Hydrology and Earth System Sciences Discussions

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Comment

Interactive comment on "Does WEPP meet the specificity of soil erosion in steep mountain regions?" *by* N. Konz et al.

Anonymous Referee #3

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This paper addresses one fundamental question, that of the title: does WEPP meet the specificity of soil erosion in steep mountain regions? To this end, the authors do the following:

1. Measure monthly sediment yield by sediment traps at three replicated hillslope sites of different land use; measure soil parameters at the sites including data uncertainty considerations; measure precipitation, soil moisture and overland flow at 10min resolution at one of the replicates of each land use including data uncertainty considerations.

2. Measure annual erosion at all nine sites via the Cs-137 method including data uncertainty considerations.

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3. Calibrate the WEPP model manually and compare simulated and measured monthly sediment yield and overland flow as well as soil moisture dynamics for the three highly instrumented sites and annual erosion for all nine sites. Further, snow depth simulations for one site are compared indirectly to nearby measurements, and an independent direct comparison between simulated and measured snow depths is made at another location.

4. Conduct a 1st order sensitivity analysis of model parameters, inputs, boundary and initial conditions using the method of Nearing et al. 1990.

The authors report the following findings/conclusions:

1. Large soil conglomerates are found in the sediment traps which are attributed to incidents of mass movement rather than soil erosion by overland flow and rain splash. The massive under-prediction of measured sediment yields (factor 20-200) the authors then attribute to these mass movement processes which are not represented in the model.

2. The model sensitivity analysis confirms sensitivities found by Nearing et al. 1990 at lowland sites but points to additional sensitivities, some of which are related to alpine conditions by the authors.

3. Soil moisture is consistently under-predicted for two land uses which the authors attribute to an over-estimation of evapotranspiration by the model. The soil moisture pattern shows promise, except during snow melt periods in March-April. This prompted additional analyses of snow depth data (item 3 above). These led the authors conclude that the timing of snow melt processes are misrepresented in the model.

4. Measured annual erosion rates were under-predicted by the model, which the authors attribute to erosion by snow gliding, snow drift and avalanches that are

not represented in the model.

5. In general, WEPP did not meet the specificity of soil erosion in this steep mountain region (title) and the specificity of erosion itself was shown to require further field investigations.

1   General comments

My general comment is that, on one hand, there is very good material in this paper, particularly with respect to the data and estimated errors, which is perhaps not utilised to its full potential; and, on the other hand, analysis is presented, particularly the sensitivity analysis, that is perhaps not necessary for answering the one question: is WEPP suitable. I was going to argue that no model application was necessary to demonstrate that WEPP cannot simulate the observed types of mass movement, but then, some interesting points arise from the soil moisture simulations and really there is data to evaluate other aspects of the model while overall it would be rejected straight away as being suitable due to missing components. If this "order of events" could be taken on board then I think the paper would be more logical (see specific comments below).

I would also sell the model application as a screening exercise as clearly manual calibration is not suitable anymore (e.g. see the paper of Brazier et al. 2000 that the authors cite), and information on data uncertainty that is there (thoughtful analysis!) is not used in the evaluation of the model so that the results are biased. This seems only justified if the model is rejected outright and it is argued that any more effort is wasted until the missing processes are implemented (pragmatism). However, model improvement should be anticipated in the discussion which should then detail what are desirable elements of future model evaluation (see specific comments below). The sensitivity analysis falls short in this respect because only 1st order sensitivity is considered, and using a technique that seems to be based on strong linearity assumptions. Higher

order sensitivities will likely be important in this highly parameterised model and they will be non-linear in most cases. The textbook by Saltelli et al. 2008 would be a good starting point for a proper analysis (Saltelli, A., M. Ratto, T. Andres, F. Campolongo, J. Cariboni, D. Gatelli, M. Saisana and S. Tarantola (2008), Global Sensitivity Analysis: The Primer, Chichester, Wiley). I do not think the authors have to do a sensitivity analysis though at this point for the pragmatic reasons stated above, so the present analysis should be taken out, as much as the authors should comment that sensitivity analysis and consequently uncertainty analysis should be the next steps (see specific comments below).

2 Specific comments

P2154 L 14-15: this seems contradictory, see below

P2156 L11-12: take out last sentence as this is covered earlier, instead explain how WEPP is going to be tested, which measurements for what components of the model etc

P2158 L5: what about errors in rainfall and flow?

P2158 L15-16: so where they damaged then before the experiments commenced?

P2161 L 19-21: I am not clear about the stabilisation of model initialisation in fig 3; should the years 1-3 not be shown as well? There is a remaining oscillation of around +/-0.005 kg ha-1, does this mean this is the residual uncertainty due to initialisation errors? Please discuss.

