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# *Interactive comment on* "Weak-constraint inverse modeling using HYSPLIT Lagrangian dispersion model and Cross Appalachian Tracer Experiment (CAPTEX) observations – Effect of including model uncertainties on source term estimation" *by* Tianfeng Chai et al.

**Anonymous Referee #2**

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This study investigates the performance of a source term estimation method using data from the CAPTEX controlled release experiment. The interest in this experiment is that the source strength is known, as in OSSE's. However, unlike theoretical OSSE experiments real data are used, which allows assessing the role of transport model uncertainties and how to account for them. In principle this is all very interesting, however, the outcome remains on a very technical level. It is not clear what we learn here that was not known already. There is little justification of the error assumptions that



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are used. In OSSE's this is fine as long as the world is self-consistent (or deliberately not), however, the use of real data calls for a justification of what is assumed. Almost no attempt is made to test whether the statistics are self-consistent (e.g. chi-squares, biased residuals, etc.). Hardly any effort is made to interpret the results: how to explain them, and to what extent are they within expectation. Furthermore, no attempt is made to relate the outcome to what was done before. These aspects will need further effort to make this manuscript suitable for publication.

Abstract, line 12, 13: To me it seems that if the problem is linear, averaging outcomes of inversions using different models should lead to the same result as using the average model for in a single inversion. Differences are then due to non-linearity (e.g. using a logarithmic cost function)

Page 5, equation 1: The smoothing part of the cost function is included but not used. In that case just leave it out.

Page 10, section 3: The explanation of how you normalize the cost function comes only at the end. To follow the discussion preceding that point it would be clearer to move it to the beginning of the section.

Table 10, 11, 12: What is missing here is an estimate of the posterior uncertainty. Otherwise there is no references to compare the actual performance to the expected performance. Without this information it is difficult to judge how important model uncertainties are. Of course, the outcome will depend on the assumed flux and observational uncertainties. However, some discussion of the validity of the assumptions regarding those is needed anyway.

Page 17, line 14-15: How significant is the finding of logarithmic inversions giving better results? Looking at your results it seems to me that they may largely be explained by a few high measurements that the model cannot really resolve at the resolution that is used. The logarithmic cost function may allow more flexibility to cope with a few "outliers". This could also explain the dependence of your results on relative observa-





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tional error. Would this conclusion be different if you filter for data that the inversion has difficulty reproducing?

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