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TECHNICAL REVIEW

OF

A BENEFIT-COST ANALYSIS OF THE
ALASKA SALMON ENHANCEMENT PROGRAM

by

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This technical review assesses several documents completed during 1992 and 1993 which together examine benefits and costs of various policy options associated with Alaska's Salmon Enhancement Program. The three documents reviewed here are:

Boyce, John, Mark Herrmann, Diane Bischak, and Joshua Greenberg. *A Benefit-Cost Analysis of the Alaska Salmon Enhancement Program, Final Report*, December 1992.

Mark Herrmann and Joshua Greenberg. *An International Marketing Model for Alaskan Salmon*, February 1, 1993.

John Boyce. *Using Participation Data to Estimate Fishing Costs for Commercial Salmon Fisheries in Alaska*, November 1992.

The tasks set out for this review are to: i) comment on the technical aspects of the studies, paying particular attention to the soundness of methodology and assumptions used; ii) discuss what confidence ought to be placed in findings, and whether the level of

caution raised at various points in the documents is appropriate; iii) offer an opinion about whether the findings ought to be relied upon to make major directional changes in the Enhancement Program; iv) identify additional steps that might be taken to further test and validate the models; v) suggest ways to incorporate net benefit analysis in future policy analysis.

I. Introduction

Alaska's Salmon Enhancement Program is large and prominent among the world's most important hatchery programs. The Program has a long and successful history during which Alaska's fish runs have risen dramatically from lows in the 1970's to record highs in the 1980's. During the past decade, and largely in response to the very success of fisheries hatchery and aquaculture programs in Alaska and elsewhere, the market for salmon has undergone important changes. Thus it seems appropriate to review the Enhancement Program in light of these new market conditions with an eye towards planning for the next decade or longer.

Alaska's Enhancement Program impacts a variety of species, gear types, and regions, and generates costs and benefits that are shared by many individuals with varying employment and income opportunities. As a result of the wide reach of the Program, any contemplated changes are bound to be both complicated and controversial. For these reasons, it is important to analyze as

fully and carefully as possible any major changes that might be under consideration. Modeling is clearly useful as a procedure to help organize the mass of data that might be brought to bear on policy analysis.

It is important to recognize the limitations of statistical and simulation modeling, however. In most cases, models cannot provide an answer to complicated policy queries. Instead, models should be thought of as organizing tools, in that they can illuminate the implications of what is known about a complicated system and they can help identify the consequences of uncertainty about other parts of the system. In the case at hand, it is certainly not obvious on first glance exactly what policy decisions regarding enhancement are best for the State. Moreover, because there is such a considerable range of uncertainty about various critical parameters, it is unlikely that any modeling effort will produce definitive conclusions.

In sum, because Alaskan hatchery policy is wide reaching, it is important to place into perspective exactly what can be expected of an undertaking like this, and to what degree specific results ought to be relied on for policy choices. In this spirit, we review various components of the study and discuss both methodological, analytical, and philosophical issues raised by the approach and results. The next section discusses the market model. This is followed by a discussion of the cost model and a third section evaluates the overall conclusions of the cost benefit study. We then return to the question of how modeling studies such

as this one ought to be used in the policy process.

II. Methodological Considerations--Market Model

The heart of the conclusions regarding the levels of net benefits of the enhancement program come from the demand and price projections. The marketing model (summarized in "An International Marketing Model for Alaskan Salmon") is a very large econometric forecasting model. The model is divided into two parts, Bloc One and Bloc Two. Bloc One is a quarterly econometric model which models the world market for salmon products. The bulk of the conceptualization for this part of the work is contained in a 1990 PHD thesis by Mark Herrmann.¹ The present study extends that work, by adding two more years of data and by changing functional forms of the demand equations to allow varying elasticities. The main output from this part of the study, aside from a large set of equations forecasting traded quantities and prices, is two ex-vessel price equations for "high" and "low" valued species. These are used in Bloc Two to generate predicted annual ex-vessel prices by species and area.

The work contained in this part of the study represents a very large effort, considerably beyond that funded by the direct budget for this part of the study. The demand modeling portion of this

¹Herrmann, M.L. *An Economic Analysis of World Salmon Markets: Effects of Salmon Farming*, PhD Thesis, Department of Economics, Washington State University.

study capitalizes on the authors' very extensive knowledge and previous work on the subject, essentially several years of intensive study of data, estimation methods, and simulation model construction of world salmon markets.² In addition to the human capital embodied in prior experience, this part of the study also benefited from auxilliary funding by Sea Grant and the Univerity of Alaska. The bottom line is that there is a large amount of effort represented here, much of which was subsidized by other funding sources.

In view of the scale and complexity of this study, it is worthwhile at this stage to retrace some of the steps involved in generating final price estimates. In any complicated modeling exercise, judgement is exercised at many points in the chain, and hence caveats apply to final results. In what follows we focus on several parts of methods followed and offer a critique and assessment.

A. Data

The first issue that arises is whether the data used in the econometric modeling effort is adequate for the task of precision

²Some of this is summarized in other papers, including: Mittelhammer, R.C., Herrmann, M.L. and B.H.Lin, *An Economic Analysis of the Pacific Salmon Industry: Effects of Salmon Farming--Final Report*, Seattle, National Marine Fisheries Service, 1990; and Herrmann, M.L., R.C. Mittelhammer, and B.H.Lin, "Import Demands for Norwegian Farmed Atlantic Salmon and Wild Pacific Salmon in North America, Japan, and the EC", *Canadian Journal of Agricultural Economics*, vol. 43, 1993.

forecasts of future prices. There are two broad issues here. One concerns whether the length of time series data is sufficient. The second concerns whether certain variable choices and aggregation procedures introduce biases.

