A model for the world: 80 years of model development and application at the International Pacific Halibut Commission

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Abstract

This is a short history of modeling work done by the staff of the International Pacific Halibut Commission, from the pioneering work of Thompson and Bell through the present.

Introduction

The Pacific halibut (*Hippoglossus stenolepis*) is distributed around the North Pacific from northern California to Hokkaido and into the Bering Sea. In North America it was the object of important aboriginal fisheries before European settlement in the latter 19th century, and when the transcontinental railroads reached Seattle and Vancouver in the 1880s it became the object of a commercial longline fishery (Bell 1981). In the classic sequence, fishing effort and catches increased rapidly in the early years of the 20th century, but catch per unit of effort (CPUE) declined steeply. Total landings peaked in 1914 and declined steadily thereafter despite a continuing increase in fishing effort (Thompson and Bell 1934).

The industry in Canada and the United States called for action by the governments to conserve the stocks, and after a couple of false starts the governments signed the Halibut Convention of 1923, which established a commission with a scientific staff. Its original name was the International Fisheries Commission. There was no danger of its being confused with any other international fisheries commission because there was no other. (It was renamed the International Pacific Halibut Commission in 1953.)

The first task of the Commission, as stated in the 1923 Convention, was to conduct a "thorough investigation" of the halibut stocks and make recommendations to the governments about regulation of the fishery. For that purpose the Commission hired William F. Thompson to recruit a staff and direct the studies. He was well qualified for the post. A student and collaborator of David Starr Jordan at Stanford, he was a proven scientist and administrator who among other things had done a study of the halibut fishery for the Province of British Columbia in 1914-15 (Bell 1981). According to office legend he was utterly humorless and known by the nickname "Woofy" although probably not addressed that way.

In the space of only a few years, Thompson set up offices on the campus of the University of Washington in Seattle, initiated a large and varied program of field studies on halibut life history, and compiled a large set of data on fishing effort and catches. On his advice, the Commission in 1928 recommended a number of regulatory measures to the governments, including catch reporting and catch limits by area. In the Convention of 1930 the governments gave the Commission authority to regulate the fishery in that fashion, and catch limits were first set in 1932, at a level intended to rebuild the stocks.

Over the next decade, the catch limits served to limit fishing effort and the stocks did indeed rebuild. Catch per effort increased substantially and total landings also recovered to some extent. It was this achievement that made the Pacific halibut fishery a model for the world, the textbook proof that excessive fishing effort could deplete a stock, and that sound management could restore and then maintain a healthy stock and a healthy industry.

Thompson's work is often cited as a triumph of management, and it was, but it was also a triumph of analysis, because the catch limits that he recommended were based on a novel analysis of the stock and the fishery. Although he did not use the term himself, he constructed one of the first population models in fisheries and used it to investigate the dynamics of the stock, the effect of fishing on the stock, and the effect of regulations on the fishery.

Since Thompson's time, the Commission has continued to collect very detailed data on the fishery and the catches, and the staff has often been at the forefront of analytical developments in fishery science. This paper recounts their work in the area of modeling, by which I mean developing a more or less complex mathematical description of some system and then either fitting the model to estimate parameter values of the system or running the model with various parameter values to investigate the behavior of the system. So it is not any kind of analysis, and I will not attempt to cover here any of the very good analytical work that the Commission staff has done in other areas (e.g., growth, fishing power, fishery oceanography).

The Classical Age: Thompson and Bell in the 1930s

The central question posed in Thompson and Bell's landmark 1934 paper was: what is the relationship between the level of fishing effort and yield (or, equivalently, CPUE). They observed at the outset that obtaining the maximum yield from the stock poses "two distinct problems: one that of maintaining the incoming young, the other that of making the best use of these after arrival at commercial size. It is apparent that the fishery has been carried on without regard to either, and it can be indicted both for wastefulness of the present supply and disregard of the future." They never presented a solution to the first problem, nor did they dismiss it. They explicitly rejected the "school of thought which holds that the number of eggs is always in excess of what is necessary to maintain the number of incoming young", and they remarked that reducing fishing mortality will increase the production of spawn, but that is as far as they got.

