Author's response to reviews

Title: Prediction and analysis of near-road concentrations using a reduced-form emission/dispersion model

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Version: 2 Date: 25 May 2010

Author's response to reviews: see over
To the editor:

Enclosed please find a revision to the manuscript entitled “Prediction and analysis of near-road concentrations using a reduced-form emission/dispersion model,” MS number 1933855235351333, submitted February 1, 2010. The two reviews were very positive, although they noted a number of possibilities for improvement. We have fully responded to all of the comments made by the reviewers, and made a number of changes to the manuscript which improves its coverage, clarifies and extends (in some cases) our interpretations, and slightly increases its scope. These changes do not substantially change the results or implications.

The attached pages provide the original comments by the reviewers, as well as our responses. The attached manuscript shows these changes in markup mode. (Please note that there may be some differences in reference numbering in the responses: the numbering in the manuscript has been revised and is correct.)

As we noted earlier, this paper provides a structured analysis using two well-developed simulation models, then presents a simplified approach. We then demonstrate the new model, and show the types of outputs that are possible and the performance expected. We highlight some broad lessons regarding the nature of these exposures. In addition to those working in exposure assessment and epidemiology, this paper is relevant to those building models or using GIS frameworks for air quality and other types of environment health research or practice.

We look forward to your comments and review. Please don’t hesitate to contact me for any reason regarding this paper.

Sincerely,
Stuart Batterman
Reviewer's report and Responses - 1
Title: Prediction and analysis of near-road concentrations using a reduced-form emission/dispersion model
Version: 1 Date: 1 April 2010
Reviewer: Gregory Howard
Reviewer's report:
Batterman, Zhang, and Kononowech

Comments
The paper presents an interesting and potentially very valuable model for near-road pollutants. If validated, this model could provide a helpful tool in evaluating near-road exposures. Unfortunately, the paper gives disappointingly little attention to validation (a limitation mentioned in the discussion). Comparison of the reduced-form model with CALINE4 is shown adequately, but only a single location is used for real-world validation, despite the essential spatial nature of both the model itself and (more importantly) the conclusions drawn from it in the second case study. The authors are relatively clear about the model and are not making inappropriate claims about its power or accuracy. However, the fact of limited validation of CO at one location may give the reduced-form model the impression of having been more carefully validated. Overall, the use of the term "case study" itself to apply equally to the Allen Park site (one receptor, for purposes of validation) and the 5/39 site (predictions across a dense receptor field, with no attempt at validation, for purposes of describing difficulties of exposure assessment) adds a bit of confusion. Similarly, the first two sentences of the Results section of the Abstract conflate the validation of the model at one point in the first case study, and the spatial predictions made in the second.

Response: First, we thank the reviewer for his careful and thorough review of this manuscript. We have many changes in response to these comments which have improved the work.

With respect to this comment, we note that the reviewer recognizes the scope of our analysis, in which focus on the application of a model but not its "validation." We did include a case study to demonstrate some of the model's capabilities and limitations. Validation is a large topic that has been addressed in the several key references for the CALINE4 model, e.g., Benson 1989, which includes a number of evaluation studies for the CALINE4 model, including those using tracer gases, the "gold standard," as we describe in the manuscript. We do not claim in any way to have validated the model, and we reviewed our language throughout the manuscript to make this entirely clear. In fact, to us "validation" of a model requires a detailed demonstration of statistical agreement with many datasets as well as a detailed scientific examination of the model mechanisms. We further discuss validation issues in the following responses, which also resulted in several revisions to the paper.

We very clearly state in Results that: "This evaluation does not constitute a full evaluation or validation of the model. Rather, it demonstrates the type of performance that can be expected in applications where site-specific data are limited."

We are unsure about the cause for the reviewer's confusion regarding the term "case study." The first case study or example compares predictions and observations at the Allen Park site; the second examines potential exposures at the M5/M39 site. However, to address the reviewer's comment, we revised a portion of the abstract (in the results section) to clarify we are talking about predictions: "The highest concentrations were predicted to occur near intersections and downwind of major roads during periods of unfavorable meteorology (e.g., low wind speeds) and high emissions (e.g., weekday rush hour)."

We also revised the text at the end of the Background section to state: "The assembled model is demonstrated using two case studies. The first compares predictions of carbon monoxide (CO) to levels monitored near a major freeway. The second case highlights issues in exposure assessment by predicting CO and PM_{2.5} concentrations in an area surrounding a major freeway and an arterial road."

