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Interactive comment on "Sulfur dioxide and primary carbonaceous aerosol emissions in China and India, 1996–2010" *by* Z. Lu and D. G. Streets

Anonymous Referee #1

Received and published: 13 September 2011

General Comments: The paper addresses the development of the emissions of key aerosol species in two largest and fastest growing developing economies. It covers the period of fast economic growth and significant environmental policy changes. The authors provide an excellent overview of all key contributions in this area over the last decade adding their own analysis drawing on their previous work as well as on findings of other groups. Their revised time series are accompanied by a well designed and described uncertainty analysis and comparison of trends with most recent remote sensing data. The paper is very well written. I find this a very useful piece of work.

Specific comments:

Page 20271, line 10-11: As a matter of fact the Monte Carlo method has been in use for a while and was also recommended by IPCC in their 1996 guidelines. I think this

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particular sentence could be deleted.

Page 20273, line 15: 'fine PM' does not necessarily have a uniform definition, i.e., in some regions is typically referred to PM2.5, in others even PM10, while here to PM1; suggest to add explicit statement

Page 20295, line 22-27: In such comparisons one would need to verify if the assumptions about the reduction efficiency are the same as otherwise penetration rates are not comparable. Are reduction efficiencies in the compared studies similar?

Page as above, line 25: 'real situation', What makes the authors believe that this time they describe real situation? Several previous studies, also their own, did not quite nail it.

Page 20304, line 21-24: It might be worth adding that some of these parameterizations and so associated uncertainties do not change much over time while others do.

Several pages and graphs where discussion of trends is carried out, specifically for BC/OC: I am a bit puzzled by small or virtually invisible effect on BC of introduction of coal briquettes which are first claimed to reduce emissions but then in Table 1 the residential coal in China does not seem to decline at all but grows, is it owing to some industrial use? But then which is the industrial coal source that grows? In general, I miss few lines of discussion where the industrial use of coal in small and large boilers as well as in brick kilns is briefly discussed, pointing to lack of measurement data and large uncertainties. The second issue is coke manufacturing where a dramatic change in manufacturing technology is claimed assuming that by 2010 all traditional coke manufacturing plants are gone [strong assumption which i guess is backed up by existing laws but i wonder if there is published evidence on enforcement]. Assuming such a big change and presumably much lower emission factors for the new process assumed [although i am not aware of any actual BC/OC measurement on a Chinese coke plant] I am a bit surprised not seeing any change in the industrial emissions of BC; was the contribution of coke small in the first place?

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Technical corrections:

Page 20270, line 14: consider changing 'monthly fractions' to 'monthly temporal distribution'?

Page 20281, line 29: 'bonds' or rather 'bounds'?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20267, 2011.