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Due to time constraints I almost exclusively focused on the revised section of the paper, so I may have overlooked some things and therefore request clarifications that are already there. The paper improved notably, but I find some things in the new theoretical section that I believe warrant attention. I explain these in detail (and offer an alternative equation and its discussion) below.

l. 24: 'Both study areas...' At this point you have not mentioned any study area yet.

l. 44-45: This is the special case of no flow, which is approximated in some locations during limited periods of time. Steady-state conditions also arise when there is flow, as long as the flow field does not change. Unit gradient flow is an example of such a steady flow, and one that is probably more abundant than hydrostatic equilibrium, because it tends to occur below the root zone in deep vadose zones. In semi-arid areas there is some evidence (although I cannot produce references) of unit gradient conditions prevailing for many years.

In the wording of the line of argument you develop in l. 48-50, unit gradient flow is a condition in which the capillary gradient has vanished, and the flow is purely controlled by gravity. This does not mean the influence of capillarity can be neglected. On the contrary, the capillary forces determine the water content and the matric potential under which the unit gradient condition will arise, given the steady water flux that results from the long-term average of the net infiltration. I would therefore argue that unit gradient flow presents a case where the interplay between gravity and capillarity has found a balance, and therefore can be invoked to support your argument.

l. 54: I find geology to be the odd one out. Tectonic uplift keeps up with erosion, or even outruns it. Therefore, the existence of mountain ranges or smaller geological features is not really co-evolutionary. Pedogenesis and geomorphology (including incision of rivers, mudslides, and everything else that is driven by (partially subsurface) water), etc. therefore are processes that respond to the geological drivers. I do not see any feedback into the geological processes that the term co-evolution suggests.

Another issue is the difference in time scales of geology and the other processes. Many of the lower mountain ranges in the world (Ural, Appalachians, German Mittelgebirge) are remnants of the large mountain range of Gondwanaland. It is difficult to argue that these ranges co-evolve with soils that developed in the Holocene. A more hydrological example of lack of co-evolution is provided by the aquifers below the Sahara and the Saudi-Arabian desert that are remnants of less extreme climatic conditions and have little or no bearing on the processes in the top meters of the subsurface. This water is fossil, and is therefore sometimes considered geological in nature, unconnected as it is from the current hydrological cycle, were it not for anthropogenic interference through pumping.

l. 113: New paragraph?

l. 119-128: I like this paragraph - it clearly outlines what we can and cannot expect when we travel down the thermodynamical avenue.

l. 135-136. ...energy is an additive quantity, while .. gravity and matric potentials are not.

This needs some clarification: we can and do add the gravitational and the matric potential all the time.

l. 143: ...optimizes...infiltration, moisture retention and drainage of catchments.

The question how and through what mechanism this optimum is defined. In more plain terms: what is a catchments' objective function, and how did the catchment find it?

But you do not need to go into that in this paper.

l. 169: The volume  $V$  for which you define the Gibbs free energy contains three phases, but you leave out the gas phase. In Eq. (2), the work term can be set to zero for the solid and the liquid phase because they can be considered incompressible. But there will be a non-zero contribution to this term for the gas phase.

Perhaps it is easier to formally limit the analysis to the water phase only of your control volume. The work term in Eq. (2) is then multiplied by theta and subsequently declared to be zero because water can be considered incompressible for normally encountered pressure ranges.

Below Eq. (1) and possibly elsewhere there are many inconsistencies in the math. The notation of units sometimes uses the division symbol ('/'), sometimes negative powers. The same variable appears in normal and in italic font, or even in upper and lower case (Gibbs free energy). This is confusing.

l. 175: The constant  $g$  does not denote the acceleration of the planet itself, but that generated by its gravitational field.

l. 195-201 (Discussion of the meaning of potential): I belong to the category of people that were taught to use the term potential for every quantity of which the gradient drives a flux of some sort, and for which the flux is proportional to that gradient. The resulting flow is termed a potential-driven flow. If the potential is a voltage, Ohm's Law emerges. If it is a temperature, Fourier's Law of heat conduction arises. In case of a solute concentration, Fick's Law of diffusion appears. If we have a hydraulic potential, we arrive at Darcy's Law.

Within the Darcian framework, the potential energies derived from the position in the gravitational field, from the position in the pore architecture that gives rise to the matric forces, from the osmotic potential derived from the presence of solutes, etc. are all considered energies that are additive. They all can perform work, so the term potential energy seems justified. The gravitational field stands out because it is independent of any property of the soil system, such as the amount of water present in the soil, the architecture of the pore space, etc. The other force fields are dynamic and influenced by the system of which the water upon which they operate is a constituent, but they are considered to exert a force on the water, just like gravity.

That being said, feel free to keep this explanation in the text. This allows both positions to be debated in the literature in the open, which is the proper way.

Eq. (4): I think you should use the general version, with the two principal radii of curvature. The limitation to cylindrical pores is neither desirable nor necessary here.

