Interactive comment on “Sensitivity of radiative properties of persistent contrails to the ice water path” by R. Rodríguez De León et al.

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We thank the referees for their careful reading of the paper, their acknowledgement of its contribution, and their very helpful comments. We intended the paper to provide an independent estimate of the variability that the radiative properties of persistent contrails may share with natural cirrus. The purpose of having an estimate that is independent of remote sensing measurements follows from the fact that the necessary conditions for contrail detection from space may preference specific stages in their development that may not be necessarily representative for global impact applications. It is for this reason that all the assumptions made in our study are based on physical parametrizations and not on radiative properties remotely retrieved. The IWC climatology used in our study, based on an extensive data base of *in situ* measurements at low, middle and high latitudes, showed a broad IWC range that we intended to apply in our experiments. The referees, however, consider that we should be very cautious when applying this range to represent the variability of persistent contrails. We believe that the referees make a fair point about this, and we are happy to reduce our assumed variability to one standard deviation in a corrected version of our manuscript. This will provide a more natural statistical approach and will leave the more speculative aspects of our conclusions for a more devoted study on the discrepancies between retrieval techniques.

Replies to referee's comments:

Anonymous Referee #2

This paper performs a sensitivity study of contrail radiative forcing to variations in IWC and contrail altitude. IWC and particle size distributions are specified as functions of temperature for three different levels of IWC corresponding approximately to a range of minimum and maximum observed distributions of natural cirrus clouds. Presumably, both geographical variations of temperature profiles and changes in flight altitude give rise to variations in IWC and Deg for a given IWC category. The approach appears to be similar to that employed by Minnis et al. (1999) but is more sophisticated in that IWC variability occurs with season and altitude. The main findings are that RF is roughly 3X greater during January than July; negative RF can occur over high latitude areas with ice-free oceans; average net radiative forcing is weakly dependent on flight level, but zonally, flight levels make a significant difference; and the optical depth range and, hence, the uncertainty in RF should be much larger than previously estimated or assumed. An argument is made that the contrail optical depths should be larger based on estimates of IWC derived from ISCCP data.
Although it has some good points (e.g., it introduces variable IWC and Deg into the analysis), this paper has some significant technical flaws in that it considers the IWC derived from ISCCP data as a reasonable value to assume for contrails because there have been observations of contrails having OD > 2. The origin of the ISCCP IWC estimates is highly questionable. The authors should stick closer to the mean values as they are more realistic. The discussion of flight level changes is confusing and does not aid our understanding. A consideration of the likelihood of forming contrails at those levels should be included in the calculations. It does little good to include unrealistic cases.
Because it has major flaws, it should not be accepted without major revisions addressing the concerns noted here.

Reply: As mentioned in the general reply, we are happy to reduce or assumed IWC variability range to one standard deviation and to modify the conclusions accordingly. We believe appropriate to clarify some points:

1. The IWC variability is based on Schiller et al.'s climatology, not on ISCCP estimates.
2. The assumption of a similar IWC dependence on temperature for natural cirrus and persistent contrails is based on measurements made during the CIRRUS III campaign. The case reported by Atlas and Wang (2010), in which a linear contrail reached an IWC close to the upper limit for mid-latitude cirrus (with an OD>2), is by no means the main reason for

adopting this assumption, but an illustration of contrail IWCs being comparable to the higher range of observed natural cirrus IWC.

3. We plan, in the future, to run our model in conjunction with a contrail formation algorithm, but at present, we believe that a separate radiative analysis, like the one we perform in our study, helps to disentangle the contributions from two highly temperature-dependent aspects of contrail effects.
4. In the manuscript we emphasise that the potential contrail coverage linked to some flight levels analysed can be negligible at low and high latitudes, but that at middle latitudes, where most of the air traffic is concentrated, contrail formation is possible for all the flight levels included in the discussion.

