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Interactive comment on "Patterns of plant rehydration and growth following pulses of soil moisture availability" *by* Andrew F. Feldman et al.

Anonymous Referee #2

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Review of Feldman et al. "Patterns of plant rehydration and growth following pulses of soil moisture availability"

Feldman et al. use SMAP soil moisture and VOD data to show that plant response times to moisture pulses (characterized by "time-to-peak" between the start of the soil moisture drydown and peak VOD") are differentiated between humid regions (with tp of around zero) and dryland regions (tp >=1 days and up to >3). Furthermore, the authors use a satellite LAI dataset to distinguish between plant rehydration versus plant growth mechanisms for explaining the dryland VOD increase. From this latter analysis, they demonstrate that at shorter timescales (tp 1-3) the VOD increase is dominated by plant rehydration, and at longer timescales (tp >3) the VOD increase is more dominated by plant growth that occurs when antecedent conditions are wetter and the pulses of a

higher magnitude.

This study represents an important advance in our ability to remotely sense relatively short timescale vegetation responses to rainfall pulses, and further adds broader scale evidence to support the pulse-driven growth in dryland regions. The study context, questions, methods and results are clearly described and analyzed. The authors neatly address many caveats of the work, including limits and uncertainties associated to temporal sampling interval of the satellite instrument. I think this is exciting work that offers promise for exploring plant responses to moisture pulses in more depth when satellite sensor temporal sampling interval and spatial resolution are increased.

Main comments

My only remaining questions are relatively minor and are related to the LAI data. I appreciate the need to use a geostationary product given you require a high temporal resolution - thus, the choice of the EUMETSAT LSA SAF LAI product. Satellite LAI data are known to agree well in terms of temporal dynamics but differ in terms of absolute magnitude (e.g. Garrigues et al., 2008; Fang et al., 2013). I am wondering how biases in magnitude would impact your results, if at all? Perhaps the rates of change in LAI would be consistent across products. Did the authors look at any other optical geostationary satellite data products that are related to leaf growth/vegetation activity (NDVI, FAPAR, LAI)? I know there are not as many geostationary optical satellite products related to vegetation activity, and I confess I am not as familiar with these products, but I think there are some. For example, the GeoNEX products (Wang et al., 2020) and related to this I think is the NDVI from the AHI sensor (Miura et al., 2019). NDVI can be calculated from GOES-16/17 (https://www.ospo.noaa.gov/Products/land/vegetation.html).

A different point related to the LAI data: How noisy are the daily fluctuations? Are the changes in LAI you see after moisture pulses clearly detectable from the noise? Are the changes you see in LAI within the product uncertainty?

As the authors nicely discuss, these results are in line with many field-based studies. Therefore, I do not expect different LAI data, or any other optical satellite data related to leaf growth, to have a strong impact on the key findings here. I would just be curious as to how much of a difference the LAI (or NDVI etc) dataset makes and would be interested to see a brief discussion on any caveats related to LAI data noise and algorithm uncertainty.

I initially had a question has to why the LAI be decreasing across shorter tp timescales, and does that mean the positive changes in VOD actually reflect an even higher increase in plants' relative water content? This seems to be happening mostly in the Sahel. I then read in the discussion that this is because these events are mostly detected during periods when shrubs are shedding leaves, which makes sense given the shorter VOD increases are happening in drier periods. Have I understood correctly, or could there be any other reason? For longer duration tp > 3 the Sahel also has some decreases in LAI, with weaker increases than other regions in Africa. Is there any other reason to think the LAI in the Sahel is either less reliable or more influenced by other factors that are confounding these results?

Line 433-435: I am not sure this analysis fully supports this conclusion: "demonstrating evidence for the pulse-reserve hypothesis and suggesting sub-weekly (rain pulse) rather than seasonal phenological controls on growth (Noy-Meir, 1973)". As the authors have demonstrated, plant growth with longer tp periods are associated with wetter preceding conditions and stronger pulses. This could be in seasons that are already favorable for growth (as the authors state in lines 368-369), suggesting seasonal phenological controls (which may include temperature constraints) are still important. The pulses just result in that extra bit of growth. Given the studies that show inter-annual variability in net CO_2 uptake is strongly linked to days with peak gross CO_2 uptake (Zscheischler et al., 2016), I am wondering whether increases in leaf growth during the longer tp periods translate to increases in carbon uptake. Perhaps SIF data would be useful in this regard. However, this is probably beyond the scope of this study.

Minor comments

Fig. 4C is not referenced in the text. Fig. S5: describe sub-figure C in caption.

References

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