**HIGHER DEGREE RESEARCH CANDIDATE AND SUPERVISOR’S**

**THESIS CORRECTIONS/AMENDMENTS REPORT**

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| **CANDIDATE DETAILS (please PRINT clearly or TYPE)** | | | |
| Family Name |  | Given Name |  |
| Student No |  | | |
| Supervisor’s Name |  | | |
| Faculty |  | | |

**Part A: Detailed report (to be completed by the candidate)**

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| **Examiner**  **Name** | **Thesis Page Reference** | **Summary/extract of Examiner’s Comments or Requirements** | **Substantive Corrections/Amendments made (additional information including a list of typographical error corrections may be included as an attachment)** | **Revised Thesis page Reference** |
| Merritt | 5 | “Flow regime is thought to be a dominant abiotic control on the composition and  structure of riparian plant communities” is too weak a description of accepted thought about the role of flow regime. | I believe my cautious phrasing is warranted given the results of Chapter 4, which show that flow regime does not necessarily explain variation in functional diversity independently of other environmental factors. In addition, much of the extant literature describes North American and European systems, with only a minority of articles addressing Australian systems. | NA |
| Merritt | 6 | Merritt takes issue with my comment that western North American systems are dominated by a limited set of species. | Have modified text from “This approach is effective in  western North America, where well-understood systems are dominated by a limited set of species.” to “This approach is effective in western North America, especially where systems are dominated by relatively few species.” | 6 |
| Merritt | 26 | Further references are suggested which describe the use of functional approaches to ecohydrology (two of which were published subsequent to publication of Chapter 2 in Journal of Ecology). | Have updated text to include suggested references.  “While ecohydrological classification is becoming established as a tool to explain plant community attributes such as species richness, stand structure and composition \citep{Poff2010a, Arthington2012}, to date only a small number of studies used functional approaches to investigating the ecohydrology of riparian plant communities \citep{bejarano2012, aguiar2013, stromberg2015riparian, Hough-Snee2015}, and use of quantitative functional traits has been rare.” | 26 |
| Merritt | 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” | I already make some suggestions regarding possible changes in riparian vegetation in reponse to climate change in the lines preceding this statement. E.g. “Projected increases in climatic variability (Hennessy et al., 2008) may therefore overlay the already strong natural variability induced by ENSO to produce significant alterations to streamflow. Under such conditions,  near-channel abundance of opportunistic terrestrial species (with their broad diversity of wood density strategies) may decline in favour of rheophyte-dominated assemblages whose ecological strategies are optimized to harsh hydrological conditions.” | 53 |
| Merritt | 68 | Merritt questions the realm of inference of the work in Chapter 3. | I discuss the generalisability of these results in the discussion on page 99 (where I suggest that I may have identified the ascending half of a unimodal curve associated with mild to intermediate environmental harshness and in the last paragraph of the discussion (pages 101-102). |  |
| Merritt | 32, 77 | The use of interpolated hydrological data is described as weak and difficult to repeat given the detail. | Unfortunately interpolation of hydrological data has limitations but in this circumstance was the only possible avenue for filling in data. Similar techniques and software have been used by contemporaries – e.g. in Arthington et al. (2012) Ecological limits of hydrologic alteration: a test of the ELoHA framework in south-east Queensland. National Water Commission, Canberra, Australia.  With respect to Chapter 4, the reader is referred to Mackay et al. (2014) for full details on the interpolation procedure. |  |
| Merritt | 136 | Concern about validity of using imputation to fill in missing trait data. | These concerns have been addressed in Penone (2014) which is cited in the text. This article reccommends imputation of missing data to reduce associated bias. A table of error estimates for imputation of missing data has been added to Appendix 3a. |  |
| Merritt | 220 | “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 of how transfer across systems would work. Examples would be useful. | Changed ““Using functional traits as descriptors of ecological strategy provides generality across systems \citep{Lavorel2002, Suding2008}” to  “Using functional traits as descriptors of ecological strategy provides generality across systems \citep{Lavorel2002, Suding2008}, for example by allowing comparison of communities with dissimilar assemblages”. |  |
| Merritt | 98 | “The statement that the traits selected were to capture a broad spectrum of ecological strategies (not biasing the work towards 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 likely to have been selected for by pressures associated with flow regime? | The point of selecting traits which capture a broad spectrum of ecological strategies was to show that that even when one doesn’t consider flow specific traits and look at traits that describe the most generally important components of ecological strategy, flow regime is still more important than soil or climate. |  |
