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## Response to Comment on “The missing ocean plastic sink: Gone with the rivers”

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## TECHNICAL RESPONSE

## Response to Comment on “The missing ocean plastic sink: Gone with the rivers”

Lisa Weiss\* and Wolfgang Ludwig

Mai *et al.* are mistaken in their assertions that we incorrectly calculated the residence time for floating microplastic stock at the ocean surface, and that most of our results are not novel. Their claim that our field-measured data and methods were not rigorous is wrong, as shown by a more careful consideration of what was done.

In their comment, Mai *et al.* claim that the study of Weiss *et al.* (1) incorrectly determined the residence time for floating microplastic (MP) stock at the ocean surface and that the majority of the results and conclusions obtained by Weiss *et al.* are not novel. We disagree with both charges. Although the study of Mai *et al.* (2), which they claim undermines the novelty of our results [Mai *et al.* was published on 23 July 2020, 1 week before we submitted our manuscript to *Science* (27 July)], applied the same kinds of methods as we used in our work (1), they did not demonstrate or discuss the consequences that methodological errors might have had on their calculation of plastic riverine budgets, while we (1) did. Furthermore, they failed to provide a global estimate for the riverine MP flux to the oceans, which could have been derived from their data (they only produced total plastic fluxes), which we also did (1). Had they done that, they would have realized that global riverine MP fluxes might be much lower than previously thought. As far as we can understand from their comments about our work, such an estimate would have been as low as 1.1 to 5.3 kt year<sup>-1</sup>. This is, compared to other literature estimates, not very different from our estimates. We are therefore highly surprised to see our estimates being qualified as “doubtable” in their comment.

We completely disagree with their argument that we selected less rigorous field-measured data and methods to calculate riverine fluxes. In fact, our database includes 293 field-measured data sampled strictly with plankton nets. Time series on the same sampling points were averaged to 96 mean MP concentration values [see table S1 of (1), “plankton net” data subset], which is far greater than the 80 instantaneous measurements gathered in the Mai *et al.* study (2). Contrary to (2), we believe that, whenever it is possible, statistical models should be based on average values that buffer the tem-

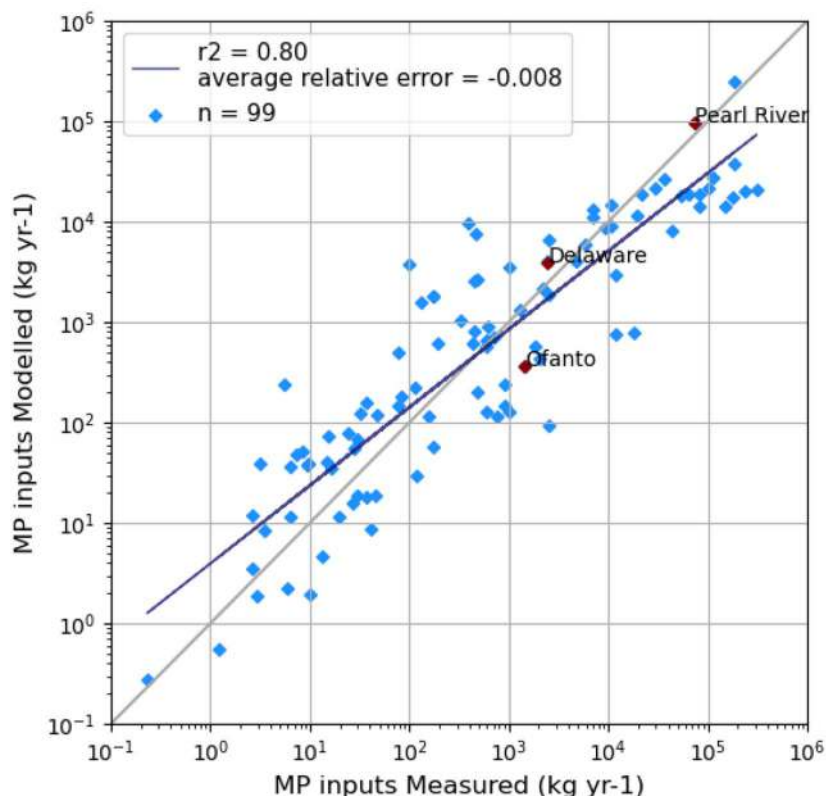
poral and seasonal variability of field measurements in order to obtain representative values of annual MP fluxes at a given river sampling station. Moreover, since we (1) considered riverine fluxes, we prioritized in our study the measurements that were strictly made in freshwater environments, with 58 sampled rivers versus 10 sampled tidal estuaries or deltas. Mai *et al.* (2) considered only 10 rivers *sensu stricto* versus 14 tidal estuaries or deltas.

