

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

Anonymous Referee #2

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

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

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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.

Major comments 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 funda-

mentally 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.

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. 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.

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.

Table 1 For comparison to other results, the normalized forcing should also be reported

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because it hard to distinguish between contrail coverage and optical depth contributions. The caption in Table 1 should note 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.

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