Atmos. Chem. Phys. Discuss., 9, S1057–S1065, 2009
www.atmos-chem-phys-discuss.net/9/S1057/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "An extreme CO pollution event over Indonesia measured by the MOPITT instrument" *by* F. Nichitiu et al.

F. Nichitiu et al.

Received and published: 26 March 2009

An Extreme CO pollution event over Indonesia measured by the MOPITT instrument
Author(s): F. Nichitiu, J. Drummond, J. Kar, and J. Zou

We thank Anonymous Referee #1 for the stimulating and helpful remarks. Here are the major points and our answers:

Referee #1 comment: Figure 8a shows measured lightning flashes per storm (FPS) versus hot-spots (HS) for large fire events (11 coincident datapoints). The figure indicates what seems to be a clear positive correlative relationship, perhaps even non-linear as they note regarding a possible saturation effect, yet removing the single Oct06 data point to the upper right would not lend support for this latter hypothesis. The important point of Figure 8a is a positive correlative relationship between intense burning

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Interactive Discussion
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events and lightning with a possible feedback process involved as the authors suggest in the Figure 6 schematic.

Authors' Response: We agree with your comment. The main issue is the strong positive correlation between HS and FPS. The possible saturation effect is an additional point that requires further study. We have adjusted the text of the paper around page 1220 line 20 to reflect this point.

This saturation effect, based practically on the single Oct 2006 data point, may explain why 2006 and 2002 are similar despite the increasing fire activity in 2006. However, the essential point here is a positive correlative relationship between intense burning events and lightning with a possible feedback process as suggested in Fig. 6.

Referee #1 comment: Intentional anthropogenic burning (agricultural waste/general land clearing - not lightning induced) may have increased markedly in Indonesia over the last 20 years including the 2006 El Nino event. It is possible that a substantial amount of the anomalous CO measured in 2006 just by coincidence was not at all lightning related, but instead caused by intentional land clearing fires. Are there any available records of intentional fires/hectares burned in Indonesia for these years to support or refute this?

Authors' Response We have made a further search of the literature on this point and have adjusted the manuscript around page 1216, line 4 as follows:

In the tropical regions, most fires are initiated by humans. There is no evidence of significant changes (or a trend) of intentional anthropogenic burning in Indonesia because the relevant statistics are not available even though the Government of Indonesia has several organisations charged with preventing, monitoring and controlling forest and land fires. The problem is that intentional fires can set off large wildfires which spread rapidly under favourable conditions that are often associated with El Nino events. Separating the controlled fires from the uncontrolled fires with catastrophic consequences is not easy (see for example: Hoffmann et al. 2000; Gutman et al. 2004; Dymond et

ACPD

9, S1057–S1065, 2009

Interactive Comment

al. 2004; Field et al. 2008; van der Werf et al. 2008).

References: Hoffmann A. A., Hinrichs A., Siegert F., International Forest Fires News, Nr 22-April 2000 (Indonesia, pp. 31)

Gutman G.et al. (eds); Land Change Science , Kluwer Academic Publishers; (Chapter 19: Land use and Fires, pp 320-350), 2004.

Dymond C. C., Field R.D., Roswintiarti O., Guswanto: Using Satellite Fire Detection to Calibrate Components of the Fire Weather Index System in Malaysia and Indonesia, Environmental Management, Vol. 35, No 4, pp. 426-440, 2005.

Field R. D. and Shen S. S. P.: Predictability of carbon emission from biomass in Indonesia from 1997 to 2006, Journal of Geophysical Research, Vol. 113 G04024, 2008.

van der Werf G. R., Randerson J. T., Giglino L., Gobron N., Dolman A. J.: Climate controls on the variability of fires in the tropics and subtropics, Global Biogeochemical Cycles, Vol 22, GB3028, 2008.

Referee#1 comment: The authors have room with ACP to discuss more details of Logan et al. [2008] regarding TES measurements of CO, H2O, O3, and possible lightning effects in 2006. Logan et al. [2008] examined tropospheric O3 and suggested significant contribution from lightning-induced NOx in late November and December 2006 when biomass burning (as CO detected by TES) decreased significantly.

Authors' Response We have added the following text on page 1212 after line 25

This big pollution event was also observed by the Atmospheric Chemistry Experiment – Fourier Transform Spectrometer (ACE-FTS) as well as by the Tropospheric Emission Spectrometer (TES) instruments (Rinsland et al 2008,Logan et al. 2008,). Logan et al. studied the tropospheric impact of this event using coincident profiles of O3, CO and water vapour from TES aboard the Aura satellite. They found large differences in the concentrations of these gases over Indonesia in October-December 2006 relative to the same period in 2005 – the first year for which TES data are available.

