Responses to reviewer #1

General Comments

This paper presents valuable contributions to the field of plastic modeling. Specifically, it helps address specific knowledge gaps in the vertical distribution of microplastics by focusing on biofouling, though it also includes other vertical mechanisms. This works goes even further to investigate global and seasonal variations that can impact biofilm growth vertical transport. Overall, the paper is clear and well-written, and I only recommend minor revisions before publication.

We thank reviewer #1 for the time spent reading and reviewing our paper, as well as their positive general comments. We appreciate that the reviewer believes that knowledge gaps are addressed with regards to the transport of ocean microplastic particles in the vertical dimension due to biofouling. We have responded to specific comments below.

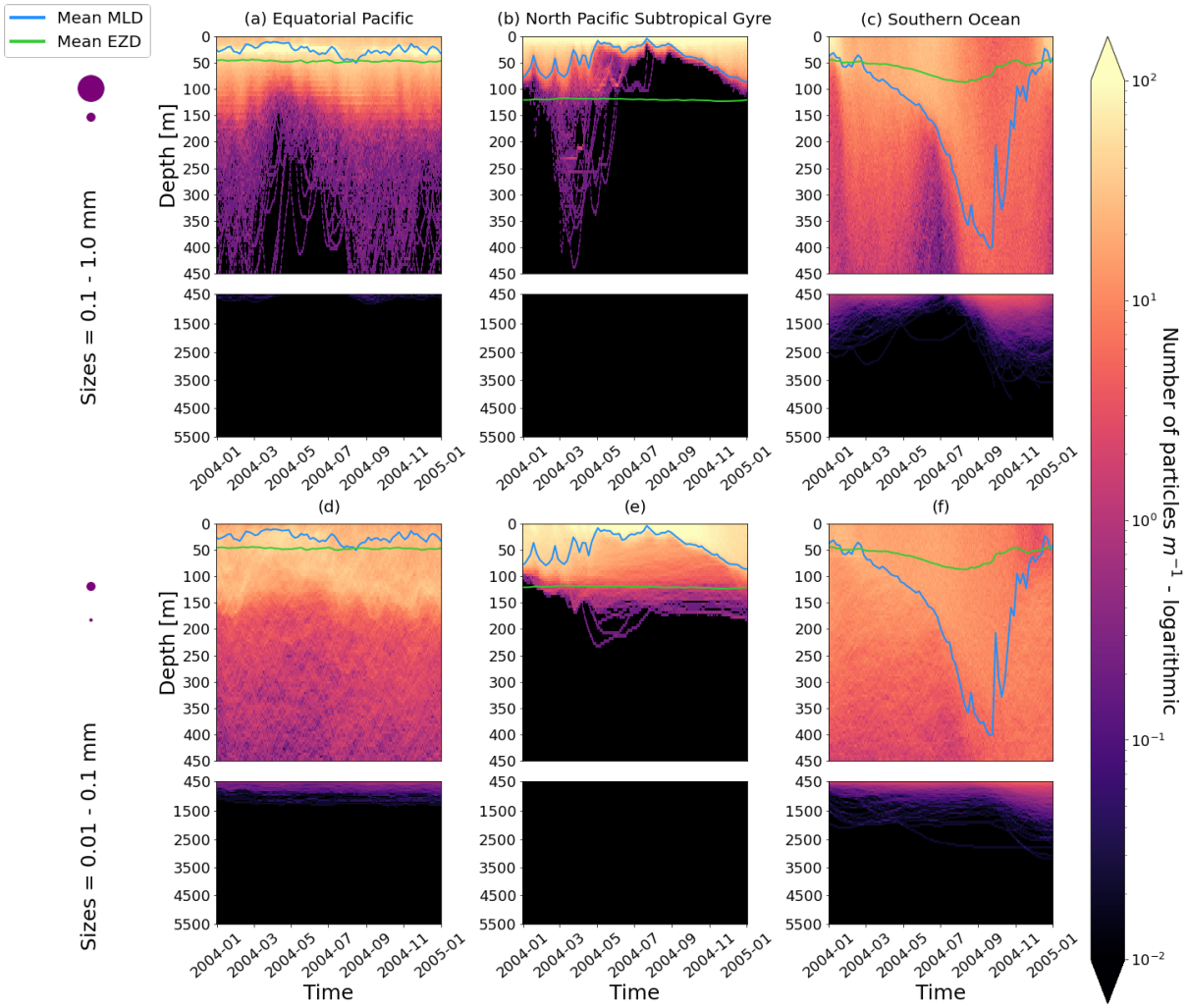
Specific Comments

The integration time step is stated to be 60 seconds around line 95. Did you examine the sensitivity of the model to this choice for time step? Is there some other justification?

Yes, there is some justification so we have now added this in the text - we thank the reviewer for the question: L98-100 "*We tested the sensitivity of our results using an integration time step of 30 seconds and the results did not change (though the simulation time was longer); with a longer integration time step (e.g. 1 hour) we lost some information for shorter oscillation frequencies (e.g. for 1 mm particles)*."

There is discussion on initial density around line 100 to justify the model choice of 920 kg/m^3, but this only mentions comparison to particles with lower density (down to 30 kg/m^3). Do you know what would happen for particles closer to the density of water but still positively buoyant such as HDPE?

We thank the reviewer for this suggestion. We previously thought that since the results in Lobelle et al. 2021 show that the LDPE (920 kg/m3) and HDPE (940 kg/m3) sinking characteristics are very similar, it was enough justification for not running simulations for denser particles. Since we add vertical mixing in this study, however, the results could differ. We have now rerun the simulations with even denser particles (1020 kg/m3) representing rigid polyamide and the results are shown in the supplementary materials as Fig. C2.



