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Interactive Comment

Interactive comment on "Interannual variability of global biomass burning emissions from 1997 to 2004" by G. R. van der Werf et al.

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1.0 Overview of the Manuscript

In this manuscript, the authors present estimates of global biomass burning emissions over the period of 1997 to 2004. The results represent a revision and extension of previous estimates this group has generated (van der Werf et al., 2004). The previous work was revised in two important areas. First, the authors used new estimates of burned area derived from several different satellite data products. Second, the authors altered their emissions estimation approach to more realistically the burning of deep surface organic layers that are common in peatlands (both boreal and tropical) and boreal forests. They extended their previous studies by estimating emissions for a



longer time period (the previous study included estimates for 1997 to 2001). This study represents the most comprehensive and complete set of biomass burning estimates generated to date. The authors have used the most reliable data available in terms of estimating burned area, and they use logical approaches for estimating the distribution of available fuels and consumption of fuel during fires in different biomes. However, while I believe the authors have improved their biomass burning emissions relative to their previous studies, I have several concerns. Given my background in boreal fires and emissions from fires, it should come as no surprise that my primary concerns are in the following areas: (a) burned area estimates for the boreal region; (b) the use of the term soil organic carbon; and (c) estimating emissions from the consumption of surface organic layer material present in peatlands and boreal forest. In addition, the authors did not present enough data that would allow comparison of the results from their study to other estimates of emissions from biomass burning.

- 2. 0 Major Concerns with the Manuscript
- 2.1 Burned area estimates in the boreal region

The data concerning the estimates of levels of area burned in the boreal forest region reported in this paper seem somewhat out of kilter to me:

1. The CIFFC data in Figure 10a seem wrong to me, about twice as great as they should be. To check these data, I obtained a report of the CIFFC and present their estimates in Table 1 for burned area in Canada for the period of 1997 to 2004. According to the CIFFC statistics in Table 1, 16.0 x 10⁴ km² burned during the study period, whereas the CIFFC data presented in Figure 10a indicate some 30.0 x 104 km² burned. Furthermore, in Giglio et al. (2005), the authors report that the MODIS area burned estimates for Canada from 2001 to 2004 were 83% of the CIFFC estimates. Based on the data presented in Figure 10, the MODIS estimates are 45% of the CIFFC estimates. However, if you use the CIFFC data in Table 1, then the value reported by Giglio et al. (2005) is obtained. Note that the comparison between the

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author's and the CIFFC estimates are much closer if the data in Table 1 are used.

2. I think the authors miscalculated the total burned area for the BONA and BOSA regions reported in Table 5. During the study period, the burned area reported by the Alaska Fire Service (see Table 1) totaled $5.6 \times 10^{4} \text{ km}^2$, which results in a total burned area in the BONA region of $21.6 \times 10^{4} \text{ km}^2$. However, in Table 5, the author's report that some $10.9 \times 10^{6} \text{ km}^2$ burned in this region, which is 50 times greater than the reported area burned. In the BOSA region, the author's and the Sukhinin estimates are very similar. Over the study period, Sukhinin reports a total 76.4 x 10^{4} km^2 of burned area for the BOSA region, whereas Table 1 reports $15.2 \times 10^{6} \text{ km}^2$ burned, which is 20 times higher.

In addition, I am also concerned about the approach used to estimate burned area prior to 2001 in the boreal region, as well as elsewhere. Kasischke et al. (2003) showed there is significant bias in the ATSR hot spot record for the boreal region, in particular, that the ratio of fire counts to burned area varied significantly between years. While the total burned areas for the BONA region match the CIFFC/ASF data fairly well, the burned areas in the BOSA region vary significantly from other observations in some years. In addition, the ATSR algorithm depends upon detection of the night-time thermal IR radiation that is emitted by fires. However, the local sampling time used for ATSR (10:30 pm) does not occur at night-time in high northern latitudes for part of the summer (e.g., around the solstice on June 21), but does occur at night time during later growing season fires. Thus, I suspect that there is a temporal sampling bias in the ATSR data that results in an incorrect seasonal distribution of burned area in boreal regions.

2.2 Use of the term "soil organic carbon (SOC)"

While the authors make an improvement over previous approaches by recognizing that deep layers of un-decomposed organic matter lying on top of mineral soils represent a significant source of fuels and emissions in some biomes, they do not present clear

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terminology in their manuscript to describe the unique biomass layers they are dealing with. Instead, they use the term "soil organic carbon (SOC)". In their discussion, they discuss active soil pools and passive pools, and make note of the fact that some SOC is not accessible to fire because it is associated with mineral soil. The authors could make a much clearer distinction of what is being talked about here a la Harden et al. (2004). What is being considered in this study is the un-decomposed surface organic matter that lies on top of mineral soils, which is referred to as the organic soil layers (in forests) or peat (in peatlands).

