Interactive comment on "From Engineering Hydrology to Earth System Science: Milestones in the Transformation of Hydrologic Science" by Murugesu Sivapalan

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General

I really enjoyed reading Siva's narrative about the transformation of hydrology from an engineering discipline to an earth system science. I also agree with the successive steps and failures that were mentioned: building up case by case from Newtonian Mechanics, looking down at multiple catchments using Darwinian approaches and the way they could be married to arrive at underlying principles and moving from prediction to understanding.

Author's response:

I am delighted to receive this positive and supportive review by Marc Bierkens. I am especially relieved that Marc saw it exactly as I intended the reader to see in my paper.

However, this narrative is somewhat biased towards surface and catchment hydrology. In other hydrological disciplines, concepts and modelling efforts have been tailored to understanding the underlying mechanisms of observed phenomena from the start. This should be acknowledged. This is particularly the case for groundwater hydrology and vadose zone hydrology. Examples of the former are the explanation of macrodispersion by considering flow in a heterogeneous medium (e.g. Gelhar et al. Water Resour. Res., 15, 1387–1397, 1979; Gelhar and Axness, Water Resour. Res., 19, 161–180, 1983). An example of the latter is the explanation of hysteresis of the soil water retention relationship and all its wetting and drying scanning curves by extending Darcy's law with a term that accounts for the change in time of interfacial area between air and water in the pores (Dynamic Effect in the Capillary Pressure–Saturation Relationship and its Impacts on Unsaturated Flow (Hassanizadeh et al., Vadose Zone Journal 2002 1: 1: 38-57). These approaches use Newtonian mechanics and Thermodynamics, but interactions with vegetation have been included more than a decade ago to understand e.g. root growth processes and root water uptake. In fact, the problem in stochastic subsurface hydrology seems to be the reverse: how can these theories designed to understand phenomena be used in practice, i.e. prediction (see e.g. the recent debate on this subject in Water Resources Research: Rajaram, Water Resour. Res., 52, 9215–9217, 2016; and related papers).

Author's response:

I will be the first to admit that my narrative is biased towards surface and catchment hydrology, and so Marc is right here, and I will make a brief clarification on this point in the paper. While it is tempting to respond to the remainder of Marc's comments here, I am afraid such a response will only cause confusion to the reader of my paper. They may be reserved for a similar historical review of groundwater/vadose zone hydrology by a specialist in these fields. Suffice it to mention that my vision for the future of catchment hydrology includes aspects of vadose zone hydrology and groundwater hydrology included as part of the whole. Notions such as phenomena and understanding take on different meanings in this holistic view, different from the ones Marc mentions.

I will however mention these two examples of phenomena in groundwater transport and vadose zone hydrology in Section 5 addressing a change of focus towards phenomena.

Apart from that, I generally agree with the narrative and the conclusions drawn from it.
I have just some additional specific remarks that may provide additional perspectives.

Author's response:

Thank you for these insightful comments. I have addressed them as well as I can, with the only consideration that I do not want to cause distraction to the overall narrative.

Specific remarks

Lines 180-182: Examples of meso-scale distributed models: I think that it is fair to refer to the codes that have been used more extensively at even larger scales and are most well known in the literature: PARFLOW, Cathy and Hydrogeosphere.

Author's response:

OK, I have included some of these, although I did not intend this to be a comprehensive list, only those I know that have been applied to large catchment to regional scales.

Lines 315-330 REW. Despite the examples given at the end of this paragraph, I don't think that REW really took off and is used much in practice. The problem is the estimation of the parameters of the constitutive relationships. These still need to be calibrated and observations are generally lacking to perform this calibration. I feel it is fair to mention this.

Author's response:

This is fair criticism. I thought I mentioned it clearly in the paper, but I have made it even more explicit in my revision. In fact, it is not the parameters, but the constitutive and closure relationships which need to be derived everywhere – this requires an organized effort, barring which people use parameters derived at other (smaller) scales, or continue to use constitutive relationships that are only applicable at the point scale.

