

Interactive comment on “Atmospheric inversion for cost effective quantification of city CO₂ emissions” by L. Wu et al.

Anonymous Referee #3

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Overall comments. The paper is an interesting observational system simulation experiment (OSSE) exploring observational network design for estimates of urban emissions of CO₂ using atmospheric inversions. As written, however, the paper has many serious problems. It is limited by a number of severe assumptions embedded in the inversion system, and by the lack of discussion of the vertical resolution of the atmospheric transport model. The overall conclusions regarding “cheap” vs. “expensive” sensors are invalid, and the alleged ability to deploy sensors at 25m AGL is misleading. The target uncertainties quoted are entirely dependent on the assumed prior uncertainties, and these uncertainty assumptions are often unjustified and untested. The discussion of the cost of measurement networks is unrealistic in the extreme and should be deleted. The manuscript contains a core of worthwhile research – an assessment of

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the sensitivity to a highly idealized inversion system to the network of instruments deployed - but the paper claims to be much more than this. This manuscript requires serious revision before it should be considered for publication. More detail on these points follows.

1. The argument concerning “cheap” vs. “expensive” sensors is misleading, lacks content, and must be deleted. The authors state that transport errors are larger than instrument errors, thus instrumental error doesn’t matter, and thus “cheap” sensors are just as good as more expensive CO₂ sensors. The authors, however, present no quantitative assessment of any real “cheap” sensor or atmospheric transport errors. The abstract makes it sound like low-cost sensors exist and have been tested and have been shown to perform well. This is wrong. Unjustified assumptions have been made so that sensor performance is irrelevant. This isn’t science; it is wishful thinking. Wishful thinking should not be published in ACP.

2. The results concerning performance of the inversion system as a function of the number of sensors is defensible within the limits of the many assumptions made by the inverse system, including the uncertainties assumed within the inversion. These assumptions, however, are buried deep in the document and in sections that are often very difficult to read. The assumptions include prior flux errors, atmospheric transport errors, and assumed coherence in the prior flux errors. Very large coherence is assumed in the prior flux errors. The final error levels are highly dependent on these largely unjustified assumptions. As the authors state, deep in the discussion section, these results “should not be over-interpreted.” But the abstract says nothing about the numerous assumptions that limit the validity of these results, and states that 5% flux uncertainty can be achieved with 70 sensors...with no caveats given whatsoever about the large volume of assumptions that condition this finding. It is even difficult to determine from the abstract that this study is an OSSE. 2.1) These limiting assumptions should be presented prominently and clearly in the methods section. 2.2) These important caveats about the significance of the study results should be made clear in

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the abstract. The abstract is very misleading and should not be published in its current form.

3. The method of relating CO₂ mixing ratio differences to fluxes is not clear, and is of critical importance to the paper. The method of choosing upwind and downwind sites is described, but how these are related to emissions of CO₂ is not described.

4. The paper is based on a pseudo-measurement network that, as noted in the abstract, collects data at 25 m above ground. The study is based, however, on an atmospheric model that has very coarse resolution – 15 km in the horizontal and, as best I can determine, about 250 m in the vertical. The model has no demonstrated capacity to represent the complexity of an urban surface either in the vertical or in the horizontal. An OSSE is limited by the quality of the modeling system applied to the system design. The authors have no basis for claiming that their results are valid for an observational network deployed at 25 m above ground with a model that has no demonstrated capacity to resolve the details of atmospheric transport in the environment of interest. If the authors must 4.1) discuss the vertical resolution of the model; 4.2) describe how the model simulates the atmospheric surface layer; 4.3) explain the true limits on observational altitude in their study given 3.1 and 3.2; and 4.4) at a minimum note how the complexity of the urban surface in the horizontal, unresolved by their modeling system, could complicate the meteorology in ways that cannot be captured by their modeling system.

5. The manuscript needs further editing. It is full of detail that is hard to follow and at times extraneous to the central message of the paper. The figures are out of order, and often the figure quality is marginal. The writing quality is poor and must be improved before this manuscript is suitable for publication.

6. The economic justification for “cheap sensors” contains a great deal of unjustified wishful thinking. For example, Appendix B states that,

“The cost of calibration is estimated to be of the same order for high precision and

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cheap sensors. The calibration for cheap sensors can be more frequent (e.g. two days) than for high precision sensors (e.g. one week), but needs less samples of calibration gas. In addition, innovative calibration procedures for cheap sensors are possible for further reductions of the calibration cost and the temporal correlation in instrument bias. For instance, a calibration center can be set up using high precision sensors to calibrate cheap sensors. One can manage two sets of cheap sensors: one in the calibration center and the other in situ in measuring. The calibration is simply performed by replacing the measuring sensors with recently calibrated ones from the calibration center. Since this new calibration method is free of calibration gas, and since the cost of replacing sensors is very limited, one can maintain a high frequency of calibration (e.g. daily). Note that the network cost can, furthermore, be reduced when pre-existing infrastructure is available, for instance the installation could be free of cost if sharing with existing air quality monitoring platforms.”

