Reviewer one comments: Darrel Baumgardner

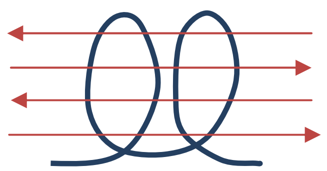
The central theme of this study is how to better understand the radiative impact of contrails by a more efficient use of modeling and measurement tools. The basic steps are: 1) forecast the best area to form contrails, 2) fly to that area and form a contrail, 3) measure the contrail properties and 4) 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?

The authors would like it noted that the formation and measurements of circular orbits is not a restriction of this method, and any flight pattern may be flown and still use the techniques described in this paper. As outlined in the paper, conditions ideal for contrail formation/sampling are primarily highsupersaturation and cloud free. Regions that are forecasted to have these conditions need to be within allowed flight areas – where the advected evolving contrail will remainwithin allowed flight areas. Restrictions on available airspace for flying are greater for EU/UK airspace thanin other regions of the world but where emissions may be of significant importance. In such cases where there is only a small area of ideal conditions constrained by airspace restrictions, a circular flight pattern like the one shown here may be preferable.

In order to collect sufficient data for statistical analysis, multiple passes, or longer passes are required. For the case study used in this paper, the conditions were such that contrails did not persist as long as we would have liked, and so did not form visible contrail-cirrus. This prevented consecutive sampling over a longer time period.

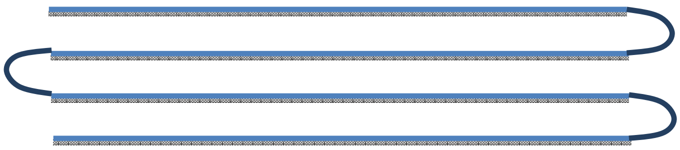
Provided that the flight is planned carefully using the forecast tools, and the wind is sufficient to produce a ‘coil’ of contrail, it should be possible to sample different sections of the contrail without contamination – by flying chords through the coil pattern something like this:



i.e. the contrail is sampled when it is younger during the crossovers when orbiting and then sampled at various ages when doing consecutive SLRS (e.g. the above allows 18 passes at various ages). The number of SLRs you would be limited to would depend the contrails lifetime. Fewer initial orbits would allow more passes later on; more orbits would allow you additional earlier measurements and more measurements at similar ages for each SLR. Larger diameter orbits would provide more ‘independent’ SLRs, but sacrifices time in their formation and so the age of each orbit would vary more widely too. There are many factors to be considered.

An alternative flight pattern might look something like this:

1
2
3
4



Once this pattern has been formed the aircraft could then choose to sample any of contrail just formed, e.g. once completed contrail 4, could fly back through (following the contrail) 1 and 3, then 2 and 4, etc. This approach would allow you to sample contrails of various ages. In this case, as well as the circular case – the additional use of forecast models and post-flight analysis using dispersion models help with identifying contrails sampled and the age when they were sampled. If the two-aircraft approach was taken, as outlined in the paper, then this information would be obtained more readily in-flight.

Changes to the text:
The following has been added to the start of section ‘3.2 Flight Patterns’: *The methodology outlined in this paper does not advocate a specific flight pattern. It is only essential that the chosen flight pattern allows successive uncontaminated sampling as the resultant contrails evolve.Due to limited favourable airspace regions, circular contrails, such as those produced from the aircraft performing orbits, were chosen for this flight as they are a distinctive pattern (often appearing as spiral contrails due to advection, e.g. Haywood et al., 2009) that can be recognised easily in satellite images, and visually when flying at higher altitudes, see Fig.1.*

The following sentence is also added to the same paragraph lower down: *Repeated uncontaminated(by the sampling aircraft) sampling of orbit contrails is more difficult to carry out for orbits compared to the sampling of straight and level runs, but here we use lidar in flight to determine the position and spread of the contrails and use the NAME model post-flight to validate.*

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.

The dispersion in the model was deliberately reduced to be minimal so that we could identify where the centre of the contrail coils were with more certainty. This approach aids the graphical representation and allows an approximate age of the contrails sampled, either from in-situ measurements or by the remote-sensing instruments, to be assessed. This current study was not intended to be a rigorous comparison between the NAME dispersion model and the measurements, but it does show that NAME can be a useful tool for estimating contrail rate of spread and hence age which would otherwise be difficult due to the confounding factors discussed in the paper.

