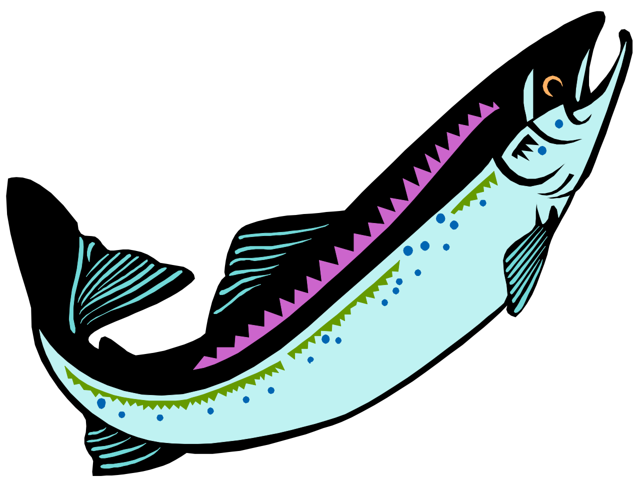
# Monitoring the Implementation and Effectiveness of Fisheries Habitat Restoration Projects

Final Report



*Prepared for:*

**California Department of Fish and Game**
**Salmon and Steelhead Trout Restoration Account Agreement No. P0210566**

*Prepared by:*

**Center for Forestry, University of California, Berkeley**
**Richard R. Harris, Principal Investigator**

March 2005

# TABLE OF CONTENTS



# TABLE OF FIGURES



# TABLE OF TABLES

**ACKNOWLEDGMENTS**

The authors wish to acknowledge the many people who have supported this effort. Barry Collins, Department of Fish and Game, and the contract managers at the Fisheries Restoration Grant Program were exceptionally helpful. We thank Tom Lisle, Matt O'Connor, Peter Cafferata, Kathleen Morgan, Sue Hilton, Mary Ann Madej, Kate Sullivan, George Ice, Koll Buer, Phil Roni, Phil Kaufmann, Steve Lanigan, Kirsten Gallo, Jeff Opperman, Randy Klein, Ken Roby, Bob Keiffer, David Lewis and numerous others who talked with us and shared their observations and constructive criticisms. We would also like to thank colleagues who worked on the early stages of this project. They include Donna Lindquist, Maggi Kelley, Faith Kearns, Karen Bromley and Mariya Shilz.

Sherry Cooper produced all the final reports, including this one.

This report should be cited as follows:

Harris, R.R., C.M. Olson, S.D. Kocher, J.M. Gerstein, W. Stockard and W.E. Weaver. 2005. *Procedures for Monitoring the Implementation and Effectiveness of Fisheries Habitat Restoration Projects.* Center for Forestry, University of California, Berkeley. 24 pp.

# INTRODUCTION

The recovery of anadromous salmonid populations in California is a major goal of the California Department of Fish and Game (DFG). The conservation and restoration of anadromous fish habitat is one of the prime strategies used by DFG to achieve this goal. In coastal California, these efforts are supported by DFG's Fisheries Restoration Grant Program (FRGP) that annually funds and oversees restoration projects implemented throughout coastal watersheds. To ensure that funds are spent wisely and effectively, DFG intends to establish a long-term, comprehensive monitoring program to evaluate and assess the effectiveness of these restoration efforts in a credible, scientific manner.

This report and the monitoring reports included with it under separate covers are the end product of a four-year effort to develop qualitative and quantitative methods for evaluating the implementation and effectiveness of fish habitat restoration projects undertaken pursuant to the FRGP. This report provides recommendations for monitoring. Recommendations are largely based on a review of similar approaches being used in other states and four years of consultation with the community of scientists engaged in monitoring. Accompanying reports include field methods that have been field-tested for specific application to coastal California. Methods were initially derived from literature reviews and refined through consultation and discussions with peers and field-testing.

# PROJECT BACKGROUND

Two interim reports were prepared over the course of this project. Both provide extensive background information that should be consulted by readers unfamiliar with this method development project. Harris et al. (2002a) includes a review of all monitoring programs in the Pacific Northwest existing as of February 2002 (see Appendix E in Harris et al. 2002a). Oregon's comprehensive anadromous salmonid monitoring program to this day remains the role model for the region. The recommendations made here mirror in part the existing Oregon effectiveness monitoring program (Lacy and Thom 2000). They also closely resemble aspects of the proposed program in Washington State (Crawford and Johnson 2004, Bruce Crawford personal communication, Washington Salmon Recovery Funding Board 2003). Harris et al. (2002a) includes effectiveness monitoring criteria and monitoring parameters for FRGP project types, which were developed with the assistance of an ad hoc scientific advisory panel (see Appendix C and D in Harris et al. 2002a). Membership in that panel included scientists from the Environmental Protection Agency, USDA Forest Service and other agencies and institutions engaged in monitoring. Harris et al. (2002a) raised a number of issues for consideration by DFG management. These concerned staffing, funding, spatial and temporal scales for monitoring and the required level of precision needed for DFG decision-making. Many of those issues remain unresolved. That report makes many recommendations on monitoring that are reiterated in this final report. It also lists methods nominated by the ad hoc panel for field-testing (Table 2 in Harris et al. 2002a). A review of all available monitoring methods is provided with comments on their accuracy, precision and potential application to restoration effectiveness monitoring (see Appendix F in Harris et al. 2002a).

Collins (2003) is a compendium of two reports, one prepared in November 2002 by this project team (Harris et al. 2002b) and another prepared pursuant to a separate contract on validation monitoring. Harris et al. (2002b) is the draft of this final report prior to extensive field-testing. It reiterates many recommendations made in Harris et al. (2002a). Appendices include refined effectiveness monitoring criteria for all project types (Appendix A in Harris et al. 2002b); and drafts of all the final reports included here. Several of these changed extensively during the process of field-testing e.g., qualitative monitoring procedures.

In summary, the report in hand represents the culmination of a four-year project. The last step to finalizing this report was critical review by DFG and external peer reviewers. Reviewer comments were used to revise and produce this final product and all of the final reports.

## THE FISHERIES RESTORATION GRANT PROGRAM[1]

The focus of the FRGP is to restore anadromous salmonid habitat with the goal of ensuring the survival and protection of the species in coastal areas of California. Since 1981, in collaboration with more than 600 stakeholders, the FRGP has invested more than $100 million and supported more than 2000 projects in over 2,600 coastal streams. Projects range from education and instream barrier removal, to riparian restoration and project monitoring (Table 1). The success of these projects has contributed to an ever-evolving program that directly benefits threatened and endangered anadromous salmonids in coastal California.

[1] This section was prepared by Barry Collins, Department of Fish and Game.

**Table 1**. Restoration Project Types Funded by the FRGP.

| Project Type | Project Type Description |
|---|---|
| AC | AmeriCorps Program only |
| CF | CA Forest Improvement Program |
| ED | Public School Watershed and Fishery Conservation Education Projects |
| FL | Fish Ladder |
| HA | Habitat Acquisition and Conservation Easements |
| HB | Instream Barrier Modification |
| HI | Instream Habitat Restoration |
| HR | Riparian Restoration |
| HS | Instream Bank Stabilization |
| HU | Watershed Restoration (Upslope) |
| MD | Monitoring Projects (Data) |
| MO | Project Monitoring Following Project Completion |
| RE | Cooperative Rearing |
| SC | Fish Screening of Diversions |
| TE | Private Sector Technical Training and Education Project Grants |
| OR | Watershed Organization Support and Assistance |
| PI | Public Involvement |
| PM | Project Maintenance |
| WC | Water Conservation Measures (Ditch Lining, Piping, Stock Water Systems, Tailwater Mgmt.) |
| WD | Water Measuring Devices (Instream and Water Diversion) |
| WP | Water Purchase |

Currently, the majority of grant funding is awarded for on-the-ground habitat restoration projects that directly improve salmonid habitat or the access of fish to salmonid habitat (referred to as "implementation projects" in this report) (Figure 1). Examples are projects designed to increase overhead canopy cover, improve spawning gravels, or increase the number and depth of pool habitats. Other projects might seek to reduce or eliminate erosion originating from upslope land use which impacts instream habitat quality through sedimentation processes; still other projects are implemented to screen water diversions and remove barriers to fish passage. FRGP funds are also awarded for indirect habitat restoration activities such as cooperative fish rearing, acquisitions of riparian easements, research, project monitoring, watershed assessment and planning, support for watershed organizations, and public outreach and education. The methods included with this report under separate covers are only intended to address on-the-ground habitat restoration projects, i.e., implementation projects.