Moreover, to prove that the inclusion of a few omitted data does not change the resulting MP river flux estimates compared to our current value, we included in our regression analysis (Fig. 1) the three rivers mentioned in Mai *et al.* (2) that are missing in our study (1):

the Pearl River (3), the Delaware Bay (4), and the Ofanto River (5). A new regression analysis was carried out with 99 records sampled with plankton nets instead of 96 as published in our paper [see table 1 of (1)]; the analysis leads to a multi-regression model very similar to the one we published, with  $r^2 = 0.80$  and a global MP flux of ~6.1 kt year<sup>-1</sup>. Figure 1 illustrates this with modeled values similar to the measured values for the three added rivers (red dots).

Our selection of field data and conversion methods is not skewed. We mentioned in section 1.3 of the supplementary materials of (1) that synthetic fibers found in river samples could account for more than 90% of the number concentrations but for less than 10% of the mass concentration (6). Consequently, for the purpose of comparing orders of magnitude between river inputs and oceanic stocks, it is of almost no importance whether Cózar *et al.* (7) had considered fibers or not, or whether we had omitted fiber counts in river samples.

In addition, being aware of the existing uncertainties in current statistical models of plastic fluxes, we cannot establish a hierarchy in the quality of predictors such as HDI or population density based only on correlation coefficients ( $r^2$ ). We have precisely demonstrated in our report (1) that the misuse of observations and methods could lead to correlations as high as  $r^2 = 0.9$  together with erroneous plastic flux estimates [see, for example, the studies of



**Fig. 1. Comparison between MP river inputs modeled and measured.** Blue dots correspond to the dataset used in our paper (1); red dots correspond to the three rivers included in Mai *et al.* (2) that are missing from our study.

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Lebreton *et al.* (8) and Schmidt *et al.* (9)]. We do not reject HDI as such, but we argue that when compared with accumulated ocean stocks, HDI does not perform better than population density as a predictor of MP fluxes [see figure 3 of our paper (1) and figure 4 of Mai *et al.* (2)].

We are fully aware of existing limitations in the use of comparisons between river flux and ocean floating stocks to estimate residence time. Accumulation of plastic at the ocean surface also involves larger plastics [up to 100 mm in the study of Cózar *et al.* (7)] and MPs from sources other than rivers. This introduces uncertainty into our conclusions, and we extensively addressed these uncertainties in section 3 of the supplementary materials of our paper. When reading their comment, we are not sure whether Mai *et al.* made the effort to read the supplementary materials.

In section 3.1.1 of the supplementary materials of (1), we also refer to the study of Eriksen *et al.* (10). We specify that macroplastics >200 mm are not included in the surface stock considered in our study because they were determined by visual survey protocol and not by plankton net sampling (10). Because it is methodologically important to consider similar sampling

methods for comparisons, we calculate that, according to (10), the mass of MP takes up >50% of the total plastic mass sampled with plankton nets at the ocean surface, not 13% as claimed by Mai *et al.* (2). Moreover, on the basis of one single study in Hong Kong waters (11), Mai *et al.* suggest in their comment that the oceanic MP surface stock could be as low as 0.4 to 1.87 kt. This is completely contradictory to the findings of Eriksen *et al.* (10), who determined the oceanic stock of MPs <5 mm to be 35.5 kt based on 680 net tows from 24 expeditions in the world's oceans. We are therefore highly surprised by the arguments Mai *et al.* (2) use to demonstrate that our calculation of MP residence time might be fundamentally erroneous. This makes us believe that they probably not only overlooked our supplementary materials, but also are not very familiar with the literature on floating plastic stocks at the ocean surface.

Also, the process of fragmentation of large plastics into smaller pieces is discussed in the supplementary materials of our paper (1). Although this is another uncertainty source, we clearly demonstrate that there are no arguments to connect this to unrealistically short

residence times, as was claimed by previous studies. In conclusion, we admit that the study of Mai *et al.* (2) determined MP in rivers with methods that are in good agreement with the methods we applied in our study. However, we firmly reject the conclusions in the authors' comment that our study is not novel, that we determined a doubtful estimate on global riverine MP fluxes, that our methods are skewed, and that the longer residence times we propose are fundamentally erroneous.

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