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Discussions

Interactive comment on "HESS Opinions "Integration of groundwater and surface water research: an interdisciplinary problem?"" *by* R. Barthel

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This contribution discusses issues on coupling of surface and groundwater processes. The question raised in the title could be easily answered with a loud YES. So why to bother?

Well, things are not thus simple and this is why this contribution by Barthel is a valuable one. It is structured in 6 sections, where he first introduces the topic by contemplating on the separation of surface hydrology and subsurface hydrology and pointing out that the regional scale is the most relevant one. I agree in particular with this last point and I think this is the most discussable issue here, even more important than the coupling issue. Barthel explains why regional scale is more relevant than smaller spatial scales

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(regarding the management questions). However, he could add some lines why an even large scale (say national, continental or even global) is actually of minor importance. Maybe he was not aware that those scales are under consideration in some modelling studies, but they are. In particular interdisciplinary modelling studies which include effects of the biosphere or atmospheric changes (sometimes called Earth System Models) approach that scales. I'd be curious to know the opinion of him regarding these macro (or mega?) scales.

The 2nd section summarizes issues and difficulties regarding coupling at the regional scale regarding (experimental) field studies, modelling and management. The next section summarizes nicely differences in these two sub-disciplines of hydrology.

In general, I liked reading this opinion paper. However, I think it is not a scientific paper in a narrow sense, because it does not contain novelties or research results. But that is probably not the intention of Barthels (keeping in mind that is a HESS "Opinion" paper). I guess (but I am not sure) that it is more to boost discussion on these coupling questions and on the difficulties o language differences between these two sub-disciplines. If this is a suitable purpose of an "HESS Opinion Paper", I suggest to publish it in HESS (Mr. Chief Editor, please decide by yourself!!).

However, prior to publication a number of issues need to be revisited, i.e. I agree with many points raised in this paper, but I also have some critiques:

Page 2022, 1st paragraph: you say a groundwater hydrograph can "be completely dominated by low flow frequency . . .", and explain this as a sign of major difference to surface hydrology. Well, the issue here is less the difference between surface/subsurface hydrology rather than the processes governing this behaviour. For example there can be surface hydrographs (both water-level or discharge time series) which show no distinct seasonality or where the seasonality is superimposed by other processes, e.g., runoff hydrographs in lowlands, where backwater effects can be important or runoff hydrographs in tidally influenced regions. On the other hand, there also can

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be groundwater-level time series which show a very clear seasonality.

Page 2022, 2nd paragraph: the differences for risk assessment between surface and subsurface risk are not THAT different. For instance: you cannot directly relate river discharge to hazard (water level, yes) of inundations. Even water level of a river needs to be translated to an inundation depth at each particular object exposed to flood risk. That means there is a general requirement to transfer the values of both surface and subsurface variables to the particular object at risk (depending on the object type and the potential hazard type).

Page 2022, 3rd paragraph: I also disagree, that in surface hydrology the "most important water . . . issue" is the prediction of discharge or floods. Instead, there is not such a thing as the "most important issue". The importance is strictly related to the purpose of the study: flood and low flow studies need to know discharge, right. However, irrigation requirement studies need to know water volume stored in reservoirs, regional hydro-climate interaction studies need to know evapotranspiration (and energy fluxes), etc. Thus, it is the purpose of the study that matters. And this is similar in both subdisciplines.

Page 2023, 1st paragraph: you state that surface models are "more conceptual and statistical based" compared to subsurface hydrology. I think in surface hydrology, both model types are developed and applied. In addition, you state that: "water going into a catchment ends up at a gauge. . ." That's not right. It also goes into the atmosphere (ET)! Don't ignore this !

Page 2024: the discussion if surface hydrology models have parameters which are not related to measurable physical or chemical properties is a rather old (but endless) one. I don't want to follow this line. But you should be aware that the subsurface model parameters are often also "conceptual" in the sense that they need calibration, e.g. it is no really possible to "measure" transmissivity .

Page 2026: You state that fully coupled models are feasible but difficult to implement.

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This difficulty is mainly to the usual constraints of labour and data scarcity. But this is only a practical (but still very important) problem. I see a more principle question in "when should one couple a surface with a sub-surface model"? and "to what degree (one way, two way ...) is a coupling useful?". There is no generally valid answer to this question. Instead this depends on the type a system under consideration (i.e. how 'strong' is the coupling) and – again – on the purpose of the study. Please look into the book of Bronstert et al. (2005) for further discussion on coupling issues.

Page 2027, line 17: insert "be" where appropriate.

Reference: Bronstert, A., Carrera, J., Kabat, P., Lütkemeier, S. (Eds.) (2005): Coupled Models for the Hydrological Cycle. Integrating Atmosphere, Biosphere and Pedosphere. Springer Publishers

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