The NAME model has been used in a previous study when modelling the evolution of satellite detected contrails, Haywood et al. (2009) and this study is a logical extension. The previous paper showed good performance of the model over much longer time scales than those modelled here.

Changes to the text:

The following sentence has been added to the 'NAME' part of section 3.1: *To allow the contrail sampling age to be pin-pointed with ease, the dispersion in the model was deliberately reduced to allow the identification of the contrail centres.*

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.

The measurements presented in this paper are shown as an example of how to utilise the proposed method. For this particular case study, conditions were such that sampling was limited due to the duration of the contrails; this is stated in the current paper. The message we hoped to convey to the reader is that the approach laid out in the paper when implemented under more favourable contrail conditions would provide ample and consistent measurements for contrails as they evolve into cirrus-like clouds, thus providing much-needed information by modellers hoping to develop contrail parameterisations.

Changes to the text:
In the first paragraph of the discussion: *It is for these reasons that measurements like those that can be obtained using the described method ~~presented here~~ are necessary to increase the scientific understanding of contrail generation and evolution.*

Towards the end of the Conclusions:*The approach demonstrated here and the data~~provided~~obtained using this method can be used to help build a physically based parameterisation of contrail spreading into cirrus and will prove useful for successive studies.*

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.

Changes to the text:
Some of the text describing past studies has now been moved up to the introduction section as requested. The discussion section now focusses on the current method and data.
An adapted manuscript shall be submitted with these comment responses.

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 mostrecent 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.

The wording of this statement, and those listed separately below, have been addressed.

Changes to the text:
Contrails therefore have the potential to contribute a significant warming of climate over the coming century…

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 FSSPand 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.

Shattering is considered to be a significant issue when there are many large particles/few smaller particles. Work is on-going in this area and the threshold size of 'large' particles is not yet agreed upon. However, it is generally thought to be a few hundred micrometres diameter. So the wording of the above sentence needs to be changed to clarify this point: that shattering is potentially an issue if larger ice crystals present. As such, this may be an issue under certain contrail sampling conditions: when sampling within aged cirrus or when sampling in highly supersaturated regions that allow crystals to grown quickly.

Changes to the text:
From section 1: *Furthermore, older measurements of in-situ contrail ice crystalswhere crystals greater than a few hundred micrometres in diameter were present 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.*

From Section 5:*Due to known issues with certain cloud probes regarding inlet shattering of larger cloud particles (Korolev et al., 2011), care should also be taken when using data from previous studies where larger ice particles were present.*

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.

In the submitted version of the manuscript, the following sentence is included: "*Data are recorded at 1 Hz, with uncertainties within ±0.5K depending on the variability of the conditions.*" Where this is in reference to the Buck instrument. The dew point temperature data used in this paper is from the General Eastern (GE) Instrument as the Buck instrument was only running for a short period of the flight where the contrail passes did not occur. This is why we have not gone into great detail regarding the Buck instrument.

However, after discussion with Alan Woolley at FAAM, the following correction has been applied to the GE dew point temperatures from which the RH values have been calculated.

The GE1011b is a chilled mirror hygrometer and suffers at lower temperatures due to lower thermal mobility across the layer of ice supported on its mirror. Perhaps more significantly, the lack of available water molecules to form the required ice layer ice causes the controlled mirror temperature to lag the true frost-point value. This effect is potentially magnified by compromises that are made in the control loop for the mirror temperature, and also the lower cooling efficiencies in the two-stage mirror Peltier at these ambient temperatures.This temperature-dependent lag has been assessed using clear air comparison of GE data with a fast-response Lyman Alpha Total Water Content (TWC) instrument (REF) from flights B412, B414 and B433. Across these three flights 26 features were identified, at a range of humidities, which were present in both the TWC and GE data, and which were judged to be attributable to the same water vapour features. The time difference between the respective feature appearing in the GE and TWC data was determined, a power law was fitted to these data, and this yielded an expression for the time lag in the GE data:

y=-1.1709 +7.5285 [MR]^(-0.82758)

Where y is the time lag and MR is the mixing ratio derived from the GE instrument and the aircraft measured static pressure.

