

Interactive
Comment

Interactive comment on “A high-resolution emission inventory of primary pollutants for the Huabei region, China” by B. Zhao et al.

Anonymous Referee #2

Received and published: 12 September 2011

The authors developed a high-resolution emission inventory of air pollutants for the Huabei region in China for the year 2003. The emission inventory is essential and important data for atmospheric science community as well as policymaker and the topic certainly is suitable for ACP. The manuscript presents the sophisticated methodology based on region-specific activity data and emission factors, estimated results and the comparison with other inventories. The author's inventory has some advantages in the spatial distribution (high-resolution of 0.1 deg) and in the input data (emission factors by local measurements, information for large point sources, and activity data from local districts). However, this manuscript doesn't include the important scientific results and the new findings for the ACP publication. I regret to say that the current manuscript needs to be improved in two aspects at least to be qualified science paper in ACP: (1) demonstrating clearly the scientific advance in the improvement of emission inventory

C8839

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



due to the methodology and input data used in the work; (2) adding the more discussion of the comparison with other inventories and the implication of their differences. In conclusion, I am recommending the major revision of this manuscript in the following points.

(Major comments)

(1) One of the advantages of the author's work is the inclusion of estimation for the emissions from lots of LPSs (Large Point Sources). In general the LPSs have a large contribution in total emissions for SO₂ and NO_x in the area, while their contribution may be relatively small for other pollutants. I recommend that the authors demonstrate the contribution of LPSs in total emissions in Huabei for each chemical compound.

(2) Second advantage of this work is the use of region-specific emission factors from Jiang and Tang (2002) and others. What is difference between the author's emission factors and those in the previous works, such as INTEX-B, EDGAR, and REAS? In addition, the implication of differences should be discussed in more detail.

(3) In the section 4.1 the authors compared the author's inventory with other inventories. An important problem is that the base years for three inventories discussed in this manuscript are different. Actually, the pollutant emissions in China was increased dramatically from 2003 to 2006 (or 2005) shown in Zhang et al. (2009; ACP) for NO_x, Lu et al. (2011; ACP) for SO₂, and Lei et al. (2011; ACP) for PM. For this reason, the inter-comparison of emission inventories for different base year should be more careful. On the other hand, there are some emission inventories for the year 2003 or 2004 in provincial level of China (e.g. Ohara et al., 2007; Lu et al., 2011). I am recommending the comparison with these inventories.

(4) In the last part of section 4.1 the authors conclude that "these kinds of differences . . . , and could be caused most probably by different emission factors used in the estimates". The authors should discuss about the main reason of differences in more detail and in a scientific sense.

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

(Minor comments)

(1) Fig. 2: We can't find the emissions from power generation, oil and solvents, and manure which are shown in Table 11. Why ?

(2) Table 11: "Industril processing" should be "Industrial processing"

(3) Fig. 5: This figure is unclear. The authors should be improved to be more visible.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20331, 2011.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper