

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

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Reviewer 1: This study assesses the temporal pattern of soil moisture and plant water content derived from SMAP to understand plant responses to precipitation pulses, which have shown significant lags in field studies. The comprehensive spatial scale of this analysis is impressive and timely. Importantly, the authors use a daily LAI product for Africa to account for the combined growth and rehydration signal inherent in VOD interpretation. I have some concerns about the correct use of Kruskal-Wallis tests and found the comparison between spatially average temporal VOD trends and results of a 1D SPAC model somewhat spurious.

Authors: Thank you for your constructive comments on our study. We will address your

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comments as discussed in the responses below.

Reviewer 1: Major comments Ln 282 - 287: This section is throwing me off because I cannot easily distinguish between rapid responses and short VOD increases; they sound the same to me. Adding the actual tp lengths would help.

Authors: We will add in parentheses in line 281 that rapid VOD responses have $tp = 0$ days and short VOD increases have tp of 1-3 days.

Reviewer 1: More importantly, the boxplots of Fig. 5 do not convey distinctions based on the KW tests. Is there some kind of non-parametric post-hoc test, equivalent to Tukey HSD, which can be used to put labels on the boxplots? As far as I am aware, the KW tests only indicate that at least one group is significantly different but does not indicate which ones. This may require pairwise Mann-Whitney tests.

Authors: It is correct that the KW test does not identify where the large differences in means are. We did all possible pairwise Mann-Whitney U tests between all variables and found statistically significant ($p < 0.05$) differences for all tests. We wish to avoid adding labels to the boxplot. Therefore, we will note that pairwise Mann Whitney U tests show significant differences in the Fig. 5 caption.

Reviewer 1: I don't think the spatially averaged time series shown in Fig. 3A and B are comparable to a 1D SPAC model result in Fig. 6A and B. Rather, both kinds of modeled drydown patterns might be found in the actual data if not aggregated spatially. The relatively occurrence of immediate versus lagged rehydration may additionally depend on antecedent soil moisture conditions.

Authors: We acknowledge the differences in spatial scale of the results. The idea is that the mechanism that dominates spatially will influence the VOD signal the most, while the other mechanism will have minor influences. We will add a few sentences explaining this more explicitly in Line 397 after the Fig. 6 discussion. For example, we would add: "Note that both conditions may be present within a coarse-resolution pixel

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because the pixel VOD spatially averages behavior over the landscape. Therefore, while plant resistance limitations may dominate landscapes that show 1-3-day VOD increases, slow infiltration responses may still be spatially prevalent, but to a lesser degree.”

Reviewer 1: Minor comments Ln 160: Does this refer to the empirical probability mass function? Probably more clear/accurate to refer to as the histogram and described as a zero-inflated Poisson.

Authors: That is the correct and more specific distinction. We will revise line 160 to be: “The t_p probability mass function within a given pixel typically has a mixed distribution with many zeros, resembling a zero-inflated poisson distribution.”

Reviewer 1: Ln 176-184: This section could use greater clarity. Make clear that non-parametric ANOVAs are to compare the covariates of ΔLAI , antecedent soil moisture, antecedent VOD, etc. between the three groups of VOD pulse responses.

Authors: We will add language to lines 180-184 for example in line 180: “The groups of three different t_p lengths are then compared for each respective metric of ΔLAI , antecedent surface soil moisture, soil moisture pulse magnitude, and antecedent VOD.” Here, we make it clearer that the timescale groups are compared for each of the respective metrics.

Reviewer 1: Ln 231: “. . . aboveground biomass requires water uptake . . .”

Authors: This change will be made accordingly in line 231.

Reviewer 1: Ln 238: “co-occur”

Authors: This change will be made accordingly in line 238.

Reviewer 1: Ln 238-241: I suggest “Seasonal detrending of VOD isolates plant re-hydration response to moisture pulses, which show multi-day increases and eventual decreases following moisture inputs.” Current phrasing is difficult to understand.

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Authors: Based on this nice wording suggestion, we propose to change these lines to “Our seasonal detrending of VOD isolates these pulsed plant growth responses from seasonal growth cycles. These isolated sub-weekly VOD responses closely link to the timing of moisture pulses suggesting a cause-effect of rain pulse followed by plant water content response.”

