

## ***Interactive comment on “Spatial and temporal variation of emission inventories for historical anthropogenic NMVOCs in China” by Y. Bo et al.***

**Anonymous Referee #1**

Received and published: 23 June 2008

I have a number of important comments on this paper that I believe should be attended to by the authors. In general, I believe the authors go too far in trying to promote their own work by casting doubt on previous work. In fact, their methodology is very similar to previous work and has few improvements. However, their results are significantly lower than all other estimates, and this paper can only be accepted if they can explain why their results are an improvement over all previous studies. On Page 13, they attempt to do this by saying "Other estimations were all quite larger than ours, which was caused by differences in studied region, source categories and emission factors adopted." This is not an adequate comparison with the work of other researchers.

First, we should observe that the two most recent and reliable studies of NMVOC emissions in China are not even mentioned. These are the TRACE-P inventory (Streets et

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



al., JGR, 2003), which has been used widely in air quality modeling both within and outside China, and the REAS inventory (Ohara et al., ACP, 2007). Next it should be mentioned that the TRACE-P inventory estimates that NMVOC emissions in China in 2000 were 17.4 Tg; and the REAS inventory estimates that NMVOC emissions in 2000 was 14.7 Tg. These values should be added to Table 11, where the present paper concludes that 2000 emissions were only 11.0 Tg. Thus, the two additional studies also support the view that NMVOC emissions in China are higher than calculated in the present paper. Finally, we might note that there has been widespread belief that emissions of NMVOC are underestimated in China, evidenced partly by the inability of models such as CMAQ to generate sufficient ozone around Beijing and other regional centers of China to agree with observations. All of these pieces of evidence make it necessary for the authors to demonstrate exactly why they think emissions are significantly less than the rest of the community thinks.

One might argue that the authors of the present paper have used better, different, or more local data. Indeed, they claim (Page 3): "these [previous] emission inventories were estimated by limited sources of Chinese data". Yet the reader will find that in fact there is very little difference between the data used in this work and previous work. The reader might hope to find that a lot of new emission factors have been included, based on measurements of Chinese sources, yet a detailed reading of the paper suggests that western sources like AP-42 were used extensively, just like in previous work. It is wrong also (same sentence on Page 3) to suggest that previous work revealed "little information of spatial variation of NMVOCs emissions." There are detailed, high-resolution NMVOC emissions maps in several of the previous papers. The only new thing about this paper, it seems to me, is the temporal variation; and that includes a number of heroic assumptions about time trends (take a look at Table 2, for example) that weaken the confidence in the year-to-year variation.

As a general summary, then, the authors claim too much for their paper, and need to do a better job of explaining why it is an improvement over previous work. They should

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

also respect the published work of other authors in this field. They are by no means the first to tackle the topic of NMVOC emissions in China.

There are a number of specific comments to make that would improve the paper:

1 Lines 35-40 should be omitted. This is not a paper concerned with the health effects of NMVOC and it is not necessary to justify interest in NMVOC on health grounds.

2 Lines 69-77. I don't understand why these three old and largely irrelevant publications are cited (and they are not databases). There are many more up-to-date emission factor databases that could be cited like AP-42, CORINAIR, etc.

3 Lines 110 and 114, "capita" is misspelled three times.

4 There is frequent misuse of English words and phrases: Line 42, explain what is meant by "an outstanding contribution", it is not clear. Line 57, do not say "a crushing force", replace with "became serious" or something similar. Line 265, not "tanglesome"; do you mean "congested"? Line 266, not "blossom" but "boom". Line 329, why is Guizhou called "a granary province"? Lines 381 and 828 and Table 11 should say "Hong Kong", and North Korea/South Korea or DPR Korea/R of Korea; "Corea" is not an English word. Also, Cambodia is preferred to "Kampuchea" these days. Line 390, "estimation" is misspelled. Line 455, "decennary" is not the word to use; "ten-year period" is correct. In several references (Cai, Li, Shao, Song, Streets, etc.) China, Beijing, etc. need initial capital letters. In the caption to Table 1, "proxy" is misspelled. No sources of emission factors are presented in Tables 8 and 9, and the text is not clear on this matter. In a number of places, "fossil fuel" is misspelled as "fossile fuel".

---

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11519, 2008.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