We have amended the explanation of the results to: L109-116 "*We have also run two sensitivity analyses for particles with a density of 30 kg/m³ (representative of expandable polystyrene) and 1020 kg/m³ (representative of rigid polyamide). (...) For the 1020 kg/m³ simulations in the NPSG (Fig. C2b and e), the majority of the larger particles mix completely to the base of the MLD (as opposed to 920 kg/m³ particles mostly staying close to the sea surface). The smaller 1020 kg/m³ particles on average resurface slower after being mixed down to 200 m in spring (as opposed to 920 kg/m³ particles that are strongly restricted by the MLD). Particles representing other sizes in other regions with a density of 30 and 1020 kg/m³ produce very similar results to the 920 kg/m³ particles.*"

Around line 180 "We define the biofilm density as 1170, although the use of a denser biofilm does not change our results." What about less dense than 1170 kg/m^3? Why would very heavy diatoms be floating at the surface? Is there a justification for that?

This is also a good point. We have now added the justification in the text, so the sentence reads: L196-199 "*We define the biofilm density as 1170 kg/m³, following results from Amaral-Zettler et al. (2021b); though it may seem counterintuitive that organisms denser than seawater are found at the surface, they are retained there*

due to upwelling or mixing. Also, they can transfer from other floating particles (seaweed, feathers, marine snow, etc) to plastic at the surface." Therefore, these heavy diatoms do not actually "float" at the surface. Empirically, it is known that attached diatoms that are denser than seawater are ubiquitous on any surface particle in the open ocean, and Amaral-Zettler et al. 2020 have shown that within 1 week buoyant plastic at the surface is consistently colonized by attached diatoms (which was also referred to on L381-382 "*... (Amaral-Zettler, 2020). In the latter study, diatoms dominate within the first week of colonisation of microplastic.*").

It should be noted that we have also stated in Section 3.4 (Model assumptions and future model developments) that this is one of our assumptions: L376-378 "*Furthermore, any biofilm on the particle is assumed to be denser than water density, though this has only been observed in coastal waters (Amaral-Zettler, 2021b)*."

Figures 4 and 5: It might be good in the caption to explain more about the white bar removed section from around 400 m to 2000m. What would be going on in this region? A continuation of the behavior from 0 to 400 m?

