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Interactive comment on "OMEN-SED 0.9: A novel, numerically efficient organic matter sediment diagenesis module for coupling to Earth system models" *by* Dominik Hülse et al.

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Dominik Hülse and co-authors present a new analytical model of early diagenesis that is much more advanced than any other analytical model that has been developed so far. It has some inherent limitations (steady state, no compaction, etc.) but it is clearly a very useful, flexible, and efficient tool. As explained and demonstrated by the authors, it can be coupled to any ocean or earth system model to define biogeochemical fluxes across the seabed. Ocean models have been previously coupled to diagenetic models that are solved numerically. However, the down-core spatial resolution of these models is very coarse and the number of sediment layers is limited in order to save

computation time. Due to these limitations, the numerical models have to assume that organic matter raining to the seabed has a low reactivity. Only under this assumption the coarse-resolution models can generate meaningful and numerically stable results. Analytical models have unlimited spatial resolution and can therefore be used to simulate the rapid degradation of very labile organic matter at the seabed. This aspect is the major advantage of the new model presented by Hülse et al.

There are three critical points that the authors should address:

1. The model ignores sulfide precipitation and pyrite formation. Consequently, dissolved sulfide produced by sulfate reduction and AOM at depth diffuses upward to be either oxidized by oxygen or released into ambient bottom waters. This is a very unrealistic set-up. In most sediments dissolved sulfide is removed from the pore water by pyrite precipitation while the remaining sulfide is oxidized with ferric iron, nitrate and nitrite before it can reach the oxic surface layer or the ocean. Aerobic sulfide oxidation is only important in highly reactive surface sediments where the diagenetic sequence is not maintained but several electron acceptors are used simultaneously. The model is based on the assumption that electron acceptors are used sequentially rather than simultaneously. Hence, it cannot simulate situations where aerobic sulfide oxidation is important but creates high rates of aerobic sulfide oxidation in geological settings where this process does in fact not occur. The authors should try to fix this problem. They could for example abandon the model parameter that defines the fraction of dissolved sulfide that escapes into bottom waters. In the modern ocean, sulfide leakage from sediments occurs only in very rare situations and it does not make sense to simulate these anoxic sediments with a model that ignores iron cycling, pyrite formation and sulfide precipitation. The authors could instead introduce a parameter that defines the fraction of sulfide that is precipitated as pyrite and update the alkalinity model accordingly.

2. The authors use an empirical equation by Middleburg et al. (1997) to define burial velocity (w) as function of water depth (Eq. 46). Unfortunately, w is seriously overes-

timates by this equation. As an example, w at 1000 m water depth results as 160 cm kyr-1 applying Eq. 46 whereas the available data indicate global mean rates in the order of 10 – 20 cm kyr-1 for this water depth (Burwicz et al., 2011). The extremely high burial velocities derived from Eq. 46 compromise the TOC concentration and other model results especially when the model is applied at global scale.

3. OMEN-SED is able to reproduce the strong down-core decrease in organic matter reactivity observed in marine sediments by using two or more organic matter fractions with widely different reactivity. This strength is nicely demonstrated in section 3.3 where the authors are able to show that typical pore water profiles are reproduced by the model applying kinetic constants (k1, k2) that span several orders of magnitude (Tab. 13). Subsequently, the authors try to reproduce the TOC distribution at the deep-sea floor by coupling OMEN-SED to an earth system model. I think that TOC in surface sediments is not a good parameter to validate the model because almost the entire organic matter raining to the deep-sea floor is degraded in the surface sediment rather than preserved as sedimentary TOC. TOC concentrations in surface sediments at the deep-sea floor are governed by TOC rain rates, mass accumulation rates (burial velocity), adsorption of organic matter on mineral surfaces, and the kinetic properties of the very small refractory fraction that survives degradation (about 1 % of the total rain rate). The strength of OMEN-SED to degrade the reactive fractions in a meaningful way does not play out in this application. Moreover, the model results are unrealistic. The best fit to the TOC data is apparently obtained assuming that the organic matter flux to the seabed is composed of two TOC fractions with very low reactivity in the order of 0.001 – 0.01 yr-1 (Fig. 12). This result is not consistent with the case study presented in section 3.3 that yields much higher k values (Tab. 13). Moreover, we have shown previously that this very low reactivity is not consistent with the benthic fluxes of oxygen and nitrate that have been measured at the seabed (Stolpovsky et al., 2015). The error may be caused by the too high burial velocities applied in OMEN-SED (Eq. 46) and/or may be related to the rain rate and reactivity of organic matter calculated in GENIE. I would encourage the authors to delete the entire section 4 of the paper because it does not add useful information but presents rather misleading results. They should aim to present other more useful applications of their highly innovative analytical model in follow-up publications.

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