THE INFLUENCE OF SIX RIVERS NATIONAL FOREST TIMBER HARVEST ON SALMON FISHERIES: FOREST SERVICE ALLEGATIONS IN SEARCH OF SCIENTIFIC SUPPORT

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ABSTRACT.

The recent Timber Harvest Scheduling study done on the Six Rivers National Forest contained an attempt at biological and economic analysis of the impact of timber harvest on salmon fisheries. The fundamental approach taken contained so many serious flaws as to render the results invalid. Errors were committed in each step of the analysis from hydrology to biology and population dynamics to improper economic analysis.

Forest Service investigators misapplied the results of research done in Idaho to estimate the influence of sedimentation induced by timber harvest on populations of juvenile salmonids, and then improperly assumed a linear and proportional relationship between reduced juvenile population and reduced recruitment of adult salmon in the current and cycle years. The nature of the multi-habitat, multi-stage lift cycle complexities connecting spawning and rearing to ultimate adult recruitment must be incorporated properly into the analysis of the effects of timber harvest.

Forest Service analysts attempted cardinal value estimates of salmon attributable to the National Forest which appear to contain systematic upward bias. Use of these values in comparison with timber values implies a mutually exclusive trade-off between timber harvest and salmon which time-series analysis does not support. Use of average values for estimation of losses of salmon resulting from effects of timber harvest is inappropriate. Demand-curve parameters should be estimated and employed to estimate marginal value changes, if any exist. Forest Service analysts misapplied the results of the Washington State study of recreational fishery values published by Mathews and Brown in 1970. The result is upward bias in recreational fishery values attributable to National Forests. The biological data base is inadequate to permit reliable estimates of the trade-off, if any, between timber harvest and salmon fishery values. Correction of these deficiencies requires a major effort at fish population monitoring over time. Correction of the economic analytical framework used for analysis also requires a substantial research effort in order to put salmon valuation on a basis comparable to that of timber resources.

INTRODUCTION

I appreciate very much the opportunity to appear before the California-Nevada Section of the American Fisheries Society and share my views concerning the linkages between timber harvest and salmonid stocks, and the valuation of salmonid fisheries attributable to particular watersheds. Before proceeding, allow me to file both a disclaimer and a note on my qualifications for speaking on the assigned subject. I am not and make no pretense at being a fishery biologist. I noted with interest and pleasure that Fred Everest, who preceded me on the program, spoke on economics and did a very creditable job. While he reinforced some points I wish to make, I do have some fundamental problems with part of his approach to salmon valuation attributable to a given forested watershed. Since we have heard a biologist speaking on economics, there may be a measure of justice in having an economist speak on fishery biology. It is with an easy conscience then, that I make salmon population dynamics the major thrust of my message. As a fishery economist I am familiar with the literature on fisheries population dynamics, one of the fundamental bases for the construction of the bioeconomic models with which we deal in fisheries economics. It might be pointed out that perhaps the first formal theory of population dynamics was advanced by an economist, and that his famous work still exerts a very powerful influence on the life sciences. Of course, Malthus was not only an economist but a preacher as well. While I cannot lay claim to being ordained, I do enjoy pounding the podium occasionally and maybe breathing a bit of fire and brimstone.

I am not going to tell you anything today that you do not already know about population dynamics, but I am going to try to remind you forcefully of some aspects of salmonid population dynamics that appear to have been ignored in some of the recent published works of the U.S. Forest Service. Moreover, I have conferred with a number of fishery biologists with respect to my conclusions and have received general agreement. These authorities included William F. Royce with whom I closely compared notes as we each worked on critiques of the fishery aspects of the Six Rivers National Forest Timber Harvest Scheduling Study on behalf of the Western Timber Association. I also have conferred with John DeWitt as we each prepared critiques of the Smith River Draft Management Plan for Terrascan, Inc. Also, George Allen discussed my work with me on several occasions.

It is highly tempting for me to blame these distinguished gentlemen for any errors I may have committed, but I am forced to excuse them from any responsibility. In preparing these remarks for delivery today, I have drawn heavily on my recent works on the topic published elsewhere. The present paper is an adaptation summary of my earlier critiques of the Six Rivers National Forest works on salmon fisheries.

The present paper is related to those sections of the "Timber Harvest Scheduling Study, Six Rivers National Forest," 1979, U.S. Department of Agriculture, Forest Service. Specific sections of the report are pages C-28 through C-31 and pages D-15 through D-18. The present writer has identified serious errors in the Six Rivers report in the treatment of the linkages between timber harvest and anadromous fish losses, and in the treatment of the resulting alleged economic losses. The errors identified render the sections on anadromous fisheries invalid. These matters are treated in three parts: (1) a brief summary of the Forest Service Study; (2) the linkages between timber harvest and salmon populations, and (3) fishery valuation issues.

Six Rivers National Forest investigators are not entirely to blame for errors contained in their report. Some of the errors are simply carried into the report from the literature on which the linkages were based. The literature on valuation of fishery resources also suffers from poor basic economic research, especially in the case of the valuation of sport fisheries. Thus, a critique of the Six Rivers study must begin with a review and critique of the literature upon which the study was based. Key documents identified for review include Brooks and Cline (1979), Platts and Megahan (1975), and Smith (1978). Additionally the major work on which Smith rested his study, Mathews and Brown (1970) is also given a brief review.