The CALINE4 model is rooted in CO prediction, although it does have a PM mode. The authors do not make clear how either the CALINE4 model as used, or the reduced-form model, were modified for use in PM_{2.5} prediction. Validation was unfortunately performed only for CO. The authors, however, describe the model as a
single entity throughout the paper, and are not careful about distinguishing between its use in CO or PM25 modes (or even the differences between the modes).

Response: The reviewer is correct in that we did not attempt any evaluation of the model for PM2.5. The CALINE4 model can simulate processes that may affect particular pollutants, in particular, it incorporates an algorithm to handle deposition and settling of pollutants, as developed by Ermak, allowing settling and deposition parameters to be specified, and it includes an “NO$_2$ option” that uses a simple set of reactions to predict the formation of NO$_2$ from precursors NO and O$_3$. Deposition and settling effects are negligible for CO (Benson, 1989). As we explained, both CALINE4 and the reduced-form model are intended for short-range impacts (few hundred meters), and settling and deposition rates are sufficiently small such that impacts are very small. Moreover, PM from tailpipe emissions is small size in size (<1 µm) and settling will be negligible in the near field. We added a paragraph in the Results in the Sensitivity Analysis section providing a summary of this discussion: “While originally developed for CO, CALINE4 can simulate formation and dispersion of NO$_2$, using a simple set of reactions to predict its formation from precursors NO and O$_3$, and PM, using algorithms to model deposition and settling processes. The case studies, described later, use the model to simulate short-range predictions of CO and PM$_{2.5}$ from vehicle exhaust. In the case studies, deposition and settling effects are negligible for CO (Benson, 1989); effects are also minor for PM$_{2.5}$.”

The modeled results of the second case study are interesting primarily for the predictions about the asymmetry of the short-term concentrations, stemming largely (it appears) from the use of short-term wind data rather than annual averages. These conclusions are likely to provide valuable insight for air pollution modelers. While these qualitative descriptions of the predictions would seem to be relatively insensitive to model validation, this predictive example should not be confused with real-world results, or even results from a spatially validated model.

Response: The reviewer is correct that no spatial validation was attempted. This is stated multiple times in the paper, and a recommendation of the work is to provide this type of study. We also believe that the reviewer is correct in that these results would apply to multiple pollutants and would also be largely insensitive to model validation.

The paper would of course greatly benefit from additional data for model validation, particularly for PM25. It is probably already too long for detailed validation, but at minimum, more discussion of validation seems important, particularly with respect to the results of the second case study. An obvious question that the authors do not address is whether these models are really better than simple proximity estimates, or simple measures based on proximity and short-term wind measurements, when compared against real-world data.

Response: In the Discussion, we have added additional text discussing the limitations of the model, specifically dealing with the second case study: “... Similarly, our second case study examining the temporal and spatial patterns in Detroit does not represent results drawn from a spatially-validated model. Analyses using much more extensive, diverse, and representative data are needed for purposes of model validation.” Responding to the second part of this comment, we also added: “Finally, while the reduced form model yields a wide range of outputs and is mechanistically-based, we do not have direct evidence indicating whether its predictions are better than much simpler measures, e.g., based on proximity or short-term wind measurements.”

Major Compulsory Revisions

1. The Methods discussion of CALINE4 describes the model as predicting CO. Moreover, while validation for CO was reported by Benson, no validation for PM is reported at all. This section should more clearly describe the use of the CALINE4 model for PM in general, and particularly for PM25. Is the only difference in the PM25 model the addition of a parameter for settling velocity? The authors should also briefly review the results of previous attempts at validation, for example, recent work by Yura et al, by Gramotnev, etc. This is particularly important given the lack of validation of the model in the current paper. In general, insofar as the reduced-form model differs between CO and PM25, these distinctions should be made throughout (e.g., Figure 4 is for CO).
Response: We have added to the paper text discussion validation issues related to PM2.5, including key points made by these (and other) citations: “Both MOBILE6.2 and CALINE4 have had previous and extensive analyses in much better controlled settings that typically show better performance. For example, Benson [10] describes three freeway studies, including those using tracer gases that largely eliminates uncertainties in the emission term, which gave 75% or more of CALINE4 predictions within a factor of two, a criterion sometimes used to define acceptable performance. Predictions of PM\textsubscript{2.5} involve more complexity and larger uncertainties, especially regarding emission factors [21], thus agreement will not be as good. Only a few studies, which have been limited in extent, have evaluated roadway models for PM\textsubscript{2.5}, and thus little quantitative performance data are available. Tests at a relatively flat suburban site gave reasonable performance; at a more complex urban site with possible street canyon effects, due to tall buildings, performance deteriorated, e.g., only 56% of predictions fell within the factor-of-two envelope of the observations. [22] Additionally, some systematic biases have been noted in CALINE4 and other roadway models, e.g., a tendency to overpredict concentrations when on-road emissions are low, and to underpredict when on-road emissions are high. [22]” We have also made the distinction requested (between CO and PM2.5) in the manuscript.

