

# **REPRESENTATIVE RIVERS**

## **An Experimental Research Program In River Recreation Management**



### **AGRICULTURAL EXPERIMENT STATION**

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**REPRESENTATIVE RIVERS**  
**An Experimental Research Program**  
**in**  
**River Recreation Management**

by

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Bulletin 53

April 1983

## ACKNOWLEDGMENTS

First I wish to say "thanks" to Dr. Ted Dyrness who had faith in me and pushed for the original cooperative agreement with the U. S. Forest Service, and who also offered two extensions of the agreement so the final report might move in a different direction than earlier versions. Dr. Roger Clark, who acted as the technical advisor and reviewer for the agreement, helped immensely in the molding of the direction of the project. Drs. Robert H. Becker and William G. Workman diagnosed my problem in trying to prepare an acceptable program. And Drs. Steve McCool, University of Montana, and Mike Manfredo, Oregon State University, pointed to the need for reorganization and a more comprehensive research program.

Within my own bailiwick, Steve Cook's help in the development of the original manuscript is recognized and appreciated. And thanks to my wife, Jo, who at least pretended to understand when I brought home the manuscript, spread it out on the supper table, and mumbled something about reviewers.

This project was funded jointly by the Pacific Northwest Forest Experiment Station through the Institute of Northern Forestry and W-133 Regional Hatch Project here at the Agriculture Experiment Station, University of Alaska, Fairbanks.

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## PREFACE

Earlier attempts at synthesizing research needs and priorities were not fruitful. It became an exercise in which I subconsciously tried to produce a document that was acceptable through peer review. I styled my writing and format after other analyses that I had read. It seemed the natural thing to do — to look just like the others so mine would be accepted. At the same time, I tried to be different. It became a matter of shifting chairs around the table, but somehow the dinner still looked the same. I did not really realize what I was doing until Drs. Workman and Becker, in separate reviews, pointed out the problem. They simply said I was not covering new ground or even looking critically at river-recreation management. The second attempt was more progressive in terms of reviewing previous research and management theory, and proposing a new approach to river recreation management research; but it was too disjointed to be effective in communicating the problems of present research and means of overcoming those problems.

At least one thing became clear — most of the research done in recreation, particularly river recreation, was based on survey research designs which required a lot of data-crunching and liberal interpretation. As reproved by W. G. Workman, "Torture the data until nature confesses." In fact, much of the belief in the process of survey research appears to be related to the apparent ability to overcome inadequacies of research design by simply increasing sample size and then manipulating that data until some significant relationship is noted.

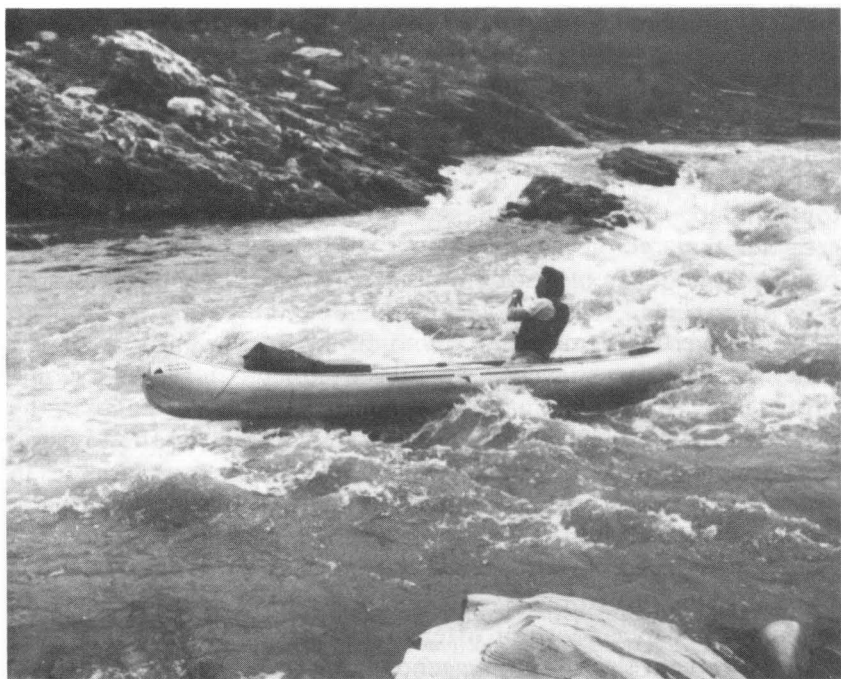


Figure 1. Canoeing Birch Creek.

## INTRODUCTION

This project was initiated in the spring of 1979 to assess the present river-management situation in Alaska and to project the need for river-recreation research in order to assist the manager. Both commercial and noncommercial uses of rivers in Alaska, as in other parts of the United States, have increased dramatically. The activities the river recreationists pursue are diverse, and range from on-water activities like floating, boating, and fishing to off-water activities such as camping, hiking, and relaxing. There are other competing uses such as residential development, mining and commercial fishing. The water itself is as diverse as the population it attracts — slow and fast, flat and seething, clear and roily, or urban and wildland. Management interests span public agencies such as the Federal agencies: U. S. Forest Service, National Park Service, Bureau of Land Management; state and local agencies:

Alaska Division of Parks, Alaska Department of Fish and Game, and city/borough government; and the private sector like outfitters, liveries, and land owners. Management programs vary from simply monitoring use to intensive control of behavior via a permit system to ration use.

Most of the reviewers of the earlier manuscripts indicated a real weakness in not fully developing a priority list of researchable topics. Since the purpose of the report was to synthesize river recreation research needs for Alaska and most problem analyses produce an agenda of research topics, the development of research priorities seemed to be a logical step. Yet, these agendas seem to only give very general direction to future research. Most of the time, the direction moves to wherever money is available for one-shot survey investigations. We seem to have forgotten that the most critical step, even in field-orientated studies, is the research design which includes the ability to control extraneous variables in order to test the effect of an experimental variable. Thus, the thesis developed in the paper is that, at present, the research process is more important than the agenda.