A BRIEF SUMMARY AND CRITIQUE OF THE FISHERY ASPECTS OF THE SIX RIVERS NATIONAL FOREST TIMBER HARVESTING SCHEDULING STUDY AND RELATED DOCUMENTS

The Six Rivers study employs evaluation techniques related to anadromous fisheries that rest heavily on the work of Brooks and Cline (1970), and Platts and Megahan (1975) and Platts (undated). Fishery valuation effects were drawn from Smith (1978). These documents were used by Six Rivers investigators as the basis for deriving an algorithm that links acres of timberland harvested annually within the streamside zone, to reduction of fishery value due to the disturbance of the streamside and the increase of delivered sediment.

A functional relationship between percent increase of delivered sediment and "percent decrease of fish population" ascribed to Platts was produced and incorporated into the algorithm. This model was then employed to analyze a number of possible alternative timber harvest schedules on Six Rivers Naitonal Forest with respect to expected fishery valuation losses. The results were compiled in Table C-11.

The sediment output index employed in the construction of Table C-11 was reviewed and discredited, both by Wooldridge (1970) and by DeWitt (1980). No attempt will be made here to review those critiques. The present writer reviewed the implications of the algorithm as it relates to salmon population dynamics and fishery valuation. The conclusion drawn was that the Six Rivers investigators had erroneously made the implicit assumption of a linear and proportional relationship between reductions in the populations of fry, and adults. That is, the "percent decrease in fish population" identified as fry by Platts, was applied directly to adult salmon escapement estimates contained in Smith (1978). These matters are discussed at greater length below.

A REVIEW OF THE LINKAGES BETWEEN TIMBER HARVEST AND ANADROMOUS SALMON POPULATIONS

The effects of timber harvest on salmom populations operate through various hydrologic effects. For anadromous stocks the essential relationship must show the influence of timber harvest not simply on incubating eggs, alevins, and fry, but on the resultant recruitment of adult salmon in the cycle year when they enter the commercial and recreational fisheries. Hydrologic effects of timber harvest that influence fry production and survival include accelerated erosion leading to increased sedimentation, reduction of watershed evapotranspiration losses and hence low-flow augmentation of streamflow, modification of the streamflow temperature and dissolved oxygen regimes, changes in biochemical oxygen demand relations through possible deposition of organic material in streams, and alteration of populations of terrestrial and aquatic stocks that enter as variables in equations of salmon population dynamics, either as elements in the salmon food chain, as predators, or as competitors. The controversy about the impact of timber harvest on salmon stocks arises because of the enormous complexities of the dynamic relationships of these interdependent effects which are only partially understood at present. The fact that this is so is indicated in the report by Gibbons and Salo (1973) on a workshop on logging and fisheries held at the University of Washington in November, 1972. The following is a quote from that report:

> From the resulting discussions as well as the questionnaire, one can assume we just don't know enough about our fish populations, especially those in logged watersheds. The most frequent question was: "How can changes in populations be measured if the effects are subtle and our knowledge of dynamics is deficient?" Life history research which is directly applicable to land-use problems is definitely needed.

The foregoing statement remains true to the present day. Yet some writers make quantitative estimates without substantiating evidence, and without any apparent qualms.

Although much is known about the influence on fry production of each of the hydrologic effects of logging singly, the combination of all the interdependent effects operating simultaneously is much more difficult to measure. The most common omission in studying the influence of timber harvest is failure to take account of the complex spawner-recruit relationships. These relationships have been intensively and independently investigated by Richer (1958), Beverton and Holt (1957), Paulik and Greenough (1966), Chapman (1970), Paulik (1970) and others. These complex relationships can be incorporated into studies that attempt to measure the influence of logging on salmon through analysis of time-series data on <u>adult</u> salmon stocks. These analyses, however, face problems of their own, which are discussed below. In any case it is not correct to assume either implicitly or explicitly that there is a linear and proportional relationship between reduced fry production in any given year and resultant reduction in recruitment of adults either in the same year or in the cycle year. The Six Rivers investigators committed that error.

It is clear that Platts' work referred to survival to incubation of eggs, and to survival rates of fry in the <u>immediate vicinity</u> of the disturbance that produces sedimentation. The work generally does <u>not</u> show how localized disturbances affect total fry production in the entire stream. A logical question to ask is whether reduced fry production in a stream segment is offset partially or wholly by increased fry survival in other parts of the stream because of relaxation of the food chain and habitat constraints. This question has not received sufficient research attention to allow a definitive answer, to the knowledge of the present writer. Furthermore, the sediment output index employed in the Six River study was not adjusted to account for the sediment interception performance of streamside protection zones.

The nature of the spawner-recruit relationship and of its possible forms was extensively explored both by Paulik (1970) and by Chapman (1970). Figures 1 and 2 are adaptations and extensions of figures 4 and 1 respectively in Paulik (1970). Figure 1 shows relationships between spawners and intermediate stages in the life cycle of an anadromous salmon stock. This figure shows clearly that excessive fry production can in fact reduce production of smolts and adult recruits, which suggests that under certain circumstances, "thinning" of the stocks of fry can actually be beneficial. Concatenation of these multi-stage, multi-habitat life-cycle relationships produces the usual spawner-recruit curve shown in Figure 2, which also clearly indicates the possibility that excessive fry production may reduce recruitment.