For example, the same problem occurs with the sensitivity analysis; the authors should either repeat the sensitivity analysis (Fig 3) for PM25, or, more likely, justify based on the point above why the CO sensitivity analysis is relevant to PM25. For example, the omission of stability category in the reduced-form model might impact PM25.

We have added a section to address this concern in the sensitivity analysis section examining CALINE4. This reads: “The CALINE4 sensitivity analysis was conducted using CO, for which deposition and settling processes have negligible effect. [10] However, results should also apply to PM\textsubscript{2.5} emissions from vehicle exhaust, which form very small particles (well below 1 \textmu m in dia), since these processes also will have only minor effects, at least at the short distances considered. [20] Additional processes sometimes relevant in modeling PM concentrations include coagulation for the ultrafine fraction, precipitation scavenging, entrainment of roadway dust, and PM emissions from tire and brake wear.”

2. p12: Ignoring the line of trees at the monitoring site, and asserting that it is "not expected to cause major changes", is questionable. The wind at the monitoring site should provide the best data for the receptor, although not for the emitters. A comparison (quantitative or qualitative) of the wind roses at the two locations would help clarify how much impact these common feature might have.

Response: We agree with this comment and have deleted this sentence. We have added the distance from the monitoring site to the meteorological station in the text (18 km). Our comparison between data at these sites showed generally minor differences, e.g., due to sheltering at the Allen Park site, wind speeds and directions (at low wind speeds) were slightly different.

3. I found the description of the wind data somewhat unconvincing, sometimes attempting to draw too much out of vague data. The important point of seasonal and diurnal variation is clear. However, the details of which lobes of wind roses are relevant at different times is hard to follow. On p28, ”SW, SW, and WSW” should be fixed. Furthermore, this description seems to ignore the presence of relatively strong wind from the WNW, a pattern even clearer in the afternoon rush hour.

Response: We have corrected the erroneous direction. We also revised much of this paragraph in order to shorten and clarify the key points. The changes are: “Wind direction and speed “roses” that show the probability and speed of winds in 16 sectors (each subtending 22.5o) are presented in Figure 7 for three cases: (a) all hours of the year; (b) the morning (7 – 9 am) rush hour period; and (c) the evening (4 – 6 pm) rush hour period. In the morning, moderate SW, SSW and WSW winds dominate (Figure 7b), while in the afternoon, winds shift to the WNW and are stronger, and occasionally moderate SSE and SE winds occur (Figure 7c). This diurnal variation is not represented by the annual patterns (Figure 7a). Other trends emerge when examining the lowest wind speeds that can produce the highest concentrations. As shown in Supplemental Figures S5 – S7, which contrast winds in the morning rush hour periods on the basis of speed, the lightest winds (\leq 2.5 m s\textsuperscript{-1}) arise primarily from the NNE and S sectors, directions not apparent in the annual analyses. Strong seasonal patterns are
shown in Supplemental Figures S8 - S11, e.g., winter is dominated by SW and WNW winds, spring with WNW winds, and summer with SW and light NNE. As discussed below, such seasonal and diurnal patterns can greatly influence concentration predictions.”

4. In the Discussion, please describe the effect of omitting calm days, and the likely impacts of this limitation for exposure misclassification or bias.

