

Peer review: Global continental and oceanic emissions of atmospheric microplastics inferred from pattern-restricted Bayesian inversion

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1 General Comments

The paper is relevant, well structured and explained. There are some major and some minor comments and clarifications needed in my opinion, and it would be appropriate to publish in Atmospheric Chemistry and Physics. I believe the paper is very much within ACP's scope: In term of the subject, this work fits within "Aerosols". In terms of Research Activity, it fits within "Atmospheric Modelling and Data Analysis". It deals with the troposphere, and inhabits the somewhere in between Chemistry and Physics scientific focus that is aerosol emissions and transport.

The emissions inventories of all pollutants tend to vary wildly from each other, and selecting "the best one" is not always straightforward. However, we are currently lacking when it comes to microplastic emission inventories. This work develops a new one with a top-down inversion approach using Bayesian statistics, which is a reasonable and decent approach. The main weakness of the paper in my opinion is the local grouping and small amount of measurements used to develop a global inventory: just 11 sites, all of them in the USA, used to develop a global inventory. I do believe that that this weak point is significant, but the authors seem aware of it and justify their decision by affirming that it was the only set of consistent data measured long enough for the inversion. This is an acceptable reasoning: it might not be ideal, but it is the best that they can do with the existing data.

The authors assume 4 main sources: agricultural nets, resuspension from bare soils, road dust and resuspension from ocean surface. **A major comment, however, is that industrial sources are not included.** This is probably reasonable at a global level, but since they use 11 sites in a very industrialized country, the industrial contributions could be significant. The authors should either include it or, if they decide not to, include a clear reason for that decision.

They distribute the sources according to satellite data for cropland, FLEXDUST for bare soil resuspension, Evaneliou 2020-2022 paper for road dust and Isobe et al 2021 for the ocean. This seems to me to be a considerable source of uncertainty, especially related to the ocean. Combining Eq. 2 and Eq. 4 makes it clear that the emissions of each source are then increased or decreased according to source weights, without any possibility of adjusting that pattern (i. e., each one of the sources increases or decreases linearly in all of the globe). I do not mind that they do it this way: it's the best they can do with the very limited information they have. However, I believe that this big source of uncertainty should be more clearly acknowledged and stated. For instance, when the authors compare their data to the other emission inventories (Figure 6), it is clear that their inventory is higher than all others in what looks to me like the Pacific east garbage patch, the Mediterranean and Black seas and the water body's surrounding Japan: all of them water sources. I believe that this linear increase might be the source of it, since it is possible that the original inventories over-or-underestimate MP concentrations in certain parts of the sea by a different proportion than in others.

The fact that the authors provide pseudocode for their procedures is also appreciated.

Another significant contribution of this paper is analyzing the uncertainty due to the Gibbs sampling itself and due to scavenging.

2 Specific comments

Lines 181-182: Although it is true that plastic degradation slows down, the main argument for non resuspension should be that the accumulation is observed, which is clear empirical evidence. In principle, there is no reason why wind that suspends snow shouldn't suspend MP that have been deposited in that snow.

Line 169: Is all dust assumed to contain the same amount of MP? Or where MP distributed differently in different areas? (E. g., dust in coastal areas or areas with large amount of open dumps and landfills might be assumed to have a larger amount of MP when defining the source patterns).

Line 199: I am not completely sure about the choice of a truncated normal distribution for the equation $p(x) = tN(0, \omega_x^{-1}I_n, [0, +\infty])$, since I would have expected it to be a lognormal. The lognormal would also avoid the need to truncate the distribution and to add the multiplier t . Since this is just an assumption on my part based on the fact that lognormal distributions are often seen in atmospheric chemistry and it is not obvious for emissions, I recognize that the authors may have given this a thought already, and would be satisfied with an explanation in their answer or a citation.

Line 205: The original description of the Gibbs sampler should be cited, since it is not common knowledge for many potential readers of the paper (<https://doi.org/10.1109%2FTPAMI.1984.4767596>).

Section 4.2: This section includes a lengthy discussion about regional emissions from specific areas. I am a bit hesitant about this, since most of this distribution is a direct consequence of prescribed S_j , and the importance of those areas depends only on the relative weights, which are assumed to be the same globally. This is mentioned in lines 314 and 315, but then the results seem to be discussed while ignoring

Lines 357-359: The fact that hotspot emissions coincide with the dataset used to scale them seems to me like something obvious due to the methodology, in which S_j was scaled with weights. Therefore, I would mention it as "Finally, oceanic hotspot emissions of MPs coincide with the locations of the garbage patches, due to the source pattern of the Isobe et al. (2021) dataset that was used to scale emissions globally."

Line 367-368: Are the large emissions in Asia a result of the inversion or of the prescribed source distribution? I also wonder about the fact that industrial activity is mentioned here, while it is not included as a source.

