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MACQUARIE University
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Wednesday, 10 February 2016

Ref: 42740959

James Richard Lawson
130 Herring Road
North Ryde NSW 2113

Dear Mr Lawson,

I am pleased to be able to advise that at its meeting held on 10 February 2016 the Program and Examination Sub-Committee on behalf of the Higher Degree Research Committee considered your examiners' reports and determined that the degree be awarded subject to the completion of corrections as specified in the examiners reports and as outlined by the supervisor to be carried out to the satisfaction of the Higher Degree Research Committee.

Please find enclosed a copy of the examiners' reports. Your supervisor(s) has also been sent a copy of the reports and the reserved copy of your thesis. It is suggested that you contact your supervisor as soon as possible for consultation concerning the nature of the corrections and any additional work that you are required to undertake to your thesis. Corrections need to be completed by no later than 09 Mar 2016.

You should provide the Committee with a detailed report outlining the amendments made to the thesis together with a statement certifying the satisfactory completion of these requirements. To assist you with your corrections report, a copy of the corrections report template is available from:
http://www.hdr.mq.edu.au/information_about/forms#thesis

This report should be forwarded to Higher Degree Research Committee via Higher Degree Research Office for further consideration and approval.

Copies of your revised thesis should only then be permanently bound and lodged at the Higher Degree Research Office. In order to be eligible to have your degree conferred, you must have completed all the necessary requirements for your degree, including submission of three permanently bound corrected copies of your thesis. One copy of your thesis will be housed in the University Library, one copy will be sent to your supervisor and one copy will be sent to the Executive Dean of Faculty.

Important: Please note that if you required and were given an ethics clearance to conduct your research, you must include a copy of your **final** ethics approval letter in your permanently bound corrected thesis as an appendix.

Should you have any queries regarding this matter, please feel free to contact me.

Yours Sincerely,

Dr Ren Yi
Director, Research Training and International Research Training Partnerships
(for and on behalf of)
Professor Sakkie Pretorius
Deputy Vice-Chancellor (Research)

Part 11

Lawson, James Richard, "Environmental Controls on the Functional Ecology of Riparian Plant Communities"

This thesis makes a strong contribution to our knowledge of the relationships between functional traits of riparian plant communities and stream hydrology. This is a timely and important topic, and aids community ecologists in moving beyond floristic composition into functional adaptations. The thesis tackles important questions that are of relevance to our future on the planet. As hydrological conditions become more unpredictable, how will the botanical communities respond?

One strength of the study is the author's facility with complex statistical techniques. Sometimes, however, emphasis on description of statistical techniques can come at the expense of a robust description of the biophysical study system. Photographs or cross-sectional diagrams of the river sites would be useful in helping the reader to visualize the study system; a diagram of the field sampling design also would be useful.

Chapter two focused explicitly on the trait of wood density. Although generally well written, the abstract, a key component of manuscripts, has a weakness in that it fails to adequately explain the conclusion and is a bit misleading. A conclusion stated in the abstract (p. 22) is that "Large, rare flood events in particular appear to favour higher wood density strategies" . In the discussion, on pages 50 and 51, we are told that the high wood density species are actually the facultative riparian species that typically grow in the rainforest and also fringe the high (infrequently flooded) surfaces of the riparian zone; the obligate riparian species have lower wood density owing, in part, to selective pressure to rapidly grow to reproductive maturity. Thus, I was not convinced that large rare floods are selecting for high woody density within the floodplain community. I would like to have seen more discussion of the evolutionary significance of rare events, and more information on biological context (or natural history) such as the lifespan and age of reproduction of the obligate riparian species. How 'rare' is such an event relative to the lifespan of the individuals in question? Are the large, rare events simply scouring out the obligate riparian species and leaving only the facultative species on the fringe of the riparian zone? In other words, perhaps there are no species that are truly adapted to extreme flood events?