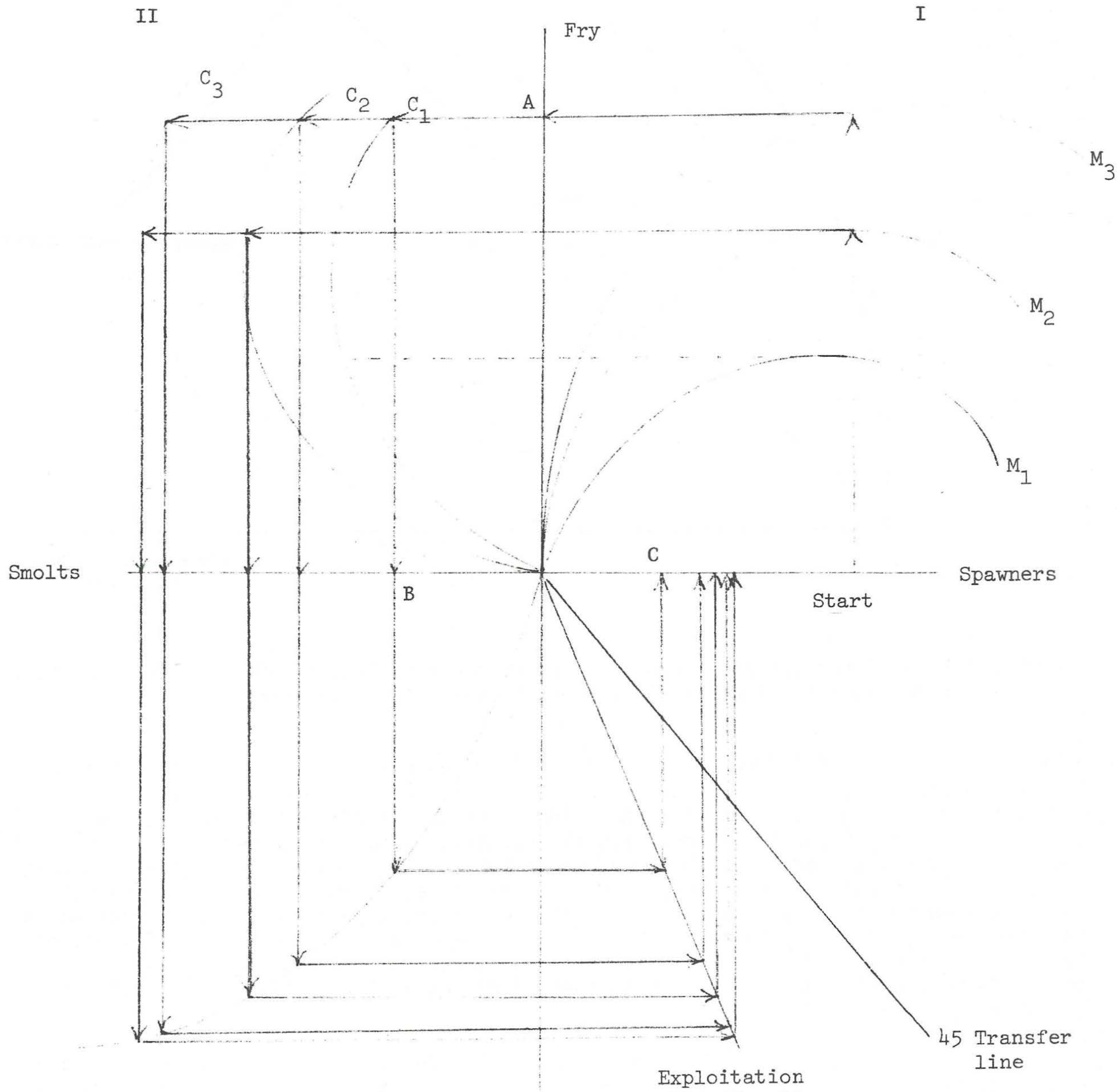

FIGURE 1. Spawner-recruit curves for intermediate life stages, adapted from Paulik 1970.

