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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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Thank you for all the very important and interesting points. In the following section we try to answer all questions and to explain all uncertainties. We think, the quality of the paper can clearly be improved by including the appeared questions in the corresponding section of the paper.

Q: 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.

A: That is exactly the problem. It is not possible to describe soil erosion processes in deterministic equations, at least with the recent used equations. Maybe one day a new parameter or a new equation is found which solves all problems. But to find this parameter, basic research is needed; there is a lack of comparable data. And these data can be ascertained by experiments, in laboratory or in field. The first idea to describe erosion processes stochastically was published by Einstein in 1937. In the time until now, different authors hint at the stochastic nature of the erosion processes but the first group that really used this approach was the group of Sidorchuk, the first publication was in 2002. But this model is not yet operable, so the recently used models are still deterministic. We completely agree with the opinion of the reviewer.

Q: 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?

A: An exceeding of the transport capacity only shows that there is a problem with the used assumptions and equations. In a supply limited environment, an exceeding can not occur because there is not enough material, in a transport limited environment an exceeding is possible if other than fluvial (e.g. gravitative) processes provide enough material. With our methods, we are not able to show completely supply limited conditions. If there is a mixture of transport and supply limited conditions and the transport capacity is exceeded than is the supply limited condition covered by the transport limited condition. It is possible that there are supply limited conditions if the transport rate is lower than the transport capacity, because it is possible that there is not enough material available for the given processes.

Q: 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.

A: The exceeding of critical shear stress is the first condition for erosion. That means, an exceeding of critical shear stress must not directly cause erosion, if transport capacity is still reached there will be no erosion, but if the critical value is not exceeded, nothing will happen.

Q: 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?

A: The first aim was to show the confusion of the shear stress history, this aim seems to



be reached. First we show the critical shear stress equations, the Shield equations are shown before because Graf modified them to reach his critical shear stress equations. After the critical shear stress equations, the Parker equation shows the opponent characteristics between shear stress and critical shear stress, the next point are the shear stress equations and finally the transport capacity equations. Equation 17 and 20 are nearly the same (in the one equation the slope is described by the tangent, in the other by the sine of the slope angle), but here it is clearly to see, that the designation of a certain parameter is not always consistent and this fact can cause problems in comparison of different studies. The second aim was to show that the physical definition of shear stress seems not to be clear. If one parameter is calculated in about 5 different equations with different input parameters, the question is what the correct equation is. In our own calculations, we only use the recent form of the equations. We will remove the shear stress history from the introduction and insert an additional chapter where we try to explain our ideas in a more meaningful way.

Q: 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.

A: This is one of our points of criticism. Govers (1992a) and Govers et al. (2007) showed that equations made for rivers can not easily be used for calculations about soil erosion. But in some cases, this is the used method. The Yalin - equation is an other typical equation for rivers which is often used for overland flow processes. In our experiments, we test rill erosion, so we are a little bit closer to the rivers but it is clear, that river equations should not be directly used for rill erosion calculations. The introduction will be restructured, also following the comments of reviewer 1.

Q: 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.

A: This paper wasn't plant as review paper. The shear stress history is needed to show the confusion in use and development and the uncertainty about the physical definition of the shear stress equation. We will join the two sections and improve the link between experiment and model. The experimental work behind the USLE concept will be inserted in this section. We did not consider the experiments of the USDA concerning the USLE because the experiments were never arranged to be used in physically based models. The WEPP model used these data so we will insert a short part about the experimental work.

Q: 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?

A: The average values in table 3 are calculated from 12 samples (3 measuring points à 4 samples in each run). Table 2 shows the constant external parameters. In a model simulation, in most cases average values are used. And because the external parameters are constant (table 2), the 4 different rills would be described by one average value of all rills. This is the reason why we calculated average values of all samples (4 experiments x 2 runs x 3 measuring points x 4 samples = 96 samples). The RME describes the difference between the average value of all experiments and the average values of one run. The given RME value is the average of the RME of the 8 runs. The RME would be much higher if we would calculate the RME between average values of all 96 samples and the single, directly measured sample. The question for us was: What would do a model simulation with our data? The sediment concentration shows clear differences in the runs, at the different measuring points and even at the different sampling times. Here, the different processes can be detected (transport of loose material by the water front, very different in the first (dry) and the second (wet) run, bank failure, low incision in the ground). But a model simulation can not describe all these different parameters. We will use boxplots in figure 3 to show the range of the RMEs of the different parameters.

Q: 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.

A: Yes, this is the reason for the higher sediment concentration in the first run. The loose material is removed mainly by the water front; this is clearly to see by regarding the SSC values of the single samples. We did not show these values because the discussion of the single samples would break the mould of the paper, in this paper we just want to show the problems between model assumptions or approaches and the experimental results. The different processes and their intensities are discussed in Wirtz et al. (2011): Field experiments for understanding and quantification of rill erosion processes; Catena, Doi: 10.1016/j.catena.2010.12.002.

Q: 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)).

A: The used equations 26 and 27 do not cover all processes which cause the measured sediment. The detachment capacity covers the effect of the flowing water, the incision in the bottom, but not the more gravitative processes like bank failure and headcut retreat. The detachment capacity per definition follows the given equation, but this approach does not consider the range of different processes. This range of processes does not show a linear relationship to the shear stress, the single incision maybe shows this linear relationship. That means the transport capacity vs. transport rate approach underestimates the true range of processes and as a consequence, the true value of material which can be transported. There is not only no linear relationship but there is no clear relationship between shear stress and soil detachment. In the discussion



we will add a section about the different groups which found linear correlations; our data show that this relationship is not given. The detachment rate does never reach the detachment capacity, that's true, but the transport rate exceeds in most cases the transport capacity. The relationship between detachment capacity and detachment rate can be correct but in the relationship between transport capacity and transport rate is something wrong. That means the relationship between transport parameters and detachment parameters is not correct. The detachment includes the affected area, the transport rate and capacity only use quantity per time. The problem is that bottom area and the area of the two flanks of the rill are summarized to the total "rill area". But bottom and flanks are affected in different ways and in different intensities, so the transport rate and capacity describe the processes "better" than the detachment values. The validity of the used equation is to be questioned, we totally agree with the referee. But this is the equation used in the current models and our question was "What would a model do with our data?".

Q: 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.

A: We separated in figure 4 the two runs but there is no difference to detect. The different process range covers in both runs the validity (if given) of the used equations. In the dry runs, in 77% of the samples, the rate exceeds the capacity, in the wet runs, in 63 % of the samples. But regarding the average values of the relationships between rate and capacity (only the values higher than 1) the b-runs show a higher value than the a-runs (7 vs. 5.7). In the a-runs, the transport of loose material is the main process, in the b-runs the bank failure and the headcut retreat. This part will be inserted in the discussion. The experiments always show different values but most studies look for a linear correlation and this approach does not bring any advance.

Q: 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)

A: Navier Stokes defines the shear stress clearly but it is not clear i fit is correct. We don't know if the Navier stokes equation is a reason for the problems in the models. There is no proof but also no disproof of the validity of the Navier Stokes equation in a 3-D situation. Most used derivations of the Navier Stokes equation are to much simplified, too many parameters are ignored or estimated. The approximation is surely a problem, the origin can be a problem; it is not proofed until now. The importance of the proof or disproof can be seen by the 1.000.000 $ price.

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