Hydrol. Earth Syst. Sci. Discuss., 4, S213–S219, 2007
www.hydrol-earth-syst-sci-discuss.net/4/S213/2007/

Hydrology and Earth System Sciences Discussions

HESSD

4, S213–S219, 2007

Interactive Comment

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 #3

Received and published: 2 May 2007

General comments

The aim of this paper to predict river sediment yields at various points in the drainage network of a study basin by means of regressions between sediment yield and two indices of basin characteristics, the drainage density and the hierarchical anomaly index. This is described as being important for predicting sediment accumulation along the stream and related risks of flooding — although the link between sediment yield and sediment accumulation is never established in the paper. The novel aspect of this paper seems to be the application of the regression models at various sub-divisions of the study basin — although it is not explained why this is important. There is obviously a need to evaluate models at various spatial scales, but the evaluation aspect of this paper is very weak due to limited and uncertain data. Importantly, the regression equations applied (including their parameter values) have been derived in previous studies for different basins and times. No evidence is presented that it is valid to transfer these equations to a different basin (and its sub-catchments of different scales) and a different time.

Following on from the weaknesses in the methodology, the importance of the modelling results in explaining the behaviour of the study basin is over-stated. Model results merely coincide with perceptions of processes in characteristic areas of the basin (high erosion rates in badlands and on south-facing slopes, sediment trapping in reservoirs), but there are no mechanisms in the models that could account for these processes 'for the right reasons'. Hence nothing is learned from this model application. I would therefore reject this manuscript for publication in HESS.

The manuscript further contains a large number of language mistakes which make it hard to follow. In the next section, I am not going to list any of these errors but will detail the specific weaknesses of the manuscript that I have introduced above.

Specific comments

Integration with previous studies & novelty of contribution

The authors cite a large number of studies which have applied regressions between river sediment yield and geomorphological, hydrological and climatic basin variables. This list, however, seems to be missing recent as well as international publications. Maybe there are none but then more of a story should be made of the fact that this paper is a revival of an old method and why it is important to re-consider it. A number of regression variables are further discussed without a clear link to the method applied in this paper or how the applied method advances from previous studies cited [p. 629, l. 27 – p. 630, l. 14].

It is described how the regressions used in this study have been derived in previous studies. Explanation should be restricted to the variables actually used, drainage density and hierarchical anomaly index. Drainage density needs brief explanation. Both variables need literature references [p. 633, l. 3 – 17].

Feasibility of method

The argumentation for the choice of the two variables drainage density and hierarchical anomaly index is not clear. First a study is cited that argues that drainage density was the most significant parameter when estimating sediment yield by multiple regression. Then, it is suggested that the hierarchical anomaly index is important as well. Finally, studies are cited that have revealed the insignificance of correlations between drainage density and sediment yield. These arguments should be re-arranged into a concise order [p. 633, l. 18 – 27].

The main argument for the use of geomorphological variables (and particularly drainage density) to predict sediment yield by regression seems to be that these variables are readily available from maps at the study scale — although the availability of data does not necessarily make it a good method. The feasibility of the method for the study site is not discussed, neither is the necessity to evaluate the method against observations. Literature references to back up the method are sparse [p. 624, l. 1 – 14].

When the four regression equations used in this study are introduced it is misleading to quote coefficients of determination (r^2) from the studies (it is not clear which) that have derived the regressions in the first place. These r^2 values are meaningless for the study site and, since no statistical evaluation of the method against local observations is carried out, are suggesting a confidence in the method which is not justified [p. 634, l. 19 – p. 635, l. 2].

In general, it is highly questionable that regressions derived for specific sites and times could be applied to other sites and times. By their very statistical nature, regression equations are descriptions of specific time series of specific systems and no theoretical

HESSD

4, S213–S219, 2007

Interactive Comment

justification is given for transferring these to other times and other systems. However, it could be argued that as the length of a time series approaches infinity, confidence in the predictive capability of a regression derived from this time series may increases (for the system under study). It could further be argued that this regression may hold for similar systems. These assumptions certainly require careful evaluation, a requirement that is not satisfied in this study.

Data availability & model evaluation

The lack of input data for many model types, which prevents their use, is appreciated, but not the limits of available data to support the model assumptions in the first place, i.e. the evaluation of models against field observations [p. 629, l. 7 – 26].

It is stated that the validation of erosion models is not easy, but literature references are missing [p. 637, l. 5 – 6].

The observations used to test the models in this study (four estimates of reservoir siltation throughout the basin) are described as being soft and uncertain, but no consequences of these findings are drawn. Data uncertainties should be included in model evaluation. It is therefore not justified to speak of a “meaningful and spatially distributed evaluation” in this study [p. 637, l. 9 – 13].

It is not clear which of the sub-catchments in Tables 2 and 3 drain into the four test reservoirs. Especially as there is a mismatch between the drainage areas given for the test reservoirs and the sub-catchment areas presented in the tables [p. 637, l. 14 – p. 638, l. 5].

Noting the reservoir siltation rates with 2 decimal places does not reflect the soft and uncertain nature of these observations [p. 637, l. 14 – p. 638, l. 5].

