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Hydrology and Earth System Sciences Discussions

Interactive comment on "Modelling the inorganic nitrogen behaviour in a small Mediterranean forested catchment, Fuirosos (Catalonia)" *by* C. Medici et al.

Anonymous Referee #1

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This study presents the application of a nitrogen model to several years of data for a Mediterranean forested catchment. The nitrogen model is coupled to three hydrologic models (one lumped and two semi-distributed). The study finds that including a riparian zone in the modeling of nitrogen improves process representation and modeling results. This highlights the importance of the near-stream zones in this region.

In general, the manuscript is well written. In particular, the authors have done a nice job reporting on the 'true' nature of their modeling efforts (ignoring the last two sentences of the manuscript – see the last specific comment). They clearly are tuning a highly-

parameterized representation of reality. They give a clear summary of the number of parameters needed (most of which require some form of fitting or optimization) to recreate the complexities of nitrogen cycling on the landscape. There is no reason this study should not be published in HESS after some minor revision. There is, however, a general comment the authors should consider in these revisions before publication.

In the adaptation of an existing nitrogen model component to the one used in the current study, there has been some additional parameterization needed to adequately represent landscape nitrogen processes. Namely, a soil moisture threshold has been added to all soil biological processes. The conceptualization make sense and is justified due to the non-linear nature of soil biological processes, however, the authors do not do an adequate job of assessing the sensitivity of the fitted soil moisture threshold values. There is some mention in the results to which model parameters are sensitive, but no real formalized comparison. I feel the manuscript would benefit if the authors presented a more structured under taking of the sensitivity analysis of all parameters (especially these soil moisture thresholds!). The soil moisture threshold seem to be a crucial conceptual (process-based understanding) and mathematical (to get that non-linearity in responses) component of this modeling effort. The question is how crucial is this threhsold value or how more crucial is it than the other parameterizations? This needs to be raised up in the manuscript as it forms somewhat of a cornerstone to the study.

Other than the above general comment, the work is well presented. There are a few specific and editorial comments (see below) that need corrected. In particular, the last comment must be considered.

Specific and editorial comments (Comments are organized according to page number P and line number L)

P5667L2: 'since these' what? The eco-systems or the processes? Clarify.

P5667L21: Change to 'Schiwinning et al. (2004a, b)'

P5668L23: Add ‘latitude’

P5670L17: Here and throughout, is it ‘semidistributed’ or ‘semi-distributed’?

P5672L23: Here and throughout, do you really mean ‘progressed’?

P5672L24: How are you defining the spatial extent of this riparian zone? What is the riparian zone? Clarify.

P5674L1: The methods use 3 years of data for calibration and 1 year for validation. Is there any real reason for this split or was it picked ad hoc? Maybe comment on this here.

P5674L4: What is ‘the temporal validation process’? This is the first time you have referred to it. What does this mean?

P5674L6: Define what you intend by ‘global’.

P5674L15: Change ‘firstly’ to ‘first’.

P5675L4: Remove ‘by’.

P5675L8: Spell out ‘E.g.’ as ‘For example’ here.

P5676L10: Remove ‘:’ and start a new sentence.

P5677L9-16: This is not a serious presentation of a sensitivity analysis. I think more rigor could be brought in as it is fairly important in this study. See general comments.

P5678L27: Change ‘influent’ to ‘influential’.

P5679L4: Change ‘call’ to ‘calls’

P5679L4: Remove ‘just’.

P5680L26: Change ‘form’ to ‘from’.

P5681L20: Change ‘closed’ to ‘close’.

P5682L1: ‘reverse flux’ of what? This is the first time you use this terminology. It needs some more explanation.

P5683L24-27: This is not a fair statement and (arguably) not a true statement. I highly doubt the models presented in this current study could be ‘easily generalized’ or ‘far extrapolated’ to other catchments. The models used were highly parameterized and require much data (i.e., 3 years of intense observations) for a calibration period. There is no support to a statement on porting the models to other Mediterranean or semi-arid regions. The authors either need to remove the last two sentences completely (which is what I suggest) OR provide some real support to such far reaching and general statements.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 5665, 2009.