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Interactive comment

Interactive comment on "Attribution of growing season evapotranspiration variability considering snowmelt and vegetation changes in the arid alpine basins" *by* Tingting Ning et al.

Anonymous Referee #1

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The authors extend existing Budyko-type approaches for decomposing monthly ET variance (eg Liu et al 2019) amongst variances (average monthly deviation from an annual mean value) in underlying physical drivers of plant water use (e.g. rainfall). In particular, the model extension now accounts for variance in snowmelt fluxes and variance in vegetation cover. The manuscript is a logical extension of work previously published on the topic. However, I do have serious concerns with clarity of presentation in some parts of the manuscript, as well as the underlying "consistency" of the datasets used in the study (detailed in specific comments below).

I also have a general question about the overall approach (that will probably reveal my own ignorance about these methods!). When I think about “variability” in ET, I first think about the year-by-year variation in the magnitude of ET in a particular month. However, if I’m understanding this manuscript correctly, the variability that is under consideration is the average of the monthly deviation of ET (or underlying drivers) from a long-term annual average. Is this interpretation correct? That is, in Equation 12 does \overline{ET} equal the long term annual mean, and does the index “i” in this case index all of the values for a given month? If so, this is somewhat confusing, as in the previous sections, “i” was used to index the month itself (not the collection of values for a given month). I ask because I can imagine another form of “variance” that is more in line with my expectations, but i’m not entirely sure how it should be interpreted with respect to the variance I described above (or if it’s even functionally different from what i described above): This is where \overline{ET} is the long term monthly mean of that particular variable (not the annual mean), and where the variance is the variance of the annual realization of that variable about it’s long term monthly mean. I think the author’s framework addresses the former definition, but am not sure. Is there a significant difference between these two interpretations? If so, what are the different types of questions that you might address with one approach or the other? Additionally, in the case of the first description (average deviation in a given month from a long term annual mean), why is this simply not referred to as seasonality? Presumably this form of “variability” can’t be used to address questions relating to long term trends, etc. I apologize if this long-winded question is a bit convoluted; I’m wrestling with some of these concepts for the first time! Thanks for any additional clarification.

COMMENTS:

-It would be helpful if the authors included units when introducing terms; e.g. What is “M” and what are its units?

-Lines 53 - 65: veg change and disturbance?

-Lines 56-57: Why the “but”?

-Lines 67-68: What do the authors mean by “which has been the foundation for decomposing ET or runoff variance and is expressed as:”. What has been the foundation for decomposing ET? Are the authors saying that “snowmelt influence has been the foundation for decomposing ET”? I’m not sure what that means.

-Lines 131-132: Δ S is computed as difference in GLDAS soil moisture down to 2m between months. However, the authors explicitly refer to groundwater as being important with respect to storage change impacts on ET in their introduction. Can these shallow soil moisture measurements reliably represent total storage changes in the catchment? Presumably, in these semi-arid basins, significant storage dynamics occur below 2m depth, both in the deep unsaturated zone and deeper groundwater. What are the consequences of this for the author’s findings? Will the impact of storage changes be significantly underestimated?

-Line 138: This seems important. Some overview of the Yang 2009 method would be helpful.

-Line 142: What is F? Assuming it’s percent forest cover. It’s unclear why we need this, and its relationship to M.

-Line 157: Perhaps useful to point out the parallel to Zeng and Cai, a further elaboration of “effective” precipitation. In their case, this included precip and deltaS. Here, snowmelt is added.

-Line 162: So, ET is obtained as the residual of a mass balance, and then this nonlinear equation is solved for “n” for each value of ET? It seems strange to me not to use the GLDAS-estimated ET (which is available), given that this is how Δ S is specified.

-Line 170: GLDAS specifically has a snowmelt band. Why not use that, given using it for other aspects of the analysis? Might be more consistent?

-Line 180: The authors state March to July are the major snowmelt months. Why then do the authors then only perform their analysis April to July? Also, why not just apply the analysis for all months of the year? What is the purpose of leaving out the rest of the year?

-Line 186: "M" has not been sufficiently defined leading up to this section.

-Line 194: Is there any basis (e.g. citation) for this functional dependence? While I agree that vegetation will play a role in determining 'n', it's also true that 'n' likely depends on other catchment features, such as soil water storage capacity. I guess the question, then, is whether these other drivers can be assumed constant through time, and thus somehow justifiably lumped into the fitted constant parameter 'a' in Equation 8. Can the authors confirm that vegetation is likely the only non-static component of the exponent 'n' through a brief review of such mechanistic models? The first one that comes to mind is Porporato et al (2004); though I'm not sure the functional form is identical to Equation 3. Porporato, Amilcare, Edoardo Daly, and Ignacio Rodriguez-Iturbe. "Soil water balance and ecosystem response to climate change." The American Naturalist 164.5 (2004): 625-632.

-Line 216: It's probably obvious to most folks, but the authors should still probably define the overbar as some long term mean. Also, would \bar{ET} also equal the long-term mean ET from Equation 3? Probably best to try to stay consistent with notation if possible.

-Line 227: What is the function "F"? It is not defined. It is referred to as a "factor" in Line 233. Also, what is the underscore notation used here e.g. "R_M". Presumably these correspond to the terms in Equation 14, but that's not very clear, and the notation is not explained or defined.

-Line 240 - 244: This is an unexpected addition that I don't fully understand. Are the authors analyzing results for different representations of effective precipitation? If so, why, and where was this motivated? I don't think it was outlined in the methods. It looks like the authors use 3 different forms of increasing complexity; precip alone; precip plus snowmelt; precip plus snowmelt plus storage differential.

-Figure 3: I'm also a bit confused on this figure. Should it be the case that the points fall on the correspondingly colored curves? Were the curves generated by fitting to the points? Why should a single curve be fit across the ensemble of ET/Pe values for each month? Isn't it reasonable to expect that even the same month in different years will have different values for "n" due to interannual variability in factors that determine "n"? (this relates to my general question about timescales at the start of the review).

-Line 245: Can the authors explain this statement? I don't understand the significance. Is this just to say that if you don't account for all potential fluxes into the rooting zone, the mass balance might be incorrect?

-Line 261: Is it true that \Delta S is expected to be small or zero if there are no inter-annual storage changes?

-Line 304 - 306: I don't think this is an explanation; it's a restatement of the finding that vegetation has a larger impact on ET variance when water is not limiting. The authors still have not answered (or ventured a hypothesis) as to why ET is more sensitive to variability in vegetation cover when water is not a limiting factor? I can think of a couple of vague hypotheses, but would love to see a bit more discussion from the authors on this point; it seems central to the paper.

-Line 313: A downward trend with respect to increasing aridity? It would be helpful if the authors continued to explicitly state the dependent and independent variables when talking about trends.

-Line 318: Elasticity has not been defined up to this point. This is an important concept that the authors should explain more clearly around Equations 13 and 14.

-Line 318: I think it would be very helpful if the authors more explicitly described this idea that the contribution is dependent on both the magnitude of the variance of the driving variable as well as the elasticity.

-Lines 320 - 324: The model developed here cannot speak to these non-stationary changes though, correct? The analysis here is only pertinent to intra-annual variability attribution, as the variance under consideration is that of the average of the monthly deviation from an annual mean, as opposed to the year-to-year variance of a particular variable about it's long term monthly mean? Again, this relates to my timescale question at the start of the review.

-Line 330: What is a "good" vegetation condition?

-Line 392: "Corrected" I assume should be "correlated"?

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