Using the above equation it was possible to derive a likely time lag that could be expected to be exhibited by the GE data at the low humidities that were experienced during B587, which was up to 232s in the driest air. An alternative time-base was then produced, and the GE data substantially corrected for this lag.

[REF: Nicholls S, Leighton J, and Barker, R: 1990, A new fast response instrument for measuring total water content from aircraft, J Atmos Oceanic Technol, 7, 706-718]

As a result of this correction to the GE time-base, Table 1 and Figure 4 have been adjusted accordingly. Changes to the text are also necessary as a result of this correction (and corrections to the CAS/CIP data):

Current Text: "*A closer examination of the RH_{ice} field within the flight area showed that during pass 4 the aircraft traversed a region where the vapour mixing ratios were higher in the northern section of orbit 2 compared to the southern section. This gradient likely explains the variation in the growth of the size mode as a larger source of water vapour would allow growth of the ice crystals to larger sizes following contrail formation compared to the passes occurring in the lower RH regions. This effect can also be seen in the satellite image (Fig 2a) and Fig. 4 where the size distributions corresponding to higher RH_{ice}show higher number and sizes compared to those corresponding to lower RH_{ice}.*"

Has been changed to.:"*A closer investigation into the particles sizes distributions (Fig. 4) and the RHice field showed that when higher RHice values were reported, particle size distributions reported by the CIP-GS probe tended to show higher number concentrations and larger particles. An exception to this is pass 3 which showed high RH, but a lower size distribution, this could be due to sampling the edge of the contrail rather than the centre.*"

Current text: *Highest RH was reported for orbit passes 4, 7 and 8. Pass 8 sampled the same section of contrail previously sampled in pass 4 (see Table 1). Peak CAS concentrations are much lower for pass , but as revealed by the lidar plot, Fig. 7, during the over-flight the contrails were lowering over time due to large scale subsidence in the region*.

Has been changed to: *Highest RH was reported for orbit passes 3, 7 and 8. Pass 8 sampled the same section of contrail previously sampled in pass 4 (see Table 1). Peak CAS concentrations are much lower for pass 8, but as revealed by the lidar plot, Fig. 7, during the over-flight the contrails were lowering over time due to large scale subsidence in the region.*

In Section 6 Conclusions, Current Text: "…*with a shift to larger (100 µm) crystals only observed when RH_{ice} was higher, thus illustrating the impact of environmental supersaturation gradients on contrail development.*

Has been changed to:"…*with an increase in size and number of particlestypically seen for passes where higher RH (>120%) was noted, thus illustrating the impact of environmental supersaturation gradients on contrail development and the need for high resolution measurements of this type.*

Alan Woolley has been added to the author list of this paper in recognition of this additional analysis.

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.

Change in text as stated.

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.

Change in text as stated.

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

Change in text as stated. All ‘CIP-15’ and ‘CIP’ have now been changed to ‘CIP-GS’.

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

Reworded in the text to convey the above.

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

These channels are now included with the corrected CAPS data.

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.

I have added the following sentence to the end of the cloud microphysics section: *It must be noted that the stated size ranges for the CIP-GS and 2D-C are the nominal size ranges for spherical particles, and the CAS-DPOL size range is for optical, water equivalent diameters.*

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 focusparticles 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.

The reconstructed technique is used here to determine the sample volume. This could be done because the particle images showed a symmetric shape. Furthermore the area equivalent sizing method is used to determine the size of the cloud particles measured by CIP. The application of the Korolev correction for out of focus particles did not alter the measured contrail size distributions significantly.

For the CAS data, the algorithm by Johnson is integrated in the analysis to counteract sizing uncertainties due to imperfect electronic responses of the amplification stages implemented in the sizing technique.

Finally, the airspeed measured by the turbulence probe is used to determine the sample volume as the

TAS measured by CAPS is systematically lower by 30% than all other velocity measurements on board the BAE146.

All plots containing CAS and CIP-GS data have been re-plotted using the newly corrected data, and changes to the text have been made accordingly.

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.

The text has been changed to: *(i.e. the accuracy of sizing was of the order of ~15 % in this size range for spherical particles with known refractive index; here we are measuring small ice crystals, so the error is 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.

Here the processing technique described by Johnson is used to adjust for cross-over mismatches.

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."

Change text as stated.

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?

