

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.

S. Grauso et al.

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We are very grateful to the Anonymous Referee for his review, which is detailed and carefully organised. It is clear that a refined evaluation of our manuscript was carried out by the Referee and we sincerely think he provided a valuable contribution to our study and to science in general.

We do not agree with the general opinion of the Referee about our analysis. The lack of a physical/mechanistic fundament and the risk originated by out-of-context extrapolations is a classical objection to the use of empirical methods, like regression equations, in hydrology. However, empirical methods have been, and still are, largely developed

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and applied, especially for the estimation of sediment yield and bed loads. These methods have been always calibrated on the basis of laboratory experiments and/or field data and then used in different, out-of-sample, situations. Of course it is advisable that the similarity of calibration and application contexts is carefully evaluated, as we believe it was done in our study.

We agree with the referee that such empirical techniques are not aimed to provide a further insight into the physics of the process. The reason for their use is to address the practical need for the evaluation of the sediment yield in real world applications. The assessment of the erosion susceptibility is an important issue in engineering practice. With our study we are trying to provide a suitable-for-purpose working approach that may allow one to gain a technically and scientifically relevant insight into the order of magnitude of the sediment yield, in order to be able to design river engineering works for the prevention of the flood risk. The empirical nature of the approach we propose to use is not different with respect to that of the USLE equation, or that of many empirical relationships used for the evaluation of bed load transport. These approaches are widely applied in practice, sometimes in very variegated contexts, for the same reasons that motivated our study. The referee rightly points out that the availability of data does not necessarily make a method good. However, the availability of data, together with practicality, makes a method an interesting opportunity for real world applications. Our study is of course application-wise rather than knowledge-wise, but nevertheless we believe it has a relevant scientific interest, especially because we successfully carried out a validation of the results.

The empirical relationships used in our paper were calibrated on a sample of 20 watersheds, which we believe it is wide, obtaining scientifically sound coefficient of determination, ranging from 0.95 to 0.96. These coefficient of determination testifies the performances of the model in calibration. Calibration was performed with respect to basins located in central and southern Italy and one of them is very close to the Calvano watershed, therefore in very similar climatic and geological conditions. In our

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opinion the extrapolation of the results is fully justified from a physical point of view. Therefore we believe we did not apply the derived relationships blindly to the Calvano watershed. Moreover, we referred to a situation where data are available for validating the model. This is a relevant and scientifically interesting opportunity, even if the reservoir siltation data are affected by some uncertainty, as it often happens when dealing with this type of information.

Overall, we are convinced that an empirical approach that has been calibrated successfully and is subsequently validated with observed data of reservoir siltation deserves to be known by the scientific community as it might be useful for engineering design. Once again, we do not claim we are contributing to the advancement of process knowledge. However, we feel that the scientific value conveyed by the paper should not be underestimated by just claiming that regression relationships are not physically based and cannot be extrapolated to different contexts without risk. In this way one would question the philosophy of an approach that provided relevant scientific achievements in hydrology.

Despite of being application-wise, our approach effectively takes into account the physical behaviours of the basin. We discussed this issues when considering the influence of basin exposure and badland areas. The Referees states that our approach does not take these behaviours into account. This is not true, as badland areas (calanchi) originate a particular morphology of the river network that affects the drainage density and the hierarchical anomaly index. The same consideration applies to exposition. In the description of the case study we detailed how the basin morphology is influenced by orientation and therefore exposure. Probably we did not efficiently explain the physical basis of the explanatory variables that we used in the regression equations, that in our opinion provide an effective synthesis of the basin behaviours that are most effective on the erosion susceptibility. We are convinced that the good results presented in the paper were not obtained by chance, though we agree that these issues could be discussed in the paper with more details.

In fact, we fully agree with the Referee that the structure of the manuscript can be improved and the analysis better explained. On the basis of his comments, we admit that we were not effective in clarifying the main objective of the study and some details of the analysis. We agree with almost all of the useful observations provided by the Referee in the second part of his review. We are looking forward to the possibility to discuss each of them in detail, if we will be asked to prepare a revised version of our manuscript.

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