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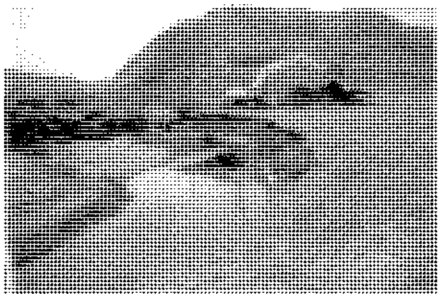

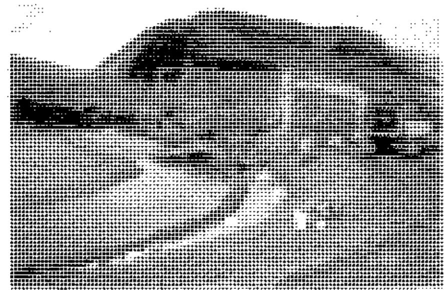

Streambank Restoration Effectiveness: Lessons Learned from a Comparative Study

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ABSTRACT: Post-treatment effectiveness monitoring should be an integral part of stream restoration efforts, but it is often neglected due to lack of funds or insufficient project planning. Here we report results of an effectiveness evaluation of a streambank restoration program for salmon streams in the southern interior of British Columbia. Restoration involved treating eroding riverbanks with bank grading, riparian plantings, and installation of rock toes, rock-wood current deflectors, and livestock exclusion fencing. Absence of pre-treatment site characterization data necessitated comparing post treatment conditions at treated sites to conditions at untreated eroding control sites. We measured in-channel and riparian conditions plus invertebrate abundance and biomass at 16 sites treated between 1997 and 2002 and 11 nearby control sites. Treatment and control sites did not substantively differ in their habitat condition or aquatic macroinvertebrate abundances, although treated sites tended to have more shrubs along the outside bank, higher inside banks, and narrower wetted widths. Absence of statistical differences between treatment and control sites might be due to low statistical power, as >50 sites per group would need to be sampled for power to reach 0.8 at the effect sizes observed. Site specific channel gradient, a variable unaffected by restoration actions, was correlated with many of the variables we measured to characterize habitat condition, thereby confounding our ability to determine the magnitude of change relating to treatment efforts. Our results demonstrate the weaknesses of relying on a post-treatment, between-group comparison experimental design for restoration effectiveness monitoring. We suggest collection of pre-treatment data should be an essential part of the restoration process so more appropriate "before-after" experimental designs can be applied.

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RESUMEN: [illegible]

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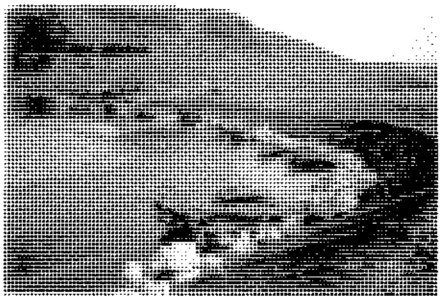
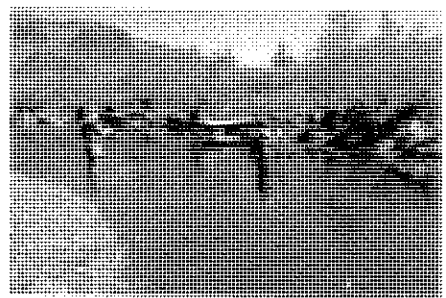




INTRODUCTION

Habitat management, including habitat restoration, entails applying one or more treatments and should be viewed as an experiment which necessitates post-treatment evaluation (Kondolf and Micheli 1995; Kershner 1997; Michener 1997; Palmer et al. 2005; Stem et al. 2005; Woolsey et al. 2007). Michener (1997) suggested the theoretical optimum for restoration effectiveness monitoring as "long-term monitoring of salient patterns and processes in adequately replicated control and experimental units at appropriate spatial and temporal scales using sound sampling design and statistical analyses." However, Michener concedes this optimum is rarely achieved and often unachievable. Unfortunately, any amount of systematic monitoring of the results of freshwater habitat management efforts remains an exception, not the rule (Kondolf 1998; Pretty et al. 2003; Quigley and Harper 2006; Reeve et al. 2006).

Limiting factors precluding efficient post-treatment evaluation often originate from insufficient pre-project planning. For example, many projects fail to incorporate effectiveness monitoring into the initial project budget and evaluation is therefore abandoned due to lack of funds (Reeve et al. 2006). Similarly, restoration practitioners often fail to provide a clear statement of project goals, and therefore effectiveness monitoring has no criteria on which to judge project success or failure (Kondolf 1995; Palmer et al. 2005; Stem et al. 2005). In other cases, projects fail to collect appropriate pre-treatment data, which precludes a before-after experimental design or its derivatives such as before-after-control-impact (BACI; Green 1979; Walters et al. 1988; Roni et al. 2005) and typically forces reliance on less powerful post-treatment between-group comparisons (Mellina and Hinch 1995; Bryant et al. 2004).

Beginning in the 1990s, the Habitat Management Unit for the Southern Interior of British Columbia of Fisheries and Oceans Canada and its local partners initiated an eroding streambank restoration program for tributaries of the Thompson River system. The three explicitly stated goals were to stop bank erosion, increase native salmonid production, and foster a stewardship mentality within the local community. Between 1992 and 2005, >200 eroding banks, spread across 5 valley floor mainstem rivers, had been treated. By 1997, largely via learning from past structural failures, treatment methods had evolved to a standard template involving bank grading, riparian plantings with willow (*Salix* spp.) cuttings, livestock exclusion fencing, and installation of a rock toe coupled with site specific mixtures of tree and/or rock current deflectors, bank contouring, and occasional plantings of deciduous (primarily *Populus balsamifera* and *Betula papyrifera*) and/or coniferous (primarily *Pseudotsuga menziesii*) trees. Pre-treatment data characterizing site conditions were not collected at any of the treated sites.

