012/


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "A methodology for in-situ and remote sensing of microphysical and radiative properties of contrails as they evolve into cirrus" *by* H. M. Jones et al.

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Received and published: 13 May 2012

The central theme of this study is how to better understand the radiative impact of contrails by a more efficient use of modeling and measurement tools. The basic steps are: 1) forecast the best area to form contrails, 2) fly to that area and form a contrail, 3) measure the contrail properties and 4) evaluate the measurements with the assistance of a transport and dispersion model. This approach is described and one case study presented to demonstrate its viability. The approach is reasonable and probably a good one to use in the future to unravel the many thorny questions associated with contrails, cirrus evolving from contrails and cirrus and contrail scarmblets.



There are many questions, however, that should be addressed before widely advertising this contrail research approach as the most optimal. These questions are related to 1) the choice of a circular orbit for laying down a contrail and the subsequent sampling strategy, 2) the analysis and interpretation of the measurements and 3) application and validation of the post-analysis simulations of transport and dispersion.

Regarding the choice of circular orbits, the only advantage that is given is that they can be more easily identified from satellite imagery but the disadvantages are not discussed. I think that if this study is meant to be a potential blueprint for future field programs, the pros and cons of each of the components need to be equally presented. For example: 1) are circular orbits more susceptible to contamination by emissions and contrails from the process of sampling? 2) are multiple passes through the contrail, needed to get better statistics on the cross section, more difficult to execute? 3) do circular contrails evolve differently than the normal linear contrails whose radiative impacts are impacting climate?

Questions regarding the analysis and interpretation of the measurements concern how the size distributions and optical array probe data are handled and are detailed more explicitly below.

The NAME model is used to determine the age of the plume but its fidelity does not appear to have been very rigorously tested other than in its ability to locate the plume, i.e. given that it is a dispersion model, how does its prediction of the plume width compare with either in situ, lidar or satellite measurements of the plume diameter? Some very rough closure would help the credibility of the model as one of the research tools.

In the abstract, introduction, discussion and summary it is emphasized that more and better measurements are needed in order to improve the parameterizations in models of contrail properties yet in the end, I was not sure what I could use from the measurements published here if I was a modeler wishing to improve my model. I think that the

discussion and summary should both make this more clear.

From a general point of view regarding writing style, it seems that much of what is found in the discussion section is either a repeat of what is in the introduction or should be in the introduction. At first I thought the many references to previous studies were in preparation to comparing with the current results but that doesn't seem to be the case. The discussion should focus on helping the reader understand the results, put them into context with what is currently known about contrail formation and evolution and highlight what remains that needs to be studied.

Finally, although this is the prerogative of the authors, there seemed to be somewhat too many hyperbolic statements made throughout the text, e.g. "Contrails are thus predicted to contribute a significant warming of climate over the coming century..". Although I agree that it is important to study contrails because of their potential contribution to changes in local climate, a statement like this seems to fly in the face of the most recent IPPC reports. A number of other declarations are made throughout the text that I will point out below that perhaps are a bit overstated. This is only my opinion and the authors are certainly not required to change their style and my final acceptance of this manuscript does not require it.

Specific Comments

Page 7832, line 26: "Furthermore, older measurements of in-situ contrail ice crystals are likely to be susceptible to artefacts as they were taken by probes susceptible to shattering of larger particles on the inlet (e.g., Korolev, 2011; McFarquhar et al., 2007), producing artificially enhanced number concentrations." This is still largely unproven for contrails, especially for young contrails where ice crystals are quite small, as in the current study, and shattering would not be a factor. Gayet et al showed with FSSP and CPI measurements that there is little evidence of shattering in contrails with small ice. The actual threshold for shattering and the magnitude of the effect still remains a moving target with no quantified results.



Page 7834, line 19. The response time is given for the General Eastern but not the Buck research instrument. It is sampled at 1 hz, but at temperature's colder than -40 with similar dew point depressions, I was under the impression that the response time was longer than a second. If so, in order to line up the RHI measurements with the particle measurements, some type of time shifting is needed. I believe that this is done by others who use this instrument.

Page 7835, line 5: Please change the acronym for the two dimensional cloud probe from "2-DC" to "2D-C". The latter is the convention. Should also probably state that this instrument has 32 diodes.

Page 7835,line 7: Regardless of whether the first size bin is used in the 2D-C, its range is still 25-800 um, 25 um resolution.

Page 7835,line 10: The conventional way to designate the DMT CIP grayscale with 15 um resolution is CIP-GS (15 um resolution). The CIP-GS has 64 diodes.