The authors are thus proposing that a 70-instrument network sprawling across a large metropolitan region would have all of the instruments replaced every 1-2 days. The cost, however, is cited to be “very limited,” and Table B1 shows no added cost for personnel for replacing 70 instruments every day. I would expect that such a schedule for instrument replacement alone would take 2-3 full time personnel. Further, Table B1 assumes that 70 free platforms with suitable characteristics for monitoring greenhouse gas emissions are available! This discussion is 1) unrealistic in the extreme and 2) unsuitable for publication. This unrealistic and misleading attempt at evaluating the economics of observational systems must be deleted from this document. It has no scientific value that I can discern.

Detailed comments.

1. Page 2, Lines 13-15. What sensors are “currently developed?” As best I can tell, no actual sensors are evaluated. This text is extremely misleading and must be modified to represent the actual content of the paper, which assumed instruments with no bias and insignificant random error exist.

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2. Page 2, Lines 13-15 What defines expensive? What defines a “megacity?” Is this different than the cities that emit 44% of global CO₂ emissions?
3. Page 2, Line 17. “25 m above ground level.” Why are the imagined sensors located at 25m above ground? Can this altitude be treated realistically in the inversion system? This is a strong recommendation that is not justified by the content of the paper. This must be carefully justified by showing that the model can simulate measurements collected at 25 m AGL or deleted.
4. Page 3, lines 5-12. These statements are not justified or quantified, thus not useful.
 - 1) Certainly additional measurements such as CO might improve an urban inversion, but this is not a new result. This paper adds nothing to the body of literature on this topic. Without new results, this should be deleted.
 - 2) The statement that “cheap” sensors can improve urban emissions estimates says nothing about the quality or characteristics of the so-called “cheap” sensors. Sensor performance should be quantified, or this text should be deleted. This is wishful thinking, not a conclusion from any research performed in this manuscript.
5. Page 3, line 26 – page 4 line 3. This is a run-on and confusing sentence.
6. Page 4, line 4. What is the “city mitigation potential?”
7. Page 4, Line 6. English needs work.
8. Page 4, line 29. economics.
9. Page 6, line 10. “required qualities”? What are “qualities?” Please be more precise.
10. Page 6, line 14. Involves.
11. Page 6, line 27. The use of continuous CO₂ measurements to monitor urban emissions is far from a new idea. Please do not claim that this is a “new type of data.”
12. Page 8, lines 3-7. The authors do not employ an economic model to determine the costs of MRV vs. atmospheric inversions. They simply take the costs of these systems

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today, and make many unrealistic assumptions about these costs. Costs are not fixed, and today’s costs should not be used to plan tomorrow’s monitoring systems. Further evaluation of this text (Appendix B) reveals many other problems, noted above in point 6 of the overall comments.

13. Page 8, line 10. “are currently developed.” If they are currently developed, please provide some citations that describe the performance characteristics of these sensors. Some imagined sensor characteristics are described in Appendix A, but without any evidence of the realism of these claims.
14. The introduction has a long discussion of greenhouse gas emissions targets and issues, but presents little insight into the performance of existing urban inversions.
15. Page 9, lines 17-24. This text needs considerable editing. It is very difficult to understand.
16. Page 9, lines 23-24. What are “city inventories that would not have access to the same level of information as national inventories.”?
17. This entire paragraph on “notional targets” should be simplified and clarified.
18. Page 10, line 25 – page 11, line 2. I believe these are hypotheses, not statements of fact. Please clarify. If they are facts, please include appropriate citations.
19. Page 11, lines 2-3. I don’t understand this sentence.
20. Page 11, line 8. reducing the reduction?
21. Page 11, line 4. What is the purpose of this paragraph?
22. Page 11, line 14. This paragraph is very difficult to follow and requires significant editing. Please explain the methods and assumptions clearly.
23. Page 12, line 11. I don’t understand the purpose or content of this paragraph.
24. Section 2.2. Notional costs. This section of the paper has serious problems.

