Atmos. Chem. Phys. Discuss., 9, S328–S333, 2009
www.atmos-chem-phys-discuss.net/9/S328/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemistry and transport of pollution over the Gulf of Mexico and the Pacific: Spring 2006 INTEX-B Campaign overview and first results" *by* H. B. Singh et al.

Anonymous Referee #1

Received and published: 21 February 2009

General comments:

This paper provides an overview of the spring 2006 INTEX-B field campaign. A well-crafted overview paper is important for such a field study, as it serves not only as a guide to the goals, operation and results of the program, but also ties the results together in a coherent manner. This paper largely achieves these objectives. The paper is well written, and follows a clear and logical thread, introducing the entire MILAGRO/INTEX-B program, describing the measurement platforms and instruments, and then focusing on the execution of the research flights with a short rationale for the flights. There is then a concise summary of the first results with a focus on the Pacific

deployment of INTEX-B.

I have two, related primary reservations about this paper. First, no historical perspective is given for the Pacific deployment of INTEX-B; an uninformed reader of this paper would receive no indication that a great deal of previous research has been carried out in this region in the spring season of the present study. Second, without this historical perspective no credit is given to related work and there is no clear indication of which first results from INTEX-B are new/original contributions. These reservations should be relatively straightforward to rectify, and I believe that that paper will be suitable for publication once these two reservations and the following specific points have been addressed.

Specific comments:

1) The abstract asserts that the second phase of INTEX-B (an approximately 4 week field study) provides "a comprehensive data set on gas and aerosol composition to test models and evaluate pathways of pollution transport and their impact on air quality and climate." I suggest that a slightly more realistic evaluation of the value of the data set be given. After all, transpacific transport is very complex, with each transported air mass exhibiting its own characteristics reflecting the specific conditions that prevailed during the transport of that particular air mass. Thus, the INTEX-B data set is not "comprehensive" in any broad sense, and will not be adequate to fully "evaluate the pathways of pollution transport and their impact on air quality and climate." However, the INTEX-B data set is certainly a very valuable addition to the existing body of data, and will accelerate and broaden the continuing evaluation that has been underway for some time.

2) The abstract further asserts that this overview provides "a context within which the present and future INTEX-B/MILAGRO publications can be understood." However, a significant shortcoming of this paper is its failure to provide an adequate context of the INTEX-B study within previous investigations of transpacific transport. This context

ACPD

9, S328–S333, 2009

should indeed be provided in this paper. I suggest inclusion of an additional section following the introduction that reviews previous field studies addressing transpacific transport, and discusses how the goals and results of INTEX-B fit within this previous work.

3) This paper is an overview paper for the ACP special section, whose major focus is the investigation of transpacific transport. The NCAR C-130 played a major role in this investigation. Hence, the C-103 instrumentation should be described in this paper, even if it will also be described in the MILAGRO special journal section.

4) The paragraph beginning on pg. 367 summarizes measurements made during the Pacific INTEX-B deployment. However, in many cases references for full descriptions of the measurements are missing, and there is no information regarding where to find the resulting data archived or discussed. This paragraph should be revised with the perspective of an interested reader kept in mind. That reader should be directed toward full descriptions of all of the relevant instrumentation and the resulting data sets.

5) Section 2.4 provides a very cursory summary of flight tracks and intercepted air masses. It would be useful to somewhat expand the discussion. A table describing the C-130 flights parallel to that of Table 5 for the DC-8 flights should be given, since these two aircraft were closely coordinated, and each contributed substantially to the INTEX-B results.

6) Section 2.5 should be more informative. The authors state: "The actual inter-comparison data and results are available at http://www-air.larc.nasa.gov/cgi-bin/ic." However, this is evidently a password-protected site, so in fact the data and results are generally not available. One measurement intercomparison of particular interest is NOy, but this is one species not specifically mentioned in this section. The DC-8 and C-130 both report measurements of NOy, but by different techniques. Each of these techniques relies on conversion of the manifold NOy species to a single species (NO_2 on the DC-8 and NO on the C-130), which is directly measured. As a consequence,

ACPD

9, S328–S333, 2009

Interactive Comment

these indirect detection schemes make the NOy measurements particularly subject to question. Is it not possible to give a synopsis of the results of this particularly important comparison in this paper?

