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Discussions

Interactive comment on "Technical Note: Alternative in-stream denitrification equation for the INCA-N model" *by* J. R. Etheridge et al.

Anonymous Referee #1

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In this technical note, the authors propose the use of an alternate equation to describe the in-stream denitrification process in order to improve the INCA-N model capability to reproduce the low nitrogen-nitrate concentrations observed during summer. This alternate formulation is based on the mass transfer coefficient, as previously suggested by Birgand et al. (2007). In order to test the benefits of this formulation, the authors compared the simulated in-stream nitrate concentrations obtained with the original INCA equation for the in-stream denitrification process and the ones obtained with the alternate approach against some available observed concentration data.

In general the manuscript is well written and clear and the authors are quite explicit and transparent about the hypothesis adopted in their work.

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Discussion Paper

As the authors stated clearly, the idea of a mass transfer coefficient is not new, but this represents an application of it considering a widely applied and well tested model, the INCA-N model, that as far I am concerned, has not been previously done.

However, there are some scientific questions that I think the authors should take into account very carefully:

1)I think the authors should explain in the manuscript the reasons why the alternative approach, based on the mass transfer coefficient, gave better results during summer then the original INCA-N approach. They should discuss explicitly the mechanism that in their opinion allows better reproducing the low summer nitrate concentrations, but not other periods, such as the beginning of the wet season in September.

To this end, considering m_INCA (eq. 1) and m_alt (eq. 4), the mass transfer coefficient could be written as:

$ro\_n = Rn \cdot h^*$

Where ro_n is the mass transfer coefficient, Rn is the INCA-N original denitrification rate and h* is a factor that can be understood as an "effective" constant water depth for the volume of water stored in the reach. Therefore, we could say that an "effective" volume of water $V^* = (A \cdot h^*)$ is calibrated when the equation 4 is used; where A is the estimated stream bottom area of the reach.

The equations 1 and 4 of the manuscript can be written as follows:

$m\_INCA = Rn \cdot V \cdot C$

$m\_alt = Rn \cdot (A \cdot h^*) \cdot C = Rn \cdot V^* \cdot C$

Where C is the in-stream nitrogen-nitrate concentration on the previous day.

Considering that all the terms of the equation 3 of the manuscript are exactly the same for the two approaches, being the denitrification term the only one that changes (as the authors stated clearly), we could say that the only difference between the results

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obtained and presented depends on the calibrated value for V* compared to the value of V simulated by the hydrological model. As a matter of fact, we can easily see from figure 1 that for most of the time the calibrated value for V* is greater than the simulated water volume V, since the dashed line representing the alternate equation is almost always lower than the continuous line representing the INCA-N model original simulation. During the summer period V* is much bigger than the simulated water volume V, so the difference between the two lines is big, while during the wet period V* is much more similar to the simulated V so the differences between the two lines are not so significant.

In my opinion, this leads to think that the reason why the INCA-N model is not able to reproduce the observed nitrate concentrations should be searched somewhere else than the in-stream denitrification equation. Actually, it seems to be much more related to the hydrological component of the model than the in-stream denitrification process regardless the type of equation used by the authors. Can this also give some clues about why the calibrated value for the mass transfer coefficient needs to be higher than the range published by Birgand et al. (2007)?

I think the authors should discuss this point carefully, because even if the evaluation of alternate equations is always of interest and it may help to understand better a model behavior, they should be more prudent in drawing conclusions from this work if the premises are not the correct ones.

2) To implement the alternate formulation the authors estimated the stream bottom area of the reach considered. I would have liked to see some sensitivity analysis results to understand how much this estimated area may affect the results, since I think there may be quite a lot of uncertainty related to this estimation.

3) Another point I would like to highlight is that the authors do not present any type of validation for their conclusion. In fact, the data set considered for the model calibration is quite short itself. This, together with point 1, makes me doubt about the robustness

and validity of the results presented. I would suggest to the authors to at least validate the model considering a different data set before any publication.

4) I could not understand very well what the authors wanted to say in chapter 3, lines 22-24, about the uncertainty associated to other parameters estimated. I suggest rephrasing the sentence and clarifying the idea.

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