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There is little information that serves as the basis for the cost of conducting an urban emissions inventory of a given accuracy. There are questionable assumptions about the cost of an atmospheric inversion (e.g. cost of sensors is the primary cost). The assumption about “cheap” sensor accuracy and precision makes the distinction among sensors meaningless, but there is no actual evaluation of any sensors. There are no assessments of actual transport errors. Assumptions about costs made in Appendix B appear to be extremely unrealistic. The claim that this study examines the benefits of low cost, poor performance vs. high cost, high performance sensors is false and should be eliminated from the paper. The assumptions about the costs of inventory vs. inversion are also highly questionable and should also be deleted.

25. Section 3.1. This introduction to the mathematics needs to utilize terminology that is specific to an urban atmospheric inversion. A “background” estimate of what, for example? Observations of what? The theory is not new. The application must be clear.

26. Page 15, line 8. “control a vector x ?” What does that mean?

27. Section 3.2 The terminology in the section “control variables” should be replaced with physically meaningful terms.

28. Page 16, line 14-15. I do not believe that computational constraints are a primary limit on the resolution of the inversion. Either modify this discussion or provide a citation that demonstrates this claim.

29. Page 16, line 27. “rest” is an unfortunate choice, since it has another meaning. “Remainder” would be better.

30. Page 17, line 1. Again, why are computational constraints invoked? What computational constraints? It is entirely possible to resolve an urban region at high resolution given current computing resources. This is not a real limit on urban inversion systems. The true reasoning for this coarse spatial resolution should be explained. The

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atmospheric transport resolution applied for this study is exceptionally coarse.

31. Figure 2. The regional colors and regional boundaries are not clear. There are lines on the map that do not correspond to the colors. What are the regional boundaries?

32. Page 17, line 18. Nordbo et al (2012) reported no flux tower measurements that were carbon neutral. Every observational data point in their paper reported a net annual carbon source to the atmosphere. The paper cannot be used to justify that urban areas are carbon neutral. (Nordbo et al (2012) also referred to Minnesota as a city, and used 500 m resolution data to derive urban fraction for flux tower sites.)

33. Page 18, lines 4-5. Please define afternoon and high wind speeds. The details are important.

34. Page 18, line 3. How are upwind and downwind sites defined?

35. Figure 3 caption. “uniform” not unifrom.

36. Page 19, line 3. What is the purpose of random selections of networks? It isn't likely that networks will be determined via a random process.

37. Page 19, line 6. Figure 5 is referenced before Figure 4.

38. Page 19, lines 13-15. How does sampling at 25m above ground, “avoid dominant influence of local emissions on concentration observations?” I don't know of any published work that shows that 25m is high enough above the surface to avoid being dominated by local emissions. I don't know how “avoiding dominant influence” or “local emissions” are defined. See the 4th main comment above. This is an unjustified and highly misleading claim that should not be published.

39. Page 19, line 18. I cannot understand what the authors are trying to say about H1. Please rewrite in clear language.

40. Page 20, line 7. Is a 15km resolution ecosystem flux model appropriate for an urban scale study? This seems exceptionally coarse.

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41. Page 20, line 28. Figure 4? The figures are out of order.
42. Page 21, line 16-18. What is the area covered at 2km x 2km resolution? There are 2km x 10km grid boxes? Why?
43. Page 21, H2: The transport model is only a 15km resolution model with 19 levels up to 500 hPa? This is very coarse resolution. Each vertical level, if evenly spaced, is approximately 250m. It is not unusual for coarse resolution models to have very unrealistic surface layer behavior when they are applied to CO₂ simulations, and a 15 km horizontal resolution model cannot take into account realistic structures in the urban surface energy balance and changes in urban roughness. How can this model be used to evaluate the suitability of measurements 25 m AGL over the highly complex urban surface? What is the profile of CO₂ close to the surface? The lack of description of the fidelity of this model for this task is a major weakness of the document. The OSSE is only as good as the model used for the OSSE. No relevant model evaluation is presented in this document.
44. Page 21, lines 25-27. I don't understand "depending on the simulation. . ." Sometimes you have initial and boundary conditions, and sometimes you don't? Please clarify. How are boundary and initial conditions optional? What determines whether or not you include CO₂ boundary and initial conditions?
45. Page 23, line 1. What does "read from the ECMWF meteorological product" mean?
46. Page 23, line 11. Why 22.5 degrees? Is there any justification for this value? Plume dispersion widths will vary with wind speed, wind shear and turbulent intensity. What is the origin of this fixed value? What limitations does this fixed value place on the results of this study?
47. Page 23, line 13. Is that 7-16% of observations once the afternoon hours have been selected? Or is that 7-16% of the total number of possible observations?
48. Figure 6. The wind rose graphics are too small to read.

