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Hydrology and Earth System Sciences Discussions

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

N. Konz et al.

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First of all we highly acknowledge the fair and very constructive contributions of both reviewers and of the editor. The suggestions helped us to restructure our findings and significantly improve the scientific content of the revised manuscript. In the following we respond to each of the reviewer’s comments; our answers are written in between #. The revised version of the manuscript will be submitted by the end of next week.

Answer Referee3

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). #We highly acknowledge the detailed comments of the reviewer#3 and fully agree with him that improvements can be made by utilizing the full potential of the available data. In fact, the basic idea of running WEPP simulations in

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our alpine catchment was to demonstrate whether a model that accounts for erosion processes of agricultural land and that is established for low lands can be used in alpine environments. The reviewer is right with his statement that a suitability assessment could be made just by looking into the governing equations, but would this also give insides into the missing process description? (Most likely not; because little is known about the driving processes of erosion in alpine environments.) The model application helps to identify relevant knowledge gaps. Since we can assume that low land erosion processes (we use this term to summarize the dominate erosion processes of agricultural low land sides) are captured well by the model and most of the used parameters are measured so that the uncertainty is within acceptable ranges relevant inaccuracies have to be caused by missing process descriptions. This becomes even more relevant if the simulation errors occur at a distinct season, e.g. in spring after snowmelt. Thus, snow cover related processes that are not taken into account by the WEPP model need further investigations and have to be incorporated into the model structure. If we understand the reviewer correct this is meant by the ´screening exercise´. However, we do not agree that the sensitivity check of model parameters is redundant due to the following reasons. The initial parameter set was derived from measurements and literature without parameter tuning. These parameters already gave the presented simulation results with good agreements during summer and shortcomings in spring after snowmelt. As stated by the reviewer manual calibration is not suitable any more, we wanted to explore additional model runs within reliable ranges of parameter values. The 1st order sensitivity analysis helped to identify the most relevant parameters that were than analysed in depth by means of Monte Carlo analysis. We computed 10000 runs with different parameter constellations but no significant improvement compared to the initial parameter set was found. In fact, the error pattern remained the same for all runs with relatively good agreements between measurements and simulations during summer and deteriorations in spring. The reviewer is right with his statement that more attention should be paid to this analysis and to the resulting missing process identification. A thoroughly parameter analysis, e.g. within the GLUE framework was

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not the scope of this study but, as also stated by the reviewer is required for a revised model version if the missing process simulations are included in a physically sound way. We follow the above discussed ideas in the revised manuscript and restructured the paper.#

2 Specific comments

P2154 L 14-15: this seems contradictory, see below #It is not contradictory but we agree that the used terminology of short- and long-term erosion is misleading. We therefore changed short-term to vegetation period erosion in order to make clear that only erosion rates occurring during the vegetation season are meant. The expression long-term is not changed because it describes the entire measurement period of 21 years. Now, it becomes clear that the erosion processes during vegetation seasons are generally well simulated, whereas the long-term rates are underestimated due to the missing winter mechanical processes (e.g. friction of snow cover, etc.).#

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. #We rephrased the last section of the introduction.#

P2158 L5: what about errors in rainfall and flow? #The error of overland flow is not known. We described in chapter 2.3.1 that the overland flow is assumed to be slightly underestimated by the measurements but cannot be reliably estimated due to missing comparative measurements. The rainfall stations operated by MeteoSwiss (Swiss Meteorological Service) are known to underestimate precipitation. Comparative measurements have shown that the underestimation ranges around 20%. Although it is well known that the precipitation amount is a driving variable for erosion the simulation error cannot be explained by wrong precipitation inputs (checked by sensitivity analysis of precipitation data).#

P2158 L15-16: so where they damaged then before the experiments commenced? #Yes, the steel plates were flattened during winter time. We repaired the traps in the field immediately after snow melt.#

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. #The first 3 years of simulation are not intended to be used for the assessment of erosion rates due to the stabilization of the vegetation cover. For the initialization, the vegetation cover and biomass parameters were given to represent the situation at the beginning of the period of interest, (see Table A2) and three years are simulated in order that the model will produce reasonable initial conditions by simulating plant development and cuttings. The simulation of vegetation cycles requires a few years to stabilize, e.g. 3 years in our case, such that the state of the system immediately at the onset of the simulation period of interest are accurate. The oscillations in erosion rates are due to non-annual plant growth and senescence. #

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)? #All parameters have given ranges either due to measurements or due to literature values especially publications by USDA. Therefore, the initial parameter set was derived from predetermined values. However, as correctly stated by the reviewer, the parameters have of course certain ranges of uncertainty. We therefore conducted Monte Carlo simulations based on the sensitivity analysis, please see our response to the general comments. The sensitivity analysis was conducted with fixed 10% margins. This corresponds to the maximum error in Table 1 and is therefore considered to be a conservative approach. We revised section 2.6 in the new manuscript.#

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

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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. #See discussion above (General comments). We added a discussion on equifinality in section 2.6 and rephrased the aim of the sensitivity analysis to only detect relevant parameters for the Monte Carlo analysis. It was not intended to use the sensitivity analysis to detect parameter uncertainties. Thus, we fully agree with the suggestion to class the model application as screening exercise. In fact this was the original purpose of the study and we try to make that clear in the revised manuscript.#

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 #We agree and deleted this section.#

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. #See above and response to general comments#

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

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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. #Please see new section 3.1.2. We reformulated the discussion according to the reviewer’s advice.#

I am 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 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? #We fully agree with the reviewer, that the potential of the available data is not used to its full extend. Furthermore, we highly acknowledge the critical

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comments of the reviewer that helped us to restructure the findings of the manuscript. The above mentioned points are considered in the revised version which now focuses more on the process identification rather than the model evaluation. The contributing area was determined by an upper boundary at 20 m above each sediment trap.#

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 #Thanks for this comment. Generally, you are right that general explanations are not necessary. However, it should be clear to the reader that the Cesium-137 originates from Chernobyl and not from bomb tests in the early 60ies.#

P2155 L2-3: give more recent ref and only 1 #We changed reference to Nearing et al. (2004). Expected climate change impacts on soil erosion rates: A review.#

P2155 L14: tone down sentences like this with “enormous”, where possible give actual figures, sometimes this is done, sometimes not #Words like enormous, quite, very are taken out of the manuscript.#

P2157 L1: ref is not in the literature list #We added the reference to 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 ... #The installation of the steel plate as well as its function is added in 3 sentences.#

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 #Thanks for this suggestion, it is included in the revised version.#

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

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sentence was taken 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 #Thanks for this suggestion, it is included in the revised version.#

P2159 L12-13: take out as covered before #This was changed after your suggestion.#

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 #The paper Konz et al. 2009 has been accepted now by the journal of Plant Nutrition and Soil Sciences. Thus, the detailed description is not necessary anymore. The reader can look up the cited reference were data uncertainty are explained in detail.

P2160 L7-8: take out sentence as this is obvious #Sentence was taken out.#

P2160 L12-17: take out except ref which should go at the end of previous sentence #This suggestion has been added to the manuscript.#

P2160 L27: mark met station in fig 1 #It is not possible to mark the met station: the station is 1 km north-east from the right hand side end of the figure. This information was added to the caption of figure 1. #

P2161 L14-18: consider combining 2 sentences in 1 short one #The sentences were shortened in the revised version. #

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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 #We agree, please see revised section 2.6.#

P2167 L11-12: take out, not necessary #The sentence was taken out.#

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

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