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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The distribution pattern of desert riparian forests and its relationship with soil moisture and soil properties in the low reaches of Heihe River Basin, China" *by* J. Ding et al.

Anonymous Referee #1

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This work presents soil water content and biogeochemical data to explain how riparian vegetation changes as distance from the river increases. Vegetation is characterized by species composition and diversity, and occurrence and coverage of different plant functional types. The topic is overall relevant for readers of HESS. The manuscript is relatively clear, but might benefit from proofreading by a native English speaker. Despite the interest of the topic, I have some concerns regarding the analyses conducted and the mismatch between the ecological processes causing the observed vegetation patterns, and the one-time soil sampling adopted for this study.

Major issues

1) Ecological processes vs. one-time sampling. The plant communities examined in this work are the result of decade- if not century-long successional dynamics, but they are treated as if they are the result of short term processes. I refer specifically to soil water content, used as a predictor of vegetation community despite being measured only once. How representative are these water content measurements of the long-term water availability? Other soil properties vary at slower rates and could be more meaningful predictors (texture, SOM). How old are the trees and shrubs in this community? Are these communities shaped by the time they spent growing on a given soil (no information is provided to this regard), or by the edaphic properties of a given site (focus of the current study)? No data are reported on the variability in river discharge – how dynamic is the riparian environment? How frequent are flooding events that can re-shape the community (and soil properties)? Without this information, it is difficult to disentangle time effects from site effects.

2) Many of the measurements used as predictors are partly correlated, making it difficult to interpret the regression results. For example, soil water content is related to texture (as noted in P4, L18). Fine textured soils can hold more water, and this effect would appear in the gravimetric water content measurements. Total nutrient (TN and TP, which I assume include organic N and P) are also correlated to SOM, since large SOM stocks are associated with large N and P stocks (as noted in P20, L30). Due to these correlations, it seems difficult to apply regression approaches that assume independence, as in this case (if I interpreted the approach correctly).

3) The conclusions are based on too short-term a study to be really useful for planning. Either a long-term monitoring or a different study to identify possible historical reasons for the observed patterns would provide (or not!) support to a possibly large and expensive conservation project.

Minor issues

I am listing here only some of the small editorial issues in this MS – better to ask a native English speaker to give a thorough proofreading.

P2, L3: "focused" rather than "stressed" P2, L11: optimum in which sense? Is biomass higher around 1000 m, or what criteria was used to establish what the 'best' conditions are? P2, L19-20: it would be better to write if the mentioned influences are positive or negative. P3, L3: vague – what ecosystem services are important in this specific context? P3, L14: the term "ecological water conveyance" is not entirely clear? Is there a more commonly used term? P4, L18: the fact that fine textured soils can hold more water than coarse textured soils was well known before Rosenthal (2005) P5, L14: "that are differently…" P5, L21: the long-term perspective is not covered in this work, so the suggested measures may be consistent with the findings, but do not take into account climate or land use change. P6, L10: "As the distance… increases, water…" P7, L14: if I understand the sampling design correctly, there are five replicate gradients (transects perpendicular to the river), each with 6 sampling points – perhaps re-phrase? P8, L10: is the importance value calculated for each plant functional type as written, or for each species? P8, L14: RF is not present in the equations. P8, L23: the thickness of the canopy layer might not tell much about the actual biomass. Perhaps leaf area would be more representative. P8, L25: what does "them" refer to? P9, L2: suggested rephrase: "… and herb layer, which can be calculated…" Equations 5-8: to calculate D the only equation needed is Eq. 6, but in that equation, what is P? Is P related to IV defined in the previous page? Presented in this way, the equations do not seem to be related to D, which is the variable that needs to be calculated (if I understood the rationale). P9, L17: the layers used for gravimetric water content are not consistent with the layers used for other analyses P16, L22: it is not entirely clear which parameters are being predicted here – presence/absence for a given species, or the diversity indices? P18, L9: suggested rephrase: "… formed a bimodal pattern and reached local maxima at the distance…" P20, L14: "and possibly inducing…" P20, L15: as explained in the major issues above, it is not easy to infer water availability effects on the plant community from a one-time water content measurement. P20, L18:

suggested rephrase: "… also partly explained the variance of the plant community, with TP representing 8.1% of the explained variance and SOM being negatively… " P20, L22: when the groundwater table is "low", shouldn't it be "below" the degradation threshold? P20, L24: what is the relation between TP and groundwater level? P21, L4: "thus halting…" P21, L7: it is also possible that the points now at 1000 m from the river have been less disturbed, and thus harbour a community with larger biomass, diversity, or coverage.

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