

## ***Interactive comment on “Model uncertainties of a storm and their influence on microplastics / sediment transport in the Baltic Sea” by Robert Daniel Osinski et al.***

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### **General Comments:**

A study presented in the manuscript investigates the transport of particles (having a given set of properties – density and size) by means of a chain of existing and well established numerical models. They include an atmospheric model, a general ocean circulation model, a spectral wind wave model and two sediment transport models in the Eulerian formulation. From the previous studies (Chubarenko, Enders, Khatmullina) the authors adopted the idea that the behaviour of small polymer particles is somewhat similar to those of natural origin, well-studied previously. This led them

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to an assumption that such particles transport reproduced by previously developed models of non-cohesive sediments transport (with modified Shields formulas) may serve as a reliable substitute for microplastics (MPs). Thus the paper addresses a scientific question relevant to the OS scope.

The aim of the research was to obtain an assessment of the sensitivity of the sediments transport model results (erosion, deposition, suspended matter transport) to a stochastic variation in the atmospheric forcing. The authors used an ensemble forecast method, e.g. a simulation with a set of scenarios based on existing reanalysis with introduced stochastic perturbation.

It is concluded that there is a high variability in the amount of transported particles during a storm event. The uncertainty is dependent on the size and density of particles. Meanwhile the space patterns of erosion and deposition areas were stable. The authors promote the use of the chain of models to forecast possible zones of MPs accumulation in order to plan field surveys.

### **Specific Comments:**

1. Please specify what makes it possible to consider your model particles as microplastics. It might also be better to separate the description of the experiments from their interpretation and application to MPs transport prediction.
2. The conclusion made in the last sentence in Abstract is poorly linked to the aim of the study and was hard to understand. Please clarify.
3. Introduction, 2nd paragraph: again two poorly linked sentences. It is not clear how the models can complement field measurements.
4. 4th paragraph: too many assumptions made unexpectedly for the reader. Maybe there is a need for more references. New assumptions could be formulated in the Methods section. ‘The interest of this study’ is not mentioned anywhere in Abstract.
5. Lack of references to existing models. For example: Ballent, A., Pando, S., Purser, A., Julianio, M. F., and Thomsen, L.: Modelled transport of benthic marine microplastic pollution in the Nazaré Canyon, *Biogeosciences*, 10, 7957–7970,

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<https://doi.org/10.5194/bg-10-7957-2013>, 2013.

Nicole Kowalski, Aurelia M. Reichardt, Joanna J. Waniek Sinking rates of microplastics and potential implications of their alteration by physical, biological, and chemical factors, *Marine Pollution Bulletin*, Volume 109, Issue 1, 2016, Pages 310-319, ISSN 0025-326X, <https://doi.org/10.1016/j.marpolbul.2016.05.064>.

A. Bagaev, A. Mizyuk, L. Khatmullina, I. Isachenko, I. Chubarenko, Anthropogenic fibres in the Baltic Sea water column: Field data, laboratory and numerical testing of their motion, *Science of The Total Environment*, Volumes 599–600, 2017, Pages 560-571, ISSN 0048-9697, <https://doi.org/10.1016/j.scitotenv.2017.04.185>.

If the transport of the MPs in the marine environment could not be investigated with the existing models, please explain.

6. Why exactly do you prefer to use the Eulerian approach?

7. Both papers Khatmullina and Isachenko and Waldschläger and Schüttrumpf report settling velocities for still fresh water. Please explain the applicability of their results to salt (brackish) turbulent marine water. How exactly do you use those formulas for the settling velocity?

8. You have not mentioned the values of critical sedimentation/resuspension shear stress and settling velocity for your particles. It might be useful for the future studies and the experiments reproduction.

9. It is important to explain why you use 10 and 330  $\mu\text{m}$  as the size of the particles, which is not common for MPs studies.

10. Page 4: final paragraph - is really hard to understand. Please clarify.

11. Page 9, line 19: 'findings indicate that bathymetry has predominant impact', how exactly do they do this? Is this statement somehow new compared to the results of Enders et al, 2019? I think that Fig. 13 might help you to highlight the new findings.

12. The authors found that with the decrease of MP density and size the ability of models to predict their transport decreases. I think this result is sufficiently supported by the experiments and should be stated more clearly! In fact you showed that small and light MPs (so called nanoplastics) are being driven by waves, while MPs (0.5-5

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mm) are affected by hydrodynamics.

13. Page 11, lines 1 and 2 – seems too obvious.

14. Page 14, 'budget methods' – please explain, what do you mean? The whole paragraph looks unclear. 15. Conclusion section – too many repetitions with the Introduction and methods.

16. Important, but somewhat discussionable is the idea regarding possible future application of the chain of models for MPs sink prediction. Your findings are based on the numerical experiments with the spectral wave model and GCM models with 1 nm grid, which might be ok for the sediments, but MPs distributions show high patchiness and probably high mesoscale variability. Which means that your models might require higher spatial resolution in order to be able to determine possible accumulation zones for the samples collection (since in situ samplings of bottom sediments for MPs are usually sparse and low in volume).

#### **Finally:**

The manuscript represents a good contribution to scientific progress in the microplastics modelling studies, fits the scope of Ocean Science and provides substantial new ideas. It utilizes valid scientific approach and applied models. However the results discussion was somewhat unclear, and the links between some of the expressed ideas seemed not obvious to me. In the Introduction and Methods section too many assumptions are made in order to link sediment transport to the MPs transport. I suggest changing the Title so that 'uncertainties' and 'modelling' look more important than 'microplastics'. The scientific results are presented in a clear, concise, and well-structured way, but I would like to ask to shorten and highlight better the conclusions drawn from those results. Since I have specific comments that may require a major revision of certain paragraphs, I prefer to skip the list of the language evaluation and leave it to the stage after resubmission.

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-28>, 2020.

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