With respect to the length of time series used, Bloc One uses quarterly data beginning in 1982 (or 1983) and running to 1991. The short data series is due to the fact that some species-specific prices and quantities were not available until 1981. A first question one should raise is whether this is enough data to confidently forecast world salmon markets over a 15 or 30 year period. On the positive side, the market has witnessed a considerable amount of supply variation, which could contribute to low variance-estimates of critical elasticities if the demand structure remained stable. On the other hand, there is evidence that the explosive growth of aquaculture has disrupted traditional trade flows, suggestive of structural change in demands.

Since 1991 is dummied out of both North American ex-vessel price equations, the model is really only estimated over nine years. Moreover, even though quarterly data are used, one could argue that the fundamental relationships are associated with markets that clear yearly, ie. it is new supply, existing inventories, and demand considerations at the point of harvest (generally the third quarter) that essentially set up the market clearing conditions in these markets. Thus any activities which take place in the three quarters where little is being caught are incidental to the fundamentals that emerge in the harvest quarter.

This suggests a very serious degrees of freedom problem, particularly if structural change has been occurring in demand relationships.

A second data issue concerns the appropriateness of using export prices as proxies for domestic wholesale prices. In principle, ex-vessel prices emerge as an equilibrium in which derived demands from the wholesale sector and the export market interact to determine allocations. Hence the ex-vessel price equation ought to include wholesale prices, export prices, substitute prices in these markets and other shifters. In practice, there is bound to be multicollinearity between prices. It is also the case that US and Canadian wholesale prices at the species specific level are not particularly reliable, compared with, for example, those from the Tokyo Central Wholesale Market. Hence data limitations force the use of export prices in these equations. The results may not be particularly biased, for example, if most of the product is shipped to a particular market, such as Japan with sockeye. On the other hand, since the model aggregates species and products into low and high valued groups, there are omitted variables that should be used to characterize the wholesale/export market shifters. With pink salmon, for example, the raw product is allocated to several markets and product forms in several countries. Hence the misspecification potential may be important. It is not clear what the consequences of this data problem may be; on the one hand there are several variables that ought to be included. On the other hand, even if data were

available, there would be multicollinearity problems if they were included.

A third issue concerns the fundamental decision to aggregate into "high" and "low" valued species. Although this is done because of certain data limitations, there are implications of resulting aggregations. The first is that while it is true that the high-valued group does indeed contain the species whose prices are high, the individual species are not sold in similar markets. Sockeye is sold almost solely to Japan, while chinook and coho are sold in fresh/frozen North American markets, for example. Sockeye clearly dominates the group in aggregate value and hence the estimating equations are "weighted" in favor of the Japanese market. The usefulness of this aggregation for predicting species-specific prices over the next 15-30 years depends on the belief that the relative market conditions between sockeye and chinook/coho will remain the same as they have been over the past nine years. An important part of the issue concerns how aquacultured fish will compete with these species in the future. My guess is that the sockeye market will suffer relatively more than the chinook/coho markets as a result of competitive pressure from aquaculture. Although it is not perfectly clear how this would work its way through the forecasting model, it is most likely that the high-valued ex-vessel prices will be overpredicted as a group, and that chinook and coho prices will be underpredicted. With respect to "low-valued" species (pink and chum), although prices have moved together, these are also sold into different

markets with very different characteristics. There is no particular reason to expect relative market conditions in the mainly fresh/frozen chum market to be like those in the mainly canned pink market. It is less clear what the major factors will be that might differentially affect these markets and what impacts these might have on under- or over-prediction of specific prices.

A fourth data issue concerns the value of information contained in the off-harvest period values of ex-vessel prices. The two most important equations in this study are the high- and low-valued ex-vessel prices. In Bloc One, these are estimated over the 1982-1991 period using quarterly data. However, what is represented by data reported to record, for example, fourth and first quarter ex-vessel prices for species such as pink, chum, and sockeye? It is my impression that virtually no harvest is recorded during these periods for these species and in fact there are not any open seasons during these quarters. Hence it is unclear, for example, what low-valued ex-vessel prices (pink and chum) from the fourth and first quarters mean and what their influence might be.

B. Estimation Techniques-Bloc One

Mostly because of the degrees of freedom problems, the authors use an estimation technique, Bayesian Bootstrapping Two Stage Least Squares (BB2SLS), that is new and relatively uncommon in the literature. Its advantage is that it can incorporate researchers' a priori ideas about ranges of certain parameters that are to be

estimated. With a short time series this is particularly useful since it effectively allows researchers to weight the parameter estimation process with priors.

An important issue is how much confidence can be placed in the weight given to the priors regarding certain coefficients versus allowing the data to estimate the parameter in question. There are two types of information produced in the estimation procedure that allow us to assess this. The first is the "Probability of the Prior Range" (PPR). This measures the proportion of estimation iterations during the bootstrapping procedure which jointly satisfy the prior ranges assigned positive density by researchers. Thus, if researchers constrain price elasticities to be negative, and actual bootstrapped estimates produce many positive valued elasticity estimates, the PPR will be low. A second measure is the "Odds in Favor of Non-Uniform Prior" which assesses the support for the researchers' non-uniform prior beliefs versus the alternative of uniform priors. Although the PPR is provided for all Bloc One equations, the Odds in Favor measure is not reported for several, including the important ex-vessel high and low price equations.