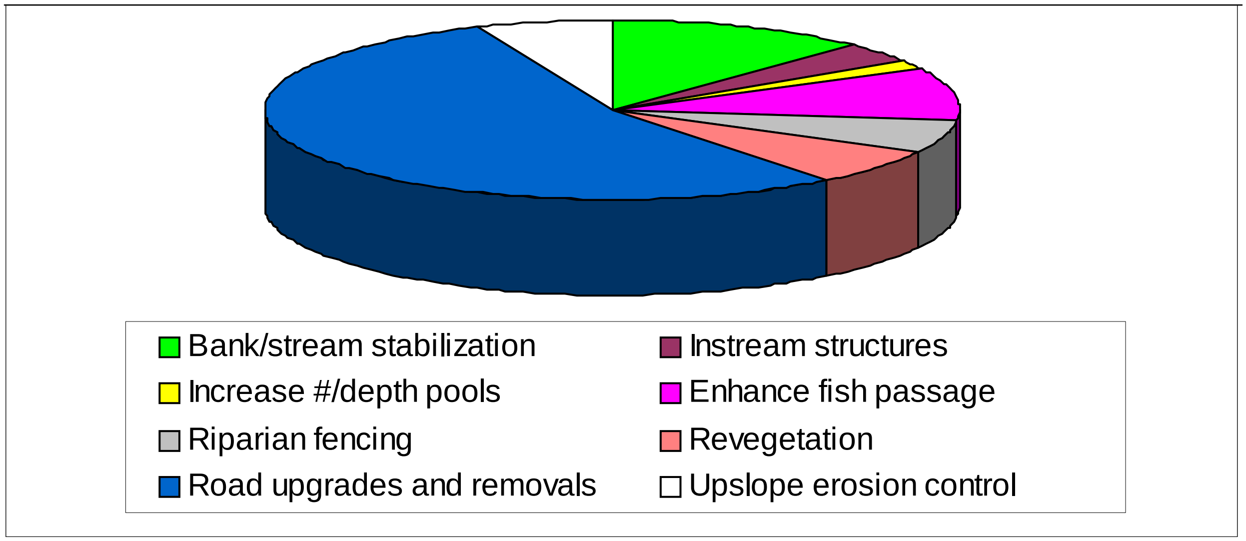



**Figure 1**. Allocation of FRGP Funding From 1998-2000 for Salmonid Habitat Restoration Activities.

## MONITORING QUESTIONS

The scope of this report and the attached reports is confined to the evaluation of physical and environmental changes caused by implementation projects, or, in the case of riparian restoration, changes in plant community characteristics. Monitoring biological responses such as changes in fish or invertebrate populations is not included. A companion project on "validation monitoring" is being completed through a contract with Humboldt State University (Collins 2003).

Since the beginning of this project there have been two basic questions that have directed the effort:

- Are fish habitat restoration projects being carried out as proposed?
- If properly implemented, are restoration projects having the intended beneficial effects on habitat?

These questions imply three activities: 1) pre-treatment site characterization for establishing the conditions prior to restoration; 2) monitoring after project completion (implementation monitoring), to determine if the restoration was done according to the approved design; and 3) post-treatment or effectiveness monitoring, to determine if the restoration is having the intended effects. Each one of these activities poses a variety of logistical and scientific challenges.

The outputs from monitoring can be used to inform DFG, the scientific community, funding sources and the public at large about the performance of the FRGP. Results may be used to modify program direction, emphasize particular types of restoration and/or improve restoration practices.

## MONITORING PROCEDURES

### Introduction

The myriad of project types and active or passive restoration activities funded by the FRGP creates a layer of complexity that confounds attempts at developing generic study designs for effectiveness monitoring. Even restoration activities that are called the same thing and have the same objectives can be vastly different on the ground. These complications raise significant issues in statistical study designs, but they are not insurmountable obstacles.

This report and the attached reports may be used for two purposes: 1) in the case of the location, photo-monitoring and qualitative reports, improving the documentation of project locations and monitoring the implementation and effectiveness of restoration activities at the project site, stream reach (e.g., piece of stream operationally defined as the location for multiple treatments) and road reach (e.g., analogous length of road that has received multiple treatments) scales; and 2) in the case of the quantitative and watershed monitoring reports, to give guidance to DFG and others (professionals and scientists skilled in monitoring) for preparing monitoring study plans. The quantitative reports list questions that are relevant to different types of restoration projects. They also supply direction on study design and methods for monitoring. They are not study plans in and of themselves. In a following section of this report some study examples are presented. Future monitoring studies may be prioritized, using the questions provided in the reports or on the basis of other questions arising either from the FRGP, the scientific community, funding agencies or the general public.

In earlier reports (Harris et al. 2002a, 2002b) various topics such as alternative monitoring precision levels were discussed. The methods provided here are both qualitative and quantitative, recognizing that different needs can be met with different levels of precision and accuracy.

There are some basic constraints associated with the FRGP that must be acknowledged in any attempts to monitor project implementation and effectiveness. Certain aspects of the program may be adapted to facilitate monitoring but others must be considered relatively fixed. First and foremost, the FRGP is proposal-driven. That is, projects are funded and approved based on proposals submitted mainly by parties outside DFG. The implication of this is that in any given year, many different types of projects may be undertaken in many different locations. Consequently, monitoring activities must be responsive to opportunities that present themselves rather than based on purely scientific or management considerations. Second, the time frame for project approval, implementation and monitoring is generally confined to a period of 10 years during which FRGP contracts stipulate that access must be provided to project sites. Most projects are on private lands and landowners cannot be expected to allow unlimited access. Third, there are limited opportunities for pre-treatment site monitoring. Most projects are conceived and proposed by proponents with neither the incentive nor the funding to do pre-treatment monitoring. Fourth, under the terms of its current permits from NOAA-Fisheries, the US Army Corps of Engineers and Water Quality Control Boards, DFG is required to monitor the effectiveness of 10 percent of the projects it approves in any year. Finally, there are a number of metrics that DFG must use to report on its accomplishments to the Pacific Coast Salmon Recovery Fund (PCSRF) administrators.

In view of these constraints and requirements, the recommended objectives for the FRGP's Restoration Monitoring Program are:

1. Qualitative implementation monitoring on all projects every year
2. Qualitative effectiveness monitoring on 10 percent of all projects every year
3. Quantitative monitoring on specific projects considered to be either high risk to the resources (i.e., endangered fish populations or critically designated habitat), experimental in nature or both
4. Quantitative monitoring using a sampling approach to assess the performance of projects (e.g., instream habitat improvements, upland erosion control, bank stabilization, etc.) that have similar objectives and similar implementation approaches
5. Quantitative monitoring at the watershed scale to assess effectiveness of upland road restoration

The qualitative monitoring methods are intended to meet the needs of Objectives 1 and 2. The quantitative monitoring methods address Objectives 3 and 4. Finally, a watershed monitoring method is provided for Objective 5.

During the course of this project it was determined that the manner in which projects are described and documented by both project proponents and DFG contract managers is highly variable. This makes monitoring difficult, simply because there is uncertainty about what was done and where it was done. To correct that problem, a method for documenting project locations in a systematic way is provided. A method for photographing project sites and features and cataloging photographs is also provided.

In the remainder of this section, approaches for each of these recommended monitoring procedures are summarized, including a discussion of study design and examples. The reports themselves should be consulted for details on field measurement techniques.

