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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new transport mechanism of biomass burning from Indochina as identified by modeling studies" *by* C.-Y. Lin et al.

Anonymous Referee #2

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Review of ACPD 9-13155-2009

General Comments

This paper concerns the transport of biomass burning emissions from the Indochina Peninsula. Southeast Asia is a significant source of biomass burning emissions. The coincidence of biomass burning emissions with rapidly growing non-fire anthropogenic air emissions, large populations and mega-cities, rapid land use change, and continued industrialization makes the general topic of significant interest to many researchers. The specific scientific question addressed by the paper, what are the transport mechanisms for biomass burning emissions in Indochina?, is scientifically relevant and within the scope of ACP.

The authors examine a single event of an elevated ozone layer observed on April 11, 2005, over Taiwan during the Indochina biomass burning season. The authors employ a trajectory model (Hysplit) and a eulerian atmospheric chemistry transport model (WRF/Chem) running a tracer module to identify a transport mechanism linking Indochina biomass burning emissions with an enhanced ozone layer observed at 4 km agl over Taiwan. The authors propose that a trough on the leeside of the Tibetan Plateau provided dynamic forcing that lofted near surface biomass burning emissions to around 3 – 4 km agl, where the steering flow transported the emissions to Taiwan.

The authors use a 'sensitivity' study to support their claim that the leeside trough was the mechanism responsible for the elevated ozone observed over Taiwan on April 11, 2005. The 'sensitivity' study consisted of a tracer transport simulation for April 14, 2005. The authors report that on this date there was no significant trough present and that weather maps showed high pressure dominated the lower troposphere over southern China and Indochina. The authors report that the tracer simulation indicated no significant tracer peak over northern Taiwan and include the April 14, 2005, 02 Z tracer profile which shows no tracer enhancement (Fig. 5g). From this single simulation, the authors suggest that transport from near surface biomass burning in Indochina to Taiwan does not occur in the absence of a leeside trough.

The authors use 60 years of NCEP/NCAR reanalysis data to propose that the Tibetan Plateau leeside trough is a regular feature in Southeast Asia during the spring. While the authors identify the mean leeside trough, they fail to discuss the potential implications of this recurring feature. It is left to the reader to surmise that the Tibetan Plateau leeside trough is likely an important agent for transporting Indochina biomass burning emissions on a regular basis.

The transport mechanism proposed in this paper is meteorologically sound. And the authors do provide circumstantial evidence supporting the proposed transport mechanism. The tracer simulation for April 10-11, 2005 lends support to the authors' conclusion that the leeside trough provided the lifting necessary to loft biomass burning

emissions to above 3 km, where upon the prevailing winds transported the emissions over Taiwan.

The study approach applied to the April 11 event is appropriate. However, the authors do not to investigate other mechanisms that may have been responsible for the ozone peak observed over Taiwan on April 11, 2005. The authors' reliance on a single event and their failure to eliminate other mechanisms makes for an incomplete study that lends only marginal support to their main conclusion. The 'sensitivity' study, as presented by the authors, does not demonstrate that the absence of a leeside trough is responsible for the lack of tracer over the Taiwan. Details of the sensitivity study are completely insufficient (see Specific Comments).

The paper does not describe the study with sufficient detail to allow reproduction by other researchers.

Appropriate references are lacking in some sections of the paper.

The paper does not adequately discuss previous published work on this subject.

I recommend prior to publication, the authors make significant revisions to the paper.

The authors need to demonstrate that their proposed mechanism was likely responsible for elevated lower trop ozone on more than one occasion. The authors should include additional elevated ozone events in their study (see Specific Comments). This is necessary to firmly establish that the transport mechanism proposed in this paper is responsible for such episodes.

The authors need to show that under conditions favorable for biomass burning transport to northern Taiwan, sufficient vertical lofting of emissions does not occur in the absence of the leeside trough. The 'sensitivity study' (Section 5) needs to be significantly improved and perhaps redesigned (see Specific Comments).

Specific concerns, outlined below, regarding the presentation and completeness of the study must also be addressed.

