[ca. 1980]

VALUES AND PROTECTION OF RIPARIAN ECOSYSTEMS[1]

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Abstract.--The riparian ecosystem is a zone of highly concentrated values associated with fish, wildlife, recreation, and water quality. Multiple use management on federal lands has often severely degraded riparian zones and associated values. This abuse must be corrected.

INTRODUCTION

Healthy riparian ecosystems have become a vanishing resource in the West particularly in arid and semiarid regions. Historically, many factors have acted to destroy or modify riparian vegetation such as roads, railroads, agriculture and logging. The most pervasive and ubiquitous negative influence however has been and continues to be grazing by domestic livestock. In recent years, an alarm has been sounded by concerned biologists and conservationists to federal agencies to institute better multiple use management of federal lands with a particular objective of restoring and protecting riparian zones and their associated fisheries, wildlife, and recreation values. One result has been several symposia on the values of riparian ecosystems and the threats to their integrity. The proceedings of these symposia edited by Cope (1979), Johnson and McCormick (1979), Johnson and Jones (1977), Menke (1979), and Graul and Bissell (1978) contain abundant data, information, and case histories of riparian significance and values, the factors causing negative impacts, and the feasibility of protection and restoration.

VALUES OF RIPARIAN ECOSYSTEMS

Some values, such as the reduction in numbers and biomass of economically important game fish and game animals attributed to loss of riparian vegetation can be quantified. Other values, associated with nongame animals, esthetics and influence on water quality are more elusive, but nonetheless real.

Winegar (1977) demonstrated the enormously greater diversity and abundance of animals in a fenced riparian zone, protected from livestock, on Camp Creek, Oregon, as compared to the grazed areas along the creek outside the protected zone. Wagner (1978) reviewed the impact of livestock on game animals but without specific reference to riparian vegetation. Destruction of riparian vegetation by livestock can essentially eliminate moose habitat from an area and severely deplete the winter food supply of elk.

At the Sparks, Nevada, livestock, fisheries and wildlife symposium, I (Behnke 1979) reviewed and summarized the evidence from four fishery studies comparing stream sections exposed to livestock with sections protected from livestock. All studies agreed that the protected sections contained three to four fold more trout biomass than the grazed sections. The common denominator in all cases was the presence of vigorous stands of riparian vegetation vs. the destruction of riparian vegetation which resulted in changes in channel morphology.

Several additional case histories of fish loss due to livestock destruction of riparian vegetation and fisheries restoration following riparian protection and restoration are found in Cope (1979). Gregg (1979) demonstrated a strong negative correlation between livestock grazing intensity and trout abundance in several western Colorado streams, and this negative relationship is expressed through impact on riparian vegetation and streambank stability. Van Velson (1979) revealed how Otter Creek, Nebraska, was converted from a silt laden stream inhabited by chubs and suckers into a premier trout stream within a few years after the riparian vegetation was restored by excluding livestock.

In relation to livestock influence on accelerated erosion and water quality, the condition of the riparian vegetation is probably the most sensitive indicator of overall watershed condition. On overgrazed and eroding rangelands, the use and impact on the riparian

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area is intensified. Thus, in general, there is a relationship between the condition of the riparian vegetation and the rate of accelerated erosion in the watershed. A BLM report on salinity problems in the upper Colorado River basin by Bentley, et al. (1978) identified livestock grazing as the greatest cause of accelerated erosion and associated salt loading of the Colorado River. The costs to downstream water users in the basin are estimated to be more than $330,000 for each additional mg/l of salt concentration. On the basis of this study, Eggleston and Bentley (1977) calculated that the elimination of livestock grazing from highly erodible public lands would have a benefit-cost ratio of 5.9:1 -- considering only the costs of increased salt concentration to downstream water users. If fisheries, wildlife and recreation losses were to be estimated from watersheds subjected to accelerated erosion, and the loss of downstream reservoir storage to sediment filling were added, the total costs to society caused by past and present grazing systems on highly erodible lands would be enormous in comparison to the benefits of meat production.

The magnitude of the problems of the impact of livestock grazing on other natural resource values can be visualized when it is realized that about 48% of the entire land mass of the 11 western states is federal land (mainly BLM and USFS) and that more than 75% of the public land area is grazed by domestic livestock. Federal lands, however, do not produce a significant amount of the total production of cattle and sheep. In the western states, according to 1972 U.S. Department of Agriculture statistics, federal lands produced 19,748,000 AUM's (animal unit months) of a total of 601,917,000 AUM's, or about 3% of the total production of sheep and cattle in the western states.

I believe that utilization of public lands by domestic livestock is a valid use of the lands under multiple use management. However, where present grazing systems continue to have a severe impact on fisheries, wildlife, and accelerated erosion, drastic changes are needed to make grazing compatible with other uses and values. I believe that the condition of riparian vegetation will prove to be the most sensitive and useful indicator of how well revised grazing systems are working to make them compatible with the objectives of multiple use management.

LIVESTOCK IMPACTS

The loss of terrestrial animals from the destruction of riparian vegetation is a straightforward situation -- the essential habitat is eliminated. Damage to aquatic ecosystems is by indrect means. Riparian vegetation provides streambank stability, shading, and cover. The loss of riparian vegetation destabilizes the banks and warms the water. Typically, grazing intensities that eliminate riparian vegetation also overgraze the watershed so that precipitation from intense rain runs overland and is not sufficiently retarded by vegetation and absorbed by the soil. This, in turn, greatly increases the the amplitude of flood peaks and sediment loads. The destabilized streambanks cannot contain the energy of high flows and, depending on the substrate, will either break down and braid out or trench down into an arroyo. Either alternative results in shallow, high velocity flows lacking adequate cover or suitable habitat for fish. The result is a crash or elimination of a trout population. The seasonal flows now change to a regime characterized by a brief period of high, silt laden flows during the wet months or after storms, followed by low or intermittant flows the rest of the year.

How many millions of pounds of trout, salmon and other game fish are lost each year on federal lands because of riparian degradation causing streams to produce below their natural carrying capacity can only be guessed. Estimates are also lacking on the numbers of game and nongame animals that could be increased if riparian ecosystems were restored to natural conditions along thousands of miles of streams.

The negative impact of livestock on riparian vegetation is not evenly distributed. In higher elevations with high levels of precipitation and good distribution of water and forage, the impact is generally light. It is the arid and semiarid regions with less than 20 in. (500 mm) annual precipitation and long grazing seasons that are particularly susceptible to destruction of riparian vegetation because livestock tend to concentrate along streams in the dry months. In the arid and semiarid foothills and plains regions the structure and diversity of riparian vegetation is of paramount importance for the abundance and diversity of terrestrial wildlife.

PROBLEMS AND SOLUTIONS

Millions of acres of arid and semiarid grasslands and riparian ecosystems in the Southwest were essentially destroyed by overgrazing during the time of the open range and converted to arroyo gutted landscapes characterized by xeric types of vegetation of little value even to livestock. In most regions of the west the arid and semiarid public lands are administered by the Bureau of Land Management. Since the days of the Grazing

Service, the predecessor organization of the BLM, this agency has been oriented to and dominated by livestock interests. Encouraging changes are taking place, but it is a long and tortuous route between high sounding policy statements on the objectives of multiple use land management emanating from Washington through various state and regional administrative layers to interpretation and implementation on the land.

If real progress is to be made, it is imperative that better communications become established between field biologists with the understanding and know-how and decision makers. For example, in a 1977 Task Force Report on forest, range, wildlife and fisheries habitat development, to the Regional and National Agricultural Research Planning and Implementation System (WRPC-RPG-2), I note statements such as: "The effects of wild and domestic grazing animals on fisheries have not been adequately measured." . . . "Although grazing by livestock and wildlife is widespread and impinges on nearly every stream and lake in the Western United States, the effects of grazing on aquatic ecosystems are virtually unknown." I would point out that sufficient knowledge and data are available on the effects of grazing on wildlife and fisheries to establish common cause and effect relationships and to implement corrective action. The papers presented in the symposia cited previously, many written by federal biologists based on studies on public lands, are abundant evidence of what is known. If the Task Force members really believe that wild grazing animals are a serious problem to aquatic ecosystems I would suggest a visit to Yellowstone or Rocky Mountain National Parks where maximum abundance of big game animals occur and to observe the condition of riparian vegetation, aquatic ecosystems and the quality of the fisheries. I would also suggest observations of the numerous long term exclosure studies on USFS and BLM lands where livestock, but not wild animals, are excluded and examine the impacts of wild animals. The danger is that there tends to be a reflex response to a crisis situation by initiating more "research," without asking the questions in need of answers. When this occurs, such research often is focused on phenomena not directly involved with cause-effect relationships and is useless for providing problem solving answers.

As a contribution toward more rapid resolution of the conflicts of livestock grazing with multiple use management on public lands, and to clarify the issues involved, I emphasize an understanding of the following points:

1. Domestic livestock can and do cause severe damage to riparian vegetation which, in turn, has a negative impact on fish, wildlife, and recreation values. There is no need to further "prove" that the negative impact occurs, but only to begin to identify the allotments where damage occurs and to quantify the extent of the damage with the subsequent loss of multiple use values.

2. Riparian vegetation can rapidly recover in from one to five years after it is protected from livestock grazing.

3. No amount of research on terrestrial or aquatic biology can provide solutions to the grazing problem. The problem, where it exists, can only be solved by preventing livestock from congregating along streams. Presently no grazing system has been demonstrated effective in protecting riparian vegetation. This is a range management problem and the highest priority must be given to the development of new grazing management systems where the present system results in riparian degradation.

4. There has not been significant improvement of the arid and semiarid western range on BLM lands since the Taylor Grazing Act of 1934. A 1975 BLM range condition report prepared for the U.S. Senate Committee on Appropriations, states that 83% of the range is in less than satisfactory (less than excellent or good) condition. In the long term, it is the livestock operators who have the most to gain from better management of the western range designed to stabilize the land and increase forage production.

It is obvious that because watersheds occur on both public and private lands, comprehensive rehabilitation projects must involve cooperative efforts of the USFS and the BLM on public lands and the Soil Conservation Service on private lands. Innovative grazing systems must be tried to find those best suited to avoid destruction of riparian vegetation. In many situations fencing will be required. In severely degraded areas, the cessation of livestock grazing for five years or more or perhaps permanently will be necessary to restore the natural vegetation and restore other natural resource values. In such cases, the financial burden imposed on livestock operators could be mitigated by a subsidy for the loss of AUM's from federal lands being rehabilitated.

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PURITY EVALUATION OF BEAR RIVER CUTTHROAT TROUT FROM MILL AND CARTER CREEKS, WASATCH NATIONAL FOREST, SUMMIT COUNTY, UTAH

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ABSTRACT

Samples of 32 specimens from Mill Creek and 16 specimens from Carter Creek of the Bear River drainage, Utah, were evaluated for purity as the original native trout. The samples are highly similar but were not drawn from a single, freely interbreeding population. The Carter Creek sample is judged to be about 95% pure and the Mill Creek sample about 90% pure. The trout of Carter and Mill Creeks represent the only known virtually pure Bear River drainage cutthroat trout in Utah. Efforts should be made to preserve and enhance the status of this rare native trout.

INTRODUCTION

I have previously written several reports on the native cutthroat trout of the Bonneville basin, *Salmo clarki utah*, for federal and state agencies. Hickman (1978) and May et al. (1978) have also summarized the taxonomic and biological information known about this subspecies. Hickman and Duff (1978) discussed the status of *S. c. utah* of the western Bonneville basin.

There is no doubt that *S. c. utah* is extremely rare. May et al. (1978) list only three known pure, native populations in the Bonneville basin of Utah (Birch Creek, near Beaver; Little Willow Creek, south of Salt Lake City; and Trout Creek, in the Deep Creek Mountains). The American

Fisheries Society recognizes S. c. utah as threatened. Stimulated by a petition from the Desert Fishes Council, the U.S. Fish and Wildlife Service's Endangered Species Office proposed an emergency listing of S. c. utah as threatened under the federal Endangered Species Act (April 1980).

In a chapter on Great Basin trouts written for a book to be published on desert fishes, I discuss the evolutionary history of the Bonneville cutthroat trout (Behnke 1981). The Bonneville basin trout was derived from the "Yellowstone" subspecies of cutthroat trout in relatively recent geologic times, about 30,000 years ago when the Bear River lost its connection to the upper Snake River and became part of the Bonneville basin. Although the Bonneville cutthroat trout is closely related to the "Yellowstone" cutthroat and bears a strong resemblance to its parental form, three relatively distinct groups of Bonneville trout can be detected from taxonomic analysis: a group associated directly with ancient Lake Bonneville, a group isolated in Snake Valley (Deep Creek Mountains), and a group native to the Bear River drainage. The "Bear River" native cutthroat trout differs from the other Bonneville cutthroat trout by having more numerous pyloric caeca and scales. Evidently the Bear River native trout, except for the population native to Bear Lake, is a fluvial specialized form. In the Thomas Fork and Smith Fork drainages of Wyoming, the native cutthroat trout is dominant over non-native trouts (Behnke 1976). Except for the Bear River drainage, S. c. utah occurs only in remote, isolated headwater streams where they are completely protected from contact with non-native trouts. In all parts of the Bonneville basin except for the Bear River drainage, the introduction of non-native trouts has invariably caused the demise of the native trout. This vulnerability to extinction of _S_. _c_. _utah_ in the rest of the Bonneville basin and the persistence of the Bear River form of _S_. _c_. _utah_ in Wyoming illustrates some real genetic based ecological divergence in the Bear River cutthroat trout that is not apparent from standard taxonomic analysis. Ecological divergence is also manifested in the success of the native trout propagation program in Bear Lake (Nielson and Archer 1977, 1978). This type of evolutionary divergence in trouts whereby a small genetic divergence results in major changes in life history and ecology, although not recognized with formal taxonomic nomenclature, is of great potential value for fisheries management programs and underlines the need to maintain all the remnants of genetic diversity in any endangered and threatened trout programs.

Previously, no pure or virtually pure populations of _S_. _c_. _utah_ were known to persist in the Bear River draingae of Utah. Thus, the present samples represent a highly significant collection.

EVALUATION OF PURITY

A letter from Mr. Mark Shaw (USFS) stated that Carter Creek has no stocking records. Mill Creek has been stocked annually with catchable rainbow trout and has been stocked in the past with Yellowstone Lake cutthroat trout (any cutthroat stocking during the past 25-30 years may have been with Strawberry Reservoir cutthroat trout. The Strawberry Reservoir cutthroat is a Yellowstone cutthroat slightly hybridized with rainbow trout). Mr. Shaw pointed out that Carter Creek is not isolated from Mill Creek. It is possible for non-native trout and hybrids to move up Carter Creek from Mill Creek.

The data from specimen examination were compared with data for diagnostic characters obtained from Bear River drainage cutthroat trout from the Thomas Fork and Smith Fork drainage, Wyoming. Any discrepancies were assessed in light of a possible hybrid influence from rainbow trout and/or Yellowstone cuthhroat trout. The spotting pattern of all specimens was evaluated to detect a hybrid influence.

Table 1. Selected taxonomic characters.

Mill Creek N=32	Gillrakers 16-21(19.1)	Pyloric caeca 34-53(41.8)	Scales, above l.l. and lat. series 37-45((41.0) 157-190(179.8)	Basibranchial teeth 2: no teeth 30:1-7(3.3)
Carter Creek N=16	18-21(19.3)	39-55(46.4)	35-45(40.7) 162-188(177.5)	2-13(7.1)
Typical modal - mean values from Bear River drainage *S. c. utah* from Wyoming				
	18-19	40-50	35-43 160-175	5-10

Both the Mill Creek and Carter Creek samples are overwhelmingly native Bear River *S. c. utah*. I estimate that the Mill Creek sample represents approximately 90% purity and the Carter Creek sample above 95%. This conclusion is surprising considering the long history of non-native trout introductions. As with the Thomas Fork and Smith Fork drainges in Wyoming, the environmental conditions in Carter and Mill Creeks must strongly favor the native genotype -- which suggests that if stocking ceases, natural selection might essentially eliminate non-native genes from these populations.

A hybrid influence from rainbow trout is typically detected first in the basibranchial teeth and spotting pattern. Two of 32 specimens from Mill Creek lack basibranchial teeth, but this is comparable to the best of my Wyoming *S. c. utah* samples. However, the average number of teeth is depressed in the Mill Creek sample (3.3) in comparison to the Carter Creek sample (7.1). There are no real differences in other characters. All specimens have 9 (a few 8) pelvic fin rays. Rainbow trout have 10 pelvic rays. The spotting pattern is variable, but this is a rather general phenomenon in *S. c. utah*. Typically, Bear River drainage *S. c. utah* has large, roundish spots relatively sparsely distributed over the sides of the body -- not greatly concentrated on the caudal peduncle area as is the case with most other subspecies of cutthroat trout. Some specimens from both samples approximate the "idealized" native spotting pattern and some have smaller, more profuse spotting. One specimen in each sample has a few spots on top of the head, which may be indicative of a rainbow trout hybrid influence. There is, however, no obvious manifestation of a hybrid influence in the spotting pattern. The variability could be a completely natural range of expression of the native genotype.

In comparison with Wyoming samples, the number of gillrakers and scales are slightly higher in the samples from Mill Creek and Carter Creek, but this is to be expected in specimens living at high elevations in the very headwater of the drainage (the Wyoming collections were made at elevations about 3000 feet lower).

In reference to the rating system of Binns (1977) used to grade relative purity of *S. c. pleuriticos* samples that I had examined for the Wyoming Game and Fish Department, I would give the Carter Creek cutthroat trout a grade of A- (virtually pure) and the Mill Creek cutthroat a grade of B+ (very good). These ratings assume added significance in light of the extreme rareness of *S. c. utah* in general and by the fact that no other population of pure or near-pure *S. c. utah* is known from the Bear River drainage of Utah.

I would point out that there is no way a population of *S. c. utah* can be "known" to be absolutely pure. The genetic relatedness to Yellowstone cutthroat trout is of such closeness that no real or consistent differences have been found in protein comparisons (electrophoresis), except in the Snake Valley form of *S. c. utah*. I discussed some of the problems inherent with various techniques to correctly assess relationships (and hybrid influences) between closely related forms of trout in my monograph on western trouts (Behnke 1980a).

RECOMMENDATIONS

Management decisions and special regulations designed to preserve and enhance the native trout populations in Mill and Carter creeks are the province of the Utah Division of Wildlife Resources. I urge that serious consideration be given to develop a fisheries management program specifically for the native trout.

Ideally, all stocking of non-native trout should cease. The fishery would then be dependent entirely on the wild, native trout and this would probably require special regulations to avoid overexploitation. If there is a large clientele attracted by the stocking of catchable rainbow trout, it may not be politically practical to cease all stocking immediately. Some information and education work will be needed to make the significance of the Carter and Mill creek trout more widely understood and appreciated. Also it might be explained that catchable trout diverted from Mill Creek would be stocked in other near-by waters so the area will not suffer a net loss of hatchery trout. The potential for creating a high quality wild trout fishery for the virtually extinct native trout should be publicized.

I have discussed the use of special regulations in previous publications (Behnke 1978, 1980b) pointing out the excellent results obtained with cutthroat trout in fisheries where most of the catch is released and recycled in the fishery. This is due to the extreme vulnerability of cutthroat trout to angling. Less than 20 hours per acre per year of angling pressure can harvest 50% or more of all catchable-size cutthroat trout in a fishery. It may take 500 to 1000 hrs/acre/yr. to harvest 50% of the catchable-size brown trout. This vulnerability to angling, however, is the reason why the most successful special regulation fisheries are based on populations of cutthroat trout -- they can be cuaght-and-released many times to maintain a high catch-rate on a limited stock of fish.

The "best" type of regulations regarding size and bag limits would depend on the population dynamics of the trout and angling pressure that would be exerted.

In my paper on the Thomas Fork drainage trout (Behnke 1976), I noted that in electrofishing samples along roadside areas, very few catchable-size cutthroat trout were taken whereas in sites further removed from easy access, the proportion of trout 150 mm or more in size dramatically increased. In the sample of 32 specimens from Mill Creek, 9 specimens are larger than 150 mm and 4 of these are more than 200 mm. This would indicate that this site is exposed to rather light angling pressure. In the Carter Creek sample, 3 of 16 specimens exceed 150 mm and 1 of these is more than 200 mm. It would appear that this site on Carter Creek has somewhat higher angling pressure.

Opportunities should be looked for to increase the distribution and abundance of the native Bear River cutthroat trout in Utah by transplants into new waters. Small headwater streams above barrier falls which now contain stunted populations of brook trout of little value could serve as transplant sites after all non-native trout are eradicated. Fish from Carter Creek, selecting for "idealized" spotting pattern could be used to establish new populations. Figure 1 is a sketch of a specimen from Mill Creek exhibiting the typical or idealized spotting pattern of *S. c. utah* native to the Bear River drainage.

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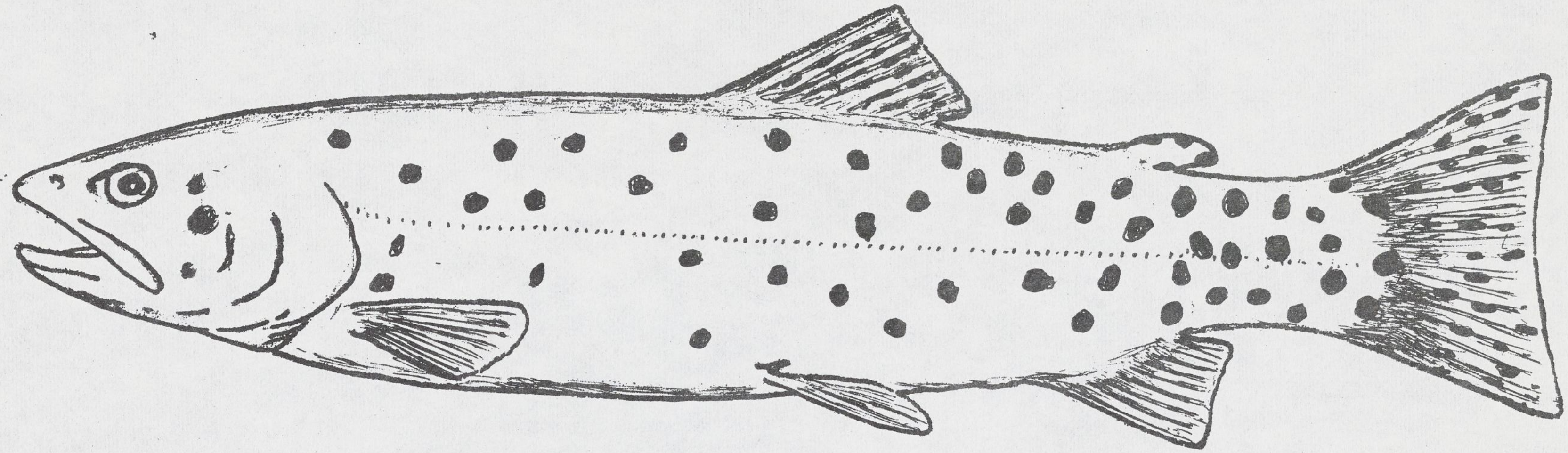

Figure 1. Typical spotting pattern of S. c. utah native to Bear River drainage. Spotting pattern is highly variable in this subspecies but is mainly characterized by large, roundish spots rather sparsely distributed over sides of body.

* nutrient recycling!

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80% imbeddedness invert.↓ growth↓

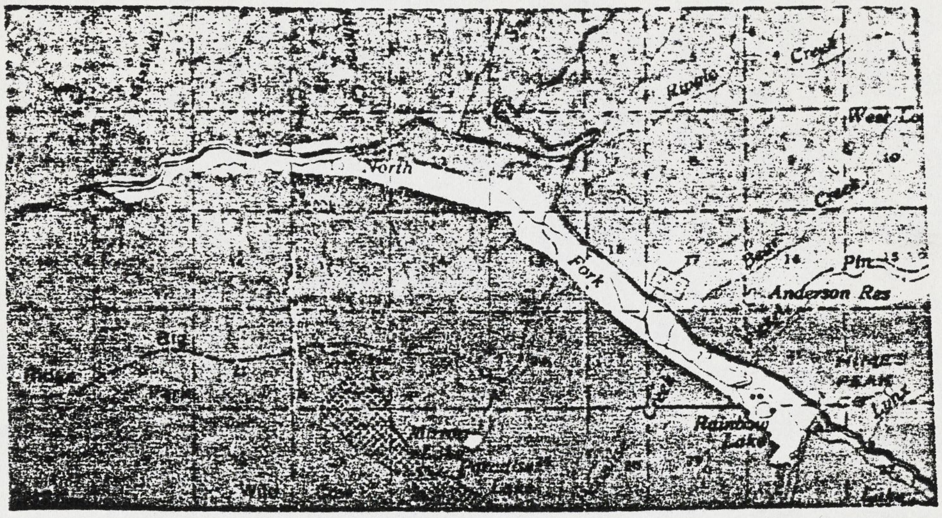
Rio Blanco Rept.

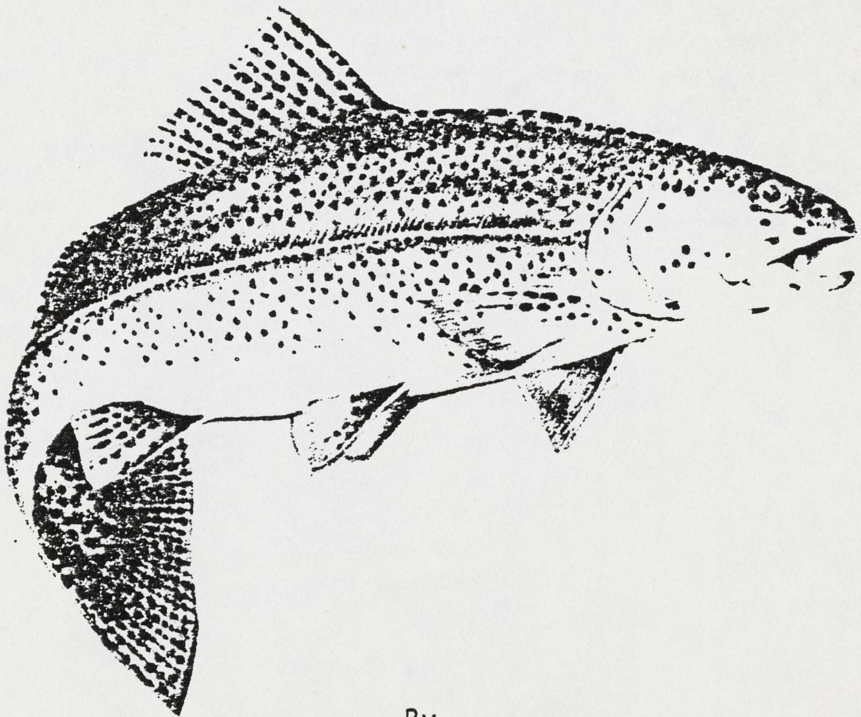
Behenke & Raleigh

FIRST CLASS

[ca. 1980]

Preliminary Analysis of the Rio Blanco Ranch Trout Habitat and Fishery





By
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and
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PRELIMINARY ANALYSIS OF THE RIO BLANCO
RANCH TROUT HABITAT AND FISHERY

EXECUTIVE SUMMARY

Selected sections of the White River, a small lake, and their trout populations, located on the Rio Blanco Ranch property, were examined and sampled on September 27 and 28, 1980. An analysis of our observations, data collected and agency records available on this section of the White River and the lake indicates that:

1) The annual flow regime of the river and water quality records are indicative of a potentially excellent trout habitat.
2) Trout food supply (invertebrate production) appears good to excellent as evidenced by:
   a) An apparently good growth rate for trout.
   b) The healthy, robust condition of the trout.
   c) An abundance of excellent invertebrate habitat.
3) Trout production in this section of the White River is below maximum and appears to be limited primarily due to:
   a) Insufficient adult cover and holding habitat.
   b) A possible lack of sufficient spawning habitat in both the river and the lake.
   c) A possible lack of suitable juvenile trout rearing area in the river.

Recommendations

Optimal trout stream habitat is characterized by clear cold water, a relatively silt-free rocky substrate, an approximate 1:1 pool-riffle ratio with areas of slow deep water, a relatively stable temperature and water flow regime, well vegetated stream banks, and abundant instream cover. Stream improvement projects center around creating, restoring, and maintaining these essential trout stream features.

The River Habitat

1. Adult holding areas. The gradient of this section of the river is such that deep pools, preferred habitat for adult trout, are inadequate in quantity and quality. The current effort to provide quality pools by log, rock and gabion structures should be continued. We would advise the following:

    a) Eliminate the apron in front of the top "weir" gabion so that the structure can develop and maintain a deep plunge pool below the structure (see figure 3). The upstream pools will eventually silt in and become progressively less useful. The apron effect can be eliminated either by removal, or by moving the top wier gabion downstream to the front edge as in figure 3.

    b) Leave some riffle areas between each structural pool area as food producing and potential spawning habitat. As mentioned above the upstream pools will eventually become largely filled in.

    c) Gabion structures provide a relatively permanent, maintenance-free structure, but do detract from the aesthetic quality of the area. A more pleasing and nearly as permanent a structure can be achieved by use of well anchored natural materials such as logs and large rocks.

    d) Many of the present man-made structures appear to be significant barriers to the upstream movements of juvenile and adult trout.

Trout presently displaced downstream may be permanently lost to upstream areas. Some seasonal movements may also be necessary for access to suitable spawning and overwintering areas, and for recruitment of young to upstream areas. The development of plunge pools below each structure will help provide access to upstream areas.

2. Juvenile rearing habitat. We were unable to locate as many juvenile trout as we expected to find on the property. High quality juvenile rearing areas appear to be in short supply. We would recommend that this aspect be examined again in late July or early August next year. The "S" shaped meander bend area with some alteration, small side channels and old beaver ponds are potentially good juvenile trout rearing areas.

3. Spawning habitat. Time did not permit us to make a thorough evaluation of the present spawning habitat, but it also appears to be in short supply. This aspect should also be investigated next summer. If instream movement of trout is restricted by the manmade structures some spawning area should be made available between each structure. If instream movement is not a problem, the meander bend area again has the potential to provide any additional spawning area deemed necessary by the study.

The Lake Habitat

1. Trout food supply. The growth and condition of trout in the lake is very good. You might consider the introduction of crawfish into the lake. They will provide some weed control and are an excellent food source for large trout. With the introduction of these large highly

nutritious food items the lake should produce some three to five pound trophy trout. We do not recommend the introduction of forage fishes into the lake.

2. Recruitment to the lake fishery. There is presently no suitable spawning area available to lake fishes. Those trout that leave the lake to spawn in the river may or may not return to the lake. Also, sufficient recruitment of juvenile trout produced in the river to the lake fishery is very doubtful. We, therefore, recommend that the lake be stocked every other year with 3000 to 5000 fingerling Snake River cutthroat trout, or that the inlet canal be developed as a limited spawning area for lake fishes. Care must be exercised to see that over-production of young fish in the lake does not occur.

Fishery Management

Specific fishery management goals should be set for the Rio Blanco Ranch trout fishery. Questions such as the following must be answered:

1. What type of fishery is desired--a wild trout, hatchery trout or combination fishery?
2. What is an acceptable catch rate?
3. Should a catch-and-release only fishery be instituted to achieve a high catch rate, or is a limited catch fishery with a slightly lower catch rate favored?
4. With a limited catch fishery, what type of regulations would produce the most satisfactory results?

We realize that this report will not satisfactorily answer all of the questions that may arise regarding fisheries management problems and options and we recommend that a personal meeting and discussion session nutritious food items the lake should produce some three to five pound trophy trout. We do not recommend the introduction of forage fishes into the lake.

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INTRODUCTION

A cursory sampling and survey program was conducted on the North Fork of the White River on the property of the Rio Blanco Ranch September 27-28, 1980. The objective of our analysis of information and observations is to diagnose the factors limiting trout production and propose options designed to maximize the quality of the trout fishery. The term "quality" can be elusive and perhaps it should best be defined by the user group -- the club members. In general, "improving fishing quality" means to increase fish abundance, which can be quantified as catch-per-man-hour, and/or an increase in the average size of the fish and proportion of fish in the catch that exceed a certain length; for example, 12 or 14 inches.

This goal could be quickly achieved by stocking large numbers of large-size hatchery trout, but to most serious anglers, the word "quality fishing," is synonymous with wild trout. Thus, our emphasis is placed on improving conditions for wild trout. The first concern to be addressed is to determine if the trout abundance in the North Fork of the White River is food limited or habitat limited. That is, would the trout population increase if food production was increased, or, is there already a surplus of food that is not utilized because of a lack of suitable trout habitat? There is no doubt in our minds that the trout population is primarily limited by habitat and not food. This is obvious from the high trout density found in the pools created by gabion dams. These pools have almost certainly decreased food production in comparison to the fast water riffle area they replaced, but they provide suitable habitat that was not there before so trout can utilize the invertebrates mainly produced in the fast riffles above the pools.

The steep gradient of the river results in a natural river channel consisting almost exclusively of shallow, high velocity water with a rubble and boulder substrate (rock of about 4 to 18 inches in diameter). Such an environment is good for invertebrate production, but is lacking in sites of slow, deep water with associated protective cover that are preferred by trout. The steep gradient and high velocity also causes a scarcity of suitable spawning gravel (1/4 to 2 inch size gravel) and calm, protected areas favorable for survival of fish in their first year of life. The annual flow regime and water quality of the river are excellent for trout, which indicate some options to increase the abundance of wild trout.

THE RIVER ENVIRONMENT

During our brief visit we did not observe all of the river on the Ranch property, but we believe we saw most of it. Because of the relative consistently steep gradient, a reach of about 1/4 to 1/2 mile of river appears to be representative of the entire river through the Ranch. That is, the characteristics of flow velocity, depth, and substrate at any reach is repeated with little significant variation throughout the Ranch (except for man-made modifications).

The U.S. Geological Survey maintains a guaging and water quality monitoring station on the North Fork at Buford. The data collected at Buford is generally applicable to the North Fork through the Ranch except that flow volume of the North Fork, is about twice as great at Buford as it is through the Ranch (Lost Creek and Marvine Creek, tributaries below the Ranch contribute about 40% of the annual flow volume at the guage).

Figure 1 illustrates the annual flow regimes for 1977 (a dry year, only 50% of normal), 1978 and 1979 (wet years with flows 10% to 15% above the long term average). In relation to flows favorable to trout, the striking feature of the North Fork hydrograph is that even in the lowest flow period of the lowest flow year, the flow is still 37% of the long term average daily flow. The long term average daily flow is 308 cubic feet per second (cfs). The average daily flow is the total annual flow volume passing the guaging station divided by 365. The mean daily flow during September, 1977, was 114 cfs (37% of 308 cfs).

Based on numerous studies, there is a definite relationship between the annual flow regime and the quality of a trout fishery. The most critical period is typically the base flow (lowest flows of late summer, fall, and winter). A base flow of 50% to 55% of the average daily flow is considered excellent for maintaining the quality of trout habitat. A base flow of about 25% to 30% is considered fair. The lowest base flows in the fall of 1978 and 1979 equalled about 55% of the average daily flow.

The water quality parameters of the North Fork -- temperature, oxygen, pH, nutrient levels, sediment load, etc. -- indicate an excellent trout environment. If such a flow regime with such excellent water quality flowed as a low gradient, meadow type of stream, a biomass of wild trout of 300 to 400 pounds per acre would be expected. Because of the steep gradient, the North Fork can produce and maintain a trout population at only a fraction of its biological potential. Between Trappers Lake and Buford the North Fork drops from 9600 feet to 7100 feet for an average gradient of about 1.5%. In comparison, artificial spawning channels, designed to maintain optimum flow velocities for spawning and egg incubation have gradients of .25% or less.

STREAM MODIFICATIONS

The action taken of construction of log dams in earlier years and gabion dams in recent years is an obvious response to the lack of suitable trout habitat in the natural stream channel. This "stair-stepping" effect creates deep, low velocity water and has been successful in achieving the desired results -- trout are concentrated in the artificial pools. A future concern is that, eventually, much of the pool areas will be lost from the natural action of a river depositing its bed load during high flows in areas of low velocity. This is evident from examination of the older log structures -- the river has essentially filled in the old pools. However, these log dams have created excellent trout habitat immediately below the dams where the turbulent overflow creates large plunge pools (fig. 2,3). The gabion dams are constructed with an "apron" below the dam to dissipate the energy of the overflow (fig. 4). This form of construction probably makes for a more stable dam but it also greatly lessens the effectiveness of the overflow for creating plunge pools (compare the characteristics of the plunge pools and the quality of trout habitat below the old log dams with the areas below the new gabion dams).

In our consideration of trout habitat in relation to possible ways to further increase wild trout abundance, we have divided "habitat" into four categories: 1. spawning and incubation, 2. nursery or rearing, 3. adult, and 4. overwintering.

<u>Spawning and Incubation Habitat</u>. Due to the high velocity flow which governs the composition of the substrate, there are very few good spawning sites where deposits of 1/4 to 2 inch size gravel occurs. The gabion dams probably are an effective block to upstream migration. Trout utilize

plunge pools and the crest of the plunge pool wave (fig. 3) to jump over stream barriers. The construction of gabions with an apron below the barrier will virtually eliminate the upstream movement of juvenile trout and make upstream movements of adult trout difficult to impossible. Thus, at least one adequate spawning area should be available between each pair of dams. The few sites where gravel does occur are found where the velocity is disrupted and diminished (allowing the deposition and maintenance of smaller diameter substrate). Such sites are typically found next to the downstream end of an island. Such sites can be observed and ways considered to duplicate these conditions in an attempt to create spawning areas. Areas near the head of gabion pools (tail of riffle coming into pool) or near a gabion dam at the downstream end of pools appear to maintain the proper current velocities that would permit the establishment of spawning gravel. A gravel bed of two to three square yards should provide space for several redds. The artificial S-shaped channel offers areas where spawning gravel could be established, perhaps with the assistance of in-stream structures designed to maintain optimum velocities (1-3 feet per second) (fig. 4). The problem associated with the S-shaped channel is that the banks are not vegetated and the channel morphology has not yet stabilized. This results in high sediment loads.

We found several young-of-the-year brook trout (born in 1980), averaging about 3 inches. Only two young-of-the-year rainbow trout (about 1 1/2 inches) ~~was~~ were observed in a small, shallow side channel. Our cursory observations indicate a severe shortage of suitable spawning sites but we would point out that with trout reproduction there can be "too much of a good thing." Streams where trout have excellent reproductive success and relatively high survival of young are characterized by dense populations of small, slow-growing trout. Improvement of spawning and rearing areas should be approached cautiously.

Rearing or nursery habitat. During the first year of life, small trout (1 to 3 or 4 inches) seek protected areas of low velocity where they can find food and avoid predation. Pockets of slow water with vegetation, side channels and small tributaries can provide good nursery habitat. Some of the gabion pools have created some areas of good nursery habitat but they also contain dense populations of large trout that are potential predators. The potential for improvement of side channels and small channels with seeps from beaver ponds might be examined. The objective would be to create areas of low velocity more than six inches deep with protective in-stream and/or overhead cover.

In our electrofishing and angling survey we sampled about 100 rainbow trout in the North Fork. Only two of these fish were juvenile rainbow trout one year of age (completing their second season of growth). Our survey was much too brief to make firm statements on the limitations of nursery and rearing habitats, but our observations lead us to believe that there is a scarcity of adequate habitat for young trout.

Observations should be made next year on the use of the artificially created S-shaped channel by young fish. This channel, perhaps with some modifications, might become an important rearing area.

Adult habitat. When trout typically attain a size of about 6 inches or more in their second or third year of life, they will generally seek a territory where they spend the rest of their life. The "microhabiat" site where an adult trout spends most of its time in a stream is characterized by an area of low velocity (so energy is not needlessly expended fighting the current) adjacent to or underlying the mainstream of the current so that minimal movement is needed to obtain food carried by the current. Deep areas of undercut banks are ideal adult habitat. The gabion pools are excellent adult habitat and some could be further improved if the structural diversity on the bottom could be increased. The plunge pools below old logs are excellent adult habitat. In long sections of shallow, high velocity flow there is little suitable habitat for trout. Such areas provide good habitat for insect production, but most of this food production is not utilized by trout because of the lack of sites where trout can live.

In shallow riffle sections trout were found behind every large boulder or log where the current flow was disrupted and a deeper area (ca. one foot or more) created with a pocket of low velocity flow. Critical observations of the characteristics of these riffle microhabitats--their position in the stream, how they were formed, their dimensions, etc.--would suggest some possible ways new microhabitats might be created with boulders, logs, or instream structures. If successful, the creation of new microhabitats in long reaches of the river, such as the canyon areas below saw mill pool, now with low densities of trout, would increase the overall trout abundance and habitat diversity in the North Fork of the White River.

<u>Overwinter Habitat</u>. In headwater trout streams a major factor limiting abundance can be the amounts of adequate overwintering habitat. As water temperatures approach 40°F feeding is reduced and trout seek winter cover. Juvenile trout spent the winter primarily within rocky,

silt-free substrate areas. Adult trout tend to overwinter in deep quiet pools. Without adequate overwinter habitat, mortality is high and a population is characterized by a preponderance of young, small fish. The size-age structure of the rainbow trout observed on September 27, 28, indicates that the gabion pools provide good overwinter survival habitat.

THE FISHERY

We found the angling quality to be excellent. During two hours of angling, Behnke caught 34 trout (22 rainbow, 11 brook, 1 cutthroat) and Mike Owen caught 46 trout in slightly more than two hours of angling. The majority of the rainbow trout caught were between 11 and 14 inches. On public waters, angling quality is typically quantified for comparative purposes as catch-per-man-hour (CPMH). A CPMH of one to two trout 10 inches or longer would be considered as excellent quality on public trout streams. A question basic to future management of the Rio Blanca Ranch fishery is: how dependent is the CPMH on the stocking of hatchery trout? Would the members favor a fishery based entirely on wild trout if it meant a reduction in CPMH?

We originally believed that we could accurately separate wild from hatchery trout by general appearance (short, blunt heads; frayed, deformed fins characterizing hatchery trout). We could not do this with much confidence. Evidently, the hatchery fish stocked were of good quality and were in the river long enough to assume a "wild" trout appearance. The scales from nine rainbow trout (six from the "laundry" pool and three from the "lower pigpen" pool) were examined to discriminate hatchery from wild trout. The scales of trout raised in a hatchery typically are characterized by a zone of regeneration and widely and evenly spaced circuli (due to rapid and uniform growth). Three, possibly four of the nine rainbow trout are judged to be hatchery trout and five, possibly six are wild trout (one specimen had both regenerated scales but with "wild" type circuli--possibly a hatchery trout surviving from the 1979 stocking). Interpretation of age from the "wild" scales indicates that the smallest trout of 10 1/4 inches is age 3 (fourth year of growth), three specimens of 13, 13 1/2, and 14 1/2 inches are age 4, and a specimen of 15 1/2 inches is age 5. This is good growth for rainbow trout in a cold, high elevation stream (fig. 6).

This very limited amount of data would suggest that perhaps 30% to 40% of the late season catch of rainbow trout might consist of hatchery fish, at least in the "laundry" pool and the "lower pig pen" pool.

If the majority of the members are in favor of a fishery based entirely on wild trout, we recommend that hatchery trout not be stocked in the future. The common arguments against the stocking of hatchery fish can be summarized as follows:

1. Hatchery trout are of inferior quality in comparison to wild fish; the artificiality of "factory"-made fish is not compatible with a quality angling experience in natural surroundings.
2. Stocking of hatchery trout depresses the population of wild trout. This was found to be the case in the Madison River, Montana. The factual content of this statement depends on the density of stocking and the rate of catch. If stocking density is high (about 50 to 100 pounds per acre in stocked sections) and removal by anglers low (10% to 15%), then the sudden creation of abnormally high densities would

likely result in a stressful situation on wild fish causing them to abandon their territories and increase natural mortality.

3. Hatchery trout breed with wild trout leading to a "weakening" or "dilution" of the wild population by making them less fit to cope with the harsh environmental conditions. Theoretically this may be a problem but under natural selection, very few hatchery fish will survive to reproduce. In each generation the environment acts as an effective sorting device, eliminating less fit genetic combinations.

We recommend that the quality of the fishery be monitored in 1981 during June, July, and August to document catch-per-man-hour and size of the rainbow trout caught. If some members believe that stocking is necessary to improve the catch-rate, future stocking should be limited to only a few of the most accessible pools. This would allow for both wild trout and hatchery trout fisheries.

THE LAKE FISHERY

We fished in the lake briefly and caught several brook trout averaging 14 to 15 inches and several cutthroat trout of 15 to 17 inches (and one rainbow trout of 14 inches). The condition of the trout in the lake is excellent, denoting an abundance of readily available food (probably consisting mainly of the amphipod Gammarus commonly called freshwater shrimp or scud).

The cutthroat trout found in the lake is the fine-spotted Snake River (Wyoming) cutthroat trout. This particular cutthroat trout can give excellent results when stocked into lakes because of its wide range of feeding. The combination of Snake River cutthroat trout and brook trout will increase the total trout production beyond that possible with either species alone. This is due to the phenomenon of ecological or interactive segregation whereby each species becomes more specialized in its exploitation of the resources when occurring in the presence of other species with somewhat similar niches. This, in turn, results in more efficient utilization of all of the resources.

We recommend that stocking of young Snake River cutthroat trout be made every other year. A stocking density of 3,000 to 5,000 two-three inch trout should be sufficient. An attempt might be made to create spawning sites in the inlet channel to the lake by structures designed to modify flow velocity so that clean gravel beds would be maintained. A danger here might be that improved spawning conditions could trigger overpopulation and stunting of brook trout.

The introduction of crawfish into the lake might be considered. The crawfish, if it could become established, would provide a large food item and would promote rapid growth of large trout. If crawfish became abundant, four and five pound trout should become more common. Crawfish can also exert effective control of rooted vegetation. Biological control of vegetation would be preferable to chemical control.

A small, red-sided fish is reported to occur in the lake. We did not see this fish but we would like to know what it is. If specimens could be obtained and preserved or frozen, we could identify the species. In general, introductions of "forage" fish into a trout lake is an unwise management practice. Minnows eat the same invertebrates that trout feed on and, when abundant, these "forage" fish can greatly decrease trout production.

The mottled sculpin, *Cottus bairdi*, was identified from the river. Although the sculpin's diet is similar to that of the trout and sculpins prey to some extent on small trout, they are a preferred food for larger trout.

REGULATIONS

We do not have sufficient information on which to base recommendations for the type of regulations needed to maximize angling quality. Regulations based on scientifically sound data should be a priority for a future fisheries management policy. We assume that most members indulge mainly in a non-consumptive fishery (releasing all or most of the catch) but some of the members all of the time and all of the members some of the time want to keep some fish to eat.

A self-sustaining trout population in a good environment can sustain a considerable harvest by angling without significant depletion of the population. This is due to the fact that angling mortality and natural mortality are largely compensatory. That is, the more fish killed by fishermen, the fewer that die from natural causes. A population with good recruitment of young fish and high production (for example, where the biomass replaces itself annually, the production/biomass ratio is 1.0) can sustain a relatively high yield to the creel with only a short-term depletion of numbers. For example, a fishery that averages a biomass of 100 pounds of trout per acre might yield a harvest of 25 to 50 pounds per year and in the following year the biomass and size-age structure might remain unchanged because fishing mortality has replaced natural mortality as the main source of total mortality (fig. 7).

Information would be needed on recruitment, production, size-age structure, mortality rates, angling pressure, and angler preferences, before the "best" type of regulations could be proposed to maximize angling quality. Types of regulations that can be considered include a minimum size limit (all fish below a certain size be released), a maximum size limit (all fish over a certain size released), and a "slot" limit (all fish between certain sizes be released--for example, release all fish between 10 and 14 inches). Each type of regulation is designed to work best for certain combinations of the interaction between fishing pressure and population dynamics.

A good rule-of-thumb is to watch the CPMH and the average size of fish in the catch. Changes in abundance of the population and changes in growth rate and relative year-class strength can be interpreted from catch statistics and remedial action can be taken by modification of the regulations.

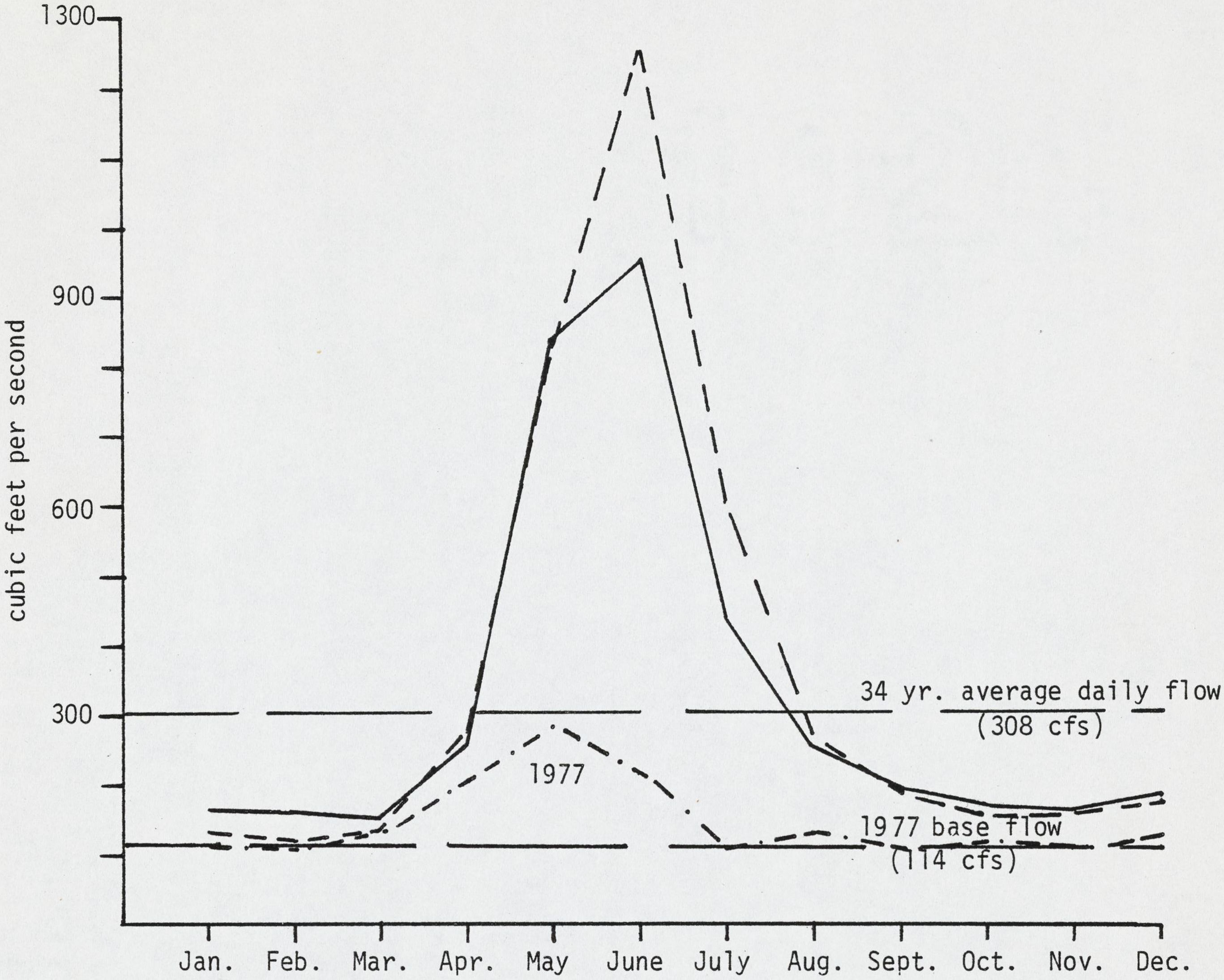

Figure 1. Annual flow regimes of White River near Buford. 1977 (a dry year), 1978, and 1979 (wet years) are illustrated. Note that lowest base flow of lowest flow year was 37% of the long term average daily flow and the base flows in 1978-79 were 55% of the long term average.

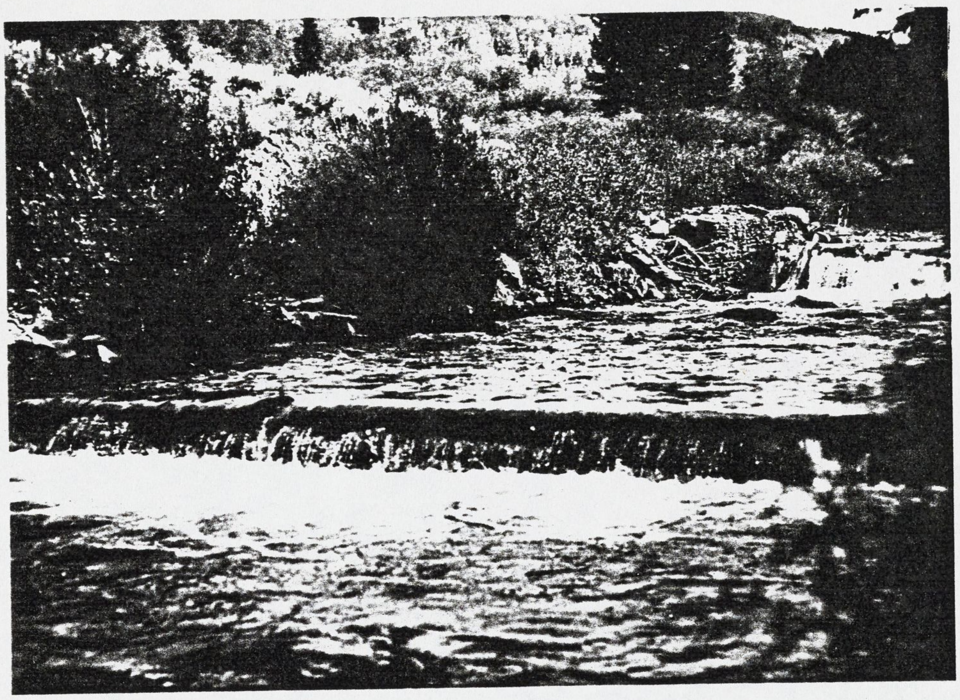

Figure 2. Log dam with good plunge pool. Although the pool above the dam is partially filled in, good trout habitat has been created and maintained below the structure.

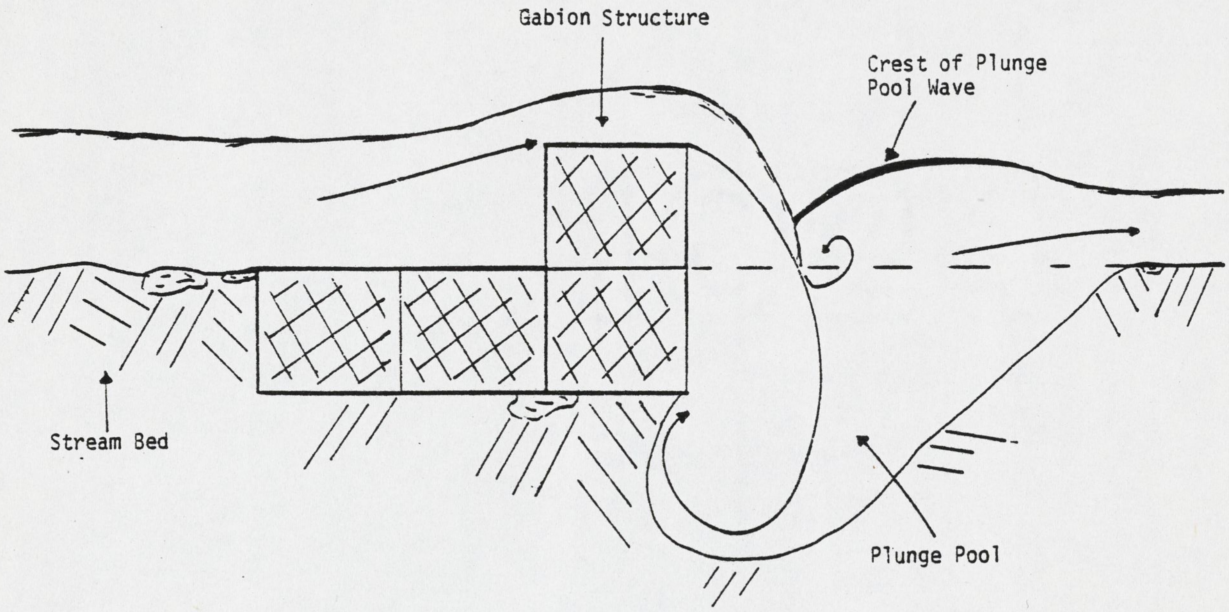

Figure 3. Plunge pools created and maintained below stream structure.

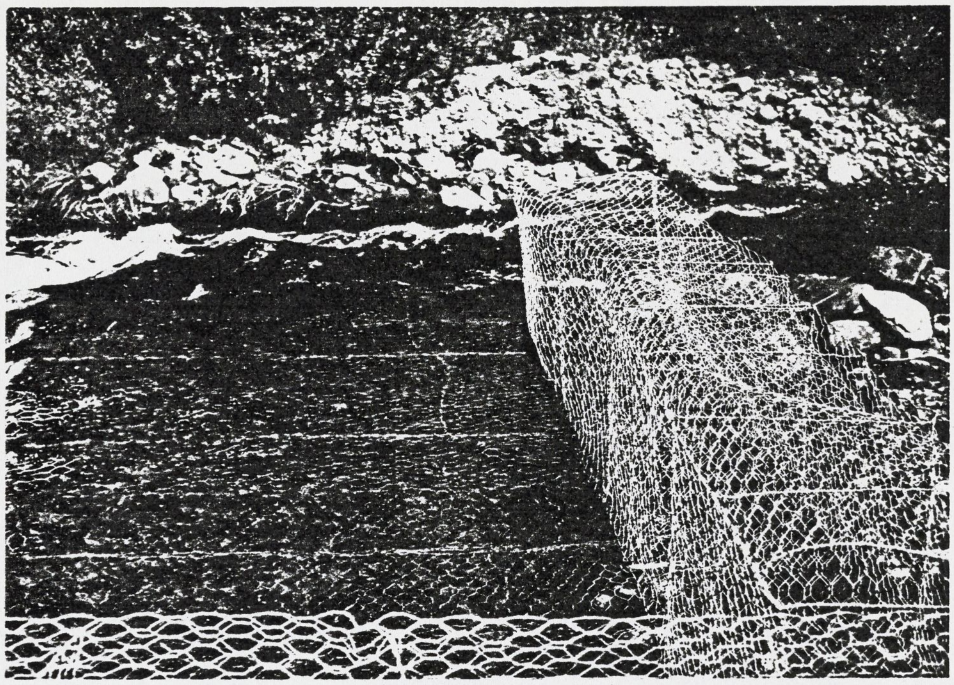

Figure 4. A. and B. Gabion dams constructed with aprons to dissipate energy of overflow. Upstream movement of fish is made difficult to impossible and the ability to create good plunge pools is impaired.

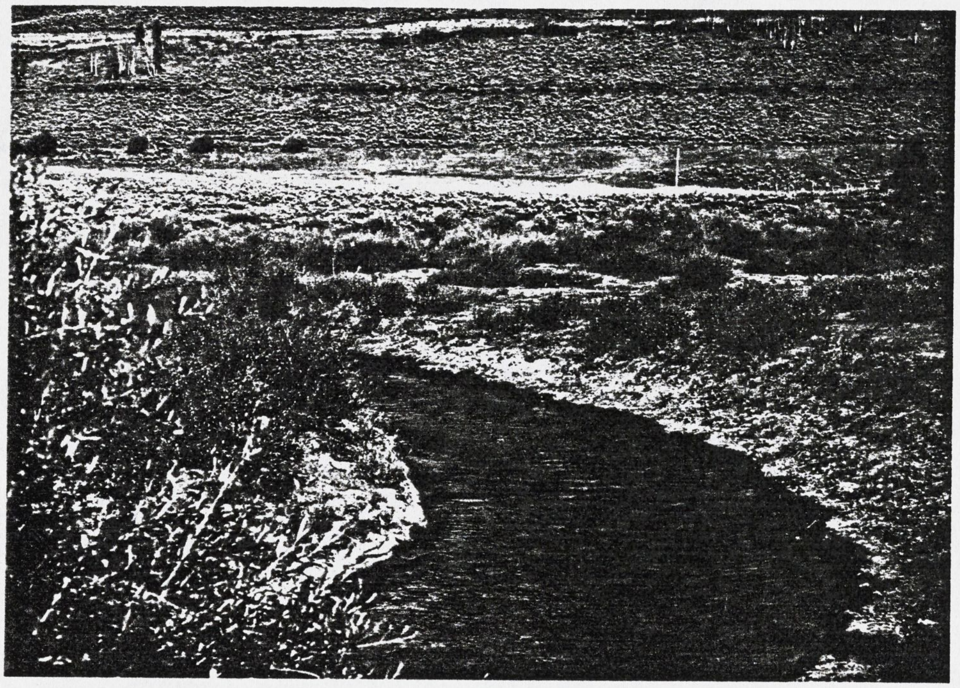

Figure 5. The S-bend artificial channel. The absence of riparian vegetation results in high sediment loads and an unstable channel. This channel offers a potential to increase spawning and nursery habitat for the river fishery.

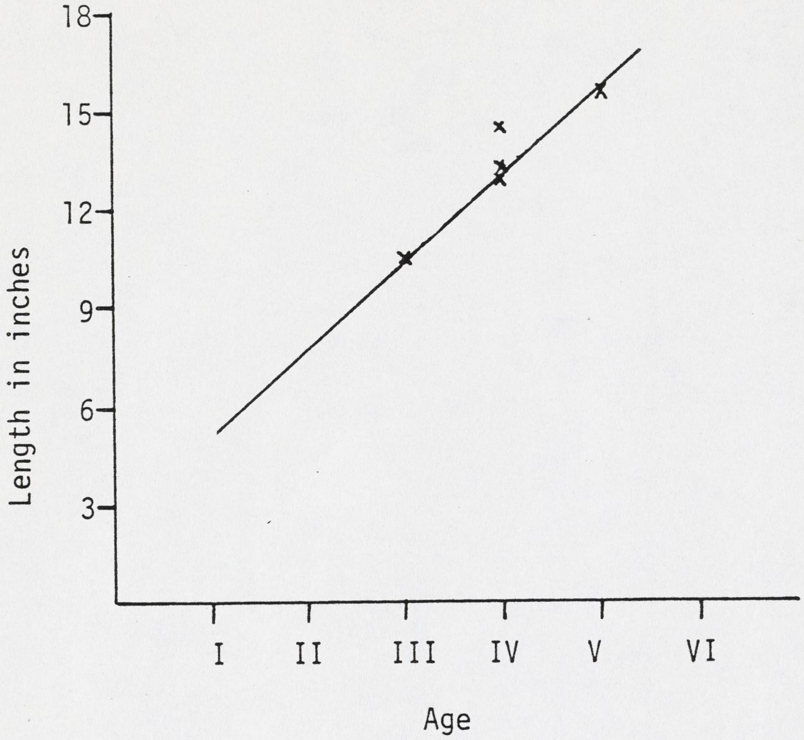

Figure 6. Probable average growth of rainbow trout in North Fork White River based on limited scale analysis.

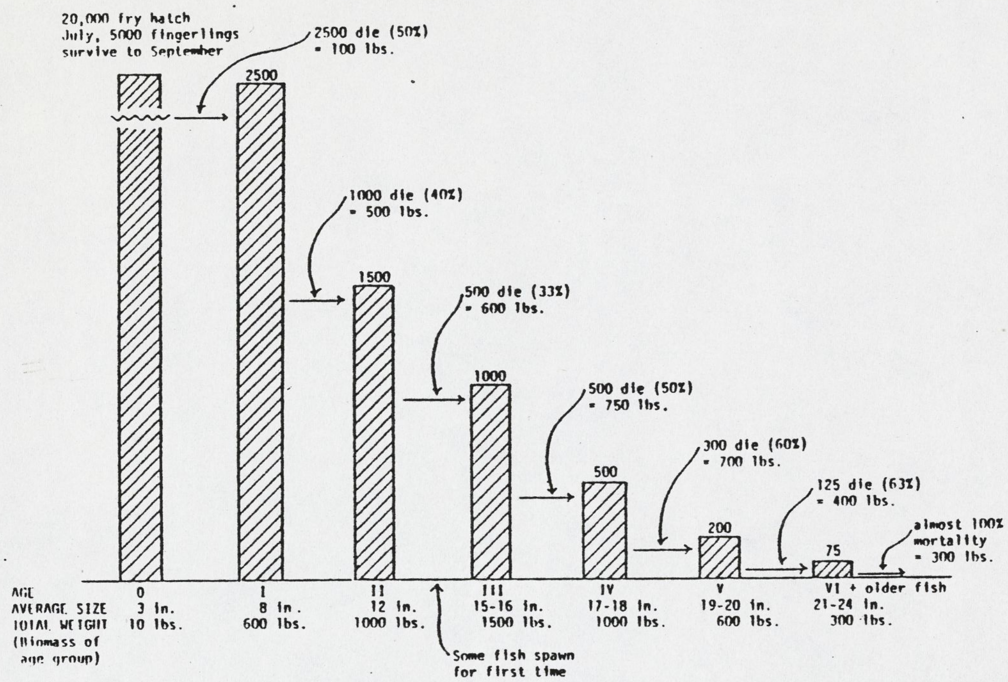

Figure 7. Hypothetical and idealized size and age structure of a trout population at the end of the growing season.

Assumptions for this model are that natural mortality rates are relatively low, growth and reproduction are good to excellent. Anglers remove only surplus production, 80% or less of the average annual natural mortality (overexploitation does not occur). In general angling mortality can substitute for about 80% of natural mortality (about 20% of natural mortality is "density independent" and would occur depsite angling mortality).

Overexploitation by anglers will occur if the number of trout removed by anglers equals or exceeds the numbers in the surplus production.

Restrictive regulations (most trout caught are released) aimed at creating a high catch-per-hour becomes necessary as angling pressure approaches surplus production.

Although the growth and survival rates in figure 7 are hypothetical, the basic principles are the same and can be applied to any trout population if the required information for each age group is known. Such information forms the basis for trout management of specific areas.

Figure 7 depicts a population with 5000 pounds of biomass (for example, 100 surface acres with 50 pounds per acre). There are 5775 fish of 8 inches or more in length. Of these 2500 will die from natural mortality (assuming no fishing) within one year. Assuming that fishing mortality is 80% compensatory to natural mortality, an angler harvest of 2000 fish would not overexploit this population and restrictive regulations would not have much impact unless the catch exceeds 2000.

ANALYSIS OF TROUT NATIVE TO SOME WESTERN VANCOUVER ISLAND STREAMS

Robert J. Behnke

November, 1981

SUMMARY

A total of 261 specimens, 124 collected in 1979 and 137 collected in 1980, from Saunders Creek, Heber River, Escalante River, Walbran Creek, Camper Creek, and Carmanah Creek, were examined to identify rainbow trout, cutthroat trout, and hybrids.

Cutthroat trout were found only in the collections from the lower Escalante River. Most other samples represent a predominently rainbow trout genotype but with a slight cutthroat trout hybrid influence. This is especially true in those samples from above barrier falls. Habitat diversity evidently is the limiting factor governing the coexistence of rainbow trout and coastal cutthroat trout. Where suitable diversity is lacking (the absence of low gradient sections with sloughs and backwaters, and small tributary streams) the two species do not coexist and hybrid populations may result that mature at a small size (100 to 130 mm). In the samples of suspected hybrids examined, the influence of cutthroat trout is extremely slight.

CHARACTER EVALUATION

The basis for identification is my characterization of "typical" coastal rainbow trout, _Salmo gairdneri irideus_, and typical coastal cutthroat trout, _S. clarki clarki_, the two species native to Vancouver Island. A study of the systematics of cutthroat trout reveals three distinctly divergent branches of the species--coastal cutthroat trout, occurring along the Pacific Coast from Prince William Sound, Alaska, to the Eel River, California, but rarely penetrating more than 200 km inland; the "westslope" cutthroat trout native to the upper Columbia River basin and to the South Saskatchewan and upper Missouri drainages; and the "Yellowstone" cutthroat trout of the upper Snake River and Yellowstone drainages. About 10 or 11 other subspecies of cutthroat trout represent subbranches off of the "Yellowstone" line. Thus, the coastal cutthroat trout represents a significant divergence long isolated from contact with any other form of cutthroat trout. There is, however, considerable genetic differentiation expressed in taxonomic characters and in life history and ecology found among local races of coastal cutthroat trout.

The evolutionary relationships of rainbow trout are more difficult to interpret because of the lack of geographic isolation between distinct ancestral forms. I currently recognize three major branches of *S. gairdneri*: a coastal rainbow trout (*S. g. irideus*); an interior group native to the Columbia River basin east of the Cascade Range and to the Fraser River basin above Hell's Gate (*S. g. gairdneri*), which is represented by races of steelhead, resident stream trout and lacustrine specialized forms ("Kamloops" trout); and, a more primitive group of the rainbow trout phylogeny native to segments of the Sacramento River basin, including the California golden trout, *S. g. aguabonita*. I have used the name "redband" trout for the latter two groups.

The taxonomic evidence indicates that considerable genetic interchange occurred during and since the last glacial epoch between all three major branches of the rainbow trout species. This makes it difficult to set definitive boundaries between the three major groups that would allow for consistent, positive identification.

Previous studies on British Columbia coastal cutthroat trout and rainbow trout found higher scale counts (in the lateral series) in rainbow trout samples than in cutthroat trout--averaging about 150 in rainbows and 140 in cutthroat--(Vernon and McMynn 1957, Hartman 1956). Hartman also found mean values of 41.9 pyloric caeca in rainbow trout and 42.6 in cutthroat trout. At the time, these results appeared quite contrary to the common belief that rainbow trout could always be separated from cutthroat trout by fewer scales and more numerous pyloric caeca. Actually, the rainbow trout used in these studies was the interior redband or "Kamloops" trout (Cultus Lake stock) and the cutthroat trout was a resident stock of coastal cutthroat (Chilliwack Lake). The scale counts of interior redband trout typically average 140 to 155 and typical caecal counts average about 40. It is a common phenomenon for resident stocks of coastal cutthroat trout to have fewer scales in the lateral series than sea-run stocks (about 140 to 150 vs. 155 to 170.

From my previous studies I characterize coastal rainbow trout (S. g. irideus) by low scale counts (averaging about 120 to 135 in the lateral series and 24 to 30 scales above the lateral line), high pyloric caecal counts (typically 50 to 60), and a complete absence of basibranchial teeth. Coastal cutthroat trout (S. c. clarki) will average about 140 to 170 lateral series scales, 34 to 40 scales above the lateral line, about 40 pyloric caeca, and 100% occurrence of basibranchial teeth in pure populations. Other characters useful to separate coastal cutthroat trout from coastal rainbow trout are the longer head and jaw in cutthroat trout, narrower, eliptical shaped parr marks in cutthroat (vs. more rounded in rainbow), more heavily spotted adipose fin (often with black bordering edge) in cutthroat trout, and 9 pelvic fin rays in cutthroat trout vs. 10 pelvic rays in rainbow trout. There are fewer vertebrae in cutthroat (typically 60 to 63 vs. 62 to 65). Rainbow trout generally have more gillrakers (18-21 vs. 15-19) and better developed gillrakers (longer, attenuated vs. short, blunt) than coastal cutthroat trout.

The character evaluation of the 261 specimens from 11 sites of 6 western Vancouver Island streams reveals that 11 specimens from the lower Escalante River are the only cutthroat trout found in the collections. There are 40 specimens of rainbow trout and two specimens identified as hybrids from the lower Escalante samples. The rainbows are probably young steelhead and the cutthroat probably represent young of a sea-run population. The other samples are identified as rainbow trout, although most of them have a definite, but very slight hybrid influence from cutthroat trout. Table 1 lists the characters found in the samples. There is a strong indication that Vancouver Island rainbow trout (and steelhead) average fewer pyloric caeca (40-50 vs. 50-60) than found in more southern populations of coastal rainbow trout. The samples collected in 1979 were partially decomposed and character values are only "best estimates." The 1979 sample values do, however, closely agree with the better preserved 1980 samples when compared from the same collection site.

Upper Saunders Creek

Nine specimens collected in 1979 show no definite indication of a cutthroat trout hybrid influence. The gillraker count (17-19) is somewhat lower than expected for pure rainbow trout but the scale counts (average 27.5 above lateral line and 124 in the lateral series) are typical values for coastal rainbow trout. No specimen has any sign of basibranchial teeth.

Upper Heber River

None of the 15 specimens collected in 1979 have basibranchial teeth, but scale counts (120-141 [131]) and more varied phenotypic appearance indicate a slight hybrid influence.

Lower Heber River

In contrast to other collections, the sample from the lower Heber River

Table 1. Character analysis of Vancouver Island native trout.

Locality	Gillrakers	Scales above and lat. ser.	Pyloric caeca	Basibranchial Teeth
Saunders Crk. upper, 1979, N=9 82-162 mm	17-19(18.0)	24-33 (27.5) 115-134(124)	-	none w/teeth
Heber R. 1979 upper, N=15 83-147 mm	17-21(18.9)	25-32 (27.6) 120-141(131)	30-42(35) (?)	none w/teeth
Heber R. 1979 lower, N=12 122-162mm	18-20(19.4)	25-30 (27.2) 127-141(133)	30-50(?)	2 w/one tooth 10 none
Escalante R. above, 1979 N=27 80-137mm	17-21(19.4)	24-32 (28.6) 121-142(131)	36-53(43) (?)	2 w/one tooth 25 none
Escalante R. below, 1979 N-26 84-144 mm	18-21(19.4)	23-31 (27.2) 109-134(123.5)	_	none w/teeth
Escalante R. below, 1979 Cutthroat N=1	17	35 147	-	19
Escalante R. below, 99-161 mm 1980 cutthroat N=10	17-19(17.8)	32-37 (34.2) 141-154(147)	35-49(42)	5-38(18.9)
Escalante R. below, 1980 rainbow 105-142mm N=14	19-22(19.6)	24-31 (26.7) 120-136(126.3)	35-53(42)	none w/teeth
Escalante R. below, 1980 hybrid N=2	19,20	29,30 133,139	-	one tooth each
Walbran, upper 1979, 99-147 mm N=34	17-21(18.8)	24-33 (28.3) 122-142(132)	34-56(42) (?)	2 w/one 32 none
Walbran, upper 1980, 98-144 mm N-20	16-21(18.6)	23-32 (28.0) 122-141(131)	33-55(43)	1 w/two 19 none
Walbran, lower 1980, 100-124mm N=15	17-21(19.0)	24-32 (28.1) 119-131(124)	36-48(41)	none w/teeth
Camper Crk. upper, 1980 88-155 mm N=21	17-21(18.8)	24-32 (27.8) 125-143(132)	42-57(49.8)	2 w/one 19 none
Camper Crk. lower, 1980 93-124 mm N=18	17-22(20.3)	24-31 (27.2) 115-140(128)	36-49(41)	1 w/one 17 none

Table 1. Continued

Locality	Gillrakers	Scales above and lat. ser.	Pyloric caeca	Basibranchial Teeth
Carmanah Crk 1980, upper N=20 105-141mm	17-21(18.9)	26-32 (29.3) 120-133(126)	40-52(44)	1 w/one 19 none
Carmanah Crk. 1980, lower N=17 75-139mm	18-22(19.0)	26-31 (28.1) 121-135(127.6	31-52(42)	1 w/one 16 none

contains a more detectable cutthroat trout influence. Two of 12 specimens have a basibranchial tooth and scale counts average slightly higher (133) than in upstream samples.

Upper Escalante River

The 27 specimens collected in 1979 are similar to the Heber River samples. Two specimens have a basibranchial tooth and scale counts average 131.

Lower Escalante River

Of 27 specimens collected in 1979, 26 are rainbow trout and one is a cutthroat trout. The cutthroat specimen clearly is distinguished by 19 basibranchial teeth (none in any rainbow specimen), 147 lateral series scales and 35 scales above the lateral line (vs. 109-134 [123.5] and 23-31 [27.2]) and 9 pelvic rays (vs. 10). Of 26 specimens collected in 1980, 10 are cutthroat trout. These specimens have 5 to 38 basibranchial teeth (18.8). This mean value is higher than I have found in any other sample of coastal cutthroat trout except for the original population found in Lake Sutherland, Washington (once erroneously described as two species-- *S. jordani* and *S. declivifrons*, in the late nineteenth century). The scale counts are clearly distinguished from the scale counts of 14 rainbow trout specimens with no overlap of counts. Differences are apparent in gillraker number and structure (17-19 [17.8] vs. 19-22 [19.6]). Both species from the lower Escalante River average 42 pyloric caeca. Two specimens each have a single basibranchial tooth. These specimens have 19 and 20 gillrakers and 133 and 139 lateral series scales. They are identified as hybrids. Evidently the environment of the lower Escalante River favors the maintenance of both the cutthroat trout niche and the rainbow trout niche. Hybrids must be at a severe selective disadvantage otherwise the effects of hybridization would be much more apparent.

In the 1980 collection, the specimens identified as cutthroat trout are numbered 5, 10, 12, 14, 15, 17, 20, 23, 25, and 26. The hybrid specimens are numbered 1 and 7. A letter from Eric Parkinson (Jan. 30, 1981) disclosed that the only cutthroat trout identified by electrophoretic analysis of proteins during his study are numbers 5, 10, 12, 14, 15, 20, 25, and 26 from the lower Escante River. Eric mentioned that a few other fish have a cutthroat character in one of three enzyme systems. Our comparative analysis is in agreement except for specimens 17 and 23 which I identify as cutthroat trout (these specimens have 13 and 24 basi~~a~~branchial teeth respectively).

Upper Walbran Creek

The 54 specimens examined from upper Walbran Creek are identified as rainbow trout with a slight cutthroat trout influence in their heredity, similar to the collections from the upper Escalante River and the Heber River. Two of 32 specimens collected in 1979 and 1 of 20 specimens of 1980 have basibranchial teeth. The phenotypic appearance suggests a cutthroat influence.

Lower Walbran Creek

The sample of 15 specimens from lower Walbran Creek represent an unhybridized population of rainbow trout--probably young of steelhead. None have basibranchial teeth, the lateral series scale count averages 7-8 less than samples from above the falls and the gillrakers are better developed.

Upper Camper Creek

This sample of 21 specimens collected in 1980 is typical of other slightly hybridized samples. Two of 21 specimens have a basiabranchial tooth.

Upper Carmanah Creek and Lower Carmanah Creek

Samples of 20 specimens from the upper section and 17 specimens from the lower section of Carmanah Creek appear virtually identical. I can find no evidence in their characters that these samples represent two separate populations although they are isolated by a falls. Mr. Parkinson's electrophoretic analysis, however, did find significant allelic frequency differences between upstream and downstream samples demonstrating that they do represent different populations. This is an example where electrophoretic data provided a valuable input to this study. The regulatory genome governing morphology undoubtedly is different between the upstream and downstream populations but I could not detect differences in the few characters analyzed. On the other hand, differences in the structural genome were found by electrophoresis. Most of the specimens from above the falls of more than 100 mm, both males and females, have developing gonads, indicating spawning the following spring (resident population). Two specimens from below the falls (123 and 128 mm) have developing testes. The gonads are undeveloped in the remaining specimens from below the falls, typical of young steelhead. Perhaps both resident and anadromous populations exist below the falls but the resident population can not be differentiated from young steelhead except by degree of sexual maturation.

ELECTROPHORETIC ANALYSIS

Electrophoretic analysis was performed by Mr. Eric Parkinson, B.C. Fish and Wildlife Branch, Vancouver. Specimens from above and below falls from Escalante, Walbran, Camper, and Carmanah creeks were analyzed. These same specimens were then forwarded to me for morphological analysis. Mr. Parkinson sent a letter (Jan. 30, 1981) and later sent a segment of his thesis detailing his study. Basically, the two types of analyses--

morphological and electrophoresis of proteins--evaluates two different components of a genome. Regulatory genes govern morphology and life history attributes. Structural genes code for proteins. The two components of the genome may evolve at different rates and it is not unusual for the two methods to lack good agreement on degrees of differences.

In the present situation we were in overall agreement that the upstream populations are different from the downstream populations and that only collections from lower Escalante Creek contained pure cutthroat trout. As mentioned, I identified 10 specimens from lower Escalante Creek as cutthroat trout whereas Mr. Parkinson identified 8 cutthroat in his letter, but 9 in his thesis. Also, Mr. Parkinson could not make positive statements regarding hybrid influence in samples that I believe to have a slight hybrid influence.

Mr. Parkinson found only two gene loci, ME and SDH, that yielded some consistent separation between cutthroat trout alleles and rainbow trout alleles. Evidently the one (or two) specimens I identified as cutthroat trout and Mr. Parkinson identified as rainbow trout from lower Escalante Creek, resulted from cutthroat trout specimens possessing a rare allele that duplicates the electrophoretic mobility of the most common rainbow allele at the ME locus (and perhaps the SDH locus). Without the benefit of "species specific" gene loci (whereby all cutthroat trout differ from all rainbow trout 100% of the time), a very slight (5 to 10%) hybrid influence would be difficult to assess. Also, the sorting out of a genome over hundreds of generations may result in the incorporation of cutthroat genes in the regulatory part of the genome but not in the structural part.

A recent thesis by Campton (1981) on electrophoretic analysis of cutthroat trout from Puget Sound drainages found five loci (GLP, IDH, SDH, ME, CPK) that significantly differ between coastal cutthroat trout and coastal rainbow trout. Campton found hybrid individuals in most streams and two streams had a high proportion of hybrids. He also verified that the Beaver Creek, Washington hatchery stock of "cutthroat" trout is hybridized.

DISCUSSION

The major problem involved for detecting and taxonomically quantifying the genetic background of Vancouver Island native trout--rainbow trout, cutthroat trout, resident, anadromous, seasonal races of anadromous forms, and varying degrees of hybrid influence, is the extremely close relationships involved. Starting with the most distantly related groups--the rainbow trout and the cutthroat trout species--we are dealing with divergence from a common ancestor of a probable magnitude of more than one million years. The genetic relatedness of rainbow and cutthroat trouts is close, however, more comparable to differences found between subspecies of most other animal species (as quantified from genetic similarity scores based on electrophoretic analysis). They can hybridize and the offspring are fully fertile. Thus, reproductive isolation between rainbow and cutthroat trout is a matter of niche separation and coexistence must occur in an environment that has a habitat diversity favoring the maitenance of both niches while selecting against any hybrids produced. Where steelhead rainbows and sea-run cutthroat coexist, the marine segment of their respective life histories is so different that a powerful selective pressure against hybridization can be assumed. The resident forms of rainbow trout and cutthroat trout are not so clearly differentiated ecologically--there is broad niche overlap, especially in streams. Glova and Mason (1977a,b,c,), Tomasson (1978), Armstrong (1971), Cooper (1970), Bustard and Narver (1975), Narver (1975), Summer (1953, 1962), Giger (1972), Nicholas (1978a,b), Ringler and Hall (1975), Hartman and Gill (1968), Neave (1944), Nilsson (1971), Idyll (1942), Ricker (1941), Withler (1966), Hassler and Van Kirk (1977), Johnson and Mercer (1976), Andrusak and Northcote (1970), and personal observations were used to assess the niches of coastal rainbow trout and coastal cutthroat trout including anadromous, resident stream and resident lacustrine stocks.

Alley and Chatwin (1979) described the late Pleistocene glacial history of southwestern Vancouver Island. It was not until about 13,000 years ago that significant areas were potentially habitable by fish. Glacier lakes were present in valleys during the later stage of glaciation and this could have provided opportunities for cutthroat, rainbow, and Dolly Varden to become established in headwater sites that are presently isolated by falls in various drainages.

A more probably explanation, hwever, of resident, native trout about barrier falls, is the rise in sea level towards the end of the last glacial epoch due to the enormous input of melting ice. In various parts of the world increases of 100 m or more in sea level have been calculated. Any falls less than 100 m above present mean sea level was probably innundated during this period. Due to the open ocean environment on the west coast of Vancouver Island, I would assume that rainbow trout (steelhead) were overwhelmingly dominant over cutthroat trout among the original invader species. The small cutthroat ancestry was incorporated into rainbow trout populations. This is manifested today by the cutthroat-like characters found in the populations. Selection would be rapid to establish completely resident populations above falls. The complete loss of "migratory" genes in individuals moving downstream over the falls would essentially fix the hereditary basis for nonmigratory behavior in a relatively few generations.

I was particularly interested in the possibility that the first invaders into deglaciated regions represented primitive or divergent forms of cutthroat or rainbow trout that might persist as relict populations isolated from later invasions by more advanced forms of the species. Nothing in the specimens I examined indicates that such an event is represented in the populations sampled. I assume that the different ecological forms (resident, anadromous, and various life history themes of each) of both rainbow and cutthroat trout of southwestern Vancouver Island arose from a common ancestral steelhead and a common ancestral sea-run cutthroat during the past 15,000 years. If divergent ancestral races of either species participatd in the early invasions of the deglaciated regions, it appears that they were thoroughly homogenized into the prevailing common ancestral forms.

The most diagnostic morphological character to assess purity of coastal cutthroat trout and coastal rainbow trout and the influence of nybridization is basibranchial teeth, supplemented by scale counts and gillraker development. I have never found basibranchial teeth in populations of coastal rainbow trout that are isolated from contact with cutthroat trout. Every specimen of coastal cutthroat trout that I have examined from populations isolated from rainbow trout (more than 200 specimens from California to Alaska), have basibranchial teeth. I would point out that the detection of basibranchial teeth is not a simple matter, particularly in small specimens. I apply alizarin-red stain to the floor of the pharynx and let it be absorbed for one day by any bony derivative before examination.

If a sample from a "strange" appearing cutthroat population shows a 10% to 20% incidence of specimens lacking basibranchial teeth, it can be assumed that the population has been introgressed by rainbow trout. If "peculiar" looking rainbow trout are examined and if any specimens have basibranchial teeth, it can be assumed that cutthroat trout genes are present in the population.

From my observations of living specimens from Walbran and Camper Creeks and of color photographs (ex. Sue Creek on Queen Charlotte Is.), it appears that a slight hybrid influence can be accurately determined simply by visual analysis with some experience. Cutthroat trout with spots highly irregular in size, shape, and distribution indicates a rainbow influence in the population. Rainbow trout, more brightly colored, with yellowish or orange tinted "cutthroat" slashes under the jaw, with highly pigmented adipose fins, etc. indicate a cutthroat influence in the population.

In larger river systems coastal cutthroat trout with three types of life histories may be found--resident in small tributaries, resident, migrating from tributaries (spawning) to the main river, and sea-run. If a lake exists in the drainage, a fourth type, a lacustrin specialized form may be found. Cutthroat trout are not known to move into deep, open ocean waters. They appear to be restricted to bays, lagoons, estuaries, and shallow inshore coastal areas (such as among islands), and only rivers associated with such environments can be expected to contain sea-run cutthroat trout. Deep, open sea areas occurring between rivers evidently acts to block interchange between populations (Utter et al. 1980, Campton 1981). Coastal rainbow trout may also have three or four life history types in larger drainages--resident river, resident lacustrine, and summer and winter runs of steelhead. Natural selection favors the fractioning of a species into distinct ecological forms because it results in more complete and efficient utilization of the entire environment and its resources, which in turn, results in increased abundance and biomass of the species. The maintenance of distinct ecological forms of a species where they occur in a continuous environment depends on reproductive isolation. This reproductive isolation can be extremely fragile especially where there is little separation in time and place of spawning among the ecological races. In such situations a wise management objective would be to avoid environmental disturbances that would tend to break down the delicate mechanisms of reproductive isolation or to cause the loss of one of the races.

It is obvious that the samples from streams above falls on western Vancouver Island examined for this study do not represent reproductively isolated populations of rainbow and cutthroat trouts. They are predominantly rainbow trout with a small (perhaps about 5%) cutthroat influence. Evidently these streams lack habitat and food diversity that would favor the coexistence of the two species. A clear definition of the niche of coastal cutthroat trout and the niche of coastal rainbow trout and a definition of habitat characteristics that favor the maintenance of the separate niches that favor one niche over the other, or act to favor breakdowns and hybridization is difficult to delineate. A major factor obscuring clean-cut definitions is that there is no single coastal rainbow trout niche and no single coastal cutthroat trout niche--each species must be at least subdivided into anadromous, resident fluvial, and resident lacustrine categories. Besides the scientific literature cited above, Roderick Haig-Brown provided some keen observations on resident and sea-run cutthroat trout of Vancouver Island in his book, "The Western Angler." Haig-Brown greatly admired the coastal cutthroat trout.

When both rainbow trout and cutthroat trout coexist as resident river populations, their niches seem to broadly overlap and their strategy to reduce competiton and avoid hybridization is to segregate to different habitat types. The cutthroat live predominantly in small headwater areas and in tiny tributaries. The rainbow predominates in the main channel environment and in steeper gradient sections of the watershed. In such situations the cutthroat trout typically has slow growth, matures at a young age and at a small size (100 to 135 mm), and rarely lifes for more than three or four years. Low gradient sections of rivers with sloughs and backwater are favored cutthroat trout habitat. In larger rivers, and particularly in lakes, the coastal cutthroat trout is highly predaceous and is frequently dominant over resident rainbow trout in abundance, growth rate and maximum size. Idyll (1942) found the cutthroat trout to be more piscivorous than the brown trout (S. trutta) in the Cowichan River on Vancouver Island. Ricker (1941) found the cutthroat trout of Cultus Lake to be the most intense predator of sockeye salmon. In lakes on Vancouver Island and the coastal regions of the mainland in British Columbia, the cutthroat trout is more predaceous than coexisting rainbow trout or Dolly Varden and the cutthroat typically attains a larger size (I would point out that there is much confusion in the literature regarding the feeding of the "Dolly Varden." The "true" Dolly Varden, Salvelinus malma, is considerably less predaceous than is the bull trout, S. confluentus. The two species have been both referred to as Dolly Varden in the literature). Nilsson and Northcote (1981) presented excellent data on 17 allopatric and 10 sympatric populations of cutthroat and rainbow trouts in British Columbia lakes. In allopatry rainbow trout grow faster and attain a greater maximum size than allopatric cutthroat populations. When the two species both inhabit the same lake, however, the reverse is true; in sympatry, the cutthroat has a more rapid growth and attains the greater size. The cutthroat was found to be considerably more predaceous than the rainbow (mainly sculpins and sticklebacks). In laboratory experiments, rainbow trout are clearly more aggressive and dominant over cutthroat trout of comparable size. The major food segregation found in the B.C. lakes was the result of rainbow trout feeding predominantly in offshore, open lake areas in midwater and on the surface (virtually entire diet of invertebrates) with cutthroat trout feeding predominantly in the littoral benthic zone with an emphasis of piscivorous feeding. The ecological relationships of coexisting cutthroat trout and rainbow trout in British Columbia lakes, in regards to growth and maximum size, seems to contradict the relationships between the two species when they coexist in river-tributary systems. It is difficult to realize that the data are based on a single subspecies of cutthroat trout *S. c. clarki*. The point is that the coastal cutthroat trout has a wide range of adaptive responses to allow it to exist in diverse environments. Given thousands of years to perfect certain aspects of these adaptive responses, many specialized races have evolved, "fine-tuned" for specific types of environments.

BIOLOGICAL IMPLICATIONS

For planning natural resource exploitation or development projects in a virgin environment it is important to know the species of fishes and the ecological forms (anadromous, resident, etc.) of the species occurring in the watersheds of the proposed impact and their economic and scientific significance in relation to human values. This statement is, of course, a very obvious premise, but the next step of predicting the short-term and long-term consequences of the proposed action, is a highly complicated matter for which no universally accepted standard or format of proven efficacy is available to follow.

A problem rooted in philosophy and logic concerns the fact that delineating components of ecosystems is a form of classification. The philosophical and logical basis for watershed classification was considered in some depth by Warren (1979) who concluded that the most universally applicable classification is one based on potential production capacity.

Another problem concerns the great natural variation in abundance of most salmonid populations. How can one know if increases or decreases in numbers are due to man-induced environmental change or to natural variation? This matter is given an excellent review (with 260 references) by Hall and Knight (1981). Hall et al. (1978) discussed "best designs" for studies assessing impacts on streams (see also Romesburg [1981] regarding the scientific basis of assessment).

The point emphasized in Holling's book, "Adaptive Environmental Assessment," that no environmental assessment prediction can ever be perfect, is important to keep in mind. However, the degree of accuracy of predictions can be greatly increased over simple chance. By developing expertise, we can load the predictive dice in our favor.

It is simply not feasible to make detailed studies of every small stream of a proposed impact site where the concerned geographic area is large. What can be done is to sort out probable cause-and-effect relationships looking for common denominators that provide relatively confident predictions of the consequence of specific changes. That is, all salmonid populations can be predicted to respond in like manner in every stream of the area given a specified change in an environmental parameter such as light, temperature, sediment, channel morphology, cover, flow regime, etc.

For example, certain assumptions can be made for practical management purposes that carrying capacity and production of salmonids will be increased if habitat conditions improve by the creation of areas of deeper, low velocity waters with abundant cover, and increased production will result if the food supply increases from increased primary and secondary production due to increased photosynthesis and/or increased enrichment. Production can be expected to increase if maximum summer water temperatures are increased from a range of 10-13°C to a range of 15 to 18°C, and production will decrease if the maximum temperatures increase to the 25°-28° range. Increased sediment loading and changes in channel morphology that increase velocity and decrease depth and cover will act to reduce carrying capacity and production (Murphy et al. 1981, Chapman and Knudsen 1980, Murphy and Hall 1981).

Salmonid population(s), abundance and biomass can be considered and assessed as being food limited or habitat limited and habitat can be considered and assessed as four components--spawning, rearing or juvenile, adult, and overwinter. It may be that an increase in the food supply will be vitiated by the limitations of overwintering habitat.

From a practical, economic point of view, greatest emphasis would be made to increase production of anadromous runs. With steelhead, coho, and chinook salmon the strategic habitat components to favor would be spawning, juvenile, and particularly overwintering habitat. A practical, "learn by doing" approach to observe what habitat characteristics are significantly associated with overwintering of salmonids and then attempt to create similar conditions, as exemplified by Pollard (1981), is to be recommended if more rapid progress is to be made for preserving and enhancing the salmonid environment in disturbed watersheds.

The salmonid populations isolated above barrier falls such as those discussed in this report and so common on Vancouver Island, characterized by slow growth, short life span, and small size, essentially have little or no economic importance because virtually no specimen attains the B.C. minimum length limit of 8 inches in its lifetime and they do not contribute to downstream and anadromous runs. Northcote (1970, 1981) and Northcote and Kelson (1981) studied a comparable situation of populations of rainbow trout above and below a falls on Kokanee Creek, tributary to Kootenay Lake. As with the present study, morphological and electrophoretic distinction revealed that the rainbow trout above and below the falls on Kokanee Creek consists of discrete populations. The innate behavior pattern for movement of the two populations differed. In several years of study, virtually no individual from the upstream population moved downstream over the falls. These results are expected on the basis of evolution and natural selection as previously discussed.

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THE NATIVE CUTTHROAT TROUT OF WYOMING

VI: Three samples collected during 1981 from Green River and Bear River drainages.

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Three samples consisting of a total of 42 specimens were examined and evaluated for purity. Two samples of S. c. pleuriticus are graded B+ (Trib. to Rock Crk.) and B- (Trib. to South Horse Crk.). The sample of S. c. utah from Water Canyon Creek appears to represent a pure, or essentially pure population and is graded A.

Tributary to Rock Creek (R111W, T38N, S11).

This sample has the phenotypic appearance of pure pleuriticus. The spotting pattern is uniform with relatively small spots. The outward appearance is similar to Lead Creek specimens (although not so "perfectly" uniform). The gillraker count of 19-22 (20.7) is higher than other pleuriticus samples yet examined and is typical of Yellowstone Lake cutthroat trout. A Yellowstone influence is ruled out, however, on the basis of spotting and basibranchial teeth. The counts of pyloric caeca and scales are typical of pleuriticus. The fact that 6 of 13 specimens (44%) lack basibranchial teeth is surprising in view of the other characters and indicates some rainbow trout genes are incorporated into the population. No other hybrid influence is apparent. I can only speculate that a rainbow trout hybrid influence affected this population long ago and over many generations virtually all rainbow genes have been sorted out; but, perhaps due to translocation on the chromosomes, a very few non-native genes were fixed in a position that results in an exaggerated suppression of basibranchial teeth.

Table 1 compares the present sample with a sample of 8 specimens collected from Rock Creek in 1977 and discussed in report 3 of this series. The taxonomic characters are virtually identical. Two of 8 specimens lacked basibranchial teeth in the 1977 sample; however, these specimens had more erratic and nonuniform spotting suggesting a hybrid influence from Snake River cutthroat trout and rainbow trout. The 1981 sample from the tributary stream has a superior phenotypic appearance as S. c. pleuriticus.

The size structure of this sample is interesting. They are relatively large trout, from 174 to 218 mm T.L. with a mean of 203 mm, and with 12 of 13 specimens exceeding 180 mm. This would indicate good adult habitat (deep, low velocity areas with good cover) and low angling pressure.

The Rock Creek under discussion is tributary to the upper Green River near Kendall and should not be confused with The Rock Creek which is tributary to LaBarge Creek.

Table 1. Comparison of taxonomic characters

	Gillrakers	Pyloric caeca	Scales above l.l. and in lateral series	Basibranchial teeth
Green R. Westside tributaries, S. c. pleuriticus				
Trib. Rock Crk. R111W, T38N, S11 N=13:174-218 (203) mm.	19-22 (20.7)	27-40 (35)	41- 48 (45) 174-201(185)	6:no teeth 7: 1-5 (3.0)
Rock Crk. coll. 1977:N=8	18-22 (20.1)	28-38 (34)	41- 46 (44) 168-196(186)	2: no teeth 6: 1-7 (3.2)
Trib.So.Horse Crk. R115W, T34N, S14 N=17:93-209 (131) mm.	16-20 (18.0)	36-49 (43)	40- 48 (44) 169-195(179)	5: no teeth 12:1-14 (4.1)
Bear R. drainage, S. c. utah.				
Water Canyon Crk. R118W, T29N, S19 N=12:122-203 (152) mm.	17-21 (19.1)	35-49 (43)	37- 45 (40) 157-186(172)	All w/teeth 1-16 (6.3)

Tributary to South Horse Creek (R115W, T34N, S14)

Several samples from the Horse Creek drainage were evaluated in earlier reports and many were summarized by Binns (1977). Except for Lead Creek, all previous samples from the Horse Creek drainage have exhibited signs of hybridization. The present sample is quite typical of other samples from this drainage with the exception of gillraker number--16-20 (18.0)--the lowest count found to date for S. c. pleuriticus. No known hybrid influence except from Bear River cutthroat or coastal cutthroat trout would cause a reduction in gillrakers. The higher numbers of pyloric caeca (43) and slightly lower lateral series scale counts (179) are indicative of a rainbow trout hybrid influence as are basibranchial teeth (29% without teeth). The spotted pattern is not quite as uniform as in the Rock Creek sample, but no obvious hybrid influence is apparent from the phenotype. This sample represents a "good" representation of pleuriticus--they are overwhelmingly of pleuriticus heredity. The size structure of this sample ranges from 93 to 209 mm T.L., but averages only 131 mm and with only 2 of 17 specimens more than 180 mm. This would indicate poor adult habitat and/or more intense angler removal in comparison to the Rock Creek tributary.

Water Canyon Creek (R118W, T29N, S19)

The taxonomic characters of this sample are wholly typical of the Bear River drainage form of S. c. utah. The spots on these specimens are somewhat smaller than typically found on other S. c. utah, but this is likely due to the small size of the specimens. All of the 12 specimens possess basibranchial teeth. I would rate this sample as "A", but before this population is used for propagation or transplanting another 10-12 specimens should be examined and a few larger specimens (ca. 225 to 250 mm) should be compared with large S. c. utah specimens from Raymond Creek or Coantag Creek to evaluate concordance in spotting. It is not unusual for a local population, although pure, to significantly differ from the "norm" of the subspecies in spotting pattern (for example, Sedge Creek cutthroat trout and Yellowstone Lake cutthroat trout in Yellowstone Park). However, for propagation of a subspecies, using diverse sources, the disruption of the phenotypic "norm" may be undesirable.

Evidently, Water Canyon Creek is a small headwater tributary to the Smith Fork of the Bear River drainage in Lincoln County.

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EVALUATION OF REDBAND TROUT COLLECTIONS FROM MALHEUR RIVER DRAINAGE, MALHEUR COUNTY, OREGON

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ABSTRACT

The taxonomic status of the redband trout of the Malheur River drainage is defined. Ten samples from the Malheur drainage and one sample from the Owyhee River drainage exhibit local variability, but little or no evidence of a hybrid influence from introduced hatchery rainbow trout was found in most samples. The overwhelming predominance of the native trout genotype in the Malheur drainage is somewhat surprising in light of a past history that would be expected to promote hybridization. The perseverance of the native genotype is attributed to its superior adaptation to local conditions.

Problems of implementing meaningful fish habitat improvement programs on federal lands are discussed.

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INTRODUCTION

The question is often asked: What is the redband trout? What is its taxonomic classification? The redband trout has been featured on television on the American Outdoors program; the redband trout was the object of a laudatory article in the October 1978 issue of the magazine, Fly Fishing the West; another article in the April 1979 issue of this magazine described catching redband trout in the Malheur River drainage in water of 85°F. Fly fishing the West has used a color photograph of a Malheur River redband trout on its advertising brochure. Ernest Schwiebert included a color illustration of the redband trout, which he "named" *Salmo oregonensis*, in his classic book, "Trout." Deacon et al. (1979) listed the redband trout, *Salmo* sp. as a species of special concern in their paper on North American fishes that are endangered, threatened, or of special concern. The most recent edition of A List of Common and Scientific Names of Fishes (1980, Am. Fish. Soc. Spec. Pub. 12), annotated the rainbow trout, *Salmo gairdneri*, to point out that the redband trout was recognized as a separate, but unnamed, species by Deacon et al. (1979) and that its systematic status was unresolved.

Popular interest in the mysterious redband trout is the result of a series of publications and reports I have written during the past several years beginning with Schreck and Behnke (1971) and most recently summarized in Behnke (1979, 1981). I have pointed out that the fish so long considered as "rainbow trout" actually consists of enormous variability of forms. From an evolutionary perspective, I recognize three major lineages of "rainbow" trout--a coastal subspecies, a group associated with isolated segments of the Sacramento River basin, and a group associated with the middle Columbia and upper Fraser river drainages, east of the Cascade

Mountains (also including the Oregon desiccating basins). I have called the latter two groups redband trout.

In a monograph on native western trouts (Behnke 1979) written for the U.S. Fish and Wildlife Service, I separated the redband trout (both Sacramento and Columbia) from the coastal rainbow trout by designating the redband trout as a distinct species, *Salmo newberryi* (the oldest name applicable to any form of redband trout). I emphasized the point, however, that my classification scheme giving species recognition to redband trout was strictly a matter of convenience to facilitate an understanding of the evolutionary diversity of rainbow trout, and that I would not recognize the redband trout as a separate species in a formal publication. During 1981 I updated and revised my 1979 monograph for publication. In the revision I include all rainbow and redband trout under *Salmo gairdneri* because there are no sharp boundaries to delimit all rainbow trout from all redband trout. The morphological, cytogenetic and biochemical evidence agrees on this point. Evidently, considerable gene flow between all three major evolutionary lines has occurred during and since the last glacial epoch, obscuring clear-cut genetic demarcation that could serve as a basis for distinct species recognition. This gene flow has been greatly stimulated in the past 50 years by the massive stocking of hatchery rainbow trout and concern has been expressed regarding the genetic purity of remnant redband populations (Deacon et al. 1979).

When the rainbow-redband trout species, *S. gairdneri*, is divided into subspecies, many complex problems are encountered in relation to determining the correct subspecific name for any particular geographic group. I have determined that the name *gairdneri* was revised by Jordan and Evermann (1896) to apply to Columbia River redband trout by their description of specimens with 137 to 177 scales in the lateral series and with about 40 pyloric caeca. These diagnostic characters separate redband trout from coastal rainbow trout (which have about 120 to 140 scales and about 50 to 60 caeca). Thus, S. g. gairdneri is the correct subspecific name to designate the rainbow (redband) trout of the Columbia River drainage east of the Cascades. As such, it includes races of anadromous steelhead trout, resident lacustrine populations (commonly called Kamloops trout), and resident stream populations such as those occurring in the Malheur River drainage. The coastal rainbow trout is recognized as S. g. irideus. Virtually all sources of domesticated stocks of hatchery rainbow trout are derived from the coastal rainbow trout.

EVALUATION OF SPECIMENS

A total of 109 specimens of 11 samples was examined. Ten of the samples were collected from the Malheur River drainage, Malheur County, Oregon, and one sample, South Fork Carter Creek, Malheur Co., is tributary to the Owyhee River. Other recent collections of redband trout made by Mr. Robert Smith of Central Point, Oregon, from the Malheur River drainages are discussed.

There is no known technique that can be used to verify the purity of a population of redband trout. It can be assumed that sometime during the Pleistocene times the ancestral rainbow trout became fragmented and gave rise to diverse evolutionary lineages. Presently, the derivatives of these divergences are represented by the redband trout of the Sacramento River basin, the redband trout of the middle Columbia River basin, and the coastal rainbow trout. As evolutionary lines become separated, they begin to slowly accumulate genetic differences. The total genetic resources of a grouping, its genome, can be considered as two components in relation to the manifestation of divergence. The metabolic genome, the enzyme systems, can be biochemically compared using electrophoresis techniques. The regulatory genome governing morphology and life history traits, makes up about 99% of the total genome and a valid assessment of its divergence requires both quantitative and qualitative characters and considerable experience of the investigator. The regulatory genome is more influenced by natural selection in relation to rate of divergence, thus, there is no reason to assume that the evidence on the magnitude of genetic differentiation derived from assessment of the enzyme systems and from morphology will be in close agreement (Clayton 1981, Todd 1981). The different rates of divergence in respect to the enzyme systems and morphology is well illustrated among the rainbow and redband trout. The Columbia River redband trout are morphologically divergent from coastal rainbow trout in several characters. In phenotypic appearance, redband trout have larger spots, more restricted to above the lateral line, their parr markings are characterized by a main row of elliptical shaped marks with a dorsal and ventral scattering of supplemental rows of smaller and irregular parr marks. The dorsal, anal, and pelvic fins are prominently tipped with white or yellow. Yellowish (sometimes orange) tints characterize the lower body coloration. The yellow and orange tints become more pronounced in a westward direction in the redband trout native to the Oregon desert basins and reach their greatest intensity in the California golden trout isolated in the southern extreme (headwaters Kern R. drainage) of the Sacramento basin. Columbia basin redband trout typically average about 40 pyloric caeca (vs. 50-60 in coastal rainbow trout), 30-32 scales above the lateral line and 140-150 scales in the lateral series (vs. 26-29 and 120-140).

Many electrophoretic studies have been devoted to rainbow and redband trout. It is apparent that the evolutionary divergence of the enzyme systems has not kept up with morphological divergence. No absolute differences have been found between Columbia River recband trout and coastal rainbow trout. The major distinction is in a form of the lactate dehydrogenase enzyme (LDH-4). Generally, most populations of rainbow and redband trout have two alleles for the gene locus coding for LDH-4, which for simplicity I will designate as allele A and allele B. Most Columbia basin redband trout populations are characterized by a high proportion (70-90%) of allele A and a low frequency of allele B. The reverse is true for most coastal rainbow trout. The problem is that natural variation in these allelic frequencies occurs in different populations and there is no way to know if a divergence from the norm is a natural event or due to hybridization. For example, several samples of redband trout from the Owyhee and Snake river drainages where electrophoretically examined for the BLM Boise District by Wishard et al. (1980).

A dendogram based on pairings of most similar genetic identity scores (quantification of the proportionally shared alleles between samples) from the electrophoretic data does group all redband trout together, separate from hatchery rainbow trout (= coastal rainbow trout genotype). However, if a matrix of the genetic similarities between all samples are examined it is seen that the population of redband trout of Little Jacks Creek is "most closely related" to hatchery rainbow trout than they are to the redband trout of Duncan Creek (tributary to Big Jacks Creek), only a few miles away (genetic identity between Little Jacks Creek and hatchery rainbows = .987 and between Little Jacks and Duncan Creeks = .977). Although the data are explicitly quantitative, the genetic identity scores do not represent true genetic identities in this case because virtually all the similarities and differences are depencent upon the frequencies of the LDH-4 A and B alleles (that is, a single gene locus, of more than 100,000 loci making up the genome, played the dominant role in determining genetic relationships in this study).

The redband trout of Little Jacks Creek had only 29% of the A (redband) allele and 71% of the B (coastal rainbow) allele. Is the high proportion of the "coastal rainbow trout" allele in Little Jacks Creek trout due to hybridization with hatchery rainbow or is it natural variation, perhaps from genetic drift, in a small, isolated population? This question can not be answered.

The foregoing discussion emphasizes the point that absolute purity of any redband trout population can not be verified with certainty. There is no "technological fix" that can resolve the problem. However, for practical purposes, trout that phenotypically and morphologically are similar to the original native trout of a particular drainage should be recognized and managed as the native trout. Although the taxonomic characters of the redband trout native to the Malheur drainage can not be known for certain because no museum collections made before introductions occurred exist, the relative uniformity of the taxonomic characters of middle Columbia River basin redband trout provides an adequate basis for comparisons and evaluation of the 1982 collections.

Table 1 lists the collections and summarizes selected taxonomic characters.

<u>Cottonwood Creek Drainage</u>. Cottonwood Creek drains to the Malheur River from the South. It can be assumed that originally all native trout populations of the Cottonwood Creek drainge would have been relatively homogeneous

Table 1. Selected taxonomic characters of redband trout from Malheur River drainage.

Locality	Gillrakers	Pyloric caeca	Scales, lat. ser., above lat. line	Pelvic fin rays
So. Fk. Squaw Crk. T21S, R40E, Sec. 24 N=14	18-20(18.7)	26-42(32.6)	31-34 (32.5) 139-162(148.4)	9-10 (9.7)
Little Malheur R. T17S, R36E, Sec. 1 N=5	18-20(19.0)	37-48(42.6)	30-33 (31.2) 134-155(142.6)	10
Hog Crk. T20S, R40E N=10	18-20(18.4)	28-43(33.8)	30-34 (31.6) 138-154(145.4)	9-10 (9.5)
Calf Crk. T20S, R38E, Sec. 24 N=11	17-21(18.8)	37-51(43.5)	28-31 (29.6) 126-143(133.8)	9-10 (9.8)
W. Cottonwood Crk. T19S, R39E, Sec. 12 N=11	18-20(18.4)	31-39(35.3)	30-33 (31.8) 138-156(146.1)	9-10 (9.4)
S. Fk. Cottonwood T23S, R39E, SEc. 11 Site 1, N=10	18-21(18.9)	32-44(38.0)	30-34 (31.5) 135-153(143.6)	9-10 (9.5)
S. Fk. Cottonwood T22S, R41E, Sec. 7 Site 2, N-13	18-20(18.9)	35-46(41.8)	30-34 (32.3) 137-152(142.7)	9-11(10.0)
S. Fk. Indian Crk. T18S, R39E, Sec. 10 N=15	18-21(19.5)	27-42(34.7)	32-35 (33.1) 143-158(149.8)	9-10 (9.7)
S. Fk. Indian Crk. T18S, R39E, Sec. 20 N=10	18-20(19.0)	32.41(36.4)	30-35 (32.6) 139-152(145.3)	9-11 (9.9)
2 specimens Pole Crk. T20S, R39E, Sec. 20 N=1	18	37	32 151	9
<u>Owyhee R. Drainage</u> S. Fk. Carter Crk. T26S, R45E, Sec. 36 N=9	17-21(19.8)	31-55(40.1)	31-35 (33.9) 134-152(144.1)	10

Therefore, a comparison of 14 specimens from South Squaw Creek with two samples of 23 specimens from two sites along South Cottonwood Creek, provides for an evaluation of possible hybrid influence in these populations.

Overall, the specimens of all three saples from the Cottonwood Creek drainage are phenotypically similar and the spotting patterns, parr marks, and fin marks are predominantly of the native redband trout. All samples have specimens with variable spotting including numerous small spots below the lateral line--possibly reflecting a hybrid influence from coastal rainbow trout. Further examination reveals that the samples can not be considered homogeneous. Gene flow within the drainage must be inhibited and the samples can be considered as representing three separate populations or genotypes. The two samples from Cottonwood Creek are most similar to each other and the sample from South Squaw Creek the most divergent, and probably the purer representative of the original native genotype. Most middle Columbia basin redband trout average about 40 pyloric caeca. The South Squaw Creek sample averages 32.6 caeca. Several other samples average about 35 caeca suggesting that the original Malheur drainage redband trout differed from other redband populations of the Owyhee and Snake River drainages by a lesser number of pyloric caeca. The Squaw Creek specimens also have low numbers of branchiostegal rays averaging 10.3 on the right side and 10.9 on the left side vs. 11.0 right and 11.4 and 11.6 left for the two South Fork samples. Although not differing in the number of gillrakers, the gillrakers on specimens collected at T23S, R39E, Section 11 from South Cottonwood Creek, are better developed--longer, more acute, with fewer rudimentary rakers. Typically, resident populations of middle Columbia basin redband trout have poorly developed gillrakers; short and blunt with three or four rudimentary knobs at the extremes of the upper and lower arches--typical of predaceous species of fish. The longer, finer, better developed gillrakers are more typical of lacustrine specialized races or anadromous steelhead redband trout. The Oregon Department of Fish and Wildlife currently propagates the redband trout native to Catlow Valley for stocking into eastern Oregon waters. The Catlow Valley redband was subjected to lacustrine specialized selection for several thousand years in a late Pleistocene lake and this led to an increased number of gillrakers, averaging 21 (Behnke 1981). Races of steelhead redband trout were native to the Malheur drainage prior to blocking of runs by dams (Fulton 1970). Thus, there may have been possibilities for a hybrid influence on the resident redband populations in the Malheur drainge from introductions of the Catlow Valley race of redband trout and from native steelhead that became "landlocked" after their runs were blocked.

Overall, however, the three samples from the Cottonwood Creek drainage exhibit little evidence of hybridization. They are good representatives of the native redband trout. The sample from South Squaw Creek is probably the purest of the three samples.

Bully Creek Drainage. Two samples from the South Fork of Indian Creek and a sample from West Cottonwood Creek are from streams in the Bully Creek drainage, which drains to the Malheur River from the north. These three samples are more homogeneous than the Cottonwood Creek drainage samples discussed above. They are relatively uniform in appearance with typical redband type of parr marks, fin markings, and spotting. The taxonomic characters are typical of what I would consider representative of the redband trout native to the Malheur drainage. All three samples exhibit low numbers of pyloric caeca, averaging about 35. All samples have low branchiostegal ray counts, averaging 10.1 and 10.2 on the right side and

10.5 and 10.6 on the left side in the two South Indian Creek samples and 10.7 and 10.8 respectively in the West Cottonwood sample. The major lack of homogeneity is in the number of gillrakers which average 18.4 in the 11 specimens from West Cottonwood Creek, 19.0 in the 10 specimens from upper South Indian Creek, and 19.5 for the 15 specimens from lower South Indian Creek.

These three samples from the Bully Creek drainage appear to represent essentially pure native redband trout.

Hog Creek, T20S, R40E. A small tributary to the Malheur River draining from the north, a few miles upstream from the confluence of the Malheur River with Cottonwood Creek. These 10 specimens possess typical native redband trout taxonomic characters (Table 1). They have a tendency for relatively large spots variably arranged on the body. This spotting variability is the only suggestion of a possible hybrid influence, but it is not strong evidence due to the great range of natural variation in spotting patterns of redband trout. I judge the Hog Creek sample to represent a population of essentially pure native trout. It would be expected that Hog Creek's proximity to the main Malheur River would have continually exposted the native trout to hybridization from stocked rainbow trout. No evidence of hybridization, however, is apparent in the 10 specimens.

Calf Creek, T20S, R38E. Tributary to Malheur River a few miles upstream from Hog Creek. These 11 speciemens exhibit the most evidence of a hybrid influence of all the samples. They have the lowest scale counts, averaging 29.6 above lateral line and 133.8 in the lateral series, and the highest number of pyloric caeca, 37-51 (43.5). Although the Calf Creek population is judged to be the most hybridized of the samples examined, it is still predominantly of the native redband trout genotype. The spotting pattern, although variable, is of relatively large spots sparsely distributed--quite distinct from the typical small, profuse spotting of hatchery rainbow trout. The Calf Creek specimens were in generally poor condition with predominantly empty stomachs. In contrast, the Hog Creek specimens were in excellent condition with considerable fat deposition around the pyloric caeca, indicating a difference in the available food supply in the two neighboring tributaries.

A single specimen was examined from Pole Creek, a small tributary between Hog and Calf creeks. The characteristics of this specimen are typical of native trout, but a larger sample would be necessary to assess the purity of the population.

Little Malheur River, T17S, R36E. These 5 specimens exhibit the most uniform appearance of any of the samples. They have relatively large, sparse spotting, predominantly above the lateral line; the parr marks and fin markings are prominent and typical of native redband trout. This sample represents the most "ideal" redband trout in phenotypic appearance. The sample size is small, but an indication of a slight hybrid influence is suggested by the relatively low lateral series scale count (142.6), the relatively high caeca count (42.6) and by all specimens having 10 pelvic rays (coastal rainbow trout almost invariably have 10 pelvic rays and redband trout have either 9 or 10). If these characters have been influenced by past hybridization, the hybrid influence does not affect phenotypic appearance in this case.

South Fork Carter Creek, T26S, R45E. This sample of 9 specimens is from the Owyhee River drainage and is distinctly set apart from all other samples by the highest number of gillrakers (19.8) and the highest number of branchiostegal rays (11.2 right, 11.6 left). The phenotypic appearance of the specimens is typical of native redband trout. The more extreme values found in this sample may be representative of the trout native to this particular region of the Owyhee drainage and can not be attributed to any hybrid influence without comparative data from several other neighboring populations. I noted that the specimens from South Carter Creek were the plumpest, best fed trout in any of the samples. Their stomachs were packed with food. Two specimens have an unusual deformity of the snout, presenting a "pug nose" appearance and a mouth morphology resembling the whitefish, *Prosopium williamsoni*.

Other Malheur Drainage Collections

I made samples of redband trout from Crane, Calamity, and Swamp Creeks in 1972. The range of character values found in these samples all fall within the ranges of the 1982 samples. In 1980 and 1981, Mr. Robert Smith collected native cutthroat trout and native redband trout from the John Day River drainage, Grant County, Oregon. Mr. Smith proceeded eastward in

Grant County to sample headwater sites in the Malheur drainage in an attempt to find cutthroat trout. Samples from Crane Creek and Big Creek did not find cutthroat trout but a specimen of bull trout, Salvelinus confluentus, was collected in Big Creek (a headwater tributary to the Malheur River). The bull trout is generally rare in the Owyhee drainage and its southernmost distribution in the Columbia basin occurs as a disjunct population in the East Fork Jarbridge River, Nevada. The status of bull trout in the Malheur drainage should be determined. It would be expected to be found only in the coldest tributaries.

Summary of Taxonomic Evaluation

The somewhat surprising conclusions concerning the taxonomic evaluation of the 1982 collections concerns the overwhelming predominance of the native redband trout genotype that is still present in all samples made from diverse segments of the drainage. The amount of variability among samples from the same subdrainage, such as Cottonwood Creek, is greater than I assume occurred under pre-Caucasian influence, and this increased variability is probably due to past hybridization with hatchery rainbow trout. However, no sample approached the character values typical of a hybrid swarm condition. That is, some slight introgression of non-native genes has occurred in some populations but they have successfully resisted genetic swamping. This situation could only result if natural selection highly favored the native genotype. Evidently the harsh environments of warm summer water temperatures in most of the drainage acts as a strong selective force to weed out non-native genes once hybridization has occurred.

Conditions for genetic swamping of native trout genotypes by non-native trout introductions were particularly favorable in the Malheur drainage. Besides the long history of hatchery rainbow trout stocking, the Malheur

River was chemically treated to eradicate all fish and heavily stocked with hatchery rainbow trout as part of the overall Vale rangeland rehabilitation program (Heady and Bartolome 1977). Also the Malheur drainage once maintained large runs of steelhead trout (anadromous redband trout) and Chinook salmon (Fulton 1970). The loss of these anadromous salmonids, especially in relation to spawning and utilization of habitat for parr and smolt rearing, must have caused major readjustments of distribution and habitat utilization by the resident redband trout. It is probable that some remnants of the native steelhead trout became landlocked and hybridized with resident redband trout. All of these factors, along with reservoir construction, flow changes, and environmental degradation would be expected to promote hybridization between native and introduced trout. Thus, I was somewhat surprised to find that the trout inhabiting many different segments of the Malheur drainage in 1982 represent the native genotype with only a slight hybrid influence indicated in a few populations. This leads to the conclusion that the native genotypes are much superior in their survival adaptations than non-native trout. If hatchery rainbow trout are not stocked into the drainage, or stocking is restricted to reservoirs, I would expect that a continual weeding out of the non-native genes will occur in those populations that have been hybridized in the past.

The BLM and the Oregon Department of Fish and Wildlife now realize that the native rainbow (redband) trout of eastern Oregon, has ecological and evolutionary distinctions from hatchery or coastal rainbow trout (Bowers et al. 1979). The Oregon Department of Fish and Wildlife now maintains a brood stock of the native redband trout from Three Mile Creek of the Catlow Valley desiccating basin, for propagation and stocking. The Catlow Valley redband trout can be distinguished from the Malheur drainage redband trout by the number of gillrakers. Most populations of Malheur drainage redband trout exhibit mean and modal values of 18 or 19 rakers. The Catlow Valley native trout have 20-22 gillrakers.

If the BLM and the Oregon Department of Fish and Wildlife were to develop a fisheries management program for the Malheur drainage, I would recommend that a brood stock of native Malheur drainage redband trout be established for stocking within the basin, and for expansion of the genetic resources of redband trout available for use in fisheries management programs.

One particular source I would recommend for a brood stock is the redband trout occurring in Swamp Creek, tributary to the South Fork Malheur. In 1972, I found the Swamp Creek trout existing in intermittent, warm (>80°F), stagnant pools. Yet they were in excellent condition and actively took flies and lures, exhibiting excellent fighting ability and considerable reserve energy in an environment that would have been lethal for most trouts.

DISCUSSION

The status of the redband trout populations in the Malheur drainage will be influenced by the decisions made in regards to land use alternatives currently under consideration by the BLM (Summary proposed land use alternatives, southern and northern Malheur Resource Area, Vale District; issued by BLM Vale District, 1982). In this document, the proposed land use alternatives are put forth as simple cost/benefit tradeoffs. I would point out that a land use decisions resulting in improved trout habitat certainly does not have to cause reduced AUM's for livestock. This fact has been clearly demonstrated by Mr. Bruce Smith, BLM fisheries biologist, Rock Springs District, Wyoming. Mr. Smith realized that stream habitat improvement on BLM lands will be limited to token plots at best if habitat improvement causes a reduction in AUM's and the antagonism of livestock interests. Thus, Mr. Smith set out to demonstrate that revised grazing programs that allow the establishment of riparian vegetation and stable banks, can also greatly increase livestock forage production, mainly by the restoration of wet meadow riparian areas that had been lost in the past from grazing practices causing channel degradation and lowering of the water table. By allowing livestock utilization of the grasses late in the year, and removing the cattle before they impact the woody riparian vegetation, considerably more livestock forage was produced and made available under the revised grazing program and the stream habitat was vastly improved (Smith 1979, 1981, 1982).

Federal agencies indeed have the administrative structure to coordinate and implement meaningful multiple use management such as the formats of MFP's, HMP's, and EIS's. The true efficacy of implementation, however, is not dependent on checking all the appropriate boxes on appropriate dates, but on the expertise and personalities of the interacting personnel that contribute to land use decisions and on the multiple use perspectives of the decision makers. A serious shortcoming I have found concerning adequate consideration of the aquatic environment in BLM land use decisions is that the aquatic biologist staff specialist position is typically a short term position, lacking continuity. A biologist typically works at the District level for two or three years. By the time sufficient experience and expertise is developed for the biologist to make effective contributions to land use decisions, he or she transfers to a new job and their potential effectiveness is lost. This problem of lack of continuity in aquatic programs must be resolved if aquatic environment and fisheries problems are to receive adequate consideration in BLM multiple use alternatives regarding land use.

One suggestion for promoting continuity in aquatic programs conducted by a succession of staff biologists is to keep the main thrust of the program simple, using a "common sense" approach. Frequently, a biologist new on the job is enthusiastic and dedicated, but naive in a belief that all problems can be resolved by "research." If only all kinds of diverse facts are known, something good must result, is a common belief. Thus, a biologist might believe that before recommendations can be made regarding the status of native trout in the waters of a District in relation to further land use alternatives, data must be available on genetic characterization of the populations, their age, growth, food habits, fecundity, etc. Actually, such information may be of academic interest, but contributes nothing to an understanding of predicting the fate of the populations under future environmental conditions of improving or degrading habitat. Typically, the biologist transfers to a new position before the "research" is completed.

Basically, in situations where aspects of fish habitat quality conflicts with livestock grazing, the problem can only be resolved by revised grazing management programs, not by any action of fisheries management. When fish populations are severely limited by poor habitat conditions resulting from lack of riparian vegetation, unstable banks, and accelerated erosion, the only solution is to keep livestock from congregating along the riparian zone either by fencing, herding, or a rotation system specifically designed to accomplish a goal of riparian restoration--there is no longer justification to consider any stream section as a "sacrifice zone" (Behnke 1978, 1979b, 1980; Behnke and Raleigh 1979).

Streambank stabilization is a prerequisite for structural devices designed for trout habitat ehnancement. With unstable banks, habitat structures may do more harm than good by forcing the stream to widen its channel and further accelerate erosion and loss of habitat. When a tributary stream or river section is characterized by stable banks and controlled erosion, structural measures can be implemented to increase trout abundance. First, a stream should be diagnosed for its potential for increased trout production by habitat improvement measures. To what degree is the present population food limited or habitat limited? Structural devices can be expected to do little to increase invertebrate production, therefore, if a trout population's carrying capacity is limited by its food supply, then habitat structures are not likely to increase trout abundance or biomass. Habitat quality can be considered for four life history aspects--spawning, rearing for first and second year of life, adult and overwinter, and plans can be developed to optimize each of these habitat types.

Good general advice regarding trout habitat problems and conflicting land uses, specific to redband trout of eastern Oregon, is given in Bowers et al. (1979). Another valuable source of pertinent information is found in the proceedings of the forum on grazing and riparian-stream ecosystems (Cope 1979). The most up-to-date compendium on what might be called the "state of the arts" in the treatment of trout habitat problems, is a package of papers compiled for a habitat symposium sponsored by the Western Division of the American Fisheries Society, held in Jackson, Wyoming, September 1982.

An excellent source of information basic for an understanding of the hydrodynamics of major stream habitat alteration, based on engineering and fluvial hydrology principles, is the publication by Barton and Cron (1979).

I would again emphasize, however, that stable streambanks and reversing trends of accelerated erosion in a watershed are basic to the success of any habitat improvement project. Because the improvement of watersheds and riparian vegetation conditions are the domain of range management, not fisheries management, the success of fishery improvement projects are dependent on the kind and degree of interaction between biologists and range managers.

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REPORT ON THREE SAMPLES OF BULL TROUT, *SALVELINUS CONFLUENTUS*, FROM GLACIER NATIONAL PARK

Robert J. Behnke

February, 1983

ABSTRACT

Detailed examination of 100 specimens of Isabel Lake, Whale Creek (Columbia River basin), and Cracker Lake (South Saskatchewan River basin), revealed no indication of a hybrid influence with eastern brook trout. Counts of mandibular pores and branchiostegal rays were lower than reported in the literature for the species.

INTRODUCTION

The bull trout, *Salvelinus confluentus*, and the Dolly Varden, *S. malma*, were long confused with each other. Cavender (1978) presented convincing evidence that the bull trout should be recognized as a separate species and selected the name *S. confluentus* Suckley 1858 as the most correct binomial. He assigned the common name of Dolly Varden to *S. malma* and chose the name bull trout for *S. confluentus* because this name has had long usage particularly in the upper Columbia River basin in reference to *S. confluentus*.

Cavender's well documented revision of western *Salvelinus* has been accepted by the American Fisheries Society's Committee on Common and Scientific Names and by the International Game Fish Association which transformed the world record "Dolly Varden" to the world record bull trout. Because it is a widely known sport fish, some state fish and game agencies have been reluctant to endorse the recognition of bull trout. For example, in a letter from the Idaho Fish and Game Commission to the American Fisheries Society's Committee on Names of Fishes (October 5, 1981), the Idaho Commission registered a protest against the change in status of the native char of Idaho from Dolly Varden, *Salvelinus malma*, to bull trout, *S. confluentus*. They questioned the validity of Cavender's publication (without presenting any evidence to challenge Cavender's conclusions) and stated: "Our laws, regulations, record books and anglers all use the name (Dolly Varden). Your decision voids our laws and confuses our anglers." Actually the name bull trout was in wide usage in the upper Columbia River basin of Idaho and Montana before the name Dolly Varden became popular. The name bull trout is a good descriptive term for *S. confluentus*, especially for large specimens with a broad, flat head and blunt snout which presents a bull-like appearance.

In its letter of protest the Idaho Commission pointed out that even if the separation of *malma* and *confluentus* as separate species is a valid taxonomic procedure, the common name of Dolly Varden, "belongs to the *Salvelinus* of the interior by right of first use." The Commission is partly correct in this claim. The name Dolly Varden was first applied (earliest reference in literature) to *S. confluentus*, but there is no "rule of priority" governing usage of common names as there is for scientific names.

David Starr Jordan published an account of how the Dolly Varden got its name in the Pacific Monthly issue of April 1906 (and elsewhere). Jordan wrote:

> "In 1878 when the present writer first tried to classify these western trout, a specimen of this *malma* was sent in from the upper Soda Springs on the Sacramento River near the foot of Mt. Shasta. The landlady at Soda Springs said of it: 'Why that is a regular Dolly Varden!' So Professor Baird said to me: 'Why not call it Dolly Varden trout.' And Dolly Varden trout it has remained to this day."

This story has been repeated many times but it is not wholly accurate. The "type locality" of the name Dolly Varden at Soda Springs on the upper Sacramento River (a branch of the headwaters of the Sacramento adjacent to the McCloud River) is correct, but anglers there were calling the native char "Dolly Varden" in 1872. Livingston Stone, writing in the first report of the U.S. Fish Commission (for 1872-73), mentioned that the char he found in the McCloud River and known to the Indians as "wye-dar-deek-it", was called "Dolly Varden" by the anglers around Soda Springs. All known specimens of McCloud River *Salvelinus* are bull trout. Thus, the first recorded use of the name "Dolly Varden" was most probably based on bull trout.

TAXONOMY AND DISTRIBUTION

Salvelinus malma and *S. confluentus* were long confused with each other because these species differ little in the numbers of vertebrae, gillrakers, and pyloric caeca -- the main characters used in *Salvelinus* taxonomy. Major specific distinctions are found in numbers of branchiostegal rays and mandibular pores, in gillraker structure and head morphology. Greater distinctions are found in life history and ecology between *malma* and *confluentus*. *S. malma* is mainly a coastal species with both resident and anadromous populations from the Columbia River northward around Alaska to the McKenzie River in North America and from the Chukokst Peninsula to Hokkaido in Asia. *S. malma* is divided into northern and southern subspecies, separated by the Alaskan Peninsula. The southern subspecies is a generalized feeder but not highly predaceous (much less predaceous than coastal cutthroat trout when they occur together). The southern form of the Dolly Varden rarely exceeds 6-7 pounds in weight. *S. confluentus* is highly predaceous when living in lakes with forage fish and attains a large size. The official world record from Lake Pend Oreille, Idaho, is 32 pounds (the maximum size of bull trout in Pend Oreille evidently greatly increased after kokanee salmon were introduced and became very abundant). Hart (1973) gives unverified records of 40 pounds, 2 ounces and 35 pounds, 7 ounces for bull trout of Kootenay Lake, B.C. (discussed under Dolly Varden, *S. malma*, by Hart).

The long evolutionary coexistence of cutthroat trout, *Salmo clarki lewisi*, and bull trout in the upper Columbia River basin may explain why *S. c. lewisi* is the least predaceous subspecies of cutthroat trout and the anti-predator defensive behavior I observed in young *S. c. lewisi* in the presence of bull trout in Glacier Park, discussed in previous reports. Comparisons from the literature of diet and behavior of coexisting coastal cutthroat trout, *S. c. clarki*, and Dolly Varden with data concerning *S. c. lewisi* and bull trout clearly illustrates the ecological distinctions between *S. malma* and *S. confluentus*. Bull trout also live in rivers and small headwater streams where without large forage they may be stunted at a small size.

As presently known, *S. confluentus* is native to the entire Columbia River basin (except for the Snake River above Shoshone Falls), but distribution is sporadic and the species becomes rare or absent in southern parts of the basin. It occurs north from the Columbia River in Puget Sound drainages and in some coastal drainages of British Columbia. It is also native to headwater tributaries of the Mackenzie and Yukon rivers, and to the north and south Saskatchewan rivers (Hudson Bay drainage). Why *S. confluentus* did not become established in the upper Missouri drainage, as did so many other species transferring from the upper Columbia and/or the South Saskatchewan basins such as *S. clarki, Pantosteus platyrhynchus, Prosopium williamsoni, Thymallus arcticus*, etc. is a minor mystery.

The southernmost distribution of *S. confluentus* is the McCloud River, California, where it may be extinct. It is also native to a few tributaries in the Upper Klamath Lake basin of Oregon. In 1968, Dr. Donald Seegrist and I collected bull trout from Long Creek of the Klamath basin and also found bull trout x brook trout (*S. fontinalis*) hybrids (discussed and illustrated in Cavendar 1978). As far as known, all species of *Salvelinus* can be hybridized and the hybrid progeny are fertile. Thus, there is concern that brook trout introduced into Glacier National Park may have hybridized with the native bull trout in some waters.

EVALUATION OF SPECIMENS

Table 1 presents data on selected characters for the three Glacier Park samples and gives typical values for *S. confluentus*, *S. malma*, and *S. fontinalis*.

Isabel Lake

Thirty specimens from Isabel Lake ranged in size from 106 to 271 mm total length. Isabel Lake is tributary to the Middle Fork of the Flathead River (Columbia R. basin). There is evidence that 1800 adult brook trout were stocked into Isabel Lake in 1927. The brighter red fin coloration on these specimens suggested a possible hybrid influence from brook trout. The meristic characters of the specimens do not indicate a hybrid influence. The lower fins are bordered by a white edge, but there is no black stripe separating the white edge from the red fin color. Hybrids with a *S. fontinalis* influence would be expected to show the black stripe effect on the lower fins. Also, *fontinalis* hybrids should exhibit mottling on the dorsal and caudal fins. These specimens show no indication of dorsal or caudal fin mottling. All specimens have basibranchial teeth (reduction and some absence of teeth expected in hybrids). This sample has the highest mean value of pyloric caeca, which might be expected from a hybrid influence, but it is only about 10 percent higher than Cavender (1978, 1980) found for a composite sample of *confluentus*. The brighter fin coloradion of these specimens may be due to a high proportion of crustaceans in the diet. If this population was hybridized with brook trout in the past, the brook trout genes were "absorbed" and mainly sorted out through many generations so that any lingering effect on the present population cannot be detected with any degree of authority.

Table 1. Taxonomic characters recorded from *S. confluentus*

Locality	Gill rakers	Pyloric caeca	Basibranchial teeth	Mandibular pores	Pelvic rays	Branchiostegal rays (total)
Isabel L. N = 30	16-19 (16.9)	25-39 (30.3)	2-11 (5.5)	10-16 (13.2)	9.0	22-26 (24.3)
Cracker L. N = 30	15-19 (16.5)	20-33 (25.4)	2-12 (5.9)	11-18 (14.0)	9-10 (9.6)	22-25 (24.1)
Whale Cr. N = 40	15-18 (16.5)	18-33 (26.8)	1-10 (5.0)	12-19 (15.3)	9-10 (9.6)	22-27 (24.5)
Samples from throughout range (Cavender 1978, 1980)	14-20 (16.6)	21-36 (27.8)	typically 3-5	12-19 (15.7)		24-31 (27.4)
Typical Dolly Varden, *S. malma*	typically 17-18	typically 22-30	present	10-15 (12)	typically 9	19-27 (22-24)
Typical brook trout, *S. fontanalis*	typically 17-18	23-24 (38)	absent	12-17 (14.9)	typically 8	20-25 (22.8)

Cracker Lake

Thirty specimens from Cracker Lake ranged from 187 to 282 mm total length. Cracker Lake drains to the Swiftcurrent River of the South Saskatchewan drainage (Hudson Bay). All specimens with basibranchial teeth, no indication of a hybrid influence.

Whale Creek

Forty specimens ranged in size from 106 to 271 mm total length. Whale Creek is tributary to the North Fork of the Flathead River (Columbia basin). All specimens with basibranchial teeth; no indication of a hybrid influence. All three samples are quite similar to each other (more similar than would be expected from samples of three isolated populations of cutthroat trout). As a group they differ from Cavender's characterization of *S. confluentus* by fewer total branchiostegal rays (adding counts of left and right sides) with a modal value of 24 versus 27 as found by Cavender for a composite of *S. confluentus* from throughout its range. The counts of mandibular pores are also lower in the Glacier Park samples (typically 13 to 15 vs. 16) compared to Cavender's counts for *confluentus*. In addition the relative head length (as expressed in percent of the standard length) is much shorter in the three Glacier Park samples in comparison to Cavender's figures (27 and 28% vs. 37%).

A study of intraspecific variation in *S. confluentus* to serve as a basis to assess geographic differentiation and possible recognition of subspecies comparable to my studies with *Salmo clarki* has not been done. Until such data are available it would be premature to speculate on possible unique attributes of the bull trout of Glacier Park. In the future, waters that have introduced populations of *S. fontinalis* should be surveyed to evaluate situations where *confluentus* and *fontinalis* have come into contact. Do they coexist with little or no hybridization or is a hybrid swarm created? What types of environments promote hybridization (most likely small headwater streams typical of the type where brook trout thrive in the Rocky Mountain region).

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These two reports were written for environmental assessment of proposal to divert ca. 1000 cfs flow through Gunnison Tunnel on year-round basis. They typify types of questions expected to be raised when flow regime to be changed.

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[Ca mid 1980s]

POTENTIAL IMPACTS OF REDUCED WINTER FLOWS IN GUNNISON RIVER ON TROUT REPRODUCTION AND GROWTH IN RELATION TO LOWER WATER TEMPERATURES AND ICE FORMATION

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ABSTRACT

Personal experience, personal communications, empirical evidence, and a survey of the literature leads to the conclusions that lower winter flows and lower water temperatures will not negatively impact reproductive success nor growth rates if incubation flows are stable. These conclusions are based on the facts that innate spawning site selection by trout place their eggs in areas protected from ice formation, and trout experience zero winter growth when temperatures descend into the 37-40° F range. Unless some section of the Gunnison River that now exhibits temperatures of 40° F or more would have the temperature reduced below 40° by reduced flow, no reduction in growth is expected -- i.e., winter growth is presently zero when temperatures are less than 40° F.

INTRODUCTION

Since writing my analysis of potential fishery impacts in the Gunnison River from flow depletions (Fisheries impact analysis for year-round flow depletion of 1000 cfs from Gunnison River in Black Canyon area) for INDECO, an expression of concern was made by FERC regarding the impact on incubating brown trout eggs due to possible ice formation in the river. Another concern has been expressed by the Colorado Division of Wildlife regarding the affects of lower winter water temperatures on growth of trout.

The following discussion, based on personal experience, personal communication, and a synthesis of the literature reveals why there is not likely to be a detectable change in survival of incubating eggs nor in trout growth as a result of lower water temperatures due to reduced winter flows.

AREA OF CONCERN AND PROBLEM

About 25 miles of the gold medal section of the Gunnison River (from diversion tunnel to confluence with North Fork) will be subjected to year-round flow depletions up to 1050 cfs if a proposed hydro electric project is constructed. My previous report discussed the reasons why the proposed depletions with a 200 cfs minimum flow would not negatively impact the present trout fishery, and could, in fact, improve the fishery especially if flows were not significantly lowered after spawning (during egg incubation period). The present concerns to be addressed regard the fact that a lesser volume of reservoir water (due to flow diversion through the Gunnison Tunnel) as it travels downstream exposed to ambient air temperature, will cool down more rapidly during winter months than under the present winter flow regime. This more rapid cooling may stimulate ice formation in the river which could possibly destroy or freeze redds, and the lower temperatures could possibly negatively impact growth. Neither of these possibilities is likely to occur because of site selection by spawning trout and the assumption that trout growth during winter (December through February) is essentially zero (or negative) under the present winter flow-temperature regime.

The winter water temperature from Crystal Dam is assumed not to exceed about 39-40°F and with gradual cooling downstream, the water temperature in the river at the confluence with the North Fork should reach 32-33° F during the coldest days of winter under present conditions. This longitudinal river so that

temperature profile can be expected to change with lower flow volume; the cooling effect toward the 32-33° F minimum will occur in a shorter distance than under the present flow regime. For example, in a section of the river that now experiences minimum winter temperatures of about 35-36° F, the minimum temperatures of a reduced flow regime may be 33-34° F. I would point out that a predictive flow-temperature model has been developed that could provide a refined analysis of downstream temperature change due to flow change given known data on flow, temperature, solar radiation, wind, channel morphology, and gradient. This model description is to be published by USFWS as Instream Flow Information Paper 16. The authors are: Theurer, F., K. Voos, and W. J. Miller. Such refinement, however, would be only of academic interest, and not truly relevant to predicting growth changes. This is due to the fact that trout exhibit zero (or negative) winter growth when water temperatures drop below about 40° F and I cannot conceive of a situation where the present flow regime maintains a temperature of 40°+ F during any season which would be significantly lowered below 40° by a flow reduction.

ICE FORMATION

Two types of ice are common in rivers exposed to harsh winter climates. Anchor ice typically forms on the upstream face of boulders protruding above the surface on extremely cold nights ($\leq$ 0° F). A sheet of ice extends below the water surface and may cover the substrate (Hynes 1970). Typically anchor ice melts during the next day before significant build-up occurs, but in many rivers, an annual accumulation of ice can be expected, such as the North Fork of the White River and Saguache Creek in Colorado, and the West Gallatin River, Montana (Brown 1953). Such accumulations may create ice jams and local flooding.

Frazil ice formation occurs when the water in a river is supercooled, typically by passing through a long, shallow riffle exposing much surface area (in relation to volume) to extremely low air temperatures. When supercooled water is seeded with ice crystals, such as from snow or anchor ice, a slush-like frazil ice is created which is carried downstream in the current until mixed with warmer water such as in a deep pool. Thus, winter temperatures in rivers in harsh winter climates remain at or near 32° F until the spring thaw. Trout species have evolved adaptations to cope with winter conditions such as moving into deep pools to overwinter and proper site selection of fall spawning species to place their eggs in areas of upwelling or downwelling where ice formation cannot destroy their redds.

The University of California, Berkeley, maintains a trout research station at Sagehen Creek in the Sierra Mountains. Ten years of basic, year-round research was conducted on the trouts of Segehen Creek (brook, brown, and rainbow) to elucidate environmental factors determining trout abundance. While a graduate student at Berkeley, I participated in these studies. Sagehen Creek is exposed to extremely harsh winter conditions and is characterized by great amounts of anchor ice and frazil ice every winter (Needham and Jones 1959). Dr. Robert Butler, Penn. St. Univ., used Sagehen Creek to produce a film on ice formation in streams. Despite the extreme winter conditions and annual problems with ice formation, the overriding environmental factor determining reproductive success and year-class strength of the trout species in Sagehen Creek proved to be floods (peak discharge), not winter temperature or ice conditions (Seegrist and Gard 1972; Gard and Seegrist 1972).

POTENTIAL IMPACTS ON REPRODUCTION

The key to avoiding overwinter loss of incubating eggs in rivers exposed to harsh climates is for the female trout to select a redd site with proper upwelling or downwelling where ice will not form and where intragravel water temperature may be slightly warmer than the water in the river channel. There is some confusion in the literature concerning the influence of ground water in redd site selection and preferences of brook trout and brown trout (Latta 1969; Hansen 1975). The explanation of this somewhat contradictory data probably can be found in the fact that groundwater influence varies greatly in quantity and quality from one stream to another and in different sections of the same stream -- presence, absence, temperatures, O_2 and CO_2 content. In any event, eons of natural selection has precisely adapted fall spawning species to "know" where to construct a redd to maximize overwinter survival of incubating eggs. This point was nicely demonstrated by Reiser and Wesche (1977) who studied the hydraulic preferences of brook and brown trout spawning in Wyoming streams exposed to severe winter conditions. These authors attempted to duplicate the trout's selection criteria by constructing artificial redds in the Laramie River at sites with "ideal" hydraulic parameters. All of the eggs froze solid in the artificial redds. Survival to hatching was found only when eggs were planted in natural redds previously constructed by female trout.

There may be situations where an unusual combination of circumstances create environments unsuitable for overwinter egg survival. The Wolf River in northern Wisconsin is known to have poor reproduction of brown trout (Andrews 1981). The Wolf River drains a region characteried by severe winters. The stream channel is underlain by bedrock and ground water influence appears to be nil. Brown trout eggs from a hatchery were planted in the Wolf River and experienced 95 to 100% mortality. Temperature recordings showed 122 consecutive days with water temperature at or near (32-33^{o} F) the freezing point. Ice formation is a common phenomenon in the Wolf River but the eggs did not freeze nor were the redds damaged. The major cause of mortality may have been the prolonged near-freezing temperatures and/or the developmental stage of the hatchery eggs when placed into the Wolf River and their sudden exposure to low temperatures. The Wolf River study suggests that some rivers may have conditions unsuitable for overwinter egg survival but such rivers must be extremely rare -- fall spawning salmonid fishes successfully reproduce in rivers within the Arctic Circle where eggs incubate through an extremely long winter. Brook and brown trout successfully reproduce in the headwaters of the Gunnison River and uncountable other streams in Colorado at considerably higher elevation in regions with much colder and longer winters than in the Black Canyon of the Gunnison. Such empirical evidence and the known biology of trout reproduction should allay concern that a reduced winter flow in the Black Canyon of the Gunnison will increase the mortality of incubating eggs from ice or associated effects of lower temperatures resulting from the lower flows. What can be predicted is that lower incubation temperatures will prolong the incubation period and hatching time would occur at a later date (about 550-600 degree day temperature units are accumulated by hatching -- decreasing the temperature by 1-2^{o} F for 90-100 days can be expected to delay hatching by about 7-10 days).

I THINK ITS MORE LIKE 1000 °days (F) (to emergence)

The major factor favoring successful egg incubation, as discussed in my previous report, is stable flows after spawning and during the incubation and hatching period. Thompson (1972) gave a "rule-of-thumb" desired requirement for regulated rivers that incubation flows should not drop below 67% of the spawning flows.

GROWTH CONSIDERATIONS

The concern that lower winter water temperatures will reduce growth rate of trout (both rainbow and brown) appears to be groundless unless there is a segment of the river that maintains temperatures of 40^{o} F or more at some time of the year under the current flow regime that will experience temperature reductions below 40^{o} due to reduced flows. Considering annual seasonal air temperatures and the temperature regime of the water released from Crystal Dam, I cannot conceive that such a situation would occur. As previously discussed, what can be expected is that reduced flow volume will cool the river more rapidly as the water travels downstream during the winter. This cooling effect would be expected to lower the present longitudinal temperature gradient in the 25 mile Black Canyon section characterized by winter lows of $35\text{-}39^{o}$ F by $1\text{-}3^{o}$ F, depending on the distance downstream from Crystal Dam. This slight decline in temperature is not expected to produce a detectable impact on growth because, except for physiological adaptations to extreme cold in lake trout and Arctic char, species of Salmo experience zero growth at temperatures between $37\text{-}40^{o}$ F (synthesis from several references listed pertaining to feeding, growth, and temperature), and this seems to be particularly true in winter when the trout's annual physiological rhythm is programmed for reduced, zero, or negative growth. In laboratory feeding experiments, the regression line predicting zero growth in brook trout is 38.6^{o} F (Haskell 1959), and approximate zero growth for brown trout would occur at about 3.8^{o} C (39^{o} F) according to the data of Elliott (1976). The laboratory feeding experiments are also supported by empirical evidence in nature. I know of no natural trout population (genus Salmo) where any growth has been documented to occur when water temperature is less than 40^{o} F. Although trout will continue to feed in water as cold as 32^{o} F, their digestion rate and food assimilation efficiency are greatly reduced (Elliott 1976).

Thus, I assume that trout growth in the Gunnison River ceases when water temperature drops below 39^{o} F under the present flow regime. The duration of the "no growth" winter period when water temperatures are less than 39^{o} F is not expected to change due to reduced flow because this period should essentially coincide with the period when 39^{o} water is released from Crystal Dam.

CONCLUSIONS

If "reduced winter flows" below dams with subsequent lower winter water temperatures have resulted in an obvious fishery problem (such as ice formation impacting reproduction or reduced growth rate due to lower winter temperature) I would expect that such a phenomenon has been documented and the information made known. In this regard I checked bibliographic or compilation sources such as Alderdice et al. (1977), Osborn and Allman (1976), and Walburg et al. (1981) for documentation. I found none. I then personally communicated with Pat Graham, Montana Fish and Game, and Tom Wesche, Wyoming Water Resource Inst., and asked them if they knew of an example where winter conditions in a regulated river caused egg incubation mortality from ice formation or reduced growth from lower water temperatures. They could not cite any example.

It should be recognized that if trout reproduction becomes more successful in the Gunnison River due to more stable incubation flows, growth rates can be expected to decline because growth is density dependent, especially during the first and second years of life.

The point to be recognized is that, based on empirical evidence, field and laboratory studies on trout physiology and life history, obvious fishery problems that may result from reduced winter flows are not apparent as interpreted from any direct cause-and-effect relationships, especially if incubation flows remain stable after spawning. All kinds of interacting factors are responsible for determining reproductive success, year-class strength, and growth. It is very difficult to isolate a single factor as the valid cause-and-effect action. Dennis Chitty, the reknowned British ecologist, once commented on mortality factors, but the essence of his remarks are applicable to any life history aspect under consideration. Chitty (1967) wrote:

> "The trouble is that animals die for all sorts of reasons (including starvation) and that anyone who works at it hard enough can find a correlation of some sort to support his views, whatever they happen to be."

The moral that can be drawn in relation to environmental assessment and mitigation concerns where priorities are to be placed. In regards to the Gunnison River trout fishery, would a study of winter growth rates under a reduced flow regime (in view that no baseline data exists for comparison) have a more positive influence on the trout fishery than a study of factors determining survival in the first year of life (nursery areas), that may lead to techniques to greatly enhance early life history survival? For example, Mundie and Traber (1983) found that by simply reducing the flow in a regulated side channel nursery area from 15 cfs to 5 cfs, 31 times more steelhead trout smolts were produced.

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[Ca mid 1950s]

FISHERIES IMPACT ANALYSIS FOR YEAR-ROUND FLOW DEPLETION OF 1000 cfs FROM GUNNISON RIVER IN BLACK CANYON AREA

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ABSTRACT

Analysis is made of potential fisheries impact of a year-round diversion of 1000 cfs. The major fishery of concern occurs in the Black Canyon area, extending 26.5 miles from Crystal Dam to the confluence with the North Fork. This fishery is designated as both a wild trout fishery and a gold medal fishery by the Colorado Division of Wildlife. Most emphasis is given to factors influencing spawning, egg incubation, hatching, and emergence of free-swimming fry. Empirical evidence correlating year-class strength to USGS flow records and instream flow analysis performed by the Colorado Division of Wildlife lead to the conclusion that a year-round reduction up to 1000 cfs, with maintenance of a 200 cfs minimum flow, would have a net positive benefit to the trout population because the lower flows would favor optimum habitat conditions for all life history stages for a greater part of the year than does the present flow regime.

INTRODUCTION

West Slope Hydro Partners has made a license application to the Federal Energy Regulatory Commission for construction and operation of hydroelectric generating facilities on an existing irrigation system. The existing irrigation system diverts water through a tunnel from the Gunnison River in the Black Canyon (diversion point approximately 1.5 miles below Crystal Dam) and has been in operation since 1910. Historically, water has been diverted only during the irrigation season, mainly from April through October, with peak diversions occurring in July and August, averaging more than 900 cfs. The proposed hydropower plans call for year-round diversion (except for annual canal maintenance shutdown) up to 1050 cfs. The resulting effect on the Gunnison River below the diversion tunnel to the junction with the Uncompahgre River, the return flow site (approximately 47 miles) would be an average increase in flow depletion of about 100 to 400 cfs from April through September, about 600 cfs during October and about 1000 cfs (theoretical potential to 1050 cfs) from November through March. A minimum flow of at least 200 cfs would be maintained at all times below the diversion point.

The regulation of Gunnison River flows by the Curecanti reservoirs has created one of the most outstanding wild trout fisheries in Colorado by greatly depressing or eliminating the annual peak flood flows releasing a more even flow distribution throughout the year, and reducing summer water temperatures, optimum for trout growth in the Black Canyon area, in comparison to historical conditions (Figures 1 and 2). The quality of the Gunnison River fishery in the Black Canyon, based on catch-per-man-hour of angling, and proportion of large trout in the population (percent of trout more than 14 inches and more than 16 inches) makes this section of the Gunnison River perhaps the finest public trout fishery in Colorado. The section of the Gunnison River from Crystal

Figure 1. Historical flow regimes in Gunnison River of Black Canyon. A. Virgin flow conditions when peak scouring flows of May to early July severely limited trout habitat. Natural reproduction during this period was probably an exceedingly rare event. B. Flow trend after completion of Taylor Reservoir (1937) and diversion through Gunnison tunnel during irrigation season. Peak flows somewhat reduced but summer flows greatly reduced. Sometimes no flow below tunnel during dry years. Trout habitat severely impacted. C. General trend of flow after Curecanti regulation (since October, 1965). More stable year-round flow and cooler summer water temperatures result in dramatic improvement in trout habitat and reproductive success. Brown trout and rainbow trout greatly increase in abundance and growth rate. Gold medal fishery established but in some years a drastic decline in flow after spawning (1982) or a high scouring flow (1983) greatly reduces year-class strength. D. General flow regime projected for year-round diversion of 1000 cfs. Note that in "normal" flow years the year round diversion with 200 cfs minimum flow would maintain flows for optimum habitat in the "E" zone. E. Zone of 200-600 cfs flows for optimum habitat conditions for all life history stages of both brown trout and rainbow trout as quantified in Figures 2, 3, and 4.

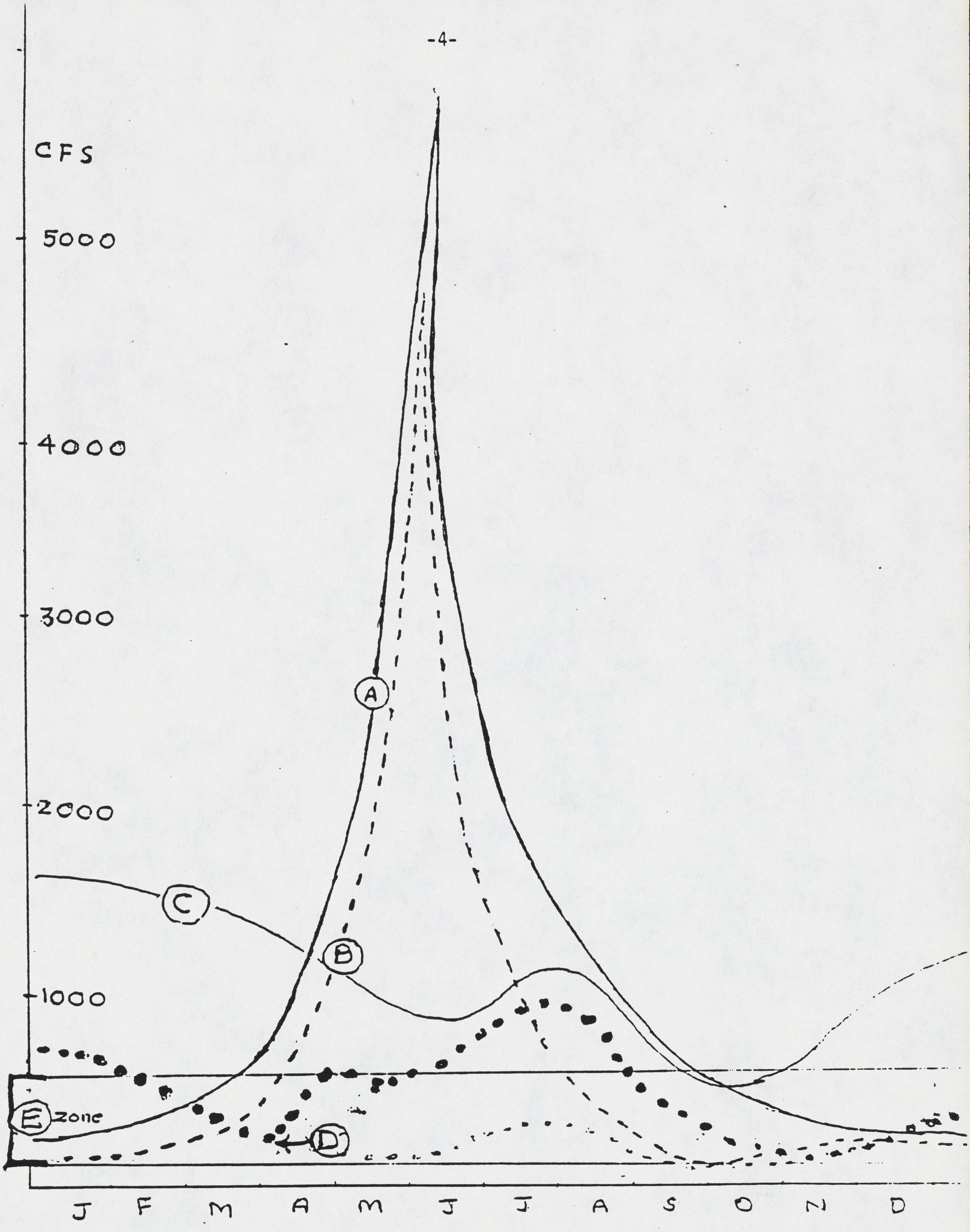
CFS
5000
4000
3000
2000
1000
A
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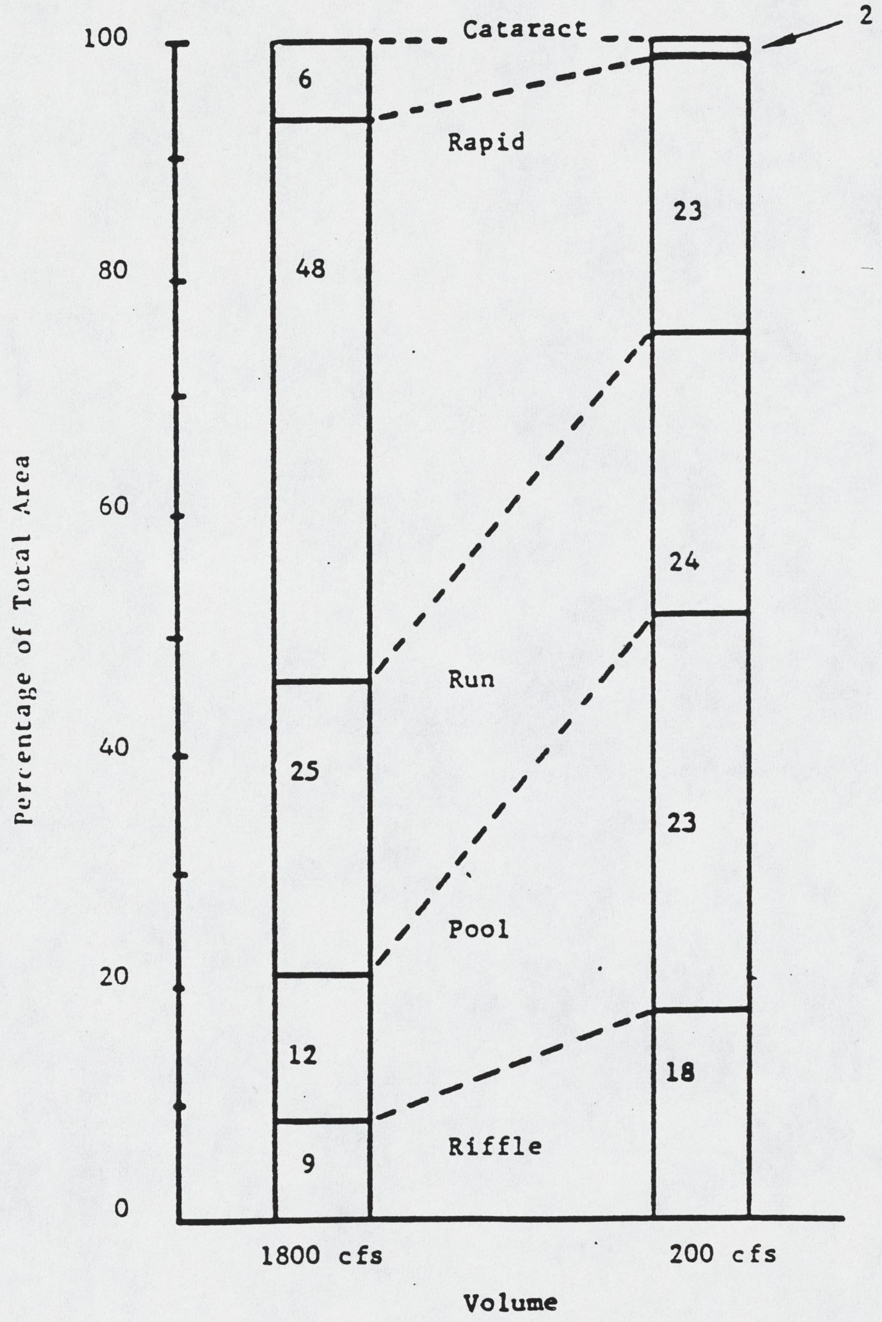
Figure 2. Illustration from Kinnear and Vincent (1967) comparing habitat changes from high flow (1800 cfs) to low flow (200 cfs). Note the increase in optimum trout habitat (pools and riffles) from 21% to 41% and decrease in areas with velocities too great for use by trout (cataracts and rapids) from 54% to 25% when the flow in the Gunnison River in the Black Canyon is reduced from 1800 cfs to 200 cfs. These habitat changes in relation to influence on the brown and rainbow trout populations are quantified in Figures 3 and 4 which present the results of instream flow analysis.


Percentage of Total Area
100
80
60
40
20
0
Cataract
2
6
Rapid
23
48
24
Run
25
23
Pool
12
18
Riffle
9
1800 cfs
200 cfs
Volume

Dam to the confluence with the North Fork (26.5 miles) has been designated as both a gold medal and a wild trout fishery (Nehring and Anderson 1982, 1983).

Thus, there is need to make a critical analysis of the potential impact of increased flow reduction on this fishery to ensure that thorough consideration is given to relate flow changes to potential positive and negative changes in trout habitat for different life history stages -- spawning and egg incubation, hatching and emergence, juvenile, and adult segments of life history.

HISTORICAL REVIEW

The total area of flow depletion impact concerns about 47 miles of the Gunnison River from the diversion tunnel intake to the confluence with the Uncompahgre River, but the area of main concern is the 26.5 mile "gold medal" trout fishery in the Black Canyon from Crystal Dam to the North Fork confluence. Although there is a fair population of large trout in the 9 mile section of the Gunnison River from the North Fork downstream to Austin Bridge (Nehring and Anderson 1982), this section is influenced by flows from the North Fork (long-term average daily flow of 445 cfs, typically ranging between 100 to 2000 cfs), and receives only a fraction of the use that is expended on the gold medal section.

The Black Canyon area was historically a transition zone between the cold, trout waters above the canyon and the warmer waters favoring species of minnows and suckers below the canyon. Environmental changes began in the late nineteenth century. The introduction of non-native species of fish and increased warming and turbidity from return irrigation flows caused the native cutthroat trout to be replaced by the introduced rainbow trout by about 1900 (Wiltzius 1978).

Based on what is known about the indigenous fish fauna of the upper Colorado River basin (Behnke and Benson 1983), the following fish species can be assumed to have existed in the Black Canyon area of the Gunnison River prior to Caucasian man's influence: Colorado River cutthroat trout (Salmo clarki pleuriticus), bluehead sucker (Catostomus discobolus), flannelmouth sucker (C. latipinnis), speckled dace (Rhinichthys osculus), roundtail chub (Gila robusta), and mottled sculpin (Cottus bairdi). During the past 100 years, the following species were introduced and established (occurring in the Gunnison River at some sites between Crystal Dam and the confluence with the Uncompahgre River): Rainbow trout (Salmo gairdneri), brown trout (S. trutta), fathead minnow (Pimephales promelas), red shiner (Notropis lutrensis) sand shiner (N. stramineus), carp (Cyprinus carpio), white sucker (Catostomus commersoni), longnose sucker (C. catostomus), and northern pike (Esox lucius). Historically, the Colorado squawfish (Ptychocheilus lucius), a federal and state listed endangered species occurred in the Gunnison River upstream to the town of Delta (vicinity of Uncompahgre confluence). The state listed endangered razorback sucker (Xyrauchen texanus) also had a similar historic distribution in the Gunnison River. These species are presently rare in the Gunnison River. Valdez et al. (1982) discussed a U.S. Fish and Wildlife Service study in the Gunnison River. Adult squawfish were collected to about 40 miles upstream from the confluence with the Colorado River (about 20 miles downstream of Uncompahgre confluence). One razorback sucker was found in the Gunnison River near Delta in 1981 (Valdez et al. 1982). Wiltzius (1978) described a razorback sucker collected above the Fifth Street bridge in Delta in 1975. There is no evidence (finding of young fish) that the squawfish or razorback sucker reproduce in the Gunnison River.

The federal and state listed endangered bonytail chub (*Gila elegans*) once shared a similar distribution pattern with the squawfish but the bonytail is now considered extinct in the entire upper Colorado River basin (Behnke and Benson 1983). The federal and state listed endangered humpback chub (*Gila cypha*) was never known to occur in the Gunnison River.

As previously mentioned, the first environmental impacts in the upper Gunnison River drainage concerned irrigation which returned warmer and more turbid waters to the river and the introductions of non-native fishes. These impacts caused the disappearance of the native cutthroat trout and its replacement by the non-native rainbow trout. The Gunnison diversion tunnel began operation in 1910 and could divert up to 1000 cfs of the Gunnison River in the Black Canyon. During low water years, essentially the entire flow of the Gunnison River was diverted in late summer and when Gunnison River flows fell below 1000 cfs, irrigation needs could not be met (Wiltzius 1978). The lack of assured irrigation water led to the construction of Taylor Park Reservoir (operational 1937) in the headwaters of the drainage to store water in the winter and spring months for release downstream to the diversion tunnel during the irrigation season. During the 1910-1965 period, the populations of rainbow and brown trout would have been severely limited in the Black

Canyon of the Gunnison by low flows and warm water during late summer and reproduction would have been limited by peak flood flows scouring the canyon. During this period, the Gunnison River above the Black Canyon was a world famous trout fishery. Because of this and the difficulty of access into the Black Canyon, few anglers fished in the canyon (Wiltzius 1978).

The most dramatic environmental change in the Black Canyon section of the Gunnison River occurred from the construction of the Curecanti impoundments (Blue Mesa Reservoir began filling in October, 1965) which regulated the Gunnison River flow by eliminating or greatly reducing the annual peak scouring flow. Also, cold water has been discharged during the summer months, which extends the zone of optimum water temperature for trout through the Black Canyon to the confluence with the North Fork. These environmental changes resulted in greatly increasing the reproductive success of trout, their abundance and growth rate (Kinnear and Vincent 1967, Wiltzius 1978, Nehring and Anderson 1982, 1983). The cooler waters and more uniform flow also affected the non-game fishes. Collections made in the Black Canyon in 1965 were predominated by three species of suckers (white, bluehead, and flannelmouth) which made up 75% of all fishes collected. The longnose sucker, a more coldwater adapted species, was not found at all. In collections made during 1975-1977, the longnose sucker was the most common fish species in the National Monument section of the Black Canyon, making up 43% by numbers of all fishes collected. The other three species of suckers comprised 25% of all fishes in the collections.

Figure 1 illustrates the changing flow regimes during historical periods in the Black Canyon section of the Gunnison River. Figure 2 illustrates why low, stable flows (10% to 20% of average daily virgin flow), increases the amount of optimum trout habitat in the Black Canyon because of the increased area of low velocity habitat.

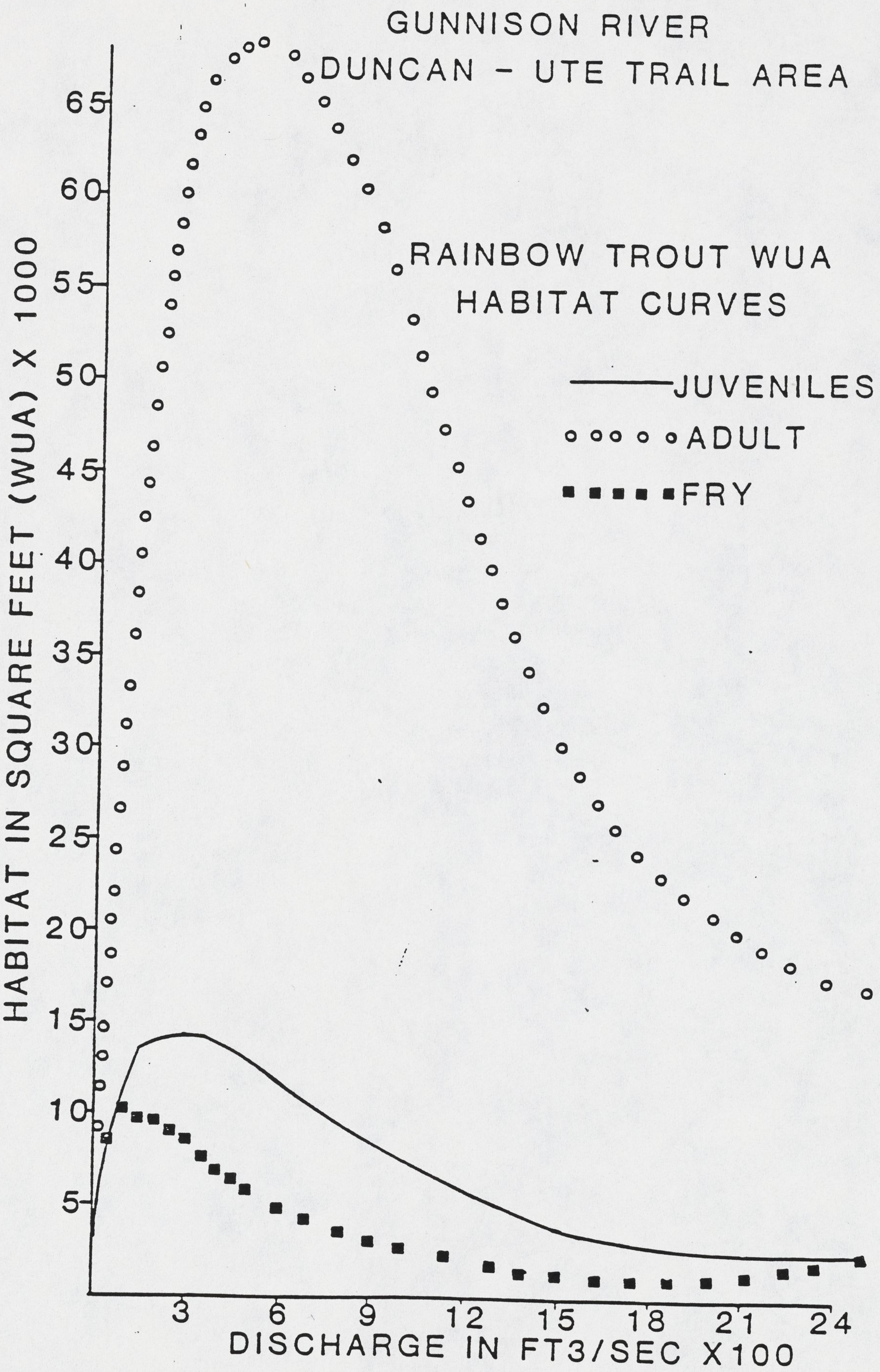
Sufficient information is now available on the biology and life history of the brown trout and rainbow trout, their preferred habitats, and environmental needs of different life history stages in relation to flows in the Black Canyon to make a critical assessment of a potentially optimal flow regime and examine how year-round diversion of 1000 cfs through the Gunnison tunnel might contribute to achieving a more optimal flow regime. The key element for a more optimal flow regime is the avoidance of short-term flow fluctuations (50-100% change in flow volume in one to a few days time, expecially during egg incubation).

IMPACT ASSESSMENT

Impoundments regulating stream flow by a more constant year-round discharge with cold summer releases create some of the most famed trout fishing in the West. For example, the Colorado River below Glen Canyon Dam, the South Platte River below Cheeseman Dam, the Frying Pan River below Ruedi Dam, the "Miracle Mile" of the North Platte River below Seminole Dam, the San Juan River below Navajo Dam, and the Black Canyon of the Gunnison below Crystal Dam. Most "tailwater" fisheries, although providing an excellent environment for adult trout (mainly stocked hatchery trout), have little or no natural reproduction due to erratic flow fluctuations and/or an unsuitable temperature regime (Mullan et al. 1976, Walburg et al. 1981). The Gunnison River in the Black Canyon has had successful natural reproduction by brown and rainbow trout in most years since flow regulation by the Curecanti Project. The designation of the Gunnison River in the Black Canyon as "wild trout" waters by the Colorado Division of Wildlife, means that this fishery must depend on natural reproduction. The most vulnerable period of a trout's life cycle is the embryonic development stage (egg incubation), hatching and emergence of free-swimming young (when they must find protected areas of little or no current velocity), and the first few weeks after emergence. Older, larger trout are highly mobile and can readily retreat to deep pool areas during periods of torrential flow or extremely low flow. Thus, the greatest emphasis for impact analysis is given to a critical evaluation of flows in relation to spawning, egg incubation, and emergence of brown and rainbow trout.

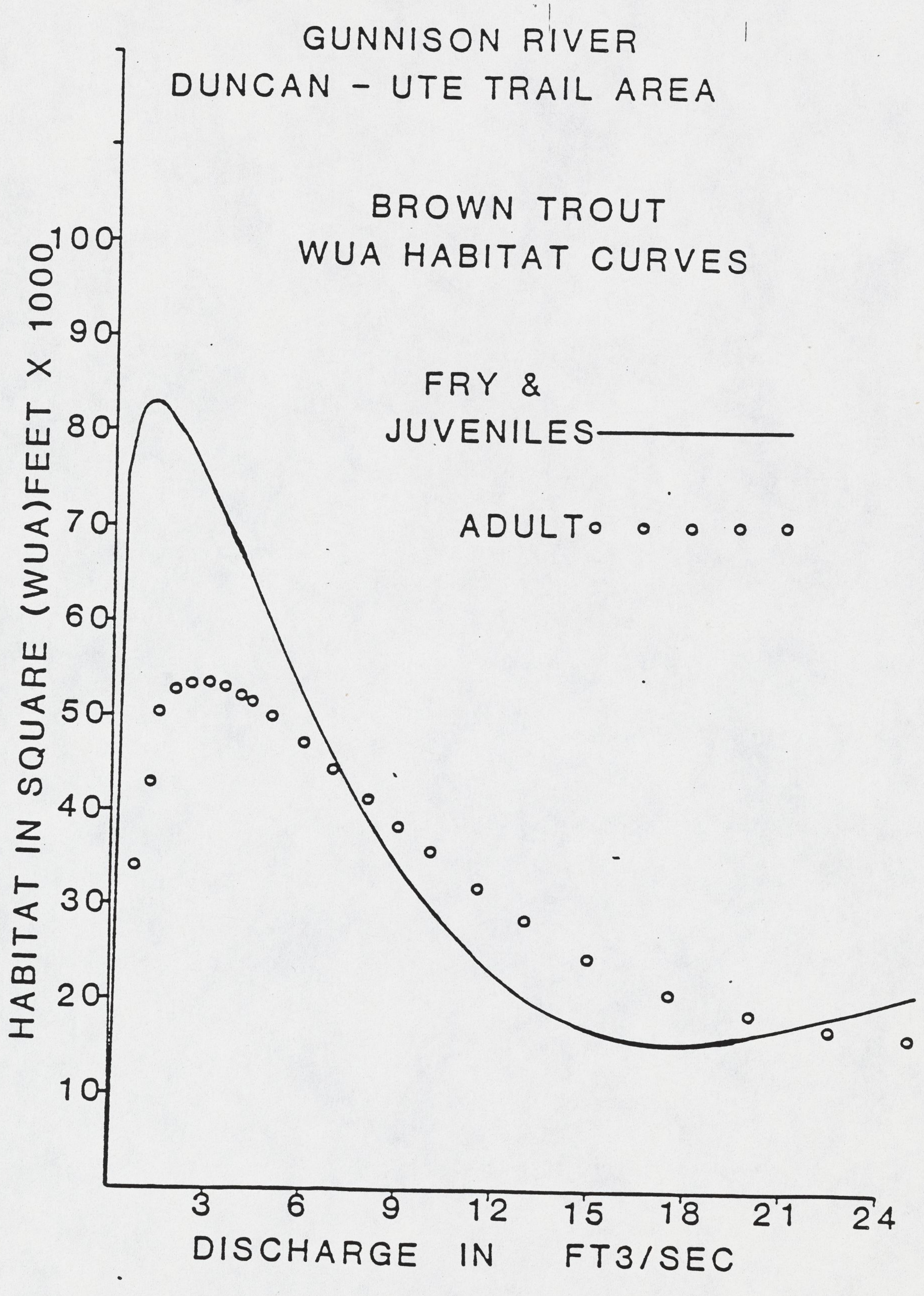
In 1982, the Colorado Division of Wildlife, in cooperation with the Bureau of Reclamation, made a detailed instream flow analysis of the Gunnison River in the Duncan-Ute trail area of the Black Canyon (Nehring and Anderson 1983). The PHABSIM model developed by the Instream Flow Group of the U.S. Fish and Wildlife Service was used which quantified the quantity and quality of habitat available to fry, juvenile, and adult brown and rainbow trout at various flows up to 2500 cfs. For all life history stages for both species, the amount of optimum habitat (weighted useable area: WUA) peaked between flows from about 150 to 600 cfs and rapidly declined at flows exceeding 1000 cfs. Approximately four times more habitat (WUA) was available for all life

Figure 3. From Nehring and Anderson (1983) graphically depicting the changes in trout habitat (weighted useable area = WUA) with changes in flow for various life history stages of rainbow trout. Note optimum habitat conditions for all life history stages occurs at flows from about 150 cfs to 600 cfs. This is due to the increase in amount of low velocity habitat. Figure 3 translates and quantifies the information of Figure 2 into units of useable trout habitat. Year-round diversion of 1000 cfs would maintain flows in the optimum range for a much greater part of the year than under past and present flow regimes.



GUNNISON RIVER
DUNCAN - UTE TRAIL AREA
RAINBOW TROUT WUA
HABITAT CURVES
JUVENILES
ADULT
FRY
HABITAT IN SQUARE FEET (WUA) X 1000
65
60
55
50
45
40
35
30
25
20
15
10
5
3
6
9
12
15
18
21
24
DISCHARGE IN FT3/SEC X100

Figure 4. Same as Figure 3 but for brown trout. Note approximate identical favorable response to 150 cfs - 600 cfs flows.


GUNNISON RIVER
DUNCAN - UTE TRAIL AREA
BROWN TROUT
WUA HABITAT CURVES
FRY &
JUVENILES
ADULT
HABITAT IN SQUARE (WUA)FEET X 1000
100
90
80
70
60
50
40
30
20
10
3
6
9
12
15
18
21
24
DISCHARGE IN FT3/SEC

history stages at 200 cfs in comparison to 2000 cfs according to the analysis (Figures 3 and 4).

There are many techniques in use to predict changes in habitat quality or fish abundance with changes in flow (Wesche and Rechard 1980), and investigators should understand that they are dealing with a great abstraction and simplification of nature in attempting to quantify a multi-dimensional niche of a species by a few components such as depth and velocity. I believe, however, that the instream flow analysis of the Gunnison River by Nehring and Anderson (1983) is accurate. This is due to the unique environment of river channels incised in deep canyons (in contrast to "normal" river channels where low flows of about 20% of the average daily flow recedes the wetted perimeter away from the undercut bank areas and causes the loss of prime habitat).

The greatly increased habitat values illustrated in Figures 3 and 4 at flows from about 150 to 600 cfs is also corroborated in Figure 2 which illustrates a change in types of habitat in the Gunnison River in the Black Canyon when flows change from 1800 cfs to 200 cfs. In changing from 1800 cfs to 200 cfs the amount of river with velocity too high to be used as trout habitat (cataracts and rapids) declines from 54% to 25%, and the amount of prime trout habitat (pools and riffles) increases from 21% to 41%.

Further corroboration was obtained by comparing size-age structure of the trout population in the Black Canyon (Nehring and Anderson 1982, 1983) to note trends in year-class strength (= success of natural reproduction for any single year) and correlate these data with U.S.G.S. flow records for the gaging station below the diversion tunnel. What becomes apparent is that lower than normal flow regimes benefit reproduction, and all life history stages (as can be interpreted from Figrues 1-4), but irregardless of the annual discharge regime, the greatest negative impact on reproduction is rapid fluctuation in flow between spawning and emergence of young. Nehring and Anderson (1983) pointed out the drastic decline in young-of-year brown and rainbow trout in 1982 compared with 1981 (88% and 95% reduction of the two species, respectively), and related the decline in spawning success to highly fluctuating flows during the March through June, 1982, period.

It is instructive to examine the 1981 flows (1981 water year) which produced strong year-classes of both trout species and the 1982 flows which produced extremely weak year-classes in order to better assess potential impacts of future increased diversion with empirical evidence.

Brown trout spawn on declining temperatures. Spawning is typically initiated when maximum daily water temperatures drop below about 48^{o}F. In most years, brown trout spawning will peak during October in the Gunnison River. The eggs incubate overwinter and hatch in late winter (late February, early March) with emergence of free-swimming fry from about late March to early or mid-April. Rainbow trout spawn on rising temperatures with spawning typically initiated when daily maximum water temperatures exceed about 42^{o}F. In most years, peak spawning will occur in April with hatching in mid-May - early June and emergence of free-swimming fry in the early to late June period.

The incubating eggs (buried about six inches in a gravel nest) must have sufficient circulation to maintain high oxygen levels (>5 ppm) and if the nest becomes filled with sediment, water circulation is cut off and the eggs perish. The sediment-free waters discharged from Crystal Dam essentially eliminate the problem of sediment and allow for adequate water circulation in nests at low flows. Trout construct their nests in gravel substrate at stream depths typically between one and two feet. Stage-flow relationships (change in river surface level correlated with change in flow) vary in different sections of a river in relation to gradient and channel configuration. Generally, a change in flow of about 100 cfs would be expected to change the river surface elevation by about one to two inches in a river the size of the Gunnison. Thus, if trout spawned at a high flow of 2000 cfs, at depths of one to two feet, and the flow decreased to 200 cfs during egg incubation, most of the nests would be stranded above the waterline.

Trout and salmon eggs can incubate in a moist environment if temperature and oxygen conditions are suitable (Reiser and White 1981). That is, developing eggs may survive dewatering for some time under certain conditions, but these conditions are improbable in the Gunnison River. For example, consider the development of the 1981 brown trout year-class (brown trout hatched in 1981) in comparison with the 1982 year-class. The 1981 year-class was initiated by spawning in the fall of 1980. Assuming most spawning occurred in October, nests were constructed and eggs began incubation at flows ranging from 556 cfs to 946 cfs. Flows ranged between 1000 to 1270 cfs from November through February. The hatching and emergence period was characterized by gradually declining flows, 1250 cfs on March 1 to 222 cfs by March 31. April, May, and June (and rest of summer months) had low flows between 148 to 624 cfs -- ideal for trout habitat, especially for the fry and juvenile stages (Figures 3, 4). Newly hatched fry cannot cope with high velocity flows (Barry Nehring informed me that the 1983 year-classes of brown and rainbow trout were essentailly lost due to the 1983 flood flows). Thus, the 1981 flow produced strong year-classes for both brown and rainbow trout.

Brown trout spawning during October, 1981, spawned at stable flows from 412 - 695 cfs. On November 3, flow suddenly dropped to 65 cfs (U.S.G.S. records are averaged for a 24-hour period and it is likely that no flow occurred at some time on November 3.). This rapid drop in flows would have stranded and dewatered the eggs in the nests. Although, as mentioned, eggs can withstand dewatering, cold temperatures likely froze the eggs causing high mortality. Flows from 104 to 130 cfs occurred on four other days in November, which probably sealed the fate of the 1982 brown trout year-class. From April 1-15, 1982, rainbow trout initiated spawning at flows between 588 and 714 cfs. Flows dropped to 187 cfs on April 20 and 197 cfs on April 27. Nehring and Anderson (1983) reported personal observation of nests stranded above the waterline and the demise of the 1982 year-class of both rainbow and brown trout.

Thus, it is possible to postulate an ideal flow regime for brown and rainbow trout natural reproduction in the Gunnison. The relationships between spawning, incubation, and hatching-emergence and flows demonstrate that after October (brown trout spawning), flows should not fluctuate drastically. A minimum instantaneous flow of 200 cfs should maintain a water surface level over virtually all spawning sites where spawning occurred at about 400 - 800 cfs flows. A low flow (ca. 200 cfs) is ideal for emergent fry with their inability to cope with high velocity flows.

The Gunnison drainage captured by the Curecanti impoundments is characterized by high variation in annual runoff. Despite the stabilizing effect of regulation, the annual flow regime expressed in total annual volume (acre feet), average monthly, and average daily flow, has exhibited considerable variation since regulation began in October, 1965. The empirical evidence demonstrate that most years of flow regulation have produced adequate to good spawning success for brown and rainbow trout, but rapid declines in flow during incubation or high flood flows during hatching and emergence can obliterate year-classes.

The year-round diversion of 1000 cfs through the diversion tunnel is predicted to have a beneficial impact on the brown and rainbow trout because it will maintain flows in the optimum range for trout habitat (200-600 cfs) for a greater part of the year and will not deplete flows below 200 cfs. The increased diversion would also reduce the rate or proportion of flow change. For example, a present change from 1500 cfs to 200 cfs during a brief period in the November - March period would be only a 500 to 200 cfs reduction with year-round diversion.

The benefits to trout spawning success by utilizing the Gunnison tunnel to divert water beyond the irrigation season was previously recognized by Wiltzius (1978) who suggested that water could be diverted through the tunnel during the normally high flow months from November through March to benefit brown trout spawning and diversion could be increased (above present diversion rate) from April through June to benefit rainbow trout spawning. Further optimization is possible if the timing of the annual "shutdown time" for canal maintenance is scheduled to a period recommended by the Division of Wildlife, and if the Bureau of Reclamation avoids rapid short-term fluctuations in release from Crystal Dam, with special attention given to stable flows during egg incubation-emergence periods.

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Prepared for USFWS
Colo. Field Office

GREENBACK CUTTHROAT TROUT FROM HUNTER'S CRE[EK]

ROCKY MOUNTAIN NATIONAL PARK

Thanks
Dr. Behnke.
-Amy

Robert Behnke
June, 1985

ABSTRACT

Seven specimens collected in Hunter's Creek, tributary to North Fork St. Vrain River in Rocky Mountain National Park are identified as pure greenback cutthroat trout, Salmo clarki stomias. I assume that the population in Hunter's Creek represents St. Vrain River greenback trout, transplanted into Hunter's Creek at an early date before hybridization with rainbow trout occurred in the St. Vrain. The Hunter's Creek population is the fourth known sourceof pure S. c. stomias.

Return to Dr. Behnke

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GREENBACK CUTTHROAT TROUT FROM HUNTER'S CREEK,

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Seven specimens collected in Hunter's Creek, tributary to North Fork St. Vrain River in Rocky Mountain National Park are identified as pure greenback cutthroat trout, *Salmo clarki stomias*. I assume that the population in Hunter's Creek represents St. Vrain River greenback trout, transplanted into Hunter's Creek at an early date before hybridization with rainbow trout occurred in the St. Vrain. The Hunter's Creek population is the fourth known sourceof pure *S. c. stomias*.

Return to Dr. Behnke

IDENTIFICATION

Seven specimens from 178 to 261 mm TL collected June 13, 1985, by Bruce Rosenlund (USFWS, Colo. Field Office) were taxonomically examined and compared with criteria for greenback cutthroat trout, *Salmo clarki stomias*.

The specimens are consistently uniform in spotting pattern and phenotypic appearance. The strikingly pronounced, large spots on the body and red-pink spawning coloration of males indicate the sample was drawn from a pure population of *S. c. stomias*.

Table 1 lists diagnostic meristic characters of the seven specimens and compares them with data from *stomias* populations from Como Creek and the headwaters of the Little South Poudre River.

Table 1. Character analysis.

	Gillrakers	Pyloric caeca	Scales above l.l. and in lat. ser.	Basibranchial teeth
Hunter's Crk. *N* = 7	18-22 (19.9)	27-35 (31.6)	48- 57 (51.6) 187-212(195.7)	7-12 (8.9)
Como Crk. *N* = 18	17-21 (19.0)	24-42 (29.4)	46- 53 (48.4) 174-205(189.3)	1 of 18,no teeth 17 w/ 3-12 (6.0)
Little So. Poudre *N* = 18	19-23 (21.3)	27-50 (35.2)	53- 60 (56.7) 205-236(216.5)	2-17 (11.1)

In all of the diagnostic characters, the Hunter's Creek specimens are intermediate between the Como Creek population and Little South Poudre population. All specimens have nine pelvic fin rays (typically 10 in rainbow trout and hybrids). The uniform spotting pattern, high scale counts, low caecal counts, well developed basibranchial teeth, and number of pelvic fin rays, all agree that no hereditary material from rainbow trout occurs in the Hunter's Creek population. The first gill arch of the specimens possess posterior gillrakers, a character typical of *S. c. stomias* but absent in rainbow trout.

Considering possible sources of non-native (to South Platte drainage) cutthroat trout that may have been stocked into Hunter's Creek -- Colorado River cutthroat, S. c. pleuriticus, and Yellowstone cutthroat, S. c. bouvieri -- the spotting pattern, coloration, caeca, scales and basibranchial teeth counts of Hunter's Creek specimens eliminate Yellowstone cutthroat and the spotting pattern and a mean value of more than 50 scales above the lateral line, rule against pleuriticus as a founder of the population.

Although only seven specimens were examined, the evidence is convincing that Hunter's Creek has a pure populationof S. c. stomias. As such, it becomes only the fourth known source of pure populations of this taxon -- that is, the Hunter's Creek population represents 25% of the known interpopulational genetic diversity of S. c. stomias.

ORIGIN

The topography of the Hunter's Creek watershed, draining through a bench, high above the North Fork St. Vrain River, which isolates upper Hunter's Creek from access to fishes from the St. Vrain, makes in relatively certain that the Hunter's Creek greenback was stocked by man. This situation is similar to the other known greenback populations in the Little South Poudre, Como Creek and Cascade Creek -- all were introduced above impassable falls where they were isolated and protected from non-native trouts.

It is unlikely that hatchery trout were used to stock Hunter's Creek. The only early propagation of greenback trout occurred at the Leadville federal hatchery from 1890 to 1896. The Leadville greenbacks were propagated from spawners from Twin Lakes. The Twin Lakes greenback possessed the lowest scale counts I have found in stomias specimens (42-53 (46.2) above lateral line and 170-202 (186.0) in lateral series, based on 20 specimens collected in 1889 and 1903). From the 1890's to 1940's the predominant cutthroat trout propagated in hatcheries and stocked in Colorado were Colorado River cutthroat, Yellowstone cutthroat and various hybrid mixtures.

Around the turn of the century, irrigation companies constructed many water storage reservoirs in the headwaters of Boulder Creek, St. Vrain and Big Thompson drainages in what is now Rocky Mountain National Park. These head-water areas were barren of fish due to impassable falls. The workmen probably transported trout from the nearest sources into the originally barren waters. Many of these reservoirs still contain greenback x rainbow trout hybrids. The trail to Sandbeach Lake crosses Hunter's Creek. Workmen regularly using this trail probably made a transplant of greenback trout from the North Fork of the St. Vrain to Hunter's Creek. Such a transplant must have occurred prior to hybridization of greenback and rainbow trout in the St. Vrain.

Keplinger Lake is at the headwaters of Hunter's Creek and a barrier falls occurs on Hunter's Creek about one-half mile above the Sandbeach Lake trail crossing. Keplinger Lake is barren of fish and Hunter's Creek above the above-mentioned falls is barren of fish (Bruce Rosenlund, personal communication). Thus, it can be assumed that if Keplinger Lake was ever stocked with non-native trout, they did not become established in Hunter's Creek, as no fish are found above the falls where adequate trout habitat exists.

BIOLOGICAL NOTES

The specimens consist of four females (178, 191, 200, 217 mm TL) and three males (198, 204, 261 mm TL). They are in excellent condition with abundant fat deposits around pyloric caeca, especially in the largest male. The three largest females had not yet spawned. One had released the eggs into the body cavity and would have spawned, probably within a day or two. The smallest female had only immature eggs but two empty egg shells in her body cavity indicated she had spawned. The testes in the two smaller males were less turgid than in the largest male, suggesting partial spawning. Based on this limited sample, it appears that spawning had not yet peaked by June 13, 1985.

Although a detailed parasitological examination was not made, the 191 mm female speciman contained several small nematodes of from 8-10 mm associated with the pyloric caeca and intestine. The nematodes were not encysted and may have exited from the stomach or intestine after the specimen was preserved in formalin.

I examined scale samples but accurate aging was not possible. I "estimate" that most of the specimens were completing their third or fourth year of life, perhaps the fifth year for the largest specimen, but distinct annuli could not be discerned.

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CRITIQUE OF INSTREAM FLOW METHODOLOGIES

Robert J. Behnke

May, 1986

ABSTRACT

During the past 30 years numerous methodologies have been developed to assess instream flow needs of fishes. A basic problem is that no methodology is likely to have success, on a broad scale, to accurately predict changes in abundance or biomass of a species with changes in flow. This is due to limitations for making predictions based on variable biological systems and the failure of any model to accurately take into account all of the subtle interacting factors that determine the well-being of a species in a particular environment in addition to physical habitat limitations. The IFIM of the US Fish and Wildlife Service is a widely used standard model that offers the advantage of comparing habitat changes (expressed as weighted useable area or WUA) for different life history stages of a species throughout an annual cycle. The problem with WUA, however, is: into what biologically meaningful terms can it be translated? It cannot accurately predict changes in numbers or biomass because the IFIM model is faced with the same problems that limit any predictive habitat model.

INTRODUCTION

It has long been the goal for environmental assessment and prediction methodologies to accurately predict quantitative changes in target species as the consequence of environmental changes. For fishes living in rivers to be subjected to a new flow regime the logical assumption has been that a new flow regime will effect changes in fish habitat which, in turn, will effect changes in the population of the target species. In the past, most efforts have been aimed at the determination of minimum flow standards. The assumption is that if flows fall below a designated minimum, an unacceptable decline in the population of the target species (or groups of species) will result. The problem has been that any predicted increase or decrease in the fish population invoking a direct cause-and-effect relationship between flow-habitat-fish has not been quantitatively verified. Quantitative changes in a fish population can not be accurately predicted from changes in the flow regime. Proponents of water projects that propose to change the flow regime in a river typically have precise figures on the value of the water; for example, the value of electrical generating capacity expressed as generation of electricity per cubic-foot-per-second flow. To meet a recommended flow standard, the costs incurred for lost generation capacity can be quantified. Water development proponents demand that the benefits to the fish be similarly quantified from a recommended flow, and this can not be done with any precision.

During the past 30 years, a variety of techniques have been used by state and federal agencies to make flow recommendations. None have been able to demonstrate their ability to quantify changes in aquatic values with changes in flow. During the past 10 years, the U.S. Fish and Wildlife Service has developed a standardized "Instream Flow Incremental Methodology" (IFIM), which allows a habitat model for different life history stages of a species to couple with a hydraulic model to quantify changes in habitat (expressed as weighted useable area [WUA]) with changes in flow by computer simulation. The problem that has become apparent in recent years is that too many people were captivated by the "illusion of technique". They had a naive faith that confused objectivity and quantification with biological reality. Although IFIM can be useful to provide insight into certain limiting factors such as spawning and incubation flows and flows with velocities excessive for the well-being of newly hatched fish, the fact remains that changes in WUA do not provide a basis to accurately predict changes in abundance or biomass of the target species. Changes in flow can be precisely translated into changes in WUA for a target species but WUA can not be accurately translated into changes in numbers or biomass of the target species. The failure to accurately predict changes in a fish population with changes in flow is not so much the failure of the IFIM methodology or any other methodology but rather the limitations for any predictions imposed by natural variation. An understanding of the limitations on prediction (or prophesizing the future) has long been a basic tenet in philosophy and logic and can be roughly expressed as follows: Accurate predictions based on observations (or data) from the present and past are possible only if the system under observation is stable, isolated and highly recurrent -- and such systems are extremely rare in nature. For example, long term and accurate observation and data collection on tidal fluctuations at a point on a seashore would allow accurate predictions of future tides (a tide table) because tides are governed by the constancy of the law of gravity and the solar system (but even with such a stable and recurrent system, unpredictable wind events can alter the accuracy of any predication).

Natural, uncontrollable variation of biological systems such as fish communities in rivers impose severe limitations on any predictive model. Hall and Knight (1981) produced a compendium of documentation on natural variation of populations of salmonid fishes in streams which clearly emphasizes this point. An understanding of the niche concept of a species will also make clear the limitations for accurate predictions of population change associated with any suspected cause. A species "niche" is the total interaction of a species with the biotic and abiotic components of its environment. The current Hutchinsonian niche concept, widely applied in ecology, conceives the niche to be "n" dimensional (unlimited number of factors influencing well-being). The "basic" or fundamental niche and "realized" niche of a species are of different "volume". That is, environmental components such as temperature, living space, predators and competitor species interact to restrict the abundance of a species in a particular environment. Because of this, the basic "niche" is reduced to the realized niche (and the population of the species exists at a lower than maximum level -- this distinction between basic and realized niche relates to the distinction between "carrying capacity" and "standing crop" or biomass of a species and concerns problems of translating WUA (weighted useable area) into biologically meaningful terms to be discussed later). The changes involved in determination of a species realized niche, introduces the concept of "niche shifts". Niche shifts may occur when two or more species interact in such a way to partition the environment and reduce interspecific competition which allows for their coexistence. When niche shifts occur, "preferences" or "suitabilities" of different environmental factors such as depth and velocity can be expected to change.

An understanding of niche theory with its "n" dimensions and "volume" subjected to continual change makes clear that any habitat model based on very few dimensions of the niche (such as depth and velocity) and expressing these dimensions as a static, deterministic, two-dimensional "suitability index" is under severe constraints for accurate prediction of niche changes expected (the new realized niche) from a change in a flow regime, especially if attempts are made to express the predicted changes in terms of abundance and biomass. Such models can be expected to work best for species with a very narrow niche, where complete dependence of the species well-being can be related to a single environmental component, such as might be conceived for a rare species of fish that is only known to occur in beds of watercress, or koala bears known to live only in eucalyptus trees. Such species, however, are rare.

The limitations of ecological models to correctly predict future population changes associated with environmental changes was clearly recognized by one of the early promoters of the use of computer simulation models for environmental assessment (Hollings 1978). Hollings emphasized that the best models can only be a highly condensed abstract of nature, that accurate predictions should not be expected, and to expect the unexpected.

HISTORICAL REVIEW

In former times dams were constructed and rivers regulated in accordance with the purpose of the dam without regard to fish. In those rivers with valuable fisheries some minimum flow was recognized to be essential. Various formulas for minimum flow were used such as the lowest natural flow for seven consecutive days during a 10 year period. The only biological basis for such minimum flows was the assumption that the present fish fauna of the river had survived such low flows in the past (at least for seven days) and they could survive at such flows in the future. In the 1950's the public became more environmentally aware and fisheries values became better documented, especially for anadromous salmon on the Pacific Coast. Many studies were initiated, with mixed success, to attempt to correlate annual or seasonal flow regimes in a specific river or a group of rivers in a geographic area with salmon production.

The earliest attempts at developing quantitative methods to relate flow recommendations to fish habitat concern the transect method whereby a transect is placed across a stream channel to measure depth, velocity and wetted perimeter (area of channel covered by water at different flows). Typically, a section of a stream designated as a "critical riffle" would be selected for the transect measurements. Arbitrary values would be selected for depth, velocity and/or wetted perimeter (for example, six inch depth, one foot-per-second velocity, and/or 70% wetted perimeter) at the critical riffle site which would be achieved with the minimum flow recommendation. Unless a series of transects are made at varying flows to derive empirical data, a formula (Manning's formula) is needed to predict the flow which meets the required depth and velocity. Elements of Manning's formula such as "roughness coefficient" and "slope" make prediction prone to considerable error. Nehring (1979) compared the "R-2" cross section method and the IFG4 method (used with IFIM studies) to check for the error between predicted and actual velocities. For the R-2 cross method 30% of 97 predictions were within 10% of the actual velocities and 7% were in error by 100-500%. For the IFG4 method, 35% of the predicted velocities were within 10% of the actual velocities and 4% were more than 100% in error. Another criticism of the simple transect method is that flow recommendations derived from it can not be readily related to biological reality. That is, if the actual flow falls below the recommended flow by 10% or 20%, or is maintained above the recommended flow by a known amount, how is the fish population affected? What parameters change? by how much? Such questions can not be answered with any confidence.

The assumption inherent in the simple transect method (its methodology) is that if a flow meets certain criteria for depth, velocity and wetted perimeter at the "critical" site, then the stream channel in other sections of the stream will contain sufficient water to maintain certain desirable habitat features. This assumption was given some credibility by Wesche (1973) who found that when flows decreased below about 25% of the average annual flow (=average daily flow), optimum trout habitat under the streambanks was rapidly lost due to declining water levels. Nehring (1979) obtained 18 flow recommendations with the R-2 cross section method which ranged from 15 to 44% of the average daily flow for the streams studied with an overall average of 26.4% of the average flow based on depth and velocity measurements made at multiple transects.

Thus, for a low effort method where significant conflicts over flow recommendations are not anticipated, a transect technique performed by an experienced and knowledgeable biologist would be acceptable and the flow recommendation in relation to the well-being of the target fish species could be expected to "be in the ballpark" but not quantitatively predictive (but no other technique or methodology can claim better predictive power).

For situations where much is at stake in regards to proposed flow changes, it was realized that more sophisticated and defensible techniques were needed (to produce evidence that might better hold up in court). In 1960, the California Department of Fish and Game developed a methodology to assess flows below dams. This methodology assumed that the basic requirements of fish include food, shelter and reproduction and that habitat parameters for food (food producing areas), shelter (resting areas with suitable cover), and reproduction (spawning areas) can be quantified and quantified changes in habitat quality can be related to flow changes. Fisheries biologists of the Pacific Gas and Electric Co. (PGE) further developed and refined the methods and methodology and adapted the model for computer simulation (Waters 1976). The PGE model is the direct antecedent of the present IFIM of the U.S. Fish and Wildlife Service.

Another well-known flow assessment technique is popularly known as the Montana or Tennant method (Tennant 1976). This method requires only USGS flow records for a stream and flow recommendations are based on percent of long term average daily flow for a stream. These range from 10% of ADF for "short term survival" to 60-100% of ADF for "optimum" fishery flows.

Allen Binns, Wyoming Game and Fish Department, developed a habitat assessment methodology for Wyoming trout streams with a quantitative output expressed as the Habitat Quality Index (HQI) (Binns and Eiserman 1979). Binns' HQI is the only widely known model that directly relates habitat variables to fish biomass (only trout). Only two flow parameters -- late summer base flow as percent of ADF, and difference between maximum and minimum flows (least difference = best and greatest difference = worst habitat conditions) -- are included in Binns' habitat model. Techniques would have to be developed to relate changes in the other habitat parameters to changes in flow in a consistent manner before the Binns HQI model could be utilized to predict changes in trout biomass to changes in flow. If attempted, a significant element of subjectivity would be introduced to the habitat assessment and would require a considerable amount of experience and expertise on the part of the biologist to make it work. Thus, I suspect that Dr. Binns might be able to predict changes in trout biomass in Wyoming streams from changes in flow with a fair degree of accuracy (ca. ± 50%), but other biologists in other areas could not duplicate his results.

A comprehensive review of instream flow methods and methodologies was prepared by Wesche and Rechard (1980). For the remainder of my critique I will mainly concentrate on the IFIM of the U.S. Fish and Wildlife Service because this has become the "standard" and often the required method of federal and state agencies. It is important that parties involved in flow determination decisions understand certain concepts and limitations to avoid expensive and meaningless work or misdirected application of a method where it does not apply such as attempting to use WUA for mitigation trade-offs. For example, a stream section to be inundated by a reservoir can be calculated to have a quantified amount of WUA for brown trout and rainbow trout and the project developer might offer to purchase or improve another stream with an equal amount of WUA (attempted by Denver Water Board for Two Forks Project on South Platte River). What must be understood is that WUA values are not equal between different streams (when correlated with fish biomass) and are not interchangeable.

Also, one overwhelming factor may preclude the use of habitat to predict occurrence and well-being of a species. If empirical evidence demonstrates that species A is never found in the presence of species B, then the presence of species B will exclude species A, no matter what a habitat model predicts for the success of species A. Such a situation occurs in the Salt River drainage where the presence of smallmouth bass excludes the occurrence of many native species such as the spike dace, loach minnow, and Gila chub.

My critique is not intended to be a negative criticism of IFIM. I believe most who have been intimately involved with IFIM will agree with the theme of my critique, but perhaps not all of the details. My words of caution are intended for those involved in negotiations and discussions of impact analysis with administrators of state and federal agencies who may have only a rudimentary and naive understanding of a particular situation causing them to invoke a reflect response demanding "baseline study", "IFIM study", etc. when such studies may be meaningless to resolve a particular problem.

DETAILS OF PROBLEMS FOR PREDICTION

The great advantage of IFIM over other methodologies is its ability to quantitatively display changes in WUA (assumed to represent the habitat quality of target species) with changes in flow, which can be plotted on an actual or proposed annual hydrograph. This allows negotiators to discuss trade-offs and mitigation for proposed projects in a quantitative manner. As such, IFIM was quickly embraced by federal agencies as a long-sought saviour to their problem of quantification of gains or losses to the biological system from flow changes. For many, the hard question of what does WUA relate to, was ignored or not even considered. When the question was asked and tested, the results were a disillusionment to many and a confirmation to those who were aware of the limitations of prediction discussed above.

The U.S. Fish and Wildlife Service contracted for validation studies of IFIM and its habitat suitability index curves (HSI), used for both IFIM and HEP (Habitat Evaluation Procedures). The Electric Power Research Institute (EPRI) contracted for its own evaluation of instream flow methodologies (Morhardt 1985), and the U.S. Department of Energy contracted with the Oak Ridge National Laboratory to test habitat evaluation models in southern Appalachian trout streams (Loar 1984). The results from attempting to correlate WUA or HSI values with fish numbers or biomass have not been encouraging. Numerous papers documenting the failure of HSI to correlate with abundance or biomass of the target species are contained in a volume edited by Terrell (1984). Layher and Maughan (1985) concluded that "broad niche" species can not be adequately represented by a simple suitability curve, that reliable habitat models may only be possible for small, homogeneous areas, and that HSI should be used only for planning and not decision making.

Several studies documented that habitat "suitability" or "preference" changes in a species in relation to daily and seasonal differences, the presence or absence of other species and other complex factors (Larimore and Garrels 1985, Li and Schreck 1984, Loar 1984, Sheppard and Johnson 1985, Moyle and Baltz 1985). That is, WUA computed on depth, velocity and cover for a species would vary when recorded at different times and/or in different streams with different fish communities. I became aware of this problem a few years ago when I was advising the U.S. Justice Department regarding a claim for instream flow on the Red River, New Mexico. The analysis showed WUA for rainbow trout was much greater than was the WUA for brown trout, yet brown trout were completely dominant over rainbow trout in the river. I realized that if the case went to court, new suitability index curves would have to be made specifically for the brown and rainbow trout of the Red River, which would agree with their relative populations in the stream. The U.S. Fish and Wildlife Service species habitat models now contain the recommendation that suitability curves should be based on site-specific studies. The problem here is that studies to obtain detailed data on a sufficient number of individuals of a species to compute original suitability curves for such parameters as depth, velocity, substrate, and cover, is time consuming and expensive.

A recent paper by Mathur et al. (1985) contains a strongly negative attack on IFIM (but it should be recognized that these authors had an "ax to grind"). In any event, it's obvious that the "bloom is off the rose" of IFIM (and HEP), but the critiques and criticisms of both the biological and mathematical-statistical bases of IFIM should have a salubrious effect in that certain problems are brought into focus and sharper, more insightful thinking should be stimulated to critically examine ways to improve predictive accuracy and to better understand the limitations for predictive accuracy of any habitat model.

FACTORS CONTROLLING FISH ABUNDANCE

One of the arguments used by defenders of IFIM is that WUA is directly related to the "carrying capacity" for a particular species and attempts to relate WUA to biomass is prone to error because a population is rarely at carrying capacity. A problem here is the definition of the elusive concept of carrying capacity and how it can be determined -- a problem without a universal solution. Putting the problem of the determination of carrying capacity aside, the associated problem is that the basic assumption of IFIM is that carrying capacity is completely controlled by physical habitat which, in turn, can be accurately represented in a model by measurements of depth, velocity, cover and substrate (the temperature factor is at least recognized and flow-temperature model is available to use with IFIM Physical Habitat Simulation System [PHABSIM]).

In high gradient streams of the Rocky Mountain region, characterized by great variation in annual flow, I would agree that most fish populations are more "habitat limited" rather than "food limited", although limitations resulting from interactions with other species may be a more powerful limitation than is the physical habitat or potential food supply. The assumption that carrying capacity of a fish population in a stream is always limited by physical habitat is certainly not universally true. A list of factors affecting fish population abundance in streams can be stated as follows.

Physical Factors

1. Flow regime
2. Habitat quality
3. Water quality (temperature, pH, turbidity, etc.)

Biological Factors

1. Food abundance and availability
2. Predation
3. Competition and all interspecific interactions
4. Movement, migration

Of these factors, IFIM can only attempt to relate "habitat quality" to "flow regime", and is limited to very few indicators of "habitat quality." Thus, again one can understand the limitations for a good correlation between WUA and species abundance or biomass.

In relation to abundance governed by food supply, I would cite the example of the Au Sable River, Michigan, the subject of a recent report I prepared for evidence in court (Behnke 1986). The Au Sable River is a famous trout stream but the fishery (mainly for brown trout) has declined from a standing crop (biomass) of about 150 pounds per acre to about 100 pounds per acre during the past 15 years. The stream is fed by ground water and maintains a relatively constant year-round flow (flow close to 100% of ADF year-round). No change in flow has occurred in historical times. The only change in habitat consisted of $250,000 of stream improvement structures placed in a nine mile section of the river in the 1970's to provide more "resting" habitat in an attempt to reverse the downward trend (it failed). The only change known to have occurred in the Au Sable River concerns water quality. A large state fish hatchery ceased operation and its nutrient-rich effluent into the river ceased, and the sewage effluent of the town of Grayling was diverted away from the river. This pollution abatement reduced nitrate-nitrogen levels by 70% in the Au Sable, which in turn decreased primary production and invertebrate production (food supply to the trout). There was no significant change in trout abundance except for fewer age IV and V fish, but the growth rate was significantly reduced so that the size of trout of any particular age was much less in comparison to the period of nutrient enrichment, and this resulted in the decrease in population biomass.

A more detailed and quantified example of increased trout production in a stream as a result of nutrient enrichment concerns Berry Creek, Oregon and the study of Warren et al. (1964) who enriched a test section of Berry Creek with sugar (sucrose). The sucrose produced a proliferation of bacterial slime which was fed upon by aquatic insects which greatly increased in abundance, increasing in turn, the food supply to the trout population. The intake of food by the trout population was doubled, but production of the trout population increased by more than seven-fold. This great increase in production is explained by an understanding of maintenance rations vs. growth rations. When a fish population is at or near the limits of its available food supply, most food is utilized for body maintenance (maintaining the status quo) and little is available for growth (production in the population is low). Once maintenance requirements are met, all additional food goes into growth. Thus, by only doubling the total food intake, production of the trout population increased more than seven-fold because the additional food produced from stream enrichment went into growth.

The lessons learned from the examples of the Au Sable River and from Berry Creek makes it clear that food cannot be ignored as a factor controlling population biomass. The physical habitat did not change in Berry Creek (WUA constant) and was improved in the Au Sable River by creating lower velocity areas with cover in the channel (WUA would have increased during time of biomass decline).

The studies of Hawkins et al. (1983) and Murphy et al. (1981) on small Oregon streams in the Cascade Mountains demonstrated a great increase in primary production (aquatic plant production), which led to increases in aquatic insect production, and increase in biomass of trout and salamanders after clear-cutting of the forest. This cause-and-effect relationship in this example is sunlight which drives primary production and which was essentially blocked from the streams by a complete canopy of vegetation before the clear-cuts. The dynamics of the energy flow in these streams, eventually producing food for the fish, changed from mainly allochthonous input (from surrounding terrestrial environment) to predominantly autochthonous (sunlight stimulating primary production in the stream) because of the removal of trees.

All of the above examples demonstrate that physical habitat, especially when interpreted only as depth and velocity, is only one of many controlling factors of fish biomass in a stream. They also reveal the range of variables and complexities that would have to be considered in an attempt to develop a stream habitat model to predict changes in biomass resulting from any environmental change.

There is undeniable evidence that physical habitat does exert a strong controlling factor on the abundance-biomass of a fish population. A section of Lawrence Creek, Wisconsin, was structurally changed to convert a predominantly wide, shallow, high velocity riffle area without cover in to a more narrow, deeper, low velocity area with overhanging bank cover (Hunt 1976). The increase in biomass of brook trout in this section after improvement approximately doubled, but the biomass of adult trout increased about four fold. This was due to the conversion of "juvenile" habitat into "adult" habitat. At the same time, it can be assumed that total invertebrate production decreased by the conversion of riffles into pools and decrease in absolute channel area after the improvement, but the improvement in habitat (deeper, slower water with cover) allowed more adult trout to more effectively utilize the food supply that was formerly underutilized. All similar types of stream improvement projects operate under the assumption that the fish population is habitat limited (at least within the section of the stream to be improved) and by creating deeper, low velocity areas with cover in areas lacking such habitat, the population will increase. It can not be assumed that the food supply will increase to any extent from fish habitat improvement, only that fish will now be able to better utilize the food that was not previously available to them. When habitat improvement has been effective for increasing fish biomass, then the above assumptions are proven correct. Where habitat improvement has not been effective, such as in the Au Sable River, then other factors, such as food is limiting (or the habitat structures were poorly designed or placed in wrong sites).

Empirical evidence relating habitat to flow and ultimately fish biomass concerns spring creeks and regulated rivers. Spring creeks typically have stable year-round flows, are low gradient, nutrient-rich, relatively deep and with macrophyte vegetation. Besides insect life, most such streams have an abundance of crustaceans (typically gammarid amphipods). The habitats of spring creeks may be more comparable to a lacustrine (lake) environment than to a high gradient, rocky, highly fluctuating stream. Spring creeks (English chalk streams, Sand Creek, Wyoming -- The stream that provided the extreme biomass point for Binns' HQI model, and gave the model such good correlation between HQI and biomass -- and some noted spring creeks in Montana) have long been recognized as the ultimate in trout streams -- biomass of 500 to 700 pounds per acre or more and rapid growth rate of the trout. In spring creeks, virtually the whole channel, in relation to depth, velocity, and cover, would be rated at maximum values. Thus, the optimum habitat allows the trout population to expand to the limits of its food supply (to attain its "carrying capacity"). When dams regulate rivers by eliminating the peak flood flows and elevate the late summer base flow above natural levels, the resulting flow regime becomes somewhat similar to a spring creek and the trout population responds in a similar manner to the improved habitat conditions (lower velocity during run-off, greater depth during late summer). Some of the most famous trout fisheries in the West are the result of river regulation -- South Platte River, Frying Pan River, Gunnison River, Colorado; "Miracle Mile" of North Platte River and Bighorn River, Wyoming, and many other examples.

Other instructive examples of changes in fish populations correlated with habitat changes concerns habitat protection measures such as fencing livestock away from streambanks on overgrazed watersheds. Results have often been dramatic with several fold increases in trout populations after riparian vegetation is restored to the banks, the stream channel stabilizes, becomes more narrow and deeper with overhanging cover (Behnke 1979). Essentially, the natural changes in habitat improvement from livestock protection is similar to the artificial improvement of Lawrence Creek, Wisconsin, discussed above. A wide, shallow, high velocity stream without cover is converted into a more narrow channel with slower, deeper water with cover. There has not been a change in invertebrate production (actually invertebrates may decrease) or in flow, but only in physical habitat that results in a large increase in the fish population.

It would be useful if before and after studies of streams subjected to natural or artificial habitat improvement and habitat changes due to flow changes from river regulation were conducted to develop and test habitat models and the accuracy of their predictions on fish population change. A problem I foresee for any complex habitat model is that simple factors such as depth and velocity can be objectively recorded by anyone following a set of rules and using standard equipment, but factors such as "cover" is subjective and different workers may arrive at very different "cover" values for the same stream. Also when "cover" is the result of complex and interacting factors, its simple compartmentalizing into standardized units for modelling may result in large errors when applied in different areas. For example, Loar (1984) found brown and rainbow trout to be negatively correlated with "cover" as measured by IFIM. I do not believe that the trout deliberately avoided cover in the Appalachian streams investigated, only that the trout's concept of "cover" differed from the IFIM concept.

Why WUA influenced by "compartmentalized" cover ratings are not interchangeable between streams or even between different sections of the same stream can be understood by comparing a holistic interpretation of "cover" and a reductionist breakdown of "cover" into measurable units. The mind of an experienced angler makes a holistic interpretation of a stream in arriving at a decision to where to concentrate his efforts -- the sites to cast bait or lures that provide the most favorable opportunity to catch larger adult trout. The largest trout select the areas of the stream with the greatest "volume" and complex cover, such as a deep hole beneath a bank, upturned tree roots, below large boulders or log jams. Those large volume areas might be considered as "first class accommodations". Other areas of the stream channels with certain parameters of depth, velocity and cover might be given an equal WUA rating by quantitative measurements and following the rules of IFIM, but to adult trout they are "second class" living accommodations, which will be used by smaller, subdominant trout. In such situations, biomass per unit area of stream channel may greatly vary between first and second class habitats even though they have equal WUA.

A major factor controlling the abundance or presence or absence of a species that can not be assessed by any present habitat suitability index for IFIM (or HEP) is the presence of other species resulting in predation and/or interspecific competition. In a typical river drainage in the West, the smaller headwater streams can be expected to be inhabited by brook trout or sometimes native cutthroat trout.

The larger (5-6-7 order streams) stream channels in the drainage will typically have brown and/or rainbow trout. This distribution pattern of trout species within a drainage is repeated over and over throughout the West. Thus, no matter how much habitat for cutthroat trout or brook trout might be quantified in a large stream, these species cannot establish viable populations in competition with brown trout or rainbow trout in a large stream environment. There are some exceptions to the rule, and these exceptions provide an opportunity to gain new, useful knowledge. In the Rio Grande drainage of Colorado, the native Rio Grande sucker has been virtually completely replaced by the introduced white sucker. In the Salt and Gila river drainages of Arizona and New Mexico, the native Gila chub, loach minnow and spike dace, do not occur (or do not maintain viable populations) in the presence of smallmouth bass. For any attempt to construct predictive habitat models for species such as the Rio Grande sucker, Gila chub, loach minnow, and spike dace, the "exclusionary principle" regarding the presence of certain non-native fish species must be recognized to have overriding power over "habitat" for predicting abundance or presence or absence.

In some instances, certain habitat components may interact in complex ways to influence fish abundance and this will interfere with predictions based on neatly compartmentalized models. IFIM assumes that fish respond to the habitat components as independent variables. If the fish utilize depth with dependence on velocity, then the assumption is violated and errors introduced for computation of weighted useable area (WUA). The dependency between depth and velocity in relation to fish use of a stream section may be relatively common in "run" areas of a river channel where large boulders that can serve as protective cover are absent. In such areas, at low flow, depth may be adequate to optimum for a species but due to the lack of cover, the species may make little use of the area because of predator avoidance (particularly if fish eating birds and mammals are common). During periods of higher flow, higher velocities create turbulent surface flow, reflecting and refracting light to such an extent that fish cannot be seen from above the surface. At such times, the fish will utilize the area with suitable depths because of the turbulence created by higher velocities (a dependency between depth and velocity influencing "useable area"). This is just one example of problems faced when attempting to develop a simple predictive habitat model which attempts to abstract the key factors controlling a species well-being. With sufficient time, money and expertise, a relatively accurate predictive habitat model might be constructed for a narrow-niche species in a small, homogeneous site with few or no interacting species, but it is highly improbable that such a site-specific model would retain its predictive accuracy when tested in different environments with different interacting species.

IFIM AND THE FUTURE

It is now apparent that the early naive hopes of many that IFIM would revolutionize the field of impact assessment for changing flow regimes by its ability to accurately predict meaningful biological changes correlated with flow changes will not be fulfilled, mainly because the quantitative output, WUA, does not accurately correlate with biologically meaningful attributes of the target species such as numbers and biomass. I do not foresee IFIM fading away from the environmental assessment scene however, because, 1. it has a large advocacy group, 2. I know of no better methodology to replace it, and 3. it's usefulness can be greatly improved over past performance in relation to new additions and refinements and particularly in relation to the experience and expertise of the user.

The personnel of the U.S. Fish and Wildlife Service Instream Flow Group are aware of IFIM problems and are continually working on ways to improve predictive accuracy. In a recently published habitat suitability index model and instream flow suitability curves for brown trout (FWS/OBS-82/10.71), I note that the suitability curves used in the habitat model are classified into four categories: 1. (most commonly used) are based on data derived from literature and professional judgement ("canned" program); 2. Curves derived from site-specific original data (utilization curves); 3. Utilization curves corrected for environmental bias -- comparing "utilization" vs. availability to arrive at "preference" curves; 4. "Conditional preference curves" to take into account interaction among variables (as discussed above for depth and velocity). I also noted that the recent velocity curves differentiated between mean water column velocity and "nose" velocity (velocity at site where fish exists). This is an important distinction because in higher gradient streams the high average current velocity will result in low WUA for most of the stream channel, although boulders, logs, etc. creating turbulent flow with small areas of microhabitat with pockets of low velocity can allow for high utilization that would be overlooked in a straightforward recording of average velocities along a transect. This former lack of distinction between average velocity and nose velocity was likely the major reason for the poor performance of IFIM when tested in Montana trout streams. Nelson (1980 a.b.) concluded that: "The weighted useable area (WUA) values generated by the IFG incremental method for the rivers of the study do not provide an accurate index of the actual amount of habitat that is available for brown and rainbow trout at the selected flow of interest. As a result, the IFG flow recommendations for the five study reaches are unreliable." This example also demonstrates the importance of experience and expertise of the user. When it is recognized that it is not the average velocity that determines "useability" but the amount of microhabitat with pockets of low velocity amidst an area of high velocity, the IFIM procedure should be modified to quantify the amount of microhabitat.

The Colorado Division of Wildlife has had some success in its application of IFIM to fishery problems (Nehring and Anderson 1984, Anderson and Nehring 1985), and this success is due to the experience and expertise of Barry Nehring. Instead of simply obtaining data for instantaneous correlations with biomass, the CDOW studies concentrate on flows in relation to determination of year-class strength (flows during spawning, incubation, and for newly hatched fry) and survival into older age classes (overwinter flow). Limiting factors (minimal WUA values) are examined for their insight into the factors determining trout abundance, particulary in rivers below dams. Nehring and Anderson have now well-documented the range of flows determining high year-class strength and low year-class strength for most of Colorado's major regulated rivers. It could be argued that this could have been accomplished without the aid of IFIM, by simply using USGS flow records, adequate sampling, and common sense. This may be true, but as a vehicle for communication of complex fishery information to non-biologists (such as administrators in water agencies), a computer printout and WUA curves relating flow to good and poor year-classes are impressive and for getting a point across.

Mr. Nehring has also developed innovative ways to manipulate WUA data to provide additional insight into problems (Nehring and Anderson 1984). If IFIM were to have more users with the experience, expertise, enthusiasm, and insights of Mr. Nehring, greater credibility of this methodology would be expected in the future. I would not, however, expect that WUA will ever be a consistently accurate predictor of a species biomass in different environments.

RECOMMENDATIONS

The problem that this report is designed to overcome concerns unwise and unwarranted demands that may be made by uninformed persons during discussions and negotiations regarding potential environmental changes resulting from a change in flow regime. Unless the spokespersons representing various agencies are extremely knowledgeable about the river and its biological system, and also knowledgeable about assessment methods and methodologies, there is likely to be a reflex reaction requesting "baseline studies", IFIM or HEP analyses, etc., before the potential problems are clearly identified.

The right questions must be asked: What river (or section of a river) will be changed? What are the target species of concern? How will the future annual hydrograph differ from the past? How might this change affect the target species, negatively and positively? What opportunities are there for flexibility and enhancement measures?

Once the area, the target species, and the issues have been defined, the questions concerning the utility of an assessment methodology to predict impacts can be addressed. This will require people with a high level of knowledge to arrive at a best solution. For example, in the Verde-Salt River drainage it might be requested that IFIM analysis be made for spike dace and/or loach minnows. The important questions to ask in such a case would be: are smallmouth bass present in the river section of concern? Is there a single example where loach minnows or spike dace maintain viable populations in the presence of bass? How reliable might be any habitat model constructed for these rare species? If models were made and incorporated into PHABSIM to correlate habitat with flow changes, how predictive would they be? What would the WUA values correlate with in regards to something meaningful about the species? How useful would the WUA values be for decision making?

In relation to the "exclusionary principle" I would point out the problem illustrated in my February report on the lower Verde trout fishery. Two bald eagle nests on the lower Verde where eagles rear young each year, makes this endangered species the species of highest priority for any environmental assessment. The eagles eat carp, suckers and catfish -- flows optimizing eagle food exclude flows for trout from serious consideration.

The point to be made is, that for the well-being of the target species in the biological system, more than technician grade studies on laying transects and recording data is needed. Holistic interpretive synthesis by persons knowledgeable about the river, its past and proposed flow regimes and of the target species is necessary. At least the input of higher level expertise should identify critical areas to see that the transects are most correctly sited. Then, analysis of depth-velocity changes might provide insights into limiting factors and opportunities for enhancement. The hard questions concerning the precise purpose of any proposed analysis and the predictive accuracy expected from any analysis should be asked during the earliest stages of negotiations.

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Behnke

ADDENDUM TO CRITIQUE OF INSTREAM FLOW METHODOLOGIES

The objectives of my critique were to demonstrate the importance of the human element of experience and expertise, critical for the environmental assessment process, the severe limitations that any habitat model is faced with in regards to predictive accuracy, and the danger of confusing objectivity, quantification, and sophistication of computer simulation with biological reality, which may cause naive negotiators to demand inappropriate studies.

As an example of this latter point, a paper by Hunn (1985) is instructive. A hydropower dam was proposed for a headwater area of the Middle Fork Cottonwood Creek, Shasta Co., California, in the Sacramento River basin. The key fish species of the Sacramento River are Chinook salmon and steelhead trout. During negotiations, the representatives of state and federal agencies specified that the instream flow incremental methodolgy (IFIM) be applied to the assessment study and that weighted useable areas (WUA) be computed for various life history stages at various flows for Chinook salmon and steelhead trout. During the study it was found that summer flows average three cfs with no flow (intermittent stream) occurring 5% of the time. Maximum summer water temperatures commonly reach 85 degrees F. As would be predicted from such flow and temperature data, the fish fauna was dominated by warm-water species of suckers and minnows. Chinook salmon and steelhead trout do not occur in the stream and most likely never occurred there. If the recommendations made in my report were followed in such a case, the expensive and meaningless assessment for "ghost" species could have been avoided. One might ask, why the biological consultants to the project did not bring up the right questions and concerns during the negotiation process? Perhaps they were not involved, but I would point out that meaningless busy work makes good business for consultants.

In my critique, I cited a paper by Mathur et. al. in a 1985 issue of the Can. J. Fish. Aquat. Sci. that was highly critical of IFIM. A major criticism is that the ultimate numerical output of IFIM, the weighted useable area (WUA) relating habitat quality to flow, does not relate to actual numbers or biomass of the fish species (it has no predictive reliability). Personnel of the USFWS' Instream Flow Group wrote a response to the Mathur et al. criticism and I obtained a copy. In the response an admission is made......" it is true that biomass predictions cannot be made with IFIM in its current configuration." The discussion then points out that it is not desirable to relate changes in WUA to changes in biomass in regards to environmental protection because, in most instances, values of fish biomass would be insignificant in relation to other uses of water in a strict cost - benefit analysis. This position creates the unusual situation whereby negotiations over a change in flows discuss trade - offs of increasing or decreasing WUA's, but with no basis to relate WUA's, to increases or decreases in fish biomass or any other value unless some arbitrary and hypothetical intrinsic "sacredness" standard can be ascribed to a WUA -- a position I doubt would "hold up in court". FWS personnel associated with IFIM realize that they must be better able to defend IFIm and are presently engaged in an effort to demonstrate correlation between WUA and trout biomass. This can be done based on some of the prequisites given in my critique -- stable, isolated, highly recurrent system and the use of a narrow niche species in a relatively homogeneous environment. Data on flows and trout abundance in the Black Canyon of the Gunnison River, Colorado, are ideal for making WUA - biomass correlations. Three dams of the Curacante Project regulate flows of the Gunnison River (transforming a highly variable natural system into a stable, isolated and highly recurrent system). The spawning, egg incubation and early life history (first 8-10 weeks after hatching) represent an extremely "narrow niche" phase of a trout's life history. Several years of monitoring have demonstrated that if flows are severely decreased after spawing, the redds containing the incubating eggs are lost. If flows are higher than normal during the 8-10 weeks after the baby trout hatch, mortality of young is excessively high (the young have limited mobility and can not tolerate high velocities). Thus, under such conditions, the abundance of each year-class of trout is highly correlated to the flow conditions during egg incubation and early life history. Total biomass of the population equals the accumulated year-classes. With such a system, WUA's calculated for egg incubation and the fry stage can demonstrate good correlation with population biomass. The problem is that such an analysis only reveals that WUA can relate to biomass if highly restrictive limitations apply to the system under study-- and such limitations are not likely in most aquatic ecosystems where IFIM would be applied.

A hydrology problem that wasn't considered in my critique concerns the innate response of river channels to constantly change. This is especially true in regulated rivers or rivers with a changed flow regime (new energy system to adapt to). As such, future changes in channel morphology would render useless any habitat quantification measurements recorded from current "baseline" conditions. This type of hydrology problem in relation to IFIM analysis was discussed by Kondolf and Sale (1985).

Finally, a word was omitted on pg. 20, line 19 of my critique. "Would the WUA values correlate with....." should read:"What would the WUA values correlate with...."

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IDENTIFICATION OF CUTTHROAT TROUT
SAMPLES FROM ROCKY MOUNTAIN NATIONAL PARK

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ABSTRACT

Three specimens from Middle Hutchinson Lake are identical to upper Hutcheson specimens (pure greenback trout). Samples from Fifth Lake and from Ten Lakes Park represent populations derived from more than one parental source. The Ten Lakes Park fish are definitely influenced by hybridization with rainbow trout. The diverse sources of cutthroat trout historically used to stock RMNP waters are examined.

Three specimens, 262, 329, and 336 mm TL, collected from Middle Hutcheson Lake, have identical spotting pattern and meristic characters of upper lake fish -- 18, 20, 22 gillrakers, 48, 50, 55 scales above lateral line, 196, 209, 214 scales in lateral series, and 4, 6, 9 basibranchial teeth. The plump condition and low density of trout in Mid Hutcheson indicates a lack of successful reproduction. The population is probably dependent on occasional movement of fish from upper lake.

Eight specimens from Fifth Lake (212-383 mm TL) and 9 specimens from Ten Lakes Park, on the west slope of Park, represent populations derived from multiple stockings of diverse parental sources. Fifth Lake specimens appear to be pure or virtually pure cutthroat trout, probably predominantly derived from Trappers Lake stock. The gillraker count, 19-22 (20.4) with 6-11 (9) posterior rakers on first arch, scale counts, 44-54 (48.4) above l.l. and 182-205 (193) in lateral series, and pyloric caeca number, 34-42 (37.5) are essentially identical to the original Trappers Lake stock. The basibranchial teeth, however, are much less developed than in Trappers Lake fish. One specimen lacks teeth and the others possess from 1 to 14 (5) tiny teeth imbedded under the skin. The spotting pattern is highly variable. Some specimens exhibit a Rio Grande cutthroat (S. c. virginalis) appearance, with large, blotch-like spots on caudal peduncle. Others resemble greenback cutthroat with more even distribution of large spots.

My overall summary and interpretation of the totality of characters of this sample is that the population is predominantly of Trappers Lake ancestry but has been slightly influenced by other parental sources probably including a very slight influence from rainbow trout. The Fifth Lake fish were in excellent condition with enormous fat deposits around the pyloric caeca. Evidently a large flight of flying ants had landed on this lake shortly before the fish were sampled. Most stomachs were gorged with ants. Small chironomids (2-3 mm larvae, 4 mm adults) made up the bulk of the rest of the diet. One specimen contained numerous stonefly larvae. Evidently feeding conditions are excellent in relation to density of this population. The sample from Fifth Lake was made on July 21. All females contained only immature eggs and no mature unspawned eggs of recent spawning were seen in body cavity. Most likely, with a short growing season, females do not spawn annually. The males had mature, but not turgid testes, indicating recent spawning.

The 9 specimens from Ten Lakes Park (295-338 mm TL) are more variable than the Fifth Lake specimens. One specimen has a typical rainbow x cutthroat hybrid spotting pattern. Most specimens lack bright coloration and phenotypically suggest Bear River cutthroat, S. c. utah. The influence of rainbow trout in this population can be detected in the basibranchial teeth. Five of nine specimens lack teeth. The remaining four have 1, 2, 3, and 4 teeth respectively. The scale counts of 40-45 (42.3) above l.l. and 157-180 (171) in lateral series indicates S. c. pleuriticus x S. gairdneri hybrid. The gillrakers of these specimens are longer and better developed than in Fifth Lake fish, but fewer in number -- 18-21 (19.2) with 4-11 (6.9) posterior rakers. Rainbow trout lack posterior gillrakers on the first arch and the reduction in both anterior and posterior rakers in this sample compared with the Fifth Lake sample (20.4 vs. 19.2 and 9 vs. 6.9) probably is a fairly accurate reflection of the degree of rainbow trout influence in these fish.

Except for two thin specimens, evidently recently spawned fish, the fish in this sample were in good condition and well fed, but not to the extent of the trout from Fifth Lake. The amount of caecal fat was much less. The major components of the diet in these specimens were chironomids and zooplankton, probably Daphnia.

IDENTIFICATION PROBLEMS

As discussed, the fish from Fifth Lake are believed to be predominantly of Trappers Lake ancestry, but not pure Trappers Lake stock because of the feeble development of their basibranchial teeth. Jim Bennett, CDOW, recently sent me a copy of meristic characters of Williamson Lake no. 3, California, trout counted by Anita Martinez.

Anita counted 2-34 (18.6) basibranchial teeth in 19 specimens, or about 50% more teeth than previously reported for Trappers Lake (or Trappers Lake fish in Williamson Lakes). Thus, a question was raised on the purity of the Williamson Lakes trout. The discrepancy in tooth counts may be explained by a 'founder effect' or genetic bottleneck changing gene frequencies in a new population established from a few parents and/or Anita's counting method of removing the lower mouth and pharynx for examination, which should find more minute teeth that would have been overlooked by the "standard" method of peering down the specimen's throat.

I could cite an example of what I believe is a nongenetic, environmentally induced change in the number of basibranchial teeth. In 1971 I examined a sample of Yellowstone Lake cutthroat trout from South Gap Lake, Snowy Range, Wyoming. This sample had a mean tooth count of 33 or 50% higher than the parental stock in Yellowstone Lake. The South Gap Lake fish were 11 years of age and extremely slow growing (280-320 mm). I suspect that if the period when basibranchial teeth first appear in young cutthroat (40-75 mm) is greatly extended by slow growth, a nongenetic increase in the number of basibranchial teeth might occur. Williamson Lake no. 3 may have a harsh environment for growth, similar to South Gap Lake. In any event, the other meristic characters and phenotypic appearance of the Williamson Lakes trout clearly indicate a pure Trappers Lake stock. A point that should be noted, however, is that the Trappers Lake cutthroat population may not have been absolutely pure in 1931 when the shipment of Trappers Lake eggs was sent to California to be stocked into Williamson Lakes. Any hybrid influence, however, prior to 1931, would have been slight because of the abundance of natural reproduction and great surplus of young in Trappers Lake, and would, most probably, have been limited to mixing with other stocks of _S. c._

pleuriticus being propagated in state hatcheries during the early 1900's (mainly cutthroat trout, S. c. pleuriticus, from Grand Mesa lakes).

The variability noted in the present samples from Ten Lakes Park and Fifth Lake, raises questions on the sources of cutthroat trout used to stock waters of RMNP over the years. In most instances, I believe it would be impossible to know with certainty the parental stocks used for specific stocking of a specific lake. It is possible, to some extent, to know the sources of some stocks used in certain years and if the year of stocking is known, an indication could be had of the parental stocks used. Such a task would be an excellent project for RMNP to undertake, perhaps if an intern with intense interest in the subject matter could be found. As examples to follow, I would point out papers produced by John Varley for Yellowstone Park -- 1979, Records of egg shipments from yellowstone fishes, 1914-1955, and 1980, A history of fish stocking activities in Yellowstone National Park between 1881 and 1980. Also, Bill Wiltzuis' CDOW publication, 1985, Fish culture and stocking in Colorado, 1872-1978, is an excellent source of historical information.

Evidently, the first organized stocking of RMNP waters was by the Estes Park Improvement Association who constructed the Estes Park hatchery in 1907 (leased to state in 1908). If most of the stocking in what is now RMNP originated through the Estes Park hatchery, it would be important to know from where did the hatchery obtain its fish. A major problem is the fact that state and federal records considered all cutthroat trout as "blackspotted" trout without regard to subspecies and it would not be uncommon to indiscriminantly mix eggs and fry of various stocks in the distribution system. What I gleaned from Wiltzuis' publication would indicate that the great majority of cutthroat trout handled in Colorado hatcheries from the 1890's to about 1940 and later were S. c. pleuriticus (and also hybrids in later years). Varley's records of shipment of Yellowstone Lake eggs shows only three shipments to Estes Park. The first was 400,000 eggs to G. H. Thomson in 1912 and two shipments to RMNP of 1.5 million in 1940 and 700,000 in 1942. From about 1890-95 after the federal fish hatchery was constructed at Leadville, a cooperative state-federal operation took greenback trout and some yellowfin trout eggs from Twin Lakes. From 1892-97 eggs were taken from

S. c. pleuriticus from Black, Sweetwater, and Freeman lakes. From 1899 - ca. 1940, the major source of cutthroat eggs of this state-federal cooperative venture was the Grand Mesa lakes, which originally had *pleuriticus*, changing to hybrids and rainbow trout in later years. Trappers Lake has had the longest continuity for propagation of *pleuriticus* dating at least to 1919, but probably much earlier. Marvine Lake *pleuriticus* were also used from 1908-1915.

use 1985 article by Van Velson on Em. Lake

One of the earliest egg taking operations was Emerald Lake which supplied millions of eggs to stock around the state from the 1890's through the 1930's. Emerald Lake was originally barren of fish and according to Wiltzius' publication, the earliest stocking of the lake was 1888 with *S. c. pleuriticus* from the Pine River (San Juan drainage). Rainbows must have been stocked about the same time or soon after because the earliest descriptions of Emerald Lake trout in the 1890's are certainly of hybrids. The photo on the front cover of Wiltzuis' publication of an Emerald Lake trout first published in Outdoor Life, March, 1901 ~~1907~~, appears to be a typical rainbow trout. Thus, the present "Emerald Lake rainbow" was distributed by the millions for many years as "blackspotted" trout.

Rio Grande cutthroat, *S. c. virginalis* and their hybrids with Yellowstone cutthroat (and/or rainbow trout) also may have been stocked into RMNP. Evidently, the first large-scale propagation of Rio Grande cutthroats, providing eggs and fish for state stocking was by Bert Hosselkus. A 1914 article reprinted in Wiltzius' publication stated that Mr. Hosselkus had, "one of the best trout hatcheries in the state" at Lost Lakes in Mineral Co. (near Creede) where he hatched 8,000,000 fry each year from trout taken from "near the head of Clear Creek", supplying 2,000,000 fry to the state.

According to Varley's report on egg shipments from Yellowstone Lake, 500,000 Yellowstone cutthroat eggs (or fry?) were shipped to "B. C. Hosselkus" of Creede, CO, August 12, 1912, and 230,000 on July 30, 1913. Thus, it can be assumed that Mr. Hosselkus, after 1914, was likely distributing a mixture of Rio Grande and Yellowstone cutthroats. The 1914 article also mentioned that James Stell of Delta Co. had recently stocked Mr. Hosselkus' trout in a chain of lakes, "high up on the Grand

CRC from Emerald were stocked in the Rio Grande in 1903 or 1904

Mesa" -- which would put Rio Grande (and Yellowstone?) trout on the Grand Mesa by 1914.

The federal hatchery at Creede, CO, received millions of eggs from Yellowstone Lake between 1931 and 1953. Eggs for federal and state propagation were also taken from Haypress Lake near Creede (still used by CDOW). Haypress Lake is a reservoir originally barren of fish and natural reproduction does not occur so each year-class was derived from the Creede hatchery. Over the years, the Haypress Lake fish became a mixture of Yellowstone Lake cutthroat and Rio Grande cutthroat, with other sources of mixing of high probability. When I examined a collection of Haypress Lake fish in 1970, I found them to be predominantly of Yellowstone ancestry, but far from pure Yellowstone cutthroat.

most recent consistent takes at Seven Lakes since 1959 — COutdoors

A source of cutthroat eggs widely used for stocking along the Front Range since the early 1900's is Seven Lakes on slopes of Pikes Peak (the "Pikes Peak" cutthroat discussed by Trojnar and Behnke, 1974, Trans. Am. Fish. Soc.). Seven Lakes are at the headwaters of Beaver Creek (Arkansas R. drainage). Due to barriers, I assume the lakes were originally barren, but nineteenth century resort hotels around the lakes should have resulted in stocking Beaver Creek greenback cutthroat. During the long history of fish culture at Seven Lakes, the lakes were stocked with cutthroat trout of diverse origins. A 1970 collection from Fifth Lake (of the Seven Lakes), currently used for cutthroat propagation, showed a predominantly greenback appearance with bright coloration. All 22 specimens had basibranchial teeth but their caecal count of 32-51 (42) are slightly high and scale counts of 42-48 (44) and 162-205 (181), slightly low for pure greenback.

In summary, the stocking of RMNP waters can be divided into three periods. The first period, prior to 1907-08 when the Estes Park hatchery was constructed to "formalize" a regular stocking program, trout were probably obtained from local sources and transported to barren waters (probable origin of Hunters Creek and Upper Hutcheson Lake greenback populations). The second period from 1907-08 to 1940's, organized stocking occurred with cutthroat trout obtained mainly from Trappers Lake, Grand Mesa Lakes, Seven Lakes, and Yellowstone Lake. The last 20

— egg takes began July, 1903

years or so before stocking ceased (1967?), trout were stocked by aircraft. The main source of cutthroat trout was probably Seven Lakes, supplemented with Trappers Lake fish.

Possibly, similar to Hunter Creek, upper Hutcheson Lake and Ypsilon Lake, other park waters may contain pure populations of stomias or pleuriticus, although the diverse sources of "blackspotted" trout stocked and multiple stockings of most lakes makes such future discoveries a "long shot" possibility, but certainly not hopeless.

IDENTIFICATION OF SEVEN SAMPLES OF CUTTHROAT TROUT FROM ROCKY MOUNTAIN NATIONAL PARK

Robert Behnke
June, 1989

Seven samples consisting of 40 specimens collected in 1987 and 1988 were examined. Most specimens indicate mixtures of greenback cutthroat trout (stomias) and Colorado River cutthroat trout (pleuriticus) with a slight influence of rainbow trout ancestry detected in specimens from the Loch and Onahu Creek. Columbine Creek may contain a pure population of pleuriticus of unknown origin. Poudre Pass Creek, although in South Platte drainage, may contain pure pleuriticus originating from the headwaters of the Colorado River.

South Platte Drainage

Poudre Pass Creek. One adult specimen of 261mm TL and six young-of-year specimens from 44-53 (48)mm; collected August 26, 1988. Two large falls on the upper Poudre River suggest that the headwaters were originally barren of fish. This adult specimen appears identical to specimens formerly examined from Willow Creek, and similar to a remnant population of Colorado River cutthroat trout that occurred in headwaters of Colorado River above Lulu City (last three adult specimens collected in 1975). I would assume that the headwaters of the Poudre River above the falls were first stocked with trout via the Grand Ditch.

Spots, relatively uniform and sparse over sides of body. Spots relatively small, smaller than pupil of eye; largest spots on caudal peduncle. Scales very numerous; 50 above lateral line and 206 in lateral series. Gillrakers well developed, 21 anterior and 14 posterior rakers on first arch; 16 basibranchial teeth and 36 pyloric caeca. Specimen in good condition, gorged with food (mainly ants and grasshoppers), with considerable fat deposits.

The six young-of-year specimens from 44-53 (48)mm are larger than might be expected for such a high elevation stream. Assuming that seven or eight weeks are required for the period from spawning to hatching and from hatching to emergence, and assuming spawning during the first week in June, the specimens collected on August 26, would have been of about four weeks post emergence age. With a size of about 20mm at emergence, growth during the first summer appears to be relatively rapid -- greater than expected for high elevation headwaters.

Fall River, above Cascade Falls. Two specimens of 248 and 254mm TL. As with the Poudre River, I assume that the headwaters of Fall River above barrier falls were originally barren of fish. These two specimens exhibit distinctly different spotting patterns, the result of past stocking of different forms of cutthroat trout. One specimen has relatively small spots all over the sides of the body, the other has larger spots all over the sides and on the ventral region. I found no trait clearly indicating an influence of rainbow trout, except perhaps for posterior gillrakers; such a hybrid influence might be apparent in a larger sample. Scales 47, 48 and 179, 182 above lateral line and in lateral series. Gillrakers 20, 21 with feeble development of posterior rakers (rainbow trout lack posterior rakers on first arch). Basibranchial teeth 2, 9; pyloric caeca 34, 40. These two specimens suggest that this population is a mixture of stomias and pleuriticus.

The Loch. Four specimens of 223, 235, 246, and 317mm TL. The three smallest specimens with relatively large, sparse spots evenly distributed over the body, have the phenotypic appearance of greenback trout, but the scale counts are much too low for stomias -- 39-46 above lateral line and 165-174 in lateral series. The largest specimen is distinctly different with a profusion of smaller, more irregular spots, especially on caudal peduncle region. This specimen has the appearance of a rainbow x cutthroat hybrid -- but its lateral series scale count of 183 and pyloric caeca count of 29 (versus 37-41 in other three specimens) are the most cutthroat-like values in the sample. Low numbers of basibranchial teeth, 1-6 (3) may also denote a rainbow trout influence, but the gillraker numbers -- all with 21 anterior rakers and 9-12 posterior rakers do not indicate a rainbow trout influence.

As with most lakes in the Park, I assume the Loch was originally barren and stocked with a variety of cutthroat and rainbow trout. Based on the spotting pattern, I believe a greenback population was the first trout to become established. Subsequent stockings, probably with various forms of pleuriticus and rainbow trout, superimposed a rather heterogeneous genotype which is reflected in the mosaic of characters.

The diet of these specimens consisted mainly of larval mayflies and chironomids.

Colorado River Basin

Onahu Creek, 1/4 mile below Julian Lake. Two specimens of 238 and 273mm TL. The larger specimen has a greenback-like spotting pattern, but with more numerous spots than typical of stomias (more typical of stomias x pleuriticus hybrid). The smaller specimen has a spotting pattern indicative of a rainbow trout influence with spots on top of head (pure cutthroat trout lack spots on top of head). Although these fish do not represent a pure population, the rainbow trout influence is slight and detectable only by spotting pattern. The meristic characters are wholly typical of cutthroat trout (both stomias and pleuriticus). Gillrakers 18, 21 anterior; 9, 11 posterior; 4, 6 basibranchial teeth, 46, 50 and 184, 196 scales above lateral line and in lateral series; pyloric caeca 34, 39. I would assume that Julian Lake was stocked with diverse forms of trout over many years and is the source of the genotypic diversity present in the Onahu Creek trout. They are overwhelmingly cutthroat trout (mainly stomias x pleuriticus) with a very slight influence of rainbow trout.

Glass Lake. Five specimens of 223, 271, 277, 279, 280mm TL, collected in 1987. The spotting pattern of the smallest specimen in this sample is typical of Hutchinson Lake greenback -- large, pronounced spots evenly distributed over body. The other four specimens have medium-large spots more typical of stomias x pleuriticus hybrid. Scale counts 44-56 (49) above lateral line and 178-213 (197) in lateral series; gillrakers 19-21 (19.4) anterior and 5-8 posterior; basibranchial teeth 5-14 (9); pyloric caeca 25-42 (32). The trout population in Glass Lake appears to have a similar origin as Dream Lake discussed in last year's report. I assume that the original population was the same form of greenback that was stocked in Hutchinson Lake. Later introduction of probably more than one form of pleuriticus resulted in present population after many generations of mixing. The spotting pattern, gillraker development, and scale counts indicate that only stomias and pleuriticus were involved in the establishment of the present genotype.

Haynack Lake. Seven specimens of 161, 211, 227, 236, 276, 282, and 353mm TL, collected August 10, 1988. These specimens are distinguished by bright coloration, due to a diet of zooplankton high in carotenoid pigment. In the spots on the lower half of the body of the largest specimen, carotein has replaced melanin so that the spots are red rather than black! These specimens also exhibit a stomias x pleuriticus spotting pattern. The six smaller specimens have a uniform spotting pattern of medium sized spots distributed over the body (indicating stabilization of a hybrid genotype). The largest specimen, however, exhibits a concentration of large spots on the caudal peduncle, typical of pleuriticus, especially Trapper's Lake pleuriticus. Scale counts 43-47 (45) above lateral line and 167-184 (177) in lateral series are somewhat lower than expected in stomias or pleuriticus or their hybrids. Gillrakers 18-21 (19.9) anterior and 5-8 posterior; basibranchial teeth 2-12 (5) and pyloric caeca 32-40 (36).

These specimens are gorged to bursting point with red zooplankton of about 2mm diameter. Zooplankton contains oil globules of orange pigment. Great amount of orange fat is deposited around pyloric caeca and caeca themselves are filled with orange mush-like food material. The zooplankton may be copepod Diaptomus. It must occur in swarms to be engulfed in great quantities by feeding fish to explain the enormous density in gut.

It would be of interest to note lakes in Park with the same species of zooplankton and the condition of trout in those lakes. A similar situation of red zooplankton fed on by the trout of Nannita Lake was discussed in the 1988 report.

Columbine Creek. Thirteen specimens from 120-229mm TL collected October 6, 1988. With a larger sample of better preserved specimens, more variation in the spotting pattern was noted compared to the 1987 sample from this creek. The spots vary in size on different specimens but are relatively uniformly distributed over the body. Scale counts 177-194 (185) lateral series and 42-48 (45) above lateral line: gillrakers 18-23 (19.9) anterior and 3-12 (8) posterior; basibranchial teeth 2-9 (5) and pyloric caeca 29-41 (37). The taxonomic characters of this sample appear to represent a pure population of pleuriticus probably established from one introduction. Their characteristics are distinctly different from the pleuriticus native to the headwaters of the Colorado River in the Park (now probably represented by populations in Willow and Poudre Pass Creeks) and from Trapper's Lake.

A major source of pleuriticus propagated in hatcheries from 1899 to 1940 (later years may have included rainbow hybrids) was the Grand Mesa. Both the U.S. Fish Commission (Leadville hatchery) and the State of Colorado shared in the Grand Mesa egg-taking operation. Some information on the Grand Mesa operation is found in Vol. 28 (1908) of the Bull. Bur. Fish., pages 697-757 -- "Fish Cultural Practices of the U.S. Bureau of Fisheries", by J. W. Titcomb. Several lakes were used to take cutthroat trout spawn on the Grand Mesa, and I would assume all the eggs were combined as "black-spotted trout". Thus, if Columbine Creek trout represent the Grand Mesa pleuriticus, it is likely the product of several populations.

Origins of Rocky Mountain National Park Trout

Possible origins of cutthroat trout introduced into Rocky Mountain National Park were discussed in my 1987 and 1988 reports. A brief review of the former information and additional information on potential sources is presented here. The trout that was first stocked was probably the native greenback cutthroat trout. Some stocking such as Hunter's Creek was most likely by individuals carrying a bucket of locally caught fish. The presence of an unusual form of greenback in Upper Hutcheson Lake and this same spotting pattern appearing in hybridized populations in Dream Lake, Glass Lake, and the Loch, indicate a common source for many of the early introductions of greenback trout; that is, a fish hatchery.

The most probable source was a hatchery in the Estes Park area operated by Lord Dunraven. In Wiltzius' publication on the history of Colorado fish culture, Lord Dunraven's hatchery is cited three times from 1896 to 1903. It could be suggested that when Lord Dunraven's hatchery ceased operations (sometime after 1903), the Estes Park Improvement Association constructed the Estes Park hatchery in 1907 to continue the supply of trout to stock local waters. The Estes Park hatchery was run by the State after 1908 and, evidently, did not propagate greenback trout. I would consider the Estes Park hatchery as mainly a receiving point for fish from other areas to be stocked in Park waters.

After the establishment of Rocky Mountain National Park, clear lines of authority regarding fish stocking responsibility between federal and state government agencies seemed to be lacking. Correspondence in the Leadville hatchery files reveals a lack of clear lines of responsibility for stocking Park waters. Letters from the Park Director to the hatchery Superintendent in 1922-23 makes clear that some federal fish were being stocked in the Park but many more were urgently needed.

In any event, both the Leadville hatchery and the Colorado Fish Commission shared in most of the cutthroat trout egg-taking operations in Colorado from about 1890 to 1940 and sources used by either agency can be considered as potential parental sources of present populations in the Park.

In addition to the lakes from which cutthroat were used for propagation mentioned in my 1987 and 1988 reports, egg-taking operations also occurred in the following waters during the 1890-1940 period which could have contributed to Park stocking (taken from appendices in Wiltzius' publication). Lakes with native populations of Colorado River cutthroat trout used in fish culture: Black Lake (1892), Grand Lake (1905-1909), Piney Lake (1912), (all of these lakes, in addition to Grand Mesa lakes, Sweetwater Lake, and Freeman Lake were used by Leadville hatchery). An early source of hybrid trout (rainbow trout x Colorado River cutthroat) was Emerald Lake (the Emerald Lake "rainbow"), used in state stocking since 1895.

Most of the Colorado River cutthroat trout stocked by the state came from Trapper's Lake and Grand Mesa lakes. Although large numbers of Yellowstone Lake cutthroat trout (subspecies bouvieri) were distributed in Colorado by both state and federal agencies. I have not detected any specimens of Yellowstone trout in Rocky Mountain National Park. Evidently, all lakes and streams with self-reproducing populations of introduced trout contained well-established populations before stocking with the Yellowstone subspecies began. Subsequent introduction of Yellowstone trout had minimal impact on the existing genotypes, but may have contributed to some of the present diversity observed. In contrast to waters of Glacier National Park, where numerous populations of introduced Yellowstone trout have been documented (Marnell et al. 1987), no record of Yellowstone trout persisting in Rocky Mountain National Park is known.

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Interpreting the Phylogeny of Salvelinus

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Abstract

Problems are defined at the intraspecific, interspecific, and intersubgeneric level for the genus Salvelinus. Distinctions are made between patterns and processes, taxonomy and systematics, and between population genetics and evolutionary genetics in an attempt to more precisely identify areas of controversy and point out gaps in our present knowledge on the phylogenetic branching sequences within the genus.

Introduction

The proceedings of this symposium will constitute the third major volume of works devoted to charrs, as well as my third contribution on the systematics and taxonomy of Salvelinus. I am not sufficiently naive as to believe that three is a magic number and that there will be a unanimous and universal agreement on my present conclusions and taxonomy arrangement. I can only make an appeal for the use of a reasonable amount of common sense to fairly evaluate the evidence presented, while realizing that reasonableness and common sense have no relevance to the belief systems of some "Arctic charr fanatics."

The main problems I address concern the classification of intraspecific units, specifically the sympatric sibling species of S. alpinus alpinus; the classification of interspecific units of charr evolution, in particular, the delineation of S. alpinus and S. malma as valid species; and intersubgeneric relationships in consideration of a strikingly differentiated new species recently discovered in Lake Elgygytyn, Chukokst Peninsula.

A new interpretation of phylogenetic branching sequences in the subgenus Salvelinus since my last paper on the subject (Behnke 1984) concerns the alignment of Dolly Varden, S. malma. Based on recent information on karyotypes, I now place malma with alpinus as a group with a most recent common ancestor. Previously, I considered malma as part of the leucomaenis-confluentus phylogeny.

As mentioned, I doubt that I can resolve all controversies regarding the interpretation of charr phylogeny for the purpose of classification, but I will put forth some concepts and definitions in hopes that the scope of future controversies can be more narrowly focused in order to clearly understand the basis of disagreements. For this purpose I introduce the concept of patterns and processes in nature and define taxonomy as a discipline mainly concerned with interpretation and arrangement of the patterns observed. Thus, if patterns (such as the diversity present in the genus Salvelinus) are accurately assessed by an experienced and expert taxonomist, a relatively accurate system of classification is possible without reference to the processes that caused the patterns (the evolutionary processes associated with zoogeography, differential selective pressures, reproductive isolation, etc.). The field of systematics, by my definition, concerns the processes of evolution that have produced the patterns of diversity. An excellent systematic study may produce useful insights into the mechanisms of differentiation, ecological segregation, and reproductive isolation without reference to taxonomy. On the other hand, an excellent systematic study may require the imposition of a taxonomy to classify the populations studied, and the disparity between the quality of the systematic study and the improvised taxonomy may be clearly contrasted. As an example of this dichotomy between the quality of a systematic study and the validity of its associated taxonomy, I would cite the excellent systematic studies of Gunnar Svardson, when he was a biologist with the Drottningholm Institute, on the whitefish genus *Coregonus* and the erroneous taxonomy he used to classify the whitefish species of northern Europe (Behnke 1970). To this day, in the European literature, erroneous reference is made to *C. peled* and *C. nasus* occurring in European lakes. The westward limits of the natural distribution of these species are the Menzen River (*peled*) and the Pechora River (*nasus*).

Svardson's taxonomic error is the result of using gillraker number and sympatric occurrence as the only criteria for species recognition. Especially with whitefishes, gillraker numbers can significantly change in different populations in a few thousand years under different selective pressures. Sympatric occurrence with reproductive isolation between closely related populations can also occur in a few thousand years (Behnke 1972, 1980). Thus, gillraker number and the ability to occur sympatrically with closely related populations are not valid phylogenetic markers that denote ancient phyogenetic branching points occurring, for example, during the Pliocene to mid-Pleistocene period (when branching leading to sufficient genetic divergence to warrant species recognition occurred).

The present Drottningholm school of systematics has followed Svardson's concepts for species recognition resulting in equally erroneous taxonomy for Salvelinus (Behnke 1984). The basic premise for species recognition of the Drottningholm school is sympatric occurrence with traits such as growth rate, maximum size, and allelic frequencies at an esterase locus loosely associated with the sympatric populations. The taxonomic fallacy of such species is that their whole foundation of reproductive isolation, life history traits, and esterase alleles is based on characters of recent evolutionary origin (during and since the last glacier period) which are subject to convergent evolution. These traits are population genetic markers, not phylogenetic markers in the sense discussed above. The Drottningholm school of systematics has long confused population genetics with evolutionary genetics.

In any event, if the publication of this present volume on charrs fails to correct the taxonomic errors of the Drottningholm school, I would hope these errors can be limited to Swedish literature and more specifically isolated and quarantined to Drottningholm Island before infection of other European literature occurs.

Intraspecific Problems

As discussed in the introduction, a major unresolved problem concerns the origins and classification of north European/Scandinavian charr which I consider as a single subspecies, *Salvelinus alpinus alpinus*, and which biologists of the Drottningholm school (Nyman et al. 1981, Nyman 1984, Hammar 1984, Gydemo 1984) recognize as three species, *S. alpinus*, *S. salvelinus*, and *S. stagnalis*. In my last paper on the subject (Behnke 1984), I thought I had made it abundantly clear why the recognition of these three species for charr populations occurring in Sweden is invalid. The first argument against recognition of three species concerns the lack of evidence that the characters used for species recognition (sympatric occurrence, life history traits, and allelic frequencies at an esterase locus) bear any relationship to phylogenetic markers denoting mid-Pleistocene or earlier branching. On the contrary, all morphological-meristic characters of Scandinavian charr populations exhibit internal cohesion expected of a subspecies whose differentiation has occurred during the past 60,000-70,000 years. That is, there is no evidence from any type of character that any of the valid species of *Salvelinus* such as *S. malma* or *S. leucomaenis* influenced the present genotypes of north European charr.

In the first charr volume (Behnke 1980), I wrote "Thus, speaking of plesiomorphic (primitive) and apomorphic (derived) characters and sister groups without a well-founded basis for assumptions on a character state would be little more than playing games." This, essentially, sums up my present argument against the three species concept for Scandinavian charr. Life history traits and esterase allelic frequencies are invalid character states to denote phylogenetic branching and characterization of species. To misuse them in this manner is game playing.

The second argument against revising the subspecies _S. a. alpinus_ into three species concerns the rules of taxonomy. The most basic rule for taxonomic revision, that of redescription of the taxa _salvelinus_ and _stagnalis_ based on topotype specimens as a basis to identify these taxes in Sweden, was ignored. If one wants to play the game of taxonomy, first learn the rules of the game.

I naively believed that the evidence and arguments presented in my 1984 paper against the revision of _S. a. alpinus_ into three species were so persuasive that no more would be heard of such erroneous taxonomy. Obviously, I was wrong. I failed to realize that once a belief system becomes firmly entrenched, it is highly resistent to evidence and reason. The Drottningholm belief system has been institutionalized on a Swedish postage stamp depicting _Salvelinus salvelinus_. An interesting aside here concerning rules of taxonomy is that although the name _S. salvelinus_ on the postage stamp is based on the mistaken assumption that the Central European subspecies is a full species, which also occurs in Sweden, the name _salvelinus_ may indeed be the correct name for the species currently recognized as _S. alpinus_. The facts of the case were pointed out to me by Vianney Legendre and Jacques Bergeron of Quebec, Canada. The basis of a name change concerns Article 68e, Absolute Tautonymy, of the international rules of zoological nomenclature. This rule states: "If any valid species-group name or its cited synonym originally included in a nominal genus-group taxon (Art. 69a) is identical with the name of that taxon, the nominal species-group taxon denoted by that name (if available) is the type species by absolute tautonymy."

Admittedly, the complexity and obtuse nature of the rules of nomenclature would discourage most biologists from playing the game. The interpretation of Article 68e by Legendre and Bergeron is that Linnaeus described three species of charr in 1758 as *Salmo alpinus*, *S. salvelinus*, and *S. umbla*, which subsequently have been combined into one species (except by the Drottningholm school) recognized as *S. alpinus*. When Sir John Richardson created the subgenus *Salvelinus* in Fauna Boreali-Americana (1836), the valid name for Arctic charr should have been changed from *alpinus* to *salvelinus* because of the rule of absolute tautonomy, according to Legendre and Bergeron's interpretation of Article 68e. I merely insert this historical note for future generations and future symposia to ponder. I do not formally propose a name change from *alpinus* to *salvelinus* for the sake of obeying the rules (a finer reading of the case may lead to a different interpretation). I would have to admit, however, that the three species, *alpinus*, *salvelinus*, and *stagnalis*, recognized by the Drottningholm school, no matter how biologically erroneous, would obviate any basis for a name change.

Several publications in recent years have provided convincing evidence that all European charr are derived from a relatively recent common ancestor and do not represent three ancient (mid-Pleistocene or earlier) evolutionary lines (species). Also, the recent evidence further supports the contention I formerly made (Behnke 1972, 1980, 1984) that sympatric occurrence of populations with reproductive isolation can come about from a common ancestor in a relatively brief period of evolutionary time and with extremely slight, quantitatively imperceptible genetic differentiation.

Hindar et al. (1986) studied esterase polymorphism in Norwegian charr populations to test the three ancient species theory to explain three basic forms of charr --- anadromous charr, benthic dwarf charr, and pelagic normal charr. They concluded that "all Scandinavian charr are derived from one recent common ancestor." That is, three forms are polyphyletic, independently evolving from one common ancestor. These authors also mentioned that one common ancestral charr introduced into some Norwegian lakes as recently as 1910 has produced normal and dwarf populations in 75 years. Supporting evidence for incipient reproductive isolation between dwarf and normal charr in these lakes was not given, however. These Norwegian lakes with apparent sympatric divergence occurring in a 75-year time period would indeed be worthy of further study.

Hindar and Jonsson (1982) described the dwarf and normal forms of charr of Vangsvatnet Lake, Norway. Athough clearcut life history differences were found between the dwarf and normal charr, no quantified genetic differentiation was demonstrated. These authors emphasized the role of niche diversity in a lake and the absence or rare occurrence of other fish species as major factors inducing intralacustrine divergence of charr.

Klemetsen et al. (1985) found sympatric charr populations occurring in several lakes on Bear Island in the Arctic Ocean. The benthic populations in the lakes on Bear Island average about one or two fewer gillrakers and about ten fewer pyloric caeca than the sympatric pelagic populations, but no differences in esterase allelic frequencies were found. These findings are relevant to the question of the origin of the sympatric populations: convergence from two ancient species that now appear closely related due to introgression, or divergence from one common ancestor in relatively recent geological time (a few thousand years). If the apparent relatedness of the benthic and pelagic populations is the result of introgression, and the only quantified assessment of the degree of introgression is esterase allelic frequencies, then a finding of no difference in esterase forms between the benthic and pelagic populations would signify that introgression is complete and the two ancestral species have merged into a single population. The evidence of life history differences and differences in numbers of gillrakers and pyloric caeca clearly demonstrate that this is not the case for sympatric Bear Island charr. The most correct interpretation of the evidence is divergence form one common ancestor in recent times with such divergence not yet reflected in different esterase frequencies.

Riget et al. (1986) discussed three sympatric populations of charr in a Greenland lake similar to the phenomenon of Bear Island charr. These authors concluded that these Greenland charr may "...represent an early step toward sympatric speciation."

Walker et al. (1988) described two forms of charr in Loch Rannoch, Scotland. The benthic and pelagic charr of Loch Rannoch are sharply differentiated in structures associated with feeding such as head shape, jaws, and gillraker morphology. As might be expected from such trenchant morphological differentiation and the niche diversity available in large, deep Loch Rannoch, there is virtually no overlap in the diet of the two forms. The forms of Loch Rannoch charr appear to be in an advanced stage of differentiation, although only a relatively short period of geological time during the postglacial period has been available to diverge from a common ancestor. Diverse water bodies of surrounding areas might suggest that the initial divergence was allopatric with rapid differentiation occurring after sympatry.

The large Icelandic Lake Thingvallavatn, although only about 11,000 years of age, presents the most extreme example of rapid divergence of *S. a. alpinus*. Sandlund et al. (1987) described four "morphs" of charr from Thingvallavatn. Two morphs are benthic (one large, one small), and two are pelagic (planktivore and piscivore). Evidently, the typical initial divergence into benthic and pelagic forms subdivided under selection for more specific feeding specializations. Only slight differences were found in allelic frequencies for esterase and Mdh 4,5, and the authors concluded: "The Arctic charr in Thingvallavatn is polymorphic. Genetically, the four morphs are very closely related. The morphs are conspecific and do not represent different evolutionary lineages. The morphs in Thingvallavatn have probably developed within the lake and the morphs are locally adapted to different niches."

One of the authors of the above-cited paper was Rolf Gydemo, and the conclusion of the authors on the origin of Icelandic charr populations in Thingvallavatn, from one common ancestor during the postglacier period, sharply contrasts with Gydemo's previous opinion. Gydemo (1984) strictly followed the party line of the Drottningholm school when he prematurely concluded: "It is clear that all of the species from Scandinavia, as defined by Nyman et al. (1981), are present in Iceland also. The landlocked charr of Iceland were already separated into species when immigration occurred." It can be pleasantly surprising to observe the difference that a few years and better data can make on prevailing opinion.

Thus, in 1988 there can be little doubt among informed and rational thinking biologists that S. alpinus can indeed initiate rapid divergence in sympatry with reproductive isolation, attaining the status of biological species. As I have previously emphasized (Behnke 1972, 1980, 1984), the taxonomic problem of recognizing all populations occurring in sympatry with reproductive isolation as species is that they are polyphyletic. That is, the subspecies S. a. alpinus in postglacial times demonstrates an amazing ability to differentiate to fill different niches in individual lakes or interconnected bodies of water. At least some of the numerous examples of sympatric populations are most likely the result of sympatric speciation. I would maintain, however, that a brief period of allopatry in lakes of different types (different selective pressures) would precondition populations for reproductive isolation and rapid divergence, once they came together in sympatry in a lake with abundant niche diversity.

To speculate on why one subspecies of charr has the ability to fraction into populations and rapidly diverge in sympatry, a few general common denominators can be suggested. Where anadromous populations of charr occur, closely related sympatric populations are rare. Evidently, anadromous charr are ecological generalists and make use of the whole lake environment for various life history stages. It appears that gene flow is promoted by anadromy. Thus, the first stage of divergence is promoted by isolation of lakes from anadromous populations. The second important factor promoting divergence into distinct ecological forms is the absence of other fish species or, if other species are present, the charr is the dominant species in numbers and biomass (the top trophic organizer). The third factor concerns the lake volume and niche diversity. The larger and more diverse the environment, the greater the selective pressures for divergence to fill different niches and the greater the opportunity to maintain reproductive isolation by spatial-temporal separation during spawning.

Concerning the probability of sympatric speciation, the most common divergence in charr populations is into benthic and pelagic forms. Most benthic populations appear similar to the parr stage of pelagic or anadromous charr. That is, a form of neoteny or paedogenesis is suggested. The provocative ideas of Balon (1984) concerning juvenilization and precocial development is pertinent here, but I limit my timorous flirtation with Balon's theories to their pertinence to the question of sympatric speciation of Arctic charr, not to all life on earth or to a grand unifying world view.

There are many common convergent features found in numerous sympatric pairs of benthic and pelagic charr. In comparison to the pelagic form, the benthic form typically exhibits a slower growth rate, earlier age at maturation, and a shorter lifespan. The benthic form retains parr-like coloration throughout its life and does not develop the bright adult coloration typical of S. alpinus. At more advanced stages of divergence, the following morphological adaptations for benthic feeding become apparent: shorter, fewer gillrakers; blunt snout; thick, short maxillary; and subterminal jaws. Exceptions can be noted for these trends for divergence into benthic and pelagic populations. For example, if there is an abundance of large-size benthic organisms such as snails, the benthic form may attain a large size and an old age. If planktonic food is very sparse, the pelagic form may be dwarfed and reproduce at a younger age. In Lake Windemere, England, the two forms of charr significantly differ in their number of gillrakers with means of 21 and 25 (Ferguson 1981), but other morphological characters and life history traits do not show clearcut differences.

Although there are exceptions to the rule, the relative consistency of the trends of differentiation of benthic and pelagic charr populations in sympatry would superficially suggest that two ancient species are involved. That is, one could easily conclude that all benthic populations are monophyletic in origin from one species and that all pelagic populations monophyletic from the second species. All attempts cited above to verify this assumption, however, have failed. With the exception of papers emanating from Drottningholm, all recent studies cited, including the work of Anderson et al. (1983) which examined electrophoretic variability of enzymes produced at 52 gene loci in Swedish charr, have agreed that all of the charr of Scandinavia, Great Britain, and Iceland are derived from one recent common ancestor in postglacial times.

There can be some semantic problems for more precisely defining "one common ancestor" and its history of dispersal to give rise to all present charr populations of Scandinavia, Great Britain, and Iceland. I assume the common ancestor to all of the present subspecies _S_. _a_. _alpinus_ had essentially acquired its basic morphological characters and modal numbers of vertebrae, gillrakers, pyloric caeca, etc., by the beginning of the last glacial epoch or about 60,000-70,000 years ago. During the glaciation, I assume that several glacial refugia were used where slight genetic differentiation could occur, such as changes in allelic frequencies for esterase.

Some credence to the hypothesis of local differentiation in different refugia can be found in Figure 3 of Nyman et al. (1981), depicting a predominance of the esterase 100 allele in populations of southern Sweden (their species "salvelinus"). This led them to assume that movement from a southern refuge accounted for one of the three "ancient evolutionary lineages." Unfortunately, they failed to test this theory, which could have been simply accomplished by counting gillrakers.

Charr populations of the Alpine lakes of central Europe are characterized by modal and mean values of 27-29 gillrakers which differentiate them (as the subspecies salvelinus) from the subspecies alpinus which has modal and mean values of 23-25. The one charr specimen that I have examined from Lake Vattern in southern Sweden has 29 gillrakers. The range of counts I have for hundreds of alpinus specimens from throughout their distribution is 21-28. Thus, if gillraker counts were given by Nyman et al. (1981) for samples depicted as red dots in southern Sweden, the hypothesis of charr from a central European (or "southern") refuge invading Sweden in postglacial time could have been tested. Another opportunity to test the "salvelinus" in Sweden hypothesis was missed during the 1986 workshop of the "International Society of Arctic Charr Fanatics" which was held in central Europe, associated with Alpine lakes of Switzerland, Austria, and West Germany. The fanatics from

Drottningholm could have taken a "busman's holiday" and brought their electrophoretic equipment along to run esterase profiles of topotypes of the taxon salvelinus to compare with what they call salvelinus in Sweden. A more significant opportunity was missed by not obtaining an electrophoretic profile of the tiefseesaibling, S. profundus, of Lake Constance, which is by far the most morphologically divergent charr in Europe (or in the whole S. alpinus species complex).

Before moving on to interspecific problems, the matter of making decisions on characters and character states as population markers (intraspecific) or phylogenetic markers (interspecific) should be elucidated with an emphasis made on the role played by taxonomic experience and professional judgment. To do this, I present some quotations from Nyman (1984) in an attempt to more precisely focus on our area of disagreement regarding the confusion of population markers and phylogenetic markers. Nyman wrote, "Taxonomy is not a matter of whether osteology holds the key to sound systematics, but rather the drawing together of conclusions from whatever methods provide convergent results...No taxonomist would give equal taxonomic weight to the numbers of rays found in a particular fin compared to the number of fins when separating two species of fish...By what divine power are we endowed with the ability to say that all polymorphism exposed by electrophoresis has equal resolving power as population and species discriminants?...Taxonomic arrangements are the result of human reasoning; thus, they only approximate translations of biological truth."

Taking these statements in order, I would fully agree with the first statement that all available evidence should be used for recognizing and diagnosing a species and to reflect the species most correct phylogenetic relationships. I would only add that to do this most correctly requires considerable taxonomic and systematic experience with the group with which one is working. The second statement illustrates this point. Typically, the number of fin rays, for example, in the anal fin is an intraspecific character, useful for denoting slight genetic divergence, whereas the number of fins (for example, absence of anal or pelvic fins) typically denotes an ancient phylogentic event, perhaps monophyletic for a whole family of fishes. There are many notable exceptions to the rule, however, of which an experienced and skilled taxonomist would be aware. For example, in the enormously diverse family Cyprinidae, certain evolutionary lines have evolved a clupeid-like body form (bream-like species) which results in a high number of anal rays (for example, 14 to 17) in comparison to numerous species and genera of minnows and chubs. In this instance, anal fin number may be diagnostic as a marker for a very ancient phylogenetic divergence characterizing a genus or a group of genera in a tribe of Cyprinidae. On the other hand, an experienced taxonomist would be aware that pelvic fins may be lost at the population level in species of pupfish (genus Cyprinodon) and stickleback (Gasterosteus) with every little genetic change. In such situations the professional judgment of an experienced taxonomist is necessary for proper interpretation of patterns and the weighting of character states in relation to deciding if they are population markers or phylogentic markers. Blindly following an inflexible rule which instructs that the number of fins is always a phylogentic (interspecific, intergeneric, interfamiliar) marker and that the number of fin rays is always a population (intraspecific) marker can result in gross systematic and taxonomic errors. An analogy here concerns the blind and unbending application of the biological species concept to the effect that all sympatric and reproductively isolated populations must be recognized as species, because the character state of reproductive isolation denotes an ancient phylogentic marker. Although the biological species concept and degree of reproductive isolation is still, in my opinion, the most powerful and informative characterization of a species, it cannot be unduly weighted in complete isolation from all other evidence (or, in the words of Nyman, "the drawing together of conclusions from whatever methods provide convergent results"). From what is presently known of local differentiation in animals in general and salmonid fishes in particular, it is obvious that the character of sympatry with reproductive isolation can evolve in a relatively brief period of evolutionary time with extremely slight genetic differentiation. The simplistic rule of reproductive isolation as the sole or dominant criterion for species recognition is no longer accepted by most contemporary evolutionary biologists, systematists, and taxonomists who recognize that additional criteria are necessary (Coyne et al. 1988). It should also be recognized that in the 1940s and 1950s during the great predominance of the biological species concept, the leading authorities on animal evolution and distribution such as Mayr, Simpson, and Darlington all regarded the theory of continental drift to be in the realm of science fiction, a theory that only the lunatic fringe of science believed in.

Nyman's third statement inquires on the divine power that allows a taxonomist to decide if a character state (such as protein polymorphism) is a population marker or a species marker. The divine power concerns brain development in *Homo sapiens*, more specifically the cerebrum or gray matter of the brain that endows humans with reason and reflective judgment to evaluate evidence. I would point out, however, that no amount of divine power can force one to use reason and reflective judgment if it conflicts with a deeply entrenched belief system.

In reference to allelic frequencies of esterase and specifically to the question of the 100 allele and the 115 allele as population or phylogenetic markers, a rational decision on the matter should not be difficult. In my last paper (Behnke 1984), I pointed out that 209 of 212 (98%) of northern Dolly Varden, *S. malma malma*, studied from 13 Canadian Arctic sites possessed the 100 allele. If, indeed, the dichotomy between the 100 and 115 alleles was a monophyletic, phylogenetic event, signifying the separation of two ancient evolutionary lines, then the charr I recognize as *S. malma malma* would be a synonym of Nyman's *S. stagnalis*. The test of esterase alleles as a population or a phylogenetic marker can be easily tested by an experienced taxonomist. If, indeed, the evolutionary line leading to *S. malma malma* is characterized by the 100 allele, then all of the morphological and anatomical characters evolved in this line and used by experienced taxonomists to diagnose the Dolly Varden charr would also be associated with the 100 allele as phylogenetic markers. It then follows that if the 100 allele in S. malma malma of Arctic Canada and the 100 allele in Swedish charr designated as S. stagnalis by Nyman denote monophyly from one common ancestor evolutionary line, then an experienced taxonomist could readily detect S. malma characters in Scandinavian charr identified as S. stagnalis by Nyman. I would admit to modest experience in the examination of fish specimens to evaluate population marker characters and phylogenetic marker characters. I can categorically state that I have found no evidence to suggest any influence of the Dolly Varden species in the present genetic composition of norther European-Scandinavian charr populations. Thus, it should be clear that change in esterase frequencies among charr populations, particularly in the subspecies S. a. alpinus, denote population differentiation, not ancient monophyletic phylogenetic markers of the type necessary to diagnose species. The most correct decision on character states as population or phylogenetic markers is more a matter of professional judgment using common sense than of divine inspiration. This brings me to Nyman's last quoted statement that taxonomic arrangements result from human reasoning (which is fallible) and can only approximate biological truths. First, I would point out that any absolute biological or scientific "truth" is unknown and unattainable to the human mind. I agree that we attempt to approximate the truth of phylogenetic branching in a system of classification. How well we do this depends on the experience and expertise of the taxonomist and the use of reason, common sense, and professional judgment to sort out patterns of diversity and to fairly evaluate all of the evidence.

Interspecific Problems

Systematic and taxonomic problems associated with interspecific diversity concern the identification of patterns with emphasis on correct interpretation of pattern diversity to denote major phylogenetic branching (species identification) by separation of patterns into primitive (plesiomorphic) characters and derived (apomorphic) characters. As discussed above, the overwhelming evidence demonstrates that alleles at esterase loci, life history differences, and sympatric occurrence with reproductive isolation are not phylogenetic markers in Arctic charr evolution, because they are subjected to rapid and convergent evolution. These patterns lack the concordance necessary to make them, in combination, phylogenetic or species markers. For example, if all pelagic piscivorous charr possessed the esterase 100 allele and all benthic charr possessed the 115 allele and these two forms occurred in sympatry throughout northern Europe, then there would be little doubt that they represent two ancestral species; and esterase alleles and life history types would be valid species markers. A review of all of the evidence, however, clearly shows this not to be true. What then are phylogenetic markers? In my previous papers on charr (Behnke 1980, 1984), I pointed out that there were limited numbers of morphological characters used in Salvelinus taxonomy and that none of these characters have a sound basis for determining character states as primitive or derived. It is possible to clearly differentiate all Dolly Varden charr classified as S. malma, from all S. alpinus based on a combination of several morphological and meristic characters, plus the "character" of sympatric occurrence between the two species throughout a vast area of Asia and North America. Because of the lack of knowledge on primitive and derived character states, however, it was not possible to authoritatively place the branching sequence of S. malma evolution within the subgenus Salvelinus. In my last paper (Behnke 1984), I mistakenly aligned malma with the leucomaenis-confluentus branch of the subgeneric phylogeny rather than with the alpinus complex branch.

I had also speculated what new contributions might be expected from biochemical genetic studies and karyology to produce better phylogenetic markers for an improved classification. During the past year, Ruth Phillips has kept me informed on her results of detailed analysis of the karyotypes of several species of charr (information presented at this symposium by Phillips). Without recounting Phillips' work, which is given in this proceedings, I will only state that, in combination, karyotype arm number, morphology of individual chromosomes, and the characteristics of the nuclear organizer region (NOR) on the chromosomes are the most powerful phylogenetic markers yet discovered for interpreting branching sequences at the generic and subgeneric levels.

Based on the karyotypes of North American lake charr (S. mamaycush, subgenus Cristivomer), the North American brook charr (S. fontinalis, subgenus Baione) and S. leucomaenis and S. confluentus of the subgenus

Salvelinus, the primitive common ancestor is assumed to have had a diploid number of 84 with 100 chromosomal arms. The diploid number of 84 was reduced to 82 in S. leucomaenis (derived condition) and further reduced (further derived) to 78 in S. confluentus (and its assumed sister species, the stone charr of Kamchatka, S. albus), but all of the leucomaenis-confluentus line of evolution retain the arm number of 100. The phylogenetic branch leading to Dolly Varden and Arctic charr reduced the arm number to 98 and also acquired a uniquely derived chromosome that differentiates malma and alpinus from all other phylogenetic lines (a phylogenetic marker chromosome).

With these new phylogenetic marker characters, it now appears clear that the initial dichotomy within the subgenus Salvelinus led to the leucomaenis-confluentus group on one side and malma-alpinus on the other. Much is yet to be learned, however, before the relationships among all forms I currently recognize as malma and alpinus are better understood. For example, the southern form of malma in both North America and Japan has a diploid number of 82 chromosomes, whereas the northern form of Dolly Varden (S. malma) and all forms of alpinus yet examined have a dipliod number of 78. There are two alternatives to explain these differences in relation to phylogeny and classification. If the diploid number of 78 possessed by S. alpinus and S. malma malma is a monophyletic event, occurring after a common ancestor separated from the southern Dolly Varden line, then the southern Dolly Varden would be recognized as a full species. The species recognition between northern Dolly Varden and Arctic charr would not be affected in this case, but the northern

Dolly Varden would share a more recent common ancestry with _alpinus_ than it does with the southern Dolly Varden.

I currently prefer the second alternative which retains the most recent common ancestry between northern and southern Dolly Varden and denotes that the transition from 82 chromosomes to 78 occurred independently in northern Dolly Varden and in Arctic charr --- but one must maintain an open mind on such matters where the present evidence is far from conclusive.

Concerning documentation of phylogenetic markers that should remove any vestige of doubt regarding the clearcut separation of _malma_ and _alpinus_ as valid species, I was elated when Tony Gharrett (University of Alaska, Juneau) informed me at this symposium of an electrophoretic study in cooperation with Japanese workers on Alaskan Dolly Varden (southern form of Dolly Varden) and Arctic charr. The study examined the products of 27 loci in 100 specimens of Dolly Varden from several localities and 30 charr from Karluk Lake (the Taranetz form of Arctic charr common to the Chukokst Peninsula and to western Alaska). Complete separation of alleles was demonstrated (fixed difference) for the Aat enzyme (believed to be Aat-4) between the two species and the dominant allele for the Llp enzyme differed between the species. Kartavtsev et al. (1983) had previously found a fixed difference between the northern form of Dolly Varden and the Taranetz form of Arctic charr of the Chukokst Peninsula for the Acph-1 enzyme and significant frequently differences for Idh and Est-2.

Now that a breakthrough has been made on documentation of phylogenetic markers, I would hope that similar studies rapidly proliferate to include all diverse forms (subspecies) of alpinus and malma from throughout their ranges. If only a small fraction of the electrophoretic studies made on Scandinavian charr had been done on malma, alpinus, and, particularly, leucomaenis (which, electrophoretically, remains essentially unknown), we should now be in the final stages of delineation of species and interspecific relationships in the subgenus Salvelinus.

K. A. Savvaitova (University of Moscow) and her students have long been the major proponents of the polymorphic species concept which considers S. malma to be one of the polymorphic forms of S. alpinus (that is, malma is a synonym of alpinus, according to the Moscow "school of charr sytematics"). Just prior to coming to the present symposium, I visited with Savvaitova in Moscow to see if she may have altered her opinion. I must state that, as with the Drottningholm fanatics, I consider K. A. Savvaitova to be an honest and decent person and a good friend in all non-Salvelinus matters; but our disagreements will continue. She told me that she will publish a book on charr next year (that will continue to treat malma as a polymorphic form of alpinus). We both agree that "solutions for charr problems" will require international cooperation (Savvaitova 1988). Concerning the continuation of problems and controversies on such matters as how many species of charr are indigenous to Scandinavia (one vs. three) and the validity of S. malma, I recognize that once certain dogma becomes deeply entrenched into a belief system, this dogma, no matter how false, is not likely to be overcome by any amount of evidence and reason to change the minds of the true believers. For consideration of the validity of the bases of controversies, I would draw an analogy to one belief system whose dogma states that 2 + 2 = 3 and to a second belief system with a dogma of 2 + 2 = 5. No amount of evidence that 2 + 2 = 4 is likely to sway the true believers of either system, but would such a disagreement be sufficiently worthy to be called a controversy or a problem?

Intersubgeneric Problems

Robb Leary (University of Montana) in his letter of June 13, 1986, kindly sent me unpublished data (in part) on his electrophoretic analysis of *Salvelinus*, including species representative of all three subgenera. These include data on up to 42 protein loci for *S. malma* and *S. alpinus* from Alaska, *S. confluentus*, *S. fontinalis*, and *S. namaycush*. Nei's genetic distance (D) among the subgenera is approximately .3 with *confluentus* (subgenus *Salvelinus*) linking to *Cristivomer* at the lowest level. This electrophoretic evidence closely agrees with the present subgeneric classification, but, as mentioned, a comprehensive electrophoretic profile of *S. leucomaenis* is lacking; and this creates a significant gap for a better understanding of its position in relation to the subgenera *Salvelinus* and *Cristivomer*.

It is of interest that of the enzymes Leary examined, no consistent differences differentiated Alaskan Dolly Varden (assumed to be southern form) from Alaskan Arctic charr (assumed to be Taranetz form of charr), but distinct differences are apparent with electrophoretic data from Swedish charr (S. alpinus alpinus). For example, at the Aat-1 locus, Swedish charr appear to be fixed for the 100 allele (which is also the predominant allele of Alaskan Dolly Varden from the Fox River), whereas the few specimens of Alaskan Arctic charr from East Finger Creek and from Dolly Varden Lake were fixed for the 54 allele (similar to S. confluentus). The Alaskan charr are fixed for the 103 allele at the Gpi-3 locus (96% in Alaskan Dolly varden), whereas Swedish charr (also fontinalis, namaycush, and confluentus) are fixed for the 108 allele at this locus. It is obvious that comprehensive electrophoretic studies based on diverse forms of charr from diverse geographical areas will be necessary before the evidence electrophoresis can be fairly evaluated for its contribution toward a better characterization of subgenera and relationships within subgenera (and before intelligent discussion on character states as population markers of phylogenetic markers is possible).

It is also obvious that international cooperation will be necessary to carry out such a comprehensive project for the Holarctic region. In relation to this, perhaps the International Society of Arctic Charr Fanatics can plan for "busmen's holidays" for their future meetings to obtain electrophoretic profiles of charr from key geographical areas. One such area that is of the highest priority for karyological and electrophoretic studies of its charr is Lake Elgygytyn on the Chukokst Peninsula.

In the last charr volume (Behnke 1984), I called attention to a highly divergent new charr species recently described from Lake Elgygytyn as S. elgyticus, the smallmouth charr. In the abstracts of the present symposium, a new genus and species is described for the longfin charr of Lake Elgygytyn by Chereshnev and Skopets. Unfortunately, neither author attended the symposium to present the paper on this "discovery of the century." I must again raise a question concerning the rules of zoological nomenclature and publication of new names. In the abstract, Chereshnev and Skopets provide an adequate description of the diagnostic characters of this new charr and give a new genus and species binomial combination for it. They cite Chereshnev and Skopets (1989) as the original describers of the new genus and species (K. A. Savvaitova told me that Chereshnev and Skopets had submitted the description for publication in Voprosy Ikhtiologii). The matter of original description, publication, and publication date relates to the question of the status of the "Abstracts of the International Symposium on Charrs and Masu Salmon, October 3-9, 1988, Hokkaido University, Sapporo, Japan." Is this a "publication" under the rules of international nomenclature? Article 8 of the code describes what constitutes a publication..."(1) be reproduced in ink on paper by some method that assures numerous, identical copies; (2) be issued for the purpose of scientific, public, permanent record; (3) be obtainable by purchase or free distribution; and (4) not be reproduced or distributed by a forbidden method" (none of the "forbidden methods" apply to the Abstracts).

The issue of the original publication and publication date of the genus and species proposed by Chereshnev and Skopets is a technical question concerning the international code of zoological nomenclature. The species is real and distinctly different from any living species of Salvelinus, but I would certainly not recognize a new genus for it. Besides the description given in the abstract, a further description was given by Chereshnev and Skopets (1988) where they called this new species the "glubokovodny" (deepwater) charr (it was captured at depths between 50 and 105 m). Some diagnostic characters include vertebral counts of 55-58(56.3), the lowest reported in Salvelinus; 44-63(54) gillrakers, by far the highest known number in the genus; 50-70(58) pyloric caeca, teeth on the vomer absent or reduced to one or two; supraorbital bones absent; orbitosphenoid greatly reduced or absent; and pelvic appendage greatly reduced or absent. In relation to phylogenetic taxonomy, I would point out that all of the diagnostic characters of this new species are derived characters, all can be derived within the limits of the genus Salvelinus, and none can be assigned to a branching in the phylogeny prior to the monophyletic origin of Salvelinus (Figure 78 of Kendall and Behnke 1984).

Evidently, Chereshnev and Skopets were aware of this problem, and in their abstract they propose to revise Salvelinus into the tribe Salvelini with Cristivomer, Baione, Salvelinus, and the new genus making up the tribe. Such a radical revision is premature and not in concordance with generic classification of Salmonidae. The new species, as presently described, does not fit in any of the three currently recognized subgenera and, as such, could be recognized as a new subgenus. The last type species of any present subgenera was described in 1814 (S. fontinalis) or 175 years ago, which denotes the significance of this new discovery.

Debate on the most correct classification of the longfin or deepwater charr of Lake Elgygytyn aside, the new species is the most important species in regards to obtaining critical karyological information (diploid number, arm number, NOR, and chromosome morphology) which would be basic to any phylogenetic allignment in the genus (also necessary for the smallmouth charr, S. elgyticus). A comprehensive electrophoretic profile would also be a necessary standard to assess relationships between and within subgenera (may serve as an "outgroup" comparison). Any electrophoretic evidence on Lake Elgygytyn charr, however, would be difficult to assess in relationship to subgeneric classification until the patterns of S. leucomaenis are known and diversity of patterns in S. alpinus and S. malma are known so that certain key loci can be identified as valid phylogenetic markers in Salvelinus evolution.

Conclusions

The problem of intraspecific classification of charrs can be greatly lessened if it is recognized that sympatric occurrence with reproductive isolation is not, in itself, a valid criteria for species recognition in Arctic charr. Also, life history type and esterase allelic frequencies are population markers, not phylogenetic markers. If population genetics is not confused with evolutionary genetics, the controversy regarding the classification of north European charr as one modern subspecies or as three ancient species will be resolved.

Interspecific problems still relate to a better characterization of Arctic charr and Dolly Varden charr. There can no longer be any reasonable doubt that the Dolly Varden is a separate species, but the relationships of the southern and northern forms of Dolly Varden and of the Taranetz and high Arctic form of Arctic charr (form typically with 25-30 gillrakers) are ill-defined. When comparative electrophoretic evidence becomes available (and possible evidence of reproductive isolation), the southern Dolly Varden and the Taranetz charr might be recognized as full species.

The new charr from Lake Elgygytyn described by Chereshnev and Skopets is the most significant charr discovery of the century (or since 1814). As such, it is the most important species for the application of modern techniques of karyology, electrophoresis, mitochondrial DNA, ribosomal DNA, etc. With such new information on the Lake Elgygytyn charr, as well as on other representative subgenera, particularly S. leucomaenis, a phylogenetic classification of the genus with much greater information content than is now possible can be achieved. I do not believe, however, that all problems and controversies can be resolved by the time of the next charr symposium because of qualitative differences in mindsets and belief systems that are impervious to resolution.

Although my contribution concerns systematics and taxonomy, a final word must be said on the practical issue of preservation of biodiversity (intraspecific) diversity in charr. Charr, in general, are highly vulnerable to environmental change. They are typically the first species to be lost from environmental disruption. As such, the proper management of charr concerns the recognition and identification (but not necessarily as taxa) of diversity for ordering of priorities for a goal of preserving as much diversity as reasonably feasible. This should be the ultimate and highest goal for systematic studies --- and, on this point, I believe I am in agreement with the Drottningholm school. A point that must be kept in mind, however, is that the most significant diversity in life history types such as anadromy, planktivore, benthivore, and predator, comes about with very little genetic differentiation and often not detectable by electrophoresis. A diversity preservation program based on a quantitative metric of allelic difference is much too crude to provide an adequate basis for such a program --- much significant diversity would not be detected by quantitative methods alone. To illustrate this point, almost all wine made from grapes comes from one species of grape, *Vitus vinifera*. If no quantitative genetic difference could be detected in the vines on Baron Rothchild's estate compared to the vines of a "vin ordinaire" plantation, should they then receive no special priority in a program to preserve the diversity of *Vitus vinifera*? Is it not reasonable to recognize and use a more qualitative evaluation in such cases?

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[ca 1990]

BRACHYCENTRUS OCCIDENTALIS CONSUMPTION BY BROWN TROUT IN THE ARKANSAS, RIVER, COLORADO - A Test of Allen's Paradox

INTRODUCTION

K.R. Allen's (1951) classic study of the ecology of brown trout in the Horokiwi stream, New Zealand, has initiated a considerable amount of controversy over the past three decades. The controversy is centered around Allen's calculation that brown trout consumed between 40 and 150 times the standing crop of benthic macroinvertebrates present in the stream at one time. This apparent contradiction was termed *Allen's paradox* by Hynes (1970), and has been tested by several investigators (see review in Allan 1983). Although there has been considerable criticism of Allen's original sampling techniques (eg. coarse mesh size and inadequate sampling of the hyporheic zone) as well as food habit analysis of the trout, refinement of these techniques has resulted in the same general results (Allan 1982a). Because it is impossible for predators to consume more than the available food source in a particular ecosystem without disastrous effects, the error involved in calculating prey production, predator consumption and estimation of prey and predator population size may separately or together bias the results. Probably the largest error when forming this relationship is the use of standing crop as a measurement of prey production. Waters (1977) found that benthic macroinvertebrate production can be 10 times the standing crop over a year's period, with those invertebrates exhibiting multiple generations per year (eg. Baetid mayflies) having a considerably higher P/B ratio. With the refined secondary production calculations available today, it would seem possible to reduce this error when comparing predator-prey relationships. However, these estimates involve an extremely intensive sampling scheme and exhaustive measurements of individuals to determine growth rates and ultimately production. Another factor which is over looked when determining Allen's paradox is that we are measuring standing crop or production while it is being reduced by the predator. Although studies have shown that trout removal does not have an effect on benthic densities (Allan 1982b), the reduction of prey densities from the substrate (epibenthic feeding) may reduce competition and as a result increase growth rates. As a result, it may appear that predators are consuming more prey than is present because we are measuring *net* productivity rather than *gross* productivity. However, due to a reduction in competition, prey productivity may be increased.

The caddisfly *Brachycentrus occidentalis* was chosen for this experiment due to its low propensity to drift into and out of the study area, and also because it is univoltine and thus many of the assumptions and errors associated with production would be avoided. In addition, *B. occidentalis* was found to be the principal food source of brown trout in the study area (Winters 1988).

METHODS

B. occidentalis samples were collected monthly, from July 1982 to August 1983, at an elevation of approximately 2,133 meters. Five samples were collected during each month, with a Surber sampler. Individuals were enumerated, dried at 60°C and weighed. Densities ($\#/m^2$) and biomass (g/m^2) were then calculated for production determination. *B. occidentalis* production was calculated using the following formula:

$$B = k\overline{P}$$

where B = production rate in g/m^2/interval

k = instantaneous growth rate in g/interval

$\overline{P}$ = mean population density in g/m^2

Instantaneous growth rate for each time interval was calculated as the natural logarithm of the ratio of the mean size at the end of the period to the mean size at the beginning of the period. Waters (1965) found that using the mean size instead of the maximum size may result in an underestimate of production, as numerous early instars would result in a low calculated biomass estimate.

Brown trout population dynamics were calculated from Nehring and Anderson (1983), at the Loma Linda site. Estimates did not include any fish less than 160 mm in length and were only calculated during the spring of the year. Due to the relative stability of the population of trout in terms of age class structure and size, it was assumed that the production/biomass (P/B) ratio was 1 for the entire year. All calculations were conducted on a *per meter* basis, and were separated into three intervals based on water temperatures. The first interval was the warm period, from July through October, when mean water temperature was 13.8°C. The cold period was from November through February, when water temperatures averaged 1.6°C, and the intermediate temperature period was from March through April, when mean water temperature was 6.1°C. *B. occidentalis* emerged during May, and was thus eliminated from the analysis. Digestion rates of *B occidentalis* larvae were derived from the evacuation rates of the caddisfly *Arctopsyche sp.* from Reimers (1957). It was assumed that complete digestion was necessary before satiation could be achieved again, which may introduce a bias towards underestimating consumption. However, this bias may have been offset by the possible longer digestion time of the *B. occidentalis* larvae due to the intact cases ingested.

RESULTS AND DISCUSSION

There was a significant relationship ($r=0.99$ $p < 0.10$) between mean water temperature and *B. occidentalis* production (Table 1). Productivity was greatest during the summer and fall months, with subsequent winter cessation. Productivity increased during the warming period in the spring prior to pupation and emergence. Hauer and Stanfod (1986) also found elevated growth rates of *B. occidentalis* during the summer and fall months, with larvae remaining active until pupation the following spring. The relatively low productivity values, especially in winter and spring, may be a result of an underestimation of production by using the mean weight of organisms rather than the maximum weight (Waters 1965). However, these values are relative, and it is doubtful that productivity would deviate far from zero during the low temperature period in winter.

The mean biomass of *B. occidentalis* larvae ingested by the brown trout was greatest during the spring sampling period, with the lowest values recorded in the fall (Table 2). However, due to the considerably faster digestion rate during the summer and fall months, the biomass of consumed *B. occidentalis* larvae is considerably higher in the summer and fall period than both the winter and spring months combined. The increased consumption of

TABLE 1: Temperature, standing crop and production values for *B. occidentalis* in the Arkansas

Time Interval	Mean Temp (c°)	*B.occ.* Biomass (g/m^2)	Production g/m^2/interval
July-Oct	13.8	0.1863	0.47
Nov-Feb	1.6	0.1134	-0.13
March-April	6.1	0.2334	0.09

TABLE 2: Brown trout foraging results on *B. occidentalis* larvae in the Arkansas River, Colorado.

Time Interval	Mean Stomach Contents	Trout Density ($\#/m^2$)	Ingested Biomass (g/m^2)	Digestion* Rate (hrs)	Biomass consumed (g/m^2/interval	Biomass consumed / Mean standing crop (Ratio)
July-Oct	0.006	0.048	0.003	16	0.549	2.947
Nov-Feb	0.04	0.048	0.002	70	0.082	0.723
Mar-April	0.08	0.048	0.004	24	0.244	1.045

* Digestion rate of the caddisfly *Arctopsyche sp.* (Reimers 1957).

B. occidentalis occurred during a period when larvae were numerous as a result of hatching from eggs. *B. occidentalis* larvae averaged 246 org/m^2 during the summer and fall period while averaging only 42.5 org/m^2 in the winter months. This trend follows a type III survivorship curve, where numerous offspring are produced, and mortality is initial high (Wilson and Bossert 1971). This may be a period when predation is actually beneficial, by "cropping off" excess larvae and reducing competition through predator attrition. The relatively low consumption rates following the summer and fall high mortality period may be a result of the reduced abundance of *B. occidentalis* as well as reduced metabolism by the brown trout.

The high predation-standing crop ratio during the summer-fall period may to be misleading (Table 2). High mortality is expected during this period according to the type III survivorship curve, and trout predation may be an integral component, through consumption. Production during this period is still considerably higher than the remainder of the year. This may indicate that although the brown trout may reduce the density of *B. occidentalis*, they may in fact be reducing competition, increasing growth and ultimately increasing production. As benthic densities of *B. occidentalis* attained relatively stable levels during the winter months, trout predation does not even account for the standing crop of *B. occidentalis*. As production of *B. occidentalis* and water temperatures increases in spring, densities are apparently low enough that trout predation approaches instantaneous standing crop. However, productivity continues during this period.

Spatial segregation may also be important in the relationship between brown trout predation and *B. occidentalis* production. Brown trout were sampled primarily in runs and small pocket water just downstream of major riffles, and in small pockets with adequate depth within the riffles (pers. obs.). *B. occidentalis* larvae, however, were sampled only in riffle areas where trout were rare. The abundance of *B. occidentalis* larvae during the summer and fall months may force them to continually migrate into areas where the brown trout can utilize them as a food source. When densities of *B. occidentalis* are reduced in winter and spring, the number available to the brown trout may be less, due to spatial segregation and reduced densities. As a result, the stabilized densities of *B. occidentalis* larvae in the riffle areas following the initial population influx may not be significantly impacted by the brown trout predation.

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DESCRIPTION OF NATIVE REDBAND TROUT FROM OKANAGAN NATIONAL FOREST, WASHINGTON

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There has been considerable confusion and controversy concerning the taxonomic and evolutionary status of the rainbow-like trout (= redband trout) native to the middle and upper Columbia River basin (east of Cascade range) and adjacent basins. To place the description-identification of the Okanagan N.F. specimens in perspective in relation to the diversity among groups of redband trout, the following points are made. The facts of historical distribution of salmonid fishes in the Columbia River basin reveal that only cutthroat trout (and bull trout) are native above barrier falls on the major tributary rivers -- Kootenay, Pend Oreille, Spokane, and Snake Rivers. This pattern indicates that no form of rainbow trout occurred in the Columbia basin before these barrier falls were created (dates not known with precision, but estimates of 30,000 to 60,000 years ago). Other areas isolated from the Columbia River basin -- Lake Chelan, Crab Creek drainage, and Waha Lake -- also had only cutthroat trout as native trout (Lake Chelan also has native bull trout). The native fish fauna of all of these areas isolated before the time of invasion by any form of rainbow trout, are undifferentiated from Columbia basin species. This evidence also supports the assumption that the time since isolation has been insufficient for differentiation to occur which would be recognized as different species or subspecies. Kokanee salmon are also not native to any of the above isolated areas lacking native rainbow trout. Certainly the rainbow trout species, Oncorhynchus mykiss, and sockeye salmon, O. nerka, existed 50,000 to 60,000 years ago, but why were they absent from the Columbia basin to gain access to the isolated areas is a great, unresolved mystery. Based on all evidence, by 50,000-60,000 years ago, when fishes should have had access to all of the isolated areas cited above, O. mykiss existed as several primitive evolutionary lines (collectively called redband trout) and a more advanced group, the coastal rainbow trout. The most ancient forms of redband trout are found in the Sacramento River basin. The first invasion of the Columbia River basin that left living descendants of the redband ancestor is probably associated with the trout native to the internal basins of southern Oregon (Fort Rock, Malheur, Catlow Valley, Warner Lakes, Chewaucan, and Goose Lake basins). About 35,000 years ago these present desert basins contained large lakes with direct or indirect connections to the Columbia basin. It was probably about this time when a redband trout from the Columbia River entered the Oregon basins (Minckley et al. 1986). The redband trout native to the Oregon basins is typical of Columbia redband trout in their morphological characters, spotting, coloration, parr marks, etc. The important distinction of the Oregon desert basins redband trout and other populations of redband trout long isolated (35,000 years?) from direct contact with the Columbia basin is that they possess the LDH B_2 100 allele (typical of coastal rainbows) rather than the 76 allelle characteristic of other redband populations of the middle-upper Columbia and upper Fraser River basins (Currens et al. 1990). That is, which invaded the area at the end of the east glacial period. As such, they should be similar to redband populations native to the Methow, Entiat, and Wenatchee river drainages.

Description of Specimens

Four collections, Windy Creek (N = 6 specimens), North Fork Toats Creek (N = 9), Middle Fork of Toats Creek (N = 10) and Hell's Hole (N = 10) are all in the Toats Creek drainage, tributary to the Silmikameen River, the major tributary to the Okanagan River. Because of proximity of populations within the Toats Creek drainage, it might be expected that there should be high similarity among the specimens of the populations sampled. Obvious differences in the size of the spots are apparent. Specimens from N. Fk. Toats Creek and Hell's Hole are typical of spotting found on "contemporary" Columbia redband trout -- relatively large, coarsely scattered over the sides of the body. Plate 17 of a "redband trout of the mid-Columbia basin" in Smith (1984) depicts this common type of spotting pattern. Specimens from Windy Creek (except largest specimen which has larger spots) and Middle Fork Toats Creek have smaller, more profuse spots. The lateral series scale counts are lower than expected for typical Columbia redband (140-150), ranging from a mean of 131 (Middle Fk. Toats) to 138 (Hell's Hole) (132 for Windy Creek specimens and 134 for N. Fk. Toats). These counts are more similar to Athasbacan redband, but are likely characteristic of Okanagan resident populations. Scales above the lateral line, range in mean values from 30 to 34, about typical of Columbia redband trout. Mean gillraker numbers are 18 to 19, slightly low but not abnormal for redband trout. All collections average 40 to 41 pyloric caeca, which is wholly typical of Columbia redband trout (coastal rainbow trout and hatchery rainbows typically have 50 to 60 pyloric caeca). One specimen from Middle Fork Toats Creek has a basibranchial tooth. Basibranchial teeth are a primitive trait, derived from a common ancestor with cutthroat trout. They occur in varying frequencies (ca. 5% of a population) in many redband populations.

Although brook trout were noted from a few sites, which denotes influence of past stocking, we did not detect any obvious influence from hatchery rainbow trout and assume that the samples from the Toats Creek drainage characterize the resident stream populations of redband trout native to the Okanagan drainage -- typical of Columbia redband trout in general but with a tendency for lower lateral series scale counts and smaller spots.

Five specimens from Sweat Creek, tributary to Granite Creek, a Columbia River tributary were also examined. Geographically, by drainage patterns, the Sweat Creek population can be regarded as remote and relatively long isolated from the Toats Creek redband trout. The S^w^eat Creek trout have small, profuse spots, indicating that perhaps this type of spotting is characteristic of upper Columbia-Okanagan redband trout. These specimens have the more typical scale counts in the lateral series expected for Columbia redband -- 136-150 (141); they also have typical pyloric caeca counts, 38-46 (41). One specimen has two basibranchial teeth. In total morphology, the Sweat Creek trout appear to represent a pure, native population of redband trout. 3 Sweat

As mentioned, we assume all of the populations examined are characterized by the LDH B_2 76 allele. If electrophoretic analysis is made in the future and the 120 allele is extant redband populations tracing their ancestry to ~~other~~ the earliest (?) redband invasion of the Columbia basin, cannot be distinguished from coastal rainbow trout (from which virtually all hatchery stocks of rainbows are derived) by electrophoresis although they are typical of Columbia redband trout in all other taxonomic characters. ✓ 1

The spread of the 76 allele through redband populations in the Columbia and upper Fraser basin evidently occurred toward the latter part of the last glaciation about 15,000 years ago when there was a direct connection between the upper Fraser basin and Columbia basin via the Okanagan River (McPhail and Lindsey 1986). Based on all of the above evidence, it would be predicted that native redband populations of Okanagan National Forest should be typical of the middle-upper Columbia, upper Fraser redband trout, in general, and possess a predominance of the LDH B_2 76 allelle.

A recent manuscript "Taxonomic study of the Athabasca rainbow trout" by L. Carl Clare; C. Hunt, and P. Ihssen, casts some doubts on the above scenarios of redband ancestral forms and times of invasions. It has long been known that a form of rainbow trout is native to the Athabasca River drainage of the Makenzie River basin (Arctic Ocean basin). The Athasbascan rainbow has been assumed to have resulted from a headwater transfer from the upper Fraser River basin at the end of the last glaciation -- ca. 15,000 years ago. If this were true the Athabascan "rainbow" should be virtually identical to the present redband trout of the upper Fraser. ✓ 2

Morphological examination of Athabascan native trout show typical redband characteristics -- elongated parr marks with supplemental rows, body covered with relatively large coarse spots, yellowish tints on body and well marked tips on dorsal anal, and pelvic fins. Their lateral series scale count of about 130-135 is low for typical Columbia-Fraser redband trout but not outside the range of variation. The electrophoretic patterns found for Athabascan trout, however, are unique, distinct from any known form of either redband or coastal rainbow trout. Because of this degree of differentiation, the authors conclude that the Athabascan trout gained access to the drainage from the upper Fraser basin prior to or at the beginning of the last glaciation, 64,000 years ago or earlier. If a previous unknown form of primitive redband trout occurred in the Fraser basin before 64,000 years ago, it should have also occurred in the Columbia basin, which deepens the mystery on the absence of any form of rainbow or redband trout from isolated areas where cutthroat trout and bull trout are the only native trout.

In summary, we now know of three ancestral groups of redband trout that may have possibly influenced native redband populations occupying various areas of the Columbia River basin -- the ancestral Columbia redband characterized by the LDH B_2 100 allele, the contemporary Columbia and upper Fraser redband with a predominance of the LDH B_2 76 allele, and the Athabascan redband, which was derived from an ancient transfer from the Fraser basin, and is characterized by unique alleles at the LDH B_2 (120 allele) and PEPA (111 allele) loci. All three forms share similar morphological features distinguishing them from coastal rainbow trout. Because of the absence of a glacial refuge in the area and lack of any evidence for isolation which would prevent gene flow, we assume that the redband trout native to waters of Okanagan N.F. represent the "contemporary" Columbia redband detected, then alleles at the PEPA gene locus should be looked at for possible ancestral connections to the Athabascan redband trout (we consider this as highly doubtful, however).

We hope to expand the preliminary mitochondrial DNA studies on rainbow and redband trout by Dennis Shiozawa (BYU) and Rick Williams (Boise St. Univ.). To date, three basic patterns of DNA have emerged, one of which might be designated as a Columbia (mid Columbia-Snake River) redband pattern. The degree of overlap and shared DNA patterns is great and no clear conclusions on ancestral-phylogenetic relationships can be made. With improved techniques allowing for more detailed examination of larger DNA segments, we are hopeful that DNA analysis can be a powerful tool to gain insights on evolutionary divergences and dispersal routes of ancestral forms.

In regards to most correct taxonomy of the Columbia-Fraser redband trout, which includes resident stream populations, large lake populations (commonly called Kamloops trout), and anadromous steelhead (east of Cascades), Williams et al. (1989) designated the "interior redband trout" as Oncorhynchus mykiss gibbsi. We believe, however, the correct subspecific designation is O. m. gairdneri. Although gairdneri is a synonym of O. mykiss at the species level, it is available for use as a subspecies. Jordan and Evermann redescribed gairdneri based on redband steelhead from the Columbia River (137-177 lateral series scales and 40 pyloric caeca clearly distinguish redband from coastal rainbow trout in the Columbia basin). Thus, the names gairdneri and gibbsi both refer to Columbia basin redband trout and following the rules of international zoological nomenclature, gairdneri is the older (1836 vs. 1856) and therefore valid name of the subspecies.

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ANALYSIS OF TROUT COLLECTED IN WYOMING DURING 1993

Robert Behnke

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This analysis concerns five collections of three subspecies: *O. c. pleuriticus* (Trail Creek and Spring Creek, tributaries to La Barge Creek), *O. c. utah* (Trespass Creek, tributary to Smith Fork), and *O. c. bouvieri* (Pumpkin Creek and West Fork Little Bighorn of Yellowstone basin). All samples, with the exception of Trail Creek, may represent pure populations of native cutthroat trout.

Green River Drainage

Twelve specimens from 89 to 200 mm TL from Spring Creek, a tributary to La Barge Creek (R116W, T29N, S15), appear to represent a pure population of *O. c. pleuriticus*. They are very similar to the fish of upper Rock Creek, another La Barge Creek tributary previously identified as pure *pleuriticus*. There is no indication in the spotting pattern of influence from either rainbow trout or fine-spotted Snake River cutthroat trout. All specimens have basibranchial teeth, 2-8 (4); scale counts are 175-201 (186) in lateral series and 40-45 (43) above the lateral line; gill rakers are 17-20 (18.7) and pyloric caeca are 31-40 (35).

The collection from Trail Creek, tributary to La Barge Creek (R116W, T29N, S8) contains eight specimens collected by Ron Remmick and four specimens collected by Paul Thompson, University of Wyoming student. Most specimens are small (68-161 mm TL) and difficult to make accurate scale counts, but repeated attempts agreed that the Trail Creek cutthroat trout have many fewer scales than expected in pure *pleuriticus*. Lateral series scale counts ranged from 158-174 (165) and scales above the lateral line are 34-41 (37) -- about 20 and 6, respectively, fewer than expected for pure *pleuriticus*. The low scale counts suggest influence of past hybridization with rainbow trout, but analysis of other characters revealed little or no rainbow influence. One specimen lacked basibranchial teeth but the others have from 3 to 13 teeth -- 90% occurrence of basibranchial teeth indicates very slight rainbow influence. Spotting patterns show no obvious hybrid influence except for one specimen with a few spots on top of the head (characteristic of rainbow trout). Gill rakers were 17-20 (18.7) and pyloric caeca were 28-40 (34.5). Evidently, a very slight, lingering influence of past hybridization is expressed mainly in the low numbers of scales and is not manifested or weakly manifested in all other characters. This population is designated as a B type and should not be used for transplanting or propagation; but, phenotypically it is a good representative of *pleuriticus* and, ecologically, it is of interest; it appears to maintain a healthy population in coexistence with brook trout, according to Paul Thompson. As such, it should be regarded as a very special type of B population.

Bear River Drainage

Trespass Creek, tributary to Smith Fork (R118W, T28N, S10) has 31 specimens 74-166 mm (second largest fish is 140 mm). Trespass Creek appears to have a "self-contained" (perhaps isolated by barrier) slow-growing population. Lateral series scale counts, 141-172 (154) and pyloric caeca counts, 28-42 (37) are more typical of *O. c. utah* of Bonneville basin proper than to other Bear River drainage *utah* (ca. 160-165 scales and 40-45 caeca, but accurate caecal counts could not be made on most of the specimens). All 31 specimens have basibranchial teeth, 2-9 (6). Scale counts above the lateral line are 34-40 (36.3) and gill rakers are 17-20 (18.0). Although the specimens are small and not well-preserved, there is no indication of a hybrid influence on spotting pattern and I believe they represent a pure population of *O. c. utah*.

Yellowstone Basin

Perhaps the most interesting and significant of the 1992 collections were sent by Bob McDowell from Pumpkin Creek and the West Fork of the Little Bighorn. The specimens are in poor condition and exhibit peculiar spotting so that I would not confidently pronounce them to be pure *bouvieri* without a larger sample of well-preserved specimens. The peculiarities, however, are not readily explained by a hybrid influence and there is a good probability that a pure population of native cutthroat trout inhabits these waters.

The two specimens from Pumpkin Creek are badly decomposed. One specimen lacks its head and the head has separated from the body of the other specimen (a large fish of about 380 mm with head attached). The nine specimens from the West Fork of the Little Bighorn (R90, T58, S34 NE) are better preserved, at least to the extent that meristic counts could be made. A note with these specimens gives the collection site as "below mouth of Pumpkin Creek," collected by Rush Lock, Earl and Pat Boyd of Sheridan. I assume that the trout in Pumpkin Creek and West Fork Little Bighorn are part of the same population.

The West Fork specimens range in size from 160-281 mm. These specimens have a distinctive spotting pattern of relatively large, very pronounced spots on the body. There is considerable variation in the arrangements of spots, some specimens have the spots mainly concentrated on caudal peduncle, others have spots rather profusely distributed anterior and below the lateral line. Comparing the "typical" spotting patterns illustrated in figures of my trout monograph, these specimens would appear as intermediate between the Yellowstone cutthroat (*bouvieri*) and Lahontan cutthroat (*henshawi*) with a touch of greenback cutthroat (*stomias*) thrown in. Most specimens express a unique type of spotting on the body right against the end of the skull, in the form of an arc of pronounced spots. Some specimens also have a pronounced spot at each nostril, but not on top of the head as is characteristic of rainbow trout. The meristic characters show no indication of hybridization. Ten specimens (9 + 1 head) all have well-developed basibranchial teeth, 5-21 (12.8). Scale counts in lateral series, 167-201 (180); above lateral line, 36-46 (39); and pyloric caeca, 37-53 (40.6), are typical of *bouvieri*. The gill raker counts are high, 19-24 (21.1), typical of Yellowstone Lake *bouvieri*. This might suggest the upper West Fork and Pumpkin Creek were originally barren of fish and a stocking of Yellowstone Lake cutthroat was made many years ago. Yellowstone Lake cutthroat trout have from 5 to 15 well developed posterior rakers on the first gill arch. All of the West Fork specimens have none or only a few vestigial posterior rakers, which rules out derivation from Yellowstone Lake trout. Also, Yellowstone Lake cutthroat average 22 basibranchial teeth and have a different spotting pattern.

Occurrence of pure populations of *bouvieri* in the Yellowstone drainage outside of Yellowstone National Park is very rare. I recall previously identifying only one pure population from the Bighorn drainage of Wyoming (South Paintrock Creek) many years ago.

The 1978 Stream Evaluation map of Wyoming shows Pumpkin Creek (just south of Montana line) and Mann Creek coming together and forming a "yellow line" (class III stream) to confluence with Little Bighorn River at the state line. I assume the "yellow line" is the West Fork of the Little Bighorn. Neither Pumpkin Creek nor Mann Creek are color coded (assumed to have no fishery value). At least until more accurate characterization can be made on these fish, I would paint a purple line (class I) on the map to denote high significance.

Historical Note

The Tongue River drainage is the easternmost tributary to the Yellowstone with native cutthroat trout. The abundance of trout in the Tongue drainage west of Sheridan may have played a role in sealing the fate of General Custer and his cavalry regiment on June 25, 1876, at the Little Bighorn. A book, "On the Border with Crook," written by Captain John G. Bourke, an aide to General George Crook, and published in 1891, recounts the wonderful fishing enjoyed by General Crook and his troops during the period of about June 19-25, 1876. After the battle of the Rosebud on June 17, Crook withdrew his troops to the base of the Bighorn Mountains and established a camp on Goose Creek, southwest of Sheridan while he sent for reinforcements. Crook was an avid sportsman who always had his fly rods and tackle along on his expeditions. He found superb fishing on Goose Creek and its tributaries. Crook caught 70 trout one day and sent out some of his men to get trout for the camp. Evidently, several hundred were brought in each day. The trout were up to about 14-15 inches according to Bourke, but larger ones were caught in some of the headwater lakes of the Goose Creek drainage in the vicinity of Cloud Peak. The lake fish were much more difficult to catch than the stream fish but weighed up to three pounds. Crook so enjoyed this sportsman's paradise that he dallied for eight days before finally getting back to business and moving north across the divide into the Little Bighorn Valley of Montana on June 26; by that time, it was too late for Custer and his troops.

With a bit of imagination, one can envision that a form of *O. c. bouvieri*, identical to the trout of Pumpkin Creek and West Fork Little Bighorn, played a major role in influencing the events that determined history. Might other such populations of this historically significant trout still occur in the Little Bighorn or Tongue River drainages?

ANALYSIS OF 1994 WYOMING CUTTHROAT TROUT COLLECTIONS

Robert J. Behnke

Three collections from the Green River drainage and three from the Bear River drainage were analyzed. All collections are from Lincoln County except for Fish Creek in Sublette County. The sample from the South Fork of Fontenelle Creek probably represents a pure population of O. c. pleuriticus, but the small sample size precludes more definitive determination. The other samples are "good" representatives of the subspecies pleuriticus and utah but indicate some lingering effects of hybridization in one or a few characters.

Green River Drainage, O. c. pleuriticus

South Fork Fontenelle Creek: R116 T25 S23; July 14, 1994; N=3; 156-220 mm TL. These three specimens appear to be pure pleuriticus. Lateral series scale counts 193-212(201), above lateral line 44-49(46) and pyloric caeca counts 32-39(36) are quite typical of pure pleuriticus. All specimens have 2-5 basibranchial teeth (3.3). Gill rakers 20-21(20.3). No indication of a hybrid influence but the population is represented by only three specimens. Because of small sample size, the rating is a tentative A.

Little Indian Creek: R117 T27 S33; August 12, 1994; N=8; 98-158 mm TL. Tributary Fontenelle Creek (? or La Barge). Specimens represent "good" cutthroat trout but not pure pleuriticus. Scale counts 153-196(174), above lateral line 32-45(38), pyloric caeca 34-48(41), gill rakers 18-21(19.6). One specimen lacks basibranchial teeth, seven speimens with 1-5(3.7) teeth. The largest specimen has spotting pattern resembling pure Green River form of pleuriticus, others are more variable in size, shape, and distribution of spots. One specimen with spots on top of head (a characteristic of rainbow trout). Overall, there is evidence of past hybridization with rainbow trout, but native cutthroat characters predominate. Grade B-.

Fish Creek: R115 T30 S29; August 4, 1994; N=9; 175-250 mm TL. Tributary in South Piney Creek drainage. Scale counts 183-196 (189) in lateral series, 38-45(42) above lateral line; caeca counts 27-33(30), gill rakers 19-21(19.8), basibranchial teeth 4-23(11.1). The meristic characters of these specimens could pass for pure pleuriticus, although basibranchial teeth are more numerous than expected. Spotting pattern appears too variable to represent a pure population. One specimen has large spots more evenly distributed over body, similar to the form of pleuriticus native to Little Snake River headwaters. Four specimens show indication of fine-spotted Snake River cutthroat influence. The spotting variation and, perhaps, the high basibranchial tooth number most likely reflects past hybridization with Snake River cutthroat. The hereditary basis for the large spotted specimen is difficult to explain, unless cutthroat trout of Yellowstone Lake origin had once been stocked in the vicinity. A hybrid influence from Yellowstone Lake cutthroat tends to increase gill raker number (toward 21) and basibranchial tooth number (toward 22). Yellowstone Lake cutthroat also have well developed rakers on the posterior side of the first gill arch. These specimens do have some development of posterior gill rakers but they are feeble. There is no indication of an influence from rainbow trout. Grade B.

Bear River Drainage, O. c. utah

Lander Creek: R117 T21 S33; July 26, 1994; N=6; 98-158 mm TL. Tributary to Smith Fork of Bear River. These six specimens could represent a pure population but the spotting pattern on these small fish does not reflect the "ideal" of Bear River utah. Scale counts 169-194(178) in lateral series and 39-43(41) above lateral line; gill rakers 17-20(18.3); basibranchial teeth 3-18(8.8). Only tentative pyloric caeca counts could be made on four fish, 31-36(34). The incision on the side of the body, to allow preservation of internal organs, was made too high on the body (missing the body cavity) and decomposition of caeca had occurred. Except for the lower than expected (tentative) caecal counts (about 40-45 expected in Bear River utah), the other meristic values are typical of pure populations. The less than ideal spotting pattern could reflect a lingering influence of past hybridization with fine-spotted Snake River cutthroat, or it could be an ontogenetic effect. Commonly, younger (age 1, into age 2, sometimes 3) cutthroat trout do not achieve the definitive spotting pattern. They tend to have relatively smaller spots arranged differently than the definitive pattern. This sample could represent an A population, but the spotting pattern of larger, older fish should be observed to compare with the "ideal" of Bear River utah (figure 8, p. 109 of my monograph; also Robert Smith's book, *Native Trout of North America*, first edition p. 85, has a color photograph of cutthroat from Thomas Fork drainage). Mainly, the spots of Bear River cutthroat are relatively large, sparse, and more evenly distributed on sides of body, rather than highly concentrated on caudal peduncle, in contrast, for example, with O. c. pleuriticus.

Packstring Creek: R119 T29 S27; September 1, 1994; N=15; 84-148 mm TL. The absence of basibranchial teeth in 6 of 15 specimens and spots on top of the head on two specimens indicate past hybridization with rainbow trout. Somewhat lower than expected scale counts also suggest influence from rainbow trout. Surprisingly, the spotting pattern on these small fish is more typical of Bear River utah than the Lander Creek specimens. This may reflect relatively older age, adult fish. Males of 100-110 mm have developing testes and would have spawned in spring of 1995 (collected Sept. 1, 1994). Evidently, the trout in this creek have a very low growth rate.

Scale counts 150-179(164) in lateral series and 34-42(38) above lateral line; pyloric caeca 36-43(37); gill rakers 17-19(18.5); basibranchial teeth lacking in 6 of 15 fish, nine fish with 2-8 (4.2).

Although obvious indications of a hybrid influence are detected, these specimens resemble "good" Bear River utah. Grade B.

North Fork Smith Fork: R118 T29 S13; July 26, 1994; N=9; 89-226 mm TL. Lateral series scales 174-193(183), above lateral line 40-47(43), all with basibranchial teeth 1-7(3), gill rakers 17-20(18.3). Specimens poorly preserved, internal decomposition obviated pyloric caecal counts. Spotting pattern variable; some specimens appear intermediate between typical Bear River O. c. utah and Green River O. c. pleuriticus. The gill raker count (18.3) is typical of Bear River drainage utah. The scale counts (183 and 43), is somewhat high for Bear River utah. The smaller, more variable spotting and higher scale counts are most likely explained by past hybridization (20 years ago or more) with fine-spotted Snake River cutthroat. I rate this sample as B (good, not pure).

[original 1/3/95]

Morphology and Systematics Overview

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I propose, only half in jest, that a license to practice conservation biology be required of all agency personnel involved with decision-making in regards to biodiversity and endangered species issues. To be granted this license, some level of understanding would be required about Darwinian evolution by natural selection and all it implies regarding co-evolution, co-adaptation, and adaptive strategies involved with niche filling. I would also require an understanding of distinctions between evolutionary genetics (by natural selection) and population genetics (controlled, laboratory, artificial selection) and the caution necessary to make extrapolations from one to the other (understand the dangers of inductive reasoning). Finally, I would also like to see an understanding of the pros, cons, and limitations of any method or technique to provide the most accurate answers to specific questions for defining evolutionary diversity and its significance -- the need to use an integrated, eclectic approach. It is better to take the time to contemplate probable answers to questions of uncertainty than to seek precise answers to irrelevant questions.

The lure of deterministic methods or models offering instant, simplistic answers to complex questions can be overwhelming to agency people involved in the decision-making process. The danger is that data, rules, or quantitative indices are substituted for thinking and critical judgement.

My hypothetical license practitioner should realize that defining the evolutionarily significant unit is not a simple matter. One size will not fit all. This truism was brought out during the conference when someone suggested that the evolutionarily significant unit could be better called the ecologically significant unit. Although this matter was not pursued further at the conference, I will give my interpretation based on evolutionary time scale, concerning the distinctions and implications between evolutionary and ecological in regards to defining significance of biodiversity.

The Endangered Species Act (ESA) defines a species (of vertebrate animals) to include intraspecific units -- subspecies and "population segments which interbreed when mature." Thus, a local population or deme is the smallest "population segment" qualifying as a vertebrate species for protection under ESA. The intraspecific diversity contained in a widely distributed anadromous species such as chinook salmon, <u>Oncorhynchus tshawytscha</u>, consists of numerous life history forms distinguished by different times of spawning runs from the ocean, different distances of spawning migration and times of spawning, different juvenile life histories, and different patterns of ocean migration. The "evolutionary" relationships of these intraspecific life history and ecological forms are "within basin."

That is, all races, populations, and demes of chinook salmon within a large river basin are more closely related to each other than they are to analogous forms of other river basins. Life history adaptations in Pacific salmon and steelhead O. mykiss, have independently arisen many times during the past 10,000 years (they are polyphyletic rather than shared synapomorphies).

A conservation strategy should aim to preserve the range of adaptiveness in a species to maintain its evolutionary options, and this is the basic issue for better definition of the ESU. The range of intraspecific ecological-life history adaptive capabilities has been evolutionarily programmed into the genome (the regulatory genome) by natural selection, but because of the relatively short evolutionary time span involved, and probable limited gene flow among populations, we should not expect that these adaptive properties can be detected or understood from molecular genetic data (of the structural genome). These adaptive properties, however, are the most important attributes for defining the evolutionary (or ecological) significant unit if our goal is to preserve the range of adaptiveness within a species.

A more common perception of evolutionary significance that ESA is designed to protect, I would equate with phylogenetic or taxonomic significance. A taxonomic hierarchy inclusively groups assemblages into higher categories: species, genera, families, orders, etc. The coelacanth, Latimeria chalumnae, and the bowfin, Amia calva, are the sole existing species of ancient lineages that have been "reproductively isolated" from all other phylogenetic lineages for about 350 million and 150 million years, respectively. Their "evolutionary significance" is essentially self-defined by their phylogenetic history.

The methods, evidence, concepts, and pertinent questions used for defining phylogenetic or taxonomic evolutionary significance can be quite different than those necessary for defining the ecological-adaptive significance of intraspecific evolution. It's a matter of evolutionary time scale. The extinction of Latimeria chalumnae would irreversibly complete the extinction of crossopterygian fishes. The extinction of the winter run of O. tshawytscha of the Sacramento River or the fall run of the Snake River would reduce the range of adaptiveness in the species, but this could be considered as potentially reversible. That is, if historical environmental conditions and selective pressures were restored, other races of the species could give rise to adaptive forms duplicating the life histories of the extinct forms, but it would likely take hundreds of generations. Thus, with the evolutionary significant ecological-adaptive unit, we might have a goal for the preservation of the range of intraspecific adaptiveness for the next 100 to 1,000 years, in hope that conditions for survival will improve.

Certainly, molecular genetics can play an important role for better definition of intraspecific diversity in relation to defining the significance of evolutionary units in certain situations. For example, how is a species structured? Bernatchez (this volume and his literature citations) used mtDNA analysis to resolve the phylogeny of diversity in North American lake whitefish, Coregonus clupeaformis, and rainbow smelt, Osmerus mordax, after all other methods had failed to resolve the question of sympatric, reproductively isolated populations. Do these sympatric pairs represent ancient monophyletic lineages (as with the whitefish) or more recent (late Pleistocene), independent, polyphyletic origins (as with smelt)? Understanding the phylogenetic structure of intraspecific diversity is important for planning strategies for the conservation of diversity in relation to the irreversibility or irreplacibility of extinctions.

A question concerning the evolutionary significance of the southernmost populations of steelhead (do they represent the native genotype or have they been thoroughly homogenized by stocking of nonnative hatchery steelhead?) could only be answered with confidence by modern molecular techniques. Nielsen et al. (1994) found unique DNA sequences in steelhead south of San Francisco Bay. These declining populations of southern steelhead do represent native populations and are worthy of protection and restoration.

My point is that phrasing the right questions in need of answers is of critical importance and should come before the "methods" of analysis are chosen. Also, we should promote the advantages of an eclectic, integrated approach to develop various lines of evidence. We should avoid bickering over what methods, concepts, and personal agendas are "superior" for defining the ESU and seek a common ground for furthering the cause of the preservation of biodiversity.

In relation to the definition of species in the Endangered Species Act and its possible changes during reauthorization of the Act, Stelle (1994) wrote, "I personally hope this question is not answered by the legislature. One of my worst nightmares envisions a congressional floor debate regarding the definition of 'subspecies' or 'distinct population.' This is an inherently scientific issue with no real place in the legislative process, and it should be resolved by scientists." If "scientists" can't agree on what is an ESU, it will likely be defined by the legislature.

This brings up my final, but most important point. We should attempt to communicate the knowledge underlying our conservation ethic beyond our own peer group (Behnke 1984). Brussard (1994) raised the question, "Why do we want to conserve biodiversity anyway?" He pointed out that we haven't been highly successful in communications and influence at various levels of society. Our failure to effectively communicate the positive aspects of biodiversity preservation and the need for an Endangered Species Act is illustrated by an article, "Better red than dead" in the Dec. 12, 1994 issue of Newsweek magazine. The article tells about the "endangered salmon bake" held in Stanley, Idaho (headwaters of Salmon River drainage with two endangered races of chinook salmon and the endangered sockeye salmon population [O. nerka] of Redfish Lake). Helen Chenoweth, newly elected Congresswoman from Idaho, spoke at the event. Ms. Chenoweth's environmental platform for the election was essentially that of the Wise Use Movement. Among her remarks to the audience was, "How can I take the salmon's endangered status seriously when you can buy a can at Albertson's?"

Evidently, the outrageously fallacious notions on evolution, extinctions, and values of biodiversity, propagandized by groups such as the Wise Use Movement were more effective in forming the opinions of Congresswoman Chenoweth and most of the voters in Idaho who elected her, than were any of the pro-environmental positions attempting to explain why we want to preserve biodiversity. Can we do a better job of communications by next election? If not, the ESU may become extinct.

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[no date]

CONSERVATION AND UTILIZATION OF THE BIODIVERSITY OF SALMONID FISHES:

ROLE OF EXPERIMENTAL HATCHERIES

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An understanding of intraspecific diversity is of critical importance for intelligent fisheries management. For example, Chinook salmon maintain three distinct races, each with different life histories, in one river of only 50 km in length (Nanaimo River on Vancouver Island). Such intraspecific diversity was evolved to maximize the abundance and survival options of a species. Transpose the Nanaimo River watershed on a map of the Columbia and Sacramento river basins and one can envision the potential genetic diversity among the populations of Chinook salmon that once occurred in these basins before the era of dams and degradation of flows and water quality. From this consideration, the futility of attempting to maintain original abundance with generic hatchery stocks becomes apparent. There is no way to recreate the broad array of the original adaptive diversity once it has been lost.

Similar examples of intraspecific diversity also characterize populations of steelhead and inland rainbow and cutthroat trout. This diversity in life history and ecology reflects different combinations of biotic and abiotic selective factors acting under natural selection over thousands of generations. Such adaptive traits as age at maturity, maximum life span, migratory behavior, feeding specializations, temperature adaptations, etc. are significant traits for utilization in management programs.

The establishment of an experimental hatchery to preserve and utilize the genetic resources of wild salmonids is a significant step for implementing a program for the utilization of biodiversity in fisheries management. In view of historical perspectives concerning how natural resource agencies conduct their business, I foresee problems likely to debilitate the effectiveness of an experimental hatchery. These problems concern the organizational mindset - to plan, to organize, to quantify in a knee-jerk manner before basic questions on the identification of types of diversity, and their potential utilization are addressed. I foresee a great amount of time and money wasted during a "wheel-spinning" stage to obtain electrophoretic profiles, karyotypes, heterozygosity indices, mitochondrial DNA data, etc. without realizing that such information is irrelevant for understanding diversities of life history and ecology. The primary concern of an experimental hatchery is an understanding of the qualitative differences in various populations and testing their performance in various environments (for example, in put-grow-and-take fisheries). Quantitative genetic studies are secondary considerations and should not delay the implementation of an applied program for intelligent utilization of biodiversty.

Except as an emergency measure, captive brood stocks of wild fish should not be maintained at the experimental hatchery or artificial selection will be inevitable.

[no date]

The Evolutionary Significant Unit in Regards to Fish Propagation and Management

Robert Behnke

The ESU concept was devised to evaluate intraspecific units of biodiversity for protection under the Endangered Species Act. The ESU is supposed to represent a significant part of the evolutionary legacy of a species. A problem concerns how evolutionary significance is defined. What evidence is used? I propose, for fisheries management, ESU can mean ecologically significant unit, which pertains to intraspecific diversity in life history, behavior and ecology. Modern genetic techniques are useful to tell us how a species is structured; for example, recognition of populations derived from different glacial refugia. These techniques are useless, however, to identify or define diverse life history forms, or to indicate how different forms will perform in different environments. Typically, the forms, races, or populations attaining the largest size are characterized by an older age at maturity, long life span, and predatory feeding specializations. These traits have a hereditary basis, but can't be "genetically" identified. We should not expect to find a "fish for all waters" because evolutionary adaptations are specific to specific conditions. For example, the Seneca Lake, New York, lake trout has shown the greatest success, by far, compared to many sources of lake trout stocked in Lake Ontario, but it was a failure when stocked in numerous Adirondack lakes.

Review and Comments on the

Salmon of the Salmon River, Idaho

Robert J. Behnke

Abstract

Currently, two races of chinook salmon and one race of sockeye salmon (of Redfish Lake) of the Salmon River are listed and protected under the Endangered Species Act. A law suit and a motion for injunction has been filed to shut down all activities of U.S. Forest Service lands which may potentially jeopardize the listed species. Possible options are examined to counter and respond to this issue in relation to cost-benefit of various actions and degree of success which might be anticipated. It is concluded that any attempt to remove the salmon from ESA protection by such means as genetic analysis, hoping to demonstrate that the present races of salmon do not qualify for ESA listing, will fail. I recommend that a policy of "sound science" be promoted to critically evaluate all evidence for "risk analysis" -- the promotion of rational and reasonable thinking to counter the charges of the law suit.

Introduction

In April 1992, the spring-summer (combined) and fall races of chinook salmon of the Snake River system were listed as threatened (recently changed to endangered) under the Endangered Species Act (ESA). The ESA requires "consultation" between any federal agency and the U.S. Fish and Wildlife Service (for terrestrial organisms and inland freshwater fishes) or the National Marine Fisheries Service (for marine life and anadromous fishes such as salmon) when any action or program of a federal agency might "jeopardize the continued existence" of a listed species. The U.S. Forest Service has "Land and Resource Management Plans" (LRMP) for each national forest for resource programs -- livestock grazing, mining, logging and associated road construction. A problem arose when certain USFS resource plans had potential conflicts with ESA in relation to negative impacts on habitat of the protected salmon. In August, 1992, the Sierra Club Legal Defense Fund, representing the Pacific Rivers Council, gave notice of intent to sue over the failure of the Wallowa-Whitman and Umatilla national forests to consult with NMFS on their resource programs (LRMP). In September, 1992 the USFS contacted the NMFS asking if NMFS wished to "informally consult" on the forest plans. In October 1992, the plaintiffs filed suit. In December, 1992, NMFS declined consultation, proposing that consultation would be more appropriate during development of "conservation strategies" which would address ESA conflicts of the forest plans. In March, 1993, the plaintiffs moved for summary judgment for an order compelling consultation. USFS admitted the forest plans (LRMP) for Umatilla and Whitman-Wallowa forests required revision to conform to ESA but claimed that consultation is not required until after the revisions are adopted. The Forest Service continued "business as usual" in their grazing and logging programs and in May 1993, the plaintiffs moved for an injunction on all ongoing grazing and logging activities which "may adversely affect "the salmon." In October, 1993, the District Court ruled in favor of the plaintiffs agreeing that consultation on ongoing forest plans is required but refused to suspend the ongoing and planned activities. In July 1994, the Circuit Court affirmed the District Court's decision but went further by ordering suspension of all ongoing and planned programs of grazing, logging and road construction until consultation is completed.

The plaintiffs now have filed for injunction to suspend all ongoing logging, grazing, mining, and road building in five national forests in Idaho in relation to "adverse affects" and "prevention of irreparable harm" to Snake River salmon (mainly Salmon River salmon). The issue, as with the Whitman-Wallowa and Umatilla forests, is failure to conduct consultation with the National Marine Fisheries Service.

In retrospect, it can be argued that the USFS set themselves up for what has happened. Evidently, clearly-defined guidelines on the ESA consultation process were sought from a court ruling. During the period from 1992 through 1994, however, the ESA protected Snake River salmon continued a drastic decline, from thousands to hundreds of chinook salmon and only one sockeye salmon returned to the Snake River in 1994. A sense of urgency prevailed and it was not difficult to portray the USFS position as so much administrative fiddling while the Snake River salmon crash toward the brink of extinction.

Chinook Salmon

Obviously, the species of chinook salmon, *Oncorhynchus tshawytscha*, is not endangered as a whole. They can be purchased every day in the market (most of the commercial salmon are now raised in cages in the U.S., Canada, and Chile). The Endangered Species Act defines a "species" eligible for listing to include subspecies and smaller units ("population segments") down to a single population (for vertebrate animals, which includes fishes). Thus the spring-summer and fall "races" of Snake River Chinook salmon, and the Redfish Lake sockeye salmon, are "parts" of their respective species. Literally hundreds of declining races or populations of chinook, coho, and sockeye salmon and steelhead trout have been defined as endangered, threatened, or "special concern" by fishery and environmental organizations and many have been proposed for ESA protection. The National Marine Fisheries Service, realizing the problem of dealing with innumerable petitions to list so many races and populations (a race can be a group of populations; the races of Snake River chinook originally consisted of many different populations, typically called "subpopulations"), developed criteria for listing of salmon races or populations. According to the NMFS position, to ~~quality~~ qualify for listing under the ESA, a population (or race) must be an "evolutionary significant unit" (ESU). The following questions are posed to determine if populations are qualified to be an ESU: Is it (or are they) genetically distinct from other populations? Do they occupy unique habitat? Do they show unique adaptation to their habitat? If they become extinct, would this be a significant loss of ecological-genetic diversity of the species?

This is NMFS "policy" based on their interpretation of the ESA, but there is no real consensus on this policy. The May 1994 issue of Environmental Law (vol. 24, no. 2) is devoted to issues of ESA reauthorization. In this issue, Daniel Rohlf has a stinging criticism of the NMFS' evolutionary significant unit policy as "scientifically and legally flawed." In May I served as a session chairman at a conference sponsored by the American Fisheries Society which sought better definition of the evolutionary significant unit. As might be expected from bringing together a large group of "experts", we did not reach any unanimous consensus.

In any event, the pertinent question is: do the spring-summer and fall races-populations of chinook salmon of the Salmon River drainage of Idaho (and the sockeye of Redfish Lake) qualify as an "endangered species" under the ESA? More precisely, do they qualify as evolutionary significant units (or "distinct population segments" of a species by the definition found in section 4 of ESA)?

I believe that there would be a rather unanimous consensus of the "best scientific opinion" that they do qualify. The reasons are as follows. The "intent of Congress in carrying out the will of the American people" when passing the Endangered Species Act of 1973 and subsequent amendments, is to prevent extinctions (or greatly reduce rate of extinctions) and preserve biodiversity. Biodiversity includes diversity among species (interspecific diversity) and within species (intraspecific diversity). The chinook salmon species is an example of a species with great intraspecific diversity. Many races-populations are the parts -- the "distinct population segments" -- which make up the species throughout its range in the North Pacific basin of Asia and North America. How many distinct races or populations originally inhabited the vast Columbia River basin might be estimated from the fact that the Nanaimo River on Vancouver Island contains three distinct populations of chinook salmon although this river is only 30 miles long. Each population has different life histories which maximize abundance of the species by reducing competition for food and space and reducing density-dependent predation on smolts migrating to the ocean by different times of migration. These finely-tuned adaptive differences in salmon populations cannot be maintained with massive hatchery propagation of "generic stocks" (one reason why hatcheries have been a dismal failure). These populations are genetically different. The genetic differences are maintained by separation of each population in time and space during spawning (the evolutionary basis for homing instinct in salmon).

The three races of chinook salmon currently listed under ESA are unique components of the species. The fall chinook salmon of the Sacramento River (the first chinook to be listed) is the only race-population that spawns in the spring, all other chinooks spawn in late summer or fall (the designation as spring, summer, or fall refers to the time the salmon enter rivers on their spawning run from the ocean). It was formerly very abundant but is now almost extinct due to altered flow-temperature regimes by Bureau of Reclamation river regulation. The spring-summer and fall run chinook of the Salmon River, Idaho, represent the most inland natural distribution of the species (700-900 miles from the ocean). The physiological and life history adaptations of Snake-Salmon River chinook are regarded as unique -- an important component of the species intraspecific diversity. That is, they must be very different in comparison with a chinook population which spawns within 50 miles of the ocean within a few weeks of entering a river and whose young migrate to the ocean soon after hatching.

An important question concerns the "integrity" of Salmon River chinook. Are they the same populations of chinook salmon found in the Salmon River 100 years ago or has hatchery propagation and stocking hopelessly mixed the original populations?

Genetics

A comprehensive work by Schreck *et. al.* (1986) examined the genetic basis of diversity among populations of chinook salmon of the Columbia River basin. They documented differences in life histories, morphology, and more quantitative genetic differences by protein electrophoresis.

Their conclusions were that there were two major groupings. One includes spring chinook from east of the Cascade Mountains together with summer chinook of the Salmon River, Idaho. The second grouping consists of spring chinook from west of the Cascades together with all fall chinook and summer chinook of the upper Columbia River. The two major groupings can be subdivided into many subgroups. That is, despite a long history of impacts from dams blocking and forcing mixing of populations and especially hatchery propagation practices that acted to force mixing of different native populations, the power of natural selection has been sufficient to maintain a large measure of the original diversity. It is likely that originally the Snake (and Salmon) River contained populations that could be more clearly grouped as spring, summer, and fall races. The blurring of the spring and summer race distinctions of Snake-Salmon River chinook is likely a result of dams and hatchery-included mixing of the races (as has been better documented for the upper Columbia River chinook).

The original petition to list Snake River chinook salmon under the ESA included three races: spring, summer, and fall. Because no consistent genetic-life history differences could be found to separately define the spring and summer races, the NMFS grouped spring and summer together as a "species" for listing. Although a case could be made that the current "spring-summer" chinook salmon of the Snake-Salmon River does not represent "pure" forms of the original races, they do represent what is left of the original genetic diversity of these races, and as such, they are regarded as "significant population segments" of the species, a significant part of the "evolutionary legacy" of the species, and a "significant evolutionary unit" of the species.

Implications and Options

A few years ago there was activity and development of petitions to list several races of salmonid fishes of the mid Columbia River (Columbia River and tributaries between McNary and Chief Joseph dams). The proposals included spring, summer, and fall chinook salmon, sockeye salmon, and steelhead trout. The Chelan Public Utility District prepared reports for NMFS demonstrating that because of the blocking of the Columbia River by Grand Coulee Dam and fish hatchery propagation and stocking practices, the present run of chinook represents a continuum lacking discrete "population segments" of spring, summer, and fall chinook populations (Preven 1992; Chapman 1993; Utter 1993). Genetic analysis (electrophoresis) was performed to support the position of a continuum. The NMFS rejected the petition to list the mid-upper Columbia salmonids and it might be assumed that "genetic analysis" was the basis for rejection.

Actually, the genetic analysis only confirmed the obvious. The position of the Chelan PUD admitted the chinook salmon presently using the mid-upper Columbia would qualify as an evolutionary significant unit because they represented what was left of the original genetic diversity of the upper Columbia salmon, but they represented a single, homogenized unit, rather than three units as proposed in petitions. The NMFS rejected any listing because the present and recent past abundance of chinook and sockeye salmon and steelhead is much greater than it was 50 to 75 years ago (based on counts at dams).

Similar genetic analysis could be done again on Salmon River chinook (very expensive analysis), but I can only believe it would confirm the NMFS position based on the genetic analysis of Schreck *et al.* (1986) and subsequent studies that Snake-Salmon River chinook salmon consists of two "distinct population segments" or two "significant evolutionary units" as presently regarded by the NMFS. I don't believe that position can be changed by genetic or any other type of analysis.

The ESA listing of spring-summer chinook, although they probably aren't "pure" representatives of the original spring and summer chinook, brings up the matter of the "hybrid policy" for ESA decisions. Rholf (1994) discussed the confusion and contradictions in applying a hybrid policy for ESA listings. Nothing in the ESA addresses the issue. In 1977, the USDI Solicitor ruled that hybrids should not be protected under the ESA. This resulted in some legal challenge, such as the Farm Bureau attempting to remove the wolf from ESA protection because genetic analysis shows what is well-known, that, occasionally, wolves hybridize with coyotes. This 1977 opinion brought ridicule from the scientific community to the effect that hybridization between closely-related species is a natural phenomenon, a natural part of evolution, and that attorneys and politicians shouldn't meddle in biological issues with which they are ignorant. In 1990, the USDI Solicitor withdrew the 1977 and subsequent hybrid opinions. The FWS and NMFS apply a hybrid policy on a case by case basis -- but not uniformly. The red wolf, an endangered species on which millions of dollars has been spent, is a hybrid between the gray wolf and coyote. The Florida cougar, also a multi-million dollar endangered species, now exists as a hybrid between two cougar subspecies -- the native cougar and a South American subspecies introduced in Florida many years ago. The endangered June sucker of Utah Lake, Utah, also exists only as a hybrid -- an interesting hybrid between two genera of suckers, *Charmistes liorus* X *Catostomus ardens*. All of the above hybrids have been and continue to be protected under the ESA. Thus, I do not believe that "proving" the spring-summer chinook of the Salmon River is, in reality, a "hybrid" can remove it from ESA protection. There would also be the fall chinook to contend with.

The law suit seeks to "enjoin all activities" on USFS lands of the Salmon River watershed until FS Land and Resource Management Plans undergo the consultation process. Supposedly, the consultation process will sort out the activities which are "likely to have adverse impacts" on the salmon, "may affect" the salmon, and those deemed to have no effect. The "adverse impacts" of mining in the Yankee Fork Ranger District is emphasized. It is mentioned that the Thompson Creek molybedmum mine is "14 miles downstream and sits five miles and several thousand feet above the Salmon River." A large, deep tailing pond is mentioned, implying impending disaster -- a potential adverse impact. In the short term, the logical tactic would appear to be making the case that any "enjoinment" should not be so broad-based. It should be selective. What ongoing and planned activities are believed to have "no effect" on the salmon or their habitat? These should be exempted from enjoinment.

I do not believe a new, Republican dominated Congress writing new amendments to the ESA will result in dramatic changes in the near-term. According to a news release after the election, the Republicans will emphasize "private property rights" and "sound science" in environmental matters. The key for reform, I believe, is "sound science". The validity of claims for or against an issue such as "adverse impact" or "no impact" on endangered species from a mining operation should be based on sound science, not emotional, unsubstantiated rhetoric.

On June 28, 1993, the U.S. Supreme Court made a ruling in the Daubert vs. Merrel-Dow Pharmaceutical case which contributed to the legal definition of "sound science" and testimony of expert witnesses. The court ruled that the reliability of scientific evidence depends on the methods used to develop the evidence. Have the methods been empirically tested and found to be valid for making accurate assessment or prediction? Critically scrutinize the "methods" used to arrive at any prediction for "adverse impact."

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THE ILLUSION OF TECHNIQUE AND FISHERIES MANAGEMENT

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I use the term illusion of technique in reference to the common phenomenon whereby the human mind is highly susceptible to indoctrination with a naive belief that chaotic systems of nature can be neatly ordered for predictive purposes if only modern technology, such as a computer simulation model, can be applied to a problem. This phenomenon leads to a naive faith that confuses objectivity, quantification, and sophistication with biological realities. The problem of erroneous predictions concerns the substitution of data for knowledge and the institutionalizing of ignorance under the guise of conflict resolution.

LIMITATIONS OF PREDICTIONS

If a regional farmer's almanac is consulted to observe times of sunrise, sunset, high tide and low tide for any given day, we would have a well-founded belief in the accuracy of these predictions. If this same almanac predicted the weather each day of the year, a year or more in advance, we might chuckle at the expected predictive accuracy of such long range weather forecasts. However, if we loosen the constraints for precision, we would have some confidence in a prediction that claims the maximum and minimum temperatures for any given day in July will be higher than for any given day in January in the Colorado-Wyoming area.

If one can comprehend the reasons why some natural phenomena can be accurately predicted and why some cannot, as illustrated in the above examples, then an understanding of the limitations for accurate predictions made on the basis of environmental or biological models should be apparent -- it concerns patterns of regularity in nature, and our interpretation of these patterns for making predictions.

To obtain consistently accurate predictions based on data from a natural system, the particular system must be stable, isolated (not subjected to external perturbations), and highly regular. Most biological systems do not meet these prerequisites. The law of gravity, the positions and motions of the sun and planets have patterns of regularities that justifies our faith in the accuracy of predictions regarding the times of sunrise, sunset, high tide and low tide. The value of empirical evidence can be demonstrated by considering the fact that accurate predictions are possible from accurate recording and interpretation of the data of regularity, even though the processes causing regularity are unknown. For example, ancient societies could have compiled the essential data on which accurate forecasts of sunrise, sunset, and tides could be made while accepting a theory that the earth is flat, stationary and the center of the universe. For long-range weather forecasts where a multiplicity of unpredictable, short-term influences act to create local conditions, a full understanding of all the processes of weather formation does little to improve long-range predictive accuracy over mythological methodologies such as the degree of fuzz development on caterpillars.

The implication for fisheries management and environmental assessment in general, is that, unless a system is extremely regular and tight cause-and-effect relationships between a proposed action, such as change in flow regime, and the target species can be empirically demonstrated, do not expect predictive accuracy from any model -- the best that can be expected is to demonstrate trends; to be in the ballpark. For example, enrichment of a pond can be expected to result in a trend for increased fish production. The precise amount of increase in a target species such as bass or trout from a known percentage increase in nitrate and phosphate cannot be accurately predicted because of the multiplicity of unknown and unpredictable phenomena that can influence the transfer of energy from primary (or bacterial) production to the target species.

The limitations on predictive accuracy associating nutrient enrichment to fish production was neatly demonstrated by Bill McConnell and students of the Colorado Cooperative Fishery Unit and David Galat in replicated microcosm experiments. Under identical conditions, great variability in fish production was found, but consistent trends were apparent. Higher trophic level species, such as smallmouth bass, always had less production than lower trophic species, such as carp. Thus, a trend associated with trophic level can be predicted, but the actual amount of production cannot be predicted from nutrient levels.

A similar situation applies to other environmental variables as they affect fishes. A computer simulation model that produces precise habitat quantification such as habitat units expressed as weighted useable area (WUA) which display changes in relation to flow changes, has indoctrinated the minds of many naive biologists and administrators who confuse quantification, objectivity and sophistication with biological reality. Such people have assumed that changes in WUA accurately predict changes in fish populations -- they do not; the best that can be hoped for is that trends can be predicted. In recent years, many biologists and administrators have become vaguely aware of this fact, but the appeal for standardization of an assessment method is strong and arguments are developed concerning the relative merits of various methods in relation to negotiability, defensibility, holding up in court, etc. The only way I envision that quantified habitat units lacking valid representation of biological reality can be negotiated and defended is if a game of environmental assessment is created and all of the players agree to play by the rules, which would include treating habitat units as currency similar to play money in the game of Monopoly. If an irreconcilable conflict arose and a case ends up in court, I doubt that the judge and opposing attorneys would agree to the rules of the game.

CONCLUSIONS

What has been said above is only a matter of common sense thinking. Why is common sense so uncommon? The pioneers and leading practitioners of simulation modeling cannot be blamed for our problems with the illusion of technique. People such as MacArthur and Wilson (Island Biogeography) and Hollings (Adaptive Environmental Assessment), who popularized biological and environmental modeling, clearly sounded warnings and cautions concerning the limitations of predictions made from highly simplified and compartmentalized abstracts of nature and emphasized the need to test and continually refine and fine-tune a model. The lure to administrators, however, of a "standard method" for conflict resolution, with or without biological reality, is great and difficult to resist. A negative aspect concerns the expenditure of considerable funds to obtain essentially meaningless data in relation to benefits to a target species when these funds might have been beneficially expended on constructive mitigation or enhancement measures if detailed knowledge of a species life history in a particular environment was used to resolve a conflict. That is, look for ways to reverse the illusion of technique by substituting human knowledge, expertise, and experience for "shotgun"-type of data and "rules".

During 1986 I was involved in an acrimonious legal action in Michigan over no-kill regulations for the Au Sable River. The backers of the no-kill regulation consistently cited a computer simulation model that "proved" a significant increase in larger trout would result from no-kill regulations, despite all empirical evidence to the contrary and a published word of warning from the creator of the model concerning its limitations for predictive accuracy. Highly trained and otherwise disciplined minds can be completely susceptible to the illusion of technique if it furthers their interests and supports a belief.

The Intermountain Region of the U.S. Forest Service published a small booklet entitled: "Macro What?". This booklet tells how analysis of aquatic invertebrates is used "to measure the effects of" such activities as hunting, fishing, camping, and livestock grazing. Are there people in the U.S. Forest Service who really believe that the best way to "measure effects" of hunting and fishing and livestock grazing is by indirectly analyzing the aquatic invertebrates rather than directly "analyzing" the hunters and fishermen or the direct livestock impact on riparian vegetation, bank stability, channel morphology, and fish population? Why not apply the "rule of parsimony" and look for the most simple and direct cause-and-effect relationship of a problem and "analyze" that rather than to instinctively "follow the rules" of a "standard method" when they are not applicable to particular situations?

Evidently, there are indeed such people, as Don Duff told us at our annual meeting, Forest Service administrators, after many years, finally agreed to institute revised grazing management on Silver King Creek, California, to enhance habitat conditions for the federally threatened Paiute cutthroat trout, after they were shown the evidence from aquatic invertebrate analysis. It must be assumed that these same administrators had been previously unconvinced by the direct evidence of cause-and-effect impact of livestock -- the barren, caved-in banks, erosion and actual trout population data -- until they were shown a "scientifically" derived metric of invertebrate diversity which "proved" the negative impact of livestock on the Paiute trout.

The moral of the story is that as long as we have to live and work with problems created by the illusion of technique, we might as well look for ways to use illusion in our favor. I would prefer, however, that in the future, we might have more knowledgeable administrators staffing resource agencies who are capable of exercising reflective judgement and a greater resistance to the illusion of technique -- but as I said, common sense is not common, and I doubt that it can be taught in school.

Belnke
[ca 1990s]

COMMENTS ON REPLACEABILITY OF TROUT HABITAT
BASED ON THE TWO FORKS COORDINATION ACT REPORT

The aquatic biology section of the USFWS Coordination Report on the Two Forks EIS concerns an essentially impossible mitigation task --- to replace the irreplaceable gold medal wild trout waters in the South Platte River that would be lost to inundation if Two Forks Reservoir is constructed (declared Class 1 or irreplaceable habitat by USFWS).

The question of "replaceability" concerns the unique set of habitat characteristics, environmental regime, and food supply for trout existing in the Cheeseman Canyon section of the South Platte River that result in a biomass of more than 500 pounds of trout per surface acre and with a highs proportion of trout 14 inches and larger than any other stream in Colorado. The density and average size of trout is such that this section of the river supports about 3,000 hours per acre per year of high quality angling based on catch-and-release regulations.

There may be people who have a naive faith that the problem of "replaceability" can be adequately treated in this case by quantifying lost habitat based on depth and velocity measurements expressed as numerical values of weighted usable area (WUA) and then calculating increases in WUA in "mitigated" streams due to proposed flow manipulations. If there are such gullible people, they would be likely candidates for a stock offering in the Brooklyn Bridge.

The obvious problem confronting any attempt at habitat quantification at one site and assuming interchangeability of these values to another site with any expectations that the values (such as WUA) are freely transferrable between sites concerns "within" and "between" comparisons of biological systems --- if the systems are not identical, values "between" systems are not equal. Is there a "gold medal WUA"? That is, do WUA values from the South Platte give any indication of the 500+ pounds per acre biomass of wild trout and the percent of fish of more than 14 inches? Depth and velocity measurements are certainly "transferrable" between streams. Eight or ten inches of depth and .2 or .3 fps velocity are indeed the same in the South Platte and in the Blue or William Fork rivers, and measurements between streams would provide identical points on habitat suitability curves used to compute WUA values; but are these the only or major controlling factors determining the expression of carrying capacity of a fish population, its growth rate, size-age structure, etc. Overwhelming empirical evidence demonstrates that they are not. Nutrient enrichment of Berry Creek, Oregon, increased trout production by sevenfold. Nutrient depletion in the Au Sable River, Michigan (from removal of sewage effluents) decreased the trout biomass by half. The trout biomass in the Frying Pan River below Ruedi Reservoir, Colorado, increased about fourfold when a new food supply (<u>Mysis</u> shrimp from reservoir) became available. All of these changes in the biomass of trout populations occurred with no change in "habitat" (i.e., the WUA of these streams did not change; only the food supply changed). Annear and Conder (1987, N. Am. J. Fish. Mgt. 7:339-350) compared habitat characteristics between Wyoming trout streams as expressed in WUA and then measured actual trout biomass in these streams to check for correlations. They found no correlation between streams. For example, sections of the Powder River and Laramie River had identical WUA values, yet the Powder River contained four times the trout biomass as the Laramie River.

Admittedly, the habitat quantification performed for the Two Forks project and reported on in the Coordination Report was more intense and sophisticated than the work done in Wyoming. One should not, however, be deluded into confusing quantification, sophistication, and great amounts of data with biological reality.

I was impressed with the thinking and rationale of the Two Forks assessment explained in the text of the Report. Current ecological concepts such as "ecological crunch" are tied into a basic assumption of the "weakest link" concept that a fish population is limited by the most limiting life history stage during the most limiting time of the year. Thus, WUA values were developed for four life history stages --- spawning, young, adult, and overwinter, and a "time series" analysis performed to denote changing WUA values in relation to flow changes. "Limiting" life history stages were highlighted by assigning "equivalent" values; for example, if spawning was believed limiting, a spawning WUA might be ranked 5 or 10 times higher than an adult WUA.

Thus, the report contains many pages of text and tables which superficially gives the impression of a highly sophisticated study (with tacit implication of a database for accurate predictions for mitigation). (accuracy) The true assessment of predictive accuracy ~~occurrence~~, however, returns to the question regarding the adequacy of depth and velocity figures to accurately characterize all meaningful parameters of a trout population. This question is recognized in the report with a statement that the habitat descriptors (essentially only depth and velocity "...must accurately describe the requirements of the species," but no discussion is devoted to the question of how accurately depth and velocity describes these "requirements").

At the Instream Flow Protection Workshop (Boulder, March 31, April 1), Clare Stalnaker (USFWS), in response to a question, stated that IFIM (Instream Flow Incremental Methodology --- used for Two Forks assessment) is a highly accurate predictor for changes in trout populations from changes in flow. This may be true, but only with a most important qualification. The qualification concerns the weakest link assumption for limiting population size. For example, if the weakest link in a population's life history is spawning and survival soon after (stages) hatching and these life history stages are extremely vulnerable to changes in flow, then accurate predictions can be expected. If depths and velocities are known in a river section during spawning (known depths of egg deposition) and subsequent flow declines to a level that exposes the nests, reproduction is eliminated. If flow greatly increases (extremely high velocities) during or in a few weeks after hatching of fry, the baby trout, ~~are~~ incapable of handling high velocity, are swept away, and the year-class lost. Under such conditions, IFIM could indeed be an accurate predictor, but simply examining U.S.G.S. flow records would also be an accurate predictor without going through an involved "time series" analysis of WUA.

I suspect if further questioned on the matter, Dr. Stalnaker would have cited data from the Gunnison River, Colorado, which reflect the above scenario as an example of accurate IFIM prediction. It is a great and erroneous inductive leap, however, to make a broad generalization to all IFIM studies. If Dr. Stalnaker believes the WUA value of the gold medal trout fishery of the South Platte is fully reflected in comparable WUA values in the Blue and Williams Fork rivers, he would be very wrong. insert

How wrong such a belief would be is suggested in the Coordination Report where "equivalent" determination for habitat among different life history stages is discussed. It was admitted that sound data was lacking to make these equivalent determinations, so "best estimates" were used. In a model, "best estimates" become "best simulations" or "BS". When the shaky factual basis of the original BS links into the model to drive the next step, BS becomes compounded. The model output may represent BS^3, which will be far removed from reality.

What might be considered as the "bottom line" of the Coordination Report is that 36,500 pounds of trout would be lost if the South Platte River is inundated, and the Denver Water Board would be responsible for "replacing" 90% of this lost biomass by increasing trout abundance in other rivers by flow manipulations, habitat improvement, etc. If this is the case, why bother to go through the great amount of work involved in the habitat quantification and modeling? How can that influence in any way the agreement to replace 90% of the lost biomass? The negative aspect of the IFIM work is that an illusion is created that an adequate mitigation will be achieved because of the great amount of study, data, and sophisticated modeling that was done. And this would be an illusion. There is no way to reproduce a 500+ pounds per acre of wild trout with the same high percentage of larger, older fish that supports 3,000 hours of angling per acre by increasing WUA values in other waters. It is comparable to trading 36,500 pounds of gold for 32,000 pounds of lead to achieve a mitigation goal.

insert take a highly constrained ~~limited~~ specific example and - - -

[ca 1990s]

CATCH-AND-RELEASE - THE LAST WORD

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Abstract

Significant progress and widespread implementation of special regulations as a management tool has occurred during the past ten years. That is, the biological or fisheries management basis for special regulations, in addition to the sociological or people management aspect, has been generally recognized and accepted. A current problem concerns agency credibility (or lack thereof) and the proper communication of this credibility to the angling public.

Introduction

Since the first catch-and-release symposium was held ten years ago, a considerable amount of new information has been developed; significant progress has been made. There is yet, however, much to be accomplished, especially in regards to salmonid regulations, agency expertise, credibility, and public education. It is safe to assume that a third symposium in this series can be planned for 1997 without fear that all the issues and problems discussed in the first and second symposia will be fully resolved by then. It would be interesting to speculate on the range of papers which might be presented at a 1997 symposium. Comparing the contents of the 1977 and 1987 symposia, it is apparent that there has been increasing interest and use of catch-and-release as a management tool in warm-water fisheries and in big gamefish marine fisheries. With warm-water fisheries, we have seen special regulations expanded beyond bass to include such species as walleye and crappie. With the great increase in big gamefish tournaments concerning species of little or no food value, catch-and-release angling is a management tool whose time has come.

Because of limitations imposed by my background, I will restrict my discussion to special regulations governing trout fisheries, but the sociological or people management problems identified are applicable to regulations for all fisheries.

Progress and Problems

To document the progress and enlightenment that have occurred during the past ten years of trout fisheries regulations and also to examine some of the darker areas in need of enlightenment, I will discuss the contents of the proceedings of the 1977 symposium and other historical sources and compare them with present attitudes, perceptions, and programs. A point I wish to emphasize is that a fisheries agency must establish expertise and credibility on which to base a strong leadership role for public acceptance and trust. Unless this leadership position is established and generally accepted, a leadership vacuum is likely to be filled by well meaning but misguided and narrowly focused extremist groups, typically devoted to the cult of no-kill regulations (and its corollary, single barbless hook flies only).

When the 1977 symposium was held, the use of special regulations in fisheries management was, by and large, viewed by most biologists and administrators as more in the realm of people management, rather than fish management. This reflected the general perceptions that sociological aspects dominate over biological aspects of regulations in sport fisheries. I suspect that because of this historical neglect of the biological or practical fisheries management aspect of special regulations, the organizers of the 1977 symposium entitled the symposium "Catch-and-Release Fishing as a Management Tool." Although the sociological aspect of regulations or people management is an important aspect of a management tool, without the biological evidence and understanding of the factors that determine success or failure of regulations to achieve a goal, an agency will lack the expertise and credibility for leadership; and the people management part of the management tool can become a nightmare of dissatisfaction and devisiveness.

The historical predominance of the sociological aspect of special regulations is apparent in the paper, "Catch-and-Release Fishing - The Pennsylvania Experience," in the 1977 proceedings. This paper discussed the results of the application of a 20-inch minimum size limit on some Pennsylvania trout streams. The streams or stream sections with this regulation did not produce increased numbers of 20+ inch trout in comparison to open areas under statewide regulations; thus, the special regulations were viewed as failures (although one stream section accumulated more than 700 pounds of trout per surface acre; and in the only comparison made between special regulation and open sections of the same stream, the special regulation section contained almost four times more trout between 10 to 20 inches -- i.e., if the goal of the regulations had been to increase the catch per hour rather than to increase the number of 20+ inch trout, they should have been great successes). The "failures" of the special regulations led to the following statement in the 1977 paper: "We have declared a moratorium on the designation of any more special regulations areas until we can sort out the facts and determine what we want to accomplish." Previously (Behnke 1980), I wrote that establishment of the "facts" (or the biological basis) and subsequent goal determinations are necessary antecedents to special regulations; and "a moratorium on special regulations in this time of need is analogous to declaring a moratorium on cancer treatment until we learn what cures work best." In regards to abdication of leadership, consider a high level spokesperson for General Motors declaring a moratorium on car manufacture until they learn how to make them better -- What would be the response of GM shareholders?

Also in 1977, the Colorado Division of Wildlife prepared a 10-year plan for the future, designed to develop management strategies to meet the demand from increasing numbers of sportsmen. In regards to trout fishing, only the unimaginative strategy of hatchery expansion was considered as a viable option to meet the increasing demand (which ignored the fact that the most rapidly increasing trout fisheries demand is for wild trout, not hatchery trout). The management tool of maintaining and increasing the catch rate of trout by recycling them in special regulation fisheries was not considered worthy of mention as a possible option in the 1977 Colorado plans. Again, the problem concerns the common attitude of fisheries professionals in the 1970s of relegating special regulations to the domain of sociology rather than biology. What should be apparent with a bit of reflection is that successful special regulation programs cannot treat sociology and biology as unrelated and isolated entities. The key to people management and leadership concerns establishing the facts or the biological basis of special regulations and effectively communicating these facts along with the aura of agency expertise and credibility to the public.

I do not mean to stigmatize individuals or agencies as bad examples. The above-cited examples were predictable in 1977 when the prevailing opinion declared that special regulations concerned people management rather than fish management. As an indication of progress since 1977, it must be mentioned that in 1987 Pennsylvania and Colorado have established strong leadership positions in the use of special regulations as a management tool.

The 1977 symposium proceedings contained success stories where under special regulations trout populations manifested enormous increase, especially with great increases in larger, older fish, in the Yellowstone River and in northern Idaho rivers. The proceedings also contained clear examples of failures of special regulations to influence trout populations in any meaningful way in small Wisconsin streams. What the proceedings lacked, however, was an overall synthesis and summary clearly identifying the factors that determined the successes and failures described. This lack of clear identification and understanding of the factors -- the biological basis -- determining the success or failure of special regulations is still prevalent in 1987, and it inhibits more widespread implementation of special regulations.

I do not have the time or space allotment to provide a complete discussion on the determinant factors governing the success of special regulations, but a few obvious considerations apparent in the 1977 papers and verified in subsequent years can be highlighted.

1. Species-specific differences to angler catch. The early examples clearly indicating success of special regulations in Yellowstone Park and in northern Idaho rivers all were based on cutthroat trout (and on cutthroat populations with a potential for an older age structure with a moderately high proportion of the population consisting of 5, 6, and 7 year-old fish and exhibiting moderately good growth rates averaging about 3 inches per year). The cutthroat trout, of all species of trout, is the species most readily caught by angling. Cutthroat trout is the species that can be expected to most favorably respond to reduction of angling mortality. In my previous paper on special regulations (Behnke 1980), I cited studies on brown trout populations in the South Platte River, Colorado, and in Hot Creek, California, where 1900 and 3800 hours per acre of annual angling pressure were required to catch each brown trout

on average two and three times respectively in catch-and-release fisheries. In our present symposium, Bob Hunt presented data on brown trout special regulation fisheries in small Wisconsin streams which indicate each brown trout may be caught two or three times with about 400 to 800 hours of angling per surface acre. These differences in brown trout resistance or susceptibility to catch are most likely due to individual characteristics of the Wisconsin streams (small, open, lacking "refuge" areas not accessible to anglers -- and the skill level of the local anglers) and they demonstrate the necessity for site-specific data on catch statistics. Even the most readily caught brown trout populations, however, pale in comparison to cutthroat trout in regards to susceptibility to angler catch. Schill and Griffith (1986) described the no-kill regulation fishery for cutthroat trout in the Yellowtone River where each cutthroat trout is estimated to be caught about ten times during a six-week period with about 500 hours of angling per surface acre -- and each trout on average is caught twice with only about 10 hours or less per acre of angling. In Yellowstone Lake overexploitation of the cutthroat population occurred in the 1960s with only five hours of angling per surface acre (papers in this symposium, by Jones and by Greswell). With such catch statistics it does not require profound thought to realize that the catch-per-angler-hour will be much higher for cutthroat trout than for brown trout if their populations and environments are similar -- and that a cutthroat population will respond to the elimination or reduction in angling mortality more rapidly and with greater magnitudes than will a brown trout (or rainbow trout or brook trout) population.

2. Size-age structure of the population. The brook trout populations in the small Wisconsin streams discussed at the 1977 symposium are typified by high recruitment and a short life cycle (virtually no trout in population more than three years of age). Very few trout in these populations grow sufficiently rapidly or live long enough to attain a length of 10 inches. Most of the production and biomass of such populations are tied up in young (0 and 1 age groups) fish of subcatchable size. The characteristics of these populations of small brook trout (or any trout species existing under similar environmental restraints) are determined by the environments they live in, and no type of regulation can do much about it. Examination of data from short-lived populations of brook trout (and brown trout) in Wisconsin and Michigan studies reveal that when total annual mortality reaches ninety percent or greater during the year a fish ages from two to three years (or in some cases with brown trout from three to four), reduction in angling mortality will do little or nothing to reduce the finality of this massive mortality. Determination of this terminal age of a population is an important consideration for understanding the limitations governing the relative success of special regulations. The ultimate explanation of a population's size-age structure concerns fish energetics and optimal foraging theory. If all sizes and age groups in a population compete for a common food supply and there is good recruitment into the population creating a great abundance of 0 and 1 age fish, the larger, older fish will be at a severe disadvantage simply because they require much more food, if only for maintenance rations, than do the smaller fish. Unless recruitment is severely curtailed or unless there is habitat (such as large, deep pools) and a food supply of large organisms available such as

fish, crayfish, scuds, etc., to allow feeding segregation between smaller fish and larger fish, do not expect special regulations to duplicate the Yellowstone or northern Idaho experiences. What can be accomplished for shorter-lived populations with some type of catch-and-release regulations under high angling pressure is the recycling of the two- and three-year-old trout to maintain a high catch rate. For populations limited by a young terminal age class, however, no types of regulations can be expected to significantly increase the proportion of older age classes beyond the terminal age determined for each particular population by each specific environment, because at such high annual natural mortality levels, angling mortality will be almost entirely compensatory and not additive. The importance of older age classes for the success of special regulations is apparent from the fact that any consistent increase in survival is compounded annually. For example, consider a hypothetical river section that contains 1,000 age 2 trout. If annual mortality is 75%, then 250 age 3 fish are expected in the population. If the 75% mortality rate is reduced to 50%, then there would be 500 age 3 fish or a 100% increase in this age class. If these same mortality comparisons are constant through age 7, there would be twice as many age 3, four times more age 4, eight times more age 5, 16 times more age 6, and 32 times more age 7 fish with 50% mortality in comparison to the 75% rate. These considerations merely represent a common sense approach for assessing the potential for success or failure of special regulations -- but "common sense" has not been strikingly obvious in the history and literature of special regulations.

People Management

The importance of the sociological or the people management aspect of special regulations cannot be denied. The most important factor, however, for people management is to establish credibility and public trust for general acceptance of proposed regulations. This can be accomplished by establishing the biological evidence and by communication of this information to the angling public. I admit, however, that this is far easier said than done. Ideally, an agency should have an authoritative spokesperson, thoroughly knowledgeable about the factors determining the successes and failures of special regulations, who is admired and respected by the anglers and who makes frequent contact with angler groups to get the message across. An example I would cite is Barry Nehring of the Colorado Division of Wildlife. Most anglers in Colorado accept the special regulations on trout streams in Colorado and believe they are achieving a goal of producing better quality wild trout fisheries, because they believe the evidence presented by Mr. Nehring; they respect the biological expertise upon which these regulations are based.

It is much more difficult and potentially more damaging to one's ego to publicly disagree with an individual, real-life authority figure such as Mr. Nehring than with the Colorado Division of Wildlife (a faceless bureaucracy).

In regards to effective communication, fisheries symposia such as the present catch-and-release symposium, are, in theory at least, designed to contribute both to fish management by promoting the exchange of information and to people management by involving sportsmen and publishing proceedings to communicate information to the public. It must be kept in mind, however, that progress in fisheries management is more of an evolutionary rather than a revolutionary process, slow and gradual; do not expect a quantum leap in progress as a result of bringing people together for topical discussion. Among the great diversity that makes up the American angling public, "cult" groups can be expected to promote with great enthusiasm their own narrow view of regulations (typically, no-kill, barbless flies only). The "cult" mentality is not receptive to new information or to information not supporting their preconceived ideas. They also can be expected to have zealous faith in the righteousness of their cause, so that any facts or evidence contrary to the cause is considered as blasphemy and piously ignored or attacked. The 1977 proceedings contained an article, "The Fly Fisherman's View of Catch-and-Release Fishing," which summarized the most frequently expressed convictions expressed in letters to Fly Fisherman magazine. One of the most common convictions of readers of the magazine was: "Barbed hooks increase fish-kill, as does mishandling of fish, making catch-and-release restrictions almost worthless without a barbless hook requirement and the proper treatment of fish." Also in the 1977 proceedings, Dick Wydoski published a paper, "Relation of Hooking Mortality and Sublethal Hooking Stress to Quality Fish Management," in which he exhaustively reviewed and summarized many studies of hooking mortality of single, treble, barbed, and barbless hooks with different species and under different conditions. Wydoski's paper obviously addressed the concerns (or, more correctly, the "convictions") of the readers of Fly Fisherman magazine. His conclusion, based on all of the studies reviewed, was: "Use of barbless hooks does not significantly reduce mortality and restrictions requiring use of barbless hooks are not biologically justified." Mongillo (1984) also reviewed and updated the literature on hooking mortality to conclude: "There is no valid technical basis for requiring single barbless hooks." This matter was further discussed at Wild Trout Symposium III in 1984 with a similar conclusion. Last year one of my articles published in Trout Magazine mentioned the consistent agreement among hooking mortality studies that demonstrate no significant differences in mortality of fish caught and released on single, treble, barbed, or barbless hooks. I received responses of disbelief and outrage. I certainly didn't intend to lead a crusade against barbless hooks, but only to point out that to achieve the broadest base of support for special regulations, unnecessary, discriminatory restrictions should be avoided. I believe the use of barbless hooks helps to promote a proper reverence for the sport, but their use should be a matter of individual choice, rather than mandated by law. The most appropriate method to encourage the more widespread use of barbless hooks is to establish evidence that a higher proportion of strikes are hooked and landed with barbless hooks in comparison to similar barbed hooks. Knutson (1987) compared catches of Chinook and Coho salmon caught with equal effort by two groups of anglers, one using barbed hooks and the other using barbless hooks. A total of 712 Chinook were landed on barbless hooks and 679 on barbed hooks. For Coho the results were 55 to 53 in favor of the barbless hooks. If similar studies are made on trout fisheries, perhaps with a view of developing the most effective design for barbless hooks, and if the results consistently demonstrate an advantage of barbless hooks in the percentage of strikes hooked and landed, there will be no need for an unnecessary and discriminatory regulation that frequently makes honest but forgetful anglers into law breakers.

Some breakdowns of communication concern the selective filtering and distortion of information to conform to preconceived notions. Clark and Alexander (1984) presented a paper at Wild Trout III symposium and published it in the proceedings. It concerned the decline of the brown trout fishery in the Au Sable River, Michigan. In comparison to the 1950s and 1960s, brown trout growth rate and biomass significantly declined during the 1970s and 1980s, despite various types of special regulations imposed on the fishery. The cause of the decline is well known and was clearly stated by Clark and Alexander. In the early 1970s after the diversion of sewage effluent and closure of a large production hatchery and the loss of its effluents, artificial enrichment of the Au Sable ceased; nitrate levels were reduced by 70%, and this reduction was reflected in reductions of primary and secondary production and, predictably, in the trout population. Thus, Clark and Alexander (1984) concluded: "No change is fishing regulations is capable of returning the number of large brown trout observed there in the past. Brown trout growth has declined, and short of fertilizing the river with sewage again, we doubt if growth can be returned to its former levels." In the spring of 1986, a letter was published in Rod and Reel magazine, stating that the Federation of Fly Fishers' Board of Directors had passed a resolution supporting no-kill regulations on the Au Sable River -- and this resolution was "based on a comprehensive report by Michigan DNR given at the Wild Trout III symposium," i.e., Clark and Alexander (1984). The letter proceeds to completely distort what Clark and Alexander had attempted to communicate with some additional innovative fabrications. The final irony was a request for readers to write to the governor of Michigan to "urge long-term studies such as have been conducted in Montana." The fact of the matter is that Clark and Alexander's paper was based on what is probably the longest continual study on an American trout stream. These Michigan DNR studies provide a wealth of information on numbers, biomass, growth, mortality rates by year-class, angler catch, etc., based on more than thirty years of sampling and creel census. Despite the soundness of the biological basis for determining the most appropriate regulations for the Au Sable, the main Au Sable River (the "Holy Waters" section) has the distinction of generating the most bitter and long-lasting controversy over angling regulations. People management indeed dominates fish management on the Au Sable. The root of the problem here is likely to be found in the workings of the administrative structure of the Michigan Department of Natural Resources. By the time the facts, data, and information so excellently developed by the biologists are transformed and communicated through administrators and the information and education section to the public, a leadership vacuum is created and dissatisfaction and disagreements are institutionalized and expressed against the DNR (the faceless bureaucracy) rather than direct communication with a real-life authority figure.

In any event, the question of to have or not to have no-kill regulations in the "Holy Waters" section of the Au Sable is now in court.

The Michigan DNR in a court deposition officially recognized that the matter is a sociological issue and not a biological issue. Seeking a sociological resolution, the DNR contracted with Michigan State University for a survey in angler attitudes on no-kill regulations for the Au Sable. I recently received "A Report to the Au Sable River Anglers form the Department of Fisheries and Wildlife, Michigan State University - Findings of the 1986 Au Sable No-Kill Attitude Survey." It is clear from the survey that the controversy has gone on too long; sharp lines have been drawn, sides chosen, and minds firmly made up; and they are not going to change. I did note, however, that misleading biological questions were incorporated into the sociological survey. Anglers who were against no-kill regulations and who expressed a belief that no-kill regulations would not improve the Au Sable fishery were asked if they would change their position "...if biological evidence from the South Branch study indicated that no-kill would produce satisfactory results in the mainstream." This is a classic case of confusing apples with oranges. The South Branch Au Sable has much reduced recruitment, much higher invertebrate abundance, and a significantly higher growth rate of its brown trout population in comparison to the main Au Sable (Stauffer 1977) -- all factors that would favor success of no-kill regulations to produce a significant increase in larger, older trout. This misinformation exemplifies the danger of inductive reasoning and is a common phenomenon of special regulation controversies. If it works in the Yellowstone River, it will work in the Au Sable or Carp Creek, etc.

Finally, I would call attention to the first paper in the 1977 proceedings, "A Tribute to Roderick L. Haig- Brown" by Richard May. The passing of Haig-Brown left a leadership vacuum for an authority figure for anglers seeking advice and guidance on technical matters such as special regulations. Besides his ability to write about a subject in a learned and fascinating manner, what set Haig-Brown apart from many other angling authors was his deep interest in his subject matter which extended far beyond the tackle and tactics of catching fish. He avidly read and understood the scientific literature. He maintained a position of knowledge and authority based on "doing his homework." He was informed before he wrote. Haig-Brown, if he were still with us, might fish with barbless hooks because of personal preference, not because he knew he would kill fewer fish. He might be in favor of no-kill regulations on the Au Sable River, but certainly not because of the Clark and Alexander report given at the Wild Trout III symposium. Before he committed himself to print on a matter, he read, synthesized, and understood the technical background of a subject. There has been no angling author of such broad appeal and influence before or since Haig-Brown whose work contains a comparable ring of authenticity and honesty. Although he had a wonderful style, substance was never sacrificed. In regards to the future successes with people management, our prospects would certainly brighten with the emergence of a communicator of the calibre of Haig-Brown.

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