## EFFECTIVENESS CRITERIA

Monitoring methods presented in the reports are based on collecting data relevant to measures of effectiveness for each of the FRGP implementation project types. The effectiveness criteria are presented in Harris et al. (2002a, 2002b) and in the reports. They were developed through an iterative process with the assistance of DFG staff and scientists engaged in monitoring. Most are similar to the effectiveness criteria used in other monitoring programs (Johnson et al. 2001, Crawford and Johnson 2004, Kershner et al. 2004). Table 2 shows some examples for several kinds of restoration project.

**Table 2**. Sample Effectiveness Criteria and Monitoring Parameters for Selected Restoration Project Types.

| Restoration Project | Example Effectiveness Criteria | Example Monitoring Parameter(s) |
|---|---|---|
| Fish passage improvement | Area of habitat made accessible | Acres of habitat above project (habitat types) |
| Instream habitat restoration | Project improves rearing habitat within the project reach | Frequency and depth of pools |
| Streambank stabilization | Reduced bank erosion | Percent of bank that is fully vegetated |
| Land use control (fencing to exclude grazing) | Livestock and/or wildlife successfully excluded from riparian zone and stream | Percent cover of riparian vegetation |
| Vegetation control (exotic plant removal) | Reduced relative abundance of exotic plants | Relative cover of native versus exotic plant species |
| Riparian planting | Survival meets or exceeds contract specifications | Percent survival of planted trees |

To enable the preparation of study plans for quantitative effectiveness monitoring, effectiveness criteria parameters for projects should be quantified in project descriptions. That is, objectives should be expressed in terms of desired environmental changes. For example, if an instream structure is intended to improve rearing habitat, the desired changes should be expressed in terms of pool frequency, instream cover and/or pool depth or other measurable environmental characteristic. These should be stated as desired pool frequency e.g., 50 percent of reach length, desired instream cover percentage, e.g., 25 percent shelter ratings or desired pool depth e.g., > three feet, or alternatively, as a percentage change in the parameter (Washington Salmon Recovery Board 2003). If objectives are vaguely stated, then it is difficult to focus the monitoring. Frequently, project descriptions do not provide this information. Quantified effectiveness criteria are also lacking from many of the restoration design specifications (Flosi et al. 1998). Changes to project documentation are therefore needed if quantitative monitoring is to be facilitated. This type of information could be included in the individual contracts (Project Purpose and Description and Statement of Work).

If project objectives are expressed as quantified changes in environmental conditions and these changes in turn, are the measures of effectiveness, then two study design problems are solved. The level of change detection is then specified. This in turn, enables development of a sampling plan, including consideration of the power of the various methods to detect the specified change. A good example of quantified thresholds of effectiveness is contained in the Washington Salmon Recovery Board monitoring publications (Washington Salmon Recovery Board 2003, Crawford and Johnson 2004). Most of Washington’s criteria require changes of 20 percent or greater for a project to be deemed effective.

## EFFECTS OF VARIABILITY ON CHANGE DETECTION

Stipulating the level of change that is to be detected is probably the most important factor affecting study design. Another important consideration is the ability to detect a change that is not just due to variability but is actually attributable to the restoration activity.

One missing element in coastal California is a compilation of estimates of the natural variability in the parameters to be monitored. These estimates may be obtained through directed research studies or through analysis of data available from sources such as DFG stream surveys. In Washington State, estimates of variability were obtained from EMAP survey data (Bruce Crawford, personal communication, Kaufmann et al. 1999). Archer et al (2004) conducted a thorough pilot study on stream measurement that separated out the effects of natural variability from other sources of variability. Their study area was the Columbia River basin. Although it is tempting to use the results of that study as a guide, the authors warn against extrapolating their findings to other streams in other regions. A better approach would be to replicate the work of Archer et al. (2004) on the California coast.

In addition to natural variability, there may be variability in measurements or estimates from observer to observer and due to temporal changes in parameters that can confound monitoring (Kaufmann et al. 1999, Archer et al. 2004). Temporal variability can be minimized e.g., by collecting data at similar times of the year or under similar flow conditions. Certain monitoring methods, such as habitat typing, have inherently high levels of observer variability but still are useful. Coupling high observer and/or environmental variability with low thresholds for change detection is a recipe for either unwieldy sample sizes or inconclusive results (Archer et al. 2004). For this reason it is critical to quantify effectiveness criteria prior to planning a study and to make the thresholds for change detection reasonable in relation to variability. Changes of 50 percent (e.g., riparian cover increases by 50 percent; pool frequency or depth increases by 50 percent; etc.) have been used in this project as a general rule for selecting and testing monitoring methods.

In summary, it is necessary to have quantified effectiveness criteria and estimates of natural variability in parameters being monitored in order to develop monitoring study plans that will yield conclusive results. It is also necessary to minimize temporal variability and observer variability in using measurement and estimation techniques. Thresholds for change detection, i.e., quantified effectiveness criteria, should be realistic, acknowledging that natural variability in stream and watershed characteristics is generally high, particularly on the coast.

## QUALITATIVE MONITORING

Qualitative monitoring is to be conducted with the use of monitoring checklists developed for each of the project types funded by the FRGP. There are checklists for pre-treatment, post-implementation and effectiveness monitoring. The checklists could be used by project proponents (pre-treatment) or by DFG contract managers during the course of their normal administrative duties (implementation and effectiveness). During 2004-05 data were collected by DFG using the checklists on a number of projects as a means of refining and improving the final products and for reporting to regulatory agencies.

The DFG Restoration Manual (Flosi et al. 1998) already provides standardized methods for stream habitat inventory for pre-treatment site and stream reach assessment, for designing and implementing fish passage improvements, and for assessing road system restoration needs. Although intended primarily to facilitate project design, these methods also provide adequate baseline information to enable qualitative post-project implementation and effectiveness evaluations. For other project types, no formal pre-treatment data collection procedures currently exist. For these project types, standardized pre-treatment checklists have been provided.

The recommended procedure for qualitative monitoring is as follows:

- Every year, after project implementation, but before the passage of the winter, implementation monitoring checklists should be completed by the assigned DFG contract manager for all finished project work. Project work rated as improperly implemented should be corrected and new implementation checklists should be completed on the corrected project. Implementation checklists should be retained in the project files and entered into the DFG monitoring database.
- After the passage of at least one, but not more than three winters, effectiveness monitoring should be conducted on 10 percent of every year's properly implemented projects that have been completed.
- Projects should be selected for effectiveness monitoring in the following way:
  The DFG monitoring coordinator should review the list of projects every year and select 10 percent in a stratified random design. Projects should be stratified first by coastal region and then by project type classification. Random selection from the stratified list should be done with the goal of selecting at least one project from each project type while sampling from all regions. If only one of a specified project type is done in a given year, it should be included in the sample.
- To facilitate effectiveness monitoring, pre-treatment monitoring checklists or comparable data should be provided for that year's 10 percent project selection list before any on-the-ground work is initiated. This information could be supplied by either the project applicant or by the assigned DFG contract manager. These data should be filed with the project application and retained in the project files. Pre-treatment data should be entered into the DFG monitoring database for approved projects.
- Effectiveness checklists should be retained in the project files and entered into the DFG monitoring database.

It is recognized that this scheme, based primarily on DFG permit requirements, may not constitute a statistically defensible design for inference on effectiveness to the population of projects implemented every year. To develop a more statistically valid sampling procedure, there will need to be an estimate of the variability in effectiveness evaluations. This can be done with 2004 monitoring data and future years' sampling can be adjusted accordingly.

The qualitative monitoring checklists provide the basis for reporting on required PCSRF metrics (the metrics are incorporated into the checklists). They also provide the basis for yearly reporting on the program's accomplishments (see section on Reporting, below).

## QUANTITATIVE EFFECTIVENESS MONITORING

Methods for quantitative monitoring of all FRGP implementation project types are included in accompanying reports. These methods or variants are generally accepted and in use throughout the Pacific Northwest. Methods evolved through a process of literature review, consultation with knowledgeable scientists and field-testing (Harris et al. 2002a, 2002b). Through field-testing, the final methods were adapted for application to coastal California. For example, field-testing and consultation indicated that standard methods of sediment sampling needed to be adjusted for use on the coast. Also, the standard DFG procedure for habitat typing was revised for use in effectiveness monitoring (see reports under separate covers).

