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## ***Interactive comment on “Airborne measurements of HCl from the marine boundary layer to the lower stratosphere over the North Pacific Ocean during INTEX-B” by S. Kim et al.***

**Anonymous Referee #3**

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Review of:

MS-NR: acpd-2008-0004, &#8220;Airborne Measurements of HCl from the Marine Boundary Layer to the Lower Stratosphere over the North Pacific Ocean during INTEX-B&#8221;, by S. Kim et al., for the MILAGRO/INTEX-B 2006 special issue.

I apologize this review is arriving so late. I was only asked recently to review this paper, apparently because there had been some difficulty in obtaining a second review.

In general, I think the paper is a solid contribution to the MILAGRO/INTEX-B special issue and should be published after some modifications.

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My major concern is that the only new science in the paper is the high levels of Cl in the MBL that is associated with dechlorination of dust due to exposure to HNO<sub>3</sub> and SO<sub>2</sub>. I think there is the potential for a lot of other new science in the paper, given the importance of HCl and the limited prior observations of this species in the UT/LS region, but this science is largely unrealized. Indeed, I find the comparisons to RAQMS to be of almost no scientific use because the discrepancies are quite large, and not enough detail is given about the chemistry in RAQMS to interpret these discrepancies. I find the sentence given at the end of the first paragraph of the Summary, "these results demonstrate the ability of RAQMS to broadly predict the impact of stratospheric mixing on the UT, to be an optimistic view on the comparisons shown in the paper, which in my opinion point out either a serious deficiency in our understand of O<sub>3</sub> and/or HCl in the UT/LS region or perhaps a serious problem with the RAQMS model. Anyway, too late in the game to make major changes, but at a minimum, I suggest de-emphasizing the RAQMS comparisons in the Summary and perhaps placing slightly stronger emphasis, in the Abstract and Summary, on the high Cl layer that is associated with dechlorination.

Minor comments:

- 1) Page 3564, line 11: rather than "in previous work", suggest in "in previous work by one group". Isn't it just the work of Keene and Graedel and Keene that is different. If this is the work of a single group (I have not checked to be sure), then this should be clarified up front.
- 2) Page 3566, line 5: How about "The primary source of Cl: there are important sources of stratospheric Cl other than CFCs, such as CH<sub>3</sub>Cl.
- 3) Page 3567, lines 6 and 7. This sentence makes it appear that Marcy et al. are wrong. But, later, the paper supports the findings of Marcy et al. Perhaps add something like "We show below, however, that most of our data are consistent with the assumption of Marcy et al. and are not consistent with the observations of Graedel

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and Keene&#8221; (in other words, let the reader know what is to follow)

4) Page 3571, lines 19 to 27. This discussion is hard to follow. On lines 19 and 20, what does &#8220;both measurement model comparisons&#8221; refer to? Two comparisons in Marcy et al.? One there, one in this paper?

Figure 7b appears to show that RAQMS HCl < Measured HCl. Therefore, the sentence &#8220;may reflect discrepancies between the RAQMS ozone analysis and measured O3 rather than HCl&#8221; seems inconsistent with what is shown. Upon revision, if possible, would be great to add a panel comparing RAQMS O3 and measured O3 for the flight segments shown in Figure 7a, represent both this panel as well as 7b as a &#8220;one to one plot&#8221; (so that differences are more readily apparent), and clean up the discussion in the above lines to more closely reflect what is shown.

5) Page 3589, figure 6a. Looks like the y-axis and x-axis are mis-labeled: the y-axis looks to me to be N2O, and the x-axis looks like HCl.

Sorry to not provide a more comprehensive review. Overall, I think the paper is fine even though it seems there is some potential scientific interpretation of HCl and O3 in the UT/LS that is not done: i.e., what do these measurements say about sources of HCl in the LMS due to short lived halogens? What are the implications of the inferred strat/trop exchange for the ozone budget in the troposphere? But, again, this is a solid paper that will be a fine contribution to the special issue.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3563, 2008.

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