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Geoscientific Model Development Discussions

Interactive comment on "Global Sensitivity Analysis of Parameter Uncertainty in Landscape Evolution Models" *by* Christopher J. Skinner et al.

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Skinner and coauthors' sensitivity analysis of landscape evolution models is a much-needed addition to the geomorphic literature. Many evolution models often have been treated as sandboxes in which to experiment with quantified conceptual process understandings rather than as predictive models, with a few exceptions that include earlier work led by coauthor Coulthard using the CAESAR model. I also think that I have known of this work in progress for some time, as my lab donated some compute time at the request of coauthor Schwanghart.

Conceptually, I find the idea of a sensitivity analysis to be a very good approach. Often, landscape evolution models include competing processes designed to simulate

the effects of real processes. Such additions typically include descriptions (qualitative and/or quantitative) if their impact on a landscape, but often do not include a more mathematical analysis of how these processes influence the solution space.

The work of Skinner and coauthors is significant and publishable, but on reading their paper, I found multiple causes for concern. I could not find easy answers to the most major concerns, enumerated below.

- The sediment transport formula was the dominant source of uncertainty, but I believe that this may in part be because these sediment transport formulas were not appropriate for Tin Camp Creek (Australia).
  - The grain size distributions for the rivers displayed significance of sand in the UK and a dominance of sand in Australia. Both the Wilcock and Crowe (2003) and the Einstein (1940's-50's) formulas are tested with coarse sand as the smallest grain size class. In the Australian case, about 50% of the sand is finer the grain size used to produce the sediment transport formula. This is at the upper limit of the curve in Figure 6 of Wilcock and Crowe (2003), where their solution begins to bend more sharply but the data end. Therefore, there is great uncertainty and little constraint in the formulation.
  - The Australian example has a dominance of sand. Are there bedforms that appear in the river? If so, could you discuss the role of their form drag, which to my knowledge is not included in your model, and how it could affect sensitivity to choice of sediment transport formulations?
  - I find your discussion of sediment transport in section 4.3 to be unnecessarily vague. It is not unusual to see in the landscape evolution modeling literature a statement to the effect of "sediment transport formulas are problematic and it is a difficult thing so the error is probably there". Scientifically, this is unhelpful and in my opinion a little lazy. I think that here you have the opportunity to analyze why this is your major source of uncertainty, which is

one way in which I hope this study can rise up above the others. Regardless of whether anyone trusts the form of your sediment transport formulations for the chosen grain size, form drag, etc., you have two mathematical formulations that must produce divergent outcomes for the set of provided hydrologic and topographic states. (I am presuming that over your 30-year time scales of interest, overall topography changes little.) Based on an analysis of these formulations, can you make a prediction of the factors that lead to this divergence?

- Your discussion notes that the sediment transport formula's importance may be overstated due to the smaller number of options for this than for the other variables. However, you do not analyze this possibility. or whether this would even lead to sediment transport formula remaining the dominant influence. Could you argue how your conclusion about sediment transport formulas is (or is not) still valid, considering this? I make a suggestion below (530-533).
- Your premise is to test landscape evolution models, but the 30-year model run period is much shorter than most geomorphic models are used. Indeed, I wonder how much landscape evolution occurs, versus how much, over these time-scales, CAESAR can be thought of as a sediment-routing model with erosion or deposition being negligible (and therefore avoiding the nonlinearity in which changes in topography affect the long-term response of a LEM.) I think that this short time scale should be made explicit early in the paper. A discussion of how these results can (or cannot) be transferred to different time scales would be helpful as well.

In addition to these, the paper would be improved by a careful set of proofreading. It is repetitive in several places and includes a number of issues in both grammar and style. The overarching issues here are:

- Many proper nouns are capitalized; why?
- Your abbreviations should be used with an "s" to indicate whether or not they are plural in a given instance
- The Morris Method is mentioned 2–3 times before it is defined or described. Its description should be more closely tied to its in-text mentions.
- You define the difference between an objective function approach and a sensitivity analysis at multiple points; reduce this to one.
- In general, many explanations are very "hand-wavey". Please do a thorough read-through to reduce the fluff and improve the density of new information. If this is not done, it will be hard for a reader to see what interesting new conclusions you have come to.

Line-by-line and section-by-section comments are as follows:

Abstract: Why 3 paragraphs? I think you can shorten and tighten this.

18. em dash after models; comma after example. Sensitivity Analyses one example here of something that is capitalized for reasons I don't understand; I don't think that this is just UK English.

47. I do not believe that your above references cover glacial or aeolian processes. Disregard if I am incorrect (does CAESAR include aeolian processes?); add references or remove these notes if I am. They are unimportant anyway to the landscapes that you are studying.

61. Comment: even few-parameter stream-power-based LEMs are quite heuristic. I do not think that your note here is unique to models with large numbers of parameters.

65. Correctness: an analysis cannot investigate, but you can.

79-85. I appreciate this list!

122. Define MM here or list section in which it is defined.

128. Sentence fragment after comma

130. Incorrect in general: many landscape evolution models are not designed to be predictive over annual to decadal time scales. I find CAESAR to be quite unique in its time-scale flexibility, due (I believe) to its explicit integration of flow and sediment transport processes.

131. I don't see what you mean by "multi-dimensional approach"

133. When is an objective function not a score between observed and simulated values? Or do you mean that we can have synthetic observations?