Another estimation issue that is relevant is selection of variables in the final form trade model and those used as instruments. There has obviously been a considerable amount of "fishing" for variables that help the system fit the data. While this is common, it is not really rigorously acceptable and the properties of resulting estimates are not known. The authors suggest that they can provide additional information on procedures

followed, including instruments used, priors, etc. Without this information, it is difficult to judge how stable the "final" reported coefficient estimates are in this system. I would be willing to bet, however, that if these were reestimated using 1992 and 1993 data, coefficients and other diagnostics would be wildly different from those reported here.

C. Bloc One Assessment

There are essentially two ways to judge how reliable an econometric model like that developed in this study might be. First, one should subject the individual estimates to "reasonableness" checks. This means, for example, that unrealistic values for particular parameters ought to be cause for concern. Early studies of salmon demand, for example, often found income elasticities as high as ten, suggesting that salmon demand would increase ten percent for every one percent increase in incomes. This is surely unreasonable as would be demand elasticities with positive signs, etc.

If classical estimation procedures were followed, one could use confidence intervals, t-tests, etc. to judge the reasonableness of estimates of coefficients. In this study, a combination of statistics is presented to substitute. Since imposing priors effectively "constrains" the BB2SLS estimation procedure, the PPR and Odds in Favor measures are important as measures of the confirmation of prior beliefs. With respect to PPR measures, many

of the equations produce reasonably high values. The high and low valued ex vessel price equations have PPR's around .85, indicating that 15% of the bootstrapped iterations produced estimates outside of the range imposed by researchers' priors. Unfortunately, since we don't have information on what priors were imposed, it is difficult to assess what these are telling us. On the supposition that the ranges are wide and mainly imposed to account for qualitative restrictions (signs) on specific parameters suggested by theory, values in the 96% range are desirable. On this test, some of the equations estimated on Bloc One pass and with others with low values, there is good indication of misspecification, since the data without priors imposed "want" to produce an incorrect sign. With respect to the Odds in Favor measure, not much can be said for two reasons. First, many of the equations do not list the measure. Second, without knowing what priors were imposed, it is difficult to judge the reasonableness of the test.

A second test is how well a model forecasts. This is important in the context of this study because that is one of the primary purposes for constructing the model. Generally the most rigorous tests of forecasting ability are those conducted out of sample. With this procedure, one retains some observations from the data and estimates the model over the others. Then, the researcher examines how close the model forecasts over the retained observations. The researchers did not do out of sample forecasting in this case because there are too few observations.

Another less demanding test is to examine goodness of fit over

the data used to estimate the model. With these tests, one measures model variation around the actual data, ability to track turning points, etc. The Bloc One goodness of fit statistics are contained in Tables 1 and 2 and include measures of deviation and Theil U-Statistics which measure systematic and unsystematic parts of forecast errors. These measures are for endogenous variable solutions using the full model, including the Norwegian aquaculture sector which is ultimately dropped in the final forecasts. The Root Mean Percent Error statistics are good indicators of the model's closeness of fit and ability to track data. As can be seen, many, if not most, of the endogenous variables are forecasted with relatively large percent errors. The important ex-vessel prices (High and Low-Valued) have RMS errors of 18 and 24 percent, respectively. These are indicative of the minimum level of confidence one might place in point estimates from the demand model. With average percentage differences between 15 and 25 percent in predicted prices, one would expect even wider confidence bounds on predictions of future revenues due to additional errors introduced associated with the forecasts of exogenous variables.

D. Bloc Two-Estimation

Bloc Two is a modeling component which first aggregates the quarterly output from Bloc One and then disaggregates over different gear types, species, and areas to produce new estimates at a detailed level. Critical to the accuracy of Bloc Two

estimates is the information transformation in the two aggregation/disaggregation steps. In the first step, the information from the quarterly Bloc One model is aggregated into yearly data. The researchers do this by weighting quarterly data by relative harvest levels. This has the practical effect of throwing out most of the information contained in three of the four quarters since most fishing is concentrated in one quarter. Hence most of the information contained in the quarterly model is dropped, although as suggested earlier, this may be of little consequence in view of its unimportance in the market clearing process. The down side is that there are effectively only about 8 data points used per equation. The two critical outputs from this first step are shown in Table 6 and Figures 11 and 12 of the Bloc One discussion (pages 76 and 77). These plot predicted yearly ex-vessel prices against actual weighted ex-vessel prices for both low and high valued salmon.

The second stage of the Bloc Two procedure uses these two aggregated ex-vessel price equations to predict separate ex-vessel prices by region and species for several combinations. This is done as follows. First, data are aggregated within species/area across gear types to eliminate gear price differences. Then, five regions are selected, including Prince William Sound, Cook Inlet, Southeast Alaska, Kodiak, and Rest-of-Alaska. For each of these, separate equations which regress the species/area specific ex-vessel prices against the actual yearly high- and low-valued ex-vessel price series are run, using OLS. The coefficients from

these are used to predict area and species specific prices for the five regions, by running through Bloc One output from the equations predicting high and low valued ex vessel prices (aggregated to yearly predictions). The results of these Bloc Two estimates are presented in Tables 15-24 and Figures 3-27 on pages 88-110).³ Measures of goodness of fit and diagnostic statistics as discussed above are presented for each of these regressions.