| Merritt | 101 | Commentary about management implications is weak. Request for suggestions about potential future shifts in functional groups under climate change. | I don’t think I can comment directly on shifts in functional groups of riparian plants because I have focused on biodiversity in general in Chapter 3, rather than responses of specific trait combinations along environmental gradients. The main management implications lie in the use of functional diversity as a proxy for ecosystem functioning and for making inferences about community assembly. I spend several paragraphs in the discussion of Chapter 3 (not just one sentence as commented by the examiner) discussing how the findings are important from an applied river management and conservation perspective (page 100-101). |  |
| Merritt | e.g. 123 | “Some terms are used in in appropriate ways – e.g. p123 ‘fluvial hydrology’, ‘hydrologic flow regime’. Fluvial means related to flowing water so stands alone. | Have changed ‘fluvial hydrology’ to ‘hydrology’ and ‘hydrological flow regime’ to ‘flow regime’ throughout the entire text. |  |
| Merritt | Chapter 4 | “Adjustment of richness by untransformed area is unconventional and generally not supported by the literature”. Merritt suggests a natural log transformation of plot area, or better, a rarefaction-based estimation of true species richness. | I have redone the analysis in Chapter 4 using Chao’s Richness Estimator (rarefaction-based estimation) instead of richness adjusted by untransformed area as used before. The results of this analysis have changed somewhat and the manuscript has been partially rewritten to reflect the altered results.  SUMMARY:  The conclusion that hydrology is again (as in Chapter 3) shown to be the ‘master variable’ controlling riparian vegetation composition is weakened. In the updated results, the optimal hydrological model is not shown to explain substantial variation independently of other environmental factors. Relationships with extent of hydrological modification are maintained, however. Overall, the combined environmental model now explains substantially less variation in species richness than previously. This has led me to conclude that other factors which were not quantified in this study, such as local site history and intraspecific interactions, may be as important in driving riparian vegetation composition as broad scale environmental conditions such as hydrology, flow modification, catchment land use, soil composition and climate.  The conclusion that “the absence of strong linkages between the extent flow modification and metrics of functional diversity or exotic abundance suggests that use of environmental flows may not be effective as a tool for riparian rehabilitation in modified subtropical landscapes such as south-eastern Queensland” is strengthened, and is not featured in the abstract of the chapter.  SPECIFIC DETAILS:  A number of minor changes have been made to the manuscript to reflect the updated result for species richness. As this represented only one component of study described in Chapter 4, the revisions have been relatively minor.   * Abstract modified to remove the claim that hydrology is shown to be the master variable. * Abstract modified to comment on the limited likelihood that environmental flows would be effective in modified SE QLD landscapes. * Added paragraph to Methods detailing estimation of species richness: “True species richness values were estimated by rarefaction according to species accumulation across the three replicate transects taken at each site. We used the "chao1" function in the fossil package in R \citep{vavrek2011fossil} to calculate abundance-based Chao's Species Estimator \citep{chao1987}. Abundance of exotic species was calculated as the number of exotic individuals divided by the total number of individuals counted at each site.” * Updated results subsection “Environmental drivers of variation in species richness” to reflect updated results. * Updated Figure 2 to reflect updated results. * Updated Discussion to remove emphasis on hydrology as the master variable controlling vegetation composition / diversity (first paragraph). * References in the discussion to the results of the species richness analyses have been updated to reflect the new results (3rd and 5th paragraphs of Discussion). * Added comment about the inability of the combined environmental model to explain substantial variation in functional diversity (as compared with in unmodified environments studied in Chapter 2): “Competitive interactions may play a more important role in assembly of diverse subtropical plant communities than in more austere environments dominated by abiotic forces \citep{callaway1995positive}. Indeed, as is characteristic of subtropical forests, many of the species identified in this study were not obligate riparian species (James et al., in review) and could not necessarily be expected to display traits associated with adaptation to the riparian environment.”   Chapter 6 (general Discussion) has also been updated to reflect the changes to Chapter 4.   * 6th paragraph of subsection “Ecological responses of riparian plant communities to hydrology” has been updated to summarise the updated results. * 1st paragraph of subsection “Could environmental flows be a useful tool for river rehabilitation in south-eastern Australia?” updated to reflect an additional relevant result (contingency of maximum flows in addition to contingency of minimum flows)   Appendix 3b updated to reflect updated results. | - |