The pulses are pretty much contiguous. The choice of this integration time is justified by the horizontal scale of the observed features (this can be seen figure 7).

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

Rosenberg, P. D., Dean, A. R., Williams, P. I., Minikin, A., Pickering, M. A., and Petzold, A.: Particle sizing calibration with refractive index correction for light scattering optical particle counters and impacts upon PCASP and CDP data collected during the Fennec campaign, Atmos. Meas. Tech. Discuss., 5, 97-135, doi:10.5194/amtd-5-97-2012, 2012– added to the reference list.

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?

As mentioned in the responses to the initial short comment, we were not validating the NAME model in this paper. 4km resolution in the meteorological data was found to be sufficient for representing the transport within the model as there was little variability in the wind speed and direction for this case study. There is no routinely archived data available at a higher resolution than this.

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

This comment has been addressed as part of the reviewer's initial short comments.

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.

I am unsure exactly what the reviewer means regarding 'greatly expanded to show only the region of the flight track', but this plot shows the flight data for just Run 2. However, the plot has been re-done as suggested, with in-contrail times shown in black, along with out of contrail times in grey.

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.

We agree with the reviewer's comments that a CPC (as for the case of BAe-146 research aircraft) can be used to detected when in young contrails, unfortunately, this instrument was not on-board the aircraft for this flight. NOx instrument, coupled with CO instrument, may also provide additional information to allow the user to determine between contrails formed by different aircraft in the science area. Having the full suite of instruments available on the aircraft would be beneficialin future.

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.

This has been addressed as part of our response to the reviewer's earlier short comment. We agree that care is needed when flight planning, and a constant steady wind is the ideal so one can predict to first order where contrails will evolve. We have learned from this case study and others as part of the COSIC campaign and hope to pass this information onto other groups who may wish to perform similar studies. We have noted potential contamination from our own aircraft for this case study in the text and identification of this was assisted by the use of the NAME model post-flight.

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, lidarand in-situ data? Wouldn't that go a long ways towards convincing future researchers to take this approach?

This comment has been addressed as part of our response to the earlier short comment. The NAME model predicts the contrail locationsto validate the in-situ measurements and interpretation of multiple contrail interceptions and hence evolution and age.

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

Change to 'not observed' in the text.

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

Rewording required. How to make this clearer?
Current text:

On several occasions when sampling contrails, CAS number concentrations increased to high values (> 50 /cm^3) for a short period of time, for these occasions, peak and mean concentration values have been reported including, and excluding this data in Table 1.

Change to:
On several occasions when sampling contrails, sudden short-livedhigh increases (to > 50 /cm^3) in CAS number concentrationswere observed.For these occasions peak and mean concentration values have been reported in Table 1 where values are reported for both including, and at the same time excluding this data in lieu of potential shattering contamination not fully corrected for.

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 acontrailing 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.

This comment has been addressed as part of an above comment. The text has been changed in the paper to state that we are presenting a limited dataset (when compared to other past journal papers) for one case study. The lack of data is an issue when considering the statistics that might be required for model testing – however, due to conditions during these flight, we were not able to get persistent contrails for long enough to allow us to performmany successive passes.

The issues mentioned can be solved by the use of the NAME model; we have shown how contrail age is determined successfully. We are saying this methodology has the potential to deliver the data required.

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 thatthis research aircraft was so-equipped.

This has been addressed as part of an earlier comment.There was no CN counter on the aircraft for this flight.

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

These plots, and the associated text have now been changed as the corrected data is available. The log scaling on the x-axis has been removed for figure 4 now.

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.

We were trying to highlight some of the issues that still exist by showing the range of concentrations and size distributions within these evolving cirrus compared to previous attempts at such measurements. For this flight contrails were not visible at the same altitude as the contrails were dispersing. Having a second higher aircraft with lidar would help with this issue.

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

Unfortunate in that it causes confusion, and means you cannot clearly say what is going on.

Current text: It is unfortunate that for this occasion, these locations and times overlap with the NAME predicted locations of signals seen during the eastward leg (as indicated by the arrows in Fig. 12).

Change to: On this occasion, these locations and times overlap with the NAME predicted locations of signals seen during the eastward leg (as indicated by the arrows in Fig. 12).

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.