Eqs. (3) and (5): In Eq. (3) you only considered a change in the matric potential and gravitational potential energies, while keeping the water content constant. The terms  $d\psi$  and  $dz$  reflect infinitesimal changes in the matric potential and position in the gravitational field. The term  $dz$  is intuitively clear. The term  $d\psi$  is harder. The only way I can think of changing the matric potential in an isothermal system where the properties of the liquid and the solid phase do not change is through the pressure of the gas phase without an equal change in the atmospheric pressure. Thermodynamically, the term with  $d\psi$  in Eq. (3) makes sense, but it is not so easy to find a physical mechanism to create the infinitesimal change at constant water content.

In Equation (5) you keep the position  $z$  constant and do not permit a change in the matric potential energy when neither the water content nor the position is changed. Equation (5) therefore is not the derivative of Eq. (3), contrary to what the paper states. Instead, by replacing  $dz$  and  $d\psi$  by  $z$  and  $\psi$  you do not permit the position of the water in the gravitational field and its elusive equivalent for the matric potential field to change. Instead you change the water content by an infinitesimal amount and show how it affects the potential energy of the water. This is much easier to interpret than the term  $d\psi$  in Eq. (3) because it is immediately obvious that the matric potential changes with the water content. The change in gravitational potential energy is also clear.

I believe the first term of the right-hand-side of Eq. (5) is incorrect though. Let me explain by carrying out the derivation operation on amounts of potential energy stored in a volume of water that is subjected to a small change. The volume of water is that in an arbitrary volume  $V$  with volumetric water content  $\theta$ . Without loss of generality, the units of  $V$  are chosen such that the volume of water within it constitutes one arbitrary unit of volume. The amount of matric energy in that volume is then equal to  $\rho g \psi \theta$ , and the amount of gravitational potential energy equals  $\rho g z \theta$  ( $\text{kg m s}^{-2}$ ), consistent with your notation. We now add an infinitesimal amount of water  $d\theta$  in an infinitesimal time interval  $dt$  and calculate the resulting change in both potential energies:

$$\rho g \frac{\partial(\psi\theta)}{\partial t} + \rho g \frac{\partial(z\theta)}{\partial t} = \rho g \left( \psi \frac{\partial\theta}{\partial t} + \theta \frac{\partial\psi}{\partial t} + z \frac{\partial\theta}{\partial t} + \theta \frac{\partial z}{\partial t} \right) = \rho g \left( \psi \frac{\partial\theta}{\partial t} + \theta \frac{d\psi}{d\theta} \frac{\partial\theta}{\partial t} + z \frac{\partial\theta}{\partial t} \right)$$

You omitted the first term within the parentheses, which represents the change in the amount of matric potential energy caused by the change in the amount of water. Note also that the use of partial derivatives in the  $d\psi/d\theta$  is incorrect because you assume that the function  $\psi(\theta)$  completely describes the behavior of  $\psi$ . This assumption is necessary and sufficient to allow  $\partial\psi/\partial t$  to be expressed as the product of  $d\psi/d\theta$  and  $\partial\theta/\partial t$ , so this is not trivial.

With this expression you can nuance the statement in l. 218-220. Only when  $d\psi/d\theta = 0$  (under saturation or complete dryness, the latter being of little interest in a hydrological journal) are the changes in matric and gravitational potential energy equal (and of the

same sign). With  $d\psi/d\theta > 0$  in hydrophilic soils, the product  $\theta d\psi/d\theta$  determines by what additional amount the potential energy in  $V$  changes when  $\theta$  is changed by an amount  $d\theta$ .

The statement in l. 221-223 is incorrect in two ways. The sum  $(\psi + z)$  denotes the potential energy of the water around a point where the values of the matric potential and  $z$  are as indicated, but only there. Multiplying this sum by the water content denotes the potential energy contained by the water in a small volume element of bulk soil surrounding that point. For larger volumes, you need the volume integral of this sum. This amounts to the sum of Eqs. (8) and (9) in de Rooij (2009), but without the division by the total amount of water in the integration volume. Section 2.3 of de Rooij (2011) addresses this issue more thoroughly by presenting formal definitions that conserve energy during upscaling operations, and also explains why the intrinsic phase average is more elegant than the phase average. Because Eq. (5) refers to a single point, this is not relevant at this point, but as soon the analysis moves to larger scales this aspect becomes important.

l. 239-241: The equation is valid irrespective of the value of the integration constant, which only reflects the reference height for the vertical coordinate. You are working in catchments with varying groundwater levels in space and time, so I do not think it is wise to fix the reference height to the groundwater level at an arbitrary point and an arbitrary time, which is what you do when you fix it to 'the groundwater level'. If you really like the Gibbs free energy to go to zero it is more correct to state that you set  $c$  to zero without loss of generality because it reflects the vertical position with respect to an arbitrary datum.

l. 242-243: I recommend to include a remark that this approach assumes hydrostatic equilibrium with a fixed groundwater level in the entire unsaturated zone, for the non-soil physicists that read HESS.

l. 264-265. I associate storage with a certain depth interval (e.g., the entire unsaturated zone). That would be equal to the integral of the water content over that depth interval. But here you use it for the water content at the top of the interval only. Why?

l. 277-278. Not only do you assume capillary contact with the groundwater, but you assume hydrostatic equilibrium throughout the profile. Capillary contact with the groundwater will be there as long as the water does not retract into pendular rings. It will simply not play much of a role higher up in the profile. This makes the assumption of hydrostatic equilibrium a rather strong one.

