

Rogozin et al.'s manuscript presents the nine-year dynamics of WUE, CUE, and LUE in a hemiboreal forest in Estonia, and determines which climatic variables drive them at daily and interannual scales. They found VPD to be the main driver of daily WUE and LUE, PAR to be the main driver of daily CUE, and temperature to be important for all three. SPEI analysis shows complex effects of drought and wetness on the three RUEs. Overall, this paper provides valuable insights on the dynamics of an understudied ecosystem type and is clearly written. Some of the methods choices should be reconsidered though, and more extensive analysis is needed to have clear takeaways from the SPEI analysis – the authors make claims that should be supported by further data and analysis from their site. The relevance of hemiboreal forests and the previous research that has been done on RUEs should also be expanded upon.

My main feedback includes:

1. Clearer relevance of their research. The introduction is superficial and doesn't give the reader sufficient reason for why the study system is unique, nor explanation of what has been researched to this point and what those studies found.
 - a. L51-53: why does occupying the boreal-temperate ecotone make hemiboreal forests especially sensitive to climate variability? More explanation would be good here – why do these forests respond more strongly to the same amount of climatic variability than another forest type would? Or do you mean that the boreal-temperate ecotone is experiencing more variability?
 - b. L65-77: In this paragraph, please make it clearer where studies in the hemiboreal zone have been conducted: it sounds like only in southern Siberia? Maybe start with this, and mention what they found? And then compare and contrast with what was found in regions close to the hemiboreal zone elsewhere?
 - c. L98-100: here in the methods, it sounds like the site is really only coniferous forest. The intro emphasizes that a hemiboreal forest is characterized by a species composition that combines elements of both northern coniferous and southern deciduous forests. The reader needs more justification/explanation here of what the deciduous characteristics are of your forest site, and what distinguishes it from other coniferous eddy covariance sites that have been studied.
2. Potential methodological issues:
 - a. Partitioning method (L153-154): consider also calculating daytime partitioning – if your results agree between the two methods, it would make them more robust. Neither partitioning method is completely reliable, and can lead to very different results depending on site. If you don't consider both, please provide a reason for choosing night time partitioning.

- b. Growing season determination: based on Fig D1 (L763), in the current GS determination method, SOS and EOS dates rely heavily on the shape of the seasonal curve instead of on when plants were active. For instance, years 2020-2022 have SOS/EOS dates well into when plants were active, while for other years the growing season starts and ends right around when plants begin showing activity. Years 2020-2022 have clearer peaks than the other years. Based on this, I believe you should use a metric that is more robust to GEP growing season shape – for instance fitting SSA and then using a threshold of max GEP for that year would yield more reliable results across years. The current approach does not capture the phase of plant activity equally well in different years, which is what growing season should measure.
 - c. WUE (L176-182): please justify why you're using eWUE instead of another WUE metric (such as inherent or intrinsic WUE). Also, you should exclude at least a day or two after a rainfall event (due to strong soil evaporation signal in ET).
 - d. LUE (L192): LUE is typically calculated using absorbed PAR, (APAR), not just PAR.
 - e. SPEI (L243): please explain in more detail what is meant. Was 3 month SPEI calculated as a rolling window and compared to each daily RUE? Does SPEI have just 1 value each month, or is a new 3 month SPEI calculated for each day?
3. Additional analysis needed to support SPEI claims: In section 4.4, better explanation is needed for why some SPEI bins show significant deviation from normal and others do not. Perhaps it has to do with the number of samples in each bin? This would be good data to provide alongside fig. 7. The range of variation for each bin should also be provided for all RUEs. Overall, additional supporting analysis is needed before clear takeaways can be made from the SPEI analysis in my opinion – I find some of the claims the authors make to be over-reaching based on the data provided.
 - a. In the WUE section (L643-666), authors imply the significant differences found during moderate drought and mildly wet conditions are minor since the actual range of variation spans the normal conditions. Is this not the case for CUE and LUE bins? This data must be provided. If significant differences in WUE can be disregarded, why can't significant differences in CUE and LUE be disregarded?
 - b. The CUE paragraph (L667 to 687) needs to be supported by another figure showing GEP, RECO, and NEP responses in the different SPEI bins to show that the behavior they are hypothesizing is in fact taking place.
 - c. When explaining LUE response to SPEI, the authors appear to contradict previous sections when trying to explain why LUE increases from normal in mild drought/wet conditions, when they say that these conditions 'do not impair ecosystem function, as boreal species are adapted to such fluctuations' (L691-692). This contrasts both WUE and CUE decreasing under mild wet conditions.

The explanation for the much higher LUE under severe drought also seems dubious without further evidence to support claims. Even if shaded canopy now has a relatively higher contribution, overall GEP would still be lowered if less of the canopy is photosynthesizing, and since LUE is calculated by GEP/PAR here, LUE should also be lower. Overall, the SPEI results seem difficult to interpret and unclear.

Other comments:

L125-126: what qualifies as unreliable values?

L151: need to define NEE

L344-346: there's hardly any difference between which variable dominates each of the RUEs in Table G1, I feel like this sentence is misleading.

L348-356: Lines identical content to L338-346, one of the two passages needs to be deleted.

Fig 5: it's hard to see the black lines, and even harder to see the grey shading.

L532-534: Please justify more why IAV of CUE is primarily driven by NEP, when there seem to be very small differences in partial r between NEP and GEP influences on CUE (0.996 vs -0.964). Here GEP seems to drive CUE only very slightly less?

L576: The structure of section 4.3 is a bit hard for the reader to follow. Perhaps small changes like combining some of the paragraphs that are about the same climatic driver could help.

L599-605: I don't think Fig. 5j really supports the slight decline in response at highest irradiance. It's not clear visually, and if the high VPD effects on WUE and LUE are discounted as model artifacts in the previous paragraph, it's hard to understand why the much milder signal of CUE decreasing at high PAR (which I honestly can't see in the fig) should be significant and not also a model artifact. Perhaps showing similar figures of GEP, RECO, and NEP vs PAR could help support these claims.

L629-631: why is air temp important for CUE in one daily analysis and not the other? Shouldn't that make it a less robust driver of CUE overall, and perhaps not be named the second-most important driver of CUE (L616)? I understand this is because of the results of the random forest model, but in this case you should justify why you trust the random forest over the GAM analysis. Since other prominent drivers of RUEs in the random forest tend to agree more with the GAM analysis than in the case of Tair and CUE, I think your analysis does not necessarily support daily Tair being a main driver of CUE.