Response: We added the following to the Discussion, under model limitations. “… For example, performance under very light winds or complex topography is unlikely to be accurate. Moreover, no predictions are made for calms, a limitation common to Gaussian plume models. This exclusion can result in significant errors and exposure misclassification, especially if calms are frequent, especially during the rush hour periods. In Detroit, calms are relatively common is early morning, before the rush hour traffic period, but typically there are several days per year when calms extend entirely through the morning rush hour (based on examining 2004 – 2006 data), and we note that the worst-case day in 2006 included 9 hours of calms. In such cases, no morning prediction would be obtained, and the daily (24-hr) prediction could be significantly biased downwards. Imposing data completeness criteria, e.g., requiring that each 24-hr prediction have complete (or nearly complete) data, partially addresses this problem in validation studies, but may perpetuate biases in epidemiological studies since exposure estimates for those days with potentially the highest concentrations are more likely to be omitted.”

Minor Essential Revisions
1. In the abstract, it would be helpful to quantify "reasonable agreement".

Response: We added that “…59% of the 24-hr data was within a factor of two of observations in the warmer months when CO emissions are more consistent.”

2. The first sentence of Results in the Abstract is about CO, but the rest (evidently) refers to both CO and PM2.5; please clarify.

Response: We clarified this by saying that: “For both CO and PM2.5, …”

3. p8: "estimated emissions on the link": please clarify the term "link" in this first use.

Response: We eliminated this jargon by simply referring the road segment, rather than “link.” This change was made throughout the text.

4. Give a more specific location for the Allen Park site (lat/long or street intersection).

Response: We added the cross street to the description: The site is 17 km SW of downtown Detroit in a flat and largely tree-free area, just north of Goddard Road and 150 m SE of interstate I75, which oriented at 40o (although it curves just S of the site).

5. Give distances to the "local airport" in both cases.

Response: This has been added.

6. p18: "The effect of these adjustments is shown in the supplemental materials (Figures S2 - S5).” There are two figures S4, so this reference will be correct if the second S4 -> S5, and then S5-S11 -> S6-S12.

Response: The numbering of the supplemental figures has been corrected, as have numbering in the text.

7. Confusingly, Figure S2 refers to a different site than S3-S5. Because there are two sites in question, it may be clearest to identify the relevant sites in the figure captions.

Response: The legends have been modified following this suggestion.

8. Figure S5 is a near duplicate of Figure 7b (with slightly different times for no given reason); one of them might be omitted.

Response: The reviewer is correct that these are essentially similar figures. The reason is to allow the reader to compare wind roses for three conditions (low speeds, high speeds, and all speeds) on one
page. Since this repetition exists only on the supplemental materials and not in the main text (subject to page length issues), we felt it was warranted.

9. p27: Why are Sat, Sun, holiday traffic volumes "based on the Allen Park data"?
   
   Response: Hourly traffic count data for M39 and Grand River, the two roads in the second case study, were available for typical weekday periods in each year, but not for holiday and weekend periods. Thus, we scaled these counts using factors derived from the Allen Park data, which were based on continuous, year round PTR data. We modified text in the Methods to more fully describe this: “We adjusted hourly volumes for Saturday, Sunday and holiday periods using factors derived from the hourly year-round PTR measurements on I-75 (described previously) since traffic volume measurements for M5 and Grand River were available for only weekday periods.”

10. p30: I cannot tell where the receptors described in Figure 9 are with respect to the GR road; perhaps they could be indicated on the map.

   Response: We added the receptor locations in Figure 9, and refer to them now in the legend and the text.

11. Miscellaneous minor grammatical fixes:

Be careful of unclosed parentheses throughout; p11, p12, p24.

Response: Corrected.

p7: "that match essentially widely-used"

Response: Corrected.

p7: "Sensitivity analyses identify critical variable" -- add "s"

Response: Corrected.

p10: "In special cases, these can be important in special cases"

Response: Corrected.

p15: Is alpha a "scale" or "scalar" coefficient?

Response: It was described as a scalar coefficient in the parent citation, but really is just a coefficient, so this is revised.

In caption for Tables S2A/S2B, spell out acronyms.

Response: This was described in the caption.

p25: "In summary,": I think you mean to refer to the April-October season, but "in summary" is confusing.

Response: We replaced “In summer”, with “During this period,” to clarify the text.

Discretionary Revisions

1. The literature review relative to near-traffic pollutants (p6) is rather brief.

   Response: There have been several excellent literature reviews, and it was not our intention to provide a comprehensive review. We added several additional citations, including the new HEI report that recently became available in January, which describes many aspects of exposure-related concerns, as well as several more specialized papers dealing with dispersion modeling.