While the pressures have been great to develop a research agenda, this paper does not offer one. In fact, the author believes that this fervent search for an agenda has caused many scientists to overlook more basic needs such as addressing the appropriate research design. The intent of this paper is to examine the problems of research design, including historical cause/effect relationships, and then to propose a new process to overcome some of the problems.

Thus, what started as a simple problem analysis for river recreation research took on a new focus — assessing and addressing the problems of previous research. Lime (1977a) best summarized these problems as having:

1. Involved one-time studies without follow-up research, resulting in unique, noncomparable data.
2. Consisted of one-river case studies devoted primarily to understanding local conditions, again limiting generality.
3. Been poorly designed in terms of limited sample size, representativeness, and methodology.
4. Emphasized description rather than detailed analysis of processes and specific interrelationships.
5. Focused on studies of a single river activity at one time of the year (usually summer).

6. Documented only on-water activities (e. g., canoeing, rafting, fishing) and ignored riparian recreation users.
7. Been conducted on Western whitewater rivers that are unique both in location (usually remote from urban centers) and management (usually designated as a National Wild and Scenic River or within a National Park or Monument).
8. Been partially analyzed and/or inaccessible in that much of the research is buried in environmental impact statements, agency management plans, informational pamphlets for specific rivers, statewide outdoor recreation plans, academic dissertations, and unpublished manuscripts.
9. Been funded and/or encouraged by land managers who frequently want one-time, one-river, single-activity, descriptive studies that reflect local conditions. Seldom has research been funded to systematically study regional, even basin-wide, rivers as a system so recreation uses can be better planned and allocated to reflect the mix of experience desired by the public.

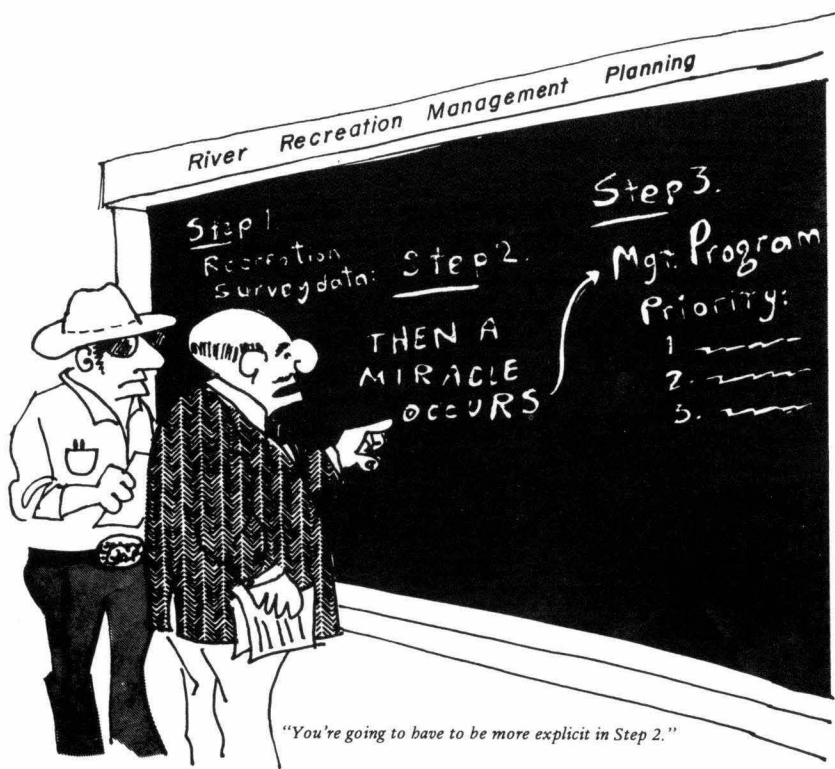
These comments are aimed primarily at sampling and research design. The problem is that we have allowed ourselves to become immersed in this type of survey research mode. The important question then is "how can we change that in the future to improve the research output?" The remainder of this paper will be devoted to assessing cause/effect relationships of the present research efforts, projecting a theoretical basis for addressing these problems, and then proposing a new program — Representative Rivers — operationalizing management theory into an effective research effort.

## HISTORICAL CAUSE-EFFECT RELATIONSHIP

Lime's summary of the problems of previous research (1977a) reflects on the one-shot case study — a preexperimental approach to research design, but this kind of design is only the effect, not the cause, of these problems. The cause is the dependency on management-funded studies for the primary research output. Unfortunately the need for these studies arises out of conflicts in which the manager needs answers immediately. Usually this occurs after the introduction of some change in the system when the manager wants to know "what happened" and "what can he do about it." As suggested in Figure 2, this dependency casts some doubts on the data and the interpretation based on that data.

More important, however, is the cause of the situation because we must work on the cause to solve the problem. The cause, as discussed earlier in this section, is the reliance on management-funded studies, but tantamount to that is the belief in the carrying-capacity concept as the primary management model. This belief has led to confusion of management and research roles and ultimately to an unhealthy interdependency that maintains that confusion.

The idea of carrying capacity began in the 1930s (Leopold, 1934) and surfaced as a primary research concern in an analysis of research problems by Dana (1957). This was reemphasized by the Outdoor Recreation Resources Review Commission (1960). Wagar (1964) more fully developed the concept by defining recreational carrying capacity and the role of the manager in establishing that capacity. Any capacity limits were based on the objectives the manager had established for the area. The bulk of the research literature and management recommendations has since revolved around carrying capacity as the basic management model. Becker and Jubenville (1982) have described this belief in the model as the "search for a magic number" — but there is no magic number, only some upper limit based on established management objectives. Researchers can only isolate, measure, and



**Figure 2.** The one-shot case-study approach to recreation-management research.

interpret variables that are integral to the concept but cannot “determine” a given carrying capacity.