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49. Page 23, line 21. This paragraph is incomprehensible.
50. Figure 7. Why are any differences that are not in the afternoon hours displayed? They are irrelevant to this OSSE.
51. Page 24, lines 5-9. A modeling system with 250m vertical resolution will have difficulty representing mole fraction differences at 25m above ground at any time of day. Figure 7 displays time series and differences that might be seriously influenced by the ability or inability of this modeling system to represent vertical mixing very close to strong sources and sinks at the earth's surface. Evaluation of the near-surface vertical profiles created in the model is essential to ensure that these results are not simply artifacts of unrealistic surface layer mixing. The quantitative horizontal gradients (the focus of the following paragraph) are very dependent on this vertical mixing.
52. Page 25, line 16, delete "can". "Even though a few cities. . ." Why is this relevant?
53. Page 26, line 2. I do not see how Figure 9a illustrates the point being made in the text. This needs significant work. What happened to figure 8? I see that Figure 9 is a correlation matrix, but no dimensions are described. As presented, this is nearly incomprehensible. It is very good that the authors are trying to explain these critical assumptions, but the presentation is not sufficient to understand the assumptions.
54. Section 3.5.2 states that the R matrix is assumed to be diagonal, but then notes that the errors are reduced for intersite differences because of the large coherence in space in errors between stations. This is inconsistent. What is the impact of this inconsistency on the validity of the results?
55. Page 27, lines 8-10. The assumed transport errors are huge, and are a critical set of assumptions in your study. The lack of spatial and temporal correlation is also a significant assumption. I find it very surprising that the sensitivity of your results to these assumptions is insignificant (line 10). I do not have supplementary figure S1. The results of the paper should depend heavily on these assumed errors. A statement that

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says the dependence is insignificant with no results presented to justify this statement is not defensible.

Results. 56. How is the mixing ratio difference between two sites attributed to a flux correction? This is not clear. This is fundamental and must be defined.

57. Page 27, lines 19-22. Airlines, powerplants and nighttime emissions from all sectors will have essentially no observational constraints from the methods proposed, save for extreme assumptions about the coherence of the errors. While there is some reason to believe that corrections to daytime emissions from roads or buildings might have some coherence with nocturnal emissions from roads and buildings, there is no reason to believe that airport emissions will be detected by two sites that are located in a region that contains an airport, but which do not encompass the emissions from the airport. It is fundamentally incorrect to say that the proposed network would reduce uncertainty in total emissions.

58. Page 28, line 4. Define "gain." Or do you mean uncertainty reduction?

59. Figure 8. The total uncertainty in your inversion approaches an asymptote as the total number of sites increases. Why? What is the limiting factor in your inversion system?

60. Page 29, line 8. $DFS/d < 10\%$? When you divide DFS by d, you get a number less than 10%? Please clarify. Do you mean DFS gained per measurement pair added is less than 10%?

61. Page 29, line 10. English, "the Iowa state of USA?"

62. Page 29, lines 11-12. The authors state, "Such small amounts result from the diffuse nature of atmospheric transport and from the uncertainty in atmospheric modeling." The authors, however, have utilized only crude assumptions about atmospheric transport modeling. Their assumptions are not the truth about atmospheric transport errors. This statement appears to be unjustified.

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63. Page 29, line 13. "the rate of effectively assimilated gradients decreases." What does this mean? This does not make sense.

64. Page 29. Lines 12-17. I cannot understand this sentence. Is this a comparison to the network studied in the Wu et al (2011) paper?

65. Page 29, line 28. "this corresponds to a level 1 quality." What does this mean? What is a "level 1 quality?"

66. Page 30, line 3. The entire discussion of network design vs. uncertainty reduction is entirely dependent upon the assumed nature of coherence in the flux errors. Huge spatial coherence is assumed (entire regions have a single correction factor for a single sector of emissions). This assumption is severe and is likely to dominate any results regarding optimal spatial network design. The results, however, do not note any dependence on the assumed uncertainties in the prior flux errors. The results also reportedly show insignificant dependence on very large assumed atmospheric transport errors, but this lack of sensitivity is not shown. Page 32, line 24, admits these limitations, but this is buried into the recesses of the paper. It is dishonest not to present these limitations prominently in the abstract. As noted by the authors, "The results obtained in this study should not be over-interpreted." That sentence belongs in the abstract, and it needs to be explained in the abstract.

67. Page 33, line 3. From this point on, the text has no specific connection to the results of this study. This text is extraneous and should be deleted.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30693, 2015.

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