

Interactive comment on “A global emission inventory of carbonaceous aerosol from historic records of fossil fuel and biofuel consumption for the period 1860–1997” by C. Junker and C. Liousse

C. Junker and C. Liousse

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We would like to express our sincere thanks to all of the reviewers, who with their detailed comments and suggestions significantly contributed to the improvement of our work. The subject of the article, our historic BC/OC emission inventory 1860–1997, has also been improved and updated since the time of the review. In the following we quote the original comments of the three reviewers, each one followed by a brief description of how we addressed the respective issue. In some cases one response is given of a group of similar comments. Carsten Junker & Cathy Liousse .

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General comments At first this paper seems to be an advance over previous work, and to contain some new approaches to a difficult problem. This could be a worthy effort with some interesting results. But after closer inspection, it seems that sufficient detail about nearly every method is not given here. This paper needs to be about twice as long to describe the work appropriately. If one follows the references for critical assumptions, they often lead to conference proceedings or unpublished work. Unless these methods could be better described, it seems to be mostly guesswork and not useful to publish. Probably the authors do have more support than they have given in this paper, so I encourage them to make it known to the reader.

__The manuscript has been thoroughly revised and extended by including
__additional material and a more detailed description of our methods.

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Specific comments Page 4898, line 26. For "their effects" probably authors mean "forcing"-I doubt that abilities to scatter and absorb change cloud condensation nuclei, for example.

__The sentence has been changed.

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Page 4899, line 18. Novakov et al (2003) did not assume that the BC/CO₂ ratio in the UK was representative for the entire world and for all burnt fuels. Though limited, they did apply different emission factors for different regions, different fuels, changing with time.

__The text has been modified.

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Page 4890, line 14 and onward. Both Novakov et al (2003) and Bond et al (2004) discussed utilities (power generation). Is this included in your industrial use?

__ Yes, even though the proportion of power generation within industrial

__ fuel usage is not explicitly given, the EF in the industrial sector as

__ defined in this inventory includes power generation.

__ This has been made clear in the text now.

Page 4901, lines 15-17. Are you suggesting that technology was actually better in developing countries before World War II? Would the colonializing countries not have left their technology behind? Probably the colonizers were also subject to resource limitations. This statement seems condescending- suggesting that some developing countries were better off before the colonizers left.

__ Today's developed countries are only considered semi-developed in colonial

__ times. Thus the colonised countries were not better off. However, in order

__ to avoid misunderstanding, all references to colonies have been removed.

Page 4902, lines 9-11. Proportionality factor isn't described, and it is not apparent from the figure. How do authors account for decrease in per-capita biofuel usage as described by Ito and Penner (2005)?

__ More detail on the proportionality calculation has been given in the text.

__ Also the error committed for developed countries (representing about

__ 14% of the world population) by not accounting for the decrease

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___in per-capita biofuel usage is mentioned now.

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Page 4903, discussion of Table 1. Are EF values for total PM which is BC and OC? This should be made clear. The values for diesel in developed countries, look like the BC emission factors from Cooke et al (1999). But some values such as hard coal do not look like either BC or PM from Cooke et al (1999). Where did these values come from? Even though there are only few emission factors here, they are inconsistent with authors' previous work. This is confusing. Fig 4 and Fig 7 seem to show BC emission factors and these are mostly consistent with Table 1. Then, if Table 1 is BC emission factors, where are the OC emission factors?

___Reviewer withdrew this comment.

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Page 4903, line 6. Liousse et al (2004) is conference proceedings. Why could the data not be published here? Otherwise there is no way to understand why it should be reduced. Page 4903, line 7. How much is EF for domestic use decreased and how much is industrial increased? Some brief text about why this change occurred should be given, even if authors plan to publish it later. Page 4903, line 10. Again this relies on unpublished data, as reported by the first reviewer. Actually how is refinery oil burned? This description is vague, and does not correspond to UNSTAT definitions. It could mean to any type of oil from light to heavy. It is also not clear why domestic use is thought to have 10 times lower emission factor than industrial use.