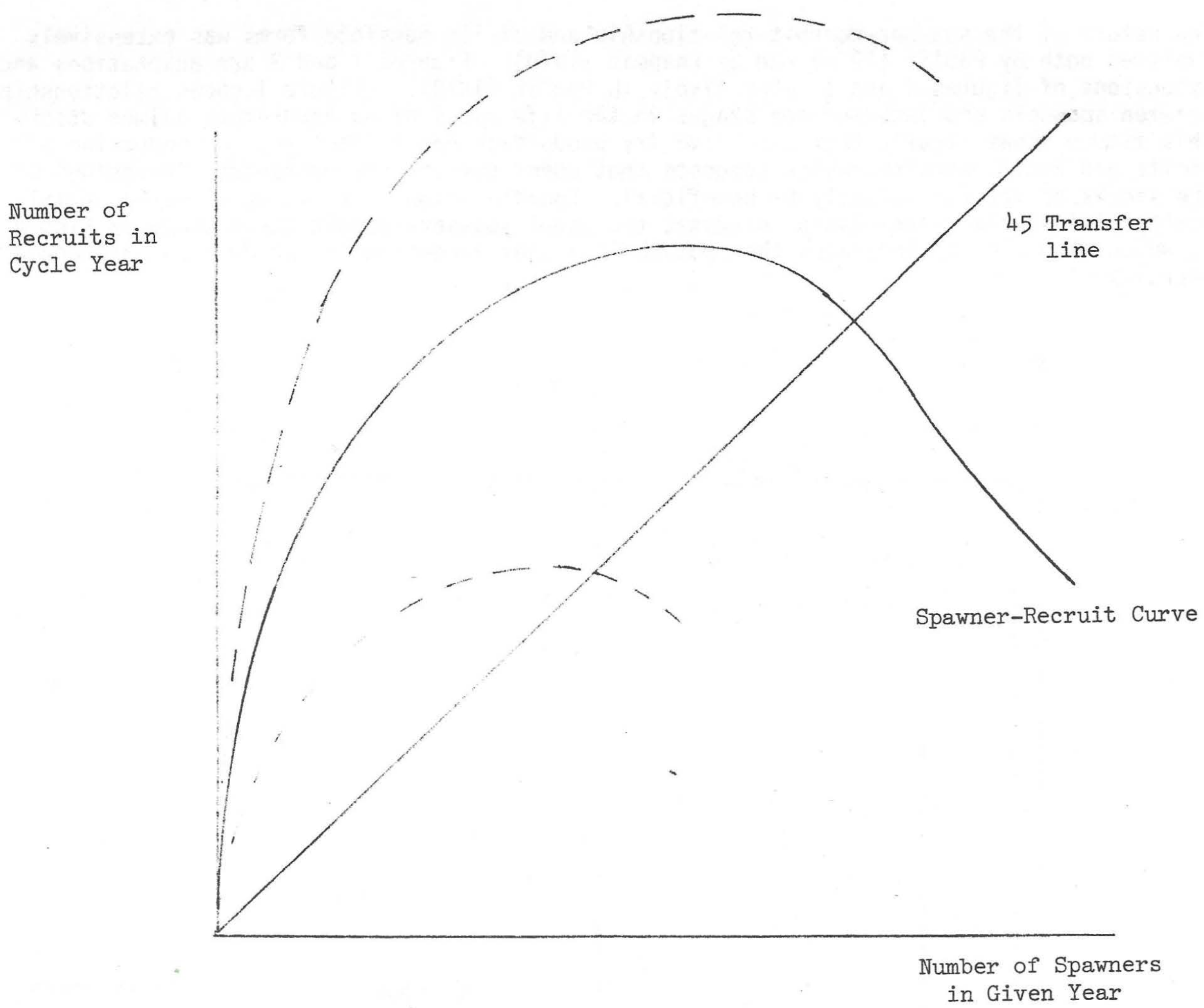

FIGURE 2. Form of the spawner-recruit curve, source from Paulik (1970). Identical to the form found by Ricker (1958), Beverton-Holt (1957) and others.

A brief explanation of Figure 1 is in order. From the point marked "Start" in quadrant I we are given the number of spawners. We move up to one of the carrying capacity curves marked M_1, M_2, or M_3. Suppose it is M_3. The curve then indicates fry production at point A. Given this fry production input into the estuary, we move in quadrant II to one of the curves marked C_1, C_2, or C_3 which depend on variable habitat conditions there. Suppose it is C_1. Smolt output will then be at level B. Ultimately this would lead to the start of the next spawning cycle at point C. Note that smolt output and adult recruitment would have been higher if curve M_1 had existed in quadrant I for the same number of starting spawners. The possibility of a family of carrying capacity curves in the ocean phase of the life cycle would further complicate identification and prediction of recruitment from given levels of spawners.

It is the general nature of many forms of biota to produce far more offspring than are ever expected to survive to maturity. For species of salmon which migrate from one habitat to the next during a complex life cycle, one must take great care in specifying the meaning of "habitat loss," or "degradation," especially if mortality or "thinning" at some point prevents excessive competition in a successive stage. This is even true of adult spawners. McNeil (1964) demonstrates, "There is evidence that the capacity of a spawning bed to produce fry is impaired by excessive numbers of adults spawning."

Successful adults recruited from naturally deposited redds amount to less than 1 percent of total eggs deposited. The thinning process takes place at every point in the migration during the life cycle. Many unresolved questions remain with respect to this process for salmon stocks. Paulik (1970) suggest that:

> The interplay of compensatory and depensatory forces can be extremely important; depensation in one stage may be overcome by strong compensation in the next stage. Strong compensating factors in one stage can effectively limit total production. When designing protection facilities such as fishways at dams, or artificial production enhancement facilities such as hatcheries, the interaction of the various life stages must be considered. (emphasis added)

Paulik goes on, in the same paper, to suggest techniques for incorporating environmental factors in the stock-recruit relation. I quote Paulik again at length:

> In many circumstances it is reasonable to assume that one of the biologically relevant parameters of a spawner-recruit curve, such as maximum recruitment, replacement recruitment, or spawners at maximum recruitment, may be directly related to an environmental factor...at extremely high spawning densities, recruitment can be increased only if environmental limitations are lifted...
>
> The function expressing the relationship between a biological parameter such as maximum recruitment and an environmental factor such as temperature, may itself reach some maximum value for an optimal temperature and fall to lower values on either side of the optimum. Environmental factors can simultanesouly shift the locations of more than one parameter. When this occurs, it may be necessary to represent recruitment as a response surface over the environmental factor - spawner plane. Obvious extentions exist in the direction of additional environmental factors.

