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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Release and dispersion of vegetation and peat fire emissions in the atmosphere over Indonesia 1997/1998" *by* B. Langmann and A. Heil

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This paper presents a model study of the aerosols released and transported during the 1997/1998 ENSO, with a particular emphasis on emissions from peatlands, which have very high emission factors. The model study is basically sound (but see comments/questions below) and is providing some new information. However, I am left a little unclear what the main purpose of this study really is. Is it a model validation study? Is it a study to determine the areas burned? Is it a study that identifies a new or investigates a particular known process? The reader could take much more profit from this paper if the authors lay out clearly what the aim of their paper is at the beginning and then follow this up in the remainder of the text. They should also emphasize the

new findings of this study. Right now the Conclusion section reads a little like a literature survey rather than conclusions from their own study. New threads of thoughts (e.g., the discussion on CO_2) should not be started in a Conclusions section. The paper would also benefit from an attempt to put the results into a broader perspective: What is the relevance of the Indonesian peat fire emissions on the global scale? For this, a comparison with burning in other tropical regions and in the boreal forest would be interesting.

Generally, I favor publication of this paper in ACP, but I encourage the authors to put some more work into the presentation and interpretation of the results before re-submitting a revised version of their paper.

1. Major specific comments

Page 2120, lines 22 and following: Langmann and Graf estimate that the sulfur content of the peat is about 2-5 g/kg. Thus, it can hardly dominate the composition of the aerosols released during peat fires. Is it justified to treat the aerosols like sulfate aerosols and assume 100% solubility? These particles will likely also have a very high BC content. At least, the authors should give an indication of the sensitivity of the model results to this assumption.

Page 2121, lines 10 and following: In the forecast mode, the meteorological model is re-started every 24 hours, while the aerosol model is run continuously. I am wondering how the inconsistency between the 30-hour forecast and the subsequent 6-hour forecast affects the results. For instance, precipitation fields will likely be displaced relative to each other in the two simulations (this effect may be particularly pronounced at the rather high resolution of the model). This enhances the likelihood of aerosols to get into precipitation at any point of their transport history (or, equivalently, a larger fraction of an aerosol plume will be affected by precipitation). Because of the non-linearity with

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rain intensity of washout processes, this in turn will lead to an overestimate of the total washout. Has such an effect been found in the model results, and are the climate model runs, therefore, not the physically more meaningful ones?

Page 2123-24: The parameter β in (1) is chosen such that the area burned is less than 1/14 of a grid cell during one week. On line 27 of page 2124 you write that the maximum area burned per week was 5% of a grid cell. I am a little confused here. Shouldn't the maximum area be 7% (1/14)? If this is not the case, why is β needed at all?

Section 4.2.1: I am confused by the differentiation of GPCC data for ocean and land. On the GPCC website, they state: "GPCC delivers two gridded datasets (pdf) of monthly precipitation for the global land-surface". Because GPCC is based on rain gauge data only (to my knowledge, at least), I do not know how you have obtained precipitation estimates over the sea from GPCC. If you find that precipitation data over the sea is incomplete or missing, you may want to inspect the GPCP website (http://precip.gsfc.nasa.gov/), where a merged product of GPCC data and rainfall estimates from satellite is available. Could your overestimate of precipitation be due to missing rainfall measurement data over the sea (except, perhaps, for areas very close to land)?

Conclusions, first paragraph: Here, the authors conclude on the areas of peatland burned, based on a comparison of their model results with measurement data. I am wondering why the authors didn't do simulations also for CO_2 and CO. Especially CO is a very good long-lived tracer, which has very high emission factors for peat burning, is unaffected by precipitation (the major uncertainty factor in the simulations), and for which high-quality measurement data are available. Couldn't the total areas and biomass of peatland burned be quantified more reliably using CO rather than aerosols?

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Interactive Comment

2. Minor specific comments

Page 2134, line 12: If I understand right, the PM10/TPM ratio is based on measurements close to the source. Is this ratio still valid after the plume has experienced long-range transport and is chemically processed?

Figure 1: Shouldn't there also be some rain forest in Northern Australia?

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2117, 2004.