E. Bloc Two-Assessment

Central to the entire procedure in Bloc Two are the two aggregated ex vessel price equations. As Figures 11 and 12 of the Bloc One discussion suggest, there is considerable error associated with predicted and actual weighted yearly prices. Part of the error is associated with the Bloc One equations themselves and another part is associated with information transformation when aggregating quarterly data to yearly data. The loss in accuracy from the latter step is particularly pronounced for the low valued species. The mean percent error increases from 18.6% in the quarterly model to 28.4% in the yearly aggregated model, while the RMSE increases from 24% to 32%. The degree to which these aggregated ex-vessel prices predicted by the model are different

³Care should be exercised in examining Figures for the study. In the Bloc One discussion, text figures are numbered from 1 to 12. At the beginning of the Bloc Two discussion there are two additional figures labelled 13 and 14 and then the subsequent figures are labelled beginning with 3 and ending with 33. Thus there are two sets of Figures labelled between 3 and 12.

from actual is substantial, as can be seen, for example, in Figure 12. In 1990, the prediction error is 50%, in 1987 it is 40%, etc. The average absolute percentage error is large by standards that one might impose to make precision judgments on major policy changes.

The above relatively imprecise prediction equations are used to make further disaggregated predictions in the second stage in Bloc Two estimation. This step also increases forecast error. For low valued salmon, errors associated with the aggregated equations are essentially reproduced for the area and species specific runs. For example, Figures 30 and 31 show predicted prices for Prince William Sound and Cook Inlet Pink fisheries. Errors in 1990 are 50%, errors for 1987 are close to 50%, etc. Although Theil statistics suggest that some of this is associated with random disturbances, a significant component appears also associated with systematic model error. In any case, the bottom line is that these are not precision estimates and one would expect a relatively large confidence interval around any predictions of revenues associated with these models. Again, it is also the case that forecasting exogenous variables introduces even more error in the predictions of future price levels.

III. Methodological Considerations--Cost Model

The cost modeling part of this study also represents a

formidable effort and an imaginative application of both economic theory and econometric methods. There are two different but complementary studies of behavior presented. The first attempts to use individual participation data to estimate entry and exit equations. This is a novel application of methods being used in valuation analysis and the intent is to determine what factors are associated with the participation of individual fishermen in fishing activities. The researcher argues that understanding individual entry/exit decisions would help to estimate costs of fishing, particularly apparent opportunity costs. This part of the study was not successful and the researcher then moved on to a second approach.

The second approach to estimating costs uses aggregate participation data, basically numbers of fishermen fishing on various days in various license groups during the season and total revenues earned during those days. The estimates obtained in this exercise are used to generate cost functions for the fisheries examined. These cost functions (or the intercept and slope coefficients associated with them) are used finally to generate estimates of producer surplus from fishing.

A. Theory

The conceptual foundation of the cost study is based on the notion that fishermen can be arrayed from low to high cost. At each point in time (each day), it is assumed that fishermen enter

the fishery until the last one is just earning an amount equal to his costs. It is not precisely clear what costs these are although it is suggested that they include real variable costs of fishing, some opportunity costs associated with labor and capital, etc. Those fishermen who have lowest costs are said to be earning "skill rents". These skill rents are what are measured and used synonymously with producers' surplus.

There is a potential methodological problem with this conceptualization that I find worrisome. Normally when we examine resources policy, we measure the success or viability of that policy in terms of surplus, but it is surplus or rent that would accrue to the resource that is important. It is general practice to assume that with an acceptable income distribution, mobile factors, competitiveness, etc., that it is desirable to attempt to increase or optimize resource rents when considering various policies. It is not clear whether the analogy carries over when, as is assumed here, the fixed factor is labor skill rather than the resource.

The cost study is unclear about exactly how these "skill rents" reflect costs and what the nature of the costs are for use in a cost/benefit study. Some questions which arise include: should public policy be trying to maximize skill rents? Are these skill rents reflecting fishery-specific skills, or skills that would earn high wages outside of the fishery? If they are not fishery specific, then they are appropriately part of opportunity costs. If they are fishery specific and just a function of random

events determining fisheries participation, then why should we be worried about them? Is the distribution of skills associated with a process whereby resources are drawn in to their highest valued uses, or are they simply accidents of history? Put another way, if all of the high-skilled Norwegian fishermen from Seattle died tomorrow and skill rents fell in half, what would we conclude about policies affecting the enhancement program, or any other fisheries policies? Another set of issues concerns the time frame. Since these are daily decisions that are being modeled, what time frame is represented by these estimated costs (ie. are they very short run variable costs, or medium run including some quasi-fixed costs, or long run including normal returns for fixed inputs)? If the time frame is the daily decision, where are resource rents in all of this? Are they assumed zero? Are they external to these calculations and, if so, should they be added to these "producer surplus" estimates?

The methodological problem that motivates the researchers' approach here is that the pure open access paradigm doesn't leave us with any room to conduct welfare analysis if we adopt it, since we must conclude that resource rents are zero both before and after a policy change. There are several ways out of this problem and the researchers choose one, that of assuming that skills are in fixed supply, surplus accrues to them, and (apparently) all resource rents (and other quasi-rents) are driven to zero. I'm not convinced that this is sensible, nor am I convinced that it is the surplus that we ought to be focusing on to evaluate fisheries

policy. Another possibility is to assume that regulations bind up input choices and hence not all rents are dissipated. There is strong evidence for this is in limited entry license prices which are positive. License prices are positive, not because of skills of labor, but because it is valuable to retain the opportunity to use the resource itself. These are the rents that economists normally use as a measure of gains and losses from policy changes and it is unclear how, if at all, the measures from this study are related.