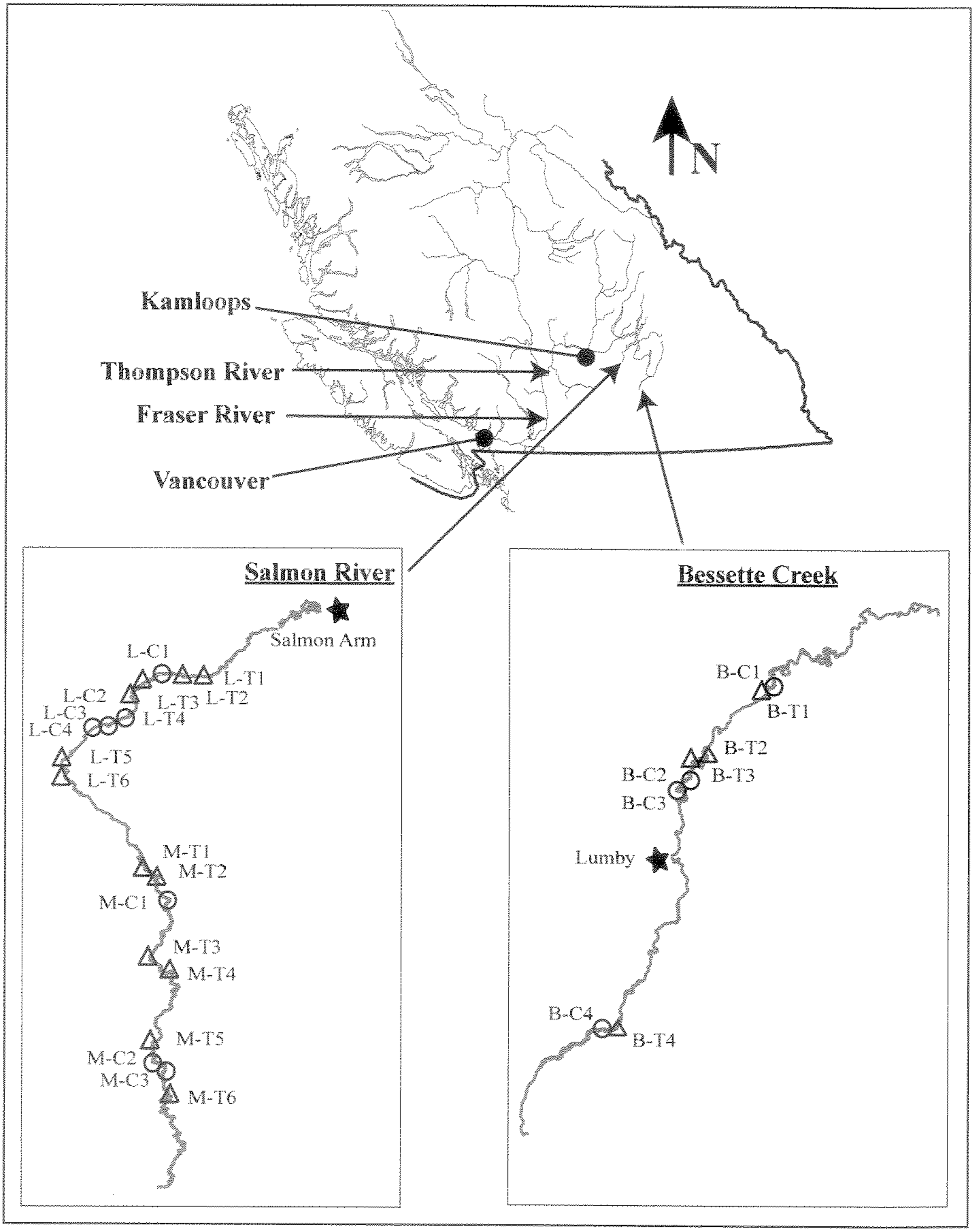
A visual survey of project structural integrity conducted in 2005 found that all of the 81 streambank restoration projects constructed along the Salmon River since 1997 had structural integrity ratings of "adequate" or better, equating to no evidence of physical failure, and that all were accomplishing their proximal goal of erosion control (S. Bennett, unpublished data). Although not explicitly quantified, structural and functional integrity of similar projects in nearby watersheds, including the approximately 20 Bessette Creek projects completed to date, also appeared to be consistently good (M. Cooperman, pers. observ.). None of the structural integrity assessments evaluated ecological effects of the bank restoration efforts.

In this article, we report results of an extensive post-treatment effectiveness evaluation of streambank restoration efforts in the Salmon River and Bessette Creek. We compare stream channel and riparian vegetation condition and aquatic invertebrate abundance and biomass at 16 sites "treated" between 1997 to 2002 to those at 11 actively eroding "control" sites. We hypothesize that relative to the control sites, treated sites would have greater in-channel habitat diversity, higher depth-to-width ratio, larger streambed mean particle size, greater riparian zone plant coverage on both banks of the channel, greater amounts of natural vegetation recruitment on point bars opposite treated banks, and greater aquatic macro-invertebrate abundances. Because our assessment was based on between-site comparisons, not before-after comparisons of individual treated sites, we also evaluated the nature of site-to-site variability and how this variability related to, and potentially influenced, site-specific conditions and response to restoration. We discuss our results in the context of the limitations of an extensive post-treatment experiment design for effectiveness monitoring and provide suggestions for future restoration monitoring efforts.

STUDY SITES

The Salmon River and Bessette Creek occupy the "interior Douglas fir—very hot—dry" biogeoclimatic zone of British Columbia (Lloyd et al. 1990), and drain to the Pacific Ocean via the Thompson River sub-basin of the Fraser River watershed (Figure 1). Valley floor elevations range between 350–500 m above sea level, annual mean precipitation is 400–500 mm, and soils consist of a blanket of poorly sorted moraine deposits within a matrix of sand-silt-clay with limited fluvial reworking (Lloyd et al. 1990). Timber harvest and irrigated agriculture-ranching are dominant land uses in both watersheds and almost all valley floor land is privately held in agriculture. In 2002, the Salmon River experienced the third highest peak discharge of the 31-year period of record (49.2 m^3/s; Water Survey of Canada station 08LE021) and Bessette Creek experienced the ninth highest peak discharge

Figure 1. The top map shows the Fraser River watershed in the lower half of British Columbia, Canada (scale 1 cm = 125 km) and the location of the Salmon River and Bessette Creek in the headwaters of the Thompson River sub-basin. The lower maps show the distribution of study sites along the two rivers. Scale for the Salmon River map is 1:100,000 and for Bessette Creek 1:50,000. Treatment sites are triangles and control sites are open circles. Study site nomenclature is described in the text..



in its 32-year period of record (32.3 m^3/s; WSC station 08LC042), indicating all treatment sites included in our study had experienced a high discharge event post-treatment and therefore had the potential to display a geomorphic response to treatment. Mean discharge during our field effort was 2.09 m^3/s in Salmon River and 2.50 m^3/s in Bessette Creek.

Miles (1995) estimated >40% of the forest cover of the Salmon River watershed has been harvested since the early 1900s, that approximately 20% of the mainstem was actively eroding, that the channel ranged from 11–211% wider than it was in the 1930s, and that in the lower 60 km of the river (the area where our study occurred) 50% of the channel had either no riparian vegetation or a riparian band less than 1 channel width wide, in contrast to abundant and well-dispersed riparian-gallery vegetation of the 1930s. Quantitative land use and impact data for the Bessette Creek watershed are not available but are assumed to be similar to those of the Salmon River owing to the close proximity of the two watersheds and similarities in general land use patterns. Sediment supply and movement through these two systems have not been studied, but are assumed to be high owing to numerous eroding banks and the rapidity with which constructed in-channel habitat structures are buried (S. Bennett, pers. observ.).

METHODS

Study site selection

Restoration activities have been conducted on about 100 eroding banks of the Salmon River and on approximately 20 eroding banks along Bessette Creek since the mid-1990s. To qualify as a treatment site in our study, the following was needed: (i) restoration activities occurred between 1997–2002 inclusive, (ii) restoration activities involved the outside bank of a channel meander of the mainstem channel on the floor of an unbound alluvial valley segment, (iii) <5% of the length of the as-built project could displayed evidence of physical failure (failure of riparian plantings was not included in this physical failure criterion), (iv) site-specific actions must have successfully accomplished the proximal goal of halting bank erosion, and (v) the site must not be directly influenced by civil engineering works, tributary inputs, other site-specific restoration activities that were unrelated to bank restoration (e.g., artificial riffles or channel re-configuration), or possess unique geological features such as local clay lenses or other erosion resistant inclusions. For inclusion as a control site, a riverbank needed to satisfy all applicable criteria above plus be actively eroding as evidenced by the face of the bank being unvegetated, near perpendicular to the water surface, and displaying signs of recent bank slumping. By limiting our study scope to only sites satisfying the above conditions, we are confident our control sites are a fair representation of pre-treatment conditions at the treated sites.