It is expected that these methods will be used as a "tool box" by people developing proposals for restoration effectiveness monitoring. Utilizing standardized methods will aid in producing comparable data sets among monitoring projects assessing the same or similar restoration techniques. Particular studies may require that these methods be refined or augmented with additional methods.

It is anticipated that two kinds of study may be done using the methods: 1) an individual project may be evaluated for effectiveness, or 2) a number of projects may be sampled and evaluated in order to determine the effectiveness of a particular type of treatment. Both potential applications are explained below.

### Monitoring the Effectiveness of Individual Restoration Projects

A single project may require monitoring if it has the potential to cause a significant impact to the resource (i.e., fish population or fish habitat) or if it is experimental, or both. For example, if a project involves major construction on a stream with known populations of listed salmonids, there may be a need to monitor it to ensure that it is having the intended beneficial effects. In another case, the project may involve an innovative, but un-tested restoration practice. Monitoring might be done in that case to determine if the practice has merit for application elsewhere. In either case, a monitoring plan, using the reports provided here or other methods, will be needed. That plan should have its own statistical procedures and study design.

Guidance on the scope and detail required for single project monitoring can be obtained from the literature. For example, Kondolf et al. (1996) monitored the persistence of a spawning gravel placement on the Merced River. They used cross sections and longitudinal profiles to determine whether or not the gravel remained in the intended location. Their results indicated that the gravel had migrated off the placement site and was not providing additional spawning habitat as expected. Merz and Setka (2003) conducted a similar study on the Mokelumne River and found the opposite. They used mapping, pebble counts, redd counts, subsurface sediment and water quality sampling and bathymetric surveys to determine if placed gravel remained stable and was used for spawning by Chinook salmon. The gravel remained in place and was used for spawning for more than two years. Both of these studies qualify as research-level monitoring and suggest that single-project monitoring should only be undertaken in exceptional cases[2]. On the coast, potential candidates for this detailed monitoring would include major passage improvements or

[2] CALFED has funded a number of detailed monitoring studies on streams draining to the Bay-Delta. Some examples include Clear Creek (near Redding), Merced River and Tuolumne River. Study designs can be reviewed at the CALFED website http://calwater.ca.gov.

construction projects involving major changes to streams, such as creation of meanders and/or side channels. Most implementation projects on the coast would not require this level of monitoring.

### Monitoring the Effectiveness of Restoration Practices or Project Types

The DFG Restoration Manual (Flosi et al. 1998) provides specifications and designs for different restoration projects. For many of these, there are no quantitative data on their effectiveness. Existing information is generally observational, based on professional judgment, or qualitative (e.g., Hopelain n.d.).

At the present time, most knowledge on the effectiveness of treatments resides in DFG contract managers or restoration practitioners who are experienced with them. Although this information may be communicated through meetings and gatherings such as the annual Salmonid Restoration Federation conferences, it is not available in the published literature. The lack of quantitative scientific knowledge on the performance of different restoration practices is a significant constraint to their improvement on the coast. Candidate practices for monitoring would include upland erosion control treatments, instream structures, passage improvements and riparian restoration. If quantitative studies on the effectiveness of these practices were conducted on the coast, this information could be shared with the scientific community, DFG and restoration practitioners and could improve program performance.

The concept underlying this kind of monitoring would be to determine if standard restoration practices are generally effective in achieving their objectives. For example, a sample of riparian restoration projects in similar environments might be monitored to determine if they meet their stated objective of increasing vegetation cover by a predetermined percentage. The results would be used to infer performance of all riparian restoration projects in similar settings. Or, a sample of instream habitat improvement projects might be monitored to see if in general, they improve the quantity and quality of pool habitat. This approach has been used in Oregon (Jacobsen and Thom 2001, Jacobsen and Jones 2004) and is currently proposed in Washington (Bruce Crawford, personal communication). For studies of this nature the goal would be to provide a basis for inferring from the sample to an entire class of treatment.

There are several considerations that apply to this kind of monitoring:

- What restoration practices have the highest priority for study?
- What are the targeted response variables and criteria for judging effectiveness?
- What is the required sampling design?
- What methods should be used to collect data?
- What methods should be used to analyze the data?

The first two points cannot be discussed here since they really depend on decisions by DFG management and cooperators. Questions of study design and methods are discussed below. Further detail on methods may be found in the reports included under separate covers.

*Study Design*

Numerous texts and published papers describe alternative approaches for evaluating the effects of projects intending to produce environmental changes. A handbook produced by the British

Columbia Ministry of Forests on statistical approaches for adaptive management (Sit and Taylor 1998) is a particularly useful review. Although focused on "adaptive management" and "environmental impact assessment," there is really no difference between impact assessment and restoration project assessment. One has a connotation of negative environmental changes and the other implies positive changes. The theory is identical in either case. Sit and Taylor (1998) discuss designed experiments, before-after-control-impact (BACI) and retrospective studies. Designed experiments are not considered feasible for impact (or restoration effectiveness) field studies (Schwarz 1998).

Retrospective studies of restoration project performance may be conducted without the benefit of pre-treatment data. This kind of study has been done to determine the persistence and effectiveness of instream structure placements in Oregon and elsewhere (Frissell and Nawa 1992, Smith 1998, Roni and Quinn 2001). In California, a retrospective study was conducted to determine the performance of road restoration sites after an extreme weather event (Madej 2001). Kondolf et al. (2001) used a type of retrospective study called "post-project appraisal" to evaluate a channel reconstruction project on the central California coast. Downs and Kondolf (2002) have advocated wider use of this approach, especially in the absence of pre-project data. Retrospective studies could be done with virtually any type of restoration practice on the coast. The sample population could include all past projects in a particular category of practice.

There are some difficulties with retrospective studies that should not be overlooked. The main drawback is that pre-treatment conditions and history are unknown so that there may be considerable variability that cannot be accounted for (Hicks et al. 1991, Roni and Quinn 2001). Smith (1998) warns that retrospective studies must be used carefully and the results are often just preliminary. Some of the limitations of retrospective studies are discussed in the two examples from the literature, below.

Roni and Quinn (2001) studied the effects of large wood placement projects on streams in western Oregon and Washington. They used the "extensive post-treatment design", which involves comparison of treatment and reference stream reaches (Hicks et al. 1991). They did not possess pre-treatment data and their study design did not depend on it. Statistical analysis consisted of comparing several habitat variables and fish densities on the treated and reference streams using paired *t*-tests. They concluded that wood placement projects did improve habitat and that densities of juvenile salmonids were significantly higher in the treated reaches. They suggested that the findings of their studies are applicable to wood placement projects throughout western Oregon and Washington.

The study design used by Roni and Quinn (2001) would not be feasible on the California coast at the present time due to the lack of reference stream information. This issue is discussed further, below.

In another study, Thompson (2002) looked at 40 instream structures that had been in place for up to 70 years. He evaluated their effects on instream habitat, riparian vegetation and channel erosion. His evaluation process included measurements of structures and comparison to assumed design criteria as well as measurements of habitat and riparian vegetation conditions. As is often the case with retrospective studies, he was unable to perform any statistical analysis. He concluded that while some structures were still creating some habitat for resident fishes, many were having detrimental effects on channel conditions and riparian vegetation. They were causing or had caused erosion and channel widening, particularly at meander bends. Treated stream reaches had 75 percent less overhead riparian canopy than nearby untreated reaches.

This study did not depend on either reference stream or pre-treatment data. Its main limitation is the lack of statistical analysis and power of inference. All of Thompson's conclusions only apply to the sites he evaluated and cannot be extrapolated to other sites.

Despite the shortcomings of retrospective studies, the approach has been used on the California coast (Madej 2001) and there are at least two FRGP retrospective studies currently in process. Pacific Watershed Associates has a FRGP contract evaluating road decommissioning projects that uses a retrospective approach (W. Weaver personal communication). Also, David Lewis of University of California Cooperative Extension has a project that is a retrospective study of riparian restoration projects in the Russian River basin.

