Reply to re-review of Gerrit de Rooij (GR)
January 2019

GR: 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.

EZ: I read Gerrit de Rooij's assessment of our revised manuscript with great interest and pleasure and thank him again for his efforts and thorough reflection. Please find my detailed answers below.

GR: l. 24: 'Both study areas…' At this point you have not mentioned any study area yet.
EZ: This is reformulated in the revised manuscript.

GR: 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.

EZ: This statement is indeed unprecise as a steady state does not necessarily correspond to hydraulic equilibrium conditions. We thus change the statement to: "Steady state, hydraulic equilibrium conditions imply"….

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.

E Z. Good point! What we in fact mean is that the geological setting constrains the evolution of the soil material. We changed the wording accordingly as follows: 'The climatological and geological setting constrains the co-development or co-evolution of soils, geomorphology and vegetation (as suggested by e.g. Troch et al., 2015; Sivapalan and Bloschl, 2015; Saco and Moreno-de las Heras, 2013). One might hence wonder whether this constrained co-development created a distinctly typical interplay of capillary and gravitational controls on soil moisture'

GR: l. 113: New paragraph?
EZ: done

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

GR: 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.

EZ: Agreed! In precise terms energy is an extensive quantity/state variable (such as mass, momentum, entropy, electrical charge), while potentials are intensive state variables (such as temperature, pressure, velocity, chemical potential). Extensive state variable are discontinuous an interfaces and they grow in an additive manner when the volume of a system is enlarged or the two systems are merged. Extensive variables are thus stock variables that can be balanced. Intensive state variables are continuous at interfaces and are not additive in the above explain sense. If we open the door between two rooms of the same temperature, temperatures don't add up (thermal energy does though). We change the wording as follows: "Secondly, energy is an extensive quantity, as such it is additive when different systems are merged, it grows with increasing system size and changes can be described through a balance. One may hence apply volumetric averaging and upscaling to energy for instance to derive macroscale effective constitutive relations and macroscale equations as shown by de Rooij (2009, 2011). In contrary the related gravity and matric potentials are intensive state variables and as such neither additive in the above specified sense, nor can their changes be balanced."

GR: 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.

EZ: I absolutely agree that this is the non-trivial to define such an optimum. The system could reach such an optimum through "mutation and selection". So this is not a target process but it implies that landscapes which do not develop are less resilient against disturbance as those how are in an optimum.

GR 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.

EZ: I agree that the work term cannot be neglected for the air phase. And please note that we state that we neglect the work term, because we focus on free energy of the water phase, right below equation 2. In the revised manuscript we additionally state this in brackets

GR: 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.

EZ. I apologize for this and we fixed this in the revised manuscript.

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

EZ. I apologize for the denglish. The german term is "Erdbeschleunigung" and I just did a word for word translation. We changed it to gravitational acceleration

GR: 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.

EZ: Good point and fair enough. I was taught that potential energy relates to the position of a test body in an elementary force field: either a test mass in the gravity field or a test electrical charge in the electrostatic field. Please note that we use the term chemical energy although they relate to the product of the chemical potential and the mass.

GR: 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.

EZ: done.

GR: 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 dp and dz reflect infinitesimal changes in the matric potential and position in the gravitational field. The term dz is intuitively clear. The term dp 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 dp 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.

EZ: The confusion is because of the fact that we have been moving away from the notations of the old masters. Neither equation 1 nor equation 3 is a total differential in the mathematical sense (I remember we stated this in a foot note in the previous manuscript, which dissipated during the revision). This is why classical text books on thermodynamics use the symbol δ instead of the d, and speak of a variation, which implies that the other factors in the product remain constant. (We added this note to the text).

$$\delta g_{\text{free}} = \rho g \theta \delta \psi + \rho g \theta \delta z = \frac{\partial g_{\text{free}}}{\partial \psi} d\psi + \frac{\partial g_{\text{free}}}{\partial z} dz$$

The fact that this is possible is usually attributed to the fact that the system is in contact with “reservoirs”. The most classical example is the heat reservoir, which may release thermal energy without changing its temperature. Honestly, I had always problems in imagining this as student. And it becomes more difficult to imagine when moving away from technical systems to natural system, as fluxes feedback on their driving boundary conditions and “the reservoir idea” becomes in most cases meaningless. In case you are interested, you may look at the my discussion of Zehe et al (2013) in HESS. In the revised manuscript we now state that Equations 1 and 3 are not total differentials.

GR: Equation (5) therefore is not the derivative of Eq. (3), contrary to what the paper states. Instead, by replacing dz and dp by z and ψ 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 that the term dp 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 θ. 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 than equal to ρgψθ, and the amount of gravitational potential energy equals ρgzθ (kg m s^{-2}), consistent with your notation. We now add an infinitesimal amount of water dθ in an infinitesimal time interval dt and calculate the resulting change in both potential energies:

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

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 ψ. This assumption is necessary and sufficient to allow $\partial\psi/\partial t$ to be expressed as the product of $d\psi/d\theta$ and $\partial\ \psi\ /\partial t$, so this is not trivial.