Chapter 3 was well written and informative and I have just one quibble. It is not until the discussion (p. 95) that we find out that the sampling regimen called for sampling an apparent subset of the riparian continuum- geomorphically homogenous sections of sloping bank. (In the methods on

page 74 we are referred to Lawson et al. 2015 for a full description of site selection criteria and vegetation survey methods. While I realize it is justifiable to no repeat methodologies within a thesis, I did not see a section in Lawson et al. 2015 about this particular aspect of site selection). As a reader, I want to know about such factors early on, in the methods section. Given that the chapter is about functional diversity, did a portion of the diversity go undetected because of the experimental sampling design?

I found Chapter 4 to be the least worthy in terms of scientific merit, primarily because of my dissatisfaction with the wording of the hypothesis including Hypothesis 1a: "Species richness and functional diversity increase and abundance of exotic species decreases monotonically with increasing hydrological heterogeneity". A hypothesis is a proposed explanation for a phenomenon. H1a (and the others) did not include a proposed explanation. Is the implication that species richness and functional diversity increase with increasing hydrological heterogeneity *because* such conditions disfavor exotic species? Thats seems kind of odd. Perhaps it would be better to reword as two independent hypotheses, e.g., "Species richness and functional diversity increase with increasing hydrological heterogeneity because physical heterogeneity favors biological heterogeneity "AND "The abundance of exotic species decreases monotonically with increasing hydrological heterogeneity because physical heterogeneity favors biological heterogeneity?" This latter one seems problematic however, because it is carrying many assumptions about exotic species . I advise revisiting the introduction (p. 125) and fleshing it out with literature, such as those by Sax et al., that describes the increase in regional floristic richness that can occur owing to influx of exotics.

Also in chapter 4, I was concerned by the lack of a standard sampling area (p. 129). A fixed sampling area (e.g. , 250 m2) seems fundamental for studies involving species richness. The author states that the difference in sampling area was controlled for in subsequent statistical analysis, but it seems that preliminary site reconnaissance should have allowed one to avoid this troubling post-sampling adjustment. Also, were forbs not sampled? "All trees, shrubs, fern rushes, and sedges" were sampled. To bolster the botanical foundation of the study, more information should be provided on which taxonomic groups were and weren't sampled. What, to the author, are sedges? Are these members of the Cyperaceae? Were members of Poaceae sampled? The rationale for excluding and including particular groups of plants should be provided.

In general, in a review, it is often easier to dwell on defects rather than on merits. I reiterate that this thesis makes an important contribution to the literature, and I hope these comments are taken in the helpful manner in which they are intended, to increase scientific rigor.

MACQUARIE UNIVERSITY

PART II: REPORT OF EXAMINER TO THE CANDIDATE

ON A THESIS SUBMITTED FOR THE

DEGREE OF DOCTOR OF PHILOSOPHY

Please state concisely the grounds on which the recommendation is based, indicating the strengths and weaknesses of the thesis. This should be in sufficient detail for candidates to gain clear understanding of your opinion of their work and the means by which your conclusions were reached.

Where further work is required, please indicate clearly those revisions and/or corrections which you wish to see made. Supplementary sheets may be attached.

Name of Candidate: LAWSON, James Richard

Comments on the Dissertation of James R. Lawson, *Environmental Controls on the Functional Ecology of Riparian Plant Communities*, Department of Biological Sciences, Macquarie University, Sydney

The dissertation uses a functional trait approach to investigate the influence of the hydrologic regime and other environmental variables shape riparian plant communities in southeastern Australia. Chapters 2 and 3 investigate the influence of flow regimes on functional trait composition and diversity in riparian plant communities. Chapter 4 investigates environmental heterogeneity as an influence on taxonomic and functional trait diversity while Chapter 5 departs from the theme to experimentally examine the interactive influences of inundation and elevated atmospheric CO_2 on gas exchange, growth and functional traits in three riparian tree species.

Overall Quality of the Dissertation

The investigations undertaken by Mr. Lawson undoubtedly offered an exceptional learning experience. The exploration of plant functional traits in response to hydrologic variability, the extensive field investigations, the in-depth data analyses, and the exposure to a broad array of disciplines and associated literatures certainly contributed to a high-level training exercise. Mr. Lawson appears to be making sustained progress toward becoming a professional ecologist. Nevertheless, I have a number of fundamental concerns that Mr. Lawson and his supervisory committee should carefully consider.