*Carrying capacity* began to show in the titles of research articles, particularly social carrying capacity. Managers, insecure in their roles because of their limited training in recreation management, confronted by burgeoning numbers of participants, and burdened by pressures for increased resource development, sought a realistic solution to the increasing problem. They turned to the academic community to provide the answers. Thus began the present period of role confusion. The manager began to take on a quasi-research role through funding of one-shot case studies “of hard data (by researchers) supporting a carrying capacity” (Hartman, 1977). The researcher in turn began to take on a quasi-management role of helping “the manager with his most difficult

problem — selecting a use figure” (Heberlein and Shelby, 1977). This belief in the carrying capacity concept has little empirical (Burch, 1981) or theoretical (Jubenville and Becker, 1982) foundation, but the symbiotic relationship between the manager and researcher should continue because of the “guaranteed-success syndrome” (Jubenville and Workman, 1980). The guaranteed-success syndrome is an outgrowth of the social-succession theory, which usually stems from on-site studies of present users about particular management programs that have already been implemented. The “success” of the program is guaranteed because, given sufficient lapse of time in between the introduction of the program and the measurement of its success, the user groups that agree with the program will succeed the former users and become the new stable clientele. The underlying assumptions are that there is a choice of recreational opportunities and it is a “personal and free choice” (Driver and Tocher, 1970). Social succession, as a theory, will be discussed in more detail later.

Management dollars are poured into the research community for a quick turn around resulting in data outputs to be used in present management. The researchers, limited by time or money, are unable to address critical research design questions and usually develop some preexperimental design (Campbell and Stanley, 1963) in which no experimental control is possible. Typically the researcher employs an *ex post facto* preexperimental design, in which conditions existing prior to the introduction of an experimental variable are not known. This design is diagrammed as:

(O)   X   O

In management-funded studies, where the manager is interested in the impact of a newly introduced program (for example a permit system to limit numbers of people), X represents the new program. In research language, X is the experimental variable or the agent of change introduced into the system to test the effects of that variable. In *ex post facto* studies, there is no systematic documentation of the existing conditions prior to introducing the experimental variable. The researcher is dependent upon the manager to reconstruct those conditions as best as he can. Thus, the observations made prior to introducing X, shown as (O), are often based on supposition or limited data having wide confidence limits (Jubenville, 1970). Without the knowledge of the original clientele and their perceptions, it is impossible to assess impacts of the pro-

grams, or the effects of change introduced into the system. The results are often of limited scientific value and are usually loosely interpreted into management recommendations on capacity limits or related management problems.

In sum, the manager has often abdicated his management responsibility by simply turning to the academic community for determination of the carrying capacity of an area. He hopes that this information will simplify the management of the area, not understanding the potential consequences of that type of decision-making on the user groups he purports to serve. Even if the studies are not actually addressing carrying capacity, the underlying construct is often the carrying-capacity concept as indicated by such terms as *control of crowding* (Lime, 1977b), *mandatory permits* (Hendee and Lucas, 1973), *allocating use* (Utter, 1977), *restricting recreational use of wildlands* (Schreyer, 1977), and so on.

Unfortunately, the belief in the carrying capacity concept is sometimes so strong as to cause the researcher to ignore the empirical data in order to ensure success of the outcome (Burch, 1981). Bultena, Albrecht, and Womble (1981) and Bultena, Field, Womble, and Albrecht (1981), working the same data base in two different articles in the same periodical, presented empirical evidence in one article that, if the manager of Denali National Park were to essentially eliminate the present capacity limits for the backcountry management zones, the average daily interparty-contact would increase from 0.7 parties per day to 0.9. There was no statistical difference noted between the two means, and both were well under the norm of not encountering more than two other parties per day which was used as the basis for the original capacities for the backcountry management zones. Yet, in the other article, they concluded:

The presence of a stringent rationing policy may be affecting the types of recreational clientele attracted to McKinley Park. Implementation of policies permitting relatively unconstrained access and complete freedom of camping activities in the Park would likely bring the displacement of some "wilderness purists" by persons seeking less density-sensitive values in their wilderness activities. Presumably, this new clientele group would be more receptive to the development of the backcountry than were our respondents. Not only would there likely be more public support for development programs, but such pro-

grams undoubtedly would be necessitated to protect the quality of the resource base. Central to future decisions about rationing at McKinley Park is the issue of whether or not the experiential goals of wilderness purists should outweigh the benefits to be obtained from granting more persons access to this majestic, and unique, natural environment. This issue will become increasingly volatile given, on the one hand, the exclusionary nature of the present policies for controlling backcountry use and, on the other hand, the intensifying public pressures (especially in Alaska) for accommodating a burgeoning demand for outdoor recreation (Bultena, Albrecht, and Womble, 1981).

This neo-Malthusian assumption on the part of the researcher "distracts us from more crucial issues. The consequence may be that a false perception of resource capacity leads us to the imposition of regulations, which indeed do create scarcity" (Burch, 1981). Ironically, the research design developed by Bultena, Field, Womble, and Albrecht (1981):

$$\begin{array}{r} X \quad O \\ \hline O \end{array}$$

is a real improvement over the typical X O design because it does allow control of several factors affecting internal validity (Campbell and Stanley, 1963). In this design, X, placed above the line, represents a change in present capacity limits by taking some of the zones which previously had a capacity limit of three parties per day and raising them up to thirty parties per day. The remaining backcountry management zones were maintained at the previous level of three parties per day — control zones. Observations, O, were then made for both experimental and control zones on interparty contact. The result, as indicated earlier, was an insignificant increase from 0.7 parties contacted in the control zones to 0.9 parties in the experimental zones.

At present time, with the limited research dollars available to the academic community, and managers with both available dollars and problems to solve, the historical, symbiotic relationship should continue. Without reflecting on the complicity in the matter, the first step is to recognize what is happening in the one-shot case-study approach and then to search for a new process in

funding and conducting recreation management research that requires the addressing of crucial research issues.