The technical coordinators of the Salmon River and Bessette Creek watershed roundtables identified 16 restored sites meeting our inclusion criteria. We located 11 eroding bank sites in proximity to the 16 treatment sites to serve as controls (Figure 1). For identification purposes, we assigned each site a unique alpha-numeric identification based on the first letter of the river segment it occurred within (L = lower Salmon River, M = middle Salmon River, B = Bessette Creek), a T or C for treatment or control, and a number. Although numbers were assigned sequentially upstream to downstream, sites were sampled in random order. Because treatment sites were located haphazardly along the river corridors it was impossible for us to assure either random or systematic random distribution of sites. Similarly, because each treatment site received a unique treatment prescription, it was impossible to have sufficient replicates within each treatment type to allow for evaluation based on groupings of specific restoration techniques. However, four treatment sites (L-T5, M-T2, B-T3, B-T4) had received notably more comprehensive restoration than other treated sites, including self-launching rock spurs and trenched rock toes coupled with multiple outward facing >1.5 m diameter root wad revetments embedded into a sloped bank with rock groins at the upstream and downstream ends of the treatments, allowing use to compare conditions at these four "intensive" treatment sites to conditions at the other 12 treatment sites.

At each location, the study "bank" was the portion of the outside meander bend that had either received restoration or was actively eroding. Each study bank occurred within a study "reach," defined as the portion of the channel lying between the upstream and downstream thalwag cross-over points bracketing a study bank. Figure 2 depicts how we organized study sites.

Channel condition

We conducted a habitat unit survey for each study reach following Bisson and Montgomery (1996). Length of a habitat unit was the longest axis, width was the widest point perpendicular to length, and depth was the deepest point. We pooled riffles and runs into "fast water," classified pools as "slow water," left glides as its own category, and determined proportion of a reach's total surface area within each habitat class. The proportion of each reach classified as fast and slow water was highly correlated (r = -.87, n = 27, P <0.001), so surface area as fast water was eliminated from subsequent analyses.

We followed Harrelson et al. (1994) to develop elevation profiles along each transect. Elevation surveys extended from 2 m outside the top of bank on each side of the channel and elevation was recorded at every 0.5 m along each transect with supplemental readings taken at top of bank, bottom of bank, and water's edge. We assumed depth equaled 2.0 m at any point too deep for safe surveying. Mean depth of a reach was the mean of all individual water depths.

We determined bankfull, active channel, and wetted widths along each transect. Active channel width was from top of the outside bank to the top of the first distinct slope change along the point bar of the inside bank. Owing to the long history of channel widening in these systems, along with our observations of large accumulations of living and dead plant material above this slope break, we interpreted the land between the slope break and the top of inside bank to be "incipient floodplain," which we considered part of the riparian portion of the fluvial system. Depth-to-width ratio of a reach was derived from mean reach depth and mean wetted width. We used wetted width because summer time water temperatures are a primary management concern and therefore it is the wetted portion of channel at base flow condition that is the variable of concern. Mean depth was significantly correlated with depth-to-width ratio (r = 0.90, n = 27, P <0.001), so mean depth was eliminated from subsequent analyses. Height of the inside bank at 1 m from the water's edge was the mean of transect specific differences in elevation between the high point within 1 m of the water's edge along the inside bank and the elevation of the water's edge on the inside bank.

We visually estimated the proportion of stream bed particles falling within particle size classes as in Harrelson et al. (1994; organic matter, silt, sand, gravel, small or large pebbles, small or large cobble, boulder) within an approximately 1 m^2 portion of the stream bed underlying the thalwag and along the inside bank point bar of each reach (Figure 2). We pooled gravel and smaller particles (β-axis <4 mm) into the category of "fine sediments" and all small pebbles and larger particles (β-axis >4 mm) into "coarse sediments" and determined the proportion of fine and coarse sediments within the two sites per reach. We limited subsequent analyses to proportion of fine sediments in point bars and proportion coarse sediments underlying the thalwag. Sediment data were not collected at L-T6 or M-T5.

Channel gradient was the difference in elevation between the upstream and downstream ends of a study reach, measured at the water's edge, divided by the distance between the two points as measured along the curvature of the water's edge along the inside bank.

Riparian assessment

We used a modified line intercept technique (McDonald 1980) along the same transect lines as the elevation survey to assess the coverage of riparian vegetation on both the inside and outside bank of each study reach (Figure 2). On the outside bank of the channel, vegetation survey started at the top of bank. On the inside bend, the vegetation survey began at the edge of the active channel. Riparian surveys extended 5 m from start points, were 1 m wide (0.5 m on each side of the transect line), and assumed to reach indefinitely upwards. For each tree or shrub of which any part of the plant entered the survey plane, we recorded the species and the length of the portion of plant within the survey plane. For the inside bank, we also tallied the number of seedlings (trees <0.5 m height) along the transect. We pooled the vegetation data from all transects on one side of the channel and determined proportional coverage by trees and shrubs, individually and combined. For example, in a case of a reach with 3 transects, the proportion outside bank covered by trees equaled the sum of the lengths of trees entering the survey planes of the 3 transects on the outside bank divided by 15 m (e.g., 3 transects, each 5 m long). For simplicity, we refer to our proportional coverage data as "coverage."

Channel condition and riparian assessments were conducted in July and August 2005, when discharge in the study systems had stabilized to summer baseflow, thereby limiting the influence of falling discharge on flow dependant values such as amount of habitat as fast or slow water. Whenever judgment was used during data collection (i.e., habitat unit survey, bankfull dimensions, sediment particle sizes), all determinations were exclusively made by the senior author, thereby standardizing measurement precision and eliminating among-crew estimation bias (Woodsmith et al. 2005).

Aquatic macroinvertebrates

During the first two weeks of September 2005, we revisited each study reach to collect six benthic macroinvertebrate samples using a Surber sampler (400 μm mesh, 0.5 m^2 quadrant size). We divided the downstream portion of the inside bank point bar at 1 m from the water's edge (e.g., same area as the point bar sediment surveys; Figure 2) into ten 1-m long intervals and used a random number table to select 6 of the 10 units for sampling. Samples were collected by agitating the surface substrate and all larger rocks and wood within the quadrant for one minute and samples were field preserved in 10% buffered formalin. We also collected 1 three-minute kick net sample per site (400 μm mesh) by moving the kick net across the channel in a upstream progressing zig-zag starting immediately upstream of the Surber sample locations. Because kick net and Surber samples support similar conclusions (Cooperman et al. 2006), we report only Surber sample results.

