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Hydrology and Earth System Sciences Discussions

Interactive comment on "A distributed continuous simulation model to identify critical source areas of phosphorus at the catchment scale: model description" *by* B. K. Koo et al.

Anonymous Referee #1

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Review summary

1) Does the paper address relevant scientific questions within the scope of HESS? YES

2) Does the paper present novel concepts, ideas, tools, or data? YES

3) Are substantial conclusions reached? NO

4) Are the scientific methods and assumptions valid and clearly outlined? SEE BELOW

5) Are the results sufficient to support the interpretations and conclusions? NO

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? NO

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Interactive Discussion

Discussion Paper

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES

8) Does the title clearly reflect the contents of the paper? YES

9) Does the abstract provide a concise and complete summary? ALMOST

10) Is the overall presentation well structured and clear? YES

11) Is the language fluent and precise? YES, WITH EXCEPTIONS

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES, WITH EXCEPTIONS

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? NO

14) Are the number and quality of references appropriate? YES

15) Is the amount and quality of supplementary material appropriate? YES

General comment

This a generally well-written, interesting description of a process-based, distributed model of phosphorus transport in catchments, which is of broad interest to HESS readers and deserves to be published after consideration of the issues raised below.

A general comment is about the applicability of this type of model. Although this model might be described as process-based, there are also a large number of empirical parameters (the model may equally well be described as semi-empirical). In order to make the model truly distributed, and able to meet the requirements the authors lay down for useful CSA models, the spatial variability of at least some of these parameters would presumably need to be represented. Also, the construction of a model of this type involves a large number of assumptions about the nature of processes and how they can be simplified (apparent from the number of times "assumed" is used in

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the paper). Some processes in CAMEL are omitted just because they are too complex to simulate (sediment transport via drains). So many such assumptions have been needed, and choices made from equally plausible equations, that the model as a whole, arguably, is quite arbitrary. And it is so complex, that it will be impossible to validate or falsify it scientifically for individual catchments. Although textbook values of most parameters are available, the associated uncertainty is very high. Exploration of the uncertainty by testing sensitivity (at least to parameter uncertainty and preferably to model structure as well) is a pre-requisite to justified application of this type of model. In CAMEL, some empirical parameter values seem to be hard-coded, for example Eq. 6 fixes the relationship between rill flow and rill flow width - implicitly assuming a consistent rill shape, slope, roughness and steady-state conditions - the effect of this on phosphorus export will therefore be untestable. The most significant limitations of the CAMEL model are not the omitted processes listed under Section 7.3, but the above-mentioned issues of uncertainty.

My main interest in this paper is the way it exposes the large number of assumptions and empirical equations needed to construct a 'process-based' model. The authors should recognise these general issues in at least a few sentences in the introduction and discussion. Given these issues, some assertions in the discussion about the suitability of CAMEL are too strong. I assume that there is a companion HESS paper giving an application. I'll be interested to see how the authors perform a scientific model assessment and what they have to say about uncertainty in identifying CSAs.

Specific comments

1. Page 1360, line 17. 2D or 3D models using grids are rarely numerically efficient for catchment nutrient modelling - the areas of the catchment which wish to simulate in higher resolution (e.g. CSAs) dictate the grid size over all (or much of) the rest of the catchment therefore making the solution unnecessarily inefficient. Finite elements (e.g. triangular) are potentially much more efficient in this respect. In any case, this sentence in the abstract is not justified by the information given in the rest of the paper

about the numerical solution scheme (see also point 7 below).

2. Page 1362, line 18. In most HRU models I know, the HRUs are connected explicitly with each other, including representation of the routing of surface flow from one HRU to the next where appropriate. (In this respect they might be more realistic than CAMEL in which every grid-square slopes into a channel).

3. Page 1369, line 14. Although there might be accurate information about drain depth and spacing (although this is rare), a large proportion of field drains in the UK were installed many decades ago and have become broken and blocked in places, and no longer function as drains. This model will mis-represent response unless effective values of spacing are used. This illustrates why a process-based model cannot work without calibration.

4. Section 3.5. This could be clearer. Presumably the hydrograph ordinates need superimposed to estimate flow at every cell output, not just the catchment output? How is the mean channel water depth related to channel width? - the authors should consider including an equation for this.

5. Page 1375, line 24. Another nice example of the empiricism of this process-based model. Is the exponent calibrated? Should "decrease" be "increase"?

6. Page 1376, line 4; Page 1386, line 12. What do the authors mean by "comprehensive"? This part of the model does not sound at all comprehensive - it is based on a simple flow routing model and some simple empirical sediment suspension/ deposition equations. This may be justifiable, but it is not "comprehensive".

7. Page 1378, line 20. More details are needed about the numerical solution scheme. The model includes a pde (Eq 15), and to simply say it is solved by Runge-Kutta is not sufficient. What time-step is used for the solution? 1 day, 1hr, or a variable time-step? The abstract says the model is numerically efficient. A variable time-step is a pre-requisite for a numerically efficient model of this nature.

8. Page 1386, lines 4, 17. "the P processes are simulated appropriately", "CAMEL satisfies the requirements". An outcome of the model description is that CAMEL may do neither of these things, and that it won't be possible to validate this either way. The authors need to be more modest and realistic, and not detract from the value of the paper by over-zealous claims.

Technical corrections, etc

1. Page 1361, line 30. "note"

2. page 1364. Data requirements. Need to include P inputs, and drain depths and spacing.

3. page 1366, line 17. "the transpiration process"

4. page 1367, line 16. mm h-1 ?

5. Equation 10. Gives preferential flow per grid cell?

6. Page 1376, line 23. Something is either in equilibrium or not - the terms "rapid equilibrium" and "slow equilibrium" need re-phrased.

7. Page 1377, line 9. The rate is assumed to vary with temperature?

8. Page 1386, line 8. Not true, sediment export from drains can be significant.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 1359, 2005.