It was their work on the second problem that broke new ground. They determined that the rate of individual growth (in weight) was 21% per year and then computed the yield that would be obtained from a year class for 120 combinations of competing rates of natural mortality (5-40%) and fishing mortality (10-100%). Other writers, notably Russell (1931), had described in a general way how the yield from a year class must depend on the rates of growth, natural mortality, and fishing mortality. What distinguished Thompson and Bell's work was that they actually did the yield calculations for a large number of cases. Russell wrote an essay; Thompson and Bell built and ran a model.

The results (their Table 8 and Figure 9) indicated that for any given rate of natural mortality, the total yield would be about the same for a wide range of fishing mortality rates. They termed this the "normal yield" and it was the mainstay of their subsequent analysis of stock dynamics and eventual catch limit recommendations. One of the implications of normal yield was that "the catch per set of gear must be equal to the total yield divided by the number of sets of gear, whatever the competition between this gear, and in so far as the total is constant, the relationship is reciprocal. This reciprocal relationship is of the greatest practical importance, because it means that after a fishery has approached a constant total yield, the use of more gear can increase that total but slightly if at all at the expense of a great decrease in return per unit of effort or per set of a unit of gear. Such a decrease means a corresponding increase in costs, with an accompanying decrease in number of spawning adults."

A second implication was that taking more (or less) than the normal yield in a given year would result in a decrease (or increase) in the standing stock. In principle, therefore, it was possible to predict the trajectory of total landings and CPUE that would result from the actual trend in fishing effort that took place in the 1910s and 1920s. Thompson and Bell did just that and showed that the predictions were very close to the observed trajectories. They did not actually fit the model in the modern sense, but by calculating the predicted values for three sets of natural and fishing mortality rates they showed that the behavior of the model matched the behavior of the stock and fishery. The stock's recovery during the 1930s, when catch limits were set below the normal yield, provided further confirmation.

Thompson would later restate the theory (Thompson 1937) and update the prediction exercise (Thompson 1950, 1952), but he never revised or extended the original analysis. If anything he seemed to simplify the analysis in his 1950 paper, treating the constant normal yield as a starting point rather than an intermediate result.

Thompson and Bell had their critics, most famously Martin Burkenroad who in the late 1940s questioned (among other things) whether the observed increase in abundance after 1930 could be accounted for by changes in fishing effort alone (Skud 1975). This dispute is referred to as the "Thompson-Burkenroad debate" and viewed as a clash between those who regarded fishing as the major cause of abundance changes and those who regarded environmental influences as more important. In fact, however, there was never any real debate between the two because Thompson did not deign to reply directly to Burkenroad, and their views were not that far apart anyway. Burkenroad (1952) did not dispute that fishing would reduce the abundance of a fish stock, and Thompson (1952) conceded that the increase in abundance after 1930 in all areas was greater than could be accounted for by the simple model. It now appears that the strength of the recovery in the 1930s was partly due to a density-dependent increase in growth rates and a climatic regime shift in the North Pacific around 1925 that increased average recruitment to the exploitable stock beginning around 1930 (Clark and Hare 2002).

From the perspective of 2002, the analysis of Thompson and Bell appears simplistic and unrealistic in that it requires the assumption of constant recruitment both for estimating potential yield (normal yield) and predicting stock behavior. On the other hand, modern simulations that account for complications like a stock-recruitment relationship, random variability in recruitment, and selectivity also show that the average yield of the halibut stock is about the same over a wide range of fishing mortality rates (Sullivan et al. 1999), so the mainstay of Thompson and Bell's theory was quite solid even though the analysis was highly simplified.

What is most impressive, again from the perspective of 2002, is the diligence of the early Commission staff in assembling a unique set of fishery data, and the originality of Thompson and Bell in modeling those data. They provided a successful example of careful data collection and detailed analysis which others would follow and improve upon. They might not have followed the most elegant route, but they were the first to climb the mountain and see the big view.