Specific Comments on the Dissertation

As for the dissertation document, there are several fundamental concerns that should be addressed:

Overall, the document would greatly benefit from extensive editing and better organization. More specifically,

1. The dissertation should be primarily the work of the author. Using "we" and "our" in the text is not appropriate as it strongly suggests that the research is not entirely that of the dissertation's author. As a result, the author's contribution to each chapter is not clear (except for Chapter 4). It would be useful to clearly state the author's contributions for each chapter as well as identify contributions from others.

2. The use of technical jargon is extensive throughout and should be eliminated wherever possible. For instance, see the discussion in Chapter 4, which was nearly impossible to understand. It would be useful, in terms of communication, to tell the stories in ways that are easily understood by a broad audience rather than requiring the reader to learn a new technical language.

3. The guiding questions in Chapters 2 and 3, as stated, tend to bias the reader toward conclusions. It would be better to phrase the questions in a neutral way; for example, "How does wood density respond to the frequency and magnitude of flooding?" or use neutral hypotheses.

4. The discussion sections for each principal chapter would benefit by having a set of appropriate subheadings. The research results are complicated, as are the central questions, and the discussion topics could be better communicated by guiding the reader through the explorations and extrapolations of results. In addition, subheadings would serve to keep unwarranted speculation at a minimum.

5. Abstracts for each chapter are generally quite vague in their summaries of central questions, approaches, and significant discoveries. Abstracts should be treated serious components of the documents; these are not.

Specific Comments on the Research

More importantly, there are also several fundamental concerns that should be addressed with respect to the research. Two of the chapters have already been published and, to be frank, I am surprised that they were accepted without addressing some of the following concerns:

1. The dissertation requires an introductory chapter that provides an in depth review and critique of what is known about factors influencing riparian characteristics, identifies major knowledge gaps, and presents a scientifically credible rationale for choosing the research topics investigated. Not having this important review appears to have led to a narrow perspective and ultimate understanding of major environmental drivers influencing riparian ecosystems, and directly contributed to issues described below.

2. Use of the literature is generally moderate-good but it should be much better. There are quite a few pertinent riparian articles that have been overlooked. Authors that quickly come to mind are: J Catford (w/ R Jansson), M Pollock, SJ Blanch, S Bechtold, K Roger's South Africa research group, Puckridge, NE Pettit, M Parsons, S Lake, Greet, KF Walker, Kominoski, J Roberts, S Capron, and Campbell. I suspect that there are many others. By not including their discoveries, the author has inadvertently formulated a limited perspective of controls on riparian ecosystems.

3. The author's focus on the above ground vegetation provides a highly restrictive perspective on factors shaping riparian characteristics. While I agree that hydrology is a master variable, others have demonstrated that site history, micro-topography, soil texture (for nutrient and water retention), fire, grazing, and root characteristics play vitally important roles in shaping riparian communities. These are ignored in the analyses and discussions, except for soils. Nevertheless, in that case, the national soil maps are inappropriate for riparian areas because of deposition/erosional processes (including layering in horizontal bands). Soils characteristics need to be determined for every vegetative plot.

4. The relatively importance of drivers shaping riparian characteristics changes during succession, from vegetative establishment to the expression of mature forests. While hydrology and water availability are important, they are not the only important environmental drivers and, as well, the relative importance can be species-specific. For instance, the interplay of soil texture and flood recession for initial colonization is vitally important for some species while, for others, it may depend on having adequate woody substrates to buffer flows, provide nutrients and help reduce browsing. Unfortunately, this important feature (i.e., changing drivers during succession and species-specific requirements) of riparian ecosystems is not addressed in the dissertation.

5. Examining wood density (stiffness) is interesting but, from a riparian perspective, so is flexibility and elasticity – which are not examined. Stiffness, flexibility and elasticity are all effective strategies for surviving high flows and should be examined. The flexibility, elasticity and three-dimensional spatial structure of riparian root systems are probably as important as above ground features in maintaining the community longer term but are not examined in the dissertation.

6. Virtually no empirical information is provided on the history, topography, land use and biotic characteristics of the sites used in NSW. Without an understanding of these sites it is very difficult for the reader to independently interpret the results in Chapters 2 and 3. This information could be included in an electronic appendix or as a separate Table.