The remaining sections develop a theoretical framework for management research and offer a new process — in this case, focusing on river recreation — called *representative rivers*.

## A THEORETICAL RESEARCH FRAMEWORK

This disillusionment was shared by both observer and participant in experimentation. For the experimenters, a personal avoidance-conditioning to experimentation can be noted. For the usual highly motivated researcher the nonconfirmation of a cherished hypothesis is actively painful. As a biological and psychological animal, the experimenter is subject to laws of learning which lead him inevitably to associate this pain with the contiguous stimuli and events. These stimuli are apt to be the experimental process itself, more vividly and directly than the "true" source of frustration, i. e., the inadequate theory. This can lead, perhaps unconsciously, to the avoidance or rejection of the experimental process. If, as seems likely, the ecology of our science is one in which there are available many more wrong responses than correct ones, we may anticipate that most experiments will be disappointing. We must somehow inoculate young experimenters against this effect, and in general must justify experimentation on more pessimistic grounds — not as a panacea, but rather as the only available route to cumulative progress. We must instill in our students the expectation of tedium and disappointment and the duty of thorough persistence, by now so well achieved in the biological and physical sciences. We must expand our students' vow of poverty to include not only the willingness to accept poverty of finances, but also a poverty of experimental results.

— Campbell and Stanley (1963)

As pointed out by Cook and Campbell (1979), "All experiments involve at least a treatment, an outcome measure, units of assignment, and some comparison from which change can be inferred and hopefully attributed to the treatment." Under laboratory conditions, control of extraneous variables can usually be accomplished to measure, with some stated degree of validity, the effects of the particular treatment. Because of the field setting in

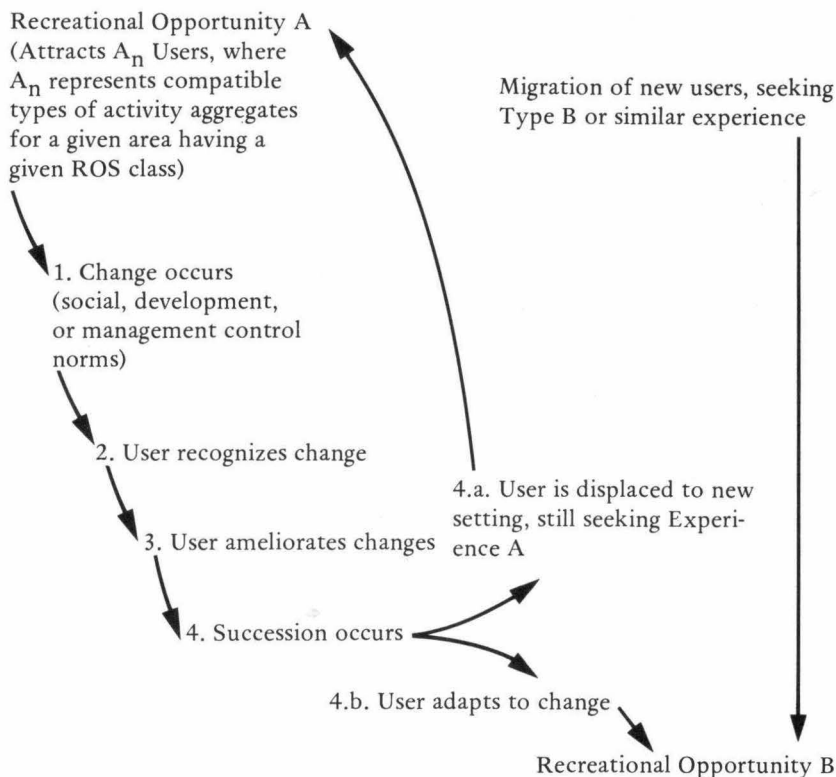
recreation management, the difficulty in controlling extraneous variables and, as stated previously, the need for immediate results, most research designs have been preexperimental ones. However, the lack of complete control of extraneous variables in field situations is not unique to recreation and has led to the development of the theory of quasi-experiments (Cook and Campbell, 1979). No environment offers total control; thus, the issue then is the amount of control. To ignore it as an issue may well lead the river recreation researcher, as stated earlier, to the "guaranteed success syndrome." For whatever reason, in a given experiment, the major function of control is to rule out threats to valid inference.

Any specific experimental effort should then address the issue of control through experimental design, including peculiarities of the particular discipline and its underlying theory.

## RECREATION MANAGEMENT THEORY

As indicated in the quote by Campbell and Stanley (1963), the true source of disappointment in the experimentation process is often inadequate theory. In recreation management research, there are vagaries of user behavior that may be inexplicable but, for the most part, behavior is predictable and consequently fits well in a theoretical construct. It is important then that the theory capture the dynamics of user behavior in a management framework. Jubenville (1981) expressed a concern for those behavioral issues which affect the overall research design but which cannot be addressed in the one-shot, questionnaire survey. These behavioral issues come into focus through the social-succession theory and the recreation management model suggested by Becker and Jubenville (1982).

**Social Succession Theory.** As depicted in Figure 3, assume that an individual is attracted to the Type A recreational-opportunity setting (Jubenville, 1981). He seeks as much information as possible to isolate the environmental setting that appears to best fit his interests, including the activity aggregate appropriate to that type of setting. Actually "succession" may appropriately take place during this planning phase. Some areas may appear to be



**Figure 3. Social succession in a recreational setting.**

desirable; but once the details are uncovered, the area may be dropped from further consideration. The recreationist may then review others to find a candidate area. There is no guarantee that every Type A area will be acceptable. The person may try several until he finds the "best" one and then return often to participate in that particular setting because he finds "his experience" satisfying.

If social succession takes place beyond that, the cause is *change* introduced into the system which is not tolerable nor mitigable by the individual. If the setting changes and causes displacement because the changes are so drastic that the individual cannot ameliorate their impacts on his individual experience, then the displacement is the *effect* of some cause initiated by the

manager — notwithstanding that some changes in the setting may not be directly controllable by the manager. The model, however, indicates that the manager is responsible for managing the recreational opportunity setting and the individual is responsible for managing his own experience. The manager must then stabilize the opportunity setting so the recreationist can make rational choices in satisfying his own experiences.

