

Earth Surf. Dynam. Discuss., referee comment RC1
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Comment on esurf-2021-102

Anonymous Referee #1

Referee comment on "Using repeat UAV-based laser scanning and multispectral imagery to explore eco-geomorphic feedbacks along a river corridor" by Christopher Tomsett and Julian Leyland, Earth Surf. Dynam. Discuss.,
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I believe that the manuscript by Tomsett and Leyland will, with some refinement, make a significant contribution to the ecogeomorphic literature. While the paper is technically robust, using a variety of tools and analyses to link remotely sensed data to hydraulically-meaningful vegetation characteristics, there are a number of deficiencies in its current form.

For one, the need for and novelty of this work needs to be more carefully constructed. To develop a spatially robust understanding of plant-flow-sediment interactions, this work comes at the problem by way of characterizing plant morphology and classifying groupings of plants based on like forms. This is in contrast to Diehl et al 2017 and Butterfield et al 2020 who instead use the identification of species, and the link between species and their functional traits, to classify groupings of plants. These are fundamentally different ways to approach this problem, creating a different product. My sense is that the authors are very aware of these differences, and reference it throughout the paper, but the differences, and strengths of the different approaches should be highlighted and made clear in the introduction. Because the approach presented in this paper does not use any ecological observations nor actually link their prediction of functional types to topographic change, the approach in its current format does not make the linkage between ecological and geomorphic processes. The authors acknowledge this in the discussion (lines 820-825), but again, this should be brought up to the introduction.

The approach described here seems to have two major benefits over Butterfield et al (2020)'s use of classified remote sensing imagery as a way to create maps at scale: 1) there is not a need to have field-measured traits to identify a species' functional type and 2) this approach can add a fourth dimension, time. The authors discuss #1 as a rationale, but #2 is poorly developed and executed. They discuss the importance of time-or seasonally- varying parameters for understanding plant-flow interactions and use seasonal differences in NDVI as a way of differentiating between different types of herbaceous plants, but do not provide any meaningful way of characterizing or classifying the differential impact of plants on fluvial processes during different seasons. For example, is

the difference in seasonal NDVI between branching and single-stemmed herbaceous plants hydraulically meaningful? Could you develop one map of functional types for the winter and one for the summer? Also, its not clear as to if the finding of a different spectral signature between the two herbaceous guilds will hold for other settings, or is it because of the difference in species type here?

As the authors think about how to more carefully frame their work, one additional consideration is the more precise use of terminology. The idea to adopt concepts from ecology into geomorphology as a way of investigating the interactions is welcome and represents a promising path forward in integrating ecological and geomorphic processes. However, I found that the authors use terms such as "traits", "functional types", and "guilds" fairly loosely. Some specific examples are provided below.

The paper uses a variety of datasets and analyzes them in technically sound ways, but there are numerous missed opportunities to take the data one or two steps further to provide a little more insight into the ecogeomorphic value of the classification system. The stated goal of linking traits-based guilds with ecogeomorphic change and capturing the temporal variability is not quite accomplished with the current analyses. My best understanding is that the authors use the long term analysis to link veg/no veg with bank erosion and the likelihood of avulsions. While this is an interesting analysis, it does not provide any details on the importance of functional groupings of plants on morphodynamic processes, nor provide insight into the change in plant-fluvial process interactions with season. Instead, can you create functional plant grouping maps for each of the four topographic change maps (Figure 10) and evaluate the relationship between erosion, deposition, or no change and functional group? Even if you cannot create unique classification of plants for each change map, assuming the distribution of plants remains the same (OR creating two classification maps- one summer one winter), can still give you some powerful data that can help achieve your stated goals to your "Aims" in section 1.5.

There is little validation in this paper to help the reader understand if this approach is helping to advance ecogeomorphic studies in a meaningful way. You must have a sense of the types of species growing at the site. If so, you should provide the reader with a summary of these types of communities and consider comparing the measured traits with traits listed in the literature or in the TRY database.

Specific Comments:

Line 1: In its current form, the title leads the reader to believe that the analyses in the paper evaluate the temporal dynamics of ecogeomorphic interactions.

