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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A meteorological overview of the MILAGRO field campaigns" *by* J. D. Fast et al.

Anonymous Referee #2

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General Comments

This paper provides a detailed summary of the meteorological conditions during the MILAGRO field campaign that took place last March. The paper is rather long and one might question whether all of the details given are really necessary or useful. Nevertheless, the paper as a whole will be a valuable resource for those conducting the numerous data analyses that can be expected from the experiment. The material is presented clearly and the analyses of the synoptic conditions should provide a good perspective for interpreting the chemistry data that were collected. Subject to fixing a few technical issues described below in the Technical Corrections section of this review, this paper can be accepted as is. However, I suggest that the authors also consider the changes described in the Specific Comments section as possible modifications that

might be helpful.

Specific Comments

1) The reference to Bossert's 1997 paper almost sounds as if he had no data to support his modeling results and this seems a bit harsh. The data available to him were certainly not as extensive as for this study but his Figure 5 does show some measurements.

2) The last sentence of the next-to-the-last paragraph on p. 2043 reads "There was no preferred direction for the remaining time periods." Does that refer to other months of the year besides March or does it mean that in March there are no preferred directions except for the 20-30% of the time when there is northeastward flow?

3) On p. 2048 there is a discussion about comparisons of rawinsonde measurements with GFS model results and some differences are noted, especially at the 700 hPa level. The authors comment that one of the reasons for the additional rawinsonde flights was to improve the GFS analyses so it is a little disappointing that these differences remain. Can any more light be shed on why these discrepancies remain? I'm certainly not expecting any new simulations to be done but some additional information or insight, if available, might be useful.

4) The first and second full paragraphs on p. 2050 might be left out without compromising the paper. The same is true for Figures 5 and 6. There's just a lot of detail on the Norte events that don't seem that important, so if the authors are looking for places where the manuscript might be shortened, this is one possibility.

5) There is a brief discussion of the K index on pp. 2053-2054. I don't have a problem with the K index being used as a general indicator of conditions of convective instability, but I think the specific probabilities of developing air mass thunderstorms are open to question. Different studies have found different probabilities but this is not acknowledged in the text. I think one should omit saying anything about the specific values and

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simply note that higher values of K indicate higher probabilities.

6) The sentence bridging pp. 2056-2057 is confusing. The sentence seems to say that the third Norte produced lower potential temperatures and higher specific humidities after the second Norte. Was that really the intention?

7) On p. 2058 the authors note that the radar wind profiler measurements show periods favorable for transport of pollutants that differ somewhat from those derived from the rawinsondes. The explanation is fine but it might then be better to leave out the shading back in Figure 3 that indicates transport periods deduced from the rawinsonde data.

8) On p. 2060 a paragraph begins with "A common feature among the six simulations was the existence of two branchesĚ", but we are then told that this feature was not actually present for one of the simulations (Figure 17e).

9) I'm not sure that the details of the simulations of CO transport shown in Figure 18 and discussed on pp. 2060-2061 add much to the paper. Some of the material seems to repeat information already presented on p. 2043 regarding the prevalence of winds from the southwest. I think Table 3 says most of what needs to be said here, and leaving out Figure 18 and shortening the discussion of the material presented on these pages by a paragraph or two would eliminate some redundancy.

10) Is there any significance to the red and blue coloring of the flight paths in Figure 1c?

11) It would be helpful to indicate the position of Mexico City on the maps in Figures 12 and 13. In figure 12 it would also be helpful to use a light grey background for the legend so that the yellow text shows up better.

Technical Corrections

1) On p. 2046 there is a sentence "Synoptic-scale predictionsĚwere made by the global version of FLEXPARTĚand the MOZARĚ, RAQMSĚ, and GEOS-CHEM modelsĚ, respectively." Why is the word "respectively" used here? It is normally used when one is

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doing something like providing two lists and linking items in the second list to items in the first.

2) I found some of the figures quite difficult to read, especially Figures 2, 6, and 17. Is it possible for the authors (or the journal) to expand these for greater legibility?

3) On p. 2060 there seem to be some typos regarding dates. In Figure 17e, March 2005 appears to be the anomalous case with no transport toward the Pacific Ocean, yet the text say "Transport towards the southwest did not occur at all at this altitude during 2004Ĕ; thus March 2004 was most unlike the other periods." Should those dates be 2005?

4) Near the bottom of the same page, there is a reference to Fig. 1a for the locations of the T1, T2, and Santa Ana sites; I believe that should be Fig. 1b.

5) Figure 2 has lots of colors but the legend explaining them is all but invisible.

6) The caption for Figure 3 says that the 700 hPa level data are in the top panels and the 500 hPa level data are in the bottom, but the labels on the y-axes of those figures say the reverse is true.

7) In Figure 7, it would be helpful if different colors were used for the lines instead of the scheme chosen.

8) In Figure 11, why are there no symbols associated with some of the vertices in the K index and precipitable water traces? Shouldn't all (or none) of the vertices have a black dot?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 2037, 2007.