7. The use of 10 x 50 m vegetative plots in NSW might be appropriate but only if scaled to the stature of the tallest trees (for example, see Van Pelt et al. 2006, Ecological Monographs). It is not clear if this was done. Further, it is not clear how geomorphic homogeneity was established, how site history influenced the communities (therefore, the results), and how human actions have influenced river flows over the life spans of the vegetation.

8. The dissertation requires a model (quantitative would be best, of course) to communicate central advances derived from the research as well as identify key linkages among chapters. Ideally this would appear and be discussed in Chapter 6, and be linked to the conclusions and rationales derived from the literature review that would be provided in an expanded introductory chapter.

Concluding Comments

The dissertation requires additional efforts in order to attain a level and scope of understanding commensurate with doctoral programs at other international institutions. It pains me to say this since I can clearly see the substantial effort that has gone into preparing this version. Certainly Mr. Lawson learned a variety of professional skills and garnered vital field experience. Unfortunately, I am not convinced that he has an adequate understanding of riparian ecosystems, one that would allow him to be a productive and accepted researcher within the broader professional community. Therefore, I recommend that the supervisory committee consider the comments provided above and, as well, ask Mr. Lawson to revise the dissertation to address them or to provide scientifically valid reasons for not doing so.

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Where further work is required, please indicate clearly those revisions and/or corrections which you wish to see made. Supplementary sheets may be attached.

Name of Candidate: LAWSON, James Richard

Well written, information rich, current with the recent peer reviewed literature (with a couple of exceptions mentioned below). This is scientifically sound, original work, and contributes to the field of functional plant ecology.

Strengths:

The study of the effects of elevated CO_2 was elegantly designed and addressed a sophisticated research question that is of relevance to managers and of general interest to researchers.

Significant findings in this dissertation that contribute to riparian ecology:

Mean wood density in riparian communities is driven by powerful but rare flow events; Greater functional richness in sites with a greater frequency of high flow periods (p. 146); Communities which are functionally diverse but species poor may have low functional redundancy (p. 162); Associations between functional trait diversity and flow-related variables; Lack of evidence that higher water use efficiency under elevated CO_2 might facilitate photosynthesis where water logging had caused stomatal closure (p. 200). This section provides a sound test of a sophisticated set of hypotheses.

(From Lawson et al. 2015) 80% of variation in functional dispersion was explained by a combination of variability in flood frequency, variability in flood magnitude, and mean daily summer flows (p. 161)

Interpretation of the findings incorporates relevant theory from ecological literature: competitive exclusion (p. 69), niche theory, etc.

Surprising findings that contradict prior notions (p. 154; p. 157). Species richness decreased as hydrological conditions became more heterogeneous; and flow homogenization by dams was associated with more species richness. This does indeed contradict the prevailing findings from the literature.

The discussion of the relationships between functional and taxonomic diversity are excellent (e.g., p. 97). The emphasis of functional diversity, redundancy and species composition is important contribution of this work. *(Continued on attached sheets)*

Signature of Examiner: .... **Date:**

The experimental work is well designed, well replicated and controlled, and provides compelling and important results. Even the experiments with counterintuitive or unremarkable results contain sound and important information and are publishable. I feel that each of the chapters (2-5) stand alone as four publishable studies. The Introduction and Discussion (chapters 1 and 6) do an excellent job of setting the stage and raising questions, and synthesizing and integrating the experimental work.

Weaknesses:

Problem statements and originality of this work are overstated at times. For example, in the Introduction it is stated that flow regime is 'thought to be a dominant abiotic control on the structure and function of plant communities'. Flow regime (and the fluvial processes associated with it) has been shown time and time again to be the master variable and is a principal abiotic control on plant individuals, populations, communities, and riparian ecosystems. In the published work Lawson states "Hydrology is widely considered to be the dominant force structuring riparian ecosystems" is the proper presentation (Lawson et al. 2015a).

Another unsubstantiated statement: "...in North America, where well-understood systems are dominated by a limited number of species..." (p. 6). This is a false pretense. There are well over 20,000 species of vascular plants and the systems are far from well-understood.