The question raised by these considerations is, precisely where in the salmon life cycle does the environmental "bottleneck" that restricts recruitment occur? Environmental factors in all of the successive habitats occupied must be considered. The present writer believes too little is known to be able to provide an answer, but some papers in the literature suggest that the essential limitations may operate most strongly in the estuarine and early stages in the oceanic migration rather than during the freshwater life stages, if freshwater fry production exceeds some minimal level. Mathews and Buckley (1976) report very high initial mortality for Puget Sound hatchery coho smolts (about 75 percent) within the first 2 1/2 months of saltwater residence. Ultimate recruitment of adults was also shown to be significantly related to smolt size at the time of hatchery release. Smolt size of natural stocks is unquestionably related to the density of juveniles during the freshwater and estuarine stages. But Mathews and Buckley did not study the relation between death rates and number of smolts in the saltwater environment.

Reimers (1973), in studying the Sixes River estuary in Oregon, reported that the estuarine density of the fry population, "is hypothesized as a major cause of the depressed rate of growth of juveniles." Reimers also showed that, over a considerable range of fry input above a minimal level, smolt output from the estuary was almost independent. This suggests that the estuary may be much more than merely figuratively, a bottleneck, although one should be careful in making this general conclusion from a sample of a single small estuary.

It seems strange to the present writer that the possibility of beneficial thinning at various stages in the life cycle has apparently never suggested itself to fishery biologists as a possible management tool, but that is again undoubtedly related to the imperfect knowledge of salmon population dynamics. Thinning is a very important tool in the hands of practicing managers of many forms of bioculture. It is common in many forms of agriculture, and is probably overdone in forestry. Perhaps mutual benefit would derive from fishery managers and forest managers having some discussions along these lines. Perhaps it would suffice to encourage some fisheries biologists to cultive a crop of carrots or onions as a possible object lesson.

The 1972 workshop reported by Gibbons and Salo (1973) reached some very interesting conclusions:

> Judging from published research, adequate impact statements (of the impact of logging on salmon) obviously will de difficult to formulate. Consequently, regulations will continue to be conservative, with an increasing amount of field supervision required to monitor whatever environmental impacts are predicted... Meanwhile, general survey-type research...has had its day; and the logger, the researcher, and the management agencies must realize that there are no finite answers, no finite guidelines, and perhaps never will be... This is particularly difficult for the land user to accept, but it is a fact of life and a state-of-the-art for some time to come. On the other hand, the fisheries resource manager has difficulty accepting that which is becoming obvious -- i.e., logging (even clearcutting) can be performed with radical damage, in fact, the changes can be so subtle as to defy measurement and at times may, indeed, be beneficial.

The latter statement is of no comfort to the forest manager, who is apparently in the position of being assumed guilty until proven innocent. The social cost of this assumption of guilt is going to be enormous if the Six Rivers study is taken as only one example of the policy implications.

I suggested earlier that one way to attempt the incorporattion of environmental factors into the complex spawner-recruit relationship is to investigate time-series data on adult salmon stocks. These analyses, as suggested, present additional difficulties. The existence of these problems is indicated by noting that where this method has been tried, no statistically significant differences in runs attributable to the effects of logging could be identified. The problems of identification are several: (a) unexplainable variation in regression equations, (b) spawner-recruitment density-dependency effects, and (c) the possibility that there are offsetting effects at different points in the life cycle as suggested by Paulik. The first difficulty is illustrated by Gibbons and Salo (1973) who state:

> Early studies attempted to evaluate the effects of logging on stream environments by comparing the numbers of adult salmon returning to logged watersheds.
>
> These studies were not capable of discerning causes and effects because they were masked by a fluctuating saltwater survival, and freshwater mortality caused by sedimentation, floods, droughts, and temperature changes. For example, it has been reported that changes in an adult salmonid population of less than 50 percent due to any one cause would be difficult to detect within the large natural variations.

The present writer interprets the situation described in a matter undoubtedly different from that of Gibbons and Salo. These tests do not provide a basis for rejection of the null hypotheses that logging does not significantly affect salmon. Further, until one finds a way to reduce unexplainable variation due to unknown events and fishing pressure, the elimination of the relatively smaller possible influence of logging by regulation of forest practices is futile. Those with regulatory authority, however, in their drive toward "conservatism" are willing to adopt extreme measures for protection of the stocks from every conceivable influence except the one factor undoubtedly most responsible for adult stock reductions, namely excessive fishing pressure. Analytically the investigator confronted with adult stock data of the type indicated in the quote is loath to follow the scientific canons with respect to drawing inferences. Instead of accepting the alternate hypothesis, namely that logging does not significantly affect salmon stocks, they instead "conclude" that their tests were inconclusive and that they must try another way to document the adverse impact. That suggests to the present writer that many are engaging in what Samuel Eliot Morrison, the famous historian, called "frame of reference" thinking, whereby one decides what the result of analysis will be and thence seeks facts to support it, ignoring any that do not.

Salo, among others, has argued recently that time-series analysis of logging and adult salmon stocks has failed to detect the influence of logging because the study streams are generally "underseeded" from the effects of over-fishing. While stocks undoubtedly receive heavy fishing pressure both offshore and in the river, the reference point in the life cycle for definition and comparison of the degree of seeding must be chosen with great care. It may not be proper to identify carrying capacity of part of a freshwater habitat for populations of juveniles of given age as the criterion for being fully seeded, because seeding to that level may by itself represent overseeding relative to some downstream bottleneck that restricts ultimate recruitment independently. Carrying capacity is a dynamic variable, not a static constant, and it changes independent of factors that visibly change the habitat. This makes reliable estimation of the carrying capacity nearly impossible. Thus, stocking artificially to such a carrying capacity estimate, and then attributing observed mortality, through all the life cycle phases including ultimate adult recruitment, to habitat changes caused by logging, is highly questionable. It ignores constraints that operate independently elsewhere.