The Dark Ages: World War II and the 1950s

After 1937 Thompson turned his attention to salmon as the first director of the International Pacific Salmon Fisheries Commission. In 1939 Canada went to war and the U.S. soon followed. The Halibut Commission staff was reduced to a skeleton during the war and was rebuilt only slowly in the late 1940s and 1950s. Management of the fishery proceeded successfully according to Thompson and Bell's prescription, but data collection suffered, especially in the 1940s, and there was no new analysis (or virtually any other published research) between 1937 and 1960.

The Renaissance: Chapman, Paulik, and Southward in the 1960s

Enter Douglas G. Chapman, a student of Jerzy Neyman at Berkeley after the war and a professor of statistics and later fisheries at the University of Washington from the 1950s through the 1980s. He took an interest in fisheries early in his career, publishing seminal papers on mark-recapture experiments and survival estimation. In the late 1950s and early 1960s he advised the IPHC staff on the application of what were then new scientific methods to the halibut fishery: yield per recruit analysis (IPHC Staff 1960) and estimation of maximum sustainable yield (MSY) by production modeling (Chapman et al. 1962). It was the MSY estimates in the 1962 paper that finally replaced Thompson and Bell's normal yield estimates as the basis for catch limits.

Just as important as Chapman's own work was his training of a large contingent of quantitative fishery scientists, including the author. In addition to being a great statistician, he was an almost saintly person, a renowned teacher, and an avid tennis player. On the tennis court he was not a particularly hard hitter but he was ambidextrous and often surprised his opponent with some completely unexpected shot. He regularly confounded players many years his junior, including the author.

One of Chapman's first students, Gerald J. Paulik, went on to become a fisheries professor himself and was an early champion of simulation modeling as a tool for investigating and evaluating fishery management strategies. This was in the mid-1960s when it first became possible to build and run realistic models on the top-end mainframe computers of the day, which had approximately the same power as the early desktop personal computers of the mid-1980s.

One of Paulik's first students was G. Morris Southward of the IPHC staff, who built and ran a large model of the halibut stock and fishery in order to compare three strategies for managing the fishery: an ad hoc rule based on recent trends in CPUE and juvenile abundance (mimicking the practice of the 1950s), a rule based on fitting the Schaefer model, and a rule based on yield per recruit analysis. The simulation model could be run in either a deterministic or stochastic mode (Southward 1968). This exercise was thoroughly modern. It showed that the estimates of the Schaefer model parameters were extremely unstable and that the ad hoc rule in fact performed quite well.

The Golden Age: Deriso, Quinn, and Parma in the 1980s

Southward left the staff for an academic career not long after earning his Ph.D. During the 1970s the staff stayed abreast of developments in fishery science, but there was little original modeling work. Myhre (1974) updated the yield per recruit analysis and on that basis recommended an increase in the commercial minimum size limit, which was adopted by the Commission. Hoag and McNaughton (1978) estimated historical abundance with the new technique of cohort analysis.

When Donald A. McCaughran became Director in 1978, one of his first steps was to hire two young quantitative scientists to revitalize the population assessment and management strategy. He chose well in recruiting Richard B. Deriso and Terrance J. Quinn II, both recent students of R. Ian Fletcher (among others) at the University of Washington. I recall Fletcher saying at the time that he expected Deriso and Quinn would "stir things up over at the Halibut Commission." They did that and more.

The decade of the 1970s was a time of rapid advances in mathematical modeling generally. Affordable computing power was increasing geometrically year by year, and a number of seminal papers on numerical model fitting algorithms were published, including the definitive papers on the quasi-Newton methods that have been the workhorses of modelers ever since. Young scientists like Deriso and Quinn who were early adopters of the new methods had the opportunity to apply them to a number of old problems, and they did so energetically and capably.