| Merritt | 8 | Incorrect citation. | This was an error in my citation management system. Fixed. |  |
| Merritt | 9 | Reference request (Stromberg 2007) | Added reference. |  |
| Merritt | 8 | Reference request (Poff 2007) | Added reference. |  |
| Merritt | 22 | Typographical suggestion (apostrophise ‘plants’ in Abstract) | Appears not to be a valid suggestion. |  |
| Merritt | 29 | “How representative were the selected study sites? To what area can the inference be made?” | Refer to the top of page 29 “These are the best represented flow classes in eastern New South Wales and Victoria” |  |
| Merritt | 31 | “Why was heartwood used and not functional xylem?” | I acknowledge that cavitation resistance is better associated with sapwood characteristics, but most environmental gradient studies of wood density have been conducted on heartwood. See Chave et al. 2009 “Towards a worldwide wood economics spectrum” |  |
| Merritt | 45 | Discrepancy between value quoted in Chapter 2 Fig 4 and in caption. | Damn, this is in the published version too! The correct value is 80.3. Fixed for thesis version. |  |
| Merritt | 48 | Request for more citations to support statement “the exact role that woody fibres play in stabilising xylem vessels appears to be inconsistent (Martinez-Cabrera et al., 2009). | Added Larjavaara reference (2010) “Rethinking the value of high wood density”, as well as canonical Hacke 2001 reference for contrast. |  |
| Merritt | 48 | Citation request (Horton et al. 2001). | Added citation. |  |
| Merritt | 50 | Request for family names of *Casuarina* and *Tristanopsis* | Added family names. |  |
| Merritt | 51 | Suggested addition to sentence | “High wood density species tended to occur further up the bank, so would be subject to only the more intense flooding events.” >> “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.)” |  |
| Merritt | 53 | Typographical error | “continents climate patterns” >> “continent’s climate patterns” |  |
| Merritt | 70 | “Metrics of functional trait diversity are more powerful than taxonomic metrics as indicators of ecosystem functioning, ecosystem resilience” – comment that quoted text seems overstated and should come after results. | I disagree that this is an overstatement. The statement is representative of the content of these references and is a key point in the argument for the relevance of the current study. |  |
| Merritt | 71 | Reference (Diaz et al. 2007) did not have full author list. | Fixed. |  |
| Merritt | 72 | Suggested reference (Merritt 2010) | Suggestion appears to be inappropriate. |  |
| Merritt | 77 | Request for detail about interpolation of hydrological data. | “Missing data were approximated by multiple linear regression (4 sites) and linear interpolation (1 site) using the Time Series Manager module in the River Analysis Package (Marsh et al., 2003).”  >>  “Missing data were approximated by multiple linear regression and linear interpolation using the Time Series Manager module in the River Analysis Package \citep{marsh2003river}. Consistency of the resulting outputs were checked by visual  inspection of hydrographs. For Mammy Johnsons River, Mann River, Sportsmans Creek and Wallagaraugh River, multiple linear regression was chosen as the most  appropriate method for estimating missing data values. Linear interpolation was used for Jilliby Creek data.” |  |
| Merritt | 81 | Request for clarification why only species present at >1% were included in the analysis. | “Only species present at >1% cover in plots were included in the analysis”  >>  “Due to data limitations, only species present at >1% cover in plots were included in the analysis” |  |
| Merritt | 86 | Request for p values and R2 values for linear regressions. Request for explanation of outliers. | P values and R2 values are provided in the text of the Results section. See Discussion, last paragraph of page 98, beginning “Two sites had anomalous values for FDis that do not fit within this conceptual  model of disturbance and low variability providing high niche heterogeneity.” |  |
| Merritt | 94 | “To our knowledge this is the first study to examine relationships between hydrological conditions and the diversity of ecological strategies within riparian vegetation communities using multiple quantitative functional traits.” – this is not true. | Removed sentence |  |
| Merritt | 122 | “According to classical niche-based theories of species co-existence e.g. \citet{Chesson2000}, where each niche is associated with an optimal ecological strategy” – Merritt says it sounds like niche is not being interpreted in the conventional sense. | I disagree with this statement. |  |