Lantz has suggested that resident cutthroat trout stocks be considered as an indicator species for the influence of logging on anadromous salmon stocks. Such an approach is clearly inappropriate because it ignores the multi-stage, multi-habitat life cycle of the anadromous stocks in comparison with a species which is present in a relatively localized freshwater habitat for its entire life. The Six Rivers study cites a 1971 Lantz report concluding a 75 percent reduction in the long-term cutthroat trout population occurred because of sedimentation and thermal pollution following logging and, therefore, because of logging. A legitimate issue with respect to these results is the extent to which the resident trout populations might have been reduced by increased recreational fishing pressure facilitated by access improvement provided by the logging road.

COMMENTS ON ALLEGED LOSS OF SALMON VALUES ATTRIBUTABLE TO TIMBER HARVEST ON THE SIX RIVERS NATIONAL FOREST

Perhaps it should go without saying that if one cannot reliably show how timber harvest affects adult stocks in the cycle year, then it follows that neither can one legitimately count the loss in value. There are, however, several points that are not obvious that need to be made. Some of the flaws in the valuation procedure used by the Forest Service are so serious that they cannot be left unchallenged.

First, the use of estimates of average annual numbers of spawners as proxies for current catch by applying catch-to-escapement ratios is questionable, since catch and escapement move in opposite directions. Also, if a disturbance generates incremental changes in stocks rather than total destruction, then it is not appropriate to apply average values to the estimated stock reduction to calculate total loss in value. Rather, demand-curve parameters must be employed to estimate the marginal change in value for use in calculating total loss. This issue is examined more extensively at a later point in the paper, and Figure 3 is employed to illustrate the point.

The Forest Service used an approach for salmon valuation which is not comparable with the correct approach taken in the report with respect to present worth calculations of alternative timber harvest schedules. It would have been highly desirable to adapt the approach taken for timber valuation to salmon valuation. This methodology was probably preempted by the approach taken by Smith (1978). The critique of the Six Rivers salmon value estimates must include a brief review of the flaws in the Smith report.

Before proceeding it may be of interest to make some gross comparisons of the cardinal-value estimates contained in the Six Rivers study with estimates made for the forest at another point in time. In a draft of a letter from Edward P. Cliff to Congressman Don Clausen prepared in the Region 5 Office (under a cover letter dated August 26, 1970, and signed by R. Max Peterson) the following very interesting statement is found:

> We estimate that approximately 35 percent of the anadromous fish utilized in the Eureka to Crescent City area originate on the Six Rivers National Forest. The average annual direct economic benefits that accrue from use of this resource for commercial and sport fisheries are estimated at a minimum of $720,000 for the Six Rivers National Forest.

Doubling this figure which more than accounts for inflation still leaves an estimate radically less (a factor about 1/10) than that obtained by Smith, cited in the Six Rivers report.

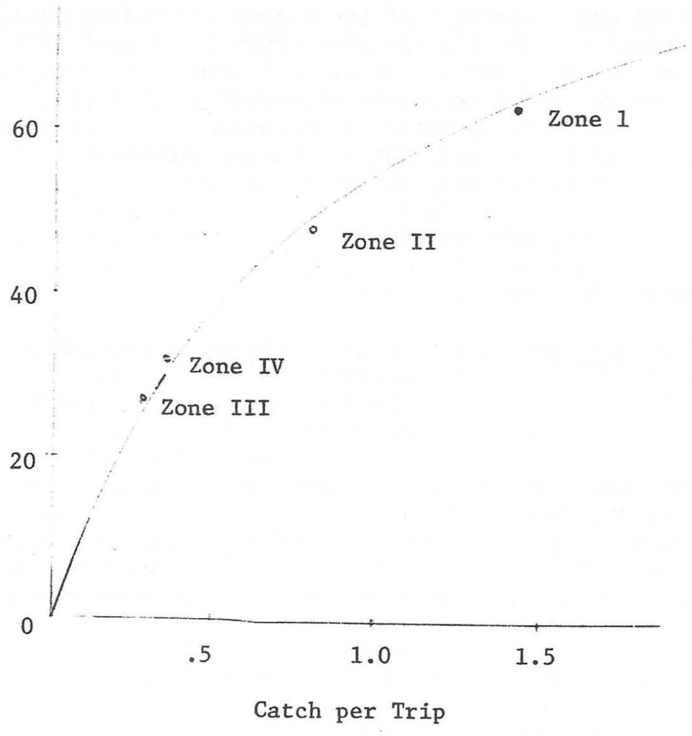

FIGURE 3. Relation between average value per trip and average catch per trip for Washington salmon sport fishery, 1967. Source: Mathews and Brown, 1970. Curve was fitted to the four data points ocularly by Mathews and Brown.