If the objective of the FRGP is to sample projects with the intention of having the power to infer to all treatments of a particular type, the retrospective study is not ideal. Retrospective studies can suffice for case studies of certain practices.

Another approach that has captured the attention of many restoration ecologists is to compare restored sites to "reference" sites assumed to possess the desired conditions (Harris 1999). For example, in Oregon, Jacobsen and Thom (2001) compared instream sediment and large wood conditions on restored stream reaches to the sediment and large wood conditions derived from a regional database of un-restored streams. They found significant differences between the restored and reference reaches (e.g., more fine sediment and higher wood loadings in restored reaches). Again, the limitation in coastal California is that no data on reference stream conditions have been analyzed, although a substantial amount of data has been collected over the years. Serious consideration should be given to assembling and analyzing this information. If and when reference data become available, this study design may be applied to restoration effectiveness monitoring. It would also be possible to use the retrospective study design employed by Roni and Quinn (2001).

For the intended application i.e., monitoring in order to determine the effectiveness of practices, the recommended approach is a before-after-control-impact (BACI) design. BACI is in wide use for impact assessment in the western United States, especially for stream monitoring (Kershner et al. 2003, Crawford and Johnson 2004). Since DFG implements many restoration projects that have similar objectives, a modified BACI design is well suited to assess their success or failure.

The BACI design was first described by Stewart-Oaten et al. (1986). The paired BACI approach statistically compares changes in restored areas (treatment) to similar, untreated areas (control) before and after treatment. Smith (2002) presents a number of models that can be used with a BACI design. There are three statistical approaches that can be used: analysis of variance, *t*-test, and regression analysis. Each approach has its strengths and weaknesses. Analysis of variance is quite powerful for detecting sources of variability and is appropriate for studies including covariates or other factors (Smith 2002, Kershner et al. 2003). Larson et al. (2004) used analysis of variance for evaluating trends in stream characteristics over time. Linear regression may be used to construct predictive models that may be validated through studies that track trends or changes over time. It has been used extensively in the fields of hydrology and watershed science (NCASI 1999). Under some circumstances, e.g., in studies seeking to attribute changes in stream conditions over time to environmental conditions, it may be appropriate

In the vast majority of cases, the recommended procedure for statistical analysis in a BACI design is the paired *t*-test (Sit and Taylor 1998) It is a simple yet very powerful analytic tool for detecting differences between means or between some mean and some threshold level i.e., quantified effectiveness criteria (Schwarz 1998, Crawford and Johnson 2004). Even small differences can be found to be statistically significant and can then be interpreted in relation to their biological significance. Use of a paired *t*-test is recommended here as the statistical procedure for evaluation of restoration effectiveness in the context of BACI studies.

Each difference between a response variable measured in the treatment and control area prior to treatment along with the difference between the response variable in the treatment and control after treatment constitutes a pair in a *t*-test The number of paired sites is the sample size. Further details on the BACI design are included in Sit and Taylor (1998) or other papers and texts (Smith 2002).

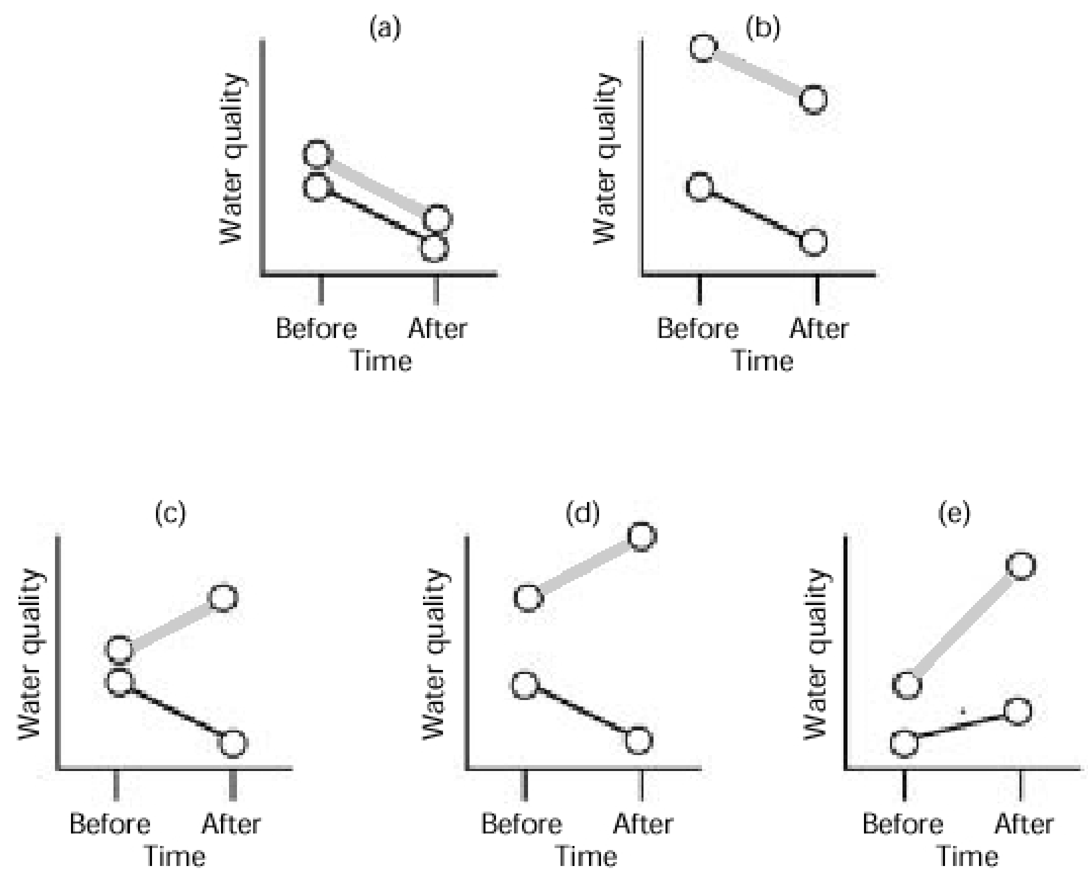



**Figure 2**. Simplified Outcomes of a BACI Design.
*Source*: Sit and Taylor, 1998. Dots are measurements of variables made before and after restoration; the control area (solid line), restored area (shaded line).

Several theoretical outcomes of a BACI study are shown in Figure 2. In (a) and (b), the parallel lines translate into no evidence of a difference due to restoration efforts. The difference in (b) between control and impact sites reflects water quality differences, but both sites experience the same temporal trend. In (c), (d), and (e), the amount change over time between the control and restored watersheds is variable, with the greatest absolute change occurring in (d) and the greatest percent change occurring in (c). All these changes are evidence of a time-treatment interaction, or that restoration has had an effect.

The main constraints to a BACI design will be the requirement for locating suitable controls and collecting pre-treatment data from treatment and control sites, reaches or watersheds. In the case of pre-treatment data, there are no established procedures or requirements currently in the FRGP for project applicants to provide these data. Consequently, either the program will have to be changed to incorporate these requirements or other provisions will be necessary. One option would be to delay implementation of approved projects long enough for DFG, project applicants or outside monitoring contractors to collect sufficient data. This would take advance planning e.g., monitoring studies to be undertaken would have to be available and funded, projects to be monitored would be selected (randomly or otherwise) in accordance with the study plan, etc.

The issue of suitable controls is discussed in the quantitative reports. Generally, control sites, reaches and watersheds need to be similar to the treated sites, reaches and watersheds in as many ways as possible. Controls also need to remain untreated for the duration of the study.

