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Soil-erosion models: where do we really stand?
Short Communication (Discussion) on the papers by Wainwright *et al*. (2008a, b, c)

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Earth Surface Processes and Landforms

Wainwright *et al*. (2008a, b, c) propose a new model for simulating catchment-scale erosion resulting from rainfall-runoff events. In justification for such a new model, several existing process-based models are criticized with regard to their theoretical underpinnings and presumptions. Since the authors do not correctly characterize the models mentioned, it is felt useful to correct the record in that regard, and to put the proposed model of Wainwright *et al.* in better perspective. The current models named in the criticism by Wainwright *et al.* include KINEROS (Woolhiser *et al*., 1990), KINEROS2 (Smith *et al*., 1995), EUROSEM (Morgan *et al*., 1998; Smith *et al*., 1995), and WEPP (Ascough *et al*., 1997). We refer to these below as the 'critiqued models.' The writers' familiarity with those models and their assumptions provide justification for the clarifications presented below. We note here that some of the models critiqued have had developments added, and published, since their initial release.

Wainwright *et al*.'s complaints and misunderstandings regarding existing models appear in our reading to be summarised by the following statements:
1. Existing models assume all sediment travels in suspension, whereas it likely does not.
2. The description of sediment movement relies on an assumption based on steady-state flow.
3. The models assume that sediment velocity is the same as water velocity.
4. The Yalin (1972) transport equation is criticized as inapplicable or misapplied [with the clear implication that existing models rely on it].
5. The models should not use settling velocity or are using it incorrectly (p. 817).

Below we address each of these points in detail. In each case the criticisms are ill-founded or result from a misunderstanding of the conceptual robustness of current catchment erosion dynamic models. Being familiar with the 'critiqued' models, we point out what we feel to be the true weaknesses of erosion models, for which the MAHLERAN model, developed by Wainwright *et al.,* offers no cure. Their model suffers from many of the same weaknesses as those critiqued, plus some unique ones of its own.

KEYWORDS: erosion; sediment; modelling

Must Sediment be Suspended?

One of the key tenets of Wainwright *et al.*'s argument is that transport in suspension rarely occurs in runoff on hillsides. Some empirical evidence is reported to support this statement, but substantial empirical evidence which would refute it is ignored. Workers since the early 1950s have reported clay enrichment in their sediment samples from hillsides (see for example Sholtenberg and White, 1953). Enrichment results from either preferential erosion of fine particles or preferential deposition of coarse material. If all the fine particles were transported as aggregates as Wainwright *et al.* imply (p. 815), then it might be expected that less particle enrichment would occur. However, the true position is likely to be, as Miller *et al*. (2009) point out: that particle enrichment will vary between soils and this is likely to be due to the way that aggregates, from the different soils, fragment when struck by raindrops and the fate of these fragments as they are transported in overland flow. Taking the position that either aggregate transport or suspended transport is the dominant mode of transport for all soils is patently wrong. The fate and behaviour of soil aggregates, micro aggregates and primary particles as they break down and are transported in runoff remains one of the great challenges facing soil scientists and geomorphologists.

The authors make much of the equation of Bennett (1974). It should be clarified that this is not so much a 'model' proposed by Bennett as it is a simple differential statement of sediment mass continuity, found in his 1974 manuscript. Bennett was showing how water flow and sediment flow are inextricably connected. It is important to note that it holds for movement of particles in the water, whether they are concentrated near the soil, near the water surface, or whether they are moving slower than the water (see below). Thus uniform suspension of sediment is not inherent in the Bennett dynamic conservation of mass equation. Indeed the authors' Equation (6a, b) is fundamentally the same as that of Bemett [the authors' Equation (3a)], as is readily seen by inspection, in this regard, it is important to understand that Equation (3a) is not dependent on Equation (2), as implied by the authors' introduction to this equation. It has no dependence on the assumptions of Foster and Meyer (1972). Any differences lie in how one formulates the supply terms: *e* in Equation (3a), and ε and d in Equation (6a). It should also be noted that there is sign error in the authors' Equation (6b). If d is positive for the rate of deposition Equation (6a), then the sign is wrong in Equation (6b), which as net erosion as $w(\varepsilon + d)$. It must be $w(\varepsilon - d)$ to be consistent with (6a).

Are the *Critiqued Models* Based on Steady-Flow Assumptions?