___The revised version of the manuscript contains now a detailed explanation

___of our choices concerning emission factors.

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Page 4903, lines 17-18. I don't think this could be correct. Bond et al (2004) seem to

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publish a lot of references for BC/TPM ratios, see Table 5, Table 7, Table 11, and also discussion on choice of different BC ratios.

__The reviewer is right in that references for BC/TPM ratios are given

__in footnotes to tables 5 and 7. We apologise.

__The text was changed accordingly.

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Page 4903, lines 18-21, the discussion of table 1. Are the values of this work and Bond et al (2004) supposed to be comparable? I could not understand what to compare.

__The comparison has been made clearer and the table is improved.

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Page 4904, lines 3-5. The authors differentiate the work of Cooke et al (1999) from that of Bond et al (2004) by saying that the sectorisation is different. But I do not really see the difference. If Cooke et al chose a value of EF for industrial combustion, it should be based on a measurement of industrial combustion. Then if Bond et al chose a technology representative of industrial combustion and used that EF, that result should be about the same. Also authors refer to a "technology factor" which is the terminology used by Novakov (2003). This is a method of decreasing emission by assuming the technology is improving at some rate. This approach isn't the same as used by Bond et al (2004) who did only present-day and not time-dependent emissions. This could be confusing. Page 4904, lines 5-6. Again see my comment above, regarding BC/TPM ratios. An uncharitable interpretation might suggest that authors have not carefully read Novakov and Bond papers, but perhaps only the presentation here is not well done.

__A more differentiated comparison of the hypotheses of the two studies

__Cooke et al. (1999) and Bond et al. (2004) is now given.

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Page 4904, lines 11-12. This is an interesting approach. Authors should indicate what level of GDP was chosen as the breaking point between developed, semi-developed, developing, and why this was chosen. Also what GDP (adjusted to what year) and whether it was in PPP. What was the source of GDP? Did this vary with time so that countries became developed as GDP raised above a certain level?

__The requested details are provided now.

__The change in country classification with time is also explained at

__another position in the text (i.e. no "developed" countries prior to 1939).

Page 4905, lines 10-13. This is also an interesting approach and if it is correct, it will be very useful. However there are no details about how the relationship between BC emission and efficiency was derived. Are they based on measurement of actual BC and efficiency together? If not what assumptions are made? BC emissions could vary by orders of magnitude (as authors themselves point out) while efficiency will vary by only a few percent, so how could this very sensitive relationship be obtained? I hoped to look up Pertuisot reference but find that this is a dissertation. Possibly the results were not published in peer-reviewed literature and authors could not cite it. If so then a further description must be given here. It isn't sufficient to present the relationship as if it has already been examined and approved by the community.

__Our method is described in detail now in the text,

__and the reference to Pertuisot has become redundant.

Page 4905, line 24, change of diesel emission factor. This is not a very important comment but I wonder why the industrialized countries change with a straight line and

the developing countries have a curved change in emission factor.

___This has been corrected.

Page 4906, line 5. Why was 1939 chosen as the division of a country's performance? Does this not create a significant discontinuity, if a country changes from 1.1 g/kg to 0.30 g/kg in a single year?

___The change from countries' development status during past periods is ___a difficult problem for drafting historic inventories.

___Novakov et al. [2003] assume that "Current emission factors for residential ___and commercial sources in developing countries are assumed equal to those ___in developed countries in the previous half century".

___Yet, any stepwise change in development status, be it in 1950 or 1940 is ___hard to justify. Ideally the transition should be continuous, and hence ___we are currently developing such an inventory. While awaiting this product ___it seems reasonable for us to choose the time of World War II for ___the transition since world politics and with them also world trade and ___world economics underwent a drastic change during this period.

___Furthermore, the discontinuity created by the stepwise change of EF ___for developed and semi-developed countries is buffered by the fact that ___no data is available for the years of 1940 to 1945.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4897, 2006.

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