**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

Interactive comment on "Relating observations of contrail persistence to numerical weather analysis output" *by* D. P. Duda et al.

D. P. Duda et al.

Response to comments by anonymous referee 1

Following the suggestion of the reviewer, the last sentence in the abstract was removed.

I am surprised that the occurrence of flight is not accounted for in the contrail occurrence prediction. Do the authors assume that, because the locations are highly populated, flight occurrence is not a limiting factor ? If so, it should be more clearly stated.

Part of the reason that Figure 1 was presented was to demonstrate that (with the possible exception of one of the GLOBE schools) the observations were taken in regions with substantial air traffic. This point is stated explicitly on page 18389, lines 23 through

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25.

Towards the end of section 3, it is said "As expected, the cirrus occurrence forecasts were much better than the contrail occurrence". Why "as expected"

P. 18396, L. 6: The cirrus occurrence forecasts were expected to be better than the contrail occurrence forecasts based on an inspection of Figure 2, which shows that the cirrus observations in Fig. 2b are more closely related to RHI than the contrail observations in Fig. 2a. The sentence has been modified to read:

"As might be expected based on an inspection of Fig. 2,..."

I am very surprised by the very last sentence of section 3. "The higher cirrus skill scores confirm that the model analyses do much better representing moisture where cirrus appears than where persistent contrails appear". I do not see the logic here. There may be other hypothesis than the humidity representation to explain a lack of skill in predicting contrails.

P. 18396, L. 14-16 (last sentence of section 3): The authors are not arguing that the humidity representation is the only reason for the lack of skill in predicting contrails. Some possible factors for the lower skill in contrail prediction are mentioned in Section 4. The last sentence has been modified to read: "The higher cirrus skill scores suggest that the model analyses do a better job representing the areas of UTH associated with cirrus than regions where persistent contrails appear. This may be related to the difficulty of the RUC and ARPS to assimilate contrail observations in their analyses."

Figure 1: The appropriate units for cumulative flight length were added to the caption.

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Major points

Abstract, Table 3: The authors agree with the reviewer's point that the differences in the lapse rate values between contrail and non-contrail observations are not statistically

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significant, and the manuscript has been modified to reflect this change.

Table 4: The suggested Monte Carlo exercises were completed to determine the significance of the HSS in the contrail cases. One thousand series of random contrail predictions for each satellite grid box were created. The random contrail predictions were determined using uniformly distributed random numbers and the mean contrail occurrence within the grid boxes (approximately 0.46). The HSS of the random forecasts were computed for each series, and the distribution of HSS for the 1000 runs were plotted to determine the variability of the HSS. The variability of HSS in the random forecasts is a function of the total number of contrail forecasts (i.e., grid boxes). For the RUC and ARPS analyses (5401 grid boxes), the HSS was no more than +0.050 and no less than -0.043. The ARPS 3-day forecasts had the smallest number of available grid boxes (2385), so that the range in HSS for the random 3-day forecasts ranged from +0.067 to -0.053. We expect that the HSS in all of the contrail cases (except perhaps the 3-day forecasts) are beyond at least three times the standard deviation of the HSS in the random forecasts, and thus are statistically significant. The result of this Monte Carlo exercise has been added to the text.

Minor points

Sec. 2.1: *Are contrails only counted when the producing aircraft has been seen, or are contrails advected into the scene counted as well? Is contrail advection accounted for at all? Is double counting possible as a consequence of advection?*

The students were only instructed to count the contrails that they could see in the sky at the time of the observation. It was not necessary to see the producing aircraft to count a contrail. We expect that any influences of contrail advection on the observations would be most likely for the spreading persistent contrail clouds, because they are most likely to be the longest-lived of the contrails and subject to the most advection. A related issue (that we can't answer completely) is what the students considered to be contrails. We expect from the contrail protocol that the students would look for line-shaped cirrus

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clouds, but from personal observing experience (DPD) it often quickly becomes difficult to distinguish diffuse contrails from other cirrus clouds without other sources of data. A cautious observer would probably only report contrails without much spreading and thus the consequences of advection would be minor. We hope the students were cautious :-).

P. 18393, L. 7: The suggested word change was made.

P. 18393, L. 18: *I wonder how the RHi in the upper troposphere is related to middle and lower troposphere cloudiness. Many of the overcast conditions should be related to other than high level clouds.*

The correlation between overcast skies and large RHImax values does suggest that the upper tropospheric moisture may be at least partly correlated to middle and lower tropospheric cloudiness. Overcast conditions are often the result of extratropical storm systems that would tend to have higher RHI throughout the troposphere. The intent of the sentence is merely to point out that in general cloudiness is correlated with high RHI.

P. 18393, L. 21: *"usual partly cloudy conditions" is a bit ugly. Are there also unusual partly cloudy conditions?*

The authors meant here that the partly cloudy conditions were "unusual" when persistent contrails were observed. The text has been modified to eliminate the confusing word.

P. 18394, L. 4: "Ice supersaturation" has been replaced with "RHI".

P. 18394, L. 22ff: The authors agree that that lapse rate differences are not statistically significant. The text has been modified to indicate this point.

P. 18396, L. 10ff: *"some dry bias..." This statement is wrong. Usually, an overcast grid box is diagnosed when RHi reaches 100%. The sentence in the manuscript turns this relation upside-down like...*

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The text was not clear. The authors are not suggesting here that RHI is diagnosed from overcast conditions in the grid box, but that 100% RHI rarely appears in areas where contrails are observed (from the surface) because those areas are rarely overcast (both in reality and within the model). The text has been modified to more clearly state our point.

P. 18396, L. 22ff: The authors agree the differences in RHI between the spreading and non-spreading persistent contrails are too small in these models to be statistically significant. The requested text was removed.

P. 18397, L. 7-9: *"Another result...". This sentence should be deleted. It is not necessary to make cirrus predictions in this way since the NWP models usually predict cirrus.*

Although cirrus cover does not have to be diagnosed using RHI fields, we want to point out to the reader that current NWP models appear to do a good job at assimilating cirrus cover within their analyses, as analyzed RHI correlates well with satellite observations of cirrus. The text has been modified to read: "Another result of this study is that the upper tropospheric RHI in the RUC/ARPS analyses correlates well with satellite observations of cirrus (probably due to the assimilation of cirrus coverage in the models), but the prognosis of contrails..."

P. 18397, L. 13: *Of course, contrail prediction needs air traffic information in advance as well. However, the US sky is usually a region of very intense air traffic. Is lack of air traffic really a possibility?*

There are some regions of the country where air traffic is probably low, as indicated by Fig. 1. The authors do not expect a lack of air traffic to be a common reason for the lack of persistent contrail formation over most of the continental US, but it is a possibility, especially if the layer of supersaturation in a region were thin.

P. 18397, L. 16: The suggested word change was included.

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Reference Travis et al.: The date of publication was added.

Tables 2 and 3: The lines associated with contrails are now indicated in bold print.

Response to comments by B. Kärcher

M. Kärcher's comments about alternative methods to simulate contrail coverage are pertinent to this article. The authors have included discussion in Section 4 about the explicit use of ice supersaturation in the ECMWF IFS model, and the parameterization of Burkhardt et al. to represent contrail coverage in an NWP model that has no ice supersaturation.

The authors agree that ice supersaturation in the RUC and ARPS is probably the incidental result of numerical issues. This point is now mentioned in the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18385, 2008.