Comment:Pg 19931: My reaction to the authors' need to increase the ISCCP water contents by 80% is one of incredulity. First, the ISCCP data have been used for years and yield quite reasonable radiation budgets compared to observations according to many references such as Zhang et al. (JGR, 2004) and Minnis et al. (1999) referenced in this paper. Second, it appears that the authors converted the cloud water paths into water contents based on the layer's physical depth. What is the source of the physical depth? This is one of the critical factors in this study, especially in the discussion section where a case for using a much larger IWC is proffered. If one keeps the same thickness and increases the WC by 80%, then the TWP must also increase by 80%. This discrepancy with our previous understanding of the quality of the ISCCP data must be explained along with the source of the cloud physical depths. This requirement to increase TWP/TWC by 80% suggests that there is something fundamentally wrong with this approach. This also affects the use of the maximum IWC from Schiller et al. and the specification of Dge in the calculations.

Reply: We understand the referee's concerns about the scaling, as probably an 80% difference appears too big, but the difference between the cloud SW RF that we obtain without the scaling (-37.4 Wm^{-2}) and ERBE's value (48 Wm^{-2}) is not inconsistent with the discrepancies reported by Zhang et al.'s (JGR, 2004), who mention differences in the fluxes at TOA of 10-15 Wm^{-2} for regional and monthly means, and who also point out that although the uncertainty in the LW is random, the SW fluxes show a bias. There are examples of very complex efforts to reconfigure ISCCP's cloud data before it is used in sophisticated radiative models, see for instance Kim and Ramanathan (JGR, 113, 2008). Most importantly, the contrail radiative forcing results obtained in our study would not be significantly affected by the 80% TWC scaling, but we take on board the valid point made by the referee about the limitations of drawing conclusions based on ISCCP retrievals without a more systematic treatment, we will therefore exclude them in the revised version of the manuscript.

Comment: Pg. 19932: And it has a huge impact on the justification for using the maximum IWC fit because "it roughly approximates the range of IWCs retrieved from the ISCCP data for ice clouds. . .". The ISCCP IWC is increased by 80% and an unknown depth is used to find IWC from IWP. This is probably one source of having such large IWC values.

Reply: The cloud climatology used in our study allocates the ISCCP's IWP in the cell immediately below the reported cloud top. The layer's physical depth varies between 1 and 3 km, so despite the fact that cirrus may not present physical depths much larger than these, the referee's concern about the combined effect of this and the scaling of the water content is justified, and we will therefore stick to the mean IWC assumption.

Comment: Of the few satellite retrievals of IWC, the MLS and CloudSat retrievals are probably the better ones. Wu et al. (JGR, 2010) report both retrievals in their paper and show that the most common measurements of IWC at 10.7 km (35 Kft) are 0.0001 and 0.008 gm-3 in July. Much larger values occur, but are typically associated with deep convection. Using average values of IWC from satellite measurements (even the "ISCCP" IWC values) to represent contrails is most likely biased high because convective clouds and cirrus/contrails are two different beasts, yet they are included in any average that is computed. Thus, assuming that range of 0.51-2.1 can be used for contrails is silly unless one states that those values are for outlier contrails. Simply because large IWC or OD contrails occur does not mean that they should be used as a reasonable value for estimating errors in the global radiative forcing budget for contrails. The authors would be better off consulting the histograms published by Palikonda et al. (2005) or Kärcher et al. (2009) for estimating realistic ranges of contrail ODs rather than going through the manipulation of ISCCP IWP data.

Reply: Please refer to our general reply, especially with respect to our aim of having an estimate independent of remote sensing retrievals. We agree with the importance of not assuming deep convection characteristics for contrails. ISCCP's convective cases are actually excluded in the climatology, and we are confident that our assumptions exclude deep convective conditions; this can be confirmed by comparing Fig. 6 and Fig. 8 in Schiller et al., and observing that the maximum envelope of most frequent (>5%) IWC excludes the higher values retrieved in the vicinity of deep convection regions.

