Reviewer's report

Title: Exploring exposure reduction priorities using regional rankings based on emissions of known and suspected carcinogens to outdoor air

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Reviewer: George Hidy

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This paper describes a method for presenting emission inventories in terms of toxicity of emitted chemical species. The authors propose that their approach allows for improvement of human exposure assessment. The following are my concerns:

1. Exposure is defined in terms of a receptor experiencing "contact" with chemicals in ambient air over a period of time. The method proposed offers insight on the annual? toxic emissions by broad source categories but cannot provide information about exposure per se. To achieve this, one must either model exposure at the receptor with emissions, meteorology and chemistry, or use receptor modeling and ambient concentration data (e.g., the well known chemical mass balance). The problem of using only the toxicity adjusted emissions by province as exposure measures is unsolvable based not accounting for atmospheric dilution rates from sources, losses by chemistry or deposition and air transport.

The exposure differentiation at the receptor is important, for example, because the five top toxins found from emissions are generally all very low in concentration in ambient air except near (adjacent to) sources. At these extremely low concentrations, one questions the actual risk of exposure relative a number of other risk factors, including contaminated water, more concentrated pollutants such as O3, accidents secondary smoking etc.,

2. The method for toxicological priority setting by emissions appears straight forward. However there are questions about the choice of emissions data and toxicity equivalent used for the study. The authors say they developed emissions for transportation, residential heating and wood burning. On what basis? There are large amounts of data on these sources so the chemical composition in the emissions should be referenced. The authors use motor vehicle emission data of Australia--It's unclear why these data were used in preference to large body of data from the US and Canada. The toxicity ranking comes from an unpublished report from Norway. This report is not readily accessible to the reader to check the methods used for toxic equivalence rankings. Since the toxicity ranking is key to the method the authors should include better discussion and/or references on this ranking or justification for using Norwegian data. It would seem that EC, IARC, WHO or EPA would be more accessible data for toxic differentiation. The numbers in Table 6 are not helpful. They appear to be toxicity relative to benzene, but arsenic, TCDD and benzopyrene toxic equivalent are extremely
high, biasing the emission modeling results without accounting for dilution to ambient levels. Attribution to the NPRI for Canada as a single source in the tables instead of collection sources in the Canadian inventory. The authors should comment on what large sources in the inventory bias the result by province. For example arsenic is usually associated with mineral processing and smelting, which are likely to have minimal effects on the general provincial population?

The authors should take a close look at the results, for example, in Table 4--contributions of the top five toxins by province do not make sense as exposure measures. Arsenic for example is generally very low in ambient concentration. It's not clear why it has a strong % TEQ in several provinces including Nunavut, Yukon, or NWT. The results suggest that transportation should be minimized in all two provinces to minimize exposure to benzopyrene (at very low ambient concentrations)

The authors could aid the message of the paper by citing a specific example of the use of this modeling to guide toxic exposure conditions perhaps in Vancouver. Does the method offer value added beyond current practice using ambient concentrations spatially disaggregated for sub-populations?

3. The source data for the modeling of emissions seems poorly documented except for the NPRI. The authors do not examine the sensitivity to the range of emission concentrations that are known to occur with transportation, residential heating and wood combustion. This is not discussed for sources in the case of NPRI either.

4. The figures submitted are complicated by a overlay of tables on a map of Canada. They are difficult to read and understand--presumably they illustrate the software CAREX output for use in risk assessment.

5. the discussion and conclusions are general, but do not provide the reader with limitations in terms of exposure or perspective on the emission differentiation by toxics. The discussion needs a critical look at the method relative to exposure estimation noting strengths and weaknesses of the method.

6. The paper does not really "explore" exposure priorities using the results. The abstract is adequate for the content of the paper.

7. The writing in general is well organized as far as it goes, but there are too many key references to data and details of emissions estimation that are missing. Some of these are noted above. For the method to be useful as an outdoor chemical exposure link, it should be coupled in a meaningful way with ambient measurements or air quality modeling to insure that the TEQs are not misinterpreted or source contributions to toxicity are not misrepresented locally given broad regional assessments.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:

I have no competing interests.