Page 7835, line 14: "poorly defined sensitivity" is defined as what? I think that is is stated incorrectly. The issue with every optical scattering instrument is the uncertainty in the lower threshold of the first channel.

Page 7835, line 17: Why are channels 27-30 not being used?

A pause here to emphasize that in this section and from here forward, please emphasize that 25-800 and 15-960 um are the "nominal" size ranges for spherical particles. The size range for the CAS-DPOL needs to be stated as "optical, water equivalent diameter" underscoring that these size ranges are based assumptions of sphericity and known refractive index, an important assumption when measuring non-spherical ice crystals and trying to explain differences in size distributions under different conditions.

The second question is how are the CIP-GS image data evaluated? Does sample volume use center-in, all-in or reconstructed? How is size defined – area equivalent (I recommend that), maximum width, maximum length, projected length or something

else? What corrections are applied? Certainly the Korolev correction for out of focus particles needs to be considered since all of the examples of images clearly show the "donut" feature that is an indication of being out of focus. This leads to oversizing and might also contribute to the "knee" in the distribution that is mentioned later.

Page 7835, line 23: An uncertainty of 15% is for spherical particles with known refractive index. For the measurements discussed in this paper, that error is clearly much larger and difficult to quantify.

Page 7836, Line 1 footnote. I think that this is overstated. There are now several processing techniques that are routinely used to adjust for these cross-over mismatches with little loss in data fidelity or increase in uncertainty.

Page 7836, Line 5: Suggest editing to instead say "The CAS instrument was fitted with a single particle backscatter depolarisation detector to help distinguish aspherical ice crystals from water droplets but these data are not reported as all particles were ice."

Page 7836, Line 17: "...600 m footprint...". So even though the vertical resolution is good enough to provide high resolution information on the structure of the contrail, it gets smeared out by the horizontal resolution of 600 m? Is there pulse to pulse deadtime, i.e. are the 600 m pixels contiguous or is there a deadzone?

Page 7837, Line 27: The Rosenberg reference is now published.

Page 7838, line 17: What was the plan for validating the NAME model? Is 4 km sufficient resolution in the meteorological data to govern transport and dispersion at the contrail scales?

Page 7839, Line 10: Here is where the disadvantages of circular patterns also needs discussing.

Page 7839, Figure 10. This figure should be greatly expanded to show only the region of the flight track and color-shaded by the CAS-DPOL or CIP-GS concentration measurements to show the actual locations of contrail penetrations. Was there no CN



counter onboard? These are quite fast response, you don't need to rely on NOx measurements, and the CN is not significantly impacted by shattering when in contrails. In my opinion, comparing CN with CAS measurements is a much more reliable way to know when you may be in the plume.

Page 7840, paragraph beginning at line 10. I have a difficult time envisioning a sampling strategy in this type of contrail that would not contaminate subsequent samples, either by the formation of new contrails or seeding with jet exhaust. Sampling strategy is critical when proposing this type of contrail. With linear contrails, the chase aircraft can sample from oldest to youngest without fear of contaminating the subsequent samples with aircraft produced ice particles (APIPS). This is an important discussion to include here.

Page 7840, line 26: "This information subsequently proved useful in determining the portion of the contrail being penetrated as a function of age." How were this information validated? It is a dispersion model, so how did the radius of the plume vary with age? Any type of closure performed with the model, satellite, lidar and in-situ data? Wouldn't that go a long ways towards convincing future researchers to take this approach?

Page 7842, line 1: "For orbit passes 4 and 8, higher CIP values are not recorded,"What does this mean, not recorded? Not observed?

Page 7842, line 4: "...have been reported including, 5 and excluding this data in Table 1.". What does this mean?

Page 7842, line 10: "...this highlights some of the difficulties in assessing contrail microphysical ice concentrations as a function of plume age." I think that not nearly enough emphasis has been placed on the very limited number of contrail penetrations that were made, as well as how the samples were taken. Because the microphysical properties depends so much on where you passed through the plume, previous studies have intentionally flown patterns that allowed many penetrations through a plume with the air mass at approximately the same age. For example, we had similar issues when

making measurements behind a lear jet (Baumgardner, D., R.C. Miake-Lye, M.R. Anderson, and R.C. Brown, 1998: An evaluation of temperature, water vapor and vertical velocity structure of an aircraft contrail, J. Geophys. Res., 103,8727-8736.) when we made 243 pentrations at distance 150 to 5 km behind a contrailing aircraft. This was barely sufficient to obtain enough statistics to validate a wake vortex model and more or less define what the plume, microphysical cross section looked like. This is the type of issue that should be part of the discussion section.