Line 484 "If the biological or ecological laws were known: "But are they not known in principle? Just as one should be able to derive Darcy's law from Newtonian mechanics (Navier stokes) and the functioning of a linear reservoir from groundwater theory*, by the same reasoning

ecological laws in the end could be derived from natural selection (with the added complexity that individuals are able to influence their own environment). If this is indeed the case, then with sufficient computational resources and individual-based ecosystem models we could derive these laws for catchments in different climates and geological settings by exhaustive simulation.
*I refer to much earlier work from Kraaijenhoff and van de Leur (1957) and De Zeeuw (thesis, 1966) on solving the linear Boussinesq equation showing when a groundwater reservoir response is linear –> for later times after rainfall and always if stream entrance resistance is high).

Author's response:

Thank you for these queries, and there is a lot to unpack here. First of all, in my view the kind of simplicity that one finds when one can describe a groundwater aquifer as a linear reservoir, does not come about by some kind of mathematical manipulation of some governing equation for homogeneous systems. The simplicity is potentially a result of the heterogeneity of the aquifer and history of how the heterogeneity may have come about, which is traced back to the co-evolution of the system. My argument is that if one looks at the system in more holistic ways, then one may find a simpler theory to describe the phenomenon. I am convinced that the practical solution is through recourse top-down data based discovery at the scales of interest to us (i.e., catchments), and the old-fashioned scientific method, not the bottom-up simulation way Marc is suggesting.

Lines 645-646: It seems that the large number of modelling approaches is perceived as a problem in Hydrology. But why? Let's compare this to the field of ecology. Here, we also have a plethora of modelling approaches and partial theories used to explain a huge diversity of interacting life forms. The underlying theory in the end is evolution (diversification by mutation and crossing and selection). This insight has been around for a long time, but it has not stopped ecological model and theory development, because at ecosystem scale, emergent phenomena can be better explained with macroscopic models. The latter is also not perceived as a problem in ecology.

Author's response:

In catchment hydrology, this question is connected to the age old debate between *uniqueness of place* and *generality of place*. Juxtaposition of Newtonian mechanics/thermodynamics on the one hand and evolution on the other dictates that reality is in the middle somewhere. So in this sense, plurality of modeling approaches is entirely appropriate (as we also decided during the PUB initiative), which I reinforced in this paper. Yet, it is a problem if we decide on a unique model for every catchment – because it works against unification of the field. This is why there is so much focus on hydrologic similarity, so that it can help us to find common models and modeling approaches for hydrologically similar catchments. This is probably similar in ecology as well. I believe that this is already well articulated in the paper so I decided not make a specific change in the revised manuscript.

Lines 882-884. The Darwinian approach to discovering underlying principles or laws. But in truth: what other laws than Newtonian mechanics, thermodynamics and evolution theory will we need to explain observed phenomena?

Author's response:

At a fundamental level, yes, I can agree that Newtonian mechanics, thermodynamics and theory of evolution is all that underpins all of science. But then this is true of all science – the only question is how do we adapt or use these to benefit us in hydrology, at the scales of interest to us. There is still a lot of freedom of choice and also limitations by the scale of observations, the information and data we have available to us, and practical considerations such as the need for predictions. I could not find a way to include this point without confusing the reader, so in the end I did not make a change to the text. Hope this is OK.

Lines 1015-1017: Horton's statement. This is indeed a good foresight that can also be explained from ecological niche theory: In case of shortage, any water that cannot be used by the prevailing species is likely to be used (forms a niche for) other species that can use it.

Author's response:

This is an interesting observation, even though this is beyond the scope of the paper. I have added a note to make a connection to ecological niche and niche construction theories in ecology, for the benefit of readers who may go into this in more depth.

Lines 1030-1032. The optimality principle. I find this questionable as a general principle and it has not been embraced by ecologists for a reason. The optimal vegetation is the one that over the long run has the highest probability of survival. In functional ecology this is better explained in terms of optimal traits (leaf form, stomatal density, color, size) of which size (maximal carbon gain) is but one.

Author's response:

I completely agree. Maximization of net carbon profit is only presented here as an example. I have clarified this point now without going into more details, as it might be distracting to the overall storyline.

Lines: 1059: Humans as intrinsic part of the catchment's cycle: if humans are considered, the catchment is not the most natural unit of description anymore. It is the overlay of river basin (regional size catchments – as also confirmed hereafter) and administrative (or governance) units that are of interest. Different impacts are also in different parts of the basin: land use change and dam building in the upper reaches; water abstraction in the middle and lower reaches.

