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Ocean Science Discussions

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

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RC: *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:

RC: *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 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.

AR: The authors would like to thank the reviewer for his-her constructive criticism.

With reference to the selection of the results, the three simulations presented in the paper were selected simply since they where the ones implemented during the MFSTEP Project. The remaining aforementioned general comments are answered in detail below, following each specific comment.

Please, find below a step-by-step response to all the comments. We have revised the manuscript very carefully and we hope that we have addressed almost all the concerns expressed by the reviewer.

Specific comments:

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

AC: Corrected: Wang and Pinardi (2002)

RC: p. 707: *Richardson number. The current shear is at the power 2.*

AR: The current shear is indeed at the power 2. The equation was written accordingly at the submitted manuscript but was not published in the same manner at the open discussion.

RC: *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.*

AR: Suspended sediments at the river mouths affecting Thermaikos have been characterized as very fine silt and clay (Karamanos and Polyzonis, 1998). Thus, in accordance to the grain size classification (McLane, 1995), the diameter of the primary particles is taken to be 3 μm, which is the mean value between very fine silt (3.9 μm) and clay ($\sim$ 2 μm). The above paragraph has been included in section 3. Description of the study area, following line 17 in p 711.

As for the initial density of the particles at their introduction to the aquatic domain from the Source Rivers, the porosity of the aggregates has been assumed to be

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0.5, thus from Eq. 13 the value of initial particle density is taken to be :

$$\rho_{ag} = (1 - 0.5) \times 2650 + 0.5 \times 1000 = 1825 kg/m^3 \tag{1}$$

RC: *The density of primary particles is noted rhos and rho0 in page 708.*

AR: There was an error in the submitted manuscript, since ρ_o in equation 13 denotes the density of the solid phase ($\sim$ 2650 kg/m^3) and ρ_s is the density of the primary aggregates. Corrected.

RC: P 708: *How do you calculate the suspended matter concentration?*

AR: Suspended matter concentration is calculated by the plethora of particles in every grid cell multiplied by the particle mass, giving the total suspended mass at the cell, divided by the volume of the cell.

RC: p709: *As the Huthnance et al., 1997 is a report, some explanations concerning how Eq 17 (critical stress for deposition) was obtained is necessary.*

AR: The parametrization used by Huthnance at al. (1997) is based on the equation proposed by Pohlmann and Puls (1994) in which the specific values of the constant parameters involved have been obtained by deposition tests with mud. The reference to Eq 17 was altered to: Pohlmann and Puls (1994)

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

AR: The erosion rate ε [kg/m^{2}s] is multiplied by the horizontal spacing, which is the area of the grid cell [m^2] and by the time step [s], thus providing the mass of eroded

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sediment [kg]. The total mass set to motion due to erosion at each grid cell is divided to the typical particle mass and the closest integer of the division gives the number of eroded particles at each cell. The injection point of these particles is calculated by an algorithm that distributes the particles evenly to the grid area. Specifically the grid cell is divided to a smaller grid the dimensions of which ($\alpha \times$ b) are the integer of the square root of the number of the eroded particles (α) and the integer of the division of the number of the eroded particles to α (b). In the form of equations, if n is the number of the eroded particles:

$$\alpha = int(\frac{n}{\sqrt{n}}), b = \frac{n}{\alpha}, nn = \alpha \times b \quad (2)$$

Thus, from the total number of the eroded particles, nn particles are evenly distributed at the center points of the new nested grid and the remaining n-nn are injected at the center of the grid cell. The above analysis has been added to the paper, following line 19 in p 709.

RC: p 709-710 *choose between N/m^2 and Pa for the stress unit.*

AR: Corrected

RC: p 709 line 20: *shelfweight?*

AR: Corrected to self-weight

RC: 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!*

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AR: It is applied to deposited sediment only.

RC: 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)?*

AR: Newly deposited sediments are the ones that undergo self-weight consolidation, by the increase of the critical stress for resuspension with time the particles remain deposited (Eq. 20, in which t is the time the particle remains in deposition). After remaining at the bed for the period considered for full consolidation the particle is considered to be and treated as part of the seabed. As far as the bed is concerned it is considered to have uniform properties with depth and the particles that enter the domain from the first time by erosion of the seabed are treated differently (Eq. 18, erosion).

RC: 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.*

AR: Unfortunately there are no available time-series for the river discharges, thus correction of the values was impossible. We agree that the winter peaks are in fact due to specific flood events but in lack of other available data these were the only

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ones that could be employed.

RC: p. 712: *nothing about TOYS. See general comment above for the choice of simulations.*

AR: Added following line 1 in p713: Input velocity and oceanographic parameters fields for TOYS are outputs of the NAS model with perpetual year forcing (Kourafallou and Barbopoulos, 2003; Korres and Lascaratos, 2003), describing seasonal circulation patterns.