While still a graduate student Deriso had single-handedly derived the delay-difference model (Deriso 1980). At IPHC he and Quinn and Philip R. Neal developed and fitted a model of commercial catch at age and catch per effort data that they dubbed CAGEAN, short for catch-age analysis (Deriso et al. 1985, Quinn et al. 1985). This method proved to be a great improvement on cohort analysis as a method of estimating present stock abundance. There had been a few earlier papers on numerically fitted models of catch at age data, but the parameter estimates were generally very unstable if there was any variance in the catch at age estimates, which of course there always was. CAGEAN estimates were quite well behaved, first because the parameter set was reduced by adopting a separable model of fishing mortality, and second because auxiliary information—in this case commercial CPUE—was included in the fit. IPHC staff also distributed the model and the Levenberg-Marquardt minimization routine as a package (in FORTRAN) that by the standards of the day was quite easy to use. CAGEAN was used for the annual stock assessment from the mid-1980s through the mid-1990s, and the staff generalized the model in several respects (Deriso et al. 1989, Quinn et al. 1990).

Like the work of Thompson and Bell, CAGEAN raised the standard for all stock assessments, at least in North America. It was the first integrated, flexible, numerically fitted model to be thoroughly studied and tested and then put into routine use in managing a major fishery. Other agencies rose to the standard later in the 1980s and 1990s. Once again the ideas were not altogether new, but the careful analysis and practical application of the approach at IPHC served as a demonstration of what could and should be done routinely.

This period also saw a resumption of simulation modeling as a method of evaluating harvest strategies, which has continued to the present. The lead scientist in this work was Ana M. Parma, a student of Chapman and Deriso at the University of Washington in the 1980s who joined the staff after completing her degree. Work done in the late 1980s was especially concerned with the effect of apparently cyclical variation in recruitment (Parma 1990, Parma and Deriso 1990a, Parma 1991).

Although not playing a direct role in assessment and management as CAGEAN did, there were a number of other models developed by the staff during this period that contributed to understanding of the stock and the fishery, including work on the size composition of catches (Deriso and Parma 1987a, Parma and Deriso 1990b) and on the relationship between density and catch rates of anglers (Deriso and Parma 1987b).

Quinn left IPHC for an academic career in 1985. Deriso went to the Inter-American Tropical Tuna Commission (IATTC) in 1988. They recently published a treatise on fish population dynamics and models, dedicated to Fletcher (Quinn and Deriso 1999).

The Modern Age: Developments in the 1990s

Patrick J. Sullivan, a student of Peter Guttorp at the University of Washington, was hired as Deriso's replacement in 1988, and he and Ana Parma led the assessment and modeling work at IPHC through most of the 1990s. The author also joined the staff in 1988, in the position of biometrician. Steven R. Hare, a student of Robert Francis at the University of Washington, was hired in 1995 to do research in fishery oceanography but was also involved in modeling work on bycatch impacts.

The 1990s were an eventful time for the assessment. Beginning in the late 1980s, halibut growth rates in Alaska declined dramatically. As a result, age-specific commercial selectivities decreased. CAGEAN did not allow for that, and by the mid-1990s it was seriously underestimating abundance. In effect, it interpreted lower catch rates as an indication of lower recruitment, whereas the real cause was lower selectivities. One symptom of the problem was a strong retrospective pattern in the model fits. Normally one expects each year's fit to reproduce the historical trajectory estimated in the previous year's fit, but in the early 1990s the estimated trajectory was jumping upward every year. On the one hand the assessment showed that recruitment was poor and the stock was declining steeply; on the other the estimates of present abundance turned out to be the same year after year. Confidence in the assessment declined, both within the staff and outside.

Sullivan and Parma diagnosed the problem correctly and remedied it by making selectivity a function of length in a successor model developed in 1995 (Sullivan et al. 1999). It was at this time that the staff adopted AD Model Builder, a package developed by David Fournier that generates analytical derivatives for models written in C++ and then minimizes the objective function to estimate the model parameters. This powerful and robust package (along with ever-increasing computer speed) made it practical to fit a model with many more parameters. In the new model, for example, the commercial catchability and selectivity parameters were modeled as random walks (with a penalty on the first differences), while survey catchability and selectivity parameters were held constant.

