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Hydrology and Earth System Sciences Discussions

Interactive comment on "Experimental validation of some basic assumptions used in physically based soil erosion models" *by* S. Wirtz et al.

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1. Introduction

Physically-based distributed modelling has become the norm in watershed analysis, witnessed by a wave of papers in hydrological journals. It seems that the motivation and interest in physically-based distributed modelling primarily comes from the "technology" side, i.e. the availability of robust numerical methods and the skill of the model developers in developing user-friendly interfaces, and from the "data" side, i.e. the explosion of new spatial data sources from remote sensing and their accessibility. Many

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very good developments have been made in these directions in the recent past. However, what has not changed much is the process-understanding and methods which are implemented in the models. We could then raise a question: is this because (a) our hydrological process understanding is already complete and satisfactory, and/or (b) models use practically the same methods because researchers are not interested in the laborious task of checking method validity for each study? Or in fact have insufficient data to do so, even if they wanted to.

In the case of physically-based distributed erosion modelling addressed by this paper, the situation is even more critical and complex. There are many fewer models that simulate sediment transport and erosion/deposition. It is inherently more difficult to simulate the detachment and subsequent transport of sediment carried by water. The processes involved are certainly not deterministic, and the predictive capabilities of detachment models such as USLE and transport models based on shear stress, stream power, etc., carry huge uncertainties. If sediment transport formulas are off by a factor of 2, we would usually call that a pretty good fit. The questions raised in the previous paragraph also apply to erosion modelling, with additional dimensions: (a) in terms of process understanding are both sediment supply and transport capacity limitations included in the model; and (b) are model predictions verifiable by observations?

The paper is placed in this context and addresses some of the questions raised above. In that sense it is a valuable contribution to the discussion on physically-based erosion models. Although the paper is very detailed in some aspects, other, perhaps more important issues, are skimmed over. I would like to raise these issues in the hope that the authors can add some insight from their viewpoint and experience from their field site, hopefully improving their manuscript in the process.

2. Supply or transport limitations?

It seems from the abstract on, that the authors' position is that the "process [of erosion] described by the models is only responsible for a part of the eroded material", thereby they cannot accurately predict sediment yield. I would take a different position, saying that the processes of erosion and sediment transport are largely stochastic, and even if we would have a perfect process-based model our predictions of sediment yield will be hugely uncertain (even biased) due to this stochasticity. The problem here in my view is not the fact that the model (or model assumptions) are imperfect, but rather that the erosion process is unpredictable in a deterministic sense.

In their paper, the authors go straight to the discussion of transport and detachment capacity of flow being deterministically related to bed shear stress or stream power. In my opinion, the more important question of the basis of physically-based erosion models is whether the model formulation intends to simulate supply or transport capacity limitations or both. For example many of the bed shear stress-above-threshold formulas mentioned in the paper were developed for transport capacity and not supply limited conditions. What is the author's opinion on this?

The authors make the statement (p.1250) that the basis of using shear stress in erosion models is that "shear stress must exceed the critical shear stress to cause erosion". Technically this is not correct. Rather the thinking is that if shear stress exceeds a critical value, the hydrodynamic forces acting on a particle exceed the resisting forces and the particle may be in motion. The state of incipient motion is not necessarily related to erosion of the bed. If we have a high shear stress but a lot of sediment supplied from upstream we will not have erosion locally, we in fact may even have deposition. Again this will depend on the role of sediment supply.

3. Which form of the bed shear formulation is best?

The authors spend a substantial part of the paper on a review of the history of shear stress, critical shear stress and transport capacity. The listing of different approaches and assumptions leading to bed shear stress estimates is a bit confusing, applied and critical shear stress estimates are mixed, there is no obvious system to the listing provided (that I could see), e.g. equations (17) and (20) are the same, or not? Also the actual experiment the authors conducted and their discussion of the results has very little to do with the lengthy description of the shear stress formulas. What was the aim of the authors with this section?

Most importantly, it is not clearly distinguished between hillslope (overland flow) and channelized flow. Some of the equations presented (e.g. Parker, 1979) were developed for gravel bed rivers for a specific range of grain sizes and flow depths, etc. This formulation cannot be compared to shear stress estimates for overland flow which have completely different ranges, yet it is overland flow that presumably is key in upland erosion models predicting surface erosion loss from rill and sheet erosion. I think it would be good if the authors think how they can synthesize the different approaches in a more meaningful way for the reader.

4. Field experiments

Field experiments are crucial sources of data for erosion modelling. This part of the paper is interesting because it highlights how and why experiments were and are being conducted. In the light of being partly a review paper, it is perhaps appropriate to mention here the outstanding USDA experimental effort behind the USLE concept. The two sections of the paper starting on pages 1255 and 1256 mix the role of experiments and the questions that are addressed in the paper (or raised in a general sense). My suggestion is perhaps to join these sections or in some other way make clear the line of thinking between what experiments teach us and which questions they raise with respect to the modelling. Finally the questions to be addressed in this paper are presented on page 1258. I find these questions well posed and very relevant.

I do have some doubts about the statement that in their experiment the authors have "constant shear stress values" and that "all needed values to calculate easily different erosion parameters are available". Insofar as I could understand, the authors conducted flow experiments in 4 rills about 10 meters long, with a constant flow for 8 minutes, with 3 measurement sites along each rill. The experimental values are not completely clear to me. For example Table 3 gives average values of how many measurements? Of the three measurements in each run? Do the data in Table 3 allow to make the statement that shear stress in constant? Table 4 has the mean values for the variables for both runs and the four rills. What do you get by mixing the two runs? Are there significant differences between the runs? For example, one of the most interesting measured variable is sediment concentration, which is quite different for the "dry" and the "wet" run in all rills. What is then the point of taking an average value and reporting a RME?

Notably, measured sediment concentration is always lower in the second run. I expect this is because sediment easily available for transport in the rill was removed in the first run and not available for the second run which is then only reliant on detachment and erosion of the rill only. Is this a possible explanation? I find some other interesting differences between these two runs in a consistently greater flow velocity in the second run after the microtopography of the rill surface has been smoothed in the first run and discharge is greater due to lower infiltration.

The discussion of the data (starting on page 1264) raises some interesting questions. I agree with the statement that on the average the transport rate should not be larger than the transport capacity, but in 75% of the cases this premise was violated - so there is a problem. The authors argue that the reason is that there "is no linear relation between shear stress and soil detachment". Do the authors mean that the detachment capacity does not follow Eq. (26)? Could it be that the transport capacity equation in Eq. (27) is in fact underestimating the true value (that one is not linear)? The data in Table 3 seem to suggest that the sediment concentration converted to a detachment rate is actually never at capacity (although one could question the validity of the capacity computed by Eq. (26)). The explanation in the discussion part of the paper is not clear to me, and quoting the myriad of previous experimental findings just clouds the arguments. Try to explain your own data first. Perhaps looking at the two runs separately in Figure 4 would help.

I do not completely agree with the arguments of the authors that another reason "why the use of the shear stress equation does not deliver satisfactory results is the origin of the equation from the Navier-Stokes equation" (p. 1268). There is no ambiguity in Navier-Stokes. Shear stress is a precisely defined variable. The problem lies in my opinion solely in its approximation of the kind in Eq. (27)

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