Without further elaboration on social succession, it seems obvious then that any research design must address the behavioral dynamics suggested in Figure 3. The succession model provides the window through which behavior should be observed and the results interpreted. If you simply “cut it” at one particular time with a one-shot, case-study, *ex post facto* design, you should always find a satisfied clientele, because those who are not satisfied have left. You will not be able to interpret the results fully because you do not know the particulars of the behavioral dynamics. There are no “before” observations and no opportunity for control of critical extraneous variables. Burch (1981) expressed similar dismay over the lack of “longitudinal and systematic studies that shaped the [management] decisions.”

Jubenville (1981), using the social-succession model, pointed to two peculiarities in recreation behavior that need to be addressed through appropriate research design:

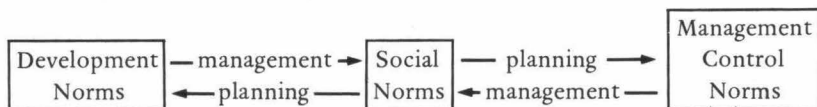
... the need for longitudinal studies. Sampling is really the window through which you observe behavior. This window, or timing of sampling, has to be large enough to capture the dynamics of social succession; otherwise, one would not know how to choose the subjects nor control extraneous variables in relation to these subjects.

This brings out the second peculiarity — the need to disaggregate subjects based on the experiences sought. One needs to be able to distinguish between Type A and Type B users, etc. If, as suggested by Becker (1978), satisfaction is an experiential function, then we must be able to control for experience. From the researcher's point of view, the ideal would be to have a paper and pencil test to distinguish between experience types. Hendee et al. (1968) offered such an instrument, called the Wilderness Scale, for distinguishing between classes of roadless experiences. Becker (1978) used observed densities to disaggregate user experience types. There are surely many

other creative techniques for disaggregating. The important concern is the recognition of the need to do so.

One of the primary means of control, as suggested above, is the disaggregation of users into homogeneous subsets corresponding to the opportunity-setting classes of the Recreation Opportunity Spectrum (Clark and Stankey, 1979). By controlling for opportunity class and, possibly, activity aggregates within classes, the results should be more meaningful to the managers who would like to transfer the results of the research to their own similar situations.

**Basic Management Model.** Understanding the basic management model is important in the research design if the research is to address those factors that are manipulable by the manager. The basic model, assuming that there are attractors within an area for particular activity aggregates, is (Becker and Jubenville, 1982):



As suggested by Frissell and Stankey (1972) and Clark and Stankey (1979), the social norms stated in managerial terms are the probability of visitor contact and acceptance modification of the environmental setting, either through recreational development or other resource uses. The social norms are the primary goals for a given area; the development and managerial norms are those tools he has available to him for achieving those goals. The management-control norms, taken here to mean direct visitor manipulation — on or off the site — are not the primary means of achieving stable social norms in the wildland settings (Stankey and Baden, 1977; Peterson and Lime, 1979). The primary achievers and stabilizers of the social norms are the development norms in terms of access and facility development and related services (Becker and Jubenville, 1982). Some further manipulation through management control, such as limiting forest camping to 7 days at the particular site, may be necessary as a secondary stabilizer. This is the essence of Wagar's (1964) thesis: use (social norms) will come into equilibrium with the setting (development norms). To ignore the development norms and simply try to achieve the desired social norms through direct manipulation of the user

(management control norms) will probably produce an artificial result that will be difficult to interpret managerially.

Wilderness is a good example of the effect of ignoring the development norms superimposed on the roadless landscape and then trying to revert use to previous levels using direct management controls. The increased access, internally and externally, plus the lack of good substitutes, has forced people into a “catch all” anchor point along the roadless portion of the opportunity spectrum. While superficially the group may appear to be homogeneous, because each group member was attracted to the same area, in reality it is probably very heterogeneous (Figure 4). As-

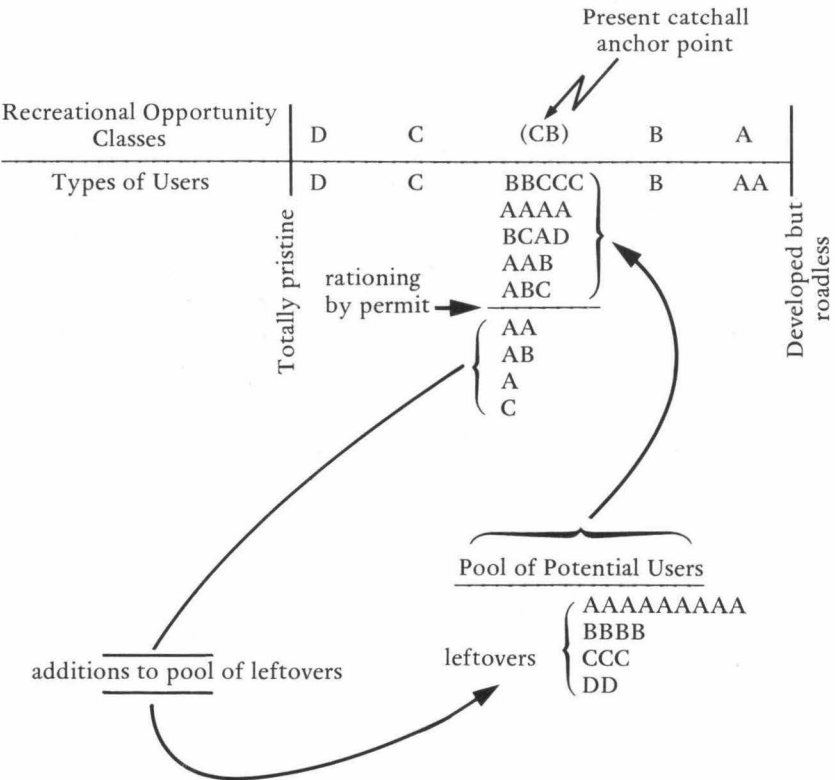
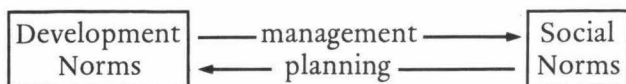


Figure 4. The theoretical migration of roadless recreational users to anchor point along the opportunity spectrum under existing management strategy where we attempt to manage for heterogeneous rather than homogeneous user populations.