When implemented in 1995 the new model naturally produced much higher estimates of abundance (and no strong retrospective pattern). Pat Sullivan had to explain this sea change to the Commission at a meeting in November, and he had to do it with little sleep and almost no preparation because work on the new assessment had gone right down to the wire. But when the time came he rose and in his usual calm and thoughtful manner gave one of the most lucid talks I have ever heard, which in an hour largely restored confidence in the assessment.

Catch limit recommendations did not increase as much as the abundance estimates because the new model also produced lower estimates of commercial selectivity, and a fresh harvest rate evaluation incorporating the higher estimates of recent recruitment (Sullivan et al. 1999) led to a lowering of the target harvest rate. Another consideration in lowering the harvest rate was the recent retrospective behavior of the assessment itself, which showed that systematic patterns in assessment errors had to be allowed for in choosing a harvest rate (Parma 1993). On the positive side, Walters and Parma (1996) found that the IPHC constant harvest rate strategy was quite robust against the climate changes that appeared to be affecting the stock.

A major source of uncertainty in all age-structured assessments is the natural mortality rate, which accounts for a large part of the estimated abundance when fishing mortality is low or moderate. A working value of 0.2 had always been used in the halibut assessment but it was very uncertain. A study of the combined effects of error in the natural mortality rate on both abundance estimates and harvest rate evaluations concluded that a lower rather than higher working estimate would be prudent (Clark 1999), so the working value was lowered to 0.15, which substantially reduced the abundance estimates and catch limit recommendations.

Another major modeling effort in the 1990s dealt with the effect on the Pacific halibut stock of juvenile halibut bycatch in other fisheries, and especially the possible interception in Alaska of large numbers of juveniles that would otherwise recruit to the Canadian fishery. The model required for this analysis was far more complex than the assessment model because it was necessary to model migration among areas and keep track of fish not just by age but also by sex and size. Little was known about juvenile migration so the study was really a sensitivity analysis of impacts across a range of assumed migration patterns. The results indicated that the recruitment losses resulting from juvenile bycatch occurred mostly in the area where the bycatch was taken (Clark and Hare 1998), which at least reduced the friction between Canada and the United States over this issue.

The Postmodern Age: A look ahead

Pat Sullivan left IPHC for an academic appointment in 1998. Ana Parma returned to Argentina in 2000 to resume her research there. The author and Steven Hare have carried on the annual assessment and harvest rate studies, along with continuing research on stock dynamics. At time of writing (end 2002) the assessment is once again showing a retrospective pattern similar to that of CAGEAN in the mid-1990s, and again because of an overly parsimonious parameterization of selectivity. It is quite likely that correcting this problem will result in another substantial increase in abundance estimates.

From the viewpoint of a fishery scientist, the development of models at IPHC over the last 20 years is a story of steady progress in methods of estimating abundance and setting a target harvest rate. From the viewpoint of a manager or vessel owner, however, it is a story of continual abrupt changes in abundance estimates and harvest policy occasioned by arcane tinkering with "the model." We have made progress, but at some cost in stability and credibility.

Ironically, our estimate of the long-term productivity of the stock has changed little since Thompson's time. We now recognize that decadal climate change has a strong influence on Pacific halibut recruitment, but the recruitment effect appears to be at least partly offset by density dependence in growth, so that production varies less than recruitment between climate regimes (Clark and Hare 2002). Thus over the years our model-based estimates of potential yield have been quite stable, while our model-based estimates of current abundance have not. This experience makes Thompson's "normal yield" approach look quite attractive again. To be sustainable, of course, the actual catch limit would have to be set somewhere below the maximum sustainable yield (MSY), as it was in Thompson's day, but that would be in keeping with the present-day precautionary approach.