Reviewer 1: Section 3.3: Is this section also limited to Africa? Make clear at the outset, rather than in Ln 278-280. Other- wise, “growth-influenced VOD” is not supported.

Authors: These results are shown for Africa only. However, Fig. 5 is almost identical if shown globally. Therefore, we will change the plot to global, but make the note in line 289 that these results can still be directly compared one-to-one with Fig. 4 since the results are nearly identical if shown only for Africa.

Reviewer 1: Ln 299-306: This uncertainty analysis should be at least mentioned in the Methods section and lead with the purpose of this approach before referring the reader to the supplement.

Authors: We will add a new section 2.4 at line 190 that describes the uncertainty analysis. It will primarily consist of lines 299-306 with more explicit details of how the analysis was conducted. The first sentence will provide more explicit motivation.

Reviewer 1: Ln 326: Plant hydraulic capacitance, or something else?

Authors: Plant hydraulic capacitance is correct. We will add that.

Reviewer 1: Ln 332: “. . . pulse, which may indicate sufficiently well-watered conditions. . .”

Authors: We will make the update accordingly.

Reviewer 1: Ln 336-337: This sentence may not be necessary. Is the emphasis on parallel soil moisture and VOD decreases following the pulse? What is “plant-storage water potential”?

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Authors: We agree that the sentence in its current location can be confusing. This sentence will be moved up to the second last line in line 334 and will be revised to: “Thereafter, VOD loss can be due to simultaneous soil and plant drying where plant rehydration becomes progressively restricted with drying soil.” We will remove the “plant-storage water potential” phrase which was meant to refer to the collective water potential of the plant at predawn, but we see how it could be ambiguous.

Reviewer 1: Ln 339: Redefine RC time constants again at the beginning of the Discussion

Authors: We will add a brief definition in parenthesis at the end of line 339 that says “(plant water uptake and storage timescales).”

Reviewer 1: Ln 380: Again, I suggest introducing the SPAC model and its purpose in the Methods section before referring the reader to the supplement.

Authors: We will add a brief new methods section (Section 2.5) introducing the SPAC modeling methods and motivating the simulations. Here, we will refer the reader to more details in the SI.

Reviewer 1: Ln 406-408: This seems in direct contradiction to the above citation of Kramer and Boyer 1995. There is also the component of fine root growth after soil rewetting. Is the SPAC model able to represent the soil-root conductance or potential growth of new roots?

Authors: The Kramer and Boyer 1995 citation is in reference to a different point about soil infiltration potentially being faster and therefore giving evidence to root resistances being the driving factor here. It is not directly about root resistances as is the point in lines 406-408. We will remove the point about macropores in lines 395-397 because it is more speculative. We will add a note about fine root growth after rewetting in line 408: “Finally, fine root growth can occur after rewetting which can contribute to decreasing root resistances (Eissenstat et al., 1999).” The SPAC model does not in-

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clude specific components due to soil-root resistance or root growth. We avoid more complex parameterizations and argue that the root resistance is sufficient to capture these dynamics discussed in lines 398-408 collectively though we don't have further information to partition the exact physiological mechanisms at large scales.

Reviewer 1: Ln 409-414: Please clarify the thoughts presented here. Is it related to VOD to water potential as well as water content? This paragraph seems out of place in this section and is not well-prefaced by the introduction.

Author: We have decided that we will remove this paragraph because it makes a subtle point that may be misleading in its current location. We will instead include a sentence in line 110 of the methods: "VOD is expected to be sensitive to rehydration because of near-linear relationships between relative water content and plant water potential, especially for herbaceous species which are primarily investigated in this study (Jones, 2014; Jones and Higgs, 1979; Konings et al., 2019; Nobel and Jordan, 1983)." Here, VOD's connection with relative water content has been established in a previous sentence and we are drawing the link between VOD and water potential.

Reviewer 1: Ln 433: ". . .demonstrating spatially-extensive evidence for. . ."

Author: We will update the text accordingly.

Reviewer 1: Ln 440: Use commas in the list

Author: We will update the text accordingly in line 440 with ". . ."patterns of rehydration, growth responses, and rain pulse dependencies . . ."

Reviewer 1: Ln 444: "show" is used twice. Perhaps "We demonstrate. . .ecosystems exhibit . . ."

Author: We will update the text accordingly.

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