"Functional approaches to investigating the ecohydrology of riparian plant communities remains largely unexplored" (p. 24). But relevant exceptions should be cited: Aguiar, F.C., J. O. Cerdeira, J. M. Maria, and M. T. Ferreira. 2013. Riparian forests of Southwest Europe: are functional trait and species composition assemblages constrained by environment? Journal of Vegetation Science 24:628-638; Bejarano, M. D., M. Marchamalo, M. G. Tangao, G. Jalon, and A. Sordo. 2012. Responses of riparian guilds to flow alteration in a Mediterranean stream. Journal of Vegetation Science 23:443-458; Hough-Snee, N., B. G. Laub, D. M. Merritt, A. L. Long, L. L. Nackley, B. B. Roper, and J. M. Wheaton. 2015. Multi-scale environmental filters and niche partitioning govern the distributions of riparian vegetation guilds. Ecosphere 6: 1-22; Stromberg, J. C., and D. M. Merritt. 2015. Riparian plant guilds of ephemeral, intermittent and perennial rivers. Freshwater Biology doi:10.1111/fwb.12686.

P. 53. Statements like: "...functional approaches to ecohydrology can give insight into likely changes in riparian plant assemblages and associated changes in ecosystem function..." could use some speculation about what they might be. There is little in the way of specific management applications of this work, although its applicability is stated several times.

In some cases, methodology and statistical analyses are rushed through without giving enough detail to repeat the work. It is unclear at times, what the realm of inference is from the work. For example, p. 68, "...our key finding is that functional trait diversity in these systems tend to be ..." How generalizable is this? The use of interpolated hydrologic data is weak and the methods are not described in enough detail for another to repeat. The use of imputation to fill in missing trait data is questionable. If this is done, the work should also provide cross validation using estimated traits for species for which the traits are known. This would validate the use of imputation.

The statement that "using functional traits as descriptors of ecological strategy provides generality across systems" needs some fleshing out. This is stated but no elaboration about what this means or how transfer across systems would work. Examples would be useful.

Jargon is used at times. This at times adds an artistic element, but at other times is distracting. A couple of examples: "Describing communities in terms of functional traits *dissolves* species distinctions..." (p. 6-7); "...infrequent but high stakes events..."(p. 48); riparian patch mosaic results from sculpting action of hydrologic processes..." could be more simply stated, "...scour and fill create heterogeneity..."; "...climate model was almost completely subsumed by the ..." (p. 142); "center of gravity" (p. 159) should be "centroid".

"...plants must navigate..." (p. 181)

The statement that the traits selected were to capture a broad spectrum of ecological strategies (not biasing the work toward traits related to flow regime, leave many questions in the reader's mind. If one were explicitly trying to relate functional traits to flow regime, would it not be defensible to select traits most like to have been selected for by pressured associated with flow regime?

P. 101. It is stressed that the findings of this work have great management implications, yet the one sentence dedicated to an application for managers is weak. "...by maintaining functional diversity...managers may increase the likelihood that riparian communities will be able to maintain critical ecosystem functions". This would be a great place to be specific: what would your gradient study suggest for future shifts in functional groups given a changing future climate? How might managers go about using this information to manage riparian areas?

Some terms are used in inappropriate ways. E.g., P. 123 'fluvial hydrology', 'hydrologic flow regime' (P. 94 and throughout). Fluvial means related to flowing water so stands alone.

Chapter 4. The adjustment of richness by untransformed area is unconventional and generally not supported by the literature. Because of the log-linear relationship between area sampled and number of species encountered, a natural log transformation to plot size or area sampled is advisable. This will very likely change the outcome of this portion of the study. An alternative approach is to estimate richness from extrapolated species accumulation curves. Since the analyses are based upon an inappropriate adjustment, the chapter should be revised prior to publication. Further, richness and diversity are terms used interchangeably in this chapter. They are different measures.

Overall, the dissertation is sound and provides new insights, original approaches, and a contribution to our field.

Detailed comments on Environmental Controls on the Functional Ecology of Riparian Plant Communities

Introduction

The publication Merritt and Poff 2010 is miscited (e.g., p. 8) as a functional guilds paper. The correct citation is Merritt, D. M., M. L. Scott, N. L. Poff, G. T. Auble, and D. A. Lytle. 2010. Theory, methods and tools for determining environmental flows for riparian vegetation: riparian vegetation-flow response guilds. Freshwater Biology 55:206-225.

