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Interactive Comment

Interactive comment on "Boreal forest fires in 1997 and 1998: a seasonal comparison using transport model simulations and measurement data" *by* N. Spichtinger et al.

Anonymous Referee #2

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The manuscript by Spichtinger et al. contrasts the impact of forest fires in a low and a high fire year (1997 and 1998) on atmospheric trace gases (CO and CO_2), aerosols (as derived from TOMS), and NO_X (as derived from GOME). The transport analysis is compelling, and the manuscript represents a major advance in our understanding of how fires from different areas of the boreal forest contribute to atmospheric trace gas observations. It was intriguing to find out that Siberia forest fires had a larger impact on atmospheric CO over Canada in 1998 than local Canadian fires. Even though the tracer analysis is an important step forward, overall the manuscript lacks an attention to detail that could potentially limit its impact. Also, a number of statements related to ENSO and NAO seem unsubstantiated by the author's data and figures.

Specific examples of possible ways to improve the manuscript include: 1. In Table 4, it is impossible to figure out that a larger fraction of Siberian fires were transported to Canada in 1998 rather than in 1997 – as the authors claim in the text in the Transport Modeling section. To figure this out, requires that the reader first divide the rows by the emissions from each source region from Table 3. Adding extra columns for these calculations would bolster the author's points.

2. It is really difficult for the reader to see from the figures (particularly Figure 4) that fires extended to higher northern latitudes in 1998 rather than in 1997 because of the NAO. It could also be due, just as easily, to more fires at higher northern latitudes in 1998 in Canada (eg. Figure 2). Please consider dropping this text (eg. Page 5, first paragraph, last sentence). Also, from the abstract, it is difficult to solely attribute this enhanced transport to El Nino, again, please consider dropping this reference.

3. The peak in CO in May in Figure 6 is not due to the initiation of boreal forest burning (it is absent from Figure 8). Instead it is a result of northern transport of fires from Southeast Asia and Central America during spring of 1998. For example, please see Figure 2 of Van der Werf et al., Continental-scale partitioning of fire emissions during the 1997 to 2001 El Nino/La Nina period, Science, 303 (5654), 73–76, 2004. More broadly, in the text on page 5, and in the conclusions, the authors neglect contributions from the northern tropics to CO anomalies at high northern latitudes. As shown in the tables in the appendix to Van der Werf et al, $\sim$20% of CO anomalies between 30 and 60 N come from fires in the tropics. These contributions are likely to be even far more significant for CO_2 than CO, since CO_2 is not destroyed via OH. The contribution from the northern extratropics, also explains, in part, why the authors underestimate the 1998 CO anomaly.

4. The manuscript need to be carefully double checked there were a number of missing words, tense mismatches. For example, on page 3, first column, the sentence starting with 'To obtain the same ratio...' does not make sense. Also, 'Inter alia' seems like an arcane term to describe the flask network.

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5. Page 2. A paper by Kasischke et al. provides a mechanistic basis for the reason ATSR to burned area relationships vary from year to year. Kasischke, et al., The use of ATSR active fire counts for estimating relative patterns of biomass burning – a study from the boreal forest region, Geophysical Research Letters, 2003.

6. Figure 1. In many ways, this figure could be enhanced if the authors simultaneously presented precipitation anomalies for the same regions. Increased temperature do not necessarily reflect drought, if PPT covaries.

7. The GOME data contributes only marginally to the author's development of the fire story. I think, however, that it is important to include, because it shows that NO_x is dominated by other sources.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2747, 2004.

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