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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General comments

The authors compare visual contrail observations from ground observers to the then pertaining atmospheric conditions as given by two weather forecast models. They investigate the use of relative humidity over ice (RHi), vertical wind speed, and atmospheric stability as predictors for occurrence of persistent contrails. Unsurprisingly, it turns out that RHi is the most significant predictor for contrails, and that upward air flow seems to support contrail persistence. On the other hand, atmospheric stability seems to have no significant influence on contrail persistence. At least the numbers in table 3 tell me that the differences are probably insignificant. Unfortunately, the two forecast models do not represent ice supersaturation (neither the analysis nor the forecast modes). This renders RHi in these models a weak contrail predictor, hence contrail

prediction from these two models seems to be a bit unreliable. Anyway, I think it is important to show this, namely that without an ice supersaturation feature in the model it will be pretty difficult to forecast contrail persistence. The paper is suitable for ACP and should be published after a thorough revision, taking into account the following points.

Major points

Abstract, table 3, etc.: The authors should either proof the statistical significance of the differences in the lapse rate values of the various contrail classes, or say that forecast or analysed atmospheric stability has no significant effect on contrail occurrence.

Table 4: Obviously, one would hardly take these two models to predict contrail persistence, in particular when one is interested in contrails occurring in an otherwise cirrus free environment, because these are probably those that have the largest climate impact. Although the hit rates are not bad, it might be only so because when contrail is predicted, cirrus is predicted as well in most cases.
The significance of the HSS in the contrails cases should be demonstrated. As there seems to be no theory of the probability distribution of HSS for the null hypothesis "forecast is equivalent to a random forecast", you have to find out the distribution function with a simple Monte Carlo exercise. That is, run 1000 series of each 366 random contrail predictions (using uniformly distributed random numbers) and store the corresponding 1000 HSS values. Then you can show how far in the wing of the HSS distribution your actual HSS values are.

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?

Pg. 18393, l. 7: shouldn't this read "period of highest contrail occurrence"?

Pg. 18393, l. 18: I wonder how the RHi in the upper troposphere is related to middle

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and lower troposphere cloudiness. Many of the overcast conditions should be related to other than high level clouds.

Pg. 18393, l. 21: “usual partly cloudy conditions” is a bit ugly. Are there also unusual partly cloudy conditions?

Pg. 18394, l. 4: “ice supersaturation” should be replaced by relative humidity with respect to ice or sentence should read “ice supersaturation of mare than a few percent are not allowed”.

Pg. 18394, l. 22 ff: “Contrails tend to be ...”. While this may be so, from your results you can hardly conclude it. The differences seem to be insignificant, see above.

Pg. 18396, l. 10 ff: “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: “100% is diagnosed when the grid-box is overcast”. Actually, the dry bias is more probably a consequence of not allowing supersaturation in the upper troposphere.

Pg. 18396, l. 10 ff: The difference of RHi between the cases with spreading and non-spreading persistent contrails is probably insignificant. Please either demonstrate the significance or change the statement.

Pg. 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.

Pg. 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?

Pg. 18397, l. 16: “analyses” should be replaced by “assimilation”.

Reference Travis et al.: Publication year missing.

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Tables 2 and 3: I suggest to underline or otherwise emphasize the lines that refer to contrails. On first sight I took the whole table as referring to the cloudiness degrees only and hence missed the most important information in the tables.

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Interactive Comment