Comment: Pg 19937: Assuming that the reference is Minnis et al. (1999), the reported best estimate was assumed to be a mean optical depth between 0.3 and 0.5 and a net forcing around 20 mW-2. Note, their Table 1 shows that the difference in net RF changes by only 3 mWm-2 between ODs of 0.3 and 0.5, so it does not make much difference to the net RF whether 0.3 or 0.5 is used. From the use of OD=0.3 in the results presented in the figures, it would be reasonable to conclude that the best estimate is close to 0.3 and not 0.5.

Reply: We will clarify in the manuscript that the RF of 0.02 Wm-2 reported in Minnis et al. (1999) represents a rounded value corresponding to an OD between 0.3 and 0.5. These two values define the higher range of satellite OD retrieved values that Minnis et al. assume in order to compensate for the narrow or optically thin contrails and non-linear contrail cirrus not accurately detected in satellite retrievals. We wanted to test this approach with Schiller et al.'s IWC climatology, but we agree with the referee's opinion that this would require an explicit analysis of the statistical differences between in situ and remote sensing IWC retrievals and it will therefore be excluded from our manuscript.

Comment: Table 1 For comparison to other results, the normalized forcing should also be reported because it hard to distinguish between contrail coverage and optical depth contributions. The caption in Table 1 should note the mean contrail cover.

Reply: We can definitely include them. And the caption in Table 1 does include the mean contrail cover.

Minor comments

Figure 5. The plot shows results for NH only but the numbers used in the discussion are global. This is confusing. Use either global or NH.

Reply: We will correct this and use global averages throughout the manuscript.

Anonymous Referee #1

Review of “Sensitivity of radiative properties of persistent contrails to the ice water path” by R. Rodriguez de Leon et al.

This paper forms a sensitivity study of contrail radiative forcing (RF) with respect to contrail optical properties (ice water content, geometrical thickness, effective ice crystal size, ambient temperature). Of all contrail RF studies using radiative transfer models with prescribed input it is the first that does not rely on globally uniform input parameters (according to my knowledge). Variability ranges specified for the input parameters are – in some cases – interdependent. A central assumption is that the microphysical properties of (aged) contrails agree, under equivalent ambient conditions, with those of natural cirrus clouds, for which much more measurement data are available. Hence, use can be made of the Schiller et al. (2008) data, far more extensive than the contrail observations compiled by Schumann (2002) or even those most recently reported by Voigt et al.(2011). The main result of the present paper is a considerably enhanced range of possible global radiative forcing values for line-shaped contrails, compared with recent assessments (IPCC, 2007; Lee et al., 2009), with the upper limit about twice as large as given by Lee et al. (2009).

I have no doubt about the correctness of the radiative transfer calculations that have been performed for this study. However, considering the input specified for these calculations, serious problems arise conceptionally as well as with respect to the interpretation of the results. I feel that the paper contains many open and hidden inconsistencies only some of which are addressed by the authors. This implies a high danger

of misleading the reader. I fear that the paper, as it stands, is confusing the issue rather than advancing the scientific knowledge on the subject.
I think the paper should me made acceptable due to the methodical progress it forms beyond existing radiative transfer studies. It can be made acceptable by critically reflecting the bold assumptions leading to the enhanced uncertainty range for global contrail RF, which I think are not justified. This holds mostly for the upper limit of the range, but to a less extent for the lower limit, too. Or, alternatively, the respective assumptions may be explained in a more convincing way. I would also feel more comfortable, if calculations for April and October could be added to form a sounder estimate for annual mean. Generally I recommend revision of the whole text for a precise presentation along the line of my minor comments.