*Sampling Considerations*
For BACI studies, pre-treatment data is essential and in some, perhaps most studies using BACI design, it may be necessary to collect data from projects implemented over several years in order to achieve a required sample size. For example, a BACI study of the effectiveness of large woody debris placement might proceed as follows. The study objective would be to determine if large woody debris placement projects generally produce a greater frequency of pools and increased residual pool depths in treated reaches. Through a process of pilot sampling and analysis it is determined that 20 treated and control stream reaches would be a sufficient sample size for this study. In any given year, the number of funded and completed wood placement projects varies. Also, to ensure statistical validity, projects should be randomly sampled. To meet BACI design criteria, projects must be sampled prior to completion so that pre-treatment data can be collected.  In this hypothetical example, sampling might proceed in the following way. In year 1, five of 10 funded large wood projects are chosen for monitoring. In year 2, six of 12 large wood projects are chosen and in year 3, 9 of 17 projects are chosen. The total sample size would therefore be 20 treated sites and an equivalent number of controls. Pre-treatment monitoring would occur in years 1, 2 and 3 on the selected sites. Post-treatment monitoring would occur for two years at each site and would be completed on all sites in year 5. Accumulated data would be analyzed in year 5 and reports produced

*Study Examples*
The quantitative reports provide questions that might be addressed through monitoring of practices as described above. Below, two examples are presented to illustrate the potential use of the BACI study design. For examples of retrospective studies, the reader should consult Roni and Quinn (2001), Madej (1999) or current FRGP contracts awarded to David Lewis and Pacific Watershed Associates.

*Example 1.*  Assume that the following question is to be addressed: Are projects that reduce encroachment of vegetation into active channels generally effective? The aim of such projects would be to reduce vegetation encroachment on spawning gravels. This is an important consideration on streams below dams, such as the Trinity River. Assume that the criterion for effectiveness is that vegetation encroaching on the bankfull channel is reduced by at least 20 percent from its current level. To answer this question, eight sites are selected at random from all channel encroachment projects. These are selected over a two-year period to ensure an adequate pool of projects to sample from. Each site is paired with a control site. Pre- and post-treatment data are collected using methods in *Monitoring the Effectiveness of Riparian Vegetation Restoration*. After data collection, a table is set up with columns for entering data collected on vegetation cover at the treatment sites and control sites before and after the treatment (Table 2). Mean (average) cover is calculated before and after the treatments.

Analysis steps include calculating the difference in vegetation cover between the treatment and control sites before treatment and then after treatment. The difference between these two differences is then calculated.

**Table 2**. Instream vegetation cover data and differences for eight pairs of treatment and control sites (example data).

| Site | Before-Control | Before-Impact | *Before-Difference* | After-Control | After-Impact | *After-Difference* | After-Difference minus Before-Difference |
|---|---|---|---|---|---|---|---|
| | --------------------Percent-------------------- | | | | | | |
| 1 | 70 | 80 | *-10* | 70 | 50 | *20* | *30* |
| 2 | 50 | 30 | *20* | 55 | 25 | *30* | *10* |
| 3 | 22 | 35 | *-13* | 19 | 10 | *9* | *22* |
| 4 | 15 | 15 | *0* | 17 | 6 | *11* | *11* |
| 5 | 40 | 45 | *-5* | 35 | 20 | *15* | *20* |
| 6 | 7 | 6 | *1* | 9 | 4 | *5* | *4* |
| 7 | 25 | 33 | *-8* | 23 | 16 | *7* | *15* |
| 8 | 35 | 20 | *15* | 36 | 12 | *24* | *9* |
| Average | 33.00 | 33.00 | *0.00* | 33.00 | 17.88 | *15.13* | *15.13* |
| SD | | | | | | | *8.43* |

In this example, the mean pre-treatment vegetation cover is 33 percent at both control and treatment sites. If the quantified effectiveness criterion is a 20 percent reduction in cover, then the target or threshold reduction is 20 percent of 33 or 6.6 percent. This means that the mean vegetation cover must be reduced to 26.4 percent on the treated sites (33 percent - 6.6 percent), to declare that these vegetation removal projects are successful.

After treatment, the mean vegetation cover across all treated sites is 17.88 percent, while the vegetation cover has not changed at the control sites. This information is adequate for deducing that these eight projects (taken as a group) have been successful. However, statistical analyses must be completed to see whether the changes detected in this sample of projects may be inferred to vegetation removal projects in general. Changes at control sites are used in this statistical calculation.

A one-tailed *t*-test is employed with the null hypothesis, $H_0$, that the difference is less than or equal to 20 percent of the pre-treatment cover or 6.6 percent (i.e., vegetation removal projects are not successful). The critical *t* with $\alpha$=0.05 and 7 degrees of freedom is 1.90. If the computed *t*-value is greater than 1.90 the null hypothesis is rejected that the difference is less that 20 percent of pre-treatment cover (i.e., vegetation removal projects are successful).

$$t = (\overline{X} - d) / SE$$

or

$$t = (\overline{X} - d) / (SD / \sqrt{n})$$

or

$$t = (15.13 - 6.6\ ) / (8.4 / \sqrt{8})$$

or

$$t = 2.86$$

Since 2.86 exceeds the critical *t*-value of 1.90, the null hypothesis is rejected that the post-treatment cover has been reduced less than 20 percent of the pre-treatment cover. Assuming our sample adequately represented all vegetation removal projects, we can infer that the general category of vegetation removal projects is successful.

*Example 2.* Assume that DFG funds a number of projects involving mechanical removal of exotic species each year and that one type of treatment is giant reed (*Arundo donax* L.) removal. The question is: are these projects successfully reducing the cover of this exotic species? It so happens that enough of these projects occur every year to provide an adequate pool to sample from. In a particular year, eight areas along eight different streams throughout California are selected at random from all such projects to determine the effectiveness of the treatment. Eight suitable control sites are also selected. There may be a number of variables or factors to monitor in association with mechanical removal of exotic plant species, but in this example, the response variable is vegetation cover of the target plant species. The threshold for achieving effectiveness is a 20 percent reduction in existing cover of *Arundo donax* L.

Methods provided in *Monitoring the Effectiveness of Riparian Vegetation Restoration* are employed for this study. Prior to treatment, the vegetation cover of the species to be removed is measured at the treatment and control sites. Eight 50-foot transects are established within the treatment and control areas prior to treatment and measurements taken and recorded. The average cover of giant reed at each treatment and control site is computed and recorded as a single measurement for each (Table 3).

**Table 3.** Exotic vegetation cover percent for eight pairs of treatment and control sites (example data).

| Site | Before-Control | Before-Impact | *Before-Difference* | After-Control | After-Impact | *After-Difference* | *After-Difference minus Before-Difference* |
|---|---|---|---|---|---|---|---|
| | *Percent* | | | | | | |
| 1 | 7 | 9 | *-2* | 6 | 5 | *1* | *3* |
| 2 | 5 | 8 | *-3* | 4 | 6 | *-2* | *1* |
| 3 | 13 | 12 | *1* | 13 | 6 | *7* | *6* |
| 4 | 33 | 24 | *9* | 31 | 16 | *15* | *6* |
| 5 | 12 | 9 | *3* | 12 | 5 | *7* | *4* |
| 6 | 17 | 16 | *1* | 14 | 6 | *8* | *7* |
| 7 | 6 | 7 | *-1* | 6 | 5 | *1* | *2* |
| 8 | 4 | 8 | *-4* | 5 | 6 | *-1* | *3* |
| Average | 12.13 | 11.63 | *0.50* | 11.38 | 6.88 | *4.50* | *4.00* |
| SD | | | | | | | *2.14* |

The following year we return to the treatment and control reaches at about the same time of year and repeat the measurement of species cover. A table is set up for the analysis with columns for before-control, before-impact, the difference between before-impact and control, after-control, after-impact, the difference between after-impact and control, and the difference between the differences (Table 3).

The objective for these projects is to reduce the mean cover (11.63 percent) of giant reed by at least 20 percent, or 2.33 percent. . This means that the exotic vegetation cover must be reduced to 9.3 percent (11.63 percent - 2.33 percent), to declare that these exotic vegetation removal projects are successful.

The null hypothesis, $H_0$, is that the reduction is less than 20 percent of the exotic vegetation cover before treatment.

After treatment, the mean exotic vegetation cover across all treated sites is 6.88 percent, with a slight decrease in the mean exotic cover at the control sites. This information is adequate for deducing that these eight projects (taken as a group) have been successful (because mean exotic cover is less than 9.3 percent). Statistical analyses are then completed to see whether the changes detected in this sample of projects may be inferred to exotic vegetation removal projects in general. Changes at control sites are used in this statistical calculation.