The authors' Equation (4) is from a 1972 paper by Foster and Meyer, and is introduced by quoting those writers' statement that it is theoretically derivable from steady flow assumptions. First, as Wainwright *et al.* should have been able to determine easily, this relation of Foster and Meyer has no use in either the EUROSEM, LISEM (De Roo *et al.*, 1996a; 1996b), or the KINEROS2 models. Thus the authors have no basis for criticism of these models on this point. These models are strictly dynamic, treating unsteady flow, and the only assumption is that at any location and time both processes of entrainment and deposition are occurring, and the balance between them determines whether there is a net loss or net gain of particles at the soil flow surface (Smith *et al.*, 1995).

It is true, as stated by the authors, that the WEPP model is a steady-state model. The sediment mass continuity equation solved in WEPP for soil detachment, transport and deposition uses an effective runoff rate for an effective duration of runoff that was based on extensive calibrations on a very large set of natural runoff plot data (Nearing *et al.*, 1989). While this may be a limitation for describing the intra-storm dynamics of sediment movement, it has never been shown that on a storm total basis that the use of a dynamic sediment routing solution results in an overall improvement in soil loss predictions for a series of storms.

Can Sediment Move Slower than Water?

The general rebuttal to this assertion is simple: certainly sediment may move slower than the water. Referring to the Bennett conservation equation, this case simply requires a more robust definition of flow concentration than usual, perhaps: call it effective flow concentration, C_e, simply defined as

$$C_e = \frac{q_s}{q}$$

where q_s is sediment unit discharge and q is water unit discharge. When particles move with water velocity, then $C_e = C$, and when particles cease to move entirely, C_e is simply 0. In fact, C_e is the sediment concentration of interest in catchment erosion modeling.

In addition, Wainwright *et al.* criticise current approaches to erosion modeling for not being able to cope with the feedback between sediment concentration and flow characteristics. While it is clear that very high sediment concentrations will increase the viscosity of the overland flow and that this will affect the flow conditions, it is unlikely that any of the developers of the critiqued erosion models ever thought that their models would be used for hyperconcentrated or debris flows (however, an attempt to apply LISEM to hyperconcentrated flow is presented by Hessel, 2006). This is obviously a point at which these approaches fail, but the approach presented in this paper will also fail under these conditions since travel distances in a debris flow will not be governed by settling distances. Thus this argument is specious at best.

Is the Yalin (1972) Equation Wrong?

As far as the writers know there is no transport capacity equation that is developed and proven specifically for very shallow flows on hillslopes, although many have been used in hillslope and catchment models. It is important to note that in many, probably most catchment runoff cases, the particular transport capacity equation is often not a critical component, as detachment from rain splash energy may dominate the erosion rates (Wei *et al.*, 2009; Kinnell, 2005). Nevertheless, it is also important to note that neither the EUROSEM, KINEROS2, or LISEM model relies on the Yalin relationship. Therefore it is curious that the authors single it out for criticism.

It is interesting to note the authors' critique of this relationship. First, they criticise a claim which is made by Ferro (1998) (not Yalin) which is independent of the validity of the Yalin equation as a description of transport capacity. Then, they point out that it is particle-size specific. But some existing models (e.g. KINEROS2, Goodrich *et al.*, 2002, 2006) treat erosion for an ensemble of particle sizes in any case, so this is not a fault in its applicability. Thirdly, the Yalin equation contains a threshold for transport capacity, as they point out, but so does the model that they propose. Thus this criticism seems pointless on many grounds.

Is Settling Velocity Useful?

It has been shown (see Smith *et al.*, 1995) that the settling velocity is theoretically proportional to k in the authors' Equation (5) for describing deposition rate of a particle of given diameter and density. This is true for cases where T_c is zero, for example. The authors nevertheless claim that k has a 'lack of clear physical meaning'. Smith *et al.*, (1995) have defined an apparent transport capacity as the concentration that would occur in a flow, *if* steady, resulting from a balance between deposition rate and entrainment rate (exclusive of rainfall dislodgement). This also leads to an expression similar to Equation (5). Then the authors claim, regarding EUROSEM, at least, that use of fall velocity is modified in that model 'because the settling velocity approach leads to significant underestimations of local deposition'. This is a significant misunderstanding on the quoters' part. Quite to the contrary, k has a very clear physical meaning, and the reason that k must often be a modification from settling velocity, in general application of Equation (5), is the fact that the flow-induced *detachment* rate, rather than settling rate, cannot, for cohesive soils, be simulated with this parameter alone. Soil surfaces commonly do not act as a perfectly loose bed of sediment, and thus relations such as Equation (5), though based on sound process assumptions, do not describe a universally symmetrical reversible process. Clearly the authors have failed to understand this important aspect of the models they are criticising.

