Reply to reviewer #2

I thank the reviewer for his generally positive comments and suggestions.

Here I respond to the comments, which I repeat at least partly in *italic*:

1. My major comment is to ask the author to extend validation of the CoCiP by using all available observations for this purpose where it is possible. For example, the author is well-aware about latest contrail results obtained in Pat Minnis' group (NASA Langley), which are not reflected in this paper. Also, latest cirrus observations by CALIPSO could be relevant for the CoCiP validation.

Preliminary results of such kind exist and have been presented in recent workshops. They will be published in follow-on papers including coauthors from the team mentioned

2. I have serious reservations about ability of bulk microphysics (BM) to describe a full life cycle of contrail. Limitations of the bulk microphysics are particularly obvious in predicting loss of the ice crystal number. Previous studies (Huebsch and Lewellen, TAC Proceedings, p.167-172, 2007; Unterstrasser and Soelch, ACP, 2010) clearly showed this drawback of the BM. Kelvin effect (which is also missed in CoCiP) is also important, especially when full contrail life cycles are studied. I understand that CPU limitations do not allow to use a size-resolving microphysics in CoCiP. Nevertheless, I got an impression that the author got overly optimistic about CoCiP ability to predict contrail ice crystal concentration. More critical analysis of this CoCiP part is needed.

I agree, a bulk model has principal limitations, and this is clearly mentioned in the paper. The Kelvin effect, for example, is important for the smallest particles and for explaining details of soot activation during contrail formation. The bulk model has no information on the ratio between largest and smallest ice particles. Hence, we cannot apply this model without treating the size distribution. In view of the many uncertainties (e.g., the correct soot size distribution), this is one of the several simplifications which can be justified only to the degree the model agrees or disagrees with observations. On the other hand, the model is obviously complete enough so that the results can be compared with observations. The results obtained agree favorably when compared to in-situ measurements.

3. Before discussing the complicated issues of contrail evolution and their RF, it is important to make sure that the CoCiP uses correct ambient atmosphere. In this context, it is important to compare primarily relative humidity at cruise altitudes and ambient cirrus clouds against available measurements. Detailed critical discussion on this subject is needed.

I agree that humidity is an essential input and this fact was identified in the paper. As explained in the paper, the consistency of the relative humidity of the ECMWF model input at cruise altitudes and ambient cirrus clouds was compared against available measurements, for example in Lamquin et al. (2009) and further papers as cited. Though not perfect, the model seems to perform reasonably. Certainly further such studies should follow.

4. More discussion on contrail lifetime and CoCiP ability to capture this important parameter is needed. Besides missing sub-grid scale processes, buoyant sloshing, Kelvin effect, the CoCiP model also uses sedimentation velocity for spherical particles and does not account for contrail radiative feedbacks (which could be important, e.g., Gounou and Hogan, JAS, 1706, 2007). I am not sure that once contrail particles start to fall down, coagulation among them is important. Many contrail evolution models (including the DLR model by Unterstrasser and Gierens) ignore ice particle coagulation, which may be a useful assumption for the CoCiP bearing in mind CPU *savings for its potential use in a global model. Recent JGR paper by Jensen et al. (10.1029/JD2010JD015417, 2011) could help here as well.*

More discussion on the contrail lifetime requires further comparisons to data. We have such data from satellite observations. However, the observations themselves need extensive explanations. Discussions of these aspects are, therefore, beyond the scope of this model description paper. We plan to describe this issue in a forthcoming paper with several co-authors.

The sedimentation velocity is not for spheres but, as was stated in the paper, for rough hexagonal
solid column ice crystals. (However, we do not suggest that this model detail is physically far better suited for this purpose than a model that would assume spheres, since large uncertainties result anyway and are included in the empirical coefficient E_A.)

I am aware of potential effects of radiative feedbacks as discussed by Jensen et al. (2011) and others. In fact, a simple radiative heating model is included in CoCiP, but requires many additional empirical parameters and further testing.

I agree that coagulation is one of the critical assumptions in this paper, and I this is stated in the paper. Coagulation is also unimportant when the contrail dries out in dry ambient air. Coagulation is also unimportant when the contrail particles stay small with small sedimentation velocity.
This is the case in particular for low ambient temperature because of low water vapor concentrations.
The paper by Jensen et al. (2001) considers tropical cirrus at temperatures below about 200 K with maximum ice water content of about 0.5 mg m^{-3}. Here we consider contrail ice at higher temperatures (up to 230 K) and ice water content up to about 10 mg m-3 (see Fig. 8). In this situation, coagulation appears to be important to explain the formation of large and quickly sedimenting ice particles which precipitate and form fall streaks, as observed (and as explained in the paper). This discussion will be added to the paper.

5. Comments on Figures: Some of them too busy. Figure 7 could have 4 panels instead of 2 panels. Fig.13 could either add new panels or drop some lines. I could not understand where are the thick and thin lines on its bottom panel. Fig.14-15 badly need more detailed figure caption explaining each and every curve.

I agree. This will be changed in the revised version of the paper.

6. Comment on paper structure. I would like to encourage the author either to move more equations from the main text to Appendices or even to separate all Appendices into a big Supporting Material. Only key equations should be left in the main text. This will make reading of this complicated paper a bit easier.

I thought that I shifted some of the secondary equations to appendices already. It is not obvious to me that separating equations from text makes reading easier. However, I will think about improvements in this direction further.

7. At the end of the paper it will be interesting to learn about possible future use of CoCiP. Projected use in a global model? Further comparison with observations? Participation in forthcoming aircraft campaigns? etc.

Some applications of the model were stated in the introduction. But the reviewer is right that further applications are possible including all those he mentioned. In fact such applications have been presented based on preliminary studies with CoCiP already at recent workshops. This includes in particular the use of CoCiP for predictions of contrail cover for suitable planning of measurements and for optimizing flight routes. Several further comparisons to observations (Lidar, Satellite, in-situ measurements) have been performed and will be described in forthcoming papers with the colleagues who generated the observations.

8. While my questions may be premature, I would like to know the answers to the following questions based on already performed CoCiP calculations: How sensitive are CoCiP results to the H2O accomodation coefficient onto ice (which varies from a few hundredths to 1 in published literature)? What regions of the atmosphere produce contrails with strongest RF? Any estimates of the global contrail RF based on CoCiP calculations? What technological options are promising for contrail mitigation?

The accommodation coefficient is important for non-equilibrium processes, and determines e.g. the speed of particle growth. Here, the model assumes local equilibrium where the accommodation coefficient does not appear in the equations.

The questions on RF concern the topic of forthcoming papers. Preliminary results, including a discussion of and technological options have been presented at an AIAA conference, see Schumann (2011), as cited in the paper.

9. Fig.11: As far as I understood the color bar for the bottom panel, cirrus+contrail optical thickness exceeds 10 for many regions over the globe. This value seems too large. Comments?

The plot shows the optical depth of ice clouds above 6 km altitude. This includes deep convective clouds with quite large optical depth, in particular in the tropics. This explanation will be included in the revised version of the paper.

*10. p.3194, line 7: Typo in: eta = F_a*V_a/(m_F*Q_fuel), i.e. the air speed V_a is missing.*

Thank you. Will be corrected in the revised paper.
A few further corrections were noted, as listed in the reply to reviewer #1.

Additional reference

Jensen, E. J., Pfister, L., and Toon, O. B.: Impact of radiative heating, wind shear, temperature variability, and microphysical processes on the structure and evolution of thin cirrus in the tropical tropopause layer, J. Geophys. Res., 116, D12209, doi:10.1029/2010JD015417, 2011.