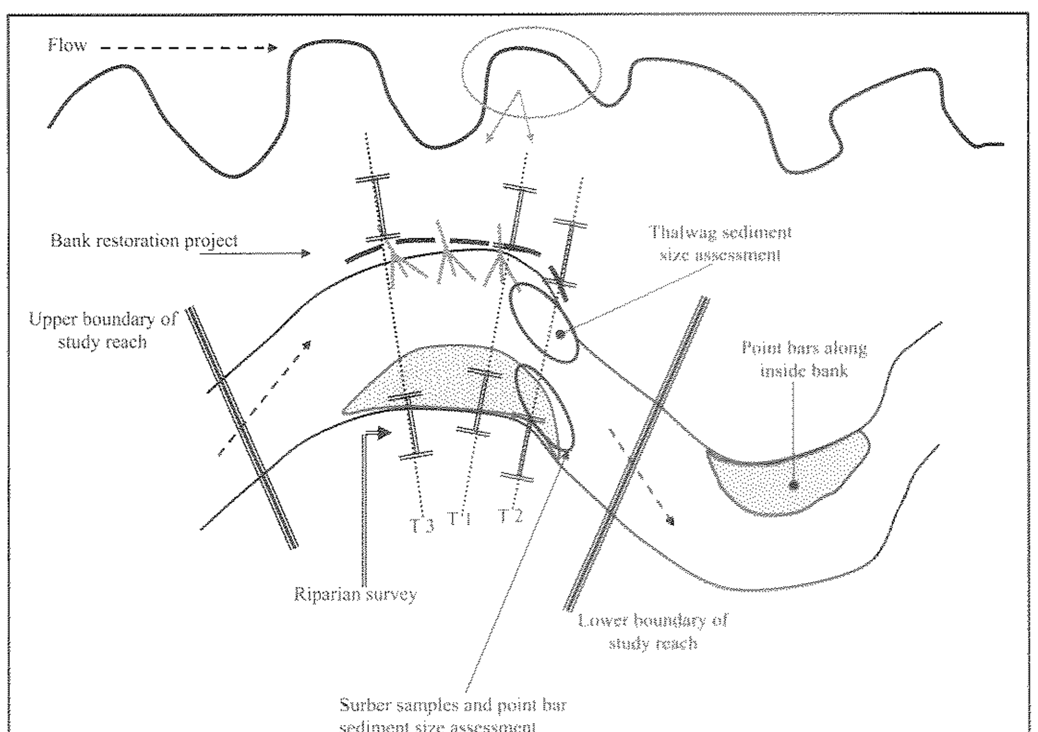


Figure 2. Schematic representation at a treatment site depicting location of three transects used for channel elevation and riparian surveys, upper and lower reach boundaries, and sediment and invertebrate sampling locations. Length of each study bank was that portion of the bank which had either received active restoration or was actively eroding. We established 3 transects for study banks up to 100 m long and 5 for banks >100 m. Transects extended across the active channel perpendicular to the thalwag. Transect 1 was always positioned at the point of maximum curvature and transects 2 and 3 were at 10% of the bank length inside the downstream and upstream ends of the study bank respectively. When applicable, transects 4 and 5 were halfway between transects 1 and 2 and 1 and 3 respectively. One control bank (B-C2) had the thalwag inflection point at the downstream end of the bank and therefore only transects 1 and 3 were established. Diagram is not to scale.

In the laboratory, we randomly selected two of the six Surber samples from each site for analysis. We picked all invertebrates under 3x magnification, identified them to order, and used the mean of the two samples per site to determine site-specific total number of macroinvertebrates, number of ephemoptera-plecoptera-trichoptera (EPT), total macroinvertebrate wet mass, and EPT wet mass. Because all possible pairings of the four response variables were significantly correlated (all P values <0.0433, n = 27), only total macroinvertebrate abundance was used in subsequent analyses.

DATA ANALYSIS

Preliminary results indicated that analyzing study sites of the three river segments of Bessette Creek, lower Salmon River, and middle Salmon River separately for differences between treatment and control sites supported the same conclusions as analyses of all segments pooled into a single population (Cooperman, unpublished data). Therefore we only report results stemming from the pooled data set.

Assessing effects of restoration activities

We used multivariate analysis of variance (MANOVA) as a single test for differences between treatment and control sites. The eight dependant variables entered into the MANOVA were HabUnits, P SA Slow, P SA Glide, D:W, Hgt IB, P TH Coarse, Cov T&S IB, #Recruits (see Table 1 for descriptions of these variables). We did not include outside bank plant abundances in multivariate analyses because the presence of plants on outside banks could be attributable to opposing mechanisms of successful establishment from restoration efforts or progression of an eroding bank into mature vegetation. We excluded P PB Fine from MANOVA due to a significant inverse relationship with P TH Coarse (r = -0.45, n = 27, P = 0.020).

We used one way analysis of variance (ANOVA) to facilitate interpretation of the MANOVA results and test specific predictions about differences between treatment and control sites (Table 1). In addition to treatment vs. control comparisons, we report the mean value of the four intensive treatment sites for each variable, but did not conduct statistical analyses of differences between intensive treatment sites and treatment or control sites due to small sample sizes. We followed ANOVA tests with a posteriori power analysis following Winer (1971) for each variable to determine the sample size needed to attain statistical power of 0.8 based on a one-tailed test at α = 0.1 and an effect size equal to the observed difference between treatment and control sites.

To test for differences in the erosion-deposition environments between treatment and control sites as a function of local channel gradient, we compared the slopes of regression lines resulting from independently regressing treatment and control site P TH coarse on gradient, and we repeated the treatment-control comparisons for the regression of P PB Fines on gradient. We tested the regression solutions for unusually influential points based on Cook's distance and DEFITS values and eliminated offending cases as needed.

We assessed differences in macroinvertebrate abundance (# Inverts) between treatment and control sites using two-way ANCOVA Analysis of Covariance with site (two levels—treatment or control) and river segment (three levels) as main effects with site specific channel gradient entered as a co-variate. We did not test for an interaction between the two main effects. To allow for posteriori power analysis for determination of statistical power of the treatment to control comparisons and to determine the desired sample sizes needed for a power of 0.8, we conducted a one-way ANOVA for differences between treatment and control sites and power analysis following the methods described above.

All statistical tests were evaluated for significance at the level of α = 0.1 owing to the higher P value criteria reducing the probability of a type II error, which can be a costly mistake in applied research given its influence on management decisions about future restoration efforts that may be based on our results (Peterman 1990).

Table 1. Variables used in data analyses, transformations applied to attain univariate normality, and abbreviations used in text and subsequent tables and figures. Log transformations are to base 10. NA = not applicable.

Group	Variable	Transformation use	Abbreviation
In-channel	Number of habitat units	NA	HabUnits
	Proportion of surface area as slow water	Arc-sine	P SA Slow
	Proportion of surface area as glide	NA	P SA Glide
	Wetted width of the active channel	NA	Width
	Depth to width ratio	NA	D:W
	Height of the inside bank at 1 m from the water's edge	Log (X)	Hgt IB
	Proportion of all sediments along the inside bank point bar in the "fine" size class	NA	P PB Fine
	Proportion of all sediments underlying the thalwag in the "coarse" size class	NA	P TH
	Gradient	NA	Gradient
Riparian	Coverage of trees on outside bank	Log (X + .01)	Cov T OB
	Coverage of shrubs on outside bank	NA	Cov S OB
	Coverage trees on inside bank	Log (X + .01)	Cov T IB
	Coverage shrubs on inside bank	Log (X + .01)	Cov S IB
	Coverage of trees and shrubs on inside bank	Log (X + 1)	Cov T&S IB
	Number of seedling trees on inside bank	Log (X + 1)	#Recruits
Aquatic macroinvertebrates	Total abundance in Surber Samples	Log (X)	#Inverts

For all ANOVAs, we tested for distributional outliers defined as values greater than 3 times the inter-quartile range from either the 25th or 75th percentile value and excluded outliers.

Characterizing site-to-site variability

We used non-metric multidimensional scaling (NMS; PC-Ord v. 4.0) to explore the nature of site-to-site variability as an aid in visualizing the relationships among measured variables. We used non-parameteric NMS ordination because it is well suited to multivariate data in which variables are measured on dissimilar scales, and because NMS is based on ranked similarities between sites it relaxes the assumption of a linear relationship between independent and dependant variables (McCune and Grace 2002) and preserves between-site distances (Clarke 1993). As such, NMS is a highly effective tool for graphical representation of community structure (Clarke 1993).

