

## ***Interactive comment on “Modelling the cohesive sediment transport in the marine environment: the case of Thermaikos Gulf” by Y. N. Krestenitis et al.***

### **Anonymous Referee #2**

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In this paper, the authors propose a model based on the particle tracking method considering the main processes affecting cohesive particles. This model is applied to the Thermaikos Gulf. Different simulations are discussed. The final objective is to use this model to forecast the seawater quality.

### **General comments:**

The model is generally well presented (some remarks and questions are given at the end of this review). My main comments concern the section Results. I am not convinced by this section: the choice of the simulations, the selection of results presented in the figures and the interpretation of these results.

Three sediment transport simulations are used (two years and one month) based on

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three hydrodynamic simulations. The justification of this choice of three simulations is not given. The origin of the simulations is not clear (one reference to Kourafalou et al., 2002 in page 712 and one reference to the POSEIDON forecasting system in page 713). Moreover, after reading this paper, I cannot see the advantage of these different simulations as no comparison is provided and only some snapshots of the different simulations are given corresponding to summer for one and winter for the two others (Figures 8, 9,10 and 11). The meteorological conditions (especially wind) present a large variability which directly influences the coastal circulation including river plumes. The consequence is an important variability of suspended particulate matter patterns making the presentation of snapshots inappropriate.

I think that one annual simulation (with a reference) would be enough with a serious analysis of the seasonal patterns of suspended matter and deposition. In the same way, the analysis of these results should be done with the help of current fields and not only with vague sentences as “an anticyclonic gyre that is probably responsible” or “the particles” probably moved – as all the results of the hydrodynamic model are available for the interpretation.

### Specific comments:

P. 704: The reference to Kourafalou is inappropriate for the association of fine particles with biochemical substances.

p. 707: Richardson number. The current shear is at the power 2. I did not find the values of the diameter and density of the primary particles.

The value of these parameters has to be justified as the results are highly sensitive to sedimentation velocities. If no justification can be given from experiments, a sensitivity study should be done. The density of primary particles is noted  $\rho_{os}$  and  $\rho_{o0}$  in page 708. P 708: How do you calculate the suspended matter concentration. p709: As the Huthnance et al., 1997 is a report, some explanations concerning how Eq 17 (critical stress for deposition) was obtained is necessary.

Eq. 18: you must explain how the erosion rate is converted in particles and where these particles are injected (centre of the grid cell?)

p 709–710 choose between N/m<sup>2</sup> and Pa for the stress unit. p 709 line 20: shelf-weight?

p 709: Sorry, it is not clear for me if Eq 19 for porosity is applied to deposited sediment only or also to suspended flocs in relation with Eq. 13. Please explain!

p 710: I don't understand how consolidation is taken into account for further resuspension as particles settle on pinpoints. It seems contradictory with the assumption that "the properties of the seabed are uniform with depth". Do you have a separate treatment for newly deposited sediments and sediment that are introduced from the seabed for the first time (old sediment)?

p 712 line 11: "that are in fact" prefer "that determine" I have a problem with the time series of the particles entering the Gulf (Figure 4). These time series correspond to a specific year. They present two pronounced peaks during winter probably associated to specific meteorological events which have induced floods. These time series are used in this paper with hydrodynamic simulations and meteorological forcing for other years which are then not correlated to these time series. I think that the authors should use something like a climatology of rivers discharge or if not available typical values for the four seasons.

p. 712: nothing about TOYS. see general comment above for the choice of simulations.

p 713 line 25 corroborated (not collaborated)

Figure 5 should present the trajectories of particles chosen for Figures 6 and 7.

Figures 6 and 7 (and 5 with the previous comment): Put some common marks at characteristic points on these figures making the readers able to associate the parameters.

Horizontal scale of Figure 6: distances seem too important (a factor 10?).

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p714 lines 23–25: explain the link with the previous sentence.

p 715: line 5 The dissimilarity... The two runs show different fates for the Pinios particles (northward or southward dispersion in Fig 9 a and b). What does it mean for these two runs? Is it a consequence of short term events with different meteorological forcing for the two runs (it could be linked to the dispersion of particles delivered during the two peaks of Fig 4)? If it is linked to long term circulation different in the two runs, it is a problem. In the first case, it is a justification of using smoother particles discharge. In the second case, the hydrodynamic simulations for the two years need to be discussed...

line 9: gyre (in place of gear).

lines 8–10: Don't you think that the northward subsurface and bottom current induced by the prevailing upwelling conditions could be responsible of the deposition north from the river outflows.

line 23: "often reported incidents"??

p 716: Conclusion lines 26–27 ...general cyclonic pattern... I think this remark was never done in the previous sections and it is not clear from Figure 9 for instance. Moreover the asymmetry between the east and west coasts is not clear from the figures. How do the authors explain that? The seasonal current fields would be interesting to clarify this point.

p 717 line 5: accurate is not suitable if we consider the large uncertainties in the parameters of the sediment transport model.

Figures: The axes labels are often inexistent.

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