A) Main concerns

• Comment: While the description on pages 19930-19932 is not completely clear to me, my impression is that no attempt is made to remove total water path (ISCCP) or ice water path values that are contaminated by convective events. I say contaminated because contrails may have a similar IWP as thin natural cirrus, but certainly not similar to cirrus related to convection. I fail to gather from the text, whether or how the considerable effort made in Schiller et al. (2008) to separate convective from non-convective cirrus (removing part of the measurement data; relying on a fit for the median rather than the mean IWPs for the fit) has found its way into the present paper. Consequently, I suspect that the IWP values assumed for a given temperature in the present paper are high biased. Or do I misunderstand all this and are the ISCCP data only used for background clouds rather than for the contrails?

Reply: The ISCCP data is not used for contrails, only for background clouds. This point has already been addressed by Referee #2 and has been answered. We will try to make it clearer in the revised manuscript that the contrail IWC assumptions only rely on Schiller et al's fits and not on ISCCP retrievals.

• Even more crucial for the basic assumption of the paper is the way in which the authors argue to create their maximum estimate of IWC and contrails optical depth (p. 19938, l. 12). I see no reason at all for assuming a maximum optical depth of 2.02 to create a maximum estimate of contrail radiative forcing, if the probability of this value according to existing contrail analyses from satellite observations is negligible (Palikonda et al., 2005).

Reply: The optical depth is a consequence, not an assumption, of the range of IWCs observed in natural cirrus. We are aware of Palikonda et al.'s optical depth probability functions, but one of the aims of our paper is to produce a variability estimate that is independent of remote sensing measurements. The minimum and maximum IWC assumptions will now be replaced by a standard deviation approach.

• As for the IWC sensitivity, I appreciate the choice of the variability limitation by removing the highest and lowest 5% quantiles from the assessment of upper and lower limits. Even so, as the large contrail IWC variability for a given temperature is created by fluctuation of independent variables (e.g., initial ice supersaturation, vertical motion, wind shear) it is questionable to assume, for the assessment of a global maximum/minimum range, the same maximum or minimum IWC everywhere. As this extreme view is combined with equally uniform assumptions on maximum and minimum geometrical thickness, I feel that the resulting upper and lower boundary thresholds for contrail RF form substantial exaggerations.

Reply: The IWC range reduction to one standard deviation will provide a more transparent interpretation the IWC's variability, unfortunately a similar approach with the physical depth, we believe, would complicate the analysis more than the fixed depth option. The aim of our paper is to provide a link between possible physical configurations and their resulting radiative impacts. Given the lack of data, it would be very difficult to include in our study a realistic variability of contrail's geometrical depth based either on physical principles or on available measurements.

• The present paper uses January and July calculations to create an annual mean by arithmetic averaging (p. 19936, l. 9). This almost certainly introduces a low bias and forms an awkward simplification when comparing with annual means from other studies, as contrail coverage tends to be larger during intermediate seasons than during solstice seasons (e.g., Ponater et al., 2002; Palikonda et al., 2005; Stuber and Forster, 2007; Rap et al., 2010).

Reply: Unfortunately only the January and July contrail coverage data from the TRADEOFF project was made available to us. We would like to add here, that the main aim of our paper is not to produce a best estimate of contrail RF but to assess the dependence of the RF's variability on the ice water path. Therefore we believe that showing the variability of IWP and RF for the months with the largest and smallest contrail coverage found in the TRADEOFF project is more important than improving the accuracy of the annual average.

• While Figures 5 and 6 are formally correct in itself, neither provides a reasonable impression on how contrail RF is expected to change by flight altitude shifting: Not Figure 5, because the dependence of cloud coverage on flight altitude is neglected, nor Figure 6 as giving a latitude dependence of RF weighted by contrail coverage masks the effect of the strong latitudinal dependence of coverage. The authors may reconsider their presentation by reflecting what additional information they like to offer beyond the much more lucid illustration by Fichter et al. (2005) of the same topic.

Reply: Our paper focuses only with the radiative aspect of contrail impacts and not on their coverage. We plan, in the future, to perform a similar sensitivity study on contrail coverage, but it is out of the scope of the present paper.