The NMS ordination incorporated all treatment and control sites (n = 27) and involved two data matrices. The first matrix included eight measures of habitat condition: HabUnits, P SA Slow, P SA Glide, D:W, Hgt IB, P TH Coarse, Cov T&S IB, #Recruits, and gradient. For the two cases where sediment size data were not available, we substituted the mean value of the applicable river segment–treatment group combination (i.e., P TH Coarse for L-T6 was the mean of P TH Coarse of all other lower Salmon River treatment sites). The second matrix contained two descriptive parameters: treatment or control, and river segment. Prior to ordination, we applied a general relativization by columns. No cases were identified as multivariate outliers (>2 standard deviations from the multivariate mean) and therefore both matrices had 27 cases. We used Euclidean distance and the "slow and through" autopilot setting to execute 50 runs with real data with random start configurations. We used 30 runs per tested dimension Monte Carlo simulation with randomized data to determine the number of dimensions to use in the final solution. We selected a three-dimension solution as the best fit and rotated the final solution to maximize correlation with gradient along the first axis.

RESULTS

Assessing effects of restoration activities

Treatment and control sites did not differ in multivariate space (MANOVA w/ 8 and 18 df, F = 0.45, F critical = 1.02, P = 0.460); however, treatment sites had narrower wetted widths and higher inside banks than did control sites (Table 2). All other in-channel response variables and channel gradient did not statistically differ between treatment and control sites (Table 2), although there were trends as treatment sites had greater mean number of habitat units and mean depth-to-width ratio, lower mean value of surface area as either slow water or glides, and lower mean value of fine particles along the edge of the inside bank point bars than did control sites. Differences in means were largest when treatment-to-control comparisons were limited to only the four intensive treatment sites, but the small number of intensive treatment sites precluded testing for statistical significance (Table 2). Coverage of shrubs on the outside bank of study reaches was the only riparian variable to statistically differ between treatment and control sites although treatment sites had greater mean values than control sites in all riparian categories (Table 3).

A posteriori power analysis indicated our statistical analyses of in-channel condition and riparian coverage typically suffered from low power (Tables 2 and 3). Excluding the two variables wetted width and height of inside bank, for which significant differences were found between treatment and control sites, the number of both treatment and control sites that we would have had to sample to find significant differences at α = 0.1 and the observed effect size ranged from 53 (mean D:W) to 17,450 (P TH Coarse), with a median of 92 (Table 2).

Sediment size distribution was related to channel gradient but thalwag sediments and point bar sediments differed in response to treatment (Figure 3). Within both treatment and control groups, the proportion of coarse sediments underlying the thalwag increased with increasing channel gradient and the two groups had very similar regression line slopes (Figure 4a; model r^2 = 19.4, model P value = 0.057, P value for differences in slopes of regression lines = 0.484, $n_{\text{treatment / control}}$ = 15/10). Conversely, point bar sediments at treatment sites had decreasing abundances of fines with increasing gradient, but control sites had increasing fines with increasing gradient (Figure 4b, model r^2 = 14.7, model P = 0.098, P for differences in slopes of regression lines = 0.049, $n_{\text{treatment / control}}$ = 15/10).

Macroinvertebrate abundance did not differ between treatment and control sites, but was affected by channel gradient and river segment (two-way ANCOVA #Inverts: co-variate Gradient w / 1 df, F ratio 3.92, P = 0.0604; Segment w / 2 df, F ratio = 2.95, P = 0.0735; Treatment or Control w/ 1 df, F ratio 0.01, P = 0.9409). Bessette Creek supported lower abundances than either the lower or middle Salmon River (mean [st error]: Bessette 296 [123], lower Salmon 552 [104], middle Salmon 466 [121]). Detecting differences in macroinvertebrate abundance between treatment and control sites with statistical power of 0.8 for a one tailed test at α = 0.1 would require sampling at 9,003 treatment sites and an equal number of control sites.

Characterizing site-to-site variability

NMS ordination required 41 iterations to produce a stable 3-dimensional solution (i.e., a solution with 3 axes) with final stress of 6.2 and instability of 0.005 (Figure 4). Stress is a measure of the suitability of the solution, as it indicates how well the solution reflects the structure of the original data set following the reduction in dimensionality. Instability is a measure of the magnitude of fluctuations in stress over the last 10 iterations of the ordination. The final stress and instability values of our solution are indicative of a stable solution (McCune and Grace 2002) with low risk for drawing false inference (Clarke 1993). The Monte Carlo test indicated our solution had lower stress than expected by chance (mean stress of Monte Carlo test: 15.0, test of difference between Monte Carlo and actual data P = 0.0476).

Cumulative variance explained in the solution was 95.7%, with axis 1 contributing 52.9%, axis 2–29.9%, and axis 3–12.9%. Axis 1 loaded with 5 parameters with correlations stronger than ± 0.5 along the axis: gradient, #Recruits, HabUnits, P TH Coarse, and Hgt IB. Axis 2 also had 5 variables load with correlations stronger than ± 0.5: D:W, P SA Slow, #Recruits, Cov T&S IB, P TH Coarse. Only gradient and P SA Slow loaded with correlation values >± 0.5 on Axis 3. Treatment and control sites are not well

Table 2. Means (± 1 standard deviation) of treatment sites (n = 16), control sites (n = 11), intensive treatment sites (n = 4), ANOVA P values for contrasts of treatment and control sites, and number of sites needed in both treatment and control groups to attain power of 0.8 (desired n). Means and standard deviations shown are for untransformed parameters, but statistical analyses were done on transformed data. For the two sediment variables (P PB Fine, P TH Coarse) treatment site n = 14. Abbreviations explained in Table 1.

Variable	Treatment	Control	P value	Desired n	Intensive
Gradient	0.00186 (.0014)	0.00232 (.0020)	0.488	105	0.00162 (.0009)
HabUnits	7.20 (3.13)	7.00 (2.24)	0.866	72	8.30 (2.40)
P SA Slow	0.11 (0.15)	0.14 (0.11)	0.410	92	0.08 (0.09)
P SA Glide	0.37 (0.25)	0.48 (0.30)	0.317	55	0.32 (0.17)
Wetted width	11.8 (2.69)	13.6 (2.07)	0.076	18	11.1 (1.70)
mean D:W	0.04 (0.01)	0.03 (0.02)	0.313*	53	0.05 (0.02)
Hgt IB	0.35 (0.22)	0.22 (0.19)	0.099	21	0.30 (0.15)
P PB Fine	0.44 (0.39)	0.57 (0.35)	0.409	75	0.41 (0.37)
P TH Coarse	0.59 (0.33)	0.59 (0.39)	0.954	17,450	0.53 (0.25)

* Note: Samples had unequal variances that could not be corrected by data transformation. Power analysis to attain "desired n" assumes the effect size is equal to that observed in our present comparisons. "Intensive" refers to a subset of 4 of the 16 treatment sites which received much more comprehensive restoration treatments than the other treatment sites. Mean values of treatment sites includes the four intensive treatment sites.

Table 3. Means (± 1 standard deviation) of treatment (n = 16) and control (n = 11) sites, ANOVA P values for contrasts of treatment and control sites, and number of sites needed in both treatment and control groups to attain power of 0.8 (desired n). Means and standard deviations shown are for untransformed data, but statistical tests were done on transformed values. Abbreviations explained in Table 1.

Variable	Treatment	Control	P value	Desired n *
Cov T&S IB	0.604 (0.52)	0.582 (0.32)	0.926	4,504
#Recruits	6.690 (11.10)	5.550 (6.90)	0.826	902
Cov T OB	0.235 (0.31)	0.150 (0.27)	0.341	–
Cov S OB	0.585 (0.31)	0.147 (0.21)	<0.001	–

* Note: Power analysis to attain desired n assumes effect size equal to that observed in our present comparisons. Desired n was only determined for the two inside bank riparian parameter.