ACPD

9, S1057–S1065, 2009

Reference: Rinsland et al 2008:

Rinsland C. P., Luo M., Shephard M. W., Clerbaux C., Boone C. D., Bernath P. F., Chiou L., Coheur P. F. : Tropospheric emission spectrometer (TES) and atmospheric chemistry experiment (ACE) measurements of tropospheric chemistry in tropical southeast Asia during a moderate El Nino in 2006, Journal of Quantitative Spectroscopy & Radiative Transfer 109, 1931-1942, 2008

And on page 1221 around line 3, the text is changed as follows:

Logan et al. 2008, point out that lightning in 2006 during November-December over Indonesia exceeded that in 2005 by a factor of 2-3 and was consistent with the higher NO2 tropospheric column observed by the SCanning Imaging Absorption SpectroMeter for Atmospheric CHartographY (SCIAMACHY) and The Ozone Monitoring Instrument (OMI) (Boersma et al. 2007). Increased lightning can contribute to enhanced O3 and therefore the O3 anomaly (defined as the difference from 2005) will persist longer than the CO anomaly. This longer persistence in O3 was observed by TES in 2006.

Reference: Boersma et al. 2007: Boersma, K.F., H.J. Eskes, J.P. Veefkind, E.J. Brinksma, R.J. van der A, M. Sneep, G.H.J. van den Oord, P.F. Levelt, P. Stammes, J.F. Gleason and E.J. Bucsela, Near-real time retrieval of tropospheric NO2 from OMI, Atm. Chem. Phys., 7, 2013-2118, 2007, http://www.atmos-chem-phys.net/7/2103/2007/

Referee #1 comment: The final Conclusions section mentions the Price and Rind 1994 modeling papers where large increases (a doubling) in CO2 could produce an escalation of dry lightning and induced fires in the tropics. In relative context what is the estimated measured time record of global CO2 and would increases in measured temperature and CO2 ($\sim$30% CO2 increase since the early 1800's?) be enough to induce a detectable lightning effect? The author's inclusion of the Price and Rind papers is somewhat speculative in view of the limited measurements in this study and time record.

ACPD

9, S1057–S1065, 2009

Interactive Comment

Authors' Response The following paragraph has been added to the conclusions section – page 1222 line 1

There is a dearth of multi-disciplinary modeling in the area of fires, land use, or influence of zonal climate change, and therefore the possibility of a future increase of natural lightning-caused fires as a result of climate change is still a matter of active research. (Goldammer and Price 1998). A recent analysis of many climate models (Li et al 2007) suggests a high probability of decreasing rainfall during the future Indonesian dry seasons. Modeling of lightning activity in this, more flammable environment would be very useful.

Reference: Goldammer J. G. and Price C.: Potential impacts of climate change on fire regimes in the tropics based on MAGICC and GISS GCM-derived lightning model; Climatic Change 39, 273-296, 1998.

Li W., Dickinson R. E., Fu R., Niu G-Y., Yang Z-L., Canadell J. G.,: Future precipitation changes and their implications for tropical peatlands; Geophysical Research Letters, Vol. 34, L01403, doi:10.1029é2006GL028346, 2007

Referee # 1 Small points:

– The reference indeed should be Logan et al. 2008 (page 1221, line 6)

– 100W to 120E should be indeed 100E to 120E Page 120 line 4 and Figure 8 caption (page 1233)

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We thank Anonymous Referee #1 for the stimulating and helpful remarks. Here are the major points and our answers:

Referee #2 comment: the main remark I can do is that the paper could be much more convincing with modeling experiments. At least it would be useful if the authors could suggest some ideas to make some numerical experiments even if modeling is out of the scope of this paper.

ACPD

9, S1057–S1065, 2009

Authors' Response: Indeed modelling is out of the scope of our paper. We have concentrated on collecting physical evidence of a possible feedback between fires and lightning.

Given the fact that some climate models suggest a decreasing rainfall during the future Indonesian dry seasons, a lightning activity model designed for these more inflammable conditions will be very interesting. We have incorporated a suggestion to this effect in the conclusions of our paper (see our answer to the first referee above)

Referee #2 comment: Typo at L=15, p=1214.

Authors' Response: Corrected

Referee #2 comment: l 17 p 1216: I find this sentence ambiguous. Maybe it is just rhetoric and I understand that a lot of fires from lightning can burn a bigger area than just a few anthropogenic fires. What anthropogenic fires represent? is it related to specific human-made things instead of biomass? I suggest the authors to simplify or clarify this point.

Authors' Response: The fires ignited from lightning may occur in remote locations, far from possible human control and interventions, they can evolve fast and burn a bigger area. In order to be clearer we have added a comment at the beginning of section 4 related to anthropogenic burning. This text is under our second response to first referee above.

Referee #2 comment: l 3 p 1221 : Concerning the NOx production and the paper of Logan et al., I would suggest the authors to argue that point using distribution of NO2 from OMI or SCHIAMACHY if available for example.

