

Round #3

by Karl Cottenie, 2020-03-09

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This preprint merits a revision

The author addressed most of the comments satisfactorily. There is only one sticking point left, regarding the effect of site selection on the inference possible.

At the end of the introduction, the author writes: "I used this hypothesis to guide and inform site selection in my re-analysis. I required sites in the re-analysis to have received both the bank stabilization and in-channel manipulations treatments (hereafter "revised" sites), although sites receiving riparian reforestation were also included if they received both the bank stabilization and in-channel manipulations treatments."

This means that the population from which the samples are drawn from, and thus what influences the inference, are all sites that received both bank stabilization and in-channel manipulations, period. Since reforestation is not part of the site selection criteria, the author cannot make any inference about reforestation.

This is similar to a bowl of 1000 marbles that have 3 colors (red, blue, and black), and that are either small or big. If you randomly select 10 red or blue marbles (but not black ones) from the bowl, and 8 of the selected red or blue marbles are small, your inference will be about the population of red and blue marbles, not about the black marbles, or about the small marbles. Maybe the black ones are big marbles, but because you did not select these, you will not be able to test that.

Response: I do not agree with this argument, because it is simply incorrect. It is true that I cannot make direct inference about reforestation; however, I cannot make any direct inference from the re-analysis about any single restoration treatment because it was not a factorial design (as has been noted in the manuscript). I am instead testing and discussing restoration in a more general sense. Having criteria for site inclusion does not preclude discussion and inference about the efficacy of restoration.

My site selection criteria made sure sites received at least the bank stabilization and in-channel manipulation treatments. Sites were still included if they received the riparian reforestation treatment as well. As I similarly noted in the previous round of reviews, 7 out of the 9 (~78%) revised sites received all three restoration treatments, while only 7 of the 13 (~54%) sites in the original study and full sites analysis received all three restoration treatments. Below is a table showing the distribution of restoration treatments between the original study and my re-analysis, both full and revised sites.

Stream	Full Sites			Revised Sites		
	Bank Stabilization	In-Channel Manipulation	Riparian Reforestation	Bank Stabilization	In-Channel Manipulation	Riparian Reforestation
Headwaters						
24				N/A	N/A	N/A
191						
227						
265						
SR						
Mainstems						
18						
19						
21				N/A	N/A	N/A
179				N/A	N/A	N/A
196				N/A	N/A	N/A
222						
289						
MB						

Note: Shaded cells represent the restoration treatments that were applied to each stream, with full sites in blue and revised sites in green. Sites excluded in the revised sites analysis are denoted with N/A.

As can be seen from this figure, there is considerable variation in the number and type of restoration treatments applied to streams in the full sites analysis. In contrast, the revised sites has more consistent application of restoration treatments, as intended by the site inclusion criteria.

I don't know what the range of restoration practices are, but out of all the possible ones, the author only selected the bank stabilization and in-channel manipulations restoration practices. That means, similarly to the marbles, that the population this analysis allows inference for is the population of bank stabilization and in-channel manipulation experiments. To call that subset "restoration" is not statistically correct, I think. Or that it reflects reforestation practices. Thus, the first sentence of "Effectiveness of local restoration" that reads: "I hypothesized that restoration would have stronger effects in headwaters relative to mainstems." is incorrect. I do think that the author should replace every instance of "restoration" with "bank stabilization and in-channel manipulations restoration practices". Further in the discussion, the author can then potentially make the case that these are the most common restoration practices, or the most effective ones, or some other argument to convince the reader that the analysis applies to restoration in general, but the author will have to make it very explicit that this is speculation, that is not necessarily supported by the data analysis.

Response: This criticism is either incorrect or simply a misunderstanding. I selected bank stabilization and in-channel manipulations as two conditions that must be met for inclusion in the

revised sites analysis; I still included sites that also received reforestation treatments because it was impossible to exclude that treatment from the analysis.