Figure 3. Predicted linear regression lines and associated data points for relationships between the proportion of thalwag sediments in the coarse size class and channel gradient (panel A) and the proportion of point bar sediments in the fine size class and channel gradient (panel B). A: Model r^2 = 19.4, model P value = 0.057, P for differences in slope = 0.4847. B: Model r^2 = 14.7, model P value = 0.098, P for differences in slopes = 0.049. The four circled data points had unusually large DFITS values but their exclusion did not affect tests of significance or interpretation and therefore they are included.

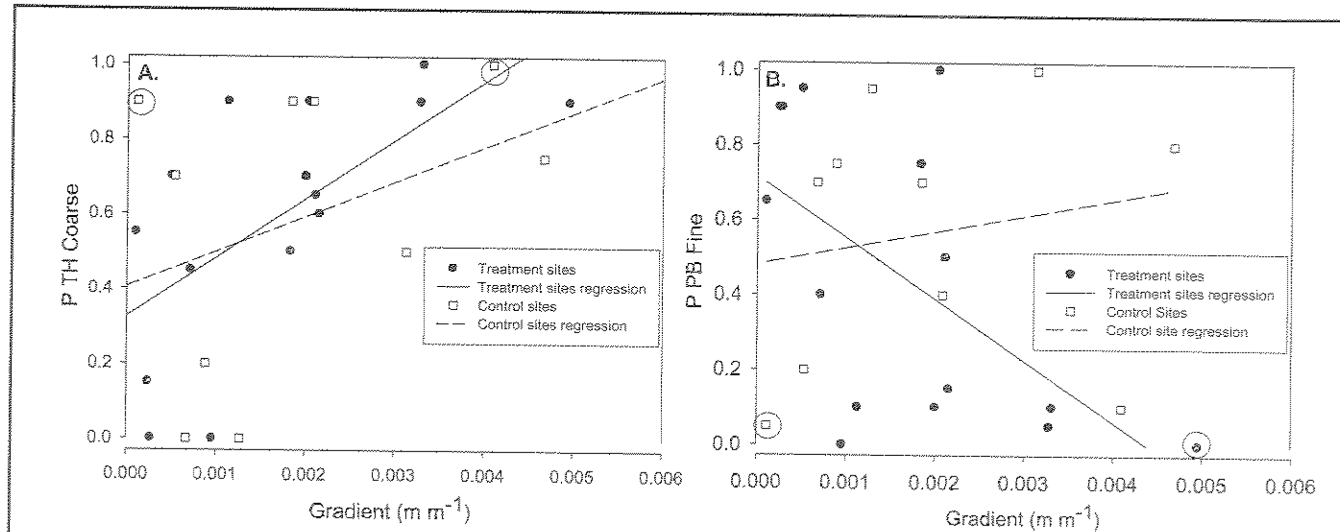


Figure 4. Results of a three dimensional non-metric multidimensional scaling ordination rotated to maximize correlation of gradient on axis 1. Axis 1 explains 52.9% of variance, axis 2 29.9% and axis 3 12.9%. Axes are labeled with variables that had correlation scores ±0.500 and arrows point in direction of increasing values. Solid squares are treatment sites (n = 16), open circles (n = 11) are control sites. The four intensive treatment sites are L-T5, M-T2, B-T4, and B-T3.

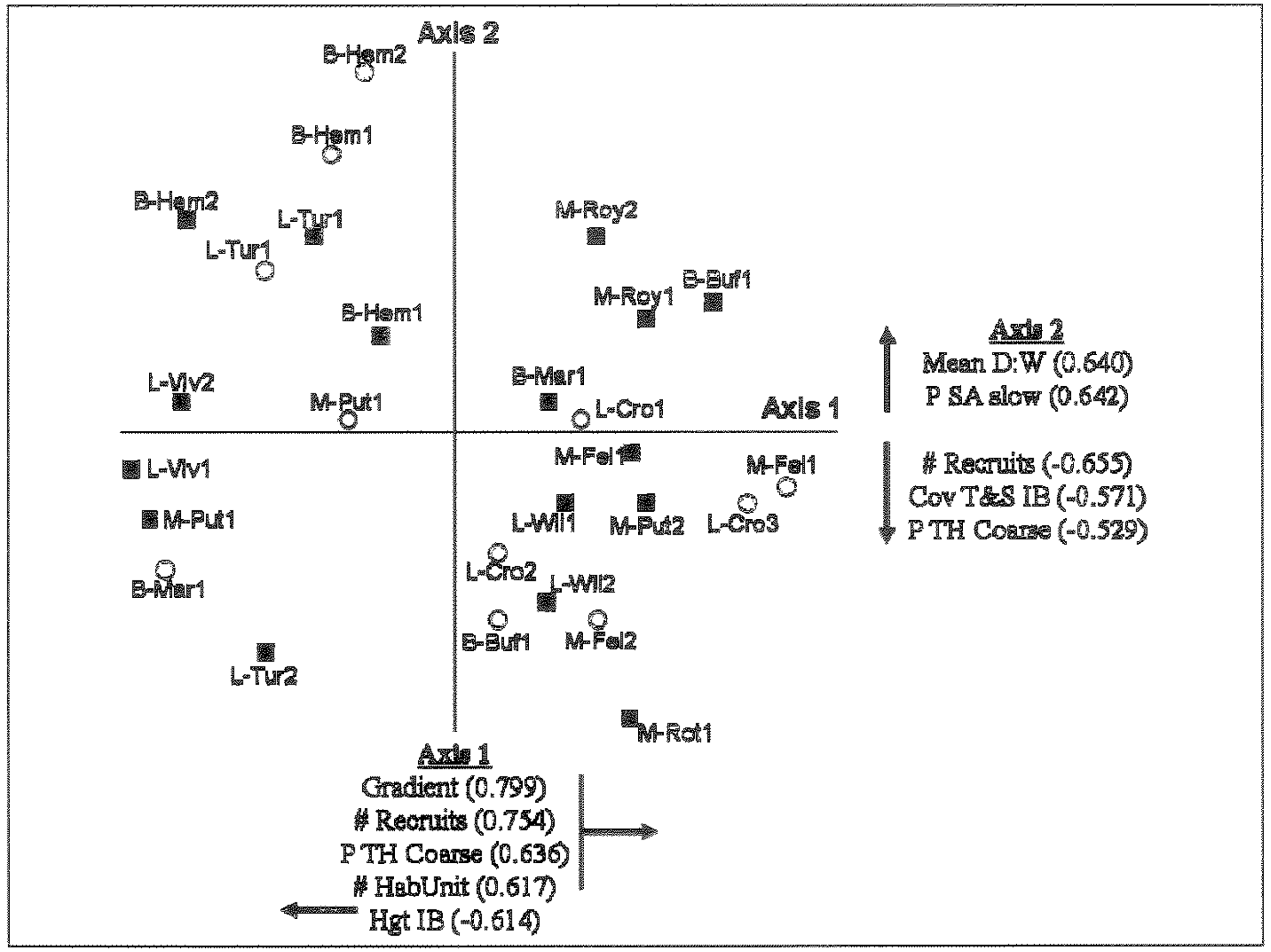
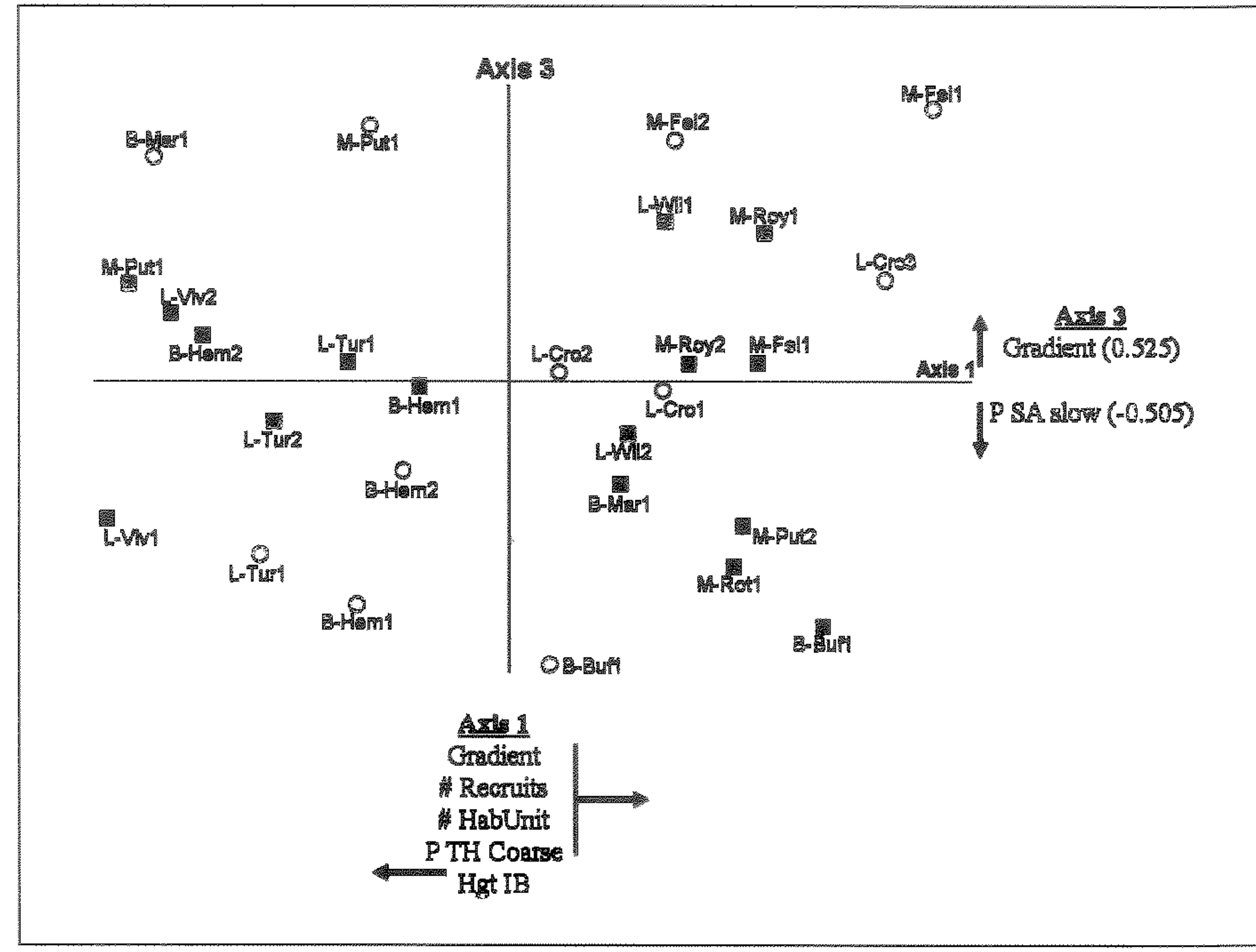