References

Bell, F.H. 1981. The Pacific halibut. Alaska Northwest Publishing Co., Anchorage, Alaska.

Burkenroad, M. D. 1952. Theory and practice of marine fishery management. J. Cons. Explor. Mer 18:300-310.

Chapman, D.G., Myhre, R.H., and G.M. Southward. 1962. Utilization of Pacific halibut stocks: estimation of maximum sustainable yield, 1960. Int. Pac. Halibut Comm. Report 31.

Clark, W. G. 1999. Effects of an erroneous natural mortality rate on a simple age-structured stock assessment. Can. J. Fish. Aquat. Sci. 56:1721-1731.

Clark, W. G., and S. R. Hare. 1998. Accounting for bycatch in management of the Pacific halibut fishery. N. Am. J. Fish. Manage. 18:809-821.

Clark, W. G., and S. R. Hare. 2002. Effects of climate and stock size on recruitment and growth of Pacific halibut. N. Am. J. Fish. Manage. 22:852-862.

Deriso, R. B. 1980. Harvesting strategies and parameter estimation for an age-structured model. Can. J. Fish. Aquat. Sci. 37:268-282.

Deriso, R. B., and A. M. Parma. 1987a. Dynamics of age and size for a stochastic population model. Can. J. Fish. Aquat. Sci. 45:1054-1068.

Deriso, R. B., and A. M. Parma. 1987b. On the odds of catching fish with angling gear. T. Am. Fish. Soc. 116:244-256.

Deriso, R. B., Quinn, T. J. II, and P. R. Neal. 1985. Catch-age analysis with auxiliary information. Can. J. Fish. Aquat. Sci. 42:815-824.

Deriso, R. B., Neal, P. R., and T. J. Quinn II. 1989. Further aspects of catch-age analysis with auxiliary information, p. 127-135. *In* R. J. Beamish and G. A. MacFarlane [ed.] Effects of ocean variability on recruitment and an evaluation of parameters used in stock assessment models. Can. Spec. Publ. Fish. Aquat. Sci. 108.

Hoag, S. H., and R. J. McNaughton. 1978. Abundance and fishing mortality of Pacific halibut, cohort analysis. Int. Pac. Halibut Comm. Sci. Rep. 65.

IPHC Staff. 1960. Utilization of Pacific halibut stocks: yield per recruitment. Int. Pac. Halibut Comm. Report 28.

Myrhe, R. J. 1974. Minimum size and age and optimum age at entry of Pacific halibut. Int. Pac. Halibut Comm. Sci. Rep. 55.

Parma, A. M. 1990. Optimal harvesting of fish populations with non-stationary stock-recruitment relationships. Nat. Resour. Mod. 4:39-76.

Parma, A. M. 1991. Performance of alternative harvest rates. Int. Pac. Halibut Comm. Report of Commission Activities 1991: 181-194.

Parma, A. M. 1993. Retrospective catch-at-age analysis of Pacific halibut: implications on assessment of harvesting policies, p. 247-265. *In* G. Kruse, D. M. Eggers, R. J. Marasco, and T. J. Quinn [ed.] Proceedings of the international symposium on management strategies for exploited fish populations. University of Alaska Sea Grant College Program Report No. 93-02 (AK-SG-93-02).

Parma, A. M., and R. B. Deriso. 1990a. Experimental harvesting of cyclic stocks in the face of alternative recruitment hypotheses. Can. J. Fish. Aquat. Sci. 47:595-610.

Parma, A. M., and R. B. Deriso. 1990b. Dynamics of age and size composition in a population subject to size-selective mortality: effects of phenotypic variability in growth. Can J. Fish. Aquat. Sci. 47:274-289.

Quinn, T. J. II, and R. B. Deriso. 1999. Quantitative fish dynamics. Oxford University Press, New York.

Quinn, T. J. II, Deriso, R. B., and P. R. Neal. 1985. Methods of population assessment of Pacific halibut. Int. Pac. Halibut Comm. Sci. Rep. 72.