I do not understand the argument against my discussion and use of restoration, and I will not concede this point. Just to talk about this study, there were three possible restoration treatments: bank stabilization, in-channel manipulations, and riparian reforestation. The distribution of how treatments were applied is in the table above. As shown in that table and noted in the previous round of review, the revised sites had greater representation of sites receiving all restoration treatments (revised = 7/9 or ~78%, full = 7/13 or ~54%). To follow the logic of your analogy, the revised sites subset still has all three restoration treatments represented. I am not losing one of the treatments (black marbles), my revised sites sample still has all restoration treatments (blue, red, and black marbles), in better representation across sites than the full sites analysis, I will add, just fewer sites (so, say 800 marbles instead of 1000). There is no loss of actual restoration representation, the only loss is of sample size and therefore statistical power. Because I still have all treatments, including riparian reforestation, represented in the population, discussion and inference about restoration is possible and valid. From the above comments, saying that the inclusion of riparian reforestation is what makes discussion and inference about restoration is arbitrary; there is no logical distinction between saying just the two channel manipulation treatments prohibits discussion of restoration, when if they just happened to have reforestation (which the majority do) that would allow discussion of restoration efficacy. Reforestation does not make or equal restoration, but going into a system, manipulating the local habitat under the intention of ‘restoring’ it to a more natural or reference state is restoration. Given the treatments applied to this system were designed to restore the streams, I argue that my statement of restoration being stronger in headwaters than in mainstems is correct. For example, if I were to change this statement to “the effects of channel manipulations were stronger in headwaters”, this would mislead the reader because reforestation treatments were still represented in the sample, the statistical analyses, and eventual inference. This same issue would apply to all instances where it has been suggested I change “restoration” to “bank stabilization and in-channel manipulations restoration practices”: reforestation treatments were still applied to the majority of these sites, and this would be a misleading and incorrect statement. What constitutes restoration can be subjective, but to say I am restricted to only discussion of channel manipulation treatments does not accurately reflect the data going into the analyses, the results of the analyses, and broader discussion and inference.

I used bank stabilization and channel manipulation treatments to guide my site selection in the re-analysis and to reduce variation in the number and type of restoration treatments applied. I did not and could not exclude reforestation from the analysis, and I cannot discuss the re-analysis and discussion of restoration as if I only looked at bank stabilization and in-channel manipulations, because that would not be an accurate reflection of the study/re-analysis.

When the author writes, correctly: "the hypothesis was intended to guide criteria for site selection and reduce variation in restoration treatments among sites and not to necessarily or strictly compare the effects in-channel manipulations and riparian reforestation treatments on biodiversity in restored streams." The flip side of the reduced variation in restoration treatments among sites is that the population sampled is different, and smaller. I think that this has to be explicitly acknowledged in the manuscript, and that the correct words have to be used.

Finally, I think that the section "Statistical inconsistencies" has indeed been toned down enough. The author writes in the response to the comment to "tone it down little" that " There is an assumption that what researchers write in their manuscripts is an honest representation of the study, but that assumption was broken and that implicit trust between reader and researcher was lost." I have an almost completely opposite view on the original authors' actions: they were extremely open about what they did and how they did it. They did provide all the code and all the data. That they provided this information when asked does not diminish that they did provide all of this, without which this manuscript would not have been possible. Did they maybe make some mistakes, yes, could they have analysed the data better, yes (see the resulting manuscript). But I am pretty sure that is the case for a lot of published papers, mine included, without there being any, I benefited from reading some of the blog posts from Stephen Hearst on the function of the Methods section in scientific papers, especially <https://scientistseessquirrel.wordpress.com/2015/02/27/reproducibility-your-methods-section-and-400-years-of-angst/>, and maybe that might help the author too.

Response: I had stated how the analysis lacked sufficient statistical power to detect the effect (lines 236-237); however to say the sampled streams are somehow "different" is misleading. The difference between the full and revised sites was the inclusion criteria (i.e. having both bank stabilization and in-channel manipulation treatments), but all three of the restoration treatments were still represented in the full sites 'sample' and the revised sites 'sample.' The only difference is the number of streams, grouped by order, in the full sites and revised sites analyses.

As for the disagreement over how the original authors conducted themselves, we are free to disagree. I have personal communications between myself, the original authors, and the handling editor at Ecological Applications that I cannot and will not share that illuminate a different story, but that correspondence must remain private has all parties have not agreed to release those communications. I do not think changing analytical methods without noting the change in text in the manuscript is part of being extremely open, but my philosophical approach to science just seems to differ.

On a final note, the blog post by Stephen Heard was indeed interesting.