EZ: Touché! To save our honor, we used the correct version proposed by GR in the study of Zehe et al (2013), in the psi based form though (Equation 7 in Zehe et al. 2013). We changed equation 5 in our manuscript accordingly. But note that this is generally not as clear as one might think. We come to the equation proposed by GR if the equation $g_{free} = \rho g\theta\psi + \rho g\theta z$ is correct, and we do believe that this is the case. But this must not be the case for all forms of energy.

What I learnt about the Gibbs fundamental theorem is that we generally **cannot** quantify the total energy content of a system by summing up all the energies, but that we can only quantify the changes in such an additive way as described by this form. This is because the total energy amount can only be quantified for a few energy forms but not for all (through their Gibbs function). We can do this for potential energy, kinetic energy, electrical energy. We can’t do this for surface energy, chemical energy and or heat. We express the change in heat/thermal energy as $dQ=TdS$ ($\delta Q=T\delta S$), but $Q=TS$ is wrong! We may express the change in chemical energy $\delta E=\mu\,\delta M$ is correct but $E=\mu\,\delta M$ etc.

GR: 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 $d\psi/d\ d\theta > 0$ in hydrophilic soils, the product $\theta\ d\psi/d\theta$ determines by what additional amount the potential energy in V changes when θ 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.

EZ: $(\psi + z)$ is the free energy density in the water phase at a point or in the wetted part of an infinitesimally small volume consisting of water only. $\theta(\psi + z)$ is the free energy density normalized with the entire volume. I think we stated this clearly and we stress that this is the free energy <u>density</u> at elevation z. For larger volumes you need to integrate $\theta\ (\psi + z)$, because the volume is only partly occupied by water. Otherwise this violates energy conservation as correctly stated by de Rooij (2009).

We changed the entire passage GR is referring to as follows:

$$\frac{\partial g_{free}}{\partial t} = \frac{\partial\left(e_{pot} + e_{cap}\right)}{\partial t} = \rho g\left[(\psi + \theta\frac{d\psi}{d\theta})\frac{\partial\theta}{\partial t} + z\frac{\partial\theta}{\partial t}\right] \text{ Eq. (5).}$$

Note that the potential energy density of soil water (the second term on the right hand side) increases linearly with increasing soil water content. On the other hand, capillary binding energy decreases with increasing soil water content, as the absolute value of the matric potential declines non-linearly with increasing soil water content. The change in capillary energy density with a given change in soil water content is determined by the product of the actual soil water and the slope of the water retention curve. We thus state that the product of the well-known soil hydraulic potential and the soil water content corresponds to the volumetric density of free energy of soil water per unit weight. The free energy of soil water for a larger volume is the volume integral of the total hydraulic potential times the soil water content over the volume of interest (de Rooij, 2009; Zehe et al., 2013):

$$E_{free} = E_{cap} + E_{pot} = \int \rho g\left(\psi(\theta) + z\right)\theta dV \text{ Eq. (6)}$$

The latter reflects both the binding state and the amount of water stored in a control volume above groundwater and thus reflects the local retention properties and the topographic setting as well. Note that the change in potential energy of soil water at a given elevation scales linearly with the soil water content. One might thus wonder whether the dominance of the one or the other energy form may at least partly influence whether a system behaves in a linear or non-linear fashion.

GR: 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.

EZ: I learned that an indefinite integration adds a constant to antiderivative and that this constant is determined at the system boundary. I do not see any reasons for setting c to zero expect the one that the matric potential becomes zero at the groundwater surface. This is now stated in the text.

GR: 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.

EZ: To be precise, we assume hydraulic equilibrium within the entire saturated zone. This is stated. The groundwater level does not need to be fixed, a declining or rising GW level will change the equilibrium profile.

GR: 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?

EZ. I removed storage here.

GR: 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.

EZ: Correct and this is clearly stated at the beginning of the section.

GR: 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.

EZ: I agree with GR that figure 2 relates pretty strong to shape of the retention curve. Yet it is different as $e_{free} = \theta (\psi + z)$ contains as a term the product of the soil water content and the matric potential. This creates a different shape, compared to the plot of the hydraulic potential, which would be for the Colpach pretty horizontal for larger saturations and then follow the retention curve. And we omitted the term water stock as recommended.

GR: 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 weight 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.