Quinn, T. J. II, Deriso, R. B., and P. R. Neal. 1990. Migratory catch-age analysis. Can. J. Fish. Aquat. Sci. 47:2315-2327.

Russell, F. S. 1931. Some theoretical considerations on the "overfishing" problem. J. Cons. Explor. Mer 6:3-27.

Skud, B. E. 1975. Revised estimates of halibut abundance and the Thompson-Burkenroad debate. Int. Pac. Halibut Comm. Sci. Rep. 56.

Sullivan, P. J., Parma, A. M., and W. G. Clark. 1999. The Pacific halibut stock assessment of 1997. Int. Pac. Halibut Comm. Sci. Rep. 79.

Southward, G. M. 1968. A simulation of management strategies in the Pacific halibut fishery. Int. Pac. Halibut Comm. Report 47.

Thompson, W.F. 1937. Theory of the effect of fishing on the stock of halibut. Int. Fisheries Comm. Report 12.

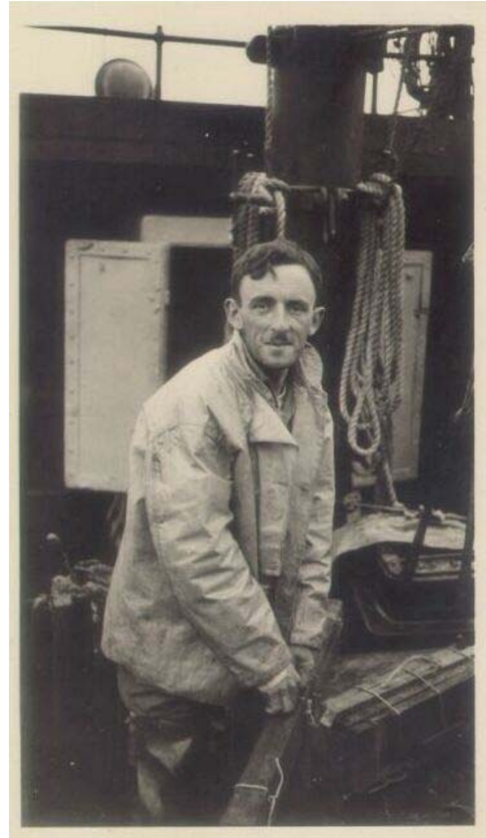
Thompson, W.F. 1950. The effect of fishing on stocks of halibut in the Pacific. University of Washington Press, Seattle.

Thompson, W. F. 1952. Condition of stocks of halibut in the Pacific. J. Cons. Explor. Mer 18:141-166.

Thompson, W.F., and F.H. Bell. 1934. Biological statistics of the Pacific halibut fishery. (2) Effect of changes in intensity upon total yield and yield per unit of gear. Int. Fisheries Comm. Report 8.

Walters, C. W., and A. M. Parma. 1996. Fixed exploitation rate strategies for coping with effects of climate change. Can. J. Fish. Aquat. Sci. 53: 148-158.

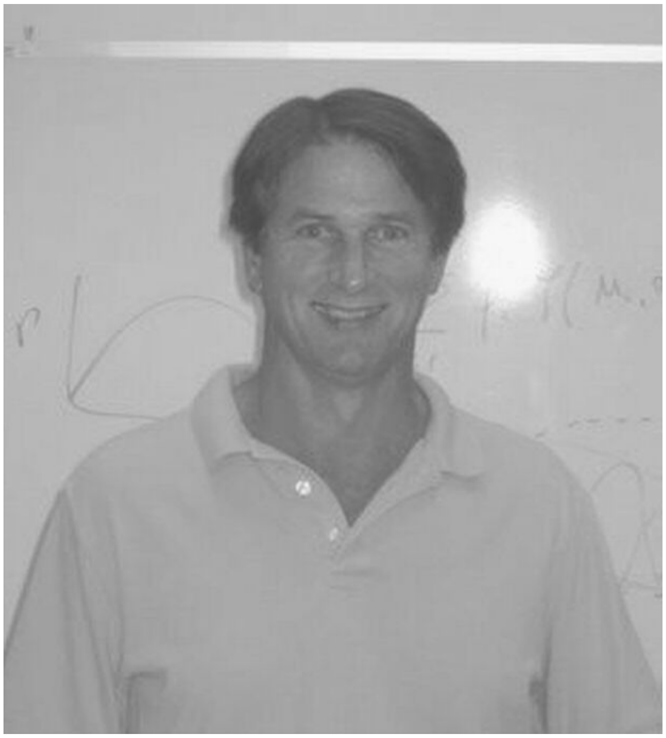
W. F. Thompson (left) at sea, 1925.