149-152. I think that point-based measurements must account for all of the complexity in the system, but may not be able to distinguish the source of the measured parameter's value.

155. What is a width function?

155. Cumulative area distribution – of what area?

156. It is not possible to use variables as an objective function. One needs... a function. This may include these variables, of course!

162. , and so are (comma)

162. "more objective" is vague: do you mean more in quantity or more as in better?

165. "data" is plural. "data" is included twice as well, and the sentence is generally awkward.

167. Really? These data are not available? Not even in heavily-monitored experimental catchments? It is difficult to make sweeping statements, so I would ask you to prove this.

175. which –> that. This is an important distinction, and is often overlooked.

176. rm "will": tense confusion

185-186. "Medium" and "small" are nearly meaningless; could you provide catchment areas?

200. Is the rainfall time-series uniform in space or not? (I read later that it need not be, so please note this here, as this sets CAESAR ahead of other LEMs)

204-205. Note how erosion and sediment transport are calculated here, as this is central to your conclusions.

205. What is an "active layer system"?

223-224. This is where singular vs. plural usage of acronyms can stand out.

240. What is the Design of Experiment? And how did it use R?

250. is –> are

250. these constitute the Main Effect. (otherwise it is not clear how two things become one)

266. elevation drop is also called "relief". I understand it here, but "relief" may be more common.

277. Grammar: Contrasting –> In contrast.

279. Comma after "Swale"

280. I thought that "rain gauge" is two words.

282. Did you therefore use uniform precipitation for the Swale catchment?

Table 1: (3 and 4) Do you mean to say that you prescribed a lateral erosion rate that is constant? This seems strange to me. (9) In/Out of what? (13) Evaporation or ET? (14) Roughness for channel, hillslope, or both?

311. It is definitively not qualitative, as it gives you numbers! Perhaps “quantitative but subjective”.

320. “Laws” is really strong. “Formulas” could be better. Or formulae, as you seem to be leaning Latin. Except with Germanic-leaning capitalization.

Figure 2: Perhaps note the D range of the data with which the sediment transport formulas were created

340-343. Section 1.3 doesn’t include what you state here. I also find it hard to believe that topography and discharge should have no relationship to geomorphic change. You will need to provide some evidence.

343-344. This is no surprise: they studied equilibrium landforms, while you are studying 30-year time scales in which only extreme events can cause significant landscape change. In other words, your time scale removes the significance of topographic evolution and its associated feedbacks on the system.

348-350. Yes, you have mentioned this (note my general comment).

370-371. How have you assessed that 10 model years is a sufficient spin-up time?

381, 383, etc. Consider giving full parameter names where possible to help the reader follow the text.

385. which –> that

Figure 3. Consider full model names; otherwise, you have converted numbers to codes, which readers will then have to cross-reference with your table.

Figure 5. Is this catchment-wide elevation change, in-channel elevation change, or otherwise? In addition, are re you certain that mean elevation change is the appropriate metric? I can imagine that significant spatial variation in aggradation vs. incision could occur, and wonder how much this may affect your results. In addition, tens of cm of incision over 30 years seems very rapid to me: could you comment on this?

433. Small “s” on “LEMS”

443. How much do you trust gauged suspended sediment discharge and the associated rating curves? I do not know that these are so straightforward either. And if you mean bedload + suspended load, then I would argue that the data generally do not exist.

4.1 (general). This section indicates to me that solving the LEM problem may be impractical due to the amount of time-lapse spatially distributed data required. Could you comment on this?

4.2. This section seems just to read, “we don’t know how these models work and what the general rules are”. It is OK to just write that! This seems to beat around the bush.

456-457. Environmental models can be transferable between catchments. For example, I would argue that a thermal model is very transferable! Please be clear in what you mean by “environmental”.

473. Your sediment transport formulas do not include thresholds. Please explain how this compares to thresholded models if you include this point.

475. Your formulas include one that performed well in the Gomez and Church test and one that was not considered. What is the basis for using GC 1989, therefore, to declare that sediment transport formulas are not good?

477. Both of the formulas that you have employed are based on theory, and fundamentally on the force balance on a grain via the Shields number. I would suggest to not simply call these “empirical”, but to actually note where the boundary between theory and empiricism lies. Indeed, these may be, for better or worse, some of the more theory-grounded components of a LEM! (Perhaps 2nd to the hydrodynamics)

479. How do you know that they were not intended to represent variations in flow conditions? This statement is inconsistent with the fact that the underlying experiments have been performed at a wide range of tau/tau_c ratios. You should be more specific

or remove this comment.

489-490. Non-stationarity in hydrologic models seems a bit off-topic here.

482-493. Do you think that the issue of scaling and calibration should deserve at least its own paragraph, if not its own section?

507-514. Do you mean that LEMs should follow hydrologic models' approach to uncertainty estimation in general (there are many such approaches), or specifically Lisflood-LP, and why? In addition, this paragraph gives little information about what these approaches are and why they are good.

523. But it *is* quantitative! I think you are again confusing "subjective" and "non-quantitative".

530-533. This is a bit of a bombshell that you are dropping on yourselves at the end: so you are unsure that the experimental design fairly weights the sediment transport formulas compared to the other values? There seems to be an easy answer, though: just take the binary extreme values of the other variables, and compare a subset of the runs with only 2 states considered for each parameter?

534-543: I think that the compute time and number of models should be mentioned far earlier in the paper (methods/results), and then perhaps referred to here as a reason for your decisions.

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