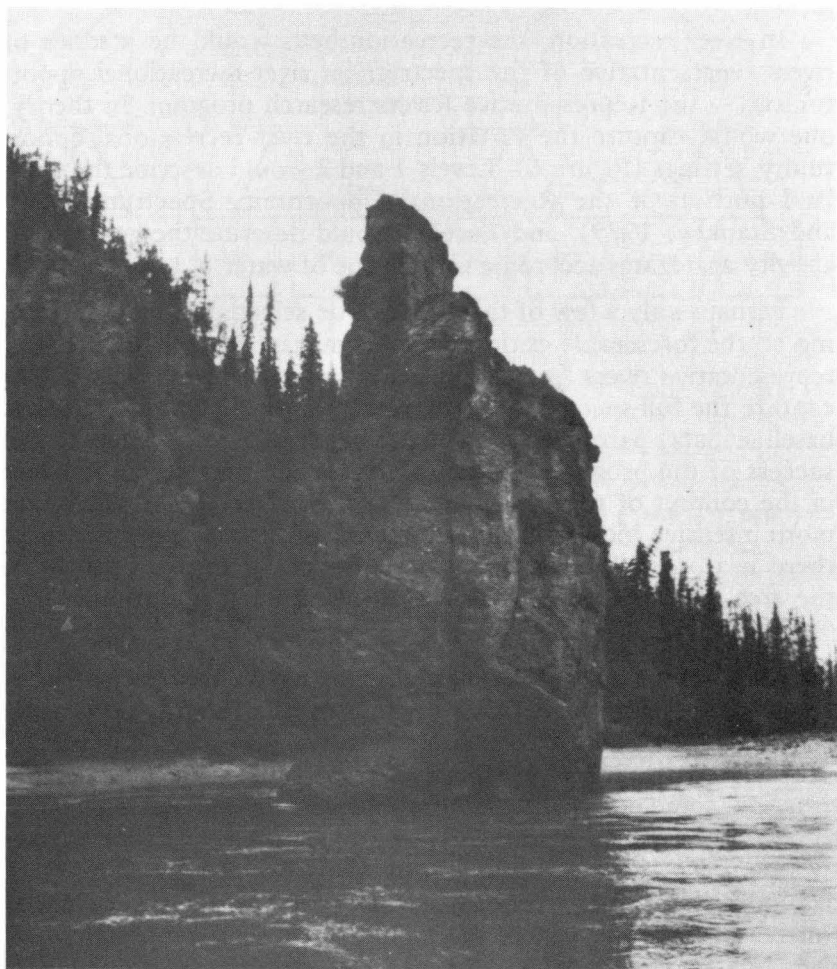
suming the area is to be managed as a Type D opportunity class (pristine wilderness), the development norms gave the users mixed signals which attracted many offsite users — the majority of which were not the original intended user. Furthermore, because the area is no longer attractive to the Type D user, any direct management control (such as a permit system) will not revert the area to former users (Type D). It will only revert to former numbers with a clientele that has, by its participation, stated that it is willing to live with higher levels of social interaction. But the development norms that shift to (CB) on the contiuum communicate to the user that this is a (CB) recreational opportunity.

Thus, in a wildland setting, the direct management controls are only secondary stabilizers of the recreational opportunity setting; the real stabilizers are the development norms placed on the landscape:



This then would mean that the development norms used in the recreation opportunity spectrum (ROS) by the manager should also be the same norms used in the initial control in the research design. Other controls for extraneous variables should focus on the peculiarities of the specific activity aggregate. In river recreation, the uniqueness of possible one-way traffic, the type of water (flat or whitewater), or similar influences may also be used as other controls in research designs.

**REPRESENTATIVE RIVERS — A RESPONSE  
TO THE THEORETICAL FRAMEWORK**



**Figure 5. Standing Rock, Middle Fork of the Koyukuk River,  
near Wiseman, Alaska.**

(photo by W. D. Workman)

Jubenville (1981) presented a new research concept called *recreationshed* — a designated research area (opportunity setting) where long-term research programs could address some of the problems of research designs. Without such efforts, there will be very little long-term benefit of recreation research because the manager would not be able to readily extrapolate to his own situation. This extrapolation, or technology transfer, is presently a *fuzzy* concept as suggested earlier by Figure 2, because of the lack of research controls in the experimental designs.

In river recreation, the recreationsheds would be a series of rivers representative of the spectrum of river recreational opportunities — the Representative Rivers research program. In theory, one would capture the variation in the river recreational opportunity settings (Figure 6). Levels 1 and 2 would describe the wild-land portion of the Recreational Opportunity Spectrum (Clark and Stankey, 1979), and Level 3 would describe the generalized activity aggregates according to the type of water.

Perhaps only a few of these would be selected at first, depending on the foreseeable critical needs of management. Ultimately all representative rivers (recreationsheds) would be brought on line to capture the full spectrum of river recreational opportunities. Good baseline data, particularly on social behavior, are essential to the success of the program. Biological data is also important, but only in the context of the behavioral use of the river. It is possible that more intensive biological measurements might be important where there is a possibility of some systematic effect on the ecology of the area such as erosion caused by logging, water pollution caused by mining, etc.

