

Interactive comment on “Prioritising the sources of pollution in European cities: do air quality modelling applications provide consistent responses?” by Bart Degraeuwe et al.

Anonymous Referee #2

Received and published: 30 June 2020

Degraeuwe et al. describe the application of the SHERPA technique for determining Source/Receptor Relationships (SRRs) to the assessment of mitigation options for annual average PM_{2.5} concentrations in 150 European cities. SRRs are calculated from the output of two Chemical Transport Models (CTMs), CHIMERE and EMEP, which are commonly used in Europe for air quality simulation. The benefit of using pre-calculated SRRs instead of directly using the CTMs themselves is that the SRRs effectively emulate the relationship between emissions in each CTM grid cell and concentrations in other grid cells without having to simulate the full set of physical and chemical processes involved. SHERPA in particular provides an efficient way of calculating cell-to-cell SRRs without having to run a large number of training simulations, by making some

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assumptions about the degree to which grid cells can influence each other based on their separation.

The authors use the two different sets of SRRs to determine the most effective options for mitigation of annual average PM_{2.5} in the 150 selected cities. They find that despite the use of different CTMs, emission inventories, and base meteorological years, the mitigation options identified for each of the cities are generally very similar. A few cases are however identified where the use of the different SRRs produces contradictory recommendations.

While the topic is certainly within the scope of GMD, and the results as presented should be of interest to the community, it seems to me that the authors have gone to an extremely minimal amount of effort with this manuscript. The quality of the manuscript in its present form is not high enough to meet the standards that this reviewer would expect from GMD. Major revisions are required before the manuscript can be published.

Firstly, the authors appear to cite mostly their own work, or the work of their colleagues. This approach may be acceptable for an internal technical report, but in the peer-reviewed literature, authors must place their work in the broader context of the work that has come earlier, and clearly explain its novelty. The use of SRRs in air quality assessment has been prevalent for a long time, and SHERPA is not the only way that exists to calculate SRRs. It is not the job of this reviewer to perform the literature survey that the authors of this manuscript have neglected, so I will not suggest any specific references. But more context is certainly needed, and not only in the introduction; while the results are new and interesting, this is no excuse for not discussing them with appropriate reference to the existing literature.

Secondly, for a technical journal such as GMD, the paper is extremely short on technical detail. In Section 3, the reader is referred to Pisoni et al. (2019) for all but a few of the relevant details. Of course the reference is appropriate in this section, but the paper should also contain enough detail to stand on its own. The authors need to

summarise the key points from this earlier work. For example, readers need to know how the SHERPA technique differs from other approaches to calculating SRRs, and how well it has been shown to work. Have mitigation options identified with SHERPA been compared with actual CTM simulations of the same mitigation options? What are the strengths and weaknesses of the approach as identified by earlier work, and what are their implications for the present manuscript?

I also have a couple of minor comments.

It would be nice to see a short explanation of how the four cities shown in detail were chosen. It's good to see an example of a situation in which the approach works well, and a situation in which it doesn't (Liege and Madrid). But what about the other two cities (Genova and Torino)? Were these chosen to highlight specific points? Or for some other reason?

For the cases when the use of the two sets of SRRs from different CTMs yields different mitigation options, the authors take the position that their method is simply unable to explain the differences. I find this somewhat lazy. Actually the disagreement could point the way to targeted CTM simulations (or other analysis) designed to specifically understand the relevant processes. It would add a lot to the paper to see some more discussion of this.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-90>, 2020.

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