B. Estimation

The basic method followed in the cost study is as follows. First, data in each of several regions were sorted by day and then by gear type. For each day, the total number of fishermen using each gear type participating was computed as well as total revenues in the statistical region.⁴ Next, regressions were run, regressing total number participating against total revenues. This gave a slope and intercept. These were then used to compute an implied cost function, which was further used to calculate an area denoted as surplus.

A first issue with this method is that there is a potential

⁴The cost study write up is unclear about the level of spatial aggregation assumed. In particular it is not clearly explained whether data points are for particular statistical subregions for any particular day, or for each opening which may span several subregions during any day, or any other aggregation.

identification problem. For this method to yield cost parameters, it must be the case that the average revenue curve in each fishery is independent of participation. Thus congestion externalities must be assumed away, for example. The researcher, while recognizing this problem, basically asserts its truth in order to justify and interpret results. There is reference to some regressions and scatter plots using average revenues but these are not included in the final report and hence it is difficult to assess whether the identification problem is relevant. One would expect that in some fisheries congestion is surely a factor and I do not believe that average revenues are independent of participation.

A second and related problem has to do with the presumed exogeneity of total revenues. The principle statistical results are based on the regressions of effort against total revenues. These regressions produce an excellent fit, with low variance estimates of the elasticities. It is not clear to me, however, that the interpretation of the causality is sensible. The researcher assumes that total revenues are exogenous, "as an alternative way of dealing with" (the simultaneous equation bias possible if average revenues are downward sloping).⁵ The reasons given are somewhat garbled and not convincing. First of all, if average revenues are exogenous, then total revenues will be endogenous by definition. Second, there is no argument that I can think of to support the idea that daily total revenues are

⁵Boyce, pg. 15.

predetermined (although one could argue that seasonal revenues are, via the season length decision on the part of managers aiming for a targeted escapement). In contrast, it is not difficult to believe that effort determines total revenues, which is an alternative way of viewing the regressions. But if this is the case, then there is an econometric problem associated with the estimated beta coefficients. In particular, OLS estimates will be biased and inconsistent.

A third issue is associated with the role of alternative fishing location opportunity costs in the estimation system. Although as discussed above it is not perfectly clear what costs are supposedly captured in the cost curve being estimated here, the inference is that it is a variable production cost curve, possibly also incorporating very short run opportunity costs of labor and capital. Importantly, however, in fisheries where there are multiple simultaneous openings, the chief opportunity cost which must be dominating daily decisions is what the boat might make in a neighboring opening in the same statistical region. Thus the costs determining the last fisherman's participation decision in any one opening surely include an estimate of what that boat might be making elsewhere on that given day. But if this is the case, the estimations are not really providing an estimate of variable direct production costs. In particular, the intercept terms in the cost equations will be biased upwards in the sense that they actually reflect estimates of opportunity costs in addition to production costs. This in turn means that producer surplus

estimates are biased downwards.⁶

C. Assessment

While I am impressed by the effort and imagination that is embedded in the cost study, I am skeptical of both the basic concepts and the numbers generated. It is never made clear exactly how skill rents and their obverse "marginal costs" are related to the costs of real resources that we ought to be measuring for a cost benefit study. Moreover, we are asked to accept, in the end, that a simple regression of participation against revenues yields enough information in a slope and intercept parameter to completely estimate all appropriate costs, real and opportunity, associated with each fishery examined.

While I remain open-minded about the potential of this parsimonious approach, I think that a better approach (or at least auxilliary approach) would be to use an accounting or "budget

⁶This is related to another general conceptual and empirical issue. If it is production costs excluding opportunity costs associated with other next best location alternatives that are being measured, then one must worry about whether the equilibrium condition is being met. That is, the model assumes that entry proceeds until marginal costs equal average revenues. But this depends upon the level of capacity vis a vis multiple fishing opportunities. There surely must be some days where there is unsufficient capacity over a whole fishing region to generate this equality in every opening. If opportunity costs are included in the estimated cost equation, then it is conceptually consistent to hypothesize that the equality is met by processes involving both a movement along the cost curve and a shifting of the cost curve reflecting opportunity costs. The paper describing cost function estimation does not satisfactorily address this point, but it appears that the first assumption above is implicit in the estimations.

generator" approach, whereby actual outlays on costs are measured, opportunity costs of labor are calculated, and these are subtracted from revenues. Other research has shown that variable costs are a low proportion of revenues. I suspect that labor opportunity costs are likewise low and hence rents or producer surplus is a higher fraction of revenues than estimated here.

IV. Policy Simulations

The final step in this study is the simulation of policy scenarios, using the market and cost models discussed above. This too represents a formidable undertaking and a relatively large-scale programming task. A range of possible scenarios were examined, including: i) the status quo of current hatchery production (case 1); ii) planned future production levels (case 2); iii) ten scenarios examining 15 percent increases and decreases in programs affecting particular species alone (cases 7-11); iv) two examining 15 percent increases and decreases of all species (cases 4a/4b); and v) three extreme scenarios involving elimination of pink, sockeye, and all hatchery production (cases 5,6,3).

