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Interactive Comment

Interactive comment on "Rapid convective outflow from the U.S. to the upper troposphere over the North Atlantic during the NASA INTEX-NA airborne campaign: flight 13 case study" *by* S. Y. Kim et al.

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Responding to Referee #3

p. 17370, latter part of Section 1: some references from the SONEX field experiment from 1997 should be included. Convective transport was active over the eastern US and Gulf Stream during this experiment.

We used one general reference (Fuelberg et al., JGR, 2000), and we inserted a sentence in Lines 5-8 on Page 5 (4th paragraph in section 1), "The Gulf Coast and off the East Coast of the United States were also found to be influenced by deep convection during SONEX field campaign over the Atlantic, which was conducted in October–November1997, by determining lightning activity (Fuelberg et al., 2000).“

p. 17371, line 13: is the CO lifetime really as long as 2 months in summer? I have always thought it was more like 1 month.

CO lifetime was estimated to be $\sim$1 month using [OH] =1.8 $\times 10^6$ molecules cm^{-3}, which is the summer [OH] in Mak and Southon (1998), and it was calculated to be $\sim$48 days using [OH] =1.0 $\times 10^6$ molecules cm^{-3}, which is the typical concentration in troposphere (Brasseur et al., 1999). Therefore, we changed the CO lifetime in the text to be “1 to 2 months in summer the range of OH concentration over 1.0 $\times 10^6$ - 1.8 $\times 10^6$ molecules cm^{-3} (Brasseur et al., 1999; Mak and Southon, 1998) “ in Lines 13 -15 on Page 6 (2nd paragraph in section 2.1).

p. 17373, first paragraph of Section 3: Is the text beginning with “According to the....“ relevant?

What we meant to state here was that the deep synoptic system during the flight 13 period evolved from the Canadian Low at 12 UTC on July 21. We revised the sentence in Lines 18–20 on Page 8 (1st paragraph of section 3) to make the relevance of this sentence clear now: “The circulation system that facilitated the transport pattern of Flight 13 evolved from a Canadian Low with cold and warm fronts situated north of Quebec, Canada at 12 UTC on July 21, as shown in the 6 hourly analyzed SLP“.

p. 17373, 2nd paragraph of Section 3: Is the stationary front referred to here the same front that was called a cold front in the previous paragraph? Did the cold front become stationary? If so, please say this in the text. Later in the paragraph a cold front is again mentioned. Did the stationary front evolve into a cold front as the small cyclone developed? Please clarify.

Yes, the cold front became a stationary front. To make it clearer, we inserted a sentence in Lines 3-5 on Page 9 (2nd paragraph of section 3) as follows. The cold front became a

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stationary front that was located over the eastern U.S. and the western Atlantic starting at 12 UTC on July 24 (Figs. 2a and 2b) and it persisted throughout the duration of flight 13.

A cold front, as described in 2nd paragraph of section 3, seemed to have evolved from the stationary front over the eastern U.S. However, other small cyclones were just generated over the stationary front during a very short time period.

p. 17374, line 9: There are both upward and downward motions associated with a jet stream (upward on one side and downward on the other). Please clarify.

Warm advection exists in the downwind of a tough, and cold advection exists in the upwind of a trough. So, we inserted “on downwind side of trough“ between the “jet stream“ and “is associated with“ in line 20 of page 9 (4th paragraph in section 3).

p. 17374, line 14: The paper would be enhanced if the height to which the convection over the southeastern US could be determined. Here GOES IR imagery is mentioned. Could you get cloud top temperatures from this imagery to yield some idea of how deep the convection was? This information could also be used to strengthen the trajectory analysis.

We obtained the imagery from the NOAA archive and added a reference to it in the text (line 5-6 in page 10; 5th paragraph in section 3). These indicated deep convection had occurred with cloud top temperatures of 200K, which was found to be above 14 km on the corresponding Skew-T plots.

p. 17374, line 24: The soundings on the skew-T diagrams do not verify the “occurrence“ of convection. The stability indices provide an idea of the “likelihood“ of convection. Please change “occurrence“ to “likelihood“.

We revised this statement (line 14 in page 10; 6th paragraph in section 3).

p. 17374, line 28: change “instable“ to “unstable“.

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We edited our mistaken spelling.

p. 17375, first paragraph: With regard to WCB transport, the Kiley et al. (2006, JGR) paper should be referenced. This paper details the effects of WCB transport during INTEX-NA. Can you sort out the contribution of the WCB vs. that of deep convection to the rapid upward transport in the Flight 13 case?

We inserted a sentence in the paragraph (lines 1-4 of page 11; last paragraph in section 3). The mixed effects of widespread convection and the WCB over the southeastern U.S. overlapped in their occurrence during July 25–28, 2004, and were also described in Kiley et al. (2006) and Cooper et al. (2006).