Figure 2. The term 'water stock' is definitely misleading here. You only concern yourself with the water in a plane at a given height above the water table, not the water below and above that plane. For that you need volume integrals. See de Rooij (2011) for the underlying theory, including the effect of the geometry of the volume of interest.

At degrees of saturation of about 0.05 (Colpach), 0.35 (Weiherbach) and 0.72 (Wollefsback) the gravitational potential contributes about 1% to the total free energy for the chosen depth to groundwater, judging from Fig. 1. From there on, Fig. 2 basically is the retention curve with the logarithmic axis replaced by a linear axis. What worries me about this figure reflects what worries me about Fig. 5: the changing amount of

water with changing matric potential is not taken into account at all. From a catchment-scale perspective this is really dangerous – you cannot really tell much about the energy status of the catchment water if you do not weigh the local energies with the local water contents. We are back to the proper way to carry out volume integrations again. This plays into the discussion at line 300, where you use the term energy deficit. But you cannot quantify this correctly because you are only able to determine the deficit of potential energy per volume (or weight) of water without being able to see the difference in volumes water at the current non-equilibrium state and the equilibrium state. But we can do that already with the pF curve, we do not need the free energy for that.

l. 301. You use the term ‘dry cohesive soils’. Why does the soil need to be cohesive for the rapid deviation from equilibrium to occur? Also, you discuss excursions away from and back to equilibrium. The soil cannot be that dry away from equilibrium, can it? Or are you talking of sands at pF 3 (consistent with 10 m deep groundwater)? In that case, the assumption of hydrostatic equilibrium with the groundwater table is illusionary. More generally, you can argue that the sensitivity to perturbations is determined by  $d\psi/d\theta$ , which typically is very large near saturation and in the dry end.

l. 304-305. The grammar of this sentence seems to be wrong, or perhaps there is a devilish typo.

l. 304-311. The soil’s behaviour will vary dramatically with the chosen reference matric potential (because that is what you fix when you set a depth to groundwater in combination with the requirement of hydrostatic equilibrium).

l. 318-319. This is the case for draining rivers. For rivers that lose water,  $c$  is larger than 1. This becomes relevant when there are ditches instead of rivers and water is let in during summer to water the soils.

l. 383: the pF requires the absolute value of  $\psi$ .

l. 388: To arrive at the stored water amount in a landscape for a given tension you need to multiply the average water content by the volume of the portion of the landscape to which the tension applies.

l. 413: a pore space of less than 20 meters? I do not understand.

Figure 5. Are the energy state functions based on a single groundwater depth again? If so, what was this depth, how was it selected, and how representative are these curves for the catchment in view of my comments above?

I do not fully grasp the vertical scales of the figures. Panel b shows that Colpach has depths to drainage between 2 and 56 m or so (I can hardly see the tick marks of the graph). You plotted the free energy between -10 and 30 m, so there seem to be about 15 m of the total range missing. If the range of panel a is more or less centered on the range of HAND values, this would lead to a reference depth to groundwater of roughly 20 m (when the range in panel a covers 10 to 50 m of HAND values). This does not seem to be representative at all of the distribution of HAND in the catchment.

For Wollefsbach, HAND ranges from 1 to 33 m, yet the range of the free energy is 80 meters. I have no idea how to interpret this or speculate about the chosen reference depth to groundwater.

Figure 6. Does the range of free energy in Wollefsbach reflect considerable drying in summer? Does that not invalidate your assumption of equilibrium with a ground water table that cannot be that deep according the reported HAND values for that catchment? Because the curve of the free energy against degree of saturation increasingly resembles the non-logarithmic retention curve I can imagine this does not matter too much, but it should perhaps be discussed.

You report alternative values of the depth to groundwater, so I assume I overlooked the best values (I only reviewed the changes, because of time constraints). Do you explain how you found these? Neither value for Colpach seems to match the HAND histogram in Fig. 5, and you do not indicate the values for Wollefsbach.

l. 531 ... large values of HAND...?

l. 686-688: I did not see much evidence for a linear dependence of the free energy on the degree of saturation (nor did I expect it). Please elaborate.

I would like to have some clarification on the determination of the depth to groundwater that separates the wet and the dry branches of your curves.

## References

de Rooij, G.H. 2009. Averaging hydraulic head, pressure head, and gravitational head in subsurface hydrology, and implications for averaged fluxes, and hydraulic conductivity. *Hydrol. Earth Syst. Sci.* 13:1123-1132. [www.hydrol-earth-syst-sci.net/13/1123/2009/](http://www.hydrol-earth-syst-sci.net/13/1123/2009/)

de Rooij, G.H. 2011. Averaged water potentials in soil water and groundwater, and their connection to menisci in soil pores, field-scale flow phenomena, and simple groundwater flows. *Hydrol. Earth Syst. Sci.* 15:1601-1614. doi:10.5194/hess-15-1601-2011