Specific Comments

#1 Inclusion of additional elevated ozone events The authors citation of a paper in review, Lin et al. (2009), implies that a large dataset of springtime ozone profiles over northern Taiwan is available. If this is the case, the authors must comment on whether the 11 April 2005 event is a unique event or if it has been observed multiple times. If observations for additional events are available for the study site the authors should acknowledge these events and discuss. The authors should determine if a trough on the leeside of the Tibetan Plateau was present and can be linked to the events. This would not necessarily require full tracer study for every event. Running Hysplit backtrajectories and WRF in met only mode could provide data as in Fig 2c, 5a & 5b. The authors need not include figures for all events, but inclusion of figures for an additional event and reporting for the simulations of all events is appropriate. For example, if elevated ozone was observed over northern Taiwan on 3 occasions in 2005, do the authors find similar leeside forcing for all events? Is there evidence that the authors' proposed transport mechanism was at play in all events? If not, why? If additional elevated ozone events are not observed over northern Taiwan the authors need to state so.

If additional elevated ozone events are not observed over northern Taiwan, the authors should consider analyzing similar cases observed over Hong Kong in 2001 and reported by Chan et al., 2003b. The authors imply their transport mechanism is an important feature for transporting biomass burning emissions in SE Asia. Analyze the Hong Kong events reported in Chan et al. (2003b) as was done for the 11 April 2005 northern Taiwan event. Such an analysis would make for a more complete and convincing study. Such an analysis could provide verification (or maybe refute) the leeside trough transport mechanism proposed by the authors. It could also establish the mechanism as being important factor over the region.

At the very least, the authors need to discuss previously reported elevated ozone – biomass burning events in the region (Chan et al,., 2003a, 2003b, Liu 1999) and

comment on the likelihood their proposed transport mechanism played a role in these events.

#2 Sensitivity Study The 'sensitivity' study, as presented by the authors, does not demonstrate that the absence of a leeside trough is responsible for the lack of tracer over the Taiwan. Details of the sensitivity study are completely insufficient. The paper provides only a vertical profile of tracer at the Taiwan site at 02Z on April 14. Only a cursory description of the synopitic conditions: "According to weather maps a high pressure system dominated over southern China and Indochina at 850 hPa with no significant trough at levels 850 and 700 hPa (not shown)." Perhaps the winds above 3 km were from the north or the northwest and the airmass over Taiwan on April 14 originated in a region with minimal biomass burning / tracer source strength, e.g. central or northeastern China, Siberia. This is left completely to the reader's imagination. At a minimum the authors should provide the results of Hysplit simulations for the sensitivity study (similar to Fig. 2, but 4/10-4/14) and near surface weather charts (similar to Fig 3.).

Can the authors identify a period with trajectories similar to those in Fig 2c (i.e. pass through areas of active biomass burning at elevations of 2 – 4 km and reach northern Taiwan 2-4 days later at elevation of >2 km) but in the absence of a leeside trough and without elevated ozone over the northern Taiwan site? Inclusion of such a non-event would greatly bolster the authors' argument that lifting prompted by the leeside trough is a key mechanism for lofting emissions.

#3 Biomass burning, MODIS data, alternative lofting mechanisms

a) MODIS data The authors must describe the MODIS data they are using. Presumably the data is the daily MODIS active fire product. Is it Terra only (MOD14) or is Aqua also used (MYD14). What MODIS collection was used? Collection 4? Where was the data obtained? Was it acquired from the Land Processes Distributed Active Archive Center (LPDAAC) ? Or was it from another source, e.g. University of Maryland? The

MODIS active fire product should be identified and described (Terra only, Terra & Aqua, Collection 4 or Collection 5, etc) and referenced (Giglio et al. 2003).

The authors should use 'active fire' detections and not simply 'fires'. A single fire may produce anywhere from a few to hundreds of MODIS active fire detections depending on the fire size, intensity, and duration, land cover, atmospheric conditions, time of day, etc.

Terra predates Aqua. The temporal coverage of the Aqua MODIS active fire detections begins on July 4, 2002. Figure 1a shows active fire detections over Indochina for 2000 – 2007. Does this figure include Aqua data? It should include only Terra, or a bogus increase in fire frequency will be seen. Does this contribute to the overall increase in fire activity post-2003? I suggest the authors use only Terra data for Figure 1 and clearly describe the data.

b) Biomass burning in SE Asia In the introduction, the authors should include relevant references for remote sensing based biomass burning estimates that include SE Asia – van der Werf et al. (2006), Giglio et al., 2006a, Giglio et al., 2006b.