| Merritt | 124 | Request for an extra reference to be added to statement. | While the effects of individual dams tend to be idiosyncratic  (Mackay et al., 2014), flow regulation typically homogenises hydrographs by removing  small-moderate flows, reducing flood peaks, altering seasonality and increasing  predictability of flows (Graf, 2006; Singer, 2007).  >>  While the effects of individual dams tend to be idiosyncratic  (Mackay et al., 2014), flow regulation typically homogenises hydrographs by removing  small-moderate flows, reducing flood peaks, altering seasonality and increasing  predictability of flows (Graf, 2006; Singer, 2007; Poff et al. 2007). |  |
| Merritt | 136 | Request for cross-validation of imputed data. | A table of error estimates for imputation of missing data has been added to Appendix 3a. |  |
| Merritt | 139 | “The use of Cailliez’s correction to make a Gower similarity matrix Euclidean needs explanation and a citation. Why was this step necessary?” | “Gower’s method, which scales traits by their range, was used to generate the required dissimilarity matrix, and Cailliez’s correction was applied to render the matrix euclidean.  >>  “Gower’s method, which scales traits by their range, was used to generate the required dissimilarity matrix, and Cailliez’s correction was applied to allow for PCoA axes corresponding to negative eigenvalues and render the matrix Euclidean \citep{Cailliez1983}.” |  |
| Merritt | 140 | “Instead of saying ‘where required’, something like ‘to more closely comply with the assumptions of statistical tests’ | Change made. |  |
| Merritt | 141 | It is common to first run a 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 1 statistical error. | A number of methods are possible for reducing the dimensionality of large, multivariate datasets. I believe I have addressed the issue of non-independence and Type 1 statistical error by using a variance partitioning approach, which facilitates model selection according to whether independent variables explain variation in the dependent variable independently of other independent variables. This allows for selection of the most parsimonious model. I also trialled model selection methods based on Akaike’s Information Criterion, which gave relatively similar results. |  |
| Merrtt | 149 | Request for axis labels to be spelled out rather than abbreviated in figures. | I did try this and unfortunately I could not get the figures to look neat. The axis labels are expanded in the captions. |  |
| Merritt | 162 | Request for citation. | “It is notable that flow modification was not significantly associated with exotic abundance, given that altered flow regimes have been linked to invasion in previous studies of regulated Australian river systems \citep{Catford2011, Greet2012c}. It may be significant that while these studies found flow modification exacerbated invasion primarily by herbaceous species,”  >>  “It is notable that flow modification was not significantly associated with exotic abundance, given that altered flow regimes have been linked to invasion in previous studies of regulated Australian river systems \citep{Catford2011, Greet2012c} ( although see \citet{Merritt2010}). It may be significant that while these former studies found flow modification exacerbated invasion primarily by herbaceous species,” |  |
| Merritt | 181 | Request for citation that riparian vegetation is often dominated by keystone species. | This is a personal observation. |  |
| Merritt | 181 | Request for elaboration on “Alteration of traits reflecting economic trade-offs is of particular significance at the seedling stage, as functional traits of trees are most strongly adapted to the regeneration niche \citep{Poorter2007}.” | “Alteration of traits reflecting economic trade-offs is of particular significance at the seedling stage, as functional traits of trees are most strongly adapted to the regeneration niche \citep{Poorter2007}.”  >>  Alteration of traits reflecting economic trade-offs is of particular significance at the seedling stage, as functional traits of trees are most strongly adapted to the regeneration niche, because of the high selection pressures in the seedling stage \citep{Poorter2007}. |  |
| Merritt | 189 | “Is specific leaf area meaningful in seedlings” | Yes, according to Poorter 2007 (and in fact may be more meaningful in seedlings than in adult plants). |  |
| Merritt | 190 | Comment that Type III sum of squares is more appropriate than Type II sum of squares analysis when sample sizes differ. Request for rationale for setting alpha = 0.1 | Lansgrud (2003) conducted a simulation study to investigate the use of Type II vs Type III sum of squares in ANOVA where sample sizes differ and concluded that Type II is associated with less bias.  The rationale given for setting alpha = 0.1 is given by the low sample size (n = 4). Alpha = 0.05 at n = 4 would be associated with a high Type II error rate.  Statistical significance was thresholded at alpha = 0.1 for photosynthetic rate, stomatal conductance and WUE measurements (n = 4) and 0.05 for all other measurements (n = 8).  >>  Statistical significance was thresholded at alpha = 0.1 (due to small sample size) for photosynthetic rate, stomatal conductance and WUE measurements (n = 4) and 0.05 for all other measurements (n = 8). |  |
| Merritt | 214 | dispersion of wood density >> dispersion of wood density | Fixed |  |
