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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 and discusses the results of a flume experiment, designed to quantify the burrowing behavior of juvenile Macomona liliana in intertidal sediments subjected to the deposition of terrestrial sediments. This is interesting, given the problem of changing sediment dynamics in many coastal/estuarine areas. The experimental design also includes a treatment that allows identification of the role of organic matter availability vs. natural 'bioturbated' sediment. In general the applied methods are OK, but the factorial design does however not allow to discriminate between effects of the absence of organic matter (and related cues, depending on burning of sediment and other manipulations) vs. the interference effect from bioturbation. The author should consider this in their discussion. Further, while the use of larvae that are known

to actively select and disperse after settlement instead of juveniles (that need to burrow to escape from predation and low tide conditions, i.e. desperate larvae/juvenile theory) would have been more informative here, the present study is still relevant considering post-recruitment dispersal induced by erosion of the sediment bed. I think the authors should add this reasoning somewhere to indicate the valid of their study.

In its current form, the paper merely focuses on the different outcome from this experiment and a previously performed experiment (that showed decreased burrowing in sediments with a layer of deposited sediment, while the current experiment shows subtle enhanced attractiveness in fine sediment deposits). This is interesting but I would suggest focusing more on the results obtained in this present study since it is difficult to compare both studies because methods of manipulation (and thus porewater chemistry) are different. Such discussion can partly remain but should not form the bulk of the current discussion. Actually, I am not surprised by these new results, the deposited sediment may simply be easier to dig into than the natural sediment and combusted sediment has been shown less attractive than sediment were microorganisms are present (e.g. Sebesvari et al. in JEMBE). This may be also simply result from changes in texture and chemical cues associated with combustion. In addition, table 2 reports the appearance of benthic features at the sediment surface of the CTS treatment, among others green patches of microphytes. It is necessary to present these results for the other treatments as well. For example, the presence of microbial biofilms has been shown instrumental to settlement decisions of benthic invertebrates. For example a couple of papers by Keough et al. in JEMBE (mid nineties) but also for the functionally very similar Macoma balthica (Van Colen et al. 2009 in Marine Biology).Undoubtedly, the appearance of such biofilms was different among the 4 treatments due to the presence/absence of grazing/bioturbation and the combustion of biofilms but no discussion on the role of these microphytes is given. Finally, the authors should also indicate that (the little amount of) dispersed organisms did not interfere with the burrowing results in the other corers. If dispersed test organisms would however deposit in the other corers, individual burrowing decisions per treatment are no longer

independent and other statistics should be used, e.g. log-linear frequency models.

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