Lines 16-19: If I understand this correctly, you used the long term analysis of channel changes and a general classification of "trees" vs "no trees" to come to these conclusions. If so, it would be more accurate to say "We show that **vegetation generally** has a role in influencing morphological change through stabilization...."

Lines 44-46: Traits-based classifications are intended to achieve this, if one can link field-measured traits to species/functional groups.

Line 55: "how vegetation is modelled" is vague. Instead specify the ways in which people model vegetation? Bulk roughness? Cylinders? Rigid vs stiff?

Line 59-61: Are you referencing aquatic vs riparian (or terrestrial) vegetation here?

Sections 1.2 and 1.3: These sections need some work to provide proper background on plant traits, their use in ecogeomorphic studies, and how existing approaches are not adequate. I found the explanation of hydraulically-relevant traits to be scattered and if I was not familiar with the literature would be lost as to what a hydraulically-relevant trait is and why its relevant. You might consider referencing Table 2 in Diehl et al 2017 and briefly describing the different traits. This will then help set the stage for Section 1.4, which should focus on how to measure these traits using remote sensing- the challenges and opportunities.

Line 70-75: I may read this incorrectly, but generally functional traits are used to define a functional group and so the argument is strange to me. This is different from either a species-specific or typological approach because functional types are groupings of species (likely typologically similar) with similar responses to the environment and with similar effects on ecosystem processes.

Line 81-82: Here it seems like you jump from traits to functional groups. The benefit of a functional group approach is the ability to generalize. If you were to take a traits-based approach alone, you would create maps of essentially different physical characteristics- say one of height, one of frontal area, etc. This would be informative, but not helpful in understanding the plant's full impact on the environment. Functional types clusters or groups plants with similar arrays of traits that, in the aggregate, explain the response (or impact) of that plant type to (on) its environment.

Line 97: Given your description in the following sentences, it might be more appropriate to change out "hydrological conditions" to "environmental conditions".

Lines 105-109: This point, that there is a lot of variability between species needs to be more carefully flushed out if it is one of your major points and rationale for your approach (vs starting with maps of species tied to traits). The traditional ecological traits-based approach relies on the fact that the traits used to define functional groups should have greater variability between species than within species. This comes up again in lines 132-134.

Line 116-117: This seems out of place.

Line 135: Do you mean "Hydraulically Relevant Functional Traits"?

Lines 162-166: These two sentences seem to contradict one another.

Lines 184-186: The height of a plant during submergence is not a trait. Instead, it's a function of the plant's height and flexibility, and maybe also other factors that determine a plant's pronation (e.g., branching structure, leaf area). This is an example of where you need to be careful with terminology. Also, the introduction of temporal variability is potentially critical to your framing of a need for 4 dimensions, but buried as an aside in this paragraph.

Line 226: Who operates this gaging station? Where did you download the data from?

Lines 287-308: Cut this section down, relying on the fact that there is a published paper. For example, there is not necessarily a need to tell the reader of this paper about the battery life of the UAV's.

Section 3.3: This is a cool methodology

Lines 342-344: Not sure what "a traits-based rather than bulk roughness approach is likely to be limited."

Section 3.3.2: Would be helpful to list all the traits you extract, or create a table. Why didn't you measure plant density? That is one that could be accomplished through remote sensing, can be important, and will vary in different parts of the river.

Line 384: Might be helpful to create a table of the guilds you adopted from Diehl et al 2017.

Line 385-386: What are "bulk roughness metrics" and how were they applied?

Line 390-392: How did you handle woody seedlings and saplings that might be a similar height to herbaceous plants?

Line 514: Change modelling to modeled

Line 556: This is the first time you bring in elevation as a "trait" to classify guilds. Is this value measuring the elevation of the ground surface around the plant?

Lines 725-729: I get that this is one of the main benefits of this work, but by taking out species consideration, you remove the capacity to evaluate the full set of feedbacks among environment-plants-topographic change and in essence you are just creating a map of plant characteristics.

Figure 11: Was this figure, and the matrix, created by comparing your guilds with topographic change? Or was it done conceptually? Again, I am not sure why you didn't perform a more comprehensive analysis of the differences in topographic change in and around the different guilds over different seasons.