| Merritt | 223 | Request for p value for Pearson correlation | Queensland dataset (Pearson's r = 0.75, p = 0.00000002) |  |
| Merritt | 223 | Typographical error: citepd >> cited | citepd >> cited |  |
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| Stromberg |  | General request for a more robust biophysical description of study systems. | I have added an Appendix (3) which provides key details of study sites as well as including photographs as requested. |  |
| Stromberg | 22 | “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 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 wood density within the floodplain community. I would like to have seen more discussion of the evolutionary significance of rare events, and more information on the 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? | The statement referenced by the examiner in the Abstract is “Large, rare flood events in particular appear to favour higher wood density strategies.” I was referring here to apparent association between the magnitude of these flood events on the community weighted mean of wood density (on which the analysis was done), rather than on specific species. *Casuarina cunninghamiana* and *Tristanopsis laurina*, which are the obligate riparian species referred to here, in fact demonstrate substantial intraspecific variation in wood density. Figure 1 in Appendix 1 shows that intraspecific variation in *C. cunninghamiana* responds over various hydrological gradients. In paragraph 2 of the Discussion of Chapter 2, I discuss this effect. Thus although the mean wood density value for these species sits towards the middle of the wood density spectrum, their wood density is higher at sites which experience harsher hydrological conditions, and their high abundance pushes the community weighted mean wood density up.  As the article is already published in the Journal of Ecology, for which it underwent an extensive process of peer review and multiple stages of revision, I have limited alterations in response to these comments to several clarifications.  The return time of the flood events in question is already described (10 to 20 year average return interval).  I have added text (in bold, below) to the discussion of Chapter 2 (paragraph 3) to comment on the evolutionary significance of these flood events. This comment also addresses Stromberg’s 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 this case, Stromberg is referring to the effects of much larger, catastrophic floods.   * “A pattern is apparent then, in which mean wood density in riparian communities is driven by powerful but relatively rare flow events (e.g. 10 to 20 year average return interval flood). **Such floods are likely to be large enough to influence demographics, but not necessarily catastrophic (i.e. presenting no opportunity for survival)**.”   In response to the request for further biological context, I have added the following sentence (in bold):  “We therefore suggest that a ‘brick house’ ecological strategy is favoured in riparian environments that experience intense flooding. This suggestion concurs with findings that trees on windy slopes tend to overcompensate for mechanical stress, with investment in defences increasing cumulatively in response to stress events \citep{telewski1995wind, Cohen1999}. **This cumulative effect may be important for long-lived woody species which experience multiple high magnitude floods during their lifetimes.**” |  |
| Stromberg | 95 | Request to provide detail about criteria for site selection (for which the reader was previously referred to Chapter 2).  “Given that the chapter is about functional diversity, did a portion of diversity go undetected because of the experimental sampling design” | Text added (in bold):  “Fifteen riparian sites were selected along gauged, partly confined rivers in the South-East Coast and south-eastern Murray Darling drainage basins of Australia. These sites were distributed across clear gradients of ecologically relevant dimensions of hydrological variation: specifically, the magnitude, frequency, duration, timing and rates of change of flow events and patterns. The study area spanned latitude -29.467 to -37.371\textsuperscript {o}S and longitude 147.413 to 152.217 \textsuperscript{o}E. Sites spanned an altitudinal range of 23 - 732 m above sea level. Site-specific details can be found in Appendix 2a. **Criteria for selection of plot locations were: geomorphic homogeneity (the plot comprising only sloping bank where possible) and lack of anthropogenic disturbance such as built structures, roads or tracks, recent logging or clearing (in the last 20-30 years), herbicide spraying or animal grazing. Variation in the maximum height above the channel edge between plots was kept to within approximately 1.5 m.** Full description of site selection criteria and vegetation survey methods can be found in \citep{Lawson2015}, as this study was undertaken simultaneously and at the same sites.”  It is possible that some diversity did go undetected due to the sampling design, but I made explicit the assumption under which I made my arguments in the discussion:  “Because we surveyed geomorphically homogeneous sections of sloping bank, our argument is presented under the assumption that functional diversity is a property of riparian communities at the reach scale. Influx of species from more physically complex adjacent patches, then, is responsible for the diversity we observed on these geomorphologically homogeneous sloping bank sections.” |  |