The calculation is:

$$t = \frac{4 - 2.33}{2.14/\sqrt{8}} = 2.22,$$

the critical $t = 1.90$, for a one-tailed test with $\alpha = 0.05$ and 7 degrees of freedom. The $t$-value calculated here is 2.22 which is larger than 1.90. Therefore we reject the null hypothesis that the reduction is less than 20 percent of the before treatment site cover.

So, based upon these data it is concluded that exotic vegetation removal treatments met or exceeded their performance criteria for this year and this set of treatment sites. We also infer that the general category of exotic vegetation removal projects are successful based on this statistical analysis of the sample of eight projects.

This study could be extended over several years to see if the effect persists or diminishes over time. With sufficient data, an analysis of variance could be used to determine the effects of site variables on effectiveness.

*Conclusions*
The results of studies such as those described above indicate that relatively few site pairs may be sufficient to provide statistical inference that our projects have met performance criteria, especially within a single year of treatment. Actually, in the first year following most kinds of habitat improvement treatments there will likely be large differences between the treatment and control. More challenging is whether or not the treatment continues to meet performance criteria years hence. Additional sites might be necessary to achieve the statistical “power” needed to provide the answers to the question of the persistence of treatment results. As experience with such analytic approaches is gained, we can draw upon the computed variance for a particular treatment and response variable to target the appropriate number of treatments to include in an analysis.

## Monitoring Restoration Effectiveness at the Watershed Scale

*Monitoring the Effectiveness of Road System Upgrading and Decommissioning at the Watershed Scale* provides methods for evaluating the effectiveness of upland road restoration in reducing sediment inputs to streams. It recommends simultaneous monitoring at the site, road/stream reach, and small watershed scales. Monitoring at all scales permits evaluation of the sources and routing of effects throughout the system. Earlier work (Harris et al. 2002a, 2002b) indicated the difficulties of detecting restoration effects at the watershed scale, especially in watersheds larger than 100 acres. For example, at Caspar Creek, Ziemer (1998) reports that land use effects (in this case, timber harvesting) could only be detected at the sub-basin (<100 acre) scale. Such is the case for most watershed monitoring (NCASI 1999). Monitoring restoration effectiveness is not different than any monitoring attempting to determine land use impacts at the watershed scale.

A BACI approach is recommended for watershed effectiveness monitoring. Figure 3 indicates a potential lay out for a watershed monitoring study. Data may be collected in a pulsed or continuous monitoring design. For example, continuous monitoring of water quantity and quality at watershed outlets may be accompanied by monitoring at treatment sites after peak streamflow events at the site and stream reach scales.

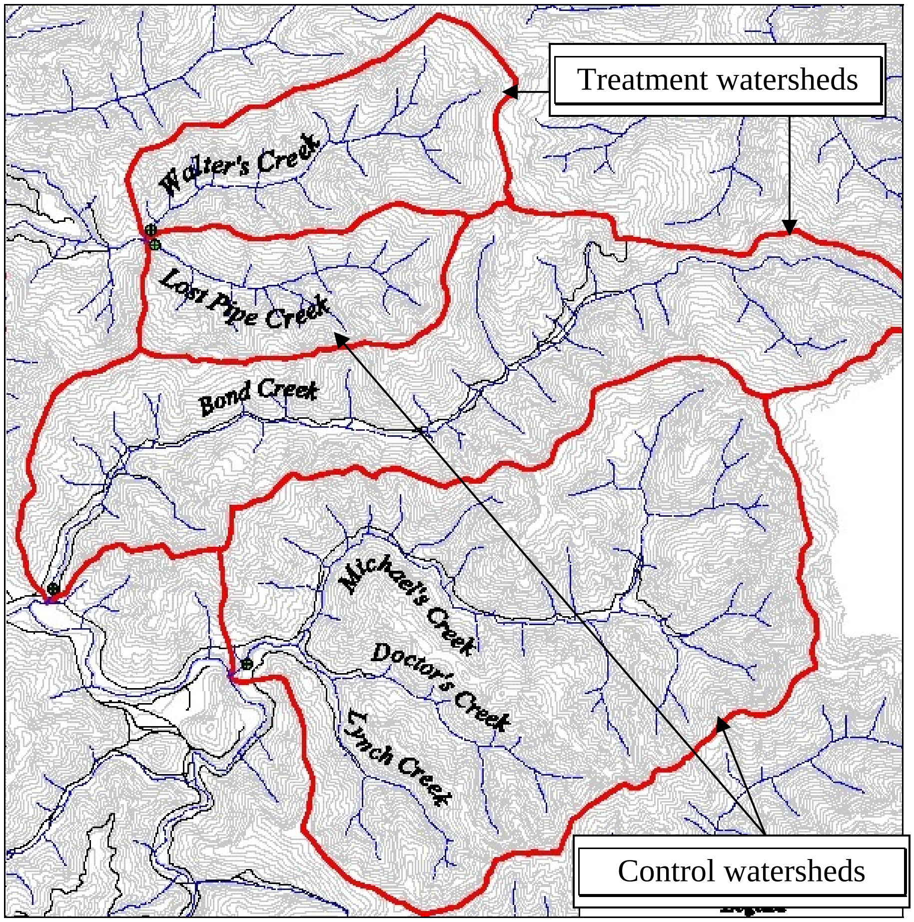



**Figure 3**. An example of a BACI paired-watershed study with two treatment and two control watersheds.

Control watersheds should be chosen on the basis of their similarity to treated watersheds in climate, vegetation, hydrology, and geomorphology. Control watersheds need to be kept as a control through the life of the effectiveness monitoring effort.

The paired watershed design facilitates use of a *t*-test for two dependent samples (i.e., measures of response variables, of which there may be many) when a number of watersheds are similarly treated and compared to their control watershed(s). Tests may also be performed at the site, stream reach and road reach scales as well. With sufficient data, sediment production models such as SEDMODL or WEPP could be validated in study watersheds (NCASI 1999).

### Quality Control and Quality Assurance

The primary goal of quality assurance and control (QA/QC) is to maintain good data quality by checking data collection standards and practices before, during, and after field work to ensure 1) appropriate data are being collected and 2) data are being collected in an appropriate manner. The intent of QA/QC is to minimize measurement error or bias as much as possible. Good quality can be attained by properly using the standardized field methods developed for implementation and effectiveness monitoring. Quality control can be strengthened through field inspection of the use of monitoring methods and a training program. Convention indicates that each monitoring study plan should have its own set of quality objectives and quality control methods

## REPORTING

An important objective for mounting a monitoring program is to increase the ability to annually report on accomplishments of restoration activities funded through the FRGP. Adequate reporting would include yearly summaries of projects undertaken and results of implementation and effectiveness monitoring. This can be accomplished with the results of qualitative monitoring. In addition to reporting on the overall program, the results of defined monitoring studies should be disseminated through administrative reports, peer-reviewed journal articles and presentations at conferences and symposia.

## SUMMARY

The procedures outlined here recommend qualitative implementation monitoring of all restoration projects. Qualitative effectiveness monitoring would be performed on 10 percent of all projects properly implemented every year. Qualitative monitoring of projects every year constitutes a methodology for assessing practices and would provide a basis for reporting on the overall performance of the FRGP. Quantitative monitoring of individual projects, groups of projects, and smaller watersheds would provide a more rigorous basis for judging the effectiveness of fisheries habitat restoration.

Adoption and implementation of these recommendations might be facilitated by changes in the process by which proposals to the FRGP are written, evaluated and chosen. To allow monitoring, proposals and funded projects should include much more definitive information on project objectives i.e., quantified effectiveness criteria. There is also a need for directed research studies or analysis of existing data to provide information on natural variability in monitoring parameters. Also, every year there should be a process for determining which funded projects are monitored for quantitative effectiveness. Finally, some process must be established to provide data as a baseline for implementation and effectiveness monitoring. The basic design for quantitative monitoring is a BACI approach. This is theoretically the most appealing design and is generally being used in other states and for other programs in California.