Lines 372-373: I wonder whether industrial activity might also be a major source in North America, and maybe also in Brazil within South America.

Tables 1 and 2: I am a bit confused about the Ocean in these two tables. If I understand them correctly, the numbers should be the same in both, since the Ocean is both an emission source and a geographical area. However, except for Brahney, numbers differ.

Line 416: This yields the obvious question of why Evangelou's dataset was not used for the inverse modeling efforts, which is probably due to the lack of unified techniques that was discussed in methodology?

Line 425 (Eq. 13): My understanding is that the authors calculate V for films assuming an equivalent cylinder, as stated in lines 420-422. However, I do find it confusing in the case of films, since they simply write $V = \pi (S/2)^2 L$. Since they affirm that the particles are a square of thickness S and a diagonal dimension L , I would think it should be $V = (L^2/2)S$, and this would yield a different volume than stated. I believe they are approximating films to a cylinder of a diameter equal to its diagonal. This might be reasonable, if that is the aerodynamic behavior, but it should be clearly stated.

Line 442 (equation 15) and Figure 4: The $R_{\log_{10}}^2$ is defined, but not used in Figure 4 nor mentioned anywhere else.

Line 449 (equation 17) and Figure 4: Here, the Spearman rank coefficient is presented and used. This coefficient shows a monotonic relationship, but not necessarily a linear one. Considering that the whole SRM approach assumes linearity, and that the Pearson coefficient was already mentioned before (line 301), why was Spearman chosen?

Lines 463-465: Yang *et al* (2025) with cylinders does not seem to have a somewhat larger fraction lying more than two orders of magnitude away from the observations, but “substantially” larger seems like an overstatement. Otherwise, Evangelou should be considered to have a “substantially” smaller fraction more than 2 orders of magnitude away from observations than this study (it is also a 4% difference), which would not be fair towards this study. Yang (2025) with spheres assumed differs substantially more from observations than the rest of the studies, but that work itself clearly states that assuming fibers for larger particles is more appropriate. *I also wanted to make sure that, when the authors mention cylinders assumption, they are doing it for ease of writing, and are referring to Yang (2025) base case, which includes spheres for particles smaller than 35 μm and cylinders for particles larger than that.*

Lines 544-554: I wonder once again whether the source distribution patterns S_j could also be playing an important role here.

Line 569: “value ($88 \pm 62 \text{ Gg y}^{-1}$) is identical to their,” should be “value ($88 \pm 62 \text{ Gg y}^1$) is identical to their 88,”?

Lines 573-574 : For North America, South America, Africa and Oceania the difference with Brahney does not seem significant, since it’s within the $\pm$ error interval.

Lines 637-640 and Figure 6 : This is confusing to me. This study’s ocean emissions are much larger than Yang’s, but that does not seem reflected here. Why is that? Does it have to do with the different sizes?

Lines 651-652: The S_j inputted by the authors did not allow for emissions over ice or snow. Therefore, having negligible emissions there is a direct consequence of the inputs, and cannot be a conclusion, since there was no way for the statistical model to generate large emission sources there.

Figure S6 : This figure is a bit tricky to analyze, since Yang and Fu go to $70 \mu\text{m}$, but the figure bins $50\text{--}100 \mu\text{m}$ which gives the impression that there are particles larger than $70 \mu\text{m}$.

Line 692: The interval seems to overlap with the one calculated by Yang et al (2025).

Line 894-912: This approach assumes a relation that might not be the case. If the ocean emissions are reduced by a factor of 10, that does not mean that increasing road emissions by a factor of 10 will be enough to offset this. A more appropriate approach would be to constrain the ocean emissions and let the model adjust the rest of the sources (or, even, only the road emissions) as needed. I do not think that comparing the results of dividing one sector by 10 and multiplying another sector by 10 is appropriate to evaluate the model, although I can see how comparing the spatial patterns of those simulations could be useful.

Line 903: A sentence looks unfinished here: “The scaled posterior emissions result in modeled concentrations that are”.

Lines 910-912: Should it be “However, the fact is that a robust methodology would require..”?

Conclusions: It would be useful to read here a suggestion on which measurements would you need in order to have a similar approach on a more global scale.

3 Formatting comments

An (admittedly) minor comment about the color schemes

The authors use what looks to me like the turbo color scheme (it might be jet) in Figures 2 and 3. Although this is not terrible, it is not ideal either. They should consider using a perceptually uniform color scheme, preferably a sequential one. Matplotlib offers several, and most other plotting tools do as well. The authors can check <https://doi.org/10.1038/s41467-020-19160-7> or <https://matplotlib.org/stable/users/explain/colors/colormaps.html> for more information on this. In Figures 5 and 6, they use what looks to me like Matplotlib's Spectral_r. This is fine for Figure 6, since it is a diverging colorbar. For Figure 5, however, a sequential color scheme would be preferable.