7) Pg. 371, line 25: The authors state: "Satellite derived tropospheric columns of NO_2 indicate an increase of some 8-10% per year from 2000 to 2006." To what area does this 8-10% increase apply? The preceding sentence in the paper seems to indicate the increase is over western North America, but I suspect the authors must mean Asia. Such an increase is not realistic for North America.

8) Beginning on Pg. 372, line 9 the authors state: "Comparison with previous observations suggests that the relative fraction of PAN has increased over time while that of HNO_3 has decreased (Molina et al., 2008)." Such an important finding requires thorough documentation. The reference given is to a non-peer reviewed IGAC Newsletter article, and the figure in that article indicates that between 1994 and 2006, the fraction of NOy accounted for by HNO_3 in the central North Pacific troposphere decreased from 65% to 15%. Such an remarkable change must be thoroughly vetted in the peer-reviewed literature before it is stated as a finding in a overview article. This is particularly the case for NOy and HNO_3 measurements, which are particularly challenging to conduct from aircraft platforms (e.g., Crosley, 1994). This discussion must be removed until it is presented in a peer-reviewed publication.

As discussed in the General Comments above, no credit is given to earlier related work in this region and season, and there is no clear indication of which first results from INTEX-B are new/original contributions. Some specific (but not a complete list of) examples where this context should be clarified are listed below. The authors should provide context for all of the results they summarize.

9) Pg. 372, line 1: The authors state "Both aircraft measurements and model results showed sustained ozone production driven by PAN decomposition in the southern branch, adding to ozone produced in the Asian continental boundary layer." Further, on

ACPD

9, S328–S333, 2009

Pg. 372, line 8 the authors state: "INTEX-B data over the Pacific clearly showed that a dominant fraction of reactive nitrogen was in the form of transported PAN." These statements should be placed in the context of earlier results (e.g., Nowak et al., 2004), which found that PAN dominated in the upper troposphere, but that HNO_3 dominated in the lower troposphere where temperatures were warm enough for PAN to thermally decompose, the resulting product NO_2 to enter the photochemical ozone production cycles, and to eventually be converted to HNO_3.

10) On page 374, line 10 the authors state: "Over the Pacific, mercury was weakly correlated with anthropogenic tracers (Swartzendruber et al., 2008)." This conclusion is in direct conflict with the work of Jaffe et al. (2005), who conclude that the Hg/CO ratio in plumes from Asia provides a clear fingerprint for Asian emissions.

11) On page 374, line 12 the authors state: "A prominent feature of the INTEX-B dataset was frequent total depletion of Hg^0 in the upper troposphere when stratospherically influenced air was encountered (Talbot et al., 2007, 2008)." This is a feature that has been reported previously (e.g. Radke et al., 2007).

Technical Corrections:

1) Pg. 389, footnote b: I assume that the data referred to here are in the NASA data archive. This should be clarified, along with information regarding access to that archive. The footnotes on Tables 2b-d should be clarified similarly.

2) Figure 3 seems to have some problems. There are only 8 rather than 20 5% binned averages. The description of the figure needs clarification.

3) The source of the data in Table 8 is not clear. Are these averages of all in situ measurements made during INTEX-B? If so, then do these quantities apply just to the spring? These details should be indicated in the table.

References:

Crosley, D.R., NOy blue ribbon panel, J. Geophys. Res., 101, 2049-2052, 1996.

ACPD

9, S328–S333, 2009

Interactive Comment

Jaffe, D., et al., Export of atmospheric mercury from Asia, Atmospheric Environment, 39(17): 3029-3038, 2005.

Nowak J. B., et al., Gas-phase chemical characteristics of Asian emission plumes observed during ITCT 2K2 over the eastern North Pacific Ocean, J. Geophys. Res., 109, D23S19, doi:10.1029/2003JD004488, 2004.

Radke L. F., H. R. Friedli, B. G. Heikes, Atmospheric mercury over the NE Pacific during spring 2002: Gradients, residence time, upper troposphere lower stratosphere loss, and long-range transport, J. Geophys. Res., 112, D19305, doi:10.1029/2005JD005828, 2007.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 363, 2009.