B) Minor remarks
1. p. 19928, l. 4: "… based on a correlation with ambient temperature derived from in situ observations …"

Reply: We will include this change.

2. p. 19928, l. 15: I don't think that the 0.08 and 0.32 optical depth values should be referred to as "measured in situ", they are rather parameterized from a correlation based on in-situ measurements of IWC and temperature. I have already expressed my doubts for the 0.51 and 2.02 OD values being referred to as "satellite retrieved".

Reply: We will avoid these references in the new version of the manuscript.

3. p. 19929, l. 14: I think you may stress that the equivalence of contrail and natural cirrus IWC is holding for cirrus in non-convective areas (see above).

Reply: We will definitely include this observation.

4. p. 19930, l. 15: Is this shape configuration similar to any of those recently tested by Markowicz and Witek (2011)? Does the choice imply the possibility of a bias?

Reply: The SW RF is largely dependent on the assumed ice crystal habit. It is therefore extremely important that the crystal shape and the assumed aspect ratio match not only the volume but also the projected-area of the PSD parameterization. In our case, out of the available crystal habits reported in Yang and in Baran's data bases, only hexagonal cylinders reproduced the measured volume and projected-area data. The other crystal habits would produce physically inconsistent results and were, therefore, not used.

5. p. 19931, l. 5: "… Krämer et al. (2009), who provide …"

Reply: We will make this change.

6. p. 19932, l. 1: I do not understand the following paragraph.

Reply: This paragraph explains that the total water content is apportioned into the liquid and solid phases using Schiller et al.'s IWC fit. We will try to rephrase the paragraph to improve its clarity.

7. p. 19932, l. 23: I do not understand why the model vertical resolution should have any implication for the specification of a horizontal cloud and contrail area.

Reply: We will expand this explanation to improve clarity. Given the impossibility in our model to define in the same cell two different IWC values (one for natural cirrus and another for contrails), we assumed the cirrus value in the cells that had both types and modified the contrail's coverage in order to emulate the contrail's contribution to the cell's ice mass according to its assumed IWC regime.

8. p. 19933, l. 7: Don't forget about the changes of thermal radiative efficiency due to seasonal changes in the temperature difference between contrail and Earth' s surface!

Reply: We will mention it.

9. p. 19933, l. 21: Rap et al. (2010) find a similar difference between all-sky and clear-sky contrail forcing as your study, in contrast to the references cited. They have used the same radiation scheme as you do, haven't they? Can an explanation be offered?

Reply: Rap et al. (2010) performed both on-line and off-line calculations, and despite using the same radiation scheme in both runs it was only in the latter that showed similar effects from background clouds to the ones found in our study. Therefore, the coincidence is not linked to the radiation scheme, but, as Rap et al. explained, to a higher correlation between contrail formation and natural clouds in their on-line calculations.

10. p. 19935, l. 20, l. 24: The choices of 200 m and 1000 m as minimum and maximum geometrical contrail thickness sounds somewhat arbitrarily when reading the text; however, in view of the statistical analysis of supersaturated layers (Spichtinger et al., 2003). resulting in a mean depth of about 500 m, the choice seems to be acceptable.

No reply needed.

11. p. 19937, l. 2: Considering that you are using a contrail distribution produced by a model including many very thin contrails (Ponater et al., 2002; Fichter et al., 2005), it might be sensible to include into your discussion the recent work of Kärcher et al. (2010), who go to some length in determining a representative value of mean and median contrail optical depth including contrails not detectable by satellites.

Reply: We mention in our manuscript Kärcher et al.'s (2009) estimate of undetected contrails, and we will include their new article in our discussion.