We thank the reviewer for spotting this since it is true that it was not very clear what the white bar represented. We do not have any missing data - we simply wanted the top panel to have a higher vertical resolution (from 0 to 450 m) than the bottom panel (450 to 5500 m) - we have changed all the figures so that the minimum and maximum of each panel is displayed. See, for example, the figure in the response to adding 1020 kg/m3 particle density results.

Though Section 3.4 does a good job addressing the model assumptions, I think it would help to address a few other assumptions. This model relies on the assumption of defouling, which has only been observed in one study, and oscillations have never been experimentally observed. Additionally, this model assumes any biofilm attachment will be significantly denser than the water density. It may be worth addressing this assumption also.

We appreciate this comment and have decided to dig a bit deeper into some of our assumptions that really cause the oscillations and the biofilm to defoul. We have therefore added the following sentences at the beginning of Section 3.4: L373-378 "*Firstly, as explained in the previous section, the model relies strongly on the assumption that biofilm respiration depends only on temperature. After a particle is biofouled and sinks, continued respiration is the main mechanism for defouling, which in turn leads to the oscillation of the microplastic. Such behaviour is still theoretical and has never been experimentally observed. Furthermore, any biofilm on the particle is assumed to be denser than water density, though this has only been observed in coastal waters (Amaral-Zettler, 2021b).*"

Technical Corrections

Line 5: Phrase "for the physics" is awkward

Thank you for noting this. We have now changed this to: L3-L5 "*The physical specifications include four vertical velocity terms: advection, wind-driven mixing, tidally induced mixing, and the sinking velocity of the biofouled particle.*"

Line 10: “when the processes affecting the settling velocity of the particle and the motion of the ocean are in equilibrium” it is not clear what this line means

This has now been clarified to: L11-L12 “*when the processes affecting the settling velocity of the particle and the seawater's vertical movement are in equilibrium.*”

Line 225: “our simulations only include vertical motion (advection and mixing) in order to isolate localised biological and physical effects on vertical particle displacement” this is confusing

We have added a couple of words to explain that it is the effect of water properties of three different regions that we are investigating. Furthermore, we have added a sensitivity analysis in the appendix to analyse whether 3D advection affects our results, so we have also added a sentence about that here: L244-247 “*We reiterate that our simulations only include vertical motion (advection and mixing) in order to isolate and contrast specific biological and physical factors that affect vertical particle displacement. We have also tested the effects of 3D advection (Appendix E) and we demonstrate that even though particles can travel for thousands of kilometers (Fig. E1) after a few months (e.g. in the EqPac), the results are not largely impacted (Figs. E2-E4)*.”

Line 235: “This seems contrasting” is awkward, maybe “this is in contrast with the findings…”

We have made this change as suggested exactly by the reviewer on L259.

Line 305: “hence Ggrow is the dominant down to the MLD” I think a word is missing here, dominant term?

Yes, we thank the reviewer for spotting this. The sentence now reads: L339 “hence G_{grow} is the dominant term down to the MLD.”

References:

Amaral-Zettler, L. A., Zettler, E. R., and Mincer, T. J.: Ecology of the plastisphere, Nature Reviews Microbiology, https://doi.org/10.1038/s41579-019-0308-0, 2020.

Amaral-Zettler, L. A., Zettler, E. R., Mincer, T. J., Klaassen, M. A., and Gallager, S. M.: Biofouling impacts on polyethylene density and sinking in coastal waters: A macro/micro tipping point?, Water Research, 201, https://doi.org/10.1016/j.watres.2021.117289, 2021b.

Lobelle, D., Kooi, M., Koelmans, A. A., Laufkötter, C., Jongedijk, C. E., Kehl, C., and van Sebille, E.: Global modeled sinking characteristics of biofouled microplastic, Journal of Geophysical Research: Oceans, pp. 1–15, https://doi.org/10.1029/2020jc017098, 2021.