Several changes in the original work plan were necessary following problems with first attempts at policy simulation. One of these was a decision to eliminate the Atlantic Salmon section of Bloc One and freeze values at their 1991 levels. The reason given is that "time constraints were too severe to get this section to simulate after initial problems were encountered. Consequently,

future prices were set equal to the 1991 third quarter price of Norwegian salmon in the EEC and North America."⁷ This is indicative of some kind of model instability, although it is unclear what the source is. My suspicion is that the estimated supply elasticities are too high. Obviously, the entire simulation output depends upon what values are assumed for those previously endogenous variables that are made exogenous. It is not clear what impact the choices made here have on results.⁸ A second modification is an effective truncation of the forecast period. The original work plan called for a 30 year simulation but the simulation model apparently begins to produce anomolous results after fifteen years or so. Again, this is indicative of some instability, which in turn suggests some misspecification. The model was thus run over a fifteen year horizon and the second fifteen years was simulated using the average of the last five years of the model's simulation outputs (i.e. years 2001-2006). A third problem surfaced during attempts to examine the "extreme" scenarios involving elimination of whole programs (Cases 3,5,6). Because the hatchery program has played a substantive role in Alaska salmon production over the past ten years, these elimination alternatives clearly place the model predictions outside of the range used to estimate the fundamental relationships. The results from these simulations appear to be too unreliable to even consider discussing in the text of the market

⁷Herrmann and Greenberg, pg 120.

⁸I also worry about using a 12% growth rate for other aquaculture (see page 113). This implies a doubling time of six years!

modelling results.⁹ Finally, a fourth problem is associated with the decision to aggregate pink/chum and chinook/coho/sockeye and the resulting implicit imposition of perfect elasticities of substitution within each group. A consequence of this is that the researchers must make ad hoc assumptions about the nature of the real substitution relationships within groups, in the absence of any actual empirical evidence. For example, if a pink salmon production reduction is simulated, the result will be a predicted increase in low-valued salmon prices, which, absent further assumptions, means a proportional increase in chum salmon prices. Thus the issue is how much interaction to assume between the two markets. The researchers bracket the possibilities by reporting runs that assume either: i) no interaction between pink and chum markets, or ii) full interaction as implied by the grouping of the two species, or iii) an "expected" interaction, assuming 25% of the full interaction results. This ad hoc procedure is at least feasible (leaving aside whether it is sensible) when one species within a group is being examined in isolation in the policy scenario but not when more than one are examined simultaneously (cases 2-4). With all of these modifications, the final results are assembled by, first, running simulations that generate harvest,

⁹This appears to be the opinion of the study authors who state (in Boyce et.al., pg. 17) "All of our estimation of demand and fishing cost parameters occurred with hatchery and production figures that were considerably higher than the level considered in cases 3,5 and 6. . . .it is particularly true for cases 3 and 5, which involve drastic reductions in pink salmon catches. Because of this, we are forced to extrapolate well beyond the data in our analysis. The result of the extreme extrapolation may (sic) be to predict prices which are above their historical levels."

price, and market changes via the market simulation model. These are then plugged into the cost model to generate producers' surplus estimates and ultimately, to compare with enhancement cost estimates.

The cost modelling begins with estimates provided by the above market simulation runs and then uses information from the statistical examination of "skill rents" to compute costs and producers' surplus. Simulated changes in catch are apportioned to each opening and then multiplied by predicted prices to get predicted average revenues on a daily basis. These are then combined with the cost study parameters to predict total participation per day and over the season, and the implied seasonal costs and skill rents.

There are two significant steps which influence final cost and surplus calculations. The first involves estimating the participation levels in each opening using estimated parameters for regions in which estimations were completed. The researcher apparently plugs average revenues back into an equation using estimated parameters without making a correction for the variance of the estimate. This has the impact of underpredicting effort because the true intercept of the cost equation is overestimated. In turn, this implies that all of the producers' surplus estimates are underestimated. A second critical step involves estimating costs and producers' surplus estimates for those regions for which the econometric studies were not done. The text documentation on this is not perfectly clear, stating that "we used the average

estimate of fishing costs for similar gear type fleets elsewhere in the state".¹⁰ It is not clear whether this means that a similar gear type was selected and the average cost per vessel day computed, or whether all similar gear types were combined and averaged, whether estimated surplus/revenue ratios were used, etc. As noted, this may be particularly significant in the large Bristol Bay drift gill net fleet, for which statistical estimates were not run. Final results are presented in several tables, showing total revenue changes, various cost changes, and summaries of net benefits.

VI. Conclusions and Presentation of Results--Assessment

A. Results and Policy Implications

Any study of a system as complex as that examined here is likely to contain components that are weaker than others. A difficulty with the write-up associated with the complete model is that, despite the attempts to bracket cases and run sensitivity analyses, it is unclear exactly how the weak and strong parts come

¹⁰Boyce et. al., pg 19. There appears to be a programming problem associated with this. For the Westward area, documentation suggests that average producer surplus from a representative set of gear groups in other regions was used. The programming problem is an unintentional repetition of a code statement which appear to weight the producer surplus unevenly toward the first region used in the average. For example in lines 281 and 284, statements $pst=ps$ and $ps=(pst+ps)/2$ are used twice, giving an average which is weighted more heavily to the first surrogate fleet.

together in the final summary conclusions. Nevertheless, as the above discussion of the various components suggests, there are some critical parts of this study that carry heavy weight in the final conclusions and which can be highlighted for this role. The most important sources of conclusions about future revenues are the low and high valued species ex-vessel price prediction equations. As discussed above, a close look at these fundamental equations suggests that forecasts made with the two equations should have large confidence intervals. One does not even need to look beyond the two equations to consider all of the complex interactions in the Bloc One and Bloc Two models; a 20-30% error in the fundamental equations will translate to roughly the same error in final conclusions about total revenues. When one notes that the conclusions regarding revenue changes for extreme policies (such as eliminating all programs) produce projected revenue changes over the status quo of only 4%, it is clear that the 4% point estimate is not significantly different from zero. My conclusion is thus that for all of the incremental policies examined (cases 4,7-11), the model projections of aggregate revenue increases and decreases are not distinguishable from no change. For the remaining extreme cases the model results are confounded enough by structural and estimation problems that results should not be considered.

