Review of

Ground-based contrail observations: comparisons with flight telemetry and contrail model estimates

by Low et al. submitted to AMT

General comments

The paper reports on the installation of a ground-based camera to take pictures of contrails in the upper troposphere and on the derivation of contrail-relevant statistics and properties from such data. Furthermore, the observations are compared on a statistical basis with model estimates obtained from a simplified contrail plume model.

I commend the authors for their efforts in setting up the camera system and developing a cost-effective approach to monitoring the initial contrail stage. The paper is generally well-written and clear. However, I feel that sections 2.4, 3.1 and 3.2, which discuss contrail occurrence frequencies and differences between observations and models, are somewhat detailed. In contrast, section 3.3, which addresses the relationship between contrail occurrence and meteorological conditions, is less detailed and only scratches the surface. This is regrettable, as a more comprehensive comparison would provide more substantial recommendations for improving the Cocip model. In its current form, the analyses are not sufficiently conclusive, and numerous questions remain unanswered. In my specific comments, I provide several recommendations for expanding this section. Moreover, some conclusions are drawn without sufficient substantiation.

I can recommend publication after major revision.

Major specific comments

As mentioned above, the current analyses leave too many open questions. I believe addressing the following questions/issues will make the study stronger.

1. It should be made clear in the abstract that the contrail-cirrus that you are able to track are not yet mature, and that the observations cannot

adequately constrain model estimates of climatically relevant contrail-cirrus that typically live much longer than 30 minutes. It is recommended that this limitation be mentioned in the abstract.

2. Contrails in Cocip are initialized after wake vortex break-up. Hence t=0 in the model refers to a contrail age of several minutes. Have you considered this temporal offset in the comparison with the observations? You have not mentioned this; hence I assume you disregarded this effect. As the contrail lifetimes in the analyses are relatively small, neglecting this offset probably plays a role. Furthermore, your large class of observed contrails with t<2 min cannot be compared to CoCip outputs. Moreover, you can’t even obtain an estimate of the contrail altitude, which is used in your retrieval algorithm, from CoCiP.
3. Comparing the contrail width for t<2 min is not particularly conclusive, as the contrail spreading occurs over longer time scales. This early stage is not simulated with CoCip (see point 1). It would be of interest to obtain a PDF of NWP RHi and the Cocip-simulated contrail lifetimes for those cases where you observe short-lived contrails (t<2 min). An analogous survey could be conducted for the class 2min <t<10min. However, for the class t>10 min, it would not be as meaningful, as the observed lifetimes are likely a lower limit of the real lifetimes. (For further information, please refer to point 6 regarding a proposed redefinition of the lifetime classes.)
   The information content of Fig. 4 is dominated by the class of very short-lived contrails. It may be beneficial to conduct an a-priori analysis of these contrails, followed by their removal from the data set. This approach would enable a more focused analysis that considers only longer-living contrails.
4. It would be beneficial to ascertain whether advection outside of the domain or contrail dissolution is the limiting factor of contrail lifetime in the observations. This should be quantified for each lifetime class.
5. One possibly interesting analysis could be cast into a scatter plot of observed and simulated lifetimes (for all those observed contrails that dissolve inside the observed region).
6. The validation of the Cocip model comprises several components.

a. Firstly, the NWP input data (RHi, T, vertical wind shear) are crucial for the contrail initialization.
b. Secondly, the Cocip model physics are of significance for the spreading and dissolution of the contrails.

The initialization aspect is of particular importance in your study, given that you are dealing with very short-lived or at most young contrail-cirrus. Nevertheless, the second bullet point is not given sufficient attention in the current study. It would be beneficial to gain further insight into contrail physical processes through a comparison exercise (despite the lack of mature contrail-cirrus with lifetimes of several hours in your data set). This is pertinent to the issue raised in point 1. Contrail model physics determine the lifecycle and climate impact estimates by Cocip. Therefore, your observations should be used to test the Cocip model physics in more detail than presently done.
The introduction of three lifetime classes is a valuable addition to the study. However, it would be more logical to categorise all observations of contrails that dissolve before vortex break-up into a single class. This class can then be employed for the validation of the Cocip initialization and classes with larger lifetimes for the validation of the Cocip physics.