P. 9. Statement in first paragraph about exotic invasion being a threat, see Julie Stromberg's work and Merritt and Poff 2010. Stromberg, J. C., S. J. Lite, R. Marler, C. Paradzick, P. B. Shafroth, D. Shorrock, J. M. White, and M. S. White. 2007. Altered stream-flow regimes and invasive plant species: the Tamarix case. Global Ecology and Biogeography 16:381-393.

Poff, N. L., J. D. Olden, D. M. Merritt, and D. M. Pepin. 2007. Homogenization of regional river dynamics by dams and global biodiversity implications. Proceedings of the National Academy of Sciences of the United States of America 104:5732-5737 would be good to cite as flow homogenization is the topic of the work and the current study finds that richness is higher in homogenized flow regimes, counter to the Poff et al. paper.

Chapter 2.

In the Abstract (p. 22) plants should be abbreviated plant's

How representative were the selected study sites, to what area can inference be made? (p. 29, bottom)

p. 31. Why was heartwood used and not functional xylem? The water use efficiency and resistance to cavitation only have effect in the active xylem tissue.

Community weighted mean and variance (p. 37). Community mean wood density varied 50% between different hydrologically modified sites. Flooding frequency had no influence on wood density (p. 39).

Not much discussion of the inter-site variability in mean wood density at low values of hydrologic variables (p. 41). Although mentioned on p. 47.

p. 45. The variance explained by the principal components analysis is different in the figure (86.5%) and the figure caption (80.3%).

p. 48. "...but the exact role that woody fibres play in stabilizing xylem vessels appears to be inconsistent..." this needs more attention. Find citations.

p. 49. For plants that are habituated to plentiful soil moisture, having no backup strategy for surviving drought conditions may be risky. See Pockman, W. T., and J. S. Sperry. 2000. Vulnerability to xylem cavitation and the distribution of Sonoran desert vegetation. American Journal of Botany 87:1287-1299 and Horton, J. L., T. E. Kolb, and S. C. Hart. 2001. Physiological response to groundwater depth varies among species and with river flow regulation. Ecological Applications 11:1046-1059.

p. 50. It would be good to include Family names for the study species to help international readers.

p. 51. "High wood density species tended to occur further up the bank, so would be subject to only the more intense flooding events." And least moisture availability (depth to water table, less frequent flooding, lower duration of flooding)

p. 53. "...continents climate patterns..." should have an apostrophe "...continent's climate patterns..."

Chapter 3.

p. 70. "Metrics of functional trait diversity are more powerful than taxonomic metrics as indicators of ecosystem functioning, ecosystem resilience..." seems overstated and like it should come after the presentation of results. Make sure the citations support this strong a statement. Diaz et al 1998, Hooper et al. 2005, Tilman et al. 1997.

P. 71. Diaz and Lavorel 2007 is mis-cited as a two author work. The correct citation is: Diaz, S., S. Lavorel, F. De Bello, F. Quetier, K. Grigulis, and M. Robson. 2007. Incorporating plant functional diversity effects in ecosystem service assessments. Proceedings of the National Academy of Sciences of the United States of America 104:20684-20689.

P 72, paragraph 1. This would be an appropriate place to cite Merritt et al. 2010. This is the paper on trait-based riparian response guilds.

P. 77. "Missing data were approximated..." a better description of how data were approximated is necessary. Detail should be provided so that another researcher could repeat the approach.

P. 81. It is not clear why species with <1% cover were excluded. It would seem that their presence would still contribute to functional diversity, functional redundancy, etc. Species with low cover are often eliminated in ordination, but it would seem appropriate to include them here.

P. 86. Figure 3.1. Please provide p-values and r^2 values. I would be interested in hearing some explanation for the outlier to the lower right of several of the plots. In the published work, p-values are provided. Please provide throughout the dissertation.

P. 86 and 88. Why would the 95% CIs require smoothing? They should be a continuously smooth like directly from the calculated CIs.

