

Interactive comment on “Theoretical Interpretation of the Exceptional Sediment Transport of Fine-grained Dispersal Systems Associated with Bedform Categories” by Tian Zhao et al.

Anonymous Referee #2

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In this manuscript the authors propose to explain the intensity of sediment transport in what is called 'fine-grained dispersal systems' (FGDS) with a set of existing empirical relations between sediment transport, the presence of bedforms and their effect on the hydrology of the system. Although a final mechanistic explanation of this highly coupled system would be certainly welcomed, this work does not really address that because by definition, it only includes empirical correlations without a clear causal relation or predictive power (beyond the conditions for which those relations were obtained in the first place).

Even more problematic, in my opinion, is the fact that based on the title and the motiva-

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tion of this study, there is no actual problem to solve. Going through the original paper by Ma et al. (2017), and the Engelund and Hansen excellent 1967 monograph in which Ma's results are based, it is clear that the central problem addressed in this paper was already solved. I mean, there is already a theoretical interpretation of the exceptional sediment transport of FGDS clearly stated in Ma et al. 2017: For fine sediments where the dominant transport mode is suspension, bedforms tend to disappear (shown by data) and the system approaches the upper-regime plane bed; in that case bedform drag is negligible and the sediment flux scales with the shear stress to power 1.5 (as in a flat bed). On the other hand, in the presence of dunes (found for pure bedload or mixed transport mode), form drag decreases the shear stress available for transport, which reduces the net sediment flux. Engelund and Hansen (1967) used rescaled experimental data to show the scaling of this effect and the implications to hydrology and sediment transport (now scaling with shear stress to a 2.5 power). Of course, their analysis contains the central effects of bedforms in an empirical form roughly equivalent to the one proposed by van Rijn (2007). Even more, in the review by Charru et al (2013) there is a potential physical mechanism for the upper-regime plane bed transition, as the characteristic wavelength of the dunes scale with the saturation length that for suspended sediment transport is very large. This essentially explains the large transport rates found in fine-sediments dominated environments.

In summary, I don't think there is enough novelty in this manuscript to justify publication, at least in the context of existing empirical formulations. Thus I recommend rejection.

In addition to those more fundamental comments, I also found the model explanation in the manuscript very difficult to follow. There are many missing equations and no clear description of the physical context where those equations fit in. Also, there is no analysis of the validity and limitations of the empirical equations; there is no proper discussion of why their model does not reproduce the empirical results of Engelund and Hansen (1967); there is no rationale for the arbitrary classification of bedforms based only on the total dimensionless shear stress (called mobility parameter) in contrast

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with well-known empirical diagrams of bedform regimes; there is no definition of mega-ripples in this context (?).

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