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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Around the world in 17 days – hemispheric-scale transport of forest fire smoke from Russia in May 2003" *by* R. Damoah et al.

Anonymous Referee #1

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General Comments

The paper deals with an interesting subject and does address relevant scientific questions. It, however, seems to me that the authors try to sell the results in a way that is a bit questionable. The novel aspect here is certainly not the fact that debris from the Asian fires in 2003 circled the globe, although this is very interesting and not widely known. This has this time already been followed up by the scientific community a few days after these events occurred, based on satellite observations and near real time modelling. The interesting aspect here is that these authors managed to simulate, in a not perfect but very reasonable way, a transport event that lasted 17 days. This is certainly something that deserves attention and should be published. The authors,

however, should address and comment on some of the questions raised in the following.

Specific comments:

(1) The authors state, in their abstract, that 'this is perhaps the first time that air pollution was observed to circle the entire globe'. This statement is, from my view, a bit strong, given the fact that the existence of the jet streams and their effects on transport is known since a while. The authors should reconsider this statement and try to find a more appropriate formulation. Maybe it is fair to say that not many events, if any, have been documented and simulated as tightly and as comprehensively as the one subject to this study.

(2) The authors state, in their introduction, that 'relatively little attention is paid to forest fires in Russia'. This to my view is true as far as public mass media attention is concerned, but in the scientific world collides with an increasing number of papers dealing with exactly that subject in recent years. The authors should reconsider this statement and should try to include and quote relevant recent work on that subject. The fires in Russia are not special in any way. It is clear that a country where two thirds of the global boreal forest is situated, two thirds of the boreal forest fire activity should occur (and actually does occur, as recent studies show).

(3) The authors quote one estimate of the average annual burning in Russia of on the order of 5 Mio ha and others on the order of 10 Mio ha. Does that indicate that estimates of average annual burning in Russia is still (as of 2004!) that uncertain? I do not believe that, and I rather believe that there should be quite comprehensive estimates (plus/minus 25-50% or so) in recent literature.

(4) These authors compare a passive tracer simulation with observations of aerosols. They do not mention, though, that in doing so they (intentionally, I believe) leave out important processes like wet deposition. If they see a structure in the measurements and a corresponding structure in the model, it is fine. If they see a structure in the model

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but no corresponding in the measurements, it is still fine since they can always argue that the debris has been deposited away in reality. If they see a structure in the data but no corresponding in the model, however, they would have a problem. Any comments from side of the authors on that (in addition to the word 'compare qualitatively')? By the way: the argument 'we do not compare tracer with CO because something like this was already done in 2001' is a bit weak, unless the event published in 2001 was very similar to the 2003 event, which I doubt. The relevant questions is: why do you not compare aerosol simulations with the aerosol measurements, there must be some arguments for that.

(5) These authors compile their model emissions based on MODIS hot spot data with a temporal resolution of, as it seems, 24 hours. They, however, do not consider cloud cover or include any other correction that accounts for the fact that clouds may cover (and thus blind out) fire hot spots for a number of days without stopping the burning (and thus the emissions). This introduces, as these authors state themselves later in the paper, 'artificial variability' into the model simulations. I would assume that this method introduces a lot of noise in the emissions estimates. So the question is: why did the authors chose this method, and why is this method better then more conservative estimates that assume constant emissions over a longer time period and are more based on observed burning areas then on hot spot data?

There are also some minor technical comments:

(1) The authors mention that the temporal resolution of the NOAA/GFS analysis data is 3 hours. I believe this is 6 hours. Please cross-check

(2) The authors release the forest fire debris into the lowest 3-km. In reality this could be considerably higher

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1449, 2004.

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Interactive Comment