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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-range transport of biomass burning smoke to Finland in 2006" *by* L. Riuttanen et al.

Anonymous Referee #3

Received and published: 6 April 2013

General

Dear editor and authors. I have made my review without looking at the other referee comments to avoid bias in opinion. Hence, please bear in mind potential repeating of messages from one of the other reviewers.

I recommend to accept the paper upon major revisions.

The paper presents important results on how much fires during dry periods can contribute to gas and particle pollution due to long range transport, and hence to the impact on human health and regional climate change. The paper is presented in an easy understandable fashion with a clear line of argument. The authors present a novel empirical method to indirectly infer the fire contribution in an easy and practical way, which

can be applied to other periods and geographic locations of intense fire episodes.

However, there is a concern that the reference period is wrongly chosen. Similar air masses (in other words wind directions) are required for a just comparison between reference and episode, see below.

Major concern

You do not show that the air masses during the reference period with almost no fires are comparable to the episode period. You should check that they are comparable, for example by plotting a wind rose for each of the periods. If they look similar, you can be confident that you are comparing the right things. But, if they are not, then you should redo the analysis, and select a proper reference period with the same wind directions as when you have the fire episodes.

I stress the concern by showing a calculation example from your paper: You claim that only $\frac{1}{4}$ of the increase in CO levels compared to the reference period are due to biomass burning during the fire episodes. This number will be different if you choose a different reference period. Hence, please show that the reference period is representable also for the fire episode. In addition, Stohl et al. (2007) show a completely different result for the contribution from biomass burning at the Zeppelin station (Atmos. Chem. Phys., 7, 511–534, 2007). In their study, biomass burning is dominating the contribution to enhanced CO levels at the Zeppelin station during the fire episodes. I don't claim that their result or that your result is wrong, but this difference alone is a cause of worry, and a base for a detailed control if everything is alright. Would be interesting if you also could comment on this difference between your results in your paper.

Specific comments

Page 4292, lines 12-26. According to your introduction, there seems to be a number of studies focusing on the effect in Finland due to the fire episode from 2006. In what

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way is your study different from the referenced ones? What did the other studies don't do that you are doing?

Equaton (2). I admire that you have tried to explain the equation very concisely, and the equation seems to be easy to use due to the small number of parameters. However, I don't quite understand. You have 3 knowns, and 3 unknowns? Please add a few sentences that explain which data points are fitted, and how they look, and how many data points that you are normally fitting (hard to understand that you will get enough data points to fit). This might add to the understanding. After all, this is the first time you present your novel method. But, please keep it short.

Method explanation, page 4296, and conclusion section. Please state clearly (but very shortly) how to produce the Hysplit trajectories. The reader should be able to use your method for other fire episodes in other parts of the world. So, the reader should have an easy task to find the information, software and the products that he/she needs to be able to quickly start a similar analysis that you have made. Please, give also your consent in the conclusion that anyone can contact the leading author, if they want help with the analysis, and that you give your approval to use the code that you have developed for the purpose (what software?), despite that it might not be user friendly yet. This would also make life easier for somebody that would like to use your tools. There is no meaning to keep your knowledge and very useful method to yourself. Another group who would use your method can have you as co-authors, so you would only gain on this deal.

Different methods. Please explain how FLEXPART is different from your method, and what advantages and disadvantages the FLEXPART approach might have compared to your approach in terms of the quantification of source contributions to the SMEAR stations, of identification of source regions, and of user friendliness, and how much analysis time the different approaches require. Compare with for example Stohl et al. (2007). I think this comparison is justifiable, since the FLEXPART method is already widely used, and well established.

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Very interesting discussion about lifetimes in section 3.2. Would you mind speculate a little bit further on the low lifetime of CO, and accumulation mode, and the long lifetime of BC? To me it seems logical that the lifetime of the accumulation mode and CO is shorter than in other studies. In a very strong fire plume like yours, the surrounding air and air aloft (not influenced by fires) have much lower concentrations of these pollutants during long range transport. Hence, when the plume air is mixing with cleaner air from the side and aloft, the concentration goes down quickly, and hence you have a relatively lower lifetime. Conversely, in other studies where we have long range transport of accumulation mode particles and CO from continents with intense car traffic, the emissions take place over a much larger area (for example all car traffic in entire Europe). Hence, the air from the side and aloft is not much cleaner than the air that you follow along a selected trajectory path, and hence we do not loose the pollutants due to mixing with cleaner air, and get a longer lifetime. Or am I wrong here? Please speculate further.

Page 4311, lines 1-3. In Helsinki not all particles are due to the smoke plume, a fraction of the particles especially smaller than 10-30 nm diameter should come from the car traffic emissions. Please comment on this. Otherwise the reader might believe that almost all the particles below 10 nm diameter come from smoke.

Page 4311, lines 4-7. Please clarify this paragraph, which seems to contain also some errors like “It shows only the difference in particle total number”. The volume distribution cannot show the difference in particle number.

Pages 4311-4312. You repeat the message about secondary particle formation in a young smoke plume both on page 4311 and on page 4312. Please shorten those sections. I don’t think that the newly formed particles in the nucleation mode are an artifact. I have seen in a residential area with extensive domestic wood combustion, new particle formation at 4-6 a.m.. This seems to happen when the old accumulation mode has died out in the morning since people don’t put wood logs in their stoves in the night, but there are still enough of condensable vapors to create a brand new

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nucleation mode of particles as the condensation sink decreases.

Technical corrections

Page 4292, lines 5-11. Please write that also forest fires contributed to the smoke plume during summer. Otherwise the reader might think that the plume reaching Finland contained only agricultural burning emissions.

Chapter 2.1. Please state clearly in the manuscript where to find information about detailed instrumentation and site description for each of the SMEAR sites (references in other words).

Page 4294. Is really all the DMPS systems in the three stations measuring from 3 to 1000 nm diameter?

Page 4299. “That is also the most common direction of pollutants arriving to Hyytiälä”. This sentence might give an impression that the wind normally comes from this direction. Please write that this is the wind sector that gives the highest pollutant concentrations.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 4289, 2013.