Author's response:

This is a very good and important point. I overlooked to mention it in the paper, an error I will now rectify. The more general point that with the inclusion of humans and their interferences in the water cycle, traditional definitions of catchment boundaries and movement of water must give way to include human oriented administrative units and transfers of water through human generated water conduits (including virtual water transfers).

Figure 7 and the arguments explaining the differences between natural and human influenced water ecosystems: I am not wholly convinced that this is universally true. It may well be a time-scale thing. If we look at a natural ecosystem, it is often evolving (in succession) where pioneers are eco-engineers making the bare soils suitable for other species to move in that in the end outcompete them. Sometimes, even lock in occurs. Think about birch trees in marshes that establish themselves during relatively dry periods and due to their evaporation are able to keep water tables deep enough for root aeration to occur even in wetter years. However, if some of the trees die (because of old age or two consecutive very wet years) creating a holes in the canopy, groundwater recharge may suddenly increases tremendously causing all the trees to die as a result of it (Brolsma et al., Ecological Modelling, Volume 221, Issue 10, 24 May 2010, Pages 1364-1377, 2010). So, vegetation may obey optimality principles on long time scales (or in climax state), but not on smaller time-scales. Reversing the argument, one might view the non-optimal or detrimental (lock-in) interactions of humans with their environment temporary and expect human-water systems at longer time scales to also show signs of optimality.

Author's response:

I presume that Marc is raising this point as a matter of principle, and I too agree in principle, and I have made a brief note in the revised manuscript. However, there is going to be a lot of argument and disagreement, and I reproduce a quote from an earlier paper by Sivapalan and Blöschl (2015) on the analogy to the vexed disagreement between stochasticity and determinism, and a useful quote from Kolmogoroff: ''*The possibility of using, in the treatment of a real process, schemes of well determined or of only stochastically definite processes stands in no relation to the question whether the real process is itself determined or random'' [Kolmogoroff, 1931, p. 417]. Clearly, whether human behavior is random (irrational) or deterministic is not a scientific question. Rather it is a pragmatic modeling choice that will (usefully) depend on whether we see clear patterns with the amount of information available. Coevolutionary models may therefore account for human behavior by a stochastic treatment*."

Lines 1126-1128: being Dutch I cannot resist to point out a more recent historical example of this lock in. I am referring to the famous Dutch polders. Many of the low lying areas in the Netherlands are the result of draining peaty soils (since the 11th century) which resulted in land subsidence and subsequent further lowering of the groundwater level to keep the soils dry leading to further subsidence etc. The result is 30% of the country lower than sea-level. An interesting by-product is the invention of Gouda cheese. The low-lying areas were only suitable for dairy farming. To save the excess milk for winter time, cheese was made out of it (see:

Erkens, G., van der Meulen, M.J. & Middelkoop, H. Double trouble: subsidence and CO2 respiration due to 1,000 years of Dutch coastal peatlands cultivation. Hydrogeol J (2016) 24: 551. https://doi.org/10.1007/s10040-016-1380-4).

Author's response:

Thank you for very much for this observation. It is an excellent example of a "lock in" phenomenon. I have included it in the text, since the concepts of lock in or path dependence are more easy to understand through this example.

Line 1301: regional hydrology. It seems you mean that the river basin would be the object of study. This is the old focus of classical Physical Geography, studying earth surface processes and their interaction with the atmosphere, the subsoil and life (including man) from sink to source, from the mountains to the sea, from headwaters to the delta.

Author's response:

I agree with this point, but want to highlight three subtle differences. Firstly, the title I used was regional process hydrology (not just regional hydrology). Secondly, the scale here is larger than the river basin, a region that represents the land area between the mountains (water towers) and the oceans at the downstream end. Thirdly, I argued for recognition of phenomena at this scale and their interpretation/explanation in terms of regional scale processes and process interactions and feedbacks. My argument is that the scale of interest of catchment hydrology must expand to include regional scale processes. I have added test to bring this out better.

Lines 1365-1366 future earth system hydrology: The crystal ball of Bierkens (2015) came to a similar conclusion.

Author's response:

Thank you for reminding me of this paper. I will now cite this reference to add strength to my argument.

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