The four observations of sediment siltation (which softness and uncertainty is not accounted for) do neither allow “to test the model equations reliably” [p. 640, l. 3 – 4] nor “a validation of the proposed method” [p. 641, l. 2 – 3]. In contrast to the confidence expressed in the manuscript, the reported four differences between observations and model simulations (two of them being -50% and +59%, Table 5) give no feeling how good these models perform on the study site.

Interpretation of results & model inconsistencies

The sediment yield of the study basin as calculated using the model equations is reported to be close to average observed yields in river basins of central Italy — although figures of average yields and corresponding references are missing. In the next sentence it is argued that the basin sediment yield may actually be underestimated. It seems impossible given the ambiguity of this information to judge the performance of the model [p. 638, l. 11 – 15].

The link high sediment yield-high sediment deposition and therefore high risk of flooding is never established in the manuscript — although it is presented as a critical aspect of the application of the models [p. 629, l. 2 – 3, p. 630, l. 17 – 19, p. 638, l. 20 – 25, p. 641, l. 13 – 16].

The sediment yield of the study basin as calculated using the model equations is reported to be lower than the sum of the sediment yields from the sub-catchments of the basin as calculated using the same or different model equations. This is attributed to re-deposition of sediment that has been mobilised in the sub-catchments before it reaches the basin outlet. It seems, instead, more likely that the modelling approach used is inconsistent across the scales it is applied to. Especially as the models have not been evaluated at the scales of application it seems misleading to draw any mechanistic conclusions from the results [p. 639, l. 1 – 10].

In much the same way, lower calculated sediment yields for various sub-catchments draining into reservoirs are explained with the sediment trap function of the reservoirs. This can only be a coincidence because the mechanism of sediment being trapped in reservoirs is not accounted for in the model equations. In fact, the result itself is not consistent across all sub-catchments draining into reservoirs as some of them have

HESSD

4, S213–S219, 2007

Interactive Comment

very high sediment yields (Table 3) [p. 639, l. 11 – 22].

The same applies for higher sediment yields reported for the badland areas of the basin — a mere coincidence as the erosion susceptibility of the substrate is not a variable in the model [p. 639, l. 23 – 29].

The same applies for higher sediment yields reported for south-facing areas of the basin compared to north-facing ones — the applied models do not have the ability to be sensitive to aspect. Again, the result itself is not consistent across all south/north-facing areas (Table 6) [p. 640, l. 18 – 24].

To demonstrate the inconsistencies of the applied models further, it is necessary to examine Tables 1 – 4 in more detail. In Table 1, the higher sediment yield of the subtotal of the “partial catchments” compared to the Calvano whole basin is explained by re-deposition in the stream. Should this explanation be true, the same effect would consistently be observed for every down-scaling step from high-order sub-catchments to low-order sub-catchments, i.e. the subtotals in Table 2 should be consistently higher than the totals of the respective “partial catchments” in Table 1. This is not the case. In fact, only the Cascianella subtotal in Table 2 is higher than the respective total in Table 1, all other subtotals are lower. This puts into question the reliability of the figures given in Tables 3 and 4. In Table 4 for example, when 42% of the basin area account for 96% of the basin sediment yield, what happens with the contribution of the remaining 58% of the basin? It must be more than 4% of the basin sediment yield, certainly when looking at the results in Tables 1 – 3. This yield cannot possibly all be deposited along the way, while the yield from the badland area is completely transferred to the basin outlet. It would be indicative to compare the yield of the badland area with the yield of the remaining area and see whether the sum matches up with the total basin yield. I would assume it does not, given the analysis above. The model inconsistencies across scales can be expressed by the following simple formula: the model results when applied to the whole basin do not equal the sum of the model results when applied to all the parts of the basin (the same applies for each down-scaling step). It is therefore misleading to compare model applications from different scales and draw any conclusions from this comparison.

As a last comment, although the models may “confirm” higher erosion susceptibility of badlands and south-facing slopes by coincidence, no mechanistic explanation is offered by the models. This means we learn nothing for modelling these processes from applying the models used in this study [p. 641, l. 8 – 12].

Miscellaneous

The description of the study area is long and could be restricted to the characteristics relevant for this study [p. 630, l. 21 – p. 632, l. 28].

Reference could be made of the origin of the DEM used and the GIS algorithms applied [p. 635, l. 14 – p. 636, l. 7].

Confusion over “sub-catchments” and “partial catchments” is generated. I think all sub-divisions of the basin are sub-catchments, only the remaining part of the basin is a “partial” catchment [p. 636, l. 9 – 26, Table 1].

Sediment yields should be noted in tonnes rather than megagrams [Section 4, Tables 1 – 6].

What is meant with “consistency with lithology” and how does this relate to the paragraph [p. 638, l. 18]?

Where does the figure 12000 Mg km^{-2} come from and how does it relate to the paragraph [p. 38, l. 19]?

What is meant with “considering bedrock type and according with the need to prevent the rapid reservoir filling” and how does it relate to the paragraph [p. 639, l. 17 – 18]?

A reference is needed for the assumed dry bulk density of 1.2 Mg m^{-3} [p. 640, l. 6].

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 627, 2007.

HESSD

4, S213–S219, 2007

Interactive Comment