Comment on hess-2021-151
M. van Noordwijk (Referee)

Referee comment on "Land-use and climate change effects on water yield from East African Forested Water Towers" by Charles Nduhiu Wamucii et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-151-RC1, 2021

General

1. The manuscript provides an interesting comparative study of the 'water towers' in East Africa and the change in terms of a simple water balance that can be inferred from a combination of various spatial data sources

2. The description of the quantitative framework can be improved, including a more consistent use of acronyms (especially for actual evapotranspiration) and equations

3. The study relies heavily on the use of a link between NDVI and the omega parameter in the Budyko framework, while the text acknowledges many factors (including soil, topography and seasonality) influence the relationship. At least in the discussion this needs some further work to see how much this could have influenced results and conclusions.

4. The eight water towers are most described as 'replicates', rather than each having a specific geographic, ecological and social context: this may be the limit of what is currently possible, but at least some of the contrasts noted call for further analysis and attribution (e.g. in relation to human population density within and surrounding the water tower.

5. It would help the paper if sharper questions would be formulated at the end of the introduction that gives structure to the subsequent discussion

6. Beyond the supply of blue water to downstream parts of the watershed, the high actual evapotranspiration in water towers plays a role in regional rainfall recycling -- at least some discussion of this aspect would be relevant.

Minor
p1, Line 17 Mention 'steady state' assumption of Budyko framework at an annual time scale
p1, Line 24 'non-resilient' suggests a binary classification, is there a more gradual description on the degree of resilience
p1, Line 29 but mountains also cause 'rainshadows' that don't get the rainfall they might have had without the presence of a mountain...
more quantitative criteria are needed to get the type of delineation that you use here

in glaciated mountain chains water flow depends primarily on temperature, without ice cap on recent rainfall -- so the temporal variability will differ and dependence on land cover increase

'receive' is a rather passive description -- isn't it 'convert atmospheric moisture into rainfall'

Some reference to Africa as geologically old shield, but rift valley plate tectonics are associated with younger and higher mountains

If you introduce more quantitative P/Epot criteria in line 31, this discussion on E African water towers becomes more meaningful, as it relates to both the P and the Epot side of the ratio.

'development of annual rainfall is nearly independent of mean annual value, showing that dry areas are highly variable in relative terms, with decadal variation super-imposed (Hulme, 1990) and not easily distinguishable from trended global climate change.'

Please unpack the sentence

possibly relevant: ET estimates for SS Africa

For corrections on common deforestation discourses, see Aleman et al. 2018

The methods of Ma et al. 2010, 2014 combine these two categories by running rainfall statistics and recorded land-use change patterns in reverse order in calibrated process-based models

Maybe mention the steady state assumptions at annual time-scale upfront. A simple equation might help here.

It would help the subsequent discussion if you formulate some clear questions here that you try to answer in the results section

if you want to avoid use of 'we', please find a less abstract passive formulation...

As 'montane forests' and 'water towers' only partially overlap, please give the quantitative definitions of both;

Public discussion on the Mau forests in Kenya described these as 'water towers', you don't; again clarifying the quantitative criteria can help

In the Dewi et al. water tower delineation no fixed contour was used for the delineation, but one relative to the watershed as a whole. Please clarify your choice here, esp regarding the two (Aberdare and Bale) that were adjusted to the surrounding areas...

Here you seem to shift from PET to ET or ETa -- the preceding paragraph only mentions Epot.

This section may be clearer if you first present a water balance equation...

Deserts tend to have wadi's -- even in zones with low average rainfall, runoff occurs and rainfall intensity exceeds instantaneous infiltration capacity. Your Budyko-based description here needs some empirical adjustment (and scale considerations)

Not only under very dry conditions... About 50% of tropics has a P/PET ratio below 0.65; only a quarter has P/PET above 1.0

Fu in stead of FU

Equation 1 -- please specify the time step (1 year?)

Wouldn't it be better to include a DeltaS storage term, and then make explicit that you assume this is zero at the time scale of your analysis (but this is a considerable source of uncertainty and error...)

Please settle on a single acronym for AET = ETa = ET

The seasonality effect is linked to the DeltaS term that you're hiding...

As this is an empirical result, please describe the data set on which it was
calibrated (from which it was derived)
p4 Line 8 So what about other influences on omega (soil types, and topography, climate
seasonality, ...) that you just mentioned? You assume that these are at the average
values in the Li et al. dataset? This will require some further justification, especially as you
operate in the relatively rare bimodal rainfall part of the world.
p4 Line 17 Please remove ;
p4 line 12 Please indicate what you treat as 'known' inputs here and what as parameters
to be estimated
p4 Line 36 So the EIBud is based on the NDVI relationship? It would help if you give more
formal definitions of the terms here
p4 Line 6 Is the DeltaEI here the same as d in Eq 5?

p5 Wouldn't it be easier and more informative to present the ETa/PET ratios?

p7 line 1 where omega values 'observed'? maybe 'derived'

Fig 7 How can Q estimates of 1000 mm/year be obtained for places with P hardly above
1000 mm/year?

p14 Discussion: A clearer structure of the discussion is needed.
p14 Line 19 As you used NDVI data, you used land cover rather than land use change as
basis...
p14 Line 21 The sensitivity to land cover change reflects the limited degree of actual
change (due to existing institutional arrangements) rather than the lack of response if
such rules would be relaxed. Please distinguish these two aspects.
p14 line 24. An alternative to describing deviations along the Y axis (vertical) is to
attribute them along the X-axis (horizontal): would such an approach be feasible?
p14 Discussion: Can you imagine doing the same analysis on the basis of ET/PET ratios
attributed to NDVI, rather than the more complex Budyko route that involves P in the
estimation of omega?
p14 Line 27: what do you mean by naturally occurring oscillations in this context? Does
the occurrence of fire (partially anthropogenic) play a role: it changes NDVI for one or
more years, increasing water yield; it may be more common on e.g. Mt Kenya and in the
Imatong mountains
p15 Line 4 Please clarify 'resilience' as bouncing back in relation to 'elasticity' that refers
to the degree of initial change, rather than its temporal dimension.

p15 line 10 Isn't this a consequence of the way water towers are defined?

Suggested additional references
Hulme, M., 1990. The changing rainfall resources of Sudan. Transactions of the Institute
of British Geographers, pp.21-34.
Ma, X. et al. 2014. Attribution of climate change, vegetation restoration, and engineering
measures to the reduction of suspended sediment in the Kejie catchment, southwest
Ma, X. et al. 2010. Sensitivity of streamflow from a Himalayan catchment to plausible
changes in land cover and climate. Hydrological Processes: An International Journal,
24(11), pp.1379-1390.