We can sort out the transport pathways by using a model or using some indicators, but these can't show the exact quantitative analysis. A modeling study can show a quantitative analysis of contribution between WCB and deep convection such as shown in Kiley et al. (2006). However, the estimation from model is dependent on the schemes used, so the results could be different from reality. H_2O_2/CH_3OOH or ultrafine aerosol concentration in free troposphere could indicate the convective activity. We used these in the 9th paragraph of the section 4. Photochemical aging of hydrocarbons can also indicate embedded convection associated with the WCB (Purvis et al., 2003).

p. 17375, last paragraph: Explain why CO_2 and COS have opposite trends with altitude compared with the other trace gases. I think this was mentioned later in the paper, but it should probably be placed here instead.

Atmospheric CO_2 and COS are removed by vegetation uptake (Sandoval-Soto et al., 2005). There is ample vegetation in SBL, while there are strong industrial and urban sources there. Therefore, the mixing ratios of CO_2 and COS were expectedly anti-correlated with those of CO and CH_4 in the flight regions which were affected by the SBL air masses. To rule out the effect of dilution that might have contributed to the lower levels of CO_2 and COS in the flight regions of interest, we demonstrate minimal dilution of air masses during fast transport. In section 4, we first showed the vertical distribution of principle chemical compounds and then discussed the key features of the distribution. Specifically, we identified the elevated urban/industrial tracers in the upper troposphere, illustrated the effect of convection and WCB on the each region, demonstrated the minimal dilution of air masses during fast transport, investigated the possibility of the SBL as the very source to the upper troposphere over the Atlantic, looked into wider regions in the upper troposphere, and finally eliminate the influence of Asian pollution. Logically such layout of our interpretation serves the analysis well, and thus we believe that the arrangement of the material is fine as is.

p. 17377-17378; The discussion of the time since convection from the trajectories and from the chemical aging should be made more precise. For region 1 the trajectories indicate 20 hours and the aging suggests 36 hours (1.5 days). For region 2 the trajectories say 1 day, but the aging says 2 days. For region 3 the aging suggests 1.7 days, but the results from the trajectories are not clearly given. Please clarify. At line 21 of p. 17378, the text says that photochemical aging gives transport times "reasonably similar" to those from the back trajectories. Is a factor of 2 reasonably similar, I guess it is better than if one method yielded 1 day and the other five days! Can any possible reasons be provided for the discrepancies?

First, the transport time for region 2 is about 1.5 days, because the air masses started to undergo fast transport over the eastern U.S. about 00 UTC on July 27 and they arrived to flight region 2 at about 13 UTC on July 28. Second, we added the sentence, “It appears that it took about 1.7 days to reach the air masses to the flight region 3.“ (lines 12-13 of page 14; 8th paragraph in section 4). From these calculations, photochemical ages are pretty similar to the transport time for regions 2 and 3. For region 1, photochemical age is different from transport time. This could be possible because we used average [OH] value in SBL to determine photochemical ages, but air masses in region 1 originally was in mid to upper troposphere over the southeastern U.S. In order to produce photochemical aging, we used [OH] and rate constant of reaction with [OH]. Rate constant is easily changed by temperature. So, approximation

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of temperature and [OH] could affect to estimation of photochemical ages. We must remember that these are all estimates, and each has uncertainties.

p. 17380, line 3: From the figure, it looks like the ozone ranges up to 120 ppb, not 110 ppbv.

This ozone mixing ratio range was picked for CO > 100 ppbv in upper troposphere about 12 km, not the entire MOZAIC data shown in figure 6. In the group of CO>100ppbv, CO ranged over 100 -139 ppbv and O_3 over 74 - 110 ppbv. In order to prevent misunderstanding, we added “in the area“ in the sentence.

p. 17380, last paragraph of Section 4: Doesn8217;t the existence of Halon 1211 at levels similar to Asian boundary layer values definitely indicate an Asian influence since there is no other source for this compound? The paragraph as it stands seems to suggest that there is conflicting evidence. Perhaps the air had Asian origins much further back in time than the 5 days transport shown on the trajectories given in the figure. Please clarify.

We mentioned “the background level of the Asian boundary layer“ not “the value of the Asian boundary layer“. The background level of the Asian boundary layer is the one with minimal influence of Asian regional emissions. However, we modified the sentence to be “background level over the western Pacific“ to avoid misunderstanding.

p. 17383, line 27: Please provide a reference for the AIRMAP data during INTEX-NA. Define TF and AI. I know they are Thompson Farm and Appledore Island, but not all readers would.

We inserted Thompson Farm and Appledore Island, but it was already explained that these were the sites in 4th paragraph of the section 2.1.

Figure 2: Need better sea-level-pressure maps! The ones here are not very readable.

We changed images. Note that analyzed sea level pressure at 00UTC July 26 was not available, thus we inserted the analyzed sea level pressure at 12 UTC July 25 because

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a stationary front existed continuously over the period.

Figure 3: The best indicator of the extent of convection is the manually-digitized radar plots. It would improve the paper if the full temporal extent of this convection was documented. Could a series of MDR plots be shown (maybe every 6 hours) from 0000 UT 26 July to 0000 UT 27 July? Does any of the convection between 0000 UT 27 July and 0000 UT 28 July also contribute to the measured outflow? If so, then these MDR maps should also be shown.

Very active convection occurred in the afternoon and the evening, indicated by MDR images at 00UTC and 18 UTC. So, we added the images at 00UTC and 18UTC for July 26–28 into Figure 4.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17367, 2007.