Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-152-RC1, 2017

Biogeosciences Discussions

Interactive comment on "Water stress induced breakdown of carbon-water relations: indicators from diurnal FLUXNET patterns" *by* Jacob A. Nelson et al.

M. G. De Kauwe (Referee)

mdekauwe@gmail.com

Received and published: 6 November 2017

Nelson et al. present a novel exploration of FLUXNET data to derive two new data-driven drought metrics. I think their approach is very interesting and this could form part of a more nuanced way to benchmark land models in the future. Ultimately, whilst I feel that this paper should be published, I think the text requires quite a lot of clarification and redrafting.

I found the introduction & methods text a bit disjointed. There is very little in the way of text to explain or set the "why is drought a big problem" argument. To me, there was a strange focus on considering the proportion of transpiration to ET. The authors spoke a lot about uncertainty, but it wasn't very clear if their focus was data (flux measurements) or model world. In short, the authors could do a better job of framing the broader problem before they get to the hydraulic and non-stomatal limitation text. In the methods, you often jump or introduce new concepts with little back story and it becomes quite distracting.

I also think the analysis of the results could be more incisive. I strongly feel the authors are doing themselves a disservice in terms of likely citations by not picking through their results a bit further (see comments).

Finally, whilst I shouldn't have to, it still true that it isn't the norm to share code, so I applaud the authors. I suspect it is likely to lead to their work being more widely used and potentially improved upon.

Introduction ————

- I don't immediately follow why you've introduced VPD into equation 2? Surely your estimation of GPP and ET have both already accounted explicitly for a VPD dependence? Then on line Pg 1, line 19 you say "more consistent" ... more consistent with what? This is probably simply my ignorance, but I'd like to follow the logic here because you then use VPD in eqn 7 and 8.

- Pg 2, line 2: "propagate errors ..." I assume you mean in terms of a model? As actually measured fluxes would account for any drought signature? Please clarify.

- Pg 2: The arguments about the uncertainty of T as a proportion of ET ... do we really think that this the chief uncertainty here is drought? To me it feels like an odd framing of the argument simply because I wouldn't expect water stress to dominate the water cycle and the uncertainty range quoted is large. I think this line of argument would be improved by simply talking about the need to understand the carbon and water cycles during water stress. I'd argue for removing all of this text.

Methods ——-

- I think it would be helpful to explain why PET was calculated. The text just jumps to we calculated PET...Also, there is a brief mention of why the approach was adopted, but it should be expanded upon. Similarly with the CSWI, you just suddenly jump to explaining it without any back story for the reader.

- The screening of data to remove contributions from the soil is potentially problematic. I've seen that other authors have used 48 time slots after rain (see Medlyn et al 2017, New Phytologist and references they cite). The authors have taken a different approach, but screening GPP < 5 g m-2 d-1 seems quite high? Presumably as you get a drought, GPP drops and this may remove some of the signal you seek to explore? Similarly an air temp of 15 deg C. Whilst admittedly not "warm", doesn't it depend where you are? There are many locations with variable day-to-day temp, even in summer. Did the authors explore any sensitivities to these assumptions?

- Similarly the assumption about precipitation and gap filling. What about filling it with reanalysis data? Assuming a gap corresponds to a 5 mm precipitation event strikes me as quite a big assumption? What happens if you simply assumed a gap = no rain? How important is this assumption for your results?

- Page 5, line 24. Is there any evidence of this shift? I'm not arguing it isn't true, but the authors don't cite any supporting literature. Later on in the text the authors cite Wilson, but are there any other citations? It would be good to support this point. Figure 1 is nice and useful for demonstrating the authors point.

- Page 8: similarly to where I've made this point before, you really need to introduce things better. Suddenly the text jumps to the "Katul" and then the "Boese" models, with little or no back story. To this point I've found this paper really interesting, but these jumps honestly make it hard to follow and are quite distracting, so I hope I'm being constructive here.

- I don't really follow the benchmarking models? As to get ET, you use GPP derived from flux data and then measured VPD and Rg? Why do you need a benchmarking model? To me, you simply need to apply the method to the raw data?

Results ——-

- Why when DWCI < 10 is it reasonable that you have decoupling? That is stated as a fact without any support? Ditto CET < -0.5. Can you not demonstrate this for a case study i.e. the 2003 summer data from Europe, or similar?

- The 7 to 8% of all points being decoupled at all sites. Does that make sense? Wouldn't you hypothesise differences based on the vegetation? Rather than expect to find a universal value? I realise you have large uncertainty bounds, but I wonder what the implication of that finding is? Does it imply anything about the method at all? I don't have an immediate suggestion, I'm simply surprised.

- It might be interesting to see figure 3 expressed in a more informative way. Perhaps by mean annual precipitation, or spring/summer precipitation and/or an aridity index? It would also be interesting to see how variable individual years are? You clearly have this information, but it is compressed in your presentation of Fig 3 and arguably this information is very interesting and I'd argue that you're selling your paper short by not exploring it. For example, how variable was 2003/2010 vs other years for European sites?

- Similarly, do you see a shift in the centroid related to specific times in the year? Which sites shift earlier? What physically can you tie this to?

- I don't find figure 4 all that informative. Again I wonder if you are exploiting the interesting findings to their fullest? Which sites are most decoupled? Which vegetation types? Does it make sense to exclude the well coupled days, you're not really interested in these days?

- I won't really comment on Fig 5 because I don't follow the motivation. Partly because of my question about VPD and partly because I don't see why the metrics which are data driven, require a benchmark like this? I'm not totally sold on this being an objective means to test the approach, but appreciate why the authors have taken this approach. This is simply my opinion and I'm sure others would disagree. My first point of my discussion text below would be the way I would have been tempted to proceed.

Discussion ———-

- How do we know the method works? What would be the best test of the method? Even if the authors don't have access to the necessary data, could they set a challenge to the community? For example, if groups had sapflux or information on non-stomatal limitations at any flux sites, do the authors have thoughts how these data could be used? How should the community push such an approach forward?

- I think the discussion of trees vs grasses is interesting and welcomed, but I wonder if the authors looked at exploring a bit more within a functional group (i.e. by aridity etc), whether they might find something else too. Up to the authors of course.

- I'd argue that the authors could set aside some text to suggesting how their approach could be used in terms of benchmarking land models during drought? I'm not saying this paper has to do such a comparison, but it might be advantageous to lay the ground work. I'm guessing that the authors see modellers as potential users of their metrics? and if so, it is worth them making a case. Assessing models for responses to drought is very complicated and so their approach is welcomed.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-152, 2017.