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Interactive comment on "Thin terrestrial sediment deposits on intertidal sandflats: effects on pore water solutes and juvenile bivalve burial behaviour" *by* A. Hohaia et al.

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This paper describes a follow-up experiment on earlier published experiments of Cummings et al. (2009) on the effect of terrestrial silt deposition on the attractiveness of estuarine sediment for larval recruitment. The earlier results demonstrated that sediment (which was defaunated and sieved prior to testing) becomes less attractive for larvae when it is sealed by a TS layer. This was reflected in a smaller fraction of test larvae that immediately burrow into the sediment.

The causes for this reaction remained unsure. Biogeochemical measurements suggested that the main effect of the silt layer on sediment biogeochemistry is 'seal-

ing'. The TS layer increases diffusion distance and therefore forces the sediment metabolism to more anaerobic pathways. It also affects pH in the upper sediment layers, even if the causes of that are not very well understood. The authors ascribe it also to changed diffusion conditions, but there is likely at least a contribution from a shift to more anaerobic pathways and therefore intensification of reoxidation of reduced substances in the upper sediment layers. In addition, surface reactions and exchanges with the clay-rich TS layer may have complicated effects on pH. Anyway, the net effect is that the sediment becomes more anoxic, more acidic and possibly more toxic. Possible cues for the animals could be the TS layer itself, due to its texture or other characteristics, or the change in chemical substances diffusing out of the sediment. In particular, substances indicating reduced conditions are prime candidates.

In order to shed more light on this, the authors have devised a clever experiment. By changing the organic content and therefore the reduced nature of the sediment they can test how important these clues are. By changing the degree of bioturbation, in comparison to the Cummings et al. experiment, they can also change the relative importance of accumulation of anoxic substances. The design is fully factorial, so that different combinations of outcomes should give a clear picture of causal mechanism. The results of the experiment were a bit surprising. TS in itself had a positive effect on the animals. Apparently they may like the texture, the substance may be easier to dig in, or something else may cause this. The animals were also not repelled at all by natural sediment covered by TS. They did react negatively, however, to the lack of organic matter (and food) in the sediment stripped of its organic content.

So, is the problem solved? It is strange that the authors have overlooked one very obvious aspect of experimental design. You cannot easily compare results of experiments separated in time and detailed methods, because there might be variability caused by the batches of animals used, the specific sediment used, the current conditions or other details of the experiments. So if we observe no negative effects of TS on natural sediments in this experiment, while Cummings et al. did observe such negative effects on sediments that had previously been sieved and defaunated, what is the cause? Is it due to the manipulations such as sieving (which brings solid reduced substances in the surface layers where they would normally not occur, leading consistently to severe depression in pH), to the effect of bioturbation in the unmanipulated sediment, or to differences in the batches of larvae? There is no way to tell, and therefore the present experiment remains inconclusive despite the clever addition of new treatments. I cannot understand why the authors have not included a 'Cummings' treatment into their present experimental design. Not only would it inform on the repeatability of the experimental results, it would also have been able to answer many questions that remain open now. The discussion should pay more attention to this aspect.

Apart from this problem, I also missed due consideration of the transient nature of the biogeochemical conditions. The sediment is relatively rich in organic matter, and it is likely that the total rate of organic matter degradation will not be greatly influenced by the oxygen conditions. At equilibrium, the DOU should therefore not change much depending on whether aerobic mineralization dominates, or anaerobic mineralization followed by reoxidation of reduced substances. However, equilibrium conditions set in only slowly because a pool of reduced substances will buffer the reactions. Consequently, it cannot be expected that the measured conditions of oxygen, redox potential and pH are stable after one day, nor that they are comparable with the conditions in Cummings et al. 2009 who used different timings and also different conditions of currents in the overlying water. Due to the sediment mixing in Cummings et al., transient phenomena in pH profiles may have been much stronger than in the present experiment. The authors should pay sufficient attention to this, and not discuss their profiles as if they were equilibrium profiles. They should also discuss possible effects of these transient phenomena (e.g. transient radical lowering of pH in the top sediment layers as observed in Cummings et al) on the larval behavior.

The manuscript is very clearly written and I have only one textual remark. Somewhere in the discussion (line 9, p. 14849) lowering of pH is associated with a decrease of

hydrogen ions, which obviously is an error.

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