The use of 'hydrological flow regime' (P. 83; and throughout) is not the conventional terminology in the hydrologic discipline. We typically refer to 'flow regime' or 'streamflow regime'.

P. 94. "To our knowledge, this is the first..." There have been a few others and they should probably be mentioned. Most recently, Stromberg and Merritt 2015 and Hough-Snee et al. 2015, but there are a couple of earlier papers by Bejarano et al. and Aguiar et al. that could be cited as exceptions to the statement:

Aguiar, F.C., J. O. Cerdeira, J. M. Maria, and M. T. Ferreira. 2013. Riparian forests of Southwest Europe: are functional trait and species composition assemblages constrained by environment? Journal of Vegetation Science 24:628-638.

Bejarano, M. D., M. Marchamalo, M. G. Tangao, G. Jalon, and A. Sordo. 2012. Responses of riparian guilds to flow alteration in a Mediterranean stream. Journal of Vegetation Science 23:443-458.

Hough-Snee, N., B. G. Laub, D. M. Merritt, A. L. Long, L. L. Nackley, B. B. Roper, and J. M. Wheaton. 2015. Multi-scale environmental filters and niche partitioning govern the distributions of riparian vegetation guilds. Ecosphere 6: 1-22.

Stromberg, J. C., and D. M. Merritt. 2015. Riparian plant guilds of ephemeral, intermittent and perennial rivers. Freshwater Biology doi:10.1111/fwb.12686.

Chapter 4.

P. 122. In paragraph 2, there is some discussion of "...each nice is associated with an optimal ecological strategy..." Seems like niche is not being interpreted in the conventional sense.

P. 124. Poff et al. 2007 would be good to cite here as the work deals explicitly with flow homogenization and its effects on biota.

p. 136. Not sure how reliable imputed data are. Could be a little circular and be suspect to a reviewer. Suggest, doing cross validation to predict traits of species for which the imputed values can be checked against true values. This should be presented to convince the reader that imputing is reasonable here.

P. 139. The use of Cailliezs correction to make a Gower similarity matrix Euclidean needs explanation and a citation. Why was this step necessary?

P. 140. Instead of saying "Where required..", something like " to more closely comply with the assumptions of xx statistical tests..."

Standardizing richness by sampling area is not standard practice in diversity and richness studies. Because of the log-linear relationship between area and richness, a natural log transformation of area is the convention in plant ecology. This is more commonly accepted. Extrapolation using species accumulation curves is even more defensible.

P. 141. It is common to first run a Multivariate analysis of variance (MANOVA) when all of the characteristics are not independent (measured on the same individuals). Then a Bonferroni (or other adjustment) must be made to the ANOVAs to prevent Type I statistical error.

Wherever r^2 values are presented, a p-value should be provided. Also, on the plots of regressions (or in the figure caption), r^2 and p-value should be provided.

P. 149. The axis labels could be spelled out rather than abbreviated, for ease of interpretation, there seems to be room and thos acronyms require an extra step in interpreting the plots.

P. 162. Other studies have shown that regulated flows do not necessarily favor non-native species (Merritt and Poff 2010).

Chapter 5.

P. 181. The statement "Riparian plant communities are often dominated by keystone species..." is not conventional knowledge and should have a citation.

P. 181. The statement "...functional traits of trees are most strongly adapted to regeneration niche..." requires some elaboration.

P. 189. Is specific leaf area meaningful in seedlings?

P. 90. MANOVA is necessary prior to the individual ANOVAs due to lack of independence in the variables measured on the same individuals. The fact that the waterlogged treatment plants were harvested at a younger age than the other two treatments, raises questions about the results.

P. 90. I think that type III sums of squares (not Type II) is advisable when sample sizes differ. Provide some rationale for setting the alpha at 0.1.

Chapter 6.

P. 214. "...dispersion of wood dispersion..." appears to be a typographical error.

P. 223. Please provide p-values along with the correlation coefficient from a Pearson correlation.

P. 223. Typographical error "citepd" should be "cited".

P. 227-228. Could the increased richness in homogenized flow regimes be a transient and short-term colonization by generalist species that will reverse as a function of competitive dominance in the future?