EZ: Sorry that I disagree. The curve accounts for the changing water content as it is the product of the $\theta (\psi + z)$. In Zehe et al. (2013) we analysed in fact the volume integrated values of free energy (taken from a calibrated model). This can be helpful, but with the integration we lose information about the distribution of energy within the system (similar as we lose information about the distribution of potentials, if we work with integral averages). Figure 5 provides information about the stratification of the energy along a representative distribution of geo-potential levels in the catchment. This is much more than an integral can provide. And as already shown in our reply to GR's last review, the information can of course be integrated (when using a calibrated model).

GR: 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.

EZ: This is obviously not well phrased. We wanted to express that small changes in the soil water content during dry conditions may cause large changes in the energy state, $d\psi/d\theta$. We clarified this in the manuscript.

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

EZ. We changed this to: Figure 2 shows that the three different soils at the same geopotential level, are characterized by distinctly different energy state curves as function of relative saturation.

GR: 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).
EZ: Absolutely true, we elaborate on this in detail in the discussion of the paper. Personally I think this is an advantage that the energy state curve is sensitive to depth to groundwater. This allows an estimation of the ground water level based in available pairs of soil water content, matric potential data and the local retention function, as we intend to show in a forthcoming paper.

GR 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.
EZ: Good point, we stress this in the revised manuscript.

GR: l. 383: the pF requires the absolute value of ψ.
EZ: We corrected this.

GR 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.

EZ: What we meant is "The averaged soil water content at each matric potential/tension-level $\bar{\theta}(\psi)$ is an estimator to the expectation value of the soil water content at this tension"

GR: l. 413: a pore space of less than 20 meters? I do not understand.
EZ. This is a misunderstanding. We mean. "The absolute values of e_{free} are in the corresponding C-regime less than 20m"

GR: Figure 5. Are the energy stare 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.

EZ.: We thought that section 2. made clear that we use HAND as an estimator of depth to ground water. To better stress this we added the following statement to the beginning of section "Note that we use HAND as an estimator for the depth to groundwater here."

That said it becomes clear that the free energy is at a saturation of 1 not equal to the HAND but equal to the product of Hand and the soil water content at saturation. We added to the figure caption: "Please note that e_{free} at a relative saturation of 1 equals the product of HAND and the soil water content at saturation." to avoid similar confusion.

GR: 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.

EZ: We do not assume an equilibrium with ground water. We just assume this for the definition of the equilibrium point. What we in fact see is that the system deviates rather far from this equilibrium but it also relaxes back to it. This is by the way our main hypothesis stated in at the end to the introduction, and it is nicely corroborated.

GR: 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 .

EZ: Obviously we did not explain this well enough (and note that the histogram of the observations points are given in Figure (3), Figure 5 provides those for the entire catchment). In this exercise we contaminated the HAND value with an error of 2m and plotted the corresponding energy state curve. This curve does considerably mismatch the observations. This corroborates on a) that HAND is a good estimator of depth to groundwater at this point and B) that an error in the estimated depth to groundwater leads to a mismatch between the theoretical state curve and the observed values. So the method is indeed sensitive to depth to groundwater, as correctly stated by GR above. But this opens in fact options for learning, as elaborated in the discussion section.

To better explain this we added the following to the new manuscript. In a further step we contaminated the HAND values of both sites with an error of 2m and plotted the corresponding energy state curves (z_{HAND} = 18 m). This curve does considerably mismatch the observations (Figure 6b, c). This corroborates a) that HAND is a good estimator of depth to groundwater at this point and b) that an error in the estimated depth to groundwater leads to a mismatch between the theoretical energy state curve and the observed values. This implies that the observed energy states will also change with changing groundwater surface, as further detailed in the discussion.

GR: 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.

EZ. Sorry to insist. The energy state curves of the Colpach show for saturation larger than 0.3 a pretty good a constant slope at a given HAND value. This is what I call a linear function and the observed states drop nicely into these linear ranges. An den plotted observations corroborate that efree grows linearly with (efree $= \theta \psi + \theta z$).

The retention function in Figure 1 shows that the matric potential in the Colpach is at the minimum observed saturation of S=0.3 (Figure 6b) equals -2 m. This implies according to Eq. 8 that $e_{\mathrm{free}} = - 0.3^{*} \theta s \, 2m + 0.3^{*} \theta s \, 20 \, m = 2.91 \, m$ and that potential energy is 10 times larger than capillary binding energy. For larger saturations the first term remains rather constant while the second grows linearly with saturation. This is now stated in the manuscript. The growth rate does of course change with HAND.

Thank you very much for the efforts,

Erwin Zehe

References

Zehe, E., Ehret, U., Blume, T., Kleidon, A., Scherer, U., and Westhoff, M.: A thermodynamic approach to link self-organization, preferential flow and rainfall-runoff behaviour, Hydrology And Earth System Sciences, 17, 4297-4322, 10.5194/hess-17-4297-2013, 2013.