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Interactive Comment

Interactive comment on "Inverse streamflow routing" *by* M. Pan and E. F. Wood

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Review of Pan and Wood HEESD-10-1-2013 Inverse Routing

The authors are correct in their starting point that it would indeed be good to have a method of going from measured streamflows to spatial patterns of runoff generation using an inversion methodology. They also recognise that this is fraught with problems and needs to be constrained. They provide a procedure that gives spatial maps of runoff but, to my mind, they oversell what information is added. This is partly because they do not closely at the sub-catchment patterns produced – most of the pattern in their maps comes from the differences between stream gauge observations of sub-catchment specific discharges. It is also partly because they assume that uncertainty in their input data is negligible (as this makes the inversion much more difficult).

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In essence this is not so different to many geophysical inversions which produce nice images but which cannot be checked. Here the check is on reproducing the gauged discharges, but that is also the input that produces the patterns.

For this paper to be publishable I would like to see the authors add an investigation of the inferred sub-catchment runoff patterns in relation to rainfall patterns. The runoff should show some time delay banding that might be quite unrelated to rainfall patterns (in some cases the inferred bands could apparently match elevation related precipitation patterns, but that should not always be the case).

I would also like to see an investigation of the impact of uncertainty on the inversions. The assumption that the gauged discharges are known without error is clearly a useful constraint, but not a very realistic one. We know very well that even USGS gauge data can be subject to some uncertainty, and for both high and low flows that might be significant. It seems unlikely that they may well have looked at this in the Kalman filter framework, but they have chosen not to present the results. Why is that? Some negative values were inferred already (p17). Does the inversion become unreasonably or unstable in the face of realistic uncertainties?

Some specific comments

P3 When precipitation is measured by rain gauges, radars, or satellite sensors, the measured value is validated at the same time and location where it rains or snows. So is evapotranspiration by towers/satellites and soil moisture by probes/microwave sensors.

it is not fully equivalent to runoff. – altimetry is not even equivalent to discharge at the same point without an (uncertain) rating curve

p5 Equation 2 is NOT the St. Venant equations but a Diffusive Wave approximation to it.

Figures 4/5. The authors are overselling their technique somewhat here. The scale of the figures disguises the fact that the routing depends only on the distances from the outlet + the routing model parameters – which are assumed known and fixed. That means that points at similar routing distances from the catchment outlet will have similar routing characteristics. Thus the structure apparent in the figures is coming directly from the discharges at the gauge sites. Differences between pixels at similar routing times from the subcatchment outlets cannot be distinguished (this is part of the ill-posedness of the inverse problem. Thus very different visual patterns would result from the inversion if, for example, the number of gauges used was halved. Thus the authors suggestion of robustness here is not being defined with respect to the identification of actual patterns of runoff (and reproduction of the actual gauge discharges is being enforced without uncertainty). As in any inverse problems, yes you get an answer, but the authors are not giving any indication of the realism of that answer over and above being consistent with the gauged discharges that they assume known.

In fact, for some of these patterns, simply plotting an explicit fixed delay specific discharge in each subcatchment would probably not look so much different (without all the matrix inversions!!). I would suggest that the authors should add such a comparison to demonstrate how much added value there is in their method.

P20 For example, runoff fields inverted from the future SWOT mission can be used to identify and correct missing or overestimated precipitation estimates from the Global Precipitation Measurement (GPM) mission.

Surely not????? 1. SWOT will only give discharges with uncertainty. 2. Inversion gives only runoff generation, a further (highly nonlinear) inversion would be necessary to get back to rainfall. A Kirchner type inversion requires single valued storage-discharge functions. 3. Inversion adds uncertainty – will this be sufficiently constrained to be used for corrections? The authors should leave this out until it is proven useful.

Abstract and Conclusions: such studies are limited to scales where the spatial and temporal difference between the two can be ignored.

But as noted above, this is really only a way of distributing the specific discharge at a gauge according to some transformed distance scale / time delays. It does not properly distribute in space because it cannot differentiate between effective different time delays. It would be really interesting to see what effect this has at much smaller scales, when the time delay "bands" inferred by the method should be apparent.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6897, 2013.