7. With regard to point 5b, it is recommended that time series of the width evolution for selected observed contrails be presented and compared to the CoCip-simulated width evolution. It would be beneficial to ascertain whether the observed spreading rates align with those observed in similar lidar measurements (https://doi.org/10.1029/95GL03549). A similar approach could be taken to assess the optical thickness. Is it feasible to determine the optical thickness from pictures?
8. In general, the fact that you observe the evolution of specific contrails is not exploited much despite this sentence in the conclusion ("*Ground-based cameras provide a cost-effective way to observe contrails, and unlike satellite imagery, their higher relative spatiotemporal resolution enables effective tracking of the formation*

and evolution of young contrails."). This aspect should be more fully explored.

9. I wonder how Cocip can be applied with vastly different timesteps It is my understanding that the implementation of model physics is valid for certain time step ranges. Using time steps of 30-60 minutes or 40 seconds should play a large role in how and which contrail physics are implemented. Hence, it would be interesting to investigate how the contrail properties depend on the time step chosen. It would also be valuable to ascertain whether simulating contrail-cirrus over several hours yields similar results irrespective of the time step. If this is not the case, how relevant is the validation of Cocip with small dt for a validation of Cocip typically run with much larger time steps?
10. Regarding the last paragraph in the conclusion: Cocip is a simplified model to study contrail-cirrus evolution. Therefore, it may not be only the input to the model (in form of NWP data), but the contrail modelling within CoCip that could require improvements. This should not be overlooked in this outlook.

Minor specific comments

a. Section 2.2.3 overemphasizes the importance of this aspect of contrail initialization. It is likely that this section has been included in the manuscript because some of the co-authors were involved in developing this Cocip extension. However, for the contrail width, which is the only contrail quantity evaluated in this study, this aspect should not play a significant role. Conversely, the physics relevant to contrail spreading of young contrail-cirrus in Cocip is not described.
b. How is contrail width defined and evaluated in Cocip? Please include a proper definition.
c. You use the term "rate", which typically refers to a change per time unit. Wouldn't "fraction" better fit in your case?
d. Line 34: it would be appropriate to also cite GCM results.
e. Figure 5: The current plot style makes it difficult to extract relevant information from Figure 5. Furthermore, the paragraph on this plot is quite short. It is recommended that the plot be improved or removed.
f. In lines 256 to 259 you mention two possible reason for the underestimation of contrail width in Cocip. But this list is by no means

exhaustive and without further analysis, this is merely a hypothesis. Therefore, it is recommended that the corresponding paragraphs in the abstract and conclusion be removed unless corroboration of this statement can be provided.

g. Line 256 “These results are consistent with Schumann et al. (2013)”: Could you please be more explicit? Did Schumann 2013 already show that contrail width is too small in CoCiP compared to observations?
h. Lines 201-204: it appears reasonable (and it is also convenient) to attribute the discrepancies solely to issues with the NWP input data. However, it would be fair to consider the potential shortcomings of Cocip that may also contribute to the discrepancies. For example, the usage as an offline model or other aspects.
i. Could you please explain explicitly what is meant by the “modelled background pixel intensity” in line 168?
j. Line 264: I believe saying Cocip simulates an ellipse in the horizontal plane is wrong. I guess what you want to say is that one of the principle axes lies in the horizontal plane.

Technical corrections

i. Fig. 6 caption mentions “false positive rate” whereas the corresponding text mentions “false negative rate”. I believe the latter is correct term.
ii. In the abstract, you mention a reduction of 17.5%. Given the uncertainties, I would feel more comfortable rounding it to 15% or 20%.
iii. Line 243: remove “an”
iv. Line 200: In all other formulas of a similar nature, the term "Camera" is the first index and "Cocip" the second. However, in this instance, the order has been reversed.
v. I believe, u and v are not defined in the text (only in plot axes)
vi. Line 307: remove “3”.