## LITERATURE CITED

Archer, E.K, B.R. Roper, R.C. Henderson, N. Bouwes, S.C. Mellison, and J.L. Kershaw. 2004. *Testing Common Stream Sampling Methods for Broad-Scale, Long-Term Monitoring.* Gen Tech. Rep. RMRS GTR-122. fort Collins, CO, USDA Forest Service, Rocky Mountain Research Station.

Collins, B.W. (editor). 2003. *Interim Effectiveness and Validation Monitoring Protocols.* California Coastal Salmonid Restoration Monitoring and Evaluation Program, California Department of Fish and Game, Fortuna, CA. 320 p.

Collins, B.W. 2004. *Personal communication.*

Crawford, B. A., and L. E. Johnson. 2004. *Procedure For Monitoring Effectiveness Of Riparian Planting Projects. MC-3.*. Washington Salmon Recovery Funding Board. 15 p.

Crawford, B., L. Singleton, P. Dickason. 2003. *Survey of Environmental Monitoring Programs and Associated Databases within Washington State.* State of Wash. Interagency Committee for Outdoor Recreation Report, Olympia, WA.

Crawford, B.A. 2004. *Personal communication*.

Downs, P.W. and G.M. Kondolf. 2002. Post-Project Appraisals in Adaptive Management of River Channel Restoration. Environmental Management 29:477-496.

Flosi, G., S. Downie, J. Hopelain, M. Bird, R. Coey, and B. Collins. 1998. *California Salmonid Stream Habitat Restoration Manual, Third Edition.* Sacramento, California, California Department of Fish and Game, Inland Fisheries Division.

Frissell, C.A., and R.K. Nawa. 1992. *Incidence and Causes of Physical Failure of Artificial Habitat Structures in Streams of Western Oregon and Washington*. North American Journal of Fisheries Management 12: 182-197.

Harris, R.R. 1999. *Defining Reference Conditions for Restoration of Riparian Plant Communities: Examples from California, USA.* Environmental Management 24:55-63.

Harris, R.R., S.D. Kocher, D. Lindquist, F. Kearns, N.M. Kelly and W.E. Weaver. 2002a. *Monitoring Fish Habitat Restoration Projects, Draft Interim Report to the Department of Fish and Game.* Center for Forestry and Center for the Assessment and Monitoring of Forest Environmental Resources, University of California, Berkeley, 123 p.

Harris, R.R., S.D. Kocher, J. Gerstein, F. Kearns, D. Lindquist, D. Lewis, J. LeBlanc, W.E. Weaver, and N.M. Kelly. 2002b. *Monitoring Fish Habitat Restoration Projects, Draft Final Report to the Department of Fish and Game.* Center for Forestry and Center for the Assessment and Monitoring of Forest Environmental Resources, University of California, Berkeley, 213 p.

Hicks, B.J., J.D. Hall, P.A. Bisson and J.R. Sedell. 1991. *Responses of Salmonids to Habitat Changes.* American Fisheries Society Special Publication 19:483-518.

Hopelain, J. nd. *Evaluation of Salmonid Habitat Restoration Structures in Northwestern California Streams Conducted During 1993 and 1995*. California Department of Fish and Game, Inland Fisheries Division.

Jacobsen, P. and B. Thom, 2001. *1998-2000 Habitat Restoration Effectiveness Monitoring for the Western Oregon Stream Project.* Oregon Department of Fish and Wildlife, Portland, Oregon. Monitoring Program Report Number OPSW-ODFW-2001-07. 25 pages.

Jacobsen, P. and K. Jones. 2004. *2000-2002 Effectiveness Monitoring For The Western Oregon Stream Restoration Program.* Oregon Department of Fish and Wildlife, Portland, Oregon. Monitoring Program Report Number OPSW-ODFW-2003-07. 59 pages.

Johnson, David H., Ned Pittman, Eva Wilder, Jill A. Silver, Robert W. Plotnikoff, Brad C. Mason, Kim K. Jones, Phil Roger, Thomas A. O'Neil, Charley Barrett. 2001. *Inventory and Monitoring of Salmon Habitat in the Pacific Northwest, Directory and Synthesis of Protocols* *for Management/Research and Volunteers in Washington, Oregon, Idaho, Montana and British Columbia.* Washington Department of Fish and Wildlife, Olympia, Washington. 212 pages.

Kaufmann, Philip R.; Levine, Paul; Robison, E. George; Seeliger, Curt; Peck, David V., 1999. *Quantifying Physical Habitat in Wadeable Streams*. Washington, D.C., U.S. Environmental Protection Agency, Environmental Monitoring and Assessment Program**,** Washington, D.C. EPA/620/R-99/003. 102 pages.

Kershner, J.L., M. Coles-Ritchie, E., Cowley, R.C. Henderson, K.Kratz, C.Quimby, D.M. Turner, L.C. Ulmer and M.R. Vinson. 2004. *A Plan to Monitor the Aquatic and Riparian Resources in the Area of the PACFISH/INFISH and the Biological Opinions for Bull Trout, Salmon and Steelhead.* Unpublished report. 104 p.

Kondolf, G.M., J.C. Vick and T.M. Ramirez. 1996. *Salmon Spawning Habitat Rehabilitation on the Merced River, California: An Evaluation of Project Planning and Performance*. Transactions of the American Fisheries Society 125:899-912.

Kondolf, G.M, M.W. Smeltzer and S.F. Railsback. 2001. *Design and Performance of a Channel Reconstruction Project in a Coastal California Gravel-Bed Stream*. Environmental Management 28:761-776.

Lacy, M. and B. Thom. 2000. *Restoration Effectiveness Monitoring Plan For The Western Oregon Stream Project, Version 2.0*. Oregon Department of Fish and Wildlife, Corvallis, OR.

Madej, M. A. 2001. *Erosion and Sediment Delivery Following Removal of Forest Roads.* Earth Surface Processes and Landforms 26:175-190.

Merz, J.E. and J.D. Setka. *Evaluation of a Spawning Habitat Enhancement Site for Chinook Salmon in a Regulated California River*. North American Journal of Fisheries Management 24:397-407.

Roni, P., and T.P. Quinn. 2001. *Distribution and Size of Juvenile Salmonids in Response to Placement of Large Woody Debris in Western Oregon and Washington Streams*. Canadian Journal of Fisheries and Aquatic Science 58:282-292.

Sit, B. and V. Taylor (editors). 1998. *Statistical Methods for Adaptive Management Studies.* Research Branch, British Columbia Ministry of Forests. Victoria, B.C. Land Management Handbook Number 42. 148 pages.

Smith, G. John. 1998. *Retrospective Studies*. In: Sit, V. and B. Taylor (editors). 1998. *Statistical Methods for Adaptive Management Studies*. Research Branch, British Columbia Ministry of Forests. Victoria, B.C. Land Management Handbook Number 42.

Stewart-Oaten, A, W.W. Murdoch, and K.R. Parker. 1986. *Environmental Impact Assessment: "Pseudoreplication" in Time?* Ecology 67(4): 929-940.

Schwarz, C.J. 1998. *Studies of uncontrolled events. In: Statistical Methods for Adaptive Management*. V. Sit and B. Taylor (eds.). Res. Br., B.C. Min. For., Res. Br., Victoria, BC, Land Manage. Handb. No. 42.

Smith, E.P. 2002. *BACI design. In: Encyclopedia of Environmetrics. Volume 1*. A.H. El-Shaarawi and W.W. Piegorsch (eds.) pp. 141-148.

Thompson, D.M. 2002. *Long-term Effect of Instream Habitat Improvement Structures on Channel Morphology Along the Blackledge and Salmon Rivers, Connecticut, USA.* Environmental Management 29:250-265.

Washington Salmon Recovery Funding Board. 2003. *Draft Monitoring and Evaluation Strategy for Habitat Restoration and Acquisition Projects*. SRFB MP-0, Olympia, WA.

Ziemer, R.R.. 1998. *Proceedings of the Conference on Coastal Watersheds: The Caspar Creek Story*. 6 May 1998; Ukiah, California. General Tech. Rep. PSW GTR-168. Albany, California: Pacific Southwest Research Station, Forest Service, U.S. Department of Agriculture; 149 p.