

Interactive comment on “A spatially distributed analysis of erosion susceptibility and sediment yield in a river basin by means of geomorphic parameters and regression relationships” by S. Grauso et al.

Anonymous Referee #2

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General comment:

The paper presents a desk study to assess sediment yield and erosion susceptibility in the Calvano catchment. The authors clearly define their work as being empirical based on “simple cartographic data” and focus in my opinion on “what is still possible to assess”. The paper applies one method for this assessment and tend to perform a validation using inaccurate, soft data of small reservoir siltation. Generally, I could think the paper was written in such a way it was good to understand.

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The article does not claim to have novel aspects, and the paper does not clearly state what has been added with this regression method using geomorphic indices by the authors. The paper also does not give insight in physical behaviour of the catchment, which it also not aims for. This focus of the article therefore is on the practical aspects of such a cartographic, desk study, evaluation of erosion susceptibility and sediment yield. I gladly reckon the value such work can have and I believe such work is worthwhile publishing if supported strongly by validation data. Such papers should, in my opinion, then clearly show and proof their applicability. It is the absence of proof of the validity of the approach/method that was missed most. In my opinion, I was left with too many open ends.

In the current form I recommend rejection of the paper. Significant major revisions are necessary to accept the paper, but honestly, it will be a tough job to gather enough validation information.

I continue with some specific comments without claiming to be exhaustive.

Specific comments:

Title: is the study not more an assessment of SSY instead of erosion susceptibility?

P 630, L.15-19: sediment accumulation (flood control) is one of the main reasons for this study, however, the relation between sediment yield and sediment accumulation is not discussed anywhere in the paper.

P 631: The description of the region / study site is quite extensive with limited direct use for the applied methodology (faults, tectonics). It is also not used in the discussion of the results. So I would propose to be more concise here.

P 632, L 4: Area is prone to floods and slope instability. That is interesting as landslides are known for their important contribution to sediment yield. But no attention is given to this source of sediment. Is the landslide susceptibility in the Calvano watershed the same as in the 20 catchment where the regression relations have been developed

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earlier by Ciccacci? I presume, it is also worthwhile to know whether riverbank stability is the same as in the Ciccacci case studies? And how does the land use change (see next comment) relate to it?

P 632, L.17-24. As in a large part of the Mediterranean also in the Calvano watershed land use has changed and natural vegetation is slowly re-colonizing the abandoned agricultural lands. The fact that Ciccacci established a regression relation 20 years ago brings to my mind that land use is most probably not the same and thus brings doubts to the applicability of the regression method to another catchment. The author should proof clearly that the application of the method is valid in their study area.

P. 633, L. 1-11. Show similarity between 20 watersheds of Ciccacci and the Calvano watershed.

P 634, L.3-14. This part seems positioned wrongly. I expect these kind of argumentations in the introduction as a motivation of the choices made, but not in a section that describes the method technically.

P 635, L.9. Are there more corrections proposed/used of the original regression relationships?

P 636, L.24-26: On what basis is an additional subdivision made for badland areas? This is “expert” knowledge, but why not also land use, or riverbank height (as related to riverbank instability).

P637: two decimals seem not appropriate to me for such an inaccurate, empirical methodology.

P 638-639: I personally would present some of the results also in graphs, tables are not always the most instructive. Catchment size and SSY using data of table 1-2-3 (three symbols) by example.

P 638, L.11-12: Reference is needed for the remarks that the SY of Calvano is very close to average SSY in central Italy.

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P 638, L.17: 1600 Mg km⁻² year⁻¹ (Lupia Palmieri, 1983). More background on this number is necessary (is climate, lithology, land use, available reservoirs, size of catchments etc comparable?), how has it been derived.

P640. L 6: The assumed bulk density should be explained and possibly have reference. Is bulk density the same for all reservoirs? It could be different!

P 640, L.18-24: What is the mechanism behind this supposed difference in SSY for North facing and South facing catchments/slopes? Is there a relation with lithology/geology by change, or has it to do with land use? In the discussion section this can be discussed in more detail.

Section 5: conclusions are a bit minimal. P 641, L.5. “can be considered reliable”. The paper did not convince to reach this conclusion, even not in the order of magnitude”. P 641, L.6-7: “matches average observed SSY in river basins of central Italy flowing to the Adriatic Sea.” See remark earlier (P638, L17)

P 641, L.12: See remark P640, L18-24.

P 641, L13-19: In the beginning of the paper the authors mention the Ungauged basin problems and the possibilities this approach has while using basically only cartographic data. Do the authors conclude that this approach is indeed direct applicable to ungauged basins in central Italy or other areas in the world?

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