12. p. 19937, l. 15: I am amazed how the authors interpret the Palikonda et al. (2005) results, whose Figure 8 indicates a large variability of contrail optical depth at least between 0.05 and 0.6 (forgetting for the moment for the even wider probability distributions in Marquart et al., 2003 and Kärcher et al., 2010). To support their 0.29 to 0.34 variability range the authors evidently use the seasonal variation of Palikonda et al's median optical thickness. To me this is comparing apples and oranges.

Reply: We will try to make it more clear that in the mentioned paragraph it is actually the seasonal variability what is compared, like with like, as the 0.29 to 0.34 OD variability is the seasonal variation of the mean optical thickness that our model produced.

13. p. 19937, l. 22: Again I suspect that the authors are confusing optical depth variability arising von fluctuating temperatures and IWCs with its seasonal variability.

Reply: We will rephrase the sentence and write "our results" instead of "values presented here" and we will try to make it clearer that we are actually comparing the seasonal variability reported by

Ponater et al. with the seasonal variability in our model.

14. p. 19937, l. 23: typo “broader”

Reply: Will be fixed.

15. p. 19938, l. 12. Apart from the fact that I would like to have explained how this new fit is constructed, the general intention seems dubious to me. Why should an optical depth range of between 0.51 and 2.02 be “more consistent with satellite measurements”, if these apparently indicate a variability range between 0.05 and 0.6 (see above)?

Reply: We will exclude the conclusions drawn from the ISCCP and the Schiller et al. data comparison.

16. p. 19938, l. 21: In Ponater et al. (2002) this is the most extreme value taken from a 10 year simulation, while also the Atlas and Wang retrieval is most probably an extreme case (as the authors of the present paper themselves acknowledge by making 1 km geometrical depth their upper limit). As I have stated it is questionable to use these values for creating an upper limit for a global radiative forcing estimate.

Reply: The reduction in the assumed IWC variability will address this concern.

17. p. 19939, l. 10: Here, and at some corresponding locations in the text before, I am wondering whether or where the present paper is discussion global optical depth means and global radiative forcing or Northern Hemisphere averages. Table 1 is indicating that these are global averages.

Reply: We will try to use global values throughout the paper.

18. p. 19939, l. 13: typo “between”

Reply: Will be fixed.

19. p. 19939, l. 15: So what? Why should IPCC’s best estimate fall into a range of maximum estimates? I think you ought to compare your own mean (or best estimate) with the IPCC best estimate.

Reply: We will avoid proffering the maximum IWC fit in the new manuscript.

20. p. 19940, l. 14ff.: The meaning of the sentence is not quite clear. If it implies that satellite and in-situ measurements of the same contrail scene are (unfortunately) missing, I do agree.

Reply: This sentence also referred to the comparison of ISCCP and Schiller et al. data, which will be excluded in the revised manuscript.

21. p. 19940, l. 20,21: I do not think that the discussed effect has be adequately separated from the temperature dependence of thermal emission in this paper to draw such a conclusion.

Reply: This is a fair point, we will rephrase this sentence to include only the dependence of the RF on the IWC and the Dge.

C) References (only if absent from the paper manuscript)

• Kärcher, B., et al., 2010, Importance of representing optical depth variability for estimates of global line-shaped contrails. PNAS, 107, 19181-19184.
• Markowicz, K. and Witek, M.,2011: Simulations of contrail optical properties and radiative forcing for various crystal shapes. J. Appl. Met. Clim., 50, 1740-1755.
• Rap, A., et al., 2010: Parameterisation of contrails in the UK Met Office climate model. J. Geophys. Res., 115, D10205.
• Schumann, U., 2002: Contrail Cirrus. In D. Lynch et al. (eds.), Cirrus, Oxford University Press, New York, U.S., 231-255.
• Spichtinger, P., et al., 2003, Ice supersaturation in the tropopause region over Lindenberg, Germany, Meteorol. Z., 143-156.

• Voigt, C. et al., 2011: Extinction and optical depth of contrails. Geophys. Res. Lett., 38, L11806.