

Interactive comment on “Particle size dynamics in abrading pebble populations” by András A. Sipos et al.

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General comments: The paper is well written and structured. The objectives are clearly outlined at the beginning and well assessed at the end of the paper. As a geologist, I find the mathematical approach to geology-related issues useful to get deeper insights on such matters, which I obviously always addressed from a process-driven point of view. However, I'm not qualified to evaluate the maths behind the methodologies of the present manuscript. I find the results consistent with my experience about sediment transport, abrasion and movement, I cannot rise any geological inconsistencies.

Specific comments: I have just a few observations/requests for the Authors: 1. for the most part the theory about sediment transport, movement activation thresholds, depo-

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sition, etc. has often been linked to the studies of Hjulström and Shields, who provided two popular diagrams that constitute the basis for such analyses. I see that the Authors did not cite them in the Introduction, and I wonder why. Are they obsolete? Or not really useful for your scope? Anyways, I'd like to ask the Authors to comment on this aspect. 2. being a field-bound geologist, I must admit (embarrassed) that I'm forced to accept mathematical models as a sort of leap of faith. Therefore, I cannot separate the model results from what I do observe on the field. My request is simple, then: you state in the Conclusions that your results are compatible with existing geological observations. I kind of reverse this statement. What would you do to further back up your conclusions? Could you think of some way to confirm your results? Just giving some perspectives about this aspect would strengthen the paper from my point of view. 3. paragraph "1.1 Geological observations": I've always been inclined to think that transport effects on sediments would prevail in a unidirectional flow, while abrasion effects would increase when the processes leading to sediment movement are more chaotic, multi-directional, and less predictable. Could you please comment on that?

Technical corrections: 1. even though largely used, "shingle beach" is not a fully technical term. I would prefer the more general "coarse-clastic beach" to refer to a beach constituted by coarse sediments. 2. typo, Figure 2 caption: "... relative size variation of the a mass...". 3. typo, paragraph 2.3 Collision kernels, line 15: "... a trade-off between between physical..." 4. typo, paragraph 3.2 Fitted lognormal distribution, line 4: "... in the discrete dsitributions."

Hope this helps!

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