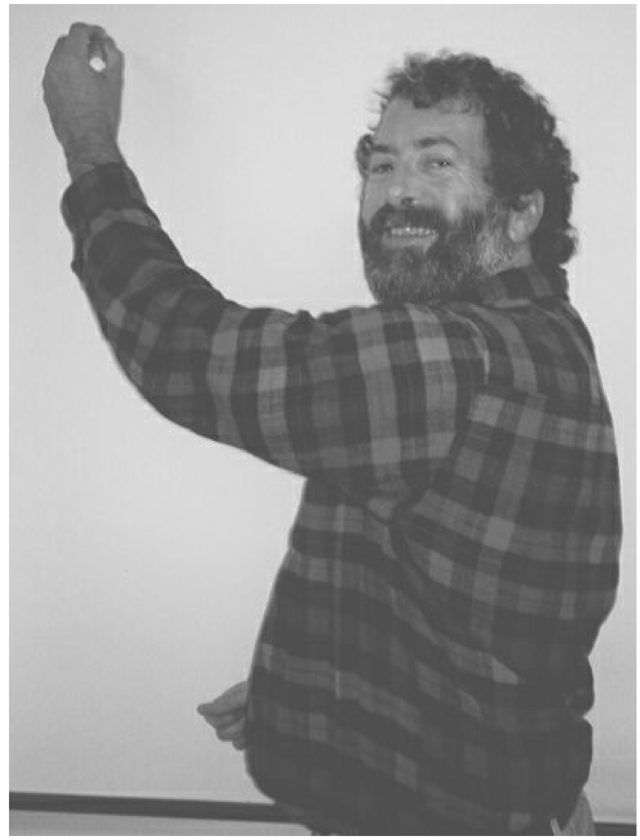
F. H. Bell at sea, 1925.

Doug Chapman ca. 1980.

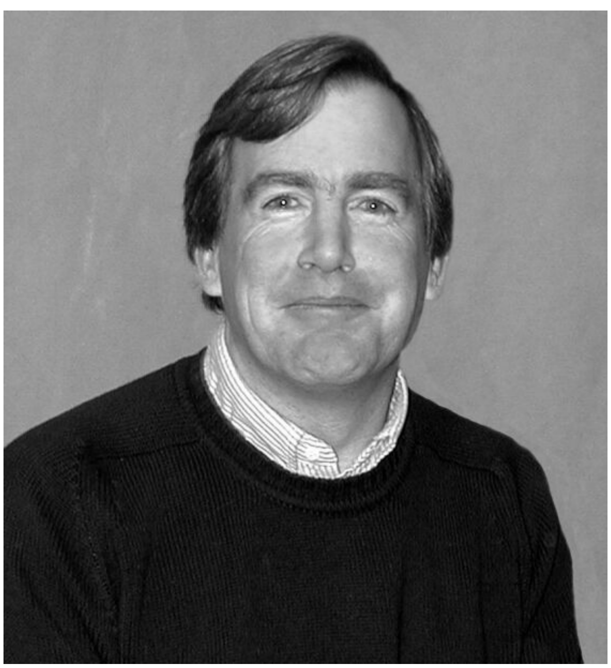
Morris Southward in New Mexico ca. 1990.



Rick Deriso at IATTC, 2003.



Terry