Figure 4 continued..



separated along any of the three axes, indicating the NMS ordination did not identify strong differences between treatment and control sites. Sites from the three river segments are similarly intermingled within the solution, indicating the three river segments had comparable habitat conditions. The 27 study sites are well distributed across all 3 axes, suggesting our solution is not influenced by a small number of unusually influential points (Figure 4).

DISCUSSION

We found limited evidence that stream restoration actions taken to stop riverbank erosion and improve salmonid production has substantially changed habitat condition at the scale of the river reach. Our multivariate tests are powerful tests of the hypothesis that bank restoration would yield measurable changes in habitat condition, as they can detect the cumulative effect of multiple small univariate differences. However, neither MANOVA nor NMS provided evidence that treatment sites substantively differed from control sites. However, treated sites did have narrower wetted widths, higher inside banks, and relatively more shrubs along outside banks, compared to unrestored control sites and these differences may be taken as indicators of the preliminary success of the restoration efforts and of processes that can lead to greater system recovery over time.

The narrower wetted widths present at treatment sites was a predicted outcome of restoration efforts and a goal of the restoration program in order to limit thermal heating of the water column. However, absence of pre-treatment and/or as-built data makes it impossible to determine if the observed width differences were a result of geomorphic adjustment to a new condition, such as sediment deposition along channel margins or channel downcutting, or simply a result of restoration actions which included dumping a large volume of material into the active channel. If narrowing was a result of bank accretion, then we can realistically expect the growth of the bank to continue until an equilibrium width is attained. If differences are a response to downcutting then undercutting and failure of treatments and a new wave of bank erosion may result. If, however, the width difference is simply equivalent to the width of the rock and wood added to the channel as part of the restoration, then there should be no expectation of future channel width adjustment. Because channel width is one of the most reliable and indicative hydraulic variables for characterizing watershed condition (Andrews 1982; Woodsmith et al. 2005 and citations therein), it would be beneficial to our effectiveness evaluation to have better insight into the mechanisms responsible for stream width dynamics.

The inside bank at treatment sites had a higher mean elevation over the water surface compared to control sites, and this may be an indication that the floodplain is re-building in these locations. Changes in inside bank height was not a predicted response to treatment and the mechanism linking stabilization of the outside bank and height of the inside bank is unclear but may be related to bank treatment halting the prograding of the inside bend point bar into the active channel and thereby promoting stabilization of the point bar deposit and vertical accretion during subsequent high water events. Land surfaces with higher elevations are less prone to inundation and therefore less susceptible to disturbance during high discharge events, and reduced disturbance may promote increased survivorship of colonizing vegetation (Hupp and Osterkamp 1985; Friedman et al. 1995; Cooperman and Brewer 2005) and initiate a positive feedback between plant establishment, sediment accretion, and floodplain building resulting in a narrower channel (Cooperman and Brewer 2005). Treatment sites did not differ from control sites in number of natural recruits or total vegetative coverage on the inside banks but the former did have greater means and variances and there was a strong positive correlation between height of the inside bank and plant abundance (Cooperman, unpublished data).

Establishment of vegetation along outside banks of river bends is a well-documented technique for stabilizing riverbanks and potentially enhancing fish habitat. Given that all plantings done as part of the restoration program were at least four years old at the time of our assessment, our results provide an indication of good survival of the plantings. However, the primary plant used at these restoration sites, locally identified as "Pacific willow" (*Salix lasiandra*) though they are probably a cultivated hybrid, has neither a large central bole or a spreading canopy and they rarely exceed 3 m in height. These growth characteristics suggests these willows are unlikely to provide extensive shading to the stream (Roni et al. 2002) or serve as significant in-channel geomorphic features upon recruitment to the active channel.

The different relationships between channel gradient and proportion of fine sediments in point bar deposits between treatment and control sites was unexpected, especially since there were no differences between the groups in terms of particle sizes underlying the channel thalwag. A likely explanation is that treatment effectively diverted energy away from the outside bank of the river bend and onto the inside bank thereby winnowing away fine particles, and the greater the gradient the more stream power diverted. This raises the question of whether or not bank stabilization exports an effect, either upstream via channel down-cutting or downstream via increased erosion, as has been documented for rip-rapped banks (Schmetterling et al. 2001). We had hoped to evaluate this potentiality, but the large site-to-site variability in the channel below study sites proved too large to disentangle from treatment effects (Cooperman, unpublished data). There was no relationship between sediment sizes in point bars and whether the next upstream meander bend was stable (with or without restoration treatment) or eroding (Cooperman et al. 2006).