It seems inconceivable to the present writer that the $11.1 million figure estimated by Smith could be correct. Although this figure contains sport fish values it seems incredible that the total value of salmon attributable to the Six Rivers National Forest is in excess of the value of commercial salmon landed in the entire state. The comparison ought to at least raise questions about the validity of the latest Six Rivers estimates.

The valuation of commercial fishery resources is a relatively simple matter of applying ex-vessel prices of commercial landings to the quantity landed. For small exogenous changes in the volume of landings, the change in total value may be estimated by simple multiplication and subtraction. However, when changes in the volume of landings independent of demand shifts are large, whatever the cause, two additional considerations must be taken into account; the parameters of the demand curve and the influence of supply disturbances on price, and the long-run supply effect occasioned by population dynamics and the adjustment of the fishery stocks. These effects are often overlooked or assumed implicitly.

The valuation of recreational fisheries is, of course, enormously more complicated, because no market transactions exist from which the value of sport-caught fish can be directly derived. A matter commonly overlooked with respect to the employment of various proxies for estimating these values is the separation of the value of the fish from the value of the fishing experience. This separation is of crucial importance in evaluating sport fisheries, because over some range of fishing success as measured by catch per angler day, the value of the fishing experience may be almost independent of the catch. In any case the catch is subject to the principle of diminishing marginal utility. The value of the fish is probably best indicated by its retail price in the marketplace, or by the retail price of close substitutes. How many fishermen have ever captured the "brag" value of a catch by buying fish to take home at the market or trout farm? How many have purchased fish to take home to authenticate that they went fishing instead of philandering? The question is, what is valued, the fish per se or some associated ephemeral experience? The value of the fishing experience (functionally related to catch) is best viewed as a residual between total value of an angler day and the market value of the fish or its close substitute.

The use of the catch-to-escapement ratios to estimate catch raises more serious problems. The problem of successful management of salmon stocks is compounded by the absence of racial selectivity in the offshore salmon fishery. Salmon management rests heavily on the regulation and control of escapement. The lack of racial selectivity of the offshore troll fisheries (both sport and commercial) means that some of the stocks will be more heavily overfished than others. It may also be the case that some stocks are actually underfished! As a result the catch-to-escapement ratio is a highly unstable variable, and serious doubt is cast on the use of a single "the" value of a spawner universally, which is based on its use.

In cases such as the Smith report where escapement has been used as a proxy for the catch by employing the average ratio derived from fin clipping and tagging studies in hatchery stocks, the actual catch attributable to a particular stream in any given year will be either overstated or understated by an indeterminate magnitude, perhaps of several orders. Such estimates may further be consistently biased in one direction or another because this method does not take into account productivity differences from stream to stream, and in different parts of the same stream.

The escapement data employed in the Six Rivers study is highly questionable, being largely guesses contained in the 1965 California Fish and Wildlife Plan. A June 1979 report, of the Pacific Fishery Management Council states:

> Reliable estimates of past and present spawning escapements for coastal streams are not available, nor do we know at what rates coastal salmon contribute to the ocean fishery. Studies are now under way to determine the abundance of salmon in the Klamath River system and their contribution to the ocean fishery, starting in 1979. Estimates of fish produced in other coastal streams can be made once the production of the Klamath system is known.

More recently, the Pacific Fishery Management Council (1980) estimated Klamath River escapement at less than 50,000 adults in 1979. Successful spawners would apparently number far less after the gillnet harvest in river is deducted. Spawning escapement is thus much less than the 183,000 (Chinook and Coho) estimated by the 1965 California Fish and Wildlife Plan. While reduced escapement in 1979 may reflect higher offshore catches, it probably also reflects lower offshore stock levels caused by reduced recruitment arising from low escapement in previous years. Thus applying a 5 to 1 catch-to-escapement ratio to the 1965 figure represents dramatic overstatement.

An alternative method of using actual escapement data might be to employ standard models such as that attributed to Ricker, or to Paulik, as the basis for bio-economic models of the fishery of a particular watershed. Such models could be used to obtain discounted present values of the stock in perpetual dynamic management, rather than to use escapement as a proxy for valuation of a single period flow. If this methodology is employed it permits consideration of the density dependent effect of escapement on recruitment to be taken into account, thus allowing the marginal value of a spawner to be estimated in a particular stream. Thus, if escapement is excessive, the resultant reduction of recruitment in the cycle year means that the marginal value of a spawning pair is negative. If escapement is less than sufficient to provide for optimal replacement levels, then the value of a pair of spawners is escalated at the margin.

The point to be made is that there is no "the" value of a salmon. One cannot take value correctly estimated for one area for application to one set of circumstances and legitimately apply them to another area and a different set of valuation purposes. This is especially true where recreational fishery values are involved because of some complexities of finding an appropriate method of proxy valuation, including the previously mentioned necessity of separating the value of the fish from the value of the fishing experience.

To illustrate the point, reference is made to Mathews and Brown (1970), where they state:

> Very commonly, in multiple use of land and water resources, a decision may be made which either permits the continuance of a particular fishery, or brings about its degradation or perhaps its termination. There are numerous examples, such as the choice of a water quality standard which could exterminate a salmon run. In cases where there is a potential loss of an existing sport fishery, a determination should be made as to the amount of compensation which the fishermen would have to receive to be no worse off after being precluded from the recreational opportunity they formerly enjoyed... Our criteria for establishing the net value of sport salmon fishing in Washington was to estimate the monetary compensation needed to replace the potential loss of salmon fishing in each of the four previously defined zones.