RC: p 713 *line 25 corroborated (not collaborated)*

AR: Corrected

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

AR: The comment was found to be correct, so Figs. 6 and 7 have been changed to be in accordance to Fig. 5. Specifically Fig. 6 concerns the particle denoted with magenta line in Fig.5 whereas Fig. 6 concerns the particle symbolized with green line in Fig.5 and both Figs now contain three subfigures (a,b,c). In subfigure a the vertical movement for each particle is presented, subfigure b shows the changes of particle characteristics with time and c depicts the movement of the particle in the horizontal plane (which is essentially the same as Fig. 5 but shows each particle movement individually). Furthermore common marks where placed in all three subfigures to enhance the clarity of the movements and alterations.

RC: *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.*

AR: Done (please see the response to the previous comment)

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

AR: The distance in fact seems too large but it corresponds to every micro-movement the particle made during its propagation in the gulf, which in general is very stochastic and forms in spirals (see Fig.5). The horizontal axis in Figure 6 denotes the total travelled distance on the horizontal plane. One could say that it is the movement of the horizontal trajectory (Fig. 5) if one could "stretch" it to a strait line.

RC: p714 *lines 23-25: explain the link with the previous sentence.*

AR: The high sediment accumulation in the inner gulf and the low depths of the area indicate high suspended sediment concentrations, which in turn denotes areas of potentially high nutrient levels and enhanced primary production, especially at periods with seawater temperatures favorable for algal growth (early spring).

RC: 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 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...*

AR: The dissimilarity related to the sedimentation in the gulf from sediments originating from Pinios is a consequence of the difference in the circulation of the gulf between the two one-year runs. Circulation of the surface waters in the outer Thermaikos (Kourafalou and Barbopoulos, 2003), as simulated with the perpetual year forcing, is anti-cyclonic in the winter and autumn seasons and turns cyclonic during spring and summer with southward freshwater flow along the western coast. Thus the depositional trend of Pinios as predicted by TOYS is northward due to the anticyclones formed in the area during winter and autumn and is considered to be non-associated to the peaks in the time-series of riverine input, since the outflow is generally higher during these two seasons. Therefore the hydrodynamics of the simulation in question would force the majority of particles from Pinios to deposit to the south, regardless of the specific values of particle inflow.

Regarding the S1A2 period there are no published data from the NAS model. However during 2002 a pilot program to study the circulation in the Aegean Sea using Global Positioning surface drifters was underway during which 30 drifters were launched in the northern Aegean (Olson et al., 2006). Some of the drifters reached and entered the Thermaikos gulf and the trajectories indicated the existence of The results have showed the presence of a coastal jet along the western side of the Aegean in 2002, which is in accordance with the tendency of particles from Pinios to move to the south of the gulf in the S1A2 experiment (fig 9b).

During J03 the main characteristic of the circulation in the gulf is a southward buoyancy driven current along the western Thermaikos, that appears strengthened by southward winds at the end of the month (Kourafalou and Tsiaras, 2006). This coastal current is responsible for the spreading of Pinios plume to the south towards the Sporades basin (fig. 9c).

RC: *line 9: gyre (in place of gear).*

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AR: Corrected

RC: *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.*

AR: We feel that there has been a misunderstanding regarding the statement in lines 8-10. This specific remark was made for movement of the surface waters of the western coast near the Pinios outflows. It is noted that under upwelling-favorable conditions the southward current at the western coast is strengthened and enhances the offshore removal of Pinios plume (Kontoyannis et al., 2003).

RC: *line 23: "often reported incidents"??*

AR: The phrase was corrected: "Surface nepheloid layers (SNL) and bottom nepheloid layers (BNL) are present in both seasons, as often reported by various researchers (Poulos et al., 2000; Karageorgis and Anagnostou, 2001)".

RC: 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.*

AR: Regarding the general circulation pattern in the gulf the remark about the cyclonic movement was made in p 715, lines 10-12. It is a common fact that highly saline north Aegean waters enter along the eastern coastline and force the waters in the gulf in a semi-permanent cyclonic movement. There are however seasonal deflections from this pattern with smaller of larger-scale anticyclonic gyres mainly due

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to the variation in riverine and Black Sea Waters (BSW) inflow (Kontoyannis et al., 2003).

The distinction between east and west coast with regard to depositional and circulation patterns may be difficult to make by the figures that show the spatial variation of the location of particles, like figure 9. This is due to the large amounts of particles included in each simulation, making visual differentiation between locations of higher and lower particle accumulation difficult. Nevertheless, sediment accumulation, as simulated by the model, is much higher along the eastern coastline.

Seasonal currents cannot be presented by the authors, since the hydrodynamic circulation is the work of other scientists, kindly provided for the purposes of the MFSTEP Project.

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

AR: 'Accurate' was used in the sense that it covers the main processes that controlled the phenomena. There are, however, uncertainties involving in the values of some constant parameters in the model. The adjective was deleted from the paper.

RC: *Figures: The axes labels are often inexistent.*

AR: Corrected

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