2.3 Estimating carbon release and emissions from the burning of surface organic layer fuels

It is not clear at all how the authors estimate surface organic layer fuel consumption in section 2.3. Clarification of the approaches used in this study is needed. Initially it seemed that the author's initially followed the approach developed by Kasischke et al. (2005) and estimated carbon release from burning of surface fuels by regulating depth of burning of the surface organic layer. However, they also state on lines 27/28 of page 3185 that the depth of burning in the boreal region varied "linearly between 0% (moist) and 33% (dry) where the 0 and 33% were based on total SOC predicted by CASA. They also note that they did not vary the bulk density of their carbon estimates as a function of organic layer depth as was done by Kasischke et al. (2005)

I am confused as to how the authors created spatial variability in their SOC estimate, e.g., accounted for the fact that different boreal regions have different levels of peatlands and permafrost forest, and hence deeper layers of surface organic matter. They say they used the values of Batjes (1996) to scale the SOC levels, but did not clearly explain how this was done. They also state they used the wetlands map of Matthews and Fung (1987) to distinguish between SOC that is accessible for fire, but do not say how they determine the fraction of an area that contains deep organic layers in forests underlain by permafrost. Finally, in the section, they refer to the "bulk density" profile of Carrasco et al. (2006), where in fact, Carrasco et al. (2006) present a carbon density

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profile. It appears to me that the authors are still using an approach that depends on fraction of available fuel available for burning and a combustion completeness based on a percent of the fuel that is consumed by the fire. It appears that they vary the amount of fuel by scaling the amount of fuel according to spatial variations in SOC according to Batjes (1996), but they do not say how this scaling is accomplished. Given this approach, it is not clear to me that assuming a maximum depth of burn of 10 cm according to Kasischke et al. (2005) is appropriate. In addition, I do not see how the use of a soil moisture scalar captures the complexity of variations in moisture of surface fuels in boreal regions. In particular, the moisture of peatlands is regulated not only by precipitation, but also surface runoff from surrounding uplands. For boreal forests underlain by permafrost, the seasonal thawing of permafrost affects fuel moisture (Kasischke and Johnstone 2005). These important processes are not discussed by the authors, and because they are not addressed, I suspect that the authors may not be capturing the correct surface fuel consumption dynamics within their approach. However, because they do not present many details on the outputs from their study (see section 2.3 below), it is difficult to determine if this is true or not. Finally, the authors choose not to consider the fact that most of the consumption of surface organic layer fuels occurs during smoldering consumption (Kasischke et al. 2005), and use a single set of emission factors to estimate trace gas emissions during fires in boreal regions. Because of this, they probably underestimate emissions of CO and CH4 in this region considerably. In summary, while the above comments appear to be somewhat critical of the approach used in this manuscript, the authors are attempting to address a very important component of estimating pyrogenic emission in boreal regions, as well as in the tropics of SE Asia. However, the approach being developed lacks any connection to numerous field-based studies of surface fuel consumption outside of Page et al. (2002). A more direct linkage of the results of field based studies of surface fuel consumption in the boreal region to the modeling approach presented in this study may help clarify the approach (see, e.g., Dyrness and Norum 1983; Kasischke et al. 2000a: 2000b; Turetsky and Weider 2001; Miyanishi and Johnstone 2002; Turetsky et

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al. 2002; Benscoter and Weider 2003; Harden et al. 2004; Kasischke and Johnstone 2005).

2.4 Comparison of Results with Other Studies

One of the challenges that confronts scientists producing and using estimates of emissions from biomass burning is assessing the variability in such estimates and ultimately determining the sources of uncertainty between estimates produced by different groups. For example, it is difficult to compare the results from this study on emissions from the boreal region because the authors present no compilations of total emissions from different regions along with estimates of burned areas from these regions. If the authors produced an additional table to Table 6 that provided information on the inter-annual variations in burned area in each region (and produced Table 6 with an additional significant number), then the readers would be able to carry out an analysis of the relative levels of carbon emissions per unit area burned in the different regions. For example, while we can directly compare the results in Table 6 for the BONA and BOSA regions to the results of Kasischke et al.'s (2005) Table 5, because area burned information is not presented, one cannot determine the source of the differences between these two studies.

3. Minor Comments

a. Line 28 on page 3177 - Other studies have shown the relationship between largescale atmospheric circulation and boreal fire activity, including Hess et al. (2001) and Duffy et al. (2005). b. Rather than using a URL address for the Canadian Large Fire Database, I would suggest using Stocks et al. (2004). c. In discussing levels of fuel consumption during fires in Section 3.4, the authors switch between kg m-2 and g m-2. They should one of the two units, and use it throughout the paper. d. Line 1 on page 3199 - The view expressed in this sentence is not new, see, e.g., French et al. 2004. e. I am not sure I agree with the last sentence of Section 3.8. Amiro et al. (2001) developed histograms based on a model output. One of the problems 6, S631-S640, 2006

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with field studies is that very few, if any, have a sufficient number of observations to generate a meaningful histogram. f. Carassco et al. (2006) is in JGR-Biogeosciences, not JGR-Biosphere.

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Table 1. Area burned estimates reported by the CIFFC, Alaska Fire Service, and A. Sukhinin based on analysis of satellite imagery (area in 104 km2; CIFFC data obtained at http://www.fire.uni-freiburg.de/inventory/database/Canada-Fire-Report-2004.pdf, and the Sukhinin data are after Sukhinin et al. 2004 for 1997 to 2002 and from http://www.fire.uni-freiburg.de/).

I apologize to the readers for the format of this table. Because ACPD does not provide a user friendly means for a reviewer to submit his/her text (other than learning an obscure publishing language), I could not provide the following table in a format that is easily read. In reviewing the table, each year is followed by: (a) burned area for Canada reported by CIFFC; (b) burned area for Alaska from the Alaska Fire Service; and (c) burned area for Russia from Sukhinin.

BONA BOSA CIFFC AFS Sukhinin 1997 0.625 0.775 1.556 1998 4.711 0.073 10.776 1999 1.706 0.426 7.181 2000 0.647 0.310 9.557 2001 0.63 0.090 8.551 2002 2.757 0.918 11.810 2003 1.637 0.247 21.763 2004 3.277 2.722 5.238 Total 15.990 5.561 76.443

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