

Interactive comment on “Global contrail radiative forcing and the impact of diurnal variations of air traffic” by N. Stuber and P. Forster

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We thank reviewer #3 for their very helpful comments which have helped to greatly improve the paper. We would like to reply as follows:

Reviewer #3's general comment: Assuming a fixed optical depth is not appropriate for calculating another "best estimate" of contrail radiative forcing.

This was their major criticism of the paper. We feel it is somewhat unjustified, as

a) previous work has adopted the same approach. Sausen et al. (2005) obtained their best estimate of contrail radiative forcing in 2000 using results from Marquart et al. (2003) and Myhre and Stordal (2001; MS2001), scaled for the year 2000. The MS2001 study used a fixed contrail optical depth of 0.3.

Further,

b) at the time the ECMWF data did not carry ice supersaturation. Therefore, using a variable optical depth by adopting their suggested methodology would have created extra uncertainty, without necessarily improving the quality of the contrail radiative forcing estimate. However, the ECMWF model has recently been improved to carry ice supersaturation, and adopting their methodology in the future is a great idea.

c) In a sensitivity test we explore the effect of optical thickness on RF.

Finally,

d) our study may not vary optical depth, but has other advantageous features which are likely to be equally important for providing a "best estimate" of contrail radiative forcing. While Marquart et al. (2003) used their GCM to determine (a variable) contrail optical depth, they had to use an a posteriori 25% offset in the longwave, to (approximately) account for the effects of longwave scattering, which are not included in the GCM's radiation scheme. We used a state of the art radiative transfer model, and we further fully accounted for the diurnal cycle of air traffic/contrail cover in our calculations.

We have added text to explain our justification for using a (fixed) contrail optical depth.

Major comments

1. See above.

2. We agree and have revised the text. We now explicitly cite two studies to motivate this choice, instead of simply referring to Stuber et al. (2006). However, in the end the choice of both contrail width and contrail lifetime does not matter greatly. Both parameters affect the contrail cover. As we later scale the contrail cover to achieve agreement with the Bakan et al. observed value, the original choice of these parameters is less relevant. Text has been added to explain this.

3. See above

4. First of all we would like to point out that we only scale with contrail cover and

fuel usage. We do not scale with contrail optical depth. We feel that the comparison with results from previous studies is an important part of the paper's conclusions. No other obvious way presented itself for comparing radiative forcing values from what are often very different approaches. These comparisons are only meant to be approximate and the referee is correct in pointing out that the scaling methodology does not work perfectly. The text has been modified to acknowledge the caveats of this scaling and to explain the reasoning.

Minor comments

1. We now discuss this point in the paper.
2. Ideally we would have liked to scale with the monthly air traffic at specific heights, rather than the total column amount. Unfortunately this was not possible: in a couple of layers which do have air traffic in, say, July, there is no air traffic in June. We have added text to explain this.
3. Thank you for spotting this! The RF values for MS2001 and Marquart et al. (2003) in Table 6 are indeed the wrong way round.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9123, 2006.

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