

Interactive comment on “Continental hydrosystem modelling: the concept of nested stream–aquifer interfaces” by N. Flipo et al.

Anonymous Referee #2

Received and published: 6 March 2014

The paper discusses continental hydrosystem modelling of nested stream-aquifer interfaces. I have read the paper twice and find the key-message very hard to see. Right now it more reads like a rather detailed list of not always well-connected references, it will need quite a bit of work to develop a message. Because the overall message is not very clear I find it very hard to comment on the paper. I also find that the link between individual chapters is not always as clear as it could be. For example section 3.2. Most of this section is a recitation of Marlad, but I struggle to see how it integrates. An option to fix this could be to make a link between physical controls on connection/disconnection and the hyporheic zone. Another example is the remote sensing part at the end. It is not integrated well in the paper, and therefore the practical use of this section remains unclear. Before a decision can be made, the authors should

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distill the messages in a clearer way. However, I believe the paper has potential and encourage the authors to submit a revised version. Below more detailed comments that might help.

• Generally I got the impression that the authors cited many papers but that these papers did not always really fit to the context. I would suggest to very carefully recheck if the citations are appropriate.

• Page 453, line 24: Why is this a paradox? I disagree, as considering the stream-aquifer interface as a continuum does not exclude that there are different functions and behaviors within this continuum. It just depends on the perspective and the questions asked. I think the reason why hydro systems modeler like turn away from a clearly defined line between these interfaces is that is just very hard to identify them.

• Page 468, line 9 (Fig 2): Even though the authors cite a paper by Brunner et al 2010 it is not picked up in the discussion of the conductance concept. Figure 2 and many more points made in this context have been discussed in detail by Brunner and should be mentioned at this place. Note that the paper by Mehl and Hill 2010 did not consider any clogging, e.g. the hydraulic conductivity is equal to the aquifer. Also, the cited paper by Irvine et al should be mentioned in this paragraph. They showed under which hydraulic conditions simplifying heterogeneity will not bring along uncertainty. As far as I know they did not directly comment on the conductance concept as is suggested elsewhere in the paper.

• The recent work by Partington et al has been missed and is highly relevant for this paper. The hydraulic mixing cell method allows to deconvolute the stream-discharge of the model into its individual components (eg Partington et al Water Resources Research Volume 49, Issue 9, 2013, Pages 5501-5519 or Partington et al Environmental Modelling and Software Volume 26, Issue 7, July 2011, Pages 886-898)

• There is little mention of uncertainty in the paper. This is a deficit, as the models will always be wrong, but the choice of model complexity is related to this uncertainty.

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It would be very useful to at least mention that there a large range of methods that can be applied to estimate this uncertainty (See e.g. the papers by John Doherty).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 451, 2014.