Authors' Response: We are have commented on this point in the answer #3 to Referee #1.

Referee #2 comment: Again, I found the last sentence a little ambiguous (Price and Rind, 1994b,c) for the purpose of this paper. What kind of phenomena that are explo-

ACPD

9, S1057–S1065, 2009

Interactive Comment

sives, is it the number of storms, more fires, more CO, all of them. In fact I find this sentence too sensational for just one case with a CO maximum. I would suggest to be more academic or explain better this sentence in the context of that paper.

Authors' Response: There was no intention of being sensational. The sentence was simply added to point out the possible linkage between climate change and the incidence of fires. We have changed the conclusions section to reflect this intention in concert with the response to Referee #1 above.

Response to SC S686 : 'Interactive comment' , Guido van der Werf, 09 Mar 2009

We thank the authors for their interest in our paper and for useful comments. Here is our response:

General Authors' Response: We agree with your comment that lightning is not the major factor of the fires in Indonesia and that the fires are initiated by humans and spread mainly due to dry environmental conditions. We also agree with the fact that some other factors could also contribute to a full understanding of fire activity in Indonesia.

What we are trying to say is that lightning, as probably modified by the smoke and aerosols can contribute in a feedback loop even if only for a short period of time and, together with the all other factors, generate more fires. An additional observation reported in our paper is the changes seen in number of Flashes Per Storm (FPS) for post fire period of lightning, the raining period, (confirmed also by the work of Logan et al. 2008) demonstrating that lightning activity could well be affected by the smoke.

Comment pages S687 (second paragraph) and S690 and also comment to P1219L3: The saturation effect will be probably be clearer if we speak about Flashes per Storm (FPS) rather than the total number of flashes. FPS is better connected to the process of electrification of the cloud which cannot go over some limit Any electrification process has a limit. A Van de Graaff accelerator for example does not collect more charge without limit. The limit dictated by the machine capacity. In the same way there must

ACPD

9, S1057–S1065, 2009

be a limit of cloud capacity for electrification. Another important factor is that it is not only the lightning frequency that is important for the fire spread, but also the incidence of positive cloud-to ground lightning strikes. Both modeling and observations suggest that these occur more often when clouds ingest smoke. This is explained in some detail in our paper at P1217 (see also the References cited).

Comment pages S687 and S689 (second paragraph). During El Nino there is an enhancement of lightning activity. We cited the paper of Hamid et al 2001 et al. because it is connected directly with Indonesia. For the global scale, we could cite here a recent paper: G. Satori, E. Williams and I. Lemperger "Variability of global lightning activity on the ENSO time scale" in Atmospheric Research 91 (2009) 500-507 that states "Global lighting is slightly enhanced in the warm El Nino phase..." And also provides additional references.

Comment page S689 and comment for P1215L26 Our analysis was indeed based on the monthly precipitation average over whole Indonesian region as was the case for the whole of our analysis.

Comment S690 comment P1216L7 See answer to Anonymous Referee above.

Comment S690 and comment to P1216L24. Indeed "a dry season during an El Nino warm period is not necessarily a dry lightning season..." The statement has been changed to: "A dry season during an El Nino warm period could be a dry lightning season with a lot of fires"

Comment S690 P1217L1 Increased convective storms should lead to precipitation if there is enough water vapour, but "the deeper convection may cause greater vertical developments, which are responsible for greater lightning activity" (Hamid et al. 2001).

Comment S690 P1219L17 Concerning weekly averages vs longer time periods.

In order to be able to observe the effects and suppress random fluctuations, a judicious choice of time periods for averaging has to be made. Since the effect is not very strong

ACPD

9, S1057–S1065, 2009

we consider monthly averages to be better than weekly in this case.

Comment S690 P1219L26 Our intention is not to say that lightning contributed to the fire events of 2006 to the exclusion of all other mechanisms. Indeed in the abstract we stated that the fire events were “probably amplified” by lightning activity – a fairly conservative statement. We are putting forward this feedback mechanism as a plausible agent in what is presumably a complex issue and do not claim to have solved the whole problem. We are changing the relevant sentence in the conclusions to more exactly reflect this position.

Comment on Fig 5 Corrected

Comment S691 Fig. 8 Fig 8b is not offset by one year. All the points are for December of the year beginning with December of 2002 and ending with December 2006.

Final Authors’ Comment We are advancing the hypothesis that it’s not just ignition that is important in spreading the fire, lightning can also play a role. There is a possibility of feedback through the influence of large quantities of smoke and aerosols on post-fire lightning. There isn’t enough data for definitive conclusions and other hypotheses, like the “threshold effect” mechanism proposed by one of the authors of this short comment (Field et al. 2008) is also plausible.

What is now required is more data from both remote sounders and in situ sampling to enable us to elucidate the relative importance of the various mechanisms.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1211, 2009.