

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

B. Duncan (Editor)

Bryan.N.Duncan@nasa.gov

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Both reviewers believe that the authors pose an intriguing hypothesis, but do not provide particularly convincing evidence. van der Werf, in a short comment, argues that the authors did not do enough to eliminate other possible causes of the enhanced fires in 2006. Therefore, I have reviewed the manuscript, the two reviewer's comments, the short comment by van der Werf, and the authors' response.

In short, I agree with the reviewers that this is an intriguing hypothesis, but completely agree with the comments of van der Werf. I believe that the authors did not satisfactorily respond to the comments of van der Werf.

Here are my own comments from my review of the manuscript, which often echo those

of van der Werf:

1) It is not adequate to look at rainfall over such a large region to conclude anything about fire activity. Drought is oftentimes highly localized. I believe that the authors need to look in more detail at the local distribution of rainfall, particularly in relation to biomass such as peat. The regional rainfall may have been similar in the three years, but the severity of drought may have been very different.

2) Smoldering combustion, such as is common in peat, is particularly dirty, producing more aerosols than during flaming combustion. This alone could cause variations in your hypothesis of the feedback between lightning and aerosols. I hope that the authors look into this issue.

3) The title of your manuscript does not properly describe the content of the manuscript. I suggest that you revise it.

4) In my own manuscript (Duncan et al., 2003 in JGR), I researched in detail the causes of fires, which were many and varied, during the 1997/98 Indonesian wildfires. I suggest that you do similar research, which may help to support/refute your hypothesis. For instance, was there a government incentive to clear land in 2006, but not in the other years? Was a particular region being cleared for new palm plantations in 2006?

5) I do not believe that the evidence you present supports the objective of your manuscript:

The question that we address in this paper is why the Indonesian fire activity of 2006 was much larger than 2002, when, according to the standard measures, the 2006 El Nino was similar or even slightly weaker than 2002.

In fact, I believe that you only present an intriguing hypothesis, which requires more work to support.

6) I do not agree with your response that modeling is out of the scope of this manuscript.

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I encourage the authors to continue their research on this intriguing idea.

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

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