Our macroinvertebrate results should be viewed cautiously owing to the samples being collected only from depositional environments (point bar deposits) where sediment particles are generally small. However, our treatment site point bars had relatively coarser sediments than those at control sites, and as coarser sediments often support more diverse and abundant invertebrate communities, it is reasonable to expect higher invertebrate abundances at treatment sites. The fact that we found no difference between treatment and control sites thus supports the conclusion that invertebrate biomass was not affected by restoration activities.

We did not test our data for spatial auto-correlation nor did we apply p-value corrections for multiple comparisons (i.e., Bonferronni adjustment). As such, our failure to find large geomorphic or ecological response to restoration can be viewed as conservative because had auto-correlation been present or post-hoc adjustments been applied, our statistical tests would have indicated even less differences between treatment and control sites than reported here (Hinch et al. 1993).

The NMS ordination illustrates the importance of pre-treatment data to effectiveness monitoring. Of the eight habitat measures used in the NMS analysis, four were strongly associated with gradient, the one parameter we measured that should be independent from presence-absence of bank restoration treatments. These four variables (HabUnits, P TH Coarse, Hgt IB, # Recruits) incorporate primary measures of reach-scale habitat diversity, sediment particle sizes, channel geomorphology, and riparian condition, and as such capture much of the ecologically relevant dynamics of interest. The strong correspondence within the NMS solution between gradient and these four variables suggests site-specific gradient is at least partly responsible for site-specific "ecology." Hence, site-to-site variability in our response variables associated with changes in local channel gradient confound our ability to determine the magnitude of change related to treatment efforts. Larson et al. (2001), in their discussion of the four types of variation that need be accounted for in monitoring programs (within-year at a site, independent year-to-year variation within a site, synchronous year-to-year variation among sites, and fundamental site-to-site variation), describe how failure to account for site-to-site variation, such as the gradient affect described here, can hinder trend detection. These authors suggest that multiple samplings at a site over time (i.e., a time-series approach comparable to before-after assessments) is the means to eliminate site-to-site variability.

Although certain statistical techniques can attempt to account for the influence of gradient on response variables, such as the use of regression residuals in a t-test comparing treatment to control sites, t-tests of residuals would still rely upon post-treatment between group comparisons and would be expected to have comparable statistical power to ANOVAs of the same data set. Also, using residuals may remove so much of the variability of interest as to overwhelm the ability to detect real between group differences. We tested regression residuals for treatment-control differences in depth-to-width ratio or height of the inside bank, the variables we measured most likely to be affected by local gradient. Results had P-values similar to those of our ANOVA tests and the t-tests suffered from comparably low statistical power (Cooperman, unpublished data). Only by holding gradient constant between treatment and control sites would we be able to fully disentangle the contribution of restoration from that of gradient.

The gradient effect exemplifies a weakness of the extensive post-treatment approach for effectiveness monitoring and it may be at least partly responsible for the low statistical power of our univariate comparisons. Our a posteriori power calculations indicate that for 7 of our 9 habitat variables, we would have needed over 100 sample sites (50 of each treatment and control) to attain a statistical power of 0.8, a standard value considered reasonable for ecological data (Peterman 1990; Steidl et al. 1997). Based on logistics of project planning, site selection, data collection within a limited field season, and the available budget, it was not possible for us to sample more than 16 treatment and 11 control sites. Even if time and money were more available, there were not many more sites that we could have examined which fit our project criteria. Thus, with "effect-size" changes of habitat variables on the orders we observed, it is not possible for us to definitively assess restoration effectiveness with the extensive post-treatment study design that we used. Our study would have greatly benefited from foresight to collect relatively inexpensive pre-restoration information (e.g., elevation cross sections, channel dimensions, rapid habitat unit, and riparian vegetation survey—total time ~ two hours for two trained people).

Our results illustrate an important lesson that should be heeded by agencies and groups wishing to conduct restoration activities and eventually assess their effectiveness. Only by using an experimental design capable of disentangling change caused by treatment from change caused by external factors and natural variability can definitive assessments of the af-

fect of treatment result. Appropriately replicated and controlled before-after designs provide the suitable mechanism for restoration monitoring. Using a before-after approach (Green 1979), or its derivatives such as BACI, beyond BACI (Underwood 1991), and staircase design (Walters et al. 1988) not only eliminates the confounding influences of site-to-site and year-to-year variability on detecting response to treatment, it can provide greater statistical power with fewer replicates (Roni et al. 2005), thereby saving time and money. Although selection of appropriate control sites is always important regardless of use of post-treatment or BACI approaches (Roni et al. 2005), only the post-treatment design is wholly dependant upon the need to assure "control" sites are suitable matches to the treatment group. The ultimate choice of which design to use is a function of logistical and budget constraints, but even simple before-after comparisons offer greater potential to detect relevant trends than do after-the-fact assessments.

One question regarding before-after approaches that is unresolved is the number of times a site needs to be sampled, both pre- and post-treatment. We suspect the answer is study specific and depends upon investigator knowledge of the potential magnitude and rate of response to treatment that can be reasonably expected. Knowledge of the generation time of populations of interest and/or the return frequency of key geomorphic and/or disturbance events such as floods, drought, and fire seem reasonable starting points. In one case, a comprehensive BACI evaluation of stream restoration in Finland found evidence of response to restoration in habitat structure, benthic invertebrates, trout abundance, and ecosystem process based on a single pre-treatment and single post-treatment sampling (Muotka and Syrjänen 2007). Alternatively, Wooolsey et al. (2007) determined sampling frequency for assessing restoration effectiveness on the Thur River of Switzerland, on a variable by variable basis. For example, they assessed wetted width twice before restoration, three times after the first flood, and once after the second flood. For surface-hyporheic exchange, sampling occurred twice before treatment and once after the first flood; and, for lateral connectivity, sampling was once each, before and after.

The absence of wide ranging large differences between the treatment and control sites compared in this study does not mean the restoration program has not yielded benefits. The treatments have been highly effective at preventing further bank erosion, the proximal programmatic goal, and have successfully established riparian vegetation. Furthermore, although small sample size precluded statistical comparisons, the mean condition of the four "intensive treatment" sites suggests that more expensive and detailed site-specific actions may yield larger favorable changes in habitat quality (e.g., greater habitat diversity, deeper and narrower channels, less fine sediments). The four intensive sites had a lower mean channel gradient than the other treatment sites or the control sites, suggesting intensive treatment attained the greater results despite having less stream power available.

Palmer et al. (2005) suggested five criteria by which to judge if restoration is successful: (i) did a predefined guiding image exist for the effort (i.e., statement of purpose and goal), (ii) has the river's condition been improved, (iii) has a more self-sustaining system emerged, (iv) did construction cause lasting harm, and (v) were both pre- and post treatment assessments done. Our biggest limitation is the lack of pre-treatment data and because of this we have limited power to make the definitive conclusions we would like. Based on the remaining criteria, the bank stabilization program described herein has been a modest success. However, criteria of less tangible issues, such as increased social awareness of the linkage between land-use and ecological consequences and the evolution of a stewardship mentality, need also be considered. A companion study conducted at the same time as the effectiveness evaluation reported and covering several watersheds of the British Columbia southern interior found that as the amount of restoration work done on a stream increased, so to did landowner awareness and appreciation of habitat restoration. Further, the increased awareness of habitat issues was matched by a corresponding increase in adoption of ecologically more benign land-use behaviors such as more extensive use of livestock exclusion fencing to control riparian grazing (Branton et al. unpublished data). As such, in a sociological context, the stream restoration effort described herein appears to have been highly successful.

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