Since the simulation was designed to examine net benefits in addition to gross revenues, the next question is what can be said of policy conclusions regarding these measures. Again, limitations of the simulation model effectively preclude the possibility of

examining elimination of the entire program, or of the pink and sockeye programs. With respect the remaining incremental scenarios, the differences between projected producers' surplus estimates are smaller in absolute magnitude than differences in revenues, as we would expect from the fact that producers' surplus estimates average 30% of total revenues. This small proportion in aggregate totals is influenced by the very small assumed percentage of surplus/revenues in the State's most valuable Bristol Bay fishery that is transferred ad hoc from other regions' drift gill net estimations. The conclusion again is similar to above, namely that the model projections of producers' surplus and net benefits for incremental changes in programs considered are not significantly different from zero.

There are thus two overall conclusions that can be drawn from this large and complicated modeling effort. The first is that the system is extremely complex and that serious data limitations prevent precise estimates of aggregate gross and net benefit measures to the State. The short time series on prices and quantities traded, coupled with evidence of structural change in the market, means that prices and revenues can only be estimated with rather wide confidence intervals. Moreover, other data limitations involving aggregation into high and low valued salmon restrict the kinds of policies that can be examined to those which are single species and incremental. On these, the evidence presented here is firmly inconclusive; none of the policies simulated can be confidently expected to increase or decrease revenues beyond the

status quo case. The second conclusion is that even less can be said about more complicated policies involving multiple species changes and/or extreme policies such as program elimination. There are clearly problems with all of these which go beyond the ability of the models to assess.

What then can be gleaned from this intensive effort? I believe that there are several important outputs from the overall effort. First, the State has never developed a comprehensive structure like this which might be used for policy analysis. At the very least, this effort has indicated where the major elements of uncertainty are and where additional effort might be concentrated. In addition, the model is the only analytical tool which gives order of magnitude estimates about different directed and broad brush policies in different regions and across Alaska as a whole. While I believe that caution should be exercised about taking the estimations too literally, they have illuminated some important things about regional distributional impacts of various programs, connections between various species' programs, distribution by gear type, etc. Thus, despite underlying problems with both the marketing and cost parts of the study, there has been a great deal learned here that should be useful in future planning.

B. Presentation of Results

A final issue that deserves discussion concerns the presentation of results. As discussed above, the effort embodied

in this study is clearly substantial. There are several complicated components and the task of drawing together results from all of the scenarios and discussing the model structure is not simple. I have had little trouble following what was done here, for the most part. The reports are clear, comprehensive and well written overall. Moreover, there are honest and complete discussions of modeling limitations and caveats to conclusions sprinkled liberally throughout all of the documents.

At the same time, I still find an almost schizophrenic mix of caution and bold statement of unsupported conclusions in the final summary document. For example, even though preceding pages in the summary document (and summary documents for supporting documents) provide extensive caveats regarding findings, the final page (Discussion of Findings) states conclusions like:

"According to our estimates, the most the enhancement program provides in additional benefits as measured by producers' surplus is \$147 million in present value over 30 years. The enhancement program, however, costs between \$300-500 million in present value over the same period."

This conclusion infers that the entire program not worth the costs. There is no caution, of course, that prices and revenues from the extreme value policy simulations are absolutely unreliable, that the surplus calculations are based on questionable methodological

and statistical assumptions, that the most important component of surplus changes, resource rents, are nowhere considered, or that the direction of bias of all of these problems is probably to understate net benefits of the program. Another final page example of excessively confident conclusions is:

"None of the alternatives involving increases in enhancement production results in increasing net benefits to any significant degree. On the other hand, all of the major reductions in the enhancement program (cases 3,5,6 and 4b) , as well as smaller reductions for pink salmon (case 7b) resulted in large increases in net benefits."

Again, there is no caution that cases 3,4,5 and 6 involve either situations well outside the range of data used to estimate equations and/or significant ad hoc assumptions which essentially preclude their usefulness. A final example, revealing this curious mix of caution and bold conclusions is:

"let us emphasize that our estimates of net benefit are based on parameters estimated from relatively short time periods and were extrapolated well beyond the time horizon for which accurate estimates may be obtained. There are also a number of exogenous factors, such as what is happening in the rest of the salmon suppliers' markets, that have changed far too rapidly over the last decade to allow precise estimates of

where the industry will be in one, two, or three decades. In spite of this, scaling back the enhancement program, and especially the pink salmon program, would increase net benefits."

The juxtaposition of these caveats and conclusions would be humorous were there not a danger that someone might simply extract the conclusion out of context. I find these kinds of summary statements troubling because, as discussed above there are enough difficulties with models and data and with the lack of confidence intervals of forecasts to be cautious rather than bold with conclusions. It is all more bothersome to see totally unsubstantiated statements like these in concluding remarks when the body of the individual reports are, by and large, quite honest about the limitations and cautious about findings.