While it would not be desirable to have comparative rivers (rivers paired experimentally) because of the geographic separation, difference in attractors, access, etc. it may be desirable to establish a "sister" river for each representative river. In this situation, some minimal baseline would be recorded and correlated to the representative river to ensure similarity of representativeness. This would guard against the loss of a given river if circumstances such as access, ownership, development, etc. changed. Further, if someone wanted to conduct a study on a representative river but the study did not fit into the overall program, it could possibly be directed to the sister river.

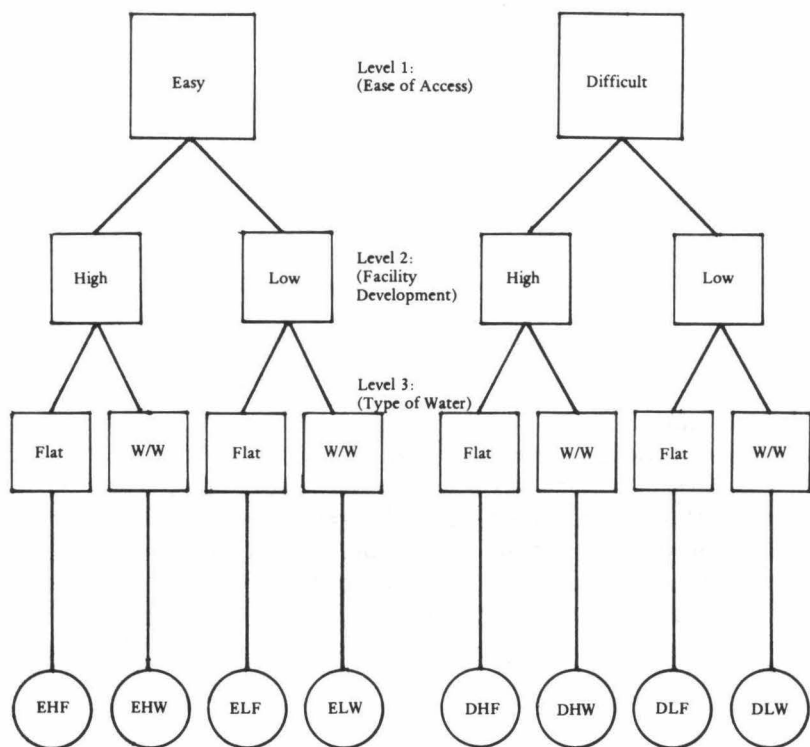


Figure 6. A theoretical system of representative rivers:

Managerially the Representative Rivers program should prove helpful because the data provided can be transferred easily by the manager. He can match his river with a similar experimental river and extrapolate to his own situation. More importantly, the researcher can now address critical design issues in his dedicated long-term effort. The continual collection of verifiable baseline data prior to the introduction of some experimental variable (such as a new management program) allows the researcher to begin to control for extraneous variables, even under field conditions. While complete control is not possible, Cook and Campbell (1979)

point to a need for control of the more important variables affecting the outcome; they call this quasi-experimentation. The researcher must then identify those extraneous variables critical to his own research problem and then select a quasi-experimental design to control for such. The Representative Rivers program does address the aforementioned need for disaggregation (the system itself is disaggregated along the recreational opportunity spectrum guidelines), and longitudinal collection of data allows the capturing of the dynamics depicted under the social succession theory.

Campbell and Stanley (1963), in earlier works, summarized the quasi-experimental designs and sources of invalidity associated with each (Table 1, pages 22-23). The effects represented in the table are described below:

### Internal Validity

1. *History*, the specific events occurring between the first and second measurement in addition to the experimental variable.
2. *Maturation*, processes within the respondents operating as a function of the passage of time per se (not specific to the particular events), including growing older, growing hungrier, growing more tired, and the like.
3. *Testing*, the effects of taking a test upon the scores of a second testing.
4. *Instrumentation*, in which changes in the calibration of a measuring instrument or changes in the observers or scorers used may produce changes in the obtained measurements.
5. *Statistical regression*, operating where groups have been selected on the basis of their extreme scores.
6. Biases resulting in differential *selection* of respondents for the comparison groups.
7. *Experimental mortality*, or differential loss of respondents from the comparison groups.
8. *Selection-maturation interaction*, etc., which in certain of the multiple-group quasi-experimental designs, such as Design 10, is confounded with, i. e., might be mistaken for, the effect of the experimental variable.

### External Validity

9. The *reactive* or *interaction effect* of *testing*, in which a pretest might increase or decrease the respondent's sensitivity or responsiveness to the experimental variable and thus make the results obtained for a pretested population unrepresentative of

the effects of the experimental variable for the unpretested universe from which the experimental respondents were selected.

10. The *interaction* effects of *selection* biases and the *experimental* variable.
11. *Reactive effects of experimental arrangements*, which would preclude generalization about the effect of the experimental variable upon persons being exposed to it in nonexperimental settings.
12. *Multiple-treatment interference*, likely to occur whenever multiple treatments are applied to the same respondents, because the effects of prior treatments are not usually erasable. This is a particular problem for one-group designs of type 8 or 9.

While all the designs may work if applied creatively, Jubenville (1981) suggested that the time series (No. 7) and equivalent time samples design (No. 8) offer the greater possible application. These two designs would be reasonably easy to implement in a recreational setting and provide control for many of the variables affecting internal validity. The use of the representative rivers themselves should improve external validity by reducing the interaction effects of selection biases and the experimental variable. The user's selection of the river would be based on the representative norms, thus reducing the probability of chance selection by atypical users (those not aligned with their desired opportunity setting, Figure 4.) whose reaction to the experimental variable (such as a permit system) would impose confounding effects on the results. Plus, the reactive effects of the experimental arrangements, providing they were unobtrusive (Webb et al., 1971) and not perceived as change within the recreational opportunity setting, should be minimized because the particular river was chosen as being representative of a class of recreational rivers. Cook and Campbell (1979), in their book, *Quasi-Experimentation: Design and Analysis Issues for Field Settings*, discuss control issues in detail and appropriate statistical analysis for the individual quasi-experimental designs.

