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Interactive comment on "Testing functional trait-based mechanisms underpinning plant responses to grazing and linkages to ecosystem functioning in grasslands" *by* S. X. Zheng et al.

Anonymous Referee #3

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This paper by Zheng and collaborators explores the variation in 9 plant functional traits in grazed and ungrazed grassland communities along a soil resource gradient. They show that trait response to grazing depends on resource availability, and that different life forms show different types of responses to grazing along the gradient. Although not entirely novel, this type of study investigating the interacting effects of resource and grazing on community assembly, functional structure and ecosystem functioning is still highly relevant. Their approach using response ratios to grazing along the resource gradient seems like a promising way to capture interacting effects. Another asset of this study is the extensive sampling design which accounts for intraspecific trait variations between communities. Moreover, results concerning the different responses of

different life-form offer a fresh insight into plant responses to grazing.

Nevertheless, despite these promising ideas, I found the manuscript and results to be confused by a lack of clear justification of hypotheses and interpretation and an unsatisfactory statistical approach. Moreover, after reading the previous paper in Biogeoscience by Zheng et al (2010), I had a hard time seeing a clear distinction and progression in the results . I believe the authors might want to consider refocusing the paper on a few less redundant points. Below are listed the main issues I have with this paper, followed by some specific comments.

Main issues: - Unclear use of concepts and lack of justification of interpretations. Expressions such as “mechanisms”, “biotic factors”, “avoidance and tolerance strategies” are used somewhat lightly and would require better justification and definition in the introduction, but also in the discussion. See detailed comments below.

- The sources of trait responses/variations (e.g. as on fig 2) are never explained or discussed clearly. Trait variation between communities may arise from 1) species replacement; 2) intraspecific trait variability which may be due to a) plastic responses to grazing and resources, or b) selection of different individual or even ecotypes with genetic differentiation. Differentiation between sources 1) and 2) is essential for interpretation, and the authors have the necessary data to address it.

- Statistical analyses are sometimes inappropriate for the data and should be improved. In particular GLM’s such as the ones on Table A2 and illustrated in Fig 2 assume independence of data points, whereas data points here are non-independent in two ways: 1) different points belong to the same species 2) measurements along the gradient are grouped per community plot. Therefore, I would recommend performing a mixed model including 2 random factors: species identity and community block. Moreover, it is incorrect to perform repeated individual t-tests (or even ANOVAs) on percentage data such as in fig 5. A global chi-square test would be more appropriate.

- The authors should use the continuous environmental gradient in all analyses, instead

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of discussing communities individually, or referring to the “three community types”. These types are only vaguely defined, and in any case there is not enough repetition per community type to allow any kind of generalization. The continuous gradient, on the other hand, is a good tool when sample size is low, and may provide finer and more generalizable insights.

Specific comments

title: too long and imprecise, please reformulate. I fail to understand what the authors refer to as “trait-based mechanisms”? Mechanisms refer to the causal physiological and developmental response of individuals to their environment and interactions with other individuals, and the consequences in terms of population and community dynamics, none of which are addressed in this study. The trait pattern observed here only hint at possible underlying mechanisms. Moreover, the consequences on ecosystem functioning are also only indirectly assessed here (via proxies such as CWMs)

Introduction:

Page 3.

First paragraph: the list of trait interpretations here is not relevant for this first paragraph of introduction, and should be moved to methods. In fact, it would be better to start by defining the different plant strategies of resistance to grazing (tolerant vs. avoidant) very clearly early on, to then be able to introduce the expected association with traits.

Page 4

Line 13 :”but also by the biotic factors (e.g., plant species or functional group identity)” I think this use of the term “biotic factors” is misleading. Biotic factors tend to refer more to external biotic interactions with either other plants, or other trophic levels, as opposed to abiotic factors. The “biotic factors” described here refer more to some “internal” factors, which are in fact simply the functional attributes of the species whose response to grazing one is considering. I would advise to remove the expression “biotic

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factor" altogether from the manuscript, and to refer to the importance of "the identity and functional attributes" of species to predict their response to grazing.

Page 5 Lines 16-19: the third question is not really addressed in this study. Mechanisms cannot be inferred directly from the trait patterns

Methods

Page 8

line 1 "including 106 different species. . ." : I do not understand what "different species" means here. Different from what? The ungrazed sites? Moreover, what is the turnover of species between grazed/ungrazed sites, and also along the gradient? This is essential to interpret the trends in traits along the gradient.

Line 12-19: "all leaves of an individual. . ." : were leaf traits measured on all leaves, young and old, or only on a selected subset of fully expanded mature leaves (cf. Cornelissen et al 2003; Pérez-Harguindeguy et al., 2013)? What method was used to measure Leaf density?

