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Interactive comment on "A biophysical approach using drought stress factor for daily estimations of evapotranspiration and CO_2 uptake in high-energy water-limited environments" *by* David Helman et al.

Anonymous Referee #1

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Helman et al. identify a lack of ground-based ET/C-uptake measurements in water-limited environments as a motivation of their study. The authors test a biophysical approach based on a satellite derived estimates, with and without a seasonal drought stress factor, across paired forest and non-forest sites.

Central to the method employed is the use of a drought stress factor. In my eyes it isn't really a drought stress modifier. Plants are responding not only to atmospheric demand, but also to supply limitations from the root-zone. The method used by the authors only considers atmospheric demand. The one strength their approach does

have is that it is cumulative over 2 months, so the effect is gradual. But why 2 months? Clearly the level of stress is sensitive to this assumption. I can't think of a physical justification for this decision. On a more fundamental level, this assumption overlooks the fact that vegetation in these water-limited regions would respond to water stress in a fundamentally different way to vegetation in more mesic regions. However, their modifier doesn't explicitly attempt to account for this in any fashion.

This issue presents itself in various forms in the manuscript. The modifier appears to lack sensitivity at various key points (see comment below & fig 2), the authors highlight that they can't account for legacy effects and that they might need a "local" drought stress factor. That final point seems inconsistent with the method. How can the authors advocate for this method as an alternative to infill flux data, whilst simultaneously advocating for the need for a locally refined modifier?

There is clearly some potential to the methods employed by the authors, but it is hard to grasp the extent of this. In part this is because a lot of their comparison is anchored in a comparison against the model without a stress factor. It is understandable why they take this approach, but there is an element of a straw man to it, meaning that we miss any interesting insight into how the new model performs. For example (fig 3), if I look carefully, the GPP model has a lot of day-to-day variability not captured at all by the model with drought stress. What drives this? Both the ET and GPP models overestimate the fluxes, is this related the flat sensitivity to drought during J-A highlighted in Fig 2?

The other core weakness I see in this paper is a comparison to some other satellite derived product. How different are these results to using other satellite derived GPP/ET products? I feel like this is an avenue of exploration that would add considerable value to the paper. The comparison to a poor model without a drought modifier doesn't add much value in my eyes. But if the authors can demonstrate clear improvement over more widely used GPP/ET products using their drought modifier then I think that is publishable. It may offer insight and comment on the need to consider their drought modifier in other satellite products. Ultimately this is an area I'd suggest the authors consider in any revisions.

The authors explore WUE. I fail to see what insight this really brings? The products used rely upon the same input remote sensing data, so the measures aren't truly independent? I realise the authors aren't the first to take this approach, but it is worth reflecting if this really makes sense? Why not instead compare to a well used satellite product? Or consider how they might adapt the drought modifier to reflect drought-adapted vegetation. The WUE bit seems tangential to the story.

Finally, I'm also struggling with PaVI-E as a validation metric? It is derived from satellite data as well, so is itself a product, one that isn't independent? Why not just stick to testing during the continuous flux period? There are also few details offered about what PaVI-E constitutes to the reader, this needs to be rectified.

Minor stuff ——— - Line 83: The argument that remote sensing estimates of ET and GPP are too complex for other communities seems a strange one. Certainly it ought to be substantiated. Given these data are provided as "products", should another community wish to use them I fail to see the complexity?

- Fig 2. Perhaps try different colours? It is really hard to tell the lines apart. There is also essentially no sensitivity of the drought stress between J and A. Does that seem realistic? The authors argue the fluxes are reduced during drought to more realistic values, but I can't really conclude that from looking at the graph. The caption and labels could be improved. It is also unclear what the "obs" are here? Are there any obs? How is the reader meant to come to a conclusion on "realism"?

- All figure captions, labels could be improved for clarity.

- Line 583: The authors advocate that the water factor should account for past stores. No information is offered as to how they propose this would work in a practical sense. I don't see any obvious way this could be integrated with the scheme they've used here.

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