Page 7842, line 22: "The PCASP did not reveal any corresponding increase in concentration, however this instrument is known to have issues when sampling in ice clouds (due to shattering issues) so we cannot independently verify these high aerosol number concentrations." A CN counter would help to resolve this problem. I thought that this research aircraft was so-equipped.

Page 7842, paragraph beginning at line 28. This is where the image analysis approach and corrections are important. This "knee", while likely a real shift in the characteristics of the crystals, might be imparted due to how the data are analyzed. In addition, there is no reason to put the size scale on a log axis. Features related to the shape of the size distribution become obscured by log scaling.

Page 7843, paragraph starting at line 18. Although this paragraph states an obvious fact about contrail sampling, it does nothing to help clarify the usefulness of the current data set, i.e. if the microphysical properties at the edge of a 5 minute old contrail look like those near the center of a 10 minute old contrail, then how can you sort this out? The modeling helps but you need to know where you actually were in the contrail.

Page 7846, line 14: "It is unfortunate. . .". Why is this unfortunate?

Page 7846, line 21: ". . .the signals do not always track one another, e.g. see the black peaks between $-1$ and $-0.5$ longitudes..". I don't understand what this means.

Page 7846, line 27: "This is potentially an effect of the aircraft passing through the



contrail on the eastward run and increasing the water vapour source, or the activation of a newer contrail on the eastward run." This is precisely what I was pointing out earlier and wonder how big of a problem this can potentially be.

Page 7847, line 11: "(see peaks at the extremes of the Fig. 12). Extremes? Extremes of what?

Page 7847, line 25: ". . .with a shoulder appearing in the size distribution with a mode at 60–70 $\mu$m, suggesting that the smaller particles present earlier in the contrail started to grow once the initial larger particles had been removed by sedimentation.". Caution that this is not a processing artifact. Is this still here after corrections for out-of-focus particles?

Page 7848, line 7: What does "radius" mean when measuring ice crystals? This is why using area equivalent diameter would be best when analyzing the CIP data and calculating the extinction since in actuality it is the cross sectional area that is important, not the radius squared.

Page 7848, Figure 14. This figure is very confusing with respect to the various panels and which lines are which.

Page 7849, line 11: "It is for these reasons that measurements like those presented here are necessary to increase the scientific understanding of contrail generation and evolution." And how have the measurements presented here icreased the scientific understand of contrail generation and evolution? This never became clear to me.

Page 7849, line 16: "The incorporation of lidar measurements into the experimental design allowed in-flight assessment as to the location and spatial extent of the contrails that were being sample. . ." To an accuracy of approximately +/- 600 m.

Page 7849, line 19: Extensive measurements? This is one of the hyperboles I was mentioning. Compared to previous studies, this is a pretty limited, although clearly interesting, data set.

Page 7849, line 20: “1 to >2000 $\mu$m”? The largest size mentioned in the instrumentation section was 960 um. How were larger sizes measured?

Page 7849, line 26: “..with more complete instrument suites..”. Perhaps a recommendation would be useful here or in the summary as to what would have made the current study more useful?

Page 7850, line 9: “Past observations of crystal growth show most ice crystals generated are 8$\mu$m with concentrations rapidly reducing to 10–15 cm$-$3 due to plume mixing (e.g., SchrÂÍoder et al., 2000 – using FSSP and impaction technique, sampling behind contrailing aircraft).” This contradicts an earlier statement about contamination by ice shattering, at least the assertion that so many previous studies are suspect. You can’t cast doubts on previous studies while at the same time comparing current studies and acknowledging similarities.

Page 7850, line 12: “Contrail ice crystals were reported to be regularly shaped crystals and . . .”. Regularly shaped crystal means well-defined habits?

Page 7850, line 3: How do you know when you are at the periphery? The sampling technique used in the past helped define when the measurements were at the periphery but this doesn’t seem possible with the current data set. Were there no video photos that might help with this?

Page 7853, line 2: “. . .care should also be taken when using data from previous studies.” Again, in my opinion this is overstated for the majority of the previous contrail studies.

Page 7855, line 20: “..the difficulty in determining what part of a 3-D contrail has been sampled (central region, edges or fall streaks).” And this confuses the reader since peripheral measurements are mentioned several times.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7829, 2012.