Lines 26-27: I am not sure I understand the justifications for the palatability score. Why not use only the plant palatability index? It seems like the browsing score is more dependent on land-use management practices (number of browsing seasons) and less on inherent properties of the plants?

Page9 line 15: Data points are not independent here, and a mixed model (GLMM) would be necessary, with species and communities added as random factors. Moreover, if we cannot disentangle species turnover from intraspecific trait variation then I fail to see the ecological meaning of the trends detected by these models.

Results

Page 10

line 4: Has any transformation of the data been carried out prior to the PCA ? Fig 1:

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Why not represent PC2 ? It seems like an important axis, at least as important as PC3, capturing two important traits (SLA and plant Height).

Line 13-17 : I disagree, PC3 does not capture the leaf economic spectrum (LES). Along the LES, Amax correlates positively with LNC(mass) and SLA, but these tend to be negatively or inversely correlated with Leaf Density (see for example Niinemets 1999). So the positive association of LD and LNC along PC3 are actually in contradiction with the LES.

Lines 23-24: "slightly decreased. . .": this is misleading as the difference is in fact not at all significant (P=0.1)

Page 11

lines 6-11: Results from the model, and especially results showing the significant interaction term between grazing and the resource gradient, should not be put into the appendix (table A2) but shown in the main text. These are in my opinion the most interesting results, provided they are maintained when the accounting for random effect of species and plot (see previous comments).

Line 26-27: This is strange, palatability usually increases in grassland species with higher leaf nitrogen content and SLA. Moreover, I do not think the palatability score should be used as a functional trait to compare species since it integrates the Browsing season index which refers to external land-use factors, and not the plant functioning itself. This needs to be clarified. Why not use the plants' Palatability Index on its own?

Page 12

lines 11-12 :" The effects of grazing on plant functional group composition differed across different vegetation types" : no proper statistical test. Figure 5 indicates that differences between grazed and ungrazed were tested by "ANOVA"? Where are the anova results shown? Do the stars represent multiple post-hoc t-tests? These tests are performed on percentage data, and an overall chisquare test performed for each

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community type would be more appropriate to test changes in life form proportions with grazing.

lines 23-24: How were these comparison tested? Since there is no real replications for each community type (the quadrats constitute only pseudo replicates), the t-tests do not seem like the most appropriate method. It would be more interesting to look at how these CWM vary along the gradient, or rather how the difference in CWM between grazed and ungrazed vary along the gradient.

Discussion

Page 13

line 9: these PCA axes, though interesting, may not be interpreted as “spectrums” in the same way the leaf economic spectrum is discussed. The LES has been defined over multiple studies, and on a much broader dataset. It also refers to a clearly identified evolutionary trade-off. Please reformulate.

Page 14

lines 6-7: “. . . indicating that plant species exhibit both avoidance and tolerance strategies to grazing” I fail to see how a decrease in height and increase SLA necessarily indicate a mixed avoidance and tolerance strategy, or even any kind of strategy in response to grazing at all. Interpreting all trait patterns in terms of strategies ignores the fact that trait variations may be non-adaptive or plastic.

Page 15

Whole paragraph 4.3 : These interpretations of plant response are speculative and do not constitute mechanisms as no measure of tolerance or avoidance is really measured. Please reformulate.

Line 26 “ the annuals and biennials with high growth rate adopted more tolerant strategies. . .”. Please reformulate this sentence. Plants species do not “adopt” strate-

gies willingly.

Line 9-11: I do not understand this sentence.

Page 16

Line 6-7: the concept of mixed strategies should be clearly defined earlier in the introduction, along with expected trait patterns.

Line 9: “It is known that SLA is a relatively stable functional trait,. . .” Stable in what way? It has rather been shown to vary with environmental conditions, and it is evolutionary quite labile (Flores et al. 2014). Please justify this sentence.

Line 13: LMA is the same trait as SLA (the inverse), there is no need to mention both.

Page 17

Lines 2-3 : “Our findings indicate that the grazing-induced shifts in functional group composition are largely dependent on site productivity, particularly water availability”: This result is not shown in the manuscript. Responses in life form proportions would need to be tested along the soil gradient.

Lines 13-15: what are the consequences of these trends in CWM ? How do the authors interpret them apart from changes in growth forms ?

Page 18

Line 9-10: “the increase in prolonged droughts together with heavy grazing may accelerate the shifts in dominance from perennial rhizomatous grasses to perennial bunchgrasses in the typical steppe and consequently decrease biodiversity and ecosystem functioning and services.” Why would this shift in composition decrease biodiversity and ecosystem services?

References cited: Cornelissen, J.H.C., Lavorel, S., Garnier, E., Díaz, S., Buchmann, N., Gurvich, D.E., Reich, P.B., ter Steege, H., Morgan, H.D., van der Heijden, M.G.A.,

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