VII. Using Models for Policy Analysis

As mentioned at the outset, it is important to recognize the limitations of statistical and simulation modeling, particularly when they have the potential to play a role in major policy decisions. It is perhaps paradoxical but unfortunately true that whenever policy decisions are obvious, one does not need a detailed policy analysis, but when decisions are "close" a policy analysis will generally not be precise enough to give an unambiguous answer. This is certainly the case in this exercise. In particular,

although this study reflects a considerable amount of effort and expertise, the estimates of both prices and costs are imprecise enough to render only one conclusion, and that is that one cannot be even reasonably certain how net benefits would change with any of the programatic changes contemplated.

In the demand model, despite the appearance of complexity and comprehensiveness based on a multi-equation structure and sophisticated estimation techniques, in the end the estimates are essentially shoehorned into their reported ranges with the Bayesian method, using unreported priors and an extremely small number of observations. It is arguable that much real information is contained in the off-season quarterly exvessel prices and hence the most important part of the model is fitted over less than 10 years of data. Any salmon market expert would attest that this decade has witnessed considerable transformation and change, particularly between aquacultured and natural fisheries, and hence it is unlikely that one could pin all of this down with so few observations. Even if these problems were ignored, the modeling results show large prediction errors which leave the impact on total revenues of any contemplated changes totally ambiguous. All of this is left out of final documents which address some of the uncertainty regarding cost and surplus estimates but which ignore those that are much more crucial to the conclusions, namely those on the demand side. With respect to cost and surplus estimates, in the end we are asked to believe that regression results yielding a slope and intercept across one year of partially complete cross

section data is all we need to confidently convert total revenue estimates into an appropriate measure of surplus that is suitable for policy analysis regarding large scale decisions and their ramifications over a 30 year time horizon.

Given the lack of precision in these results, it is clear that they should not play a disproportionate role in any current policy decisions over hatchery production plans. My assessment of the fallout over these studies suggests a misunderstanding on the part of both the contractors and State about the proper role of policy studies such as these. On the part of the researchers, the conclusions are seriously oversold. While the analysis is clever and ambitious, it in no way supports the kind of grandiose conclusions contained in the final report. This may be attributed to naivete or overzealousness, but the economics profession is also generally at fault for not stressing enough basic methodological issues such as regression fragility and the caveats that lie behind exclusive focus on efficiency analysis. The latter is particularly pervasive; many economists do what was done here, blindly reporting simple measures such as net benefits, downplaying weaknesses of the analysis and confidence bounds, and then boldly concluding that policy X ought to be undertaken.

There are few, if any, studies carried out even under the best of circumstances that might give economists justification for making policy pronouncements as strong as those contained in summary documents of this report. Economists and economics are useful for doing predictive analysis which deals with modeling

systems in order to describe what might happen under alternative scenarios. Responsible policy analysis provides as much detail as possible to policy makers, including distribution impacts, efficiency impacts, and nonquantifiable impacts in a manner that is neutral. In addition, although perhaps unsettling for those striving for the precision evident in the hard sciences, it is important to point out weaknesses and gray areas in order to judge how much confidence can be placed in impacts forecasts. In the end, for most difficult cases, policy makers rather than economists ought to be making the tradeoffs and dealing with the uncertainties of knowledge that are inevitable in any close call.

It strikes me in reviewing the misunderstandings generated from these studies, that similar future problems could be avoided if the State followed certain different procedures. First, it should be recognized that studies of this type cannot be used to answer close and contentious calls regarding issues of distribution and allocation. It is irresponsible to turn these essentially political issues over to economists to look for easy and ultimate decisions. Instead, policy analysis and modeling ought to be looked at as one of several tools to help organize data, to help clarify the tradeoffs involved, and to identify the major areas of uncertainty and the role that these play in making a final decision. In this way economics becomes an input and guide for critical analysis rather than an arbitrator in the decision process. Second, care needs to be taken in managing the results from these types of studies so that premature and biased

information is not used politically by various publics interested in pushing their positions. Given the complexity of these issues and the normal ambiguity of any given study, it is likely that preliminary and inconclusive results (the norm) can be exploited in a manner intended to influence policy. This case certainly shows evidence of this, although there are some institutional peculiarities which have exacerbated the problem. In particular, because the study was underfunded by the State and used additional funding from UA and Sea Grant, the simple advisory objectives to the State were compromised somewhat. What the State of Alaska wanted was a tool to use in analyzing current and future policy issues. What both Sea Grant and UA wanted was to display to the public that they are putting research dollars to good use. This type of public relations objective often means publishing provocative results to grab attention, ignoring the tentative and imprecise nature of the conclusions, etc. Third, understanding complicated systems like these is a never ending task. The most important output that has emerged from this study is that it has pointed to where the critical uncertainties are. For example, although everyone would agree that more pink salmon will depress prices, the critical question is what is the elasticity of exvessel demand for salmon? This study has come to one conclusion, using a very limited amount of data and a technique which effectively allows the opinion of the researcher as much or more weight than objective data to come to an answer. No intelligent researcher would stake much on these specific conclusions however, and further

analysis ought to be conducted to help pin down this critical piece of information. Similarly, with the cost study, the results point more to what we don't know than what we do know with uncertainty. For example, this study raises important questions like: what surplus ought to be measured as an efficiency measure in fisheries? What sort of equilibrating process is in force in fisheries on a daily basis? Weekly? Long run? How should we organize and categorize costs in assessing fishing net benefits? Do behavior and costs differ from year to year? All of these are important both conceptually and quantitatively and, while the current study attempts to answer some of these, the issues are certainly not resolved with precision. The next step for the State ought to be to assess alternative modeling, estimation, and data sources and continue a long term research plan to help shed more light on these unanswered issues.