

This is my second review of this paper. The authors have addressed most of the questions that I had asked earlier but some of the responses were not included in the revised manuscript. I still have a few more concerns that need to be addressed.

My main question is whether the benzene and toluene emissions in the PRD are now better constrained or not. The authors justify the need for the top-down approach by arguing that the variation of bottom-up estimates is large. What I see in the results though, is that the uncertainty of the top-down estimate is not any smaller. A legitimate question that one might ask then, is whether these emissions are now better constrained or not. What do we learn from this study? I would like to see some more discussion devoted to the benefit of using the top-down approach in this particular case.

In my previous review, I asked the following question:

In terms of the prior emissions, why did the authors use the averaged emissions using MEIC v1.2 and Yin et al. (2015) for toluene for the PRD region? Did the posterior emissions change much if only MEIC v1.2 was used instead? And why was this approach not used for benzene? I feel that a similar type of sensitivity analysis using different emissions inventories might be more interesting, in addition to changing simulation length and chemical loss as currently done in the paper.

This is the response I received from the authors:

According to the comments, we tested the impact of the choice of a priori emissions. A posteriori emissions for PRD from inversions using MEIC v1.2, Yin et al. (2015) and the averaged emissions using MEIC v1.2 and Yin et al. (2015), respectively, were 13.8 Gg/month, 11.1 Gg/month and 12.0 Gg/month. Thus the difference is less than 15% which is not a big value and within the a posteriori uncertainty reported in this study. We also did the tests for benzene, and we found the difference of a posteriori emissions is about 10% using different a priori emissions. The estimated emissions for toluene by MEIC v1.2 and Yin et al. (2015) are quite different, thus we used the averaged value as the a priori emission; while the estimated emissions for benzene by MEIC v1.2 and Yin et al. (2015) are relatively close, thus we used one of them (MEIC v1.2 used in our study).

I now understand the authors' reasoning for their prior emissions but this is not reflected in the revised text. The authors are arguing that the posteriors change only by 10-15%, "which is not a big value." If this is the case, then why don't they simply pick one of the bottom-up estimates and state in the text that using different prior values does not change the results?

I also asked the following question in the previous review:

I am also curious if the inversion is conducted for the two species together or separately. I would also like to see a better justification as to why 100 and 70% were chosen for prior emissions uncertainty for benzene and toluene, respectively. Also, it would be good to include an equation showing how the posterior uncertainty is calculated.

This is the response I received:

The inversion is conducted separately for benzene and toluene. As for the a priori uncertainty, we determined it by looking at the variations of different bottom-up estimates for that particular species. For benzene, our priori emissions were 3.1 Gg/month, while the RCP 2.6 was 3.7 Gg/month, Yin et al. (2015) was 4.4 Gg/month, REAS v1.1 was 0.7 Gg/month. Thus, the largest deviation is about $1 - 0.7/3.1 = 80\%$, and then we set the a priori uncertainty as 100%. For toluene, our priori emissions were 11.5 Gg/month, while the RCP 2.6 was 3.6 Gg/month, Yin et al. (2015) was 5.2 Gg/month, REAS v1.1 was 3.8 Gg/month and MEIC v1.2 was 14.9. Thus, the largest deviation is about $1 - 3.6/11.5 = 69\%$, and then we set the a priori uncertainty as 70%.

Again, this explanation is not included in the revised manuscript. I am also puzzled why benzene uncertainty is 100% rather than 80%, as explained above. If these numbers are indeed derived from the calculations above, I do not see a reason for deviating from them. I also would like to see the equation for posterior uncertainty that I had asked earlier.

I also have some minor comments on the figures.

For Figure 1, I suggested that the map includes the names of the cities. The authors have made changes and the maps now include city names. However, without markers pointing to the location of those cities, the readers are left unclear where these cities are located.

In Figure 7 all the simulated values are showing zero at the end of the time series. Is this an artifact of the plotting procedure? The same goes to Figure 3a.

What do the arrows in Figure 8 represent and what does the legend "not scientifically determined" mean?