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> Interactive Comment

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

Anonymous Referee #3

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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 If one follows the references for critical assumptions, they often lead to conference proceedings or unpublished work.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



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the authors do have more support than they have given in this paper, so I encourage them to make it known to the reader.

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.

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.

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?

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.

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)?

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

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factors, where are the OC emission factors?

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.

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

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.

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

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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.

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?

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.

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.

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?

Page 4906, line 10 and onward, and figure 5a. This seems really arbitrary. Not only

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have authors extrapolated from period 1970-1997 back to 1860– over 100 years!– but there is obviously a significant discontinuity in the data around 1985. Such data shouldn't be used in this way.

There is a factor of 10 difference in the emission factor of coal, between industrial and residential. For this reason the authors are justified in saying that some sectorisation is needed. However this unreliable extrapolation does not seem like a good method for accomplishing it. Perhaps the correct data are not available. In that case one should not argue that the work is an advance because it considers sectorisation.

Page 4906, line 24 and onward, and Figure 6. I also agree that this separation may be necessary but again I think that the extrapolation method is poor. Authors say that their method leads to slight overestimation. This is an understatement. It appears that there are 4 or 5 data points from the U S EPA data from 1950-1965 and not only the values but also the form of the curve is extrapolated beyond this short period.

Page 4907, discussion of Fig 7. What is the difference between Fig 7 and Fig 4 which cover the same time period and some of the same fuels? How was Fig 7 derived? No details are given in the text. I assume that crude oil emission factor was applied to everything that is not diesel?

Results, general comment. Now BC and OC emissions are presented. But there has not been any discussion of OC emission factors except for the biofuel. There were some changes to BC emission factors and others were not comparable with Cooke et al. (1999), their supposed source. If such changes were made then the OC emission factors should also change. Was the change proportional to the change in BC?

Figure 6. It is useful to have this comparison. I think the figure would be more clear if the first curve was labeled "calculations based on fuel data of Etemad et al" and so on. It was not immediately clear that the first two curves show BC emissions from this work.

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Page 4909, lines 9-10. I think it is strange that authors' present-day fossil fuel BC emissions are lower than those of Novakov et al (2003), since the latter authors used the Cooke et al (1999) emission factors, so they should have got the same result. Or did the change in emission factors for this work affect the total emissions so much?

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

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