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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Northern Hemisphere Contrail Properties Derived from Terra and Aqua MODIS Data for 2006 and 2012" *by* David P. Duda et al.

Anonymous Referee #1

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The paper addresses important objectives: Northern Hemisphere (NH) contrail properties, in terms of coverage, optical depth, particles sizes and radiative forcing, and their changes from 2006 to 2012. It uses valuable data at high level of remote sensing expertise: multispectral MODIS data with high spatial and some temporal resolution from two polar orbiting satellites (AQUA and TERRA), from 2 years. It uses an established algorithm which has been shown to be able to detect linear contrails, at least over quasi-homogeneous surfaces (such as the oceans) and for weak traffic where overlap from various contrails and overlap with other clouds is less important.

The method was known to suffer from spatially variable detection efficiencies and from

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possibly large false detection rates from misinterpretation of linear structures in natural cirrus. The overpass times of the satellites changed somewhat between the years 2006 and 2012. The changes may have some impact on the results in particular in regions with strong diurnal traffic cycles, such as over the North Atlantic. For correction, meteorological data and traffic data are used, which unfortunately are different in several respect and it is not clear whether the quality of the data over the two observation periods is sufficient to allow for an unbiased comparison of the results from the two one-year periods.

The paper comes to important conclusions: most contrails are 2 h old when detected by the satellites. That conclusion is reasonable and consistent with a few other studies (not only form their own team).

Further the paper suggests that the NH contrail coverage increased from 0.136 % to 0.140 % in coverage or by 3 % in relative terms. Unfortunately, error estimates on these results are missing and difficult. One cannot be sure about the significance of the small changes because that would require an overall accuracy better than 3 %. So, these data should be presented together with error estimates, which may be large. At present the abstract presents the coverage results as if they were accurate to 3 digits. That needs to be changed.

Figure 1 shows the derived annual mean global distribution of detected contrail coverage. The result suggests a strong contrail maximum over the North Atlantic. The result of Figure 1 may be technically correct but the overall result does not look plausible. It contradicts many other studies in terms of the spatial distribution of contrail coverage. See all the global model studies on contrails that have been published so far since 1998 (see reviews in IPCC 1999, 2013, etc.). All of them show contrail maxima over the continents, not over the North Atlantic.

The authors discuss traffic and potential contrail coverage computed for the given traffic using numerical weather prediction data but do not show a NH map of the absolute

values of potential contrail coverage and the product of the potential coverage with traffic for comparisons. Figure 2 only shows differences in these parameters between the two annual periods. I strongly suggest to add a plot of the expected coverage and to point out that Figure 1 suffers from the spatially variable detection efficiency.

I suggest that the paper presents a table for the nine air traffic regions identified in Figure 5, comparing the observed contrail properties (coverage, RF, etc.) with computed or model-estimated contrail properties.

The authors cite Meyer et al. (2002, JGR, doi: 10.1029/2001jd000426) but the list of references misses this paper. Another paper, Meyer et al (2007, Int. J. Rem. Sens., doi: 10.1080/01431160600641707) also discussed contrail coverage, and their Table 1 shows what I was looking for: a comparison between observed and computed contrail coverage over various regions of the world. Of course, nowadays such a comparison can be made far better than >10 years ago, and other model results became available in the literature.

The discussion o altitude changes is not convincing. There is no reasonable and testable argument given for why the mean cruise altitude of air traffic should have been increasing by 0.26 km or 0.79 km over the NH or over the Pacific during the just <6 years since 2006, except that two data sets of different origin indicate this. I suggest skipping this discussion and the related Table 2.

The values given for global radiative forcing do not yet contain error bounds for possibly underestimated contrail coverage over the continents. I suggest that the authors estimate possible underestimates over the continents (e.g., from the mentioned comparison to model data) and use such estimates to derive an upper bound on contrail coverage or RF from their data.

The discussion of “interannual” changes should be reduced. There is no significance in the detected “interannual variability” if only 2 years are considered. The best one could do is to report differences between the two years. So instead of saying the cover

changed from 2006 to 2012, they should say the data from 2012 and 2006 show differences, but should add that the differences can have many reasons, including true contrail changes, humidity changes, traffic changes, changes in the observation method, etc.

I encourage the authors to carefully revise the paper and to publish the facts and the data sets, with proper comparisons to model results, more restrictive conclusions, and self-critical discussion.

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