2. In the description of PM25 model predictions on p28, it would be interesting to present measured data locally or nearby, if available. If not an actual attempt at model validation, it would at least add context, as the modeled numbers seem quite low.
Response: For interest, we have added to the results a short discussion of this issue. “We note that the PM2.5 predictions due to roadway emissions are small relative to monitored concentrations, e.g., the annual and 98th percentile 24-hr averages in 2004 at Allen Park were 15 and 37 µg m\(^{-3}\), respectively, levels similar to those measured at other Detroit area sites. The small contribution from roadways is due to the dominance of other emission sources, including long range transport of PM2.5, and, as discussed later, potential underestimates of PM2.5 emission factors by MOBILE6.2.”

3. Finally, I would suggest that, if and when published, the authors make their code and methods available for download and use, perhaps under some permissive license (e.g., the GPL).

Response: We will endeavor to do this, although this will require additional work to make it more “user friendly”.

Level of interest: An article of importance in its field

Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests.
Reviewer's report and Responses - 2

Reviewer's report

Title: Prediction and analysis of near-road concentrations using a reduced-form emission/dispersion model

Version: 1 Date: 19 February 2010

Reviewer: Rich Cook

Reviewer's report:

This paper will be of significant interest to scientists doing research on exposures to air pollutants near roads. In particular, this research will help modelers identify those variables likely to have the largest impact on results, enabling them to better focus data gathering efforts. Second, results from application of the modeling approach can clarify locations and conditions where emissions are likely to have the greatest impact, which can inform design of monitoring studies.

However, a number of discretionary revisions may improve the overall clarity of the paper. These potential revisions are as follows:

1) In the introduction, clarify the scope of this reduced model – it is designed for microscale analyses and is limited to several pollutants. It appears that the emissions submodel includes VOC, CO, NOx and PM, but this is not explicitly stated in the text.

Response: In this paper, we discuss only CO and PM2.5 although the model structure is amenable to predicting other pollutants, potentially including NOx and VOCs (although the model does not include chemistry or reactions at present. In response the reviewer’s comment, however, we have revised the introduction to provide this clarification: “The first objective of this paper is to develop a streamlined model designed for microscale analyses, specifically, to predict short- and long-term air pollution concentrations of carbon monoxide (CO), particulate matter below 2.5 µm dia (PM2.5) and other air pollutants near roads that match widely-used and validated emission and dispersion models.”

2) Better articulate the need for a reduced form model in the introduction. Describe the types of assessments where it is most applicable, what the current data collection and processing challenges are, and how this reduced form model addresses those challenges.

Response: We have revised the introduction and addressed this comment as follows: “The reduced form model has several advantages over existing models, including the ability to predict concentrations for an arbitrarily large number of receptors and time periods, fast computations, and relatively limited data needs. All of this can facilitate use of the model in exposure assessment, epidemiology and risk assessment applications, especially if predictions are needed for a large number of receptors and/or road segments. Also, the simple form of the reduced-form model permits easily incorporation into GIS.”

3) In the first sentence of the results section, revise to read, “Results from previous sensitivity analyses have shown that MOBILE6.2 emissions are most sensitive to several inputs.”

Response: This change is made.

4) Include as a reference on MOBILE6.2 sensitivity analyses the following paper:


Response: We have now cited the first paper. We are aware of the second paper, as well as others, and this is a useful reference, but this has been largely addressed using other citations. Also, this journal has a somewhat restricted availability compared to ones cited, thus we have opted to include only the first citation.

5) Sensitivity analyses in the past have shown that fuel RVP is an important parameter. Why was this not included in the emissions submodel? Also, it would be helpful to explain that were other pollutants included, other fuel parameters would have been important to include (e.g. fuel benzene for benzene emissions).

Response: We agree. We have broadened the discussion of fuel parameters. For example, in this section we have added that: “Other fuel parameters are important, e.g., Reid vapor pressure strongly affects the evaporative (but not tailpipe) emissions of VOC emissions.”

6) In the discussion of limitations, address implication of the recently released MOVES emissions model.

Response: In the section discussing MOVES, we have added text to respond to this suggestion: “MOVES has several capabilities that should improve emission estimates, e.g., with appropriate inputs it can provide estimates at national, county and, most importantly to the present application, project level scales, it incorporates substantial new emissions test data, and vehicle classification is based on Federal Highway Administration’s system, which will facilitate the use of existing highway activity information. Potentially, MOVES’ outputs can be incorporated into the reduced-form model.”

I declare that I have no competing interests.
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: