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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic smoke – record high air pollution levels in the European Arctic due to agricultural fires in Eastern Europe" *by* A. Stohl et al.

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This is an exciting paper which shows in a very detailed case study that burning fields in Eastern Europe can severely pollute the atmosphere at one of Europe's most remote background stations, Zeppelin in Spitsbergen. It also gives evidence of a link between a warm anomaly in spring in this arctic region and the transport of pollution, with the perspective that future global warming will bring more pollution at least into some parts of the Arctic.

Below I am presenting some specific remarks which the authors are invited to consider in a final version of their paper.

1. Title, abstract, introduction and conclusions emphasise the “record” nature of the events described. From a scientific point of view, the “record” as such is less meaningful than a quantification of how extreme the events were. The usual tool would be an extreme value analysis, and results could be presented as estimated return periods of the observed events. Some of the species may not yet be observed for a very long time, so that “records” might be set by moderately rare events.

2. The evidence presented in the paper that the smoke and pollution is mainly from *agricultural* fires is not fully conclusive. The fractions of emissions in different land-use classes as presented in Table 1 depend on several uncertain parameters, such as the combustible biomass per square metre. The values given for grass and cropland appears to be high (0.5–1 kg m^{-2}), considering that we are referring to managed, agricultural land and the beginning of the growing season. Furthermore, I am wondering what the difference is between *forests* and *woodland*. It is quite astonishing that fires can be ignited on fields and agricultural grasslands immediately after snowmelt in spring, when there is little biomass and everything is soaked with meltwater. What convinced me finally was a look at a high-resolution MODIS image available on the web at http://www.fire.uni-freiburg.de/GFMCnew/2006/05/0501/20060501_ru.htm. Maybe the authors could quote this link. On the other hand, the link to the on-line newspaper *Baltic Times* which they do provide is practically useless, as it points only to the home page of the newspaper, not to a specific article. If the authors have any hypothesis on the fire mechanisms under the circumstances given, I would encourage them to share it. Otherwise it could be indicated as a topic for further studies.

3. The possible influence of clouds on the fire counts has already been discussed to some extent. However, I am wondering what the average lifetime of these small fires is, and what the frequency of unobstructed observation by the satellite is in

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this time of the year. I think this issue should be discussed more.

4. In lines 15–25 on p. 9658 and on p. 9669, the impression is created that a warm anomaly in the arctic were a precondition to pollutant transport into it (in the cold season). I think this is not true. Air masses from lower latitudes may be advected into the arctic, bringing at the same time pollution and warmer temperatures. Or in other words: While I would agree that a very cold atmospheric boundary layer indicates that the affected area has not been influenced by non-arctic air masses (during air-mass lifetime), I would not think that it prevents the transport of such air masses—at least not for simple thermodynamic reasons. The stability of the dome of arctic air rather is the result of large-scale circulation patterns. These circulation patterns are certainly influenced by the temperatures in the arctic, so that a certain feed-back mechanism is possible, but there are also other influences, and it could well be that the case studied by the authors is such an example where arctic air masses have been displaced by moderate air masses.

5. Unfortunately, in the relatively young field of receptor-oriented transport and diffusion modelling, we do not yet have an established terminology. I found the following terms used by the authors in this context:

   - retroplumes (5x)
   - potential emission sensitivity (PES) function, PES footprint, and similar (11x)
   - footprint layer (1x)
   - footprint emission sensitivity (1x)
   - potential source contribution (PSC) (2x)

   Retroplume is used also by several other authors, and to my understanding it has no physical meaning and is used in a visually appealing sense for the plume formed by the backward-in-time transport of an adjoint tracer. Used in this sense, I have no objections. *Footprint* is a term that has unfortunately been established

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already by micrometeorologists, but it is absolutely non-sense. Firstly, it is not the measurement but the emission which leaves an imprint (not "foot"print) in the atmosphere, and secondly, any imprint will be transported downstream and not upstream. Let us keep this word out of air pollution meteorology! The quantity called *PES* by the authors already has an established name: *source-receptor relationship*. Wotawa et al. (2003) have augmented this phrase to the more intuitively understandable *source-receptor sensitivity*. Other wordings that have been used include *field of regard*, *field of view*, *illumination function*, *influence area*. None of them introduces *potential* as a part of their wording. On the other hand, phrases such as *potential source contribution function* have widely been used in statistical evaluation of trajectories, where they denote a more or less rough approximation to the source-receptor relationship, hence the usage of *potential*. I think it is not helpful to introduce a very similar wording here, and even more so for what the authors call *PSC*, as it designates not only a potential, but rather an actual contribution of a source to a modelled receptor value. It is admitted that a modelled contribution does not necessarily mean also a contribution in the real world, especially if relevant processes such as chemical transformation have not been included in the model, but I am afraid the wording selected here rather contributes to confusion than clarity. I am also not fond of the copious use of shorthands such as PES or PSC instead of plain language. A phrase such as *PES footprints of the retroplumes* (p. 9670, l. 27) exemplifies where we can get if we don't pay proper attention to nomenclature.

6. I don't know the paper quoted as Stohl et al. (2006), but at least the present manuscript does not sufficiently corroborate the claim of the conclusion in l. 23 on p. 9686 that biomass burning has been underestimated as a source of aerosols and trace gases in the Arctic. Firstly, the present manuscript does not include a comprehensive review of the existing literature with respect to this issue, and secondly, it presents only a case study and thus does not permit a general judgement

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on the relative importance of different sources.

7. The paper is rather long and it includes a large number of figures. I think it could be shortened somewhat without taking away essential information. My suggestions are as follows:

   - There are some repetitions or unnecessary lengthy formulations in sections 1 and 2. Figure 1 could be omitted.
   - The presentation of analytical methods and instruments could be compressed into a compact table including the references.
   - Section 6 is rather detailed and could also be shortened. Fig. 6 could be omitted (maybe replaced by a hint on available on-line weather maps).
   - Section 8 is too long compared to its scientific content. Fig. 24 could be omitted.

8. The paper makes abundant use of abbreviations. Please try to reduce their number for the sake of the casual reader, and avoid them in the abstract, the conclusions and the figure captions.

9. I am not a native English speaker, but I think that the word *enhance* carries a positive notion and would normally be used for something desirable. It seems thus not suitable to describe elevated concentrations of pollutants (similarly, nobody would speak about *enhanced morbidity*).

10. Where data are openly available, it would be good to give a hint where and/or how they may be obtained (now, only indicated for EMEP).

11. I am wondering if EMEP CO emissions are indeed limited to fossil fuel burning, or if they include also emissions from sources such as waste incineration and fuel wood.

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12. In Section 5, it is not stated how long particles were tracked in the forward simulations.

13. Source-receptor relationships are reported in the text in units of ps kg^{-1} and in the figures as ns kg^{-1}. I presume this means picoseconds and nanoseconds, respectively. As this is a rather unusual unit, it might be helpful to express it as 10^{-12} s kg^{-1} or similar, or at least to explain it once, and to use the same units in text and figures.

14. In Figure 5, I am wondering if the density of fire counts may be so high that the coloured dots overlay each other and the visible colouring may thus not represent the real land-use distribution of the fire counts. I am also wondering what happened to the land use class 5 (mixed forests).

15. In Figure 7, I find that the AOD lines are not sufficiently visible. I would prefer a side-by-side plot of tracer concentrations and optical depths.

16. In Figure 9, the numbers indicating the daily intervals are hardly visible. Put them into a light-coloured box.

17. Figure 11 is not of publication quality, it has been compressed too strongly with a lossy compression method.

18. Why is the CO record plotted in light gray in Figs. 12ff. instead of a well-visible colour like, e.g., black?

19. The time series plots in Figs. 12ff. would benefit from plotting a 1-day grid raster.

20. The complete itemisation of the conclusions is not necessary.

21. Related to the recommendation to ban agricultural waste (*waste* or above-ground parts of plants?) burning, I agree that the pollution caused is a good argument for a ban, but such a decision certainly involves further judgements.

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Reference

Gerhard Wotawa, Lars-Erik DeGeer, Philippe Denier, Martin Kalinowski, Harri Toivonen, Real D'Amours, Franco Desiato, Jean-Pierre Issartel, Matthias Langer, Petra Seibert, Andreas Frank, Craig Sloan and Hiromi Yamazawa (2003): Atmospheric transport modelling in support of CTBT verification – Overview and basic concepts. *Atmos. Environ.*, **37** (18), 2529-2537.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9655, 2006.