

Interactive comment on “A new approach to estimate pollutant emissions based on trajectory modelling and its application in the North China Plain” by W. Y. Xu et al.

Anonymous Referee #2

Received and published: 12 January 2012

This manuscript proposes a modified way of obtaining emission inventory information from particle trajectories. Results are presented using BC and CO measurements from a site in the North China Plain.

General comments:

This paper proposes a new method to use trajectories to evaluate emission inventories. Unfortunately, there are 2 problems: 1. the paper does not describe more recent developments which have been found to be useful, 2. the method is not convincing either in its formulation or its results. I will take each issue in turn:

1. See for example Stohl et al., ACP 2009 “An analytical inversion method for deter-
C14167

mining regional and global emissions of greenhouse gases: Sensitivity studies and application to halocarbons” and references.

2. I'm not sure that Eq. 3 is valid. The emissions are in units of concentration / hr which does not make much sense. The residence time based on a single trajectory seems to be rather arbitrary, and I cannot see how this equation improves on Ashbaugh et al.'s work. Eq. 5 introduces a fudge factor that lets you tweak the results so that they look like the input that you are comparing them too. By the time you get to Eq. 9, it seems you would be better off doing a sum of concentrations in each “cluster” and comparing those values, without any recourse to trajectories (the residence times might well cancel each other out mostly if you do the substitutions).

Basing the analysis on a single trajectory and calculating a life time based on when it exceeds an arbitrarily chosen boundary layer height seems rather perilous. I would recommend doing true particle trajectories (hundreds / thousands of particle releases), use WRF PBL heights, calculate residence times by the method of Ashbaugh.

Fig. 4 shows the results of the method. However, because the domain is much larger than the different source regions considered in the text, it is very difficult to see what is going on.

Overall, it seems that the study would have been better served by starting out with PSCF or CFA or some of the more recent developments. If the results from these were found to have specific problems, then the paper could show results from the modified method explaining how it improves on the previous methods.

Specific comments:

“energy statistics” – could you please be more specific about what you mean by this? It seems you are talking about energy consumption data.

The introduction has some inaccuracies in the description and could use some more recent references, including review papers. For example, there are recent papers that

evaluate the types of method that this paper would expand upon: Kabashnikov et al., Atmospheric Environment 2011 and Scheifinger et al., Atmospheric Environment 2007.

Pg 31139, line 2-9: The description of bottom-up and top-down approaches is incorrect.

Pg 31139, line 23-28: My impression was that PSCF is the name that Zeng & Hopke (1989) gave to Ashbaugh's method.

Pg 31140, line 10: Seibert's method, CFA, is a variation of PSCF.

Pg 31140, line 14: The method of Stohl et al., 1996 is mainly a development to make use of multiple sites.

The "clustering" is not really a clustering but a categorization by average direction. Using a clustering algorithm would give you clusters that reflect the dominant flow types based on the data itself. This would be a big improvement on what is described in the paper.

The choice of WRF domains seems a bit odd – the nested domain is nearly as big as the first domain. In the text you should include the input global data (GFS from NCEP at 0.5 degree resolution?)

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31137, 2011.