The four zones included three marine zones, (1) ocean, Ilwaco north of La Push; (2) Strait of Juan de Fuca and (3) Puget Sound. Zone 4 comprised all freshwater areas open to salmon angling in Washington State. There are several points to be made about this aspect of the Mathews and Brown study and the Smith report which was derived from it.

An important implication of the variation in angler day values from zone to zone is that it is hazardous to translate values from one zone to another. This seems particularly pertinent when lifted to an area two whole states removed from the zone for which the valuation was estimated.

The Mathews and Brown figures may themselves contain an upward bias. They made several estimates based on different methodologies including: (1) gross expenditures per angler day; (2) willingness-to-pay for the right to fish; and (3) willingness to <u>sell</u> the right to fish assuming an absolute property right exists.

Mathews and Brown rejected the first method as being essentially meaningless, which is correct. They estimated values for methods 2 and 3 from responses to survey questionnaires and rejected method 2 on the grounds that they believed the results to be biased downwards. This belief is undoubtedly well grounded and rests on two considerations. First, willingness to pay is constrained by the financial condition of the respondent, and second, if the respondent believes his answer will have an influence on the size of license fees, he will misrepresent his answer to avoid an increase. Mathews and Brown thus expressed a preference for method 3. But method 3 may contain an upward bias in a manner symmetrical to the downward bias of method 2. Mathews and Brown attempted to deal with this problem by assigning arbitrarily a value of $500 per year to those respondents who answered their question by stating that they would not sell at any price. While Mathews and Brown argue that this technique makes their estimates minimal, it by no means answers the charge of misrepresentation upward of the price at which the respondents would have been willing to sell their rights. I believe, therefore, that their estimates in fact are biased upward. I note that Everest has cited emprical evidence to support my purely <u>a priori</u> conclusion.

It is clear in the Mathews and Brown study that the figures apply to total elimination of the sport fishery and not to marginal changes in fishing success. On page 12, Mathews and Brown state:

> Although total value is of interest, it is not likely that the salmon fishery in any zone will be suddenly eliminated entirely. More likely is a gradual erosion of average catch per trip. The data yielded a very good relationship between average catch per trip and average value per trip (Figure 2) <u>which should be useful in determining loss in value from environmental degradation of salmon-producing areas.</u> (emphasis added)

Their figure shows a curve which exhibits diminishing marginal utility to increased daily catch. It is reproduced here as Figure 3. Such a relationship is pertinent in the separation of the value of the fish from the value of the fishing experience previously mentioned here.

Data given in Mathews and Brown may be reinterpreted in order to derive the relevant marginal value curve. This is done in Table 1. Interpretation of these data produces interesting results. What is the value derived from a trip in which no fish are caught? It is not zero! The estimate is obtained as follows:

TABLE 1. Angler days per salmon caught by zone in Washington, 1967

Zone	Catch per Angler Day	Angler Days Per Salmon Caught	Average Value Per Angler Day
3	0.29	3.45	$27
4	0.34	2.94	$32
2	0.80	1.25	$48
-	1.00	1.00	$55
1	1.40	.71	$63

Source: Column 3 calculated from data contained in Mathews and Brown (1970). Other data as in original, rounded.

1. Interpolate between Zone 1 and Zone 2 to find the value of a trip where one fish is caught. (One trip per fish = one fish per trip). The value is found to be $55.

2. In Zone 3 it takes 3.45 trips per fish and these trips had an average value of $27 each for a total of $93.15.

3. One of these trips would have been valued at $55, leaving $38.15 to cover the value of the remaining 2.45 trips.

4. Therefore, the value of trips in which the fisherman is "skunked" is $15.60.

5. The marginal contribution of the fish for a one-fish catch is therefore $55 minus $15.60 which equals $39.40.

The same approach can be used to estimate the marginal value of a second fish for a two-fish trip. Note that for Zone 1, the trips per salmon caught is 0.71. If five trips were taken, another way to express this is that about seven fish would be caught. This would generate total value of five times $63 per trip, or $315. Three of the five trips could be considered to generate one-fish catches each, while the other two trips each produce two fish. Three of the trips then generate $55 value each for a total of $165 which leaves $150 to cover the two other trips each of which produce two fish. Thus a two-fish trip is worth an estimated $75. The marginal value of the second fish in a two-fish catch is thus $20.

The Mathews and Brown study does not include data with would permit estimation of the marginal value of the third fish in a three fish angler day, but it can be supposed to be less than $20 (in 1967 dollars), but probably not as low as zero, which linear extrapolation would indicate.

What can we infer from the behavior of fishermen who release their catch? It may be more than merely an attempt to preserve the fishery, perhaps for the benefit of some other fishermen. It may instead indicate that the marginal value of the fish is zero. The derivation of a marginal value curve is necessary to evaluate the loss in value when the catch per angler day is <u>reduced</u> by environmental effects, rather than being eliminated altogether.

The Six Rivers investigators should have employed such methods instead of inferring the total loss of the fishery or inappropriately applying average instead of marginal values.

Where does this leave the Smith report, and by inference, salmonid-fishery aspects of the Six Rivers Timber Harvest Scheduling Study? The answer is obvious. With respect to economic "analyses" of fisheries contained therein, without any redeeming merit. A drastically altered approach to valuation is called for, and a sound biological data base is needed if anything better than a wild guess is to be made.

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