| Stromberg | Chapter 4 | Dissatisfaction with wording of hypotheses. | I have reworded my set of hypotheses following the examiner’s suggestion:  “To this end, we used a functional trait diversity approach to examine vegetation responses to hydrological alteration in a modified landscape in south-east Queensland, Australia. Our aim was to tease apart the environmental factors associated with taxonomic and functional diversity and the abundance of exotic species in riparian plant communities. A set of hypotheses about environmental heterogeneity – diversity relationships guided our approach: 1a.) species richness and functional diversity increase and abundance of exotic species decreases monotonically with increasing hydrological heterogeneity; 1b.) species richness, functional diversity and abundance of exotic species show unimodal relationships with hydrological heterogeneity; 2.) species richness and functional diversity decrease and abundance of exotic species increases along gradients of increasing flow modification and catchment land-use intensity.”  >>  “To this end, we used a functional trait diversity approach to examine vegetation responses to hydrological alteration in a modified landscape in south-east Queensland, Australia. Our aim was to tease apart the environmental factors associated with taxonomic and functional diversity and the abundance of exotic species in riparian plant communities. A set of hypotheses about environmental heterogeneity – diversity relationships guided our approach: 1.) species richness and functional diversity increase monotonically as a function of hydrological heterogeneity; 2.) abundance of exotic species declines monotonically with increasing hydrological heterogeneity, assuming greater niche heterogeneity retards competitive exclusion; 3.) species richness, functional diversity and abundance of exotic species show unimodal relationships with hydrological heterogeneity, due to microfragmentation and intermediate disturbance-type effects; 4.) species richness and functional diversity decrease along gradients of increasing flow modification and catchment land-use intensity, as an outcome of environmental homogenisation; 5.) abundance of exotic species increases along gradients of increasing flow modification and catchment land-use intensity, as an outcome of environmental homogenisation.”  I have also reworded paragraphs in the Results and Discussion of Chapter 4 which refer to these hypotheses, and in the general discussion (Chapter 6). Changes are not described here as they are simple substitutions of numbers/letters. |  |
| Stromberg | Chapter 4 | “I was concerned about 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 authors 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.”  “To bolster the botanical foundation of the study, more information should be provided on which taxonomic groups were and weren’t sampled.” | Unfortunately I did not collect the vegetation survey data and was not involved in the design of the survey. I have spoken with my collaborator (Cassandra James) about this issue, however. Due to the high degree of human modification of riparian landscapes in the study systems, sampling area was typically constrained by stark land-use boundaries.  I have reworked the analysis to use a rarefaction-based estimation of true species richness, as per examiner Merritt’s suggestion (see above comments), and I believe this satisfactorily addresses the issue of sampling area variability.  I have updated the text to clarify that all vascular plants were surveyed.  “All **vascular plants** within a 5 m band centred on the transect line were identified and counted. Species identifications were confirmed by the Queensland Herbarium.” |  |
| Naiman |  | “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.” | I have provided an additional Appendix (3) containing information about biophysical characteristics of the study sites, including:  Biogeography:   * IBRA region, Koppen climate zone, mean annual rainfall, mean annual temperature, elevation   Vegetation and site history:   * Canopy height, vegetation structure, dominant species   Photographs representative of the three flow classes (sensu Kennard et al. 2010) from which the study sites in Chapters 2 and 3 were drawn. |  |
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**Part B: Confirmation by the Principal Supervisor**

**(to be completed by the supervisor and endorsed by the Head of Department and the Associate Dean (HDR) of Faculty)**

**To: Higher Degree Research Committee.**

I confirm that I have seen the corrected copies of the thesis by the above mentioned student with the title of:

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I have read the reports of the examiners, and I can confirm that the candidate has completed the corrections and/or amendments as required by the examiners and the Higher Degree Research Committee.

I recommend that the thesis now be accepted and passed.

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| Signature | Signature | Signature |
| Name | Name | Name |
| **Supervisor** | **Head of Department** | **Associate Dean (HDR) of Faculty** |