Table 1. Sources of invalidity for quasi-experimental designs 7 through 12.

	Sources of Invalidity									
	Internal								External	
	History	Maturation	Testing	Instrumentation	Regression	Selection	Mortality	Interaction of Selection and Maturation, etc.	Interaction of Testing and X	Interaction of Selection and X
<i>Quasi-Experimental Designs:</i>										
7. Times Series O O O O X O O O O	-	+	+	?	+	+	+	+	-	?
8. Equivalent Time Samples Design $X_1 O X_0 X_1 O X_0 O$ , etc.	+	+	+	+	+	+	+	+	-	?
9. Equivalent Mat. Samples Design $M_a X_1 O M_b X_0 O M_c X_1 O M_d X_0 O$ , etc.	+	+	+	+	+	+	+	+	-	?
10. Nonequivalent Control Gr. Design $\frac{O \quad X \quad O}{O \quad O}$	+	+	+	+	?	+	+	-	-	?
11. Counterbalanced Designs $\frac{X_1 O \quad X_2 O \quad X_3 O \quad X_4 O}{X_2 O \quad X_4 O \quad X_1 O \quad X_3 O}$ $\frac{X_3 O \quad X_1 O \quad X_4 O \quad X_2 O}{X_4 O \quad X_3 O \quad X_2 O \quad X_1 O}$	+	+	+	+	+	+	+	?	?	?
12. Separate-Sample Pretest-Posttest Design R O (X) R X O	-	-	+	?	+	+	-	-	+	+

Table 1. *Continued*

		Sources of Invalidity									
		Internal							External		
		History	Maturation	Testing	Instrumentation	Regression	Selection	Mortality	Interaction of Selection and Maturation, etc.	Interaction of Testing and X	Interaction of Selection and X
12a.	$R \quad O \quad (X)$ $R \quad (X) \quad O$ <hr/> $R \quad O \quad (X)$ $R \quad X \quad O$	+	-	+	?	+	+	-	+	+	+
12b.	$R \quad O_1 \quad (X)$ $R \quad O_2 \quad (X)$ $R \quad X \quad O_3$	-	+	+	?	+	+	-	?	+	+
12c.	$R \quad O_1 \quad X \quad O_2$ $R \quad X \quad O_3$	-	-	+	?	+	+	+	-	+	+

Note. In the table, a minus indicates a definite weakness, a plus indicates that the factor is controlled, a question mark indicates a possible source of concern, and a blank indicates that the factor is not relevant.

It is with extreme reluctance that this summary table is presented because it is apt to be "too helpful," and to be depended upon in place of a more complex and qualified presentation . . . No + or - indicator should be respected unless the reader comprehends why it is placed there. In particular, it is against the spirit of this presentation to create uncomprehended fears of, or confidence in, specific designs (Campbell and Stanley, 1963).

## OPERATIONALIZING THE REPRESENTATIVE RIVERS PROGRAM

The Representative Rivers program is management oriented in that it would represent the primary management classes in the recreation opportunity spectrum and the primary activity aggregates. In addition, there would be opportunities for controlling threats to valid inference that are not possible under the present one-shot, case-study approach. The program can also take advantage of the incidental/accidental introduction of change into a given river system because of the ongoing collection of baseline data prior to the introduction of the experimental variable X — the time series design. Thus, the Representative Rivers program should improve transfer of knowledge to other management situations because we would be able to eliminate or drastically reduce confounding effects in studying particular management programs.

It should also be attractive to those researchers who have funding and are looking for an appropriate field research setting where the recreational opportunity setting is controlled and good baseline data exist for further experimental control.

The assumption is that agencies, which heretofore spent monies on the one-shot case studies, would pool their monies and enter into a cooperative research effort. The representatives of the agencies would form the oversight group (board of directors) which, along with the research director, would establish the actual criteria for selecting the rivers and focus on specific research priorities. The director would be responsible for the establishment and ongoing collection of baseline data and, ultimately, for acting as a research broker. The specific research needs would be identified within the representative rivers; the research design issues would be specified; and then the request for proposals within those project design and monetary constraints would be sent out to the research community. A proposal review committee of scientists and managers would select those proposals that best

address the topical and design questions within the dollar constraints.

The results from these river studies would be immediately available to the agencies for use in upgrading their own programs. Ideally, scientists and allied managers would be available to co-operating agencies for consulting on specific problems on other rivers. In addition, planned periodic short courses and new employee training could also be used as program techniques in the technology transfer.

It is essential that the diversity captured in the representative rivers reflect primarily the recreation opportunity settings. The program should not get bogged down into simply seeking diversity by selecting lands and waters from all the agencies. This is because a given recreational opportunity setting should have a structural social stability that transcends agency boundaries (Clark and Stankey, 1979).

The intent is to focus the limited dollars on specific research requirements for Alaskan rivers. Only a small percentage of the dollars would go into administration of the program; the majority would go into the identified, management-oriented research projects and the transfer of the information to the manager. As a part of this, some minimum logistical support should be developed to encourage nonaffiliated or nonconsortium funded projects, were they to fall within the scope of the identified research needs.

In sum, all of the problems of river-recreation research identified by Lime (1977a) and reviewed in the beginning of this bulletin — research design, sampling, comprehensiveness, and technology transfer — are addressed in the Representative Rivers program. It is a first in recreation research in that management and research are equal partners — each enhancing the other, but each respecting the role of the other.

Ultimately, good management is dependent on good research. The Representative Rivers program is aimed at improving the research output and technology transfer, with the ultimate aim of improving river recreation management. However, as indicated earlier, we “must justify experimentation on more pessimistic grounds — not as a panacea, but rather as the only available route to cumulative progress” (Campbell and Stanley, 1963). While the present mode of management-funded, one-shot case studies may provide positive answers, even at times “guaranteed success” (Jubenville and Workman, 1980), it is not the route to cumulative progress in river recreation management.

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