The authors should provide a brief description of biomass burning in Indochina during the spring and cite an appropriate reference(s). Is the burning agriculture related? Land clearing, burning of crop residues, shifting cultivation,..?

c) Lifting of emissions The authors need to address the possibility that thermal buoyancy of fire plumes is responsible for the lofting of emissions to above 3 km. With a little effort, the authors could provide estimates for the range of plume rise heights likely for biomass burning in Indochina. Survey the literature to obtain estimates for a range of fuel load, fuel consumption, and burned area of typical fires in the region. Then estimate the range of initial buoyancy flux, and estimate plume rise height using the Briggs equations (see chapter 10, Arya 1999). These would be simple, back of the envelope calculations. But they would provide an assessment as to the likelihood that emissions lofting could be achieved by plume buoyancy alone. The authors should provide such

a simple analysis.

Technical Corrections

P13156 L4: The uses of 'fires' is not strictly correct, suggest 'active fire detections'

L24-27, P13157 L1-3: Needs additional references (van der Werf et al. (2006), Giglio et al., 2006a, Giglio et al., 2006b)

P13158, L19-20: What version of WRF/Chem was used? Please specify.

P13159, L1: use 'active fire detections' or 'hot spots'

P13160, L10-13: What version of WRF/Chem was used? Did the authors develop and use their own tracer module or did they use a tracer module available with WRF/Chem? Please specify.

P131609: Please describe WRF/Chem simulation. How many vertical levels, where the vertical levels 'stretched', what is the height (agl) of the lowest model layer?

P13161, L8-10: It is not unreasonable that the fire plumes could reach 1-2 km agl in height. This should be discussed. Please see specific comment #3c above.

P13163, L11: Should read 'Fig 5g'

P13164, L16: 'profound' is a poor choice of adjectives

P13164, L24: The uses of 'fires' is not strictly correct, suggest 'active fire detections'

P13165, L2-4: "The sensitivity study suggested what we proposed; there is no tracer concentration peak when there is no leeside trough." I do not believe this statement is accurate. Please see specific comment #2 above.

P13165, L5-7: The author's have not demonstrated that thermal buoyancy of fire plumes is not adequate to loft emissions above 3 km. Please see specific comment #3c above. P13167, L13: Reference Liu et al. 2003, title should read '. . .Asian pollution..' not '. . .Asian combustion. . .'

Figure 1 Please note whether this is Terra or Terra & Aqua. Should probably only include Terra as non Aqua prior to July 2002. Depending on the data source, Aqua data may be incomplete prior to mid-2003. a) & b) y-axis should read 'active fire detections' not fires. Include y-axis label for 1a. The caption should read 'active fire detections' not 'fires'

Figure 2 Again, caption should read 'active fire detections' not 'fires'

Figure 3 Site source of analysis in the caption

Figure 4 Include the vertical level of these figures. Is this a pressure level or model level? What level is it?

Figure 6c & 6d y-axis label is needed

Several instances of poor or awkward grammar are present: P13156, L10-12 P13157, L8-11 P13158, L9-11 P13158, L24 P13162, L15-17 P13164, L4 P13164, 17-19 P13156 L14-15

References:

Arya, S.P. (1999) 'Air Pollution Meteorology and Dispersion', Oxford University Press.

Chan et al. (2003a) J. Geophys. Res., vol 108, D1, 4015

Chan et al. (2003b) J. Geophys. Res., vol 108, D20, 8800

Giglio, et al. (2003). An enhanced contextual fire detection algorithm for MODIS. Remote Sensing of Environment, 87, 273-282.

Giglio et al. (2006a) Global distribution and seasonality of active fires as observed with the Terra and Aqua Moderate Resolution Imaging Spectroradiometer (MODIS) sensors. J. Geophys. Res., vol 111, G02016

Giglio, et al. (2006b) Global estimation of burned area using MODIS active fire observations. Atmospheric Chemistry and Physics, 6, 957-974.

van der Werf et al. (2006) Interannual variability in global biomass burning emissions from 1997 to 2004. Atmospheric Chemistry and Physics, 6, 3423-3441.

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