The MAHLERAN Model

After the attempt to discredit existing process-based hillslope erosion/sediment transport models, the authors present their model. It is instructive to see if this assemblage of concepts improves on the critiqued models. Unfortunately, much of the workings of this model are obscured in the description. The authors specify four modes of sediment movement (Figure 4), including transport by splash, by 'unconcentrated' flow, by 'concentrated' flow, and 'in suspension'. Definitions are important in the field of erosion modeling, since for example 'concentrated flow' and 'rill flow' are often used to describe the same thing, and other terms may be used with different meanings. Apparently the second and third categories of MAHLERAN transport are moving either as bed load or in some other manner than suspension. Although it is difficult to be sure, apparently the 'transport distances' defined for the various transport categories are used in connection with the 'virtual velocities' to determine a 'virtual' deposition rate, which is used in tandem with the various detachment rates for the four categories of flow transport, to be used in Equation (6). Herein lie some of the difficulties in this model. In 'unconcentrated flow', the virtual velocity is dependent on rainfall energy, despite the existence of another flow category for splash, so that unconcentrated virtual velocities cease at the cessation of rainfall regardless of the value of flow stream power, and even if the particles were buoyant! In addition, using a few reasonable values in Equation (17), it appears that the virtual velocity of D_{50} particles is only about 5% of the water velocity, but just as soon as the shear velocity reaches its threshold u^{**} those particles' velocities will jump by a factor of 20. The slow movement so described, is applied to all particle sizes, however small.

Another conceptual weakness lies with the lack of any relationship for transport capacity. What this means is that for this model, as soon as water carrying sediment leaves an area having a source of sediment, into a lined channel for example, simulated deposition must occur, regardless of the slope or stream power of the water in that conveyance.

The splash detachment model, Equation (8), has dislodgement a function of particle size through three parameters for each size. Can one conceptually defend the idea that for a given surface particle size distribution the rainfall energy would cause selective *dislodgement* of the soil particles at different rates? [This question is not to be confused with the probably different rates of dislodged particle *settling*.] These conceptual shortcomings are most troublesome in a model which has been justified by dismissing existing models as 'a suite of models that claim to be process based, but which, in fact have little demonstrated basis in reality.'

Finally we note that there are approximately 20 or more parameters and empirical constants in the model. With this number, from a systems analysis perspective, almost any result can in principle be fitted, but what if one moves to a different climate or soil type? For example, how is one to determine the three parameters for Equation (8) for each soil particle size class for a new soil?

What are the Real Problems with Current Models?

All computational models of the rainfall erosion processes on hillslopes require conceptual abstractions in order to render the complexity of nature into a form that yields estimation (erosion, hydrology, and hydraulics). Wainwright *et al.* often state the important dependence of the erosion model on the underlying hydrology and hydraulic model. In contrasting a steady state versus a dynamic approach, there is a strong argument that the use of the dynamic solution itself may be a source of potential model prediction error associated with the intra-storm temporal variation in soil erodibility. Soil erodibility changes as a function of many factors, including soil moisture, aggregate stability, and surface sealing that vary greatly during a storm event. We do not possess the scientific understanding of how the erodibility changes during a storm for different antecedent condition, soil types, and cover. Until these factors are assessed, and until the relative contributions to prediction capability for both the dynamic and steady-state methods are assessed in a robust manner (which has never been done), then the discussion of prediction capability associated with the dynamic or steady state solution of the sediment continuity equation is speculation at best.

The spatial scale of model computation and representation is also a critical abstraction from reality that can induce model error. On page 969 of Wainwright *et al.* (2008b), they note that 'there is an issue relating to the mismatch of the point measurements in the field and the model, which represents conditions averaged over a cell' (e.g. computational model cell) in explaining the mismatch in modeled versus observed flow depth in the model evaluation section on the large rainfall simulator plot. In other words the 0·61 m model cell size is not sufficiently small to accurately delineate areas of inundation (concentrated flow in rills) versus non-inundated areas. This leads to a dependence of model results on computational cell size which they clearly illustrate in paper c with a sensitivity analysis of cell size. This also implies that parameters derived from some form of calibration or optimization are cell size dependent. Wainwright *et al*. (2008c) elude to this in explaining the difference between model results using a cell size of 0·61 m on the larger simulator plot and a 0·5 m cell size on the variable sized natural rainfall plots noting 'that the optimization of the erosion parameters that was carried out contains an implicit scale dependence'.

They thus come up with a scale-dependent parameterized model at the scale of their computational grid size. What is the difference between this and say the KINEROS2 model whose equivalent computational grid size corresponds to unit flow width with a spatial computational interval (*dx*)? These are both 'macro' models in that they approximate partial differential equations which are valid as time and space intervals approach zero with finite lengths and time-steps. What gain Wainwright *et al.* achieve in describing modeled erosion behavior in going to a 0·61 m computational cell scale they pay for by requiring 'spatial estimates of infiltration, flow roughness, vegetation and particle size characteristics' at each cell (page 968, paragraph 2, paper b). If such empirical data are not available, calibration or optimization are typically employed (e.g. 'There is a need to account for the local characteristics of the soils at the site, which has been carried out through optimization due to the lack of any direct empirical data') (bottom of page 973, paper b),

Wainwright *et al.'s* criticism of process-based erosion models bears resemblance to a critique made of physically-based hydrology models in the oft cited papers of Grayson *et* *al*. (1992a, and b). What most papers fail to cite is the response by Smith *et al*. (1994) who note that the physical processes of conservation of mass and energy are quite valid at sufficiently small time and space scales. The real challenge is how do we robustly characterize and/or parameterize the variability of the abiotic and biotic media over, and through which, the processes are occurring.

There are a number of challenges for modelers of this process, outside the innate complexity and spatial/temporal variability of any natural catchment and soil as noted above. Unfortunately, neither the critique of Wainwright *et al*. (2008a) of existing models nor the model they propose has fully addressed these challenges. One challenge is to be able to reliably measure or estimate the degree of effect on detachment rate by the cohesive nature of most soils. This includes accounting for crusting at the surface, and destruction of such a crust upon tillage.

Another shortcoming with the capability of current models is their inability (although not conceptually prohibited) to track erosion and deposition over a variable hillslope for a longer period and to recalculate the surface particle size changes due to local 'armoring' in areas subject to alternate detachment and deposition. Admittedly, this is not a common situation, as the slope is a significant control of these processes, and areas of deposition would be expected to be subject to deposition from storm to storm, except for rainfall detachment.

The greater challenge for erosion modelers is that of site characterization and parameter measurement. When confronted with a new site of interest, we have little hope of being able to efficiently measure even a few significant parameters that would allow simulation within a reasonable confidence interval, not to mention the challenges of finding the numerous parameters for the model proposed by Wainwright *et al.*

Have existing models been exposed to a critical series of experiments designed to test their inner workings? Here the honest answer is that this effort has been patchy and often focused on edge of field results rather than internal processes (e.g. Folly *et al*., 1999; Mati *et al*., 2006). As such, the authors are to be commended for conducting intra-plot experiments. However, there are a number of process-based evaluation papers which have shown good agreements between the results of the Rose theory and experiments and field measurements at a variety of scales (Beuselinck *et al*., 2002; Motha *et al*., 2002; Van Oost *et al*., 2004; Fiener *et al*., 2008), and this suggests that there is a demonstrable basis in reality. In general, model approaches are subject to significant uncertainties, see (Quinton, 1997; Brazier *et al*., 2000), but so will the approach presented in the Wainwright *et al.* papers. The family of models which have utilised Bennett's (1974) approach are process-based and do have some basis in reality, that is clear, but like all models they are hypotheses about what investigators believe the world is like and as Kant (1988) stated ' hypotheses always remain hypotheses, that is, presuppositions whose complete certainty we can never attain.' The MAHLERAN approach has numerous parameters that cannot be parameterised outside of the best controlled laboratory experiments and process descriptions with serious conceptual shortcomings. The problem we face is not whether a model with suspension as an assumption is better than one that relies on travel distances, but rather how we can use the data we obtain to better understand what are the key processes and in which situations they are important. It seems that Wainwright *et al.* propose to replace one modelling approach which relies heavily on parameters and relationships which are hard to measure with another, with similar if not greater weaknesses. In summary, the authors feel the modeling approach by Wainwright *et al.* proved what was already known with prior models that they critique. In the closing sentences of the abstract of their third paper, Wainwright *et al.* state 'We suggest that there are major weaknesses in the current understanding and data underpinning existing models. Consequently, a more holistic re-evaluation is required that produces functional relationships for different processes that are mutually consistent, and that have appropriate parameterization data to support their use in a wide range of environmental conditions.' We suggest in this context that '*existing models*' include the Wainwright *et al.* MAHLERAN model (*emphasis added*).

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