The text the reviewer is referring to was in relation to a previous version of Figure 12 where the first channel of the CIP instrument had been included in the number concentration and so the ‘black’ data for CIP showed peaks in the range -1 to -0.5 longitudes that consisted solely of measurements in the first bin. However, a similar thing also occurs for the ‘red’ data in the current version of Figure 12. The point being made was that peaks in the difference size ranges (from the different instruments with different measurement size ranges) are not always synchronous. Sometimes CIP NC increases, while CAS NC decreases, and vice versa due to the evolving size distributions. The text has been changed to emphasise this.

Current text: Although increases in concentration from the background level are seen at the same time for each of the probes shown in Fig. 12, the signals do not always track one another, e.g. see the black peaks between -1 and -0.5 longitudes. This change in the measured concentrations for the different probes (which measure different size ranges) is indicative of the change in structure across a contrail edge and through different parts of the contrail.

Change to:*Although increases in concentration from the background level are seen at the same time for each of the probes shown in Fig. 12, the signals are not always synchronous, e.g. as in the red peaks between 1 and 0.5 longitudes where the peak show a similar magnitude for the CAS probe, but not for the CIP probe which measure particles in a different size range.*

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.

This comment has been answered as part of the short comment response above. If the approach is modified to that outline in the paper – specifically the use of a second over-head aircraft that can inform the sampling aircraft of the exact location of the contrails, this can be avoided. We have used the NAME model in this instance to indeed note the fact that there was potential contamination for this one pass.

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

The text has been changed to: *see the peaks at the western edge of Fig. 12a*

Page 7847, line 25: ": : :with a shoulder appearing in the size distribution with a mode at 60–70 _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?

The plot that this refers to has been re-plotted using the corrected CAPS data, the new figure and associated changes in text are now included in the new version of the manuscript.

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.

In this case 'radius' is half the area equivalent diameter.

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

This figure has been updated to include the additionally corrected CAPS data, and we have attempted to make the figure clearer to the reader.

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 increased the scientific understand of contrail generation and evolution? This never became clear to me.

This sentence has been changed to: *It is for these reasons that measurements like those that can be obtained using the described method are necessary to increase the scientific understanding of contrail generation and evolution.*

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.

The reviewer comment is correct but does not need a response here becauseit isalready discussed in the section where we introduce the lidar. The observed features have a horizontalextent much wider than 600 meters so this is not a limitation.

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.

The word 'extensive' has been removed and replaced with 'detailed'.

Page 7849, line 20: "1 to >2000 _m"? The largest size mentioned in the instrumentation section was 960 um. How were larger sizes measured?

We thank the reviewer for noticing this mistake; this has been changed to an upper limit of 960 μm.

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?

Current text: The combined use of the above allows us to report aerosol and cloud microphysics information for contrails of known age and it is expected that future experiments with more complete instrument suites will benefit from this study.

Change to: The combined use of the above allows us to report aerosol and cloud microphysics information for contrails of known age and it is expected that future experiments with more complete instrument suites*(cloud microphysics probes covering a variety of size ranges and providing particle images; fast response aerosol particle counters and composition measurements; fast response trace gas measurements; lidar and radiometers for remote sensing)would be a benefit for future studies using this approach.*

Page 7850, line 9: "Past observations of crystal growth show most ice crystals generated are 8_m with concentrations rapidly reducing to 10–15 cm 3 due to plume mixing (e.g., SchrÂ´loder 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.

We have changed the earlier text to state that shattering is potentially a problem particularly if larger crystals are present. This study focuses on contrails with small size distributions, mostly <20um.

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

Yes – and this has been changed in the text as part of the response to one of the earlier short comments.

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 peripherybut this doesn't seem possible with the current data set. Were there no videophotos that might help with this?

There was video for this flight, however it was not easy to see the contrail when flying at the same altitude as they were not long-lived or particularly dense. We determine that we are in the periphery by using the NAME model to confirm the position of the contrail, and by using the observed increase in number concentrations above the background level. If a second aircraft were used as is suggested in the discussion section of the paper, it would be able to pass this information in-flight, and down-facing video or lidar data post-flight would help with data interpretation.

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.

Reword to: *care should also be taken when using data from previous studies where larger ice particles were present.*

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.

We are suggesting that our methodology helps to improve this post flight and can remove a good deal of this uncertainty from the analysis.