Section 2.6: Make clear there are additional parameters to be calibrated after some were estimated by experiments. Also avoid confusion between model input variables, parameters, initial and boundary conditions (throughout the manuscript). Error ranges were available for some inputs and parameters (tab 1), why were these not used (P2161 L26-27)? The sensitivity analysis (SA) is only 1st order and hinges on strong linearity assumptions (equation 1). Discussing the appropriateness of this (e.g. with

the help of Saltelli et al. 2008) would probably lead to a rejection of the technique so the whole SA should best be left out. The most important link between sensitivity and uncertainty is not made but should be as part of the discussion: sensitive factors (inputs, parameters etc) -> uncertainty in model predictions if factors are in error -> they are (tab 1 and text) -> uncertainty propagation is necessary to reflect this in the predictions -> ... The calibration paragraph misses one important paradigm, that of "equifinality" of model parameters (e.g. the Brazier et al. 2000 ref and more recently Beven 2006, A manifesto for the equifinality thesis, Journal of Hydrology 320(1-2), 18-36). Some discussion should be taken on board as to the appropriateness of any of the three alternative techniques. It should become clear that manual calibration and fixing of parameters that are known to be uncertain (tab 1 and text) will be biasing the results. The only way around this in the manuscript from my perspective would be to class the model application as a screening exercise, as I said above.

P2163 L18-25: consider taking out, it does not seem necessary to speculate further because WEPP has been shown to fail to predict these sediment yields

P2164 L3-18: sensitivity analysis is not needed at this stage because the model clearly misses components, it could be stated here that sensitivity/uncertainty analysis would be the next step once the model is improved

Section 3.1.2: It should be said up front that the model has failed due to the lack of certain observed mass transport mechanisms, but that it is still interesting to evaluate the soil moisture component of the model separately in this section (and long-term erosion in the next section). The quality of the model predictions should be quantified (do not use "quite well"), either using a performance measure or quote absolute/relative differences. In any case, the uncertainty in the soil moisture data (P2158 L5) should be considered, perhaps as uncertainty bounds in the graphs and quoted differences. I im not convinced by the under-estimation argument (P2164 L25-27), it is clearly not a bias as stated currently because the effect is not seen in every month. It is probably a mixture of measurement and model error. For this reason, P2164 L28-P2165 L1 stands a bit isolated and should be integrated in a more rounded discussion. It should

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be explained better what the maximum standard deviations are (maximum of what? P2165). The transition from model-based findings to additional field-based analyses (P2166) is a strong aspect of the manuscript, this could even be made more explicit to the advantage of the authors.

P2167 L4-7: what is with the evidence of mass movement found in the sediment traps, is this not the first explanation?

P2167 L18-20: can it not be stated exactly what was simulated by the model at that time?

P2167 L20-22: How was this found? By modelling — then it would be highly questionable and probably should not feature here — or are there additional field observations to support this statement?

P2167 L28-P2168 L5: the evapotranspiration (ET) discussion should be linked to the hypothesis of over-predicting ET in section 3.1.2 to see if findings are consistent — at the moment both sections seem contradictory

P2169 L1-2: replace the last sentence with a more considered statement of what the missing processes are and how they could be tackled next including the reported data uncertainties etc, in general more could be made of the learning experience through the field experiments which is valuable in its own right (without the model)

Finally, how was the contributing area of each sediment trap determined to compare erosion rates to the simulations (tab 2, fig 5 and corresponding text)? Does not another level of uncertainty come in here?

3 Technical corrections

P2154 L12: take out ref to Chernobyl accident, in general cut down explanations of techniques throughout the manuscript which readers can look up elsewhere, the refs are given

P2155 L2-3: give more recent ref and only 1

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P2155 L14: tone down sentences like this with "enormous", where possible give actual figures, sometimes this is done, sometimes not

P2157 L1: ref is not in the literature list

P2157 L21-22: it is not too clear how the flow is captured — I know this is difficult to describe in brief ...

P2158 L12-15: replace 3 sentences with something like "However, we installed the sediment traps in July 2006 one year before the beginning of the experiments. This ensured the recovery of the soil edges and regrowth of the grass and, therefore, mitigated the above error.", other sentences throughout the manuscript could be shortened like this as well

P2158 L19-21: last sentence does not seem to add much so consider taking out

P2158 L25-P2159 L6: add refs in parentheses right after previous sentence and end with "using a NaI scintillation detector.", cut out all text in between as this is unnecessary information given the refs

P2159 L12-13: take out as covered before

P2159 L15-18: take out but keep ref as this explains it all — by the way, "in review" papers should be referenced as such and not "2009" (this applies to Schaub et al. and Konz et al.), these should be accepted before this manuscript can be accepted

P2159 L20-22: report 3 individual standard deviations, mean is confusing, make clear that in this case the standard deviation is from 3 replicated Cs measurements at each site — by the way, wherever "mean" or "maximum" standard deviations are reported throughout the manuscript it should be noted out of which population, often this is not apparent

P2160 L7-8: take out sentence as this is obvious

P2160 L12-17: take out except ref which should go at the end of previous sentence

P2160 L27: mark met station in fig 1

P2161 L14-18: consider combining 2 sentences in 1 short one

P2162 L14-17: this sentence should be taken out here, PEST is not the only optimisation technique, in the discussion section a more rounded discussion should be made

of automatic optimisation versus manual calibration versus the equifinality approach, some more key refs would be needed

P2167 L11-12: take out, not necessary

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2153, 2009.

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