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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.

A. Stohl et al.

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We thank Petra for her very positive assessment of, and the many comments on our paper. In the following, we repeat her comments in italics and use normal font for our responses.

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.

Petra is right that the word “record” should not be used in a scientific paper without a good reason and/or quantification. Ozone and aerosol optical depth are the two parameters that have the longest data record at Zeppelin. The episode in spring 2006 was completely outside the normal distribution of these parameters. For instance, for ozone the previous record of 61 ppbv was exceeded by 22 ppbv. The 99-percentile hourly value in most previous years was below 50 ppb and in no year exceeded 54 ppbv. Thus, given that the episode value was so much higher than the highest values measured in the past, it does not seem possible to derive a reasonable return period using extreme value analysis, based on the 17-year data record for ozone. This, and the fact that most other measured species also exhibited the highest value ever measured, seems enough justification for using the word “record”. This does not mean that this record will hold forever, though. We believe it is also a result of a changing Arctic climate and in such a climate events like the one discussed may become more frequent. But this is another reason why return periods derived from data measured in the past may actually be misleading.

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

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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.

Korontzi et al. (2006) discuss the agricultural burning in this region (one of the most active worldwide, in this regard) in some detail, showing also that there are distinct peaks in the burning before and after the growing season. The farmers certainly have to wait for the soil to dry before the fires can burn efficiently. However, it seems that natural fires would need even drier conditions than the artificial agricultural fires and so are even less likely to burn so early in the year. Newspaper reports from the region confirm that widespread agricultural fires occurred in April/May 2006. The internet link we provide is to the main web page of "The Baltic Times". A search on their website for "fires" points to several articles in the time frame of interest. One of these reads:

Grass fires sear Latvian countryside

May 03, 2006

By Elizabeth Celms

RIGA - Five people are dead, more than 18,000 hectares of land have been charred and the forest fires continue as Latvia faces one of its most destructive grass-burning seasons in recent history. The situation has leapt so far out of control that the government was called on April 28 to find a solution. “The burning of old grass has grown into a plague that is costing the state dearly,” President Vaira Vike-Freiberga said. “By setting their fields on fire, people unleash a monster they are unable to control,” she continued, adding that farmers clearly don’t understand the nature of fire.

END QUOTE

We believe that practically all of the fires were set on agricultural/grass land but some got out of control and have burned non-agricultural areas, too. Whether the area thus affected was significant compared to agricultural lands is unknown, however, and a major source of uncertainty for the emission estimate. The link provided by Petra actually

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presents the same data (MODIS hot spots, shown in red) that we have used (e.g., our Fig. 5), so we see no need to provide an extra link.

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

Generally, there were not many clouds in the area in late April and early May. In fact, only the dry weather allowed the farmers to start the fires. We have tried to compensate for clouds during what seemed to be the most contaminated period, as explained in the paper. Overall, however, we do not think that clouds are the major source of uncertainty in our emission estimate.

Another issue is the fact that most of the individual fires do not burn for days but likely for a few hours only. Thus, many fires may have not been detected at all. We have assumed a rather large area burned per hot spot (see also our response to the comment of Kevin Tansey) and this shall also compensate for undetected fires in the general area of the burning.

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

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by the authors is such an example where arctic air masses have been displaced by moderate air masses.

It is an observational fact that the Arctic is much colder than most of the mid-latitude source regions and this simple fact, under present conditions, rules out warm emission regions as source regions for the Arctic lower troposphere. This realization was an important milestone in attributing Arctic Haze to certain source regions. This does not mean that the Arctic has to warm first, before pollution can be transported into it. Petra is absolutely right in saying that the warmth and the pollution can be advected together. However, under present climate conditions, such extreme warm air advection as it has occurred in spring 2006 is not typical.

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 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

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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.

The terminology in our field is indeed not well established. Unfortunately, different authors use different terms for the same quantities and there are arguments for and against most of these terms. Before justifying our terminology, we would like to say that this is not the right paper to establish or discuss the terminology in backward modelling. However, this discussion may lead to a clarification of the respective terms and, thus, contributes to establishing a terminology. The terminology we use was meant to be simple, accurate, concise, and appealing also to non-specialists. Since the main "customers" of our calculations are experimental people, we definitely want to avoid unnecessarily complicated terms. In fact, we followed much of Petra's terminology for some time (see, e.g., ICARTT website http://esrl.noaa.gov/csd/ICARTT/analysis/) but this was detrimental to our communication with the experimentalists, so we had to change something.

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- "Retroplume" as we use it is not an exact term but qualitatively refers to the main pathway by which an air mass has arrived at a receptor point. It is used in analogy to the word "plume" that is an accepted and widely used term in forward modelling.

- Footprint: We agree that this term is somewhat unfortunate and understand Petra's concerns. Yet another argument against this term is that it is generally used for a layer of limited depth (the depth can in principle approximate zero but counting statistics often require a quite substantial depth) and not to an actual "footprint", which implies a 2-dimensional imprint at the surface. On the other hand, this is an accepted term already and it also has its advantages. It is our experience that experimental people not familiar with backward modelling accept and understand this term. Before, we also used terms like "source-receptor relationship near the surface" but such terms not only require a full line of text but also cause people scratching their heads.

- Emission sensitivity: We think this term is at least equally accurate as "source-receptor relationship" or "source-receptor sensitivity". The word "relationship" is actually rather vague as it does not specify the kind of relationship. "Source-receptor sensitivity" is an awkward wording, as it implies a sensitivity to a source-receptor – but what is that? It is actually a sensitivity to emissions, thus our wording.

- Potential emission sensitivity: Since we use idealized tracers that are not removed from the atmosphere, the calculated emission sensitivity is an upper limit and, thus, only potential. If there are processes that remove the tracer (e.g., wet or dry deposition, or chemical reactions), this potential is not exhausted and, thus, the true emission sensitivity is lower than the potential one. When communicating with experimental people (who measure actual substances, not tracers), it is not a good idea to use terms that raise expectations that are actually not fulfilled by the modelling concept. By omitting the word "potential", we would raise such expectations.

- Source contribution: This is the mapped distribution of how strongly sources contribute to a modeled mixing ratio at the receptor, i.e. the product of the emission sensi-

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tivity and the emission flux.

- Potential source contribution: Again, the contribution is only an upper limit, as removal processes would reduce the contribution and, in the real world, the potential is never exhausted. We are aware of the term "potential source contribution function" (PSCF), which is used in some papers using trajectory statistics. However, this is a term used by a very small group of people and, thus, we see no reason to not use a similar term (PSC) for our purposes.

The phrase "PES footprints of the retroplumes from the Zeppelin station" is, admittedly, not fortunate and we will think about a better wording. However, in Petra's terminology, one would need to call it "Surface-layer source-receptor relationship of the backward calculations from the Zeppelin station" (or similar), which is also not a good example for a clear terminology.

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

We agree with Petra and will change the wording from "clearly show" to "suggest".

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.

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- 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.

- Figure 1 is important, as it shows the warmth that accompanied the air pollution, so we will keep it. However, we agree that sections 1 and 2 can be shortened. We will remove one sentence from section 1 and the final paragraph of section 2.

The "Observations" section is less than 2 A4 pages long (including site descriptions) and will be even shorter in the final ACP format. Since the type of observations (satellite, in-situ; trace gases, aerosols) are very different, it is not straightforward how to put all into one table. Therefore, we will leave this section unmodified.

We agree that section 6 is rather long. We managed to shorten it by about one paragraph but will keep the synoptic chart, as it is important for the understanding of the meteorological situation.

We disagree on section 8. Albedo effects of aerosol deposition are currently an extremely hot topic, also because of a lack of measurements that models could be checked with. Thus, we think that these observations, though quite limited, are particularly important!

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.

We agree with Petra and will remove abbreviations from the figure captions and the conclusions (there are none in the abstract that are not defined there).

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).

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Thanks for the hint. We will correct this to elevated, or similar.

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).

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.

Petra is right that the EMEP CO emissions are not only from FFC – they also include biofuel burning, waste incineration, etc. We will keep the terminology since FFC is the dominant contribution but we will explain that this also includes other sources.

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

Right. We will add a sentence that the particles were tracked for 20 days.

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.

We agree and will change this such that only nanoseconds are being used. We will also write out "nanoseconds" upon first use.

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).

Yes, indeed, in some regions the colored dots overlay each other. However, we have experimented with different plotting sequences, which showed that the color distribution is roughly representative of the landuse distribution. The land use class 5 was also combined with 1 and 4, which we forgot to mention – this will be changed.

In Figure 7, I find that the AOD lines are not sufficiently visible. I would prefer a side-

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by-side plot of tracer concentrations and optical depths.

Figure 7 is, admittedly, the result of a compromise between reducing the number of figures and clarity. However, the lack of visibility of the AOD isolines is also a result of the ACPD format, which allows only half a page for each figure (including the caption). We expect this to be much improved in the final version, where this figure should be approximately twice as large.

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

We have changed the font, such that the numbers will be much clearer in the final version of the paper.

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

We will try to improve the quality of this figure but so far had difficulties in enhancing is quality.

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

The CO is repeated from Fig. 10 (where it is shown in black) in each plot to allow comparisons between the plots. However, the new information is more important and, thus, we wanted the CO lines to be more in the background - thus, the choice of color. We experimented with thin black lines but found this to be less satisfying.

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

Probably. However, in some figures we have also plotted other vertical lines (marking extreme values in Fig. 12, or defining periods in Fig. 14) and these figures would become quite crowded, with additional vertical lines plotted every day. Therefore, we will keep the figures unchanged.

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The complete itemisation of the conclusions is not necessary.

This is a matter of taste. We find it an efficient way to separate the individual conclusions on very different topics. It would not be so easy to separate these independent points in a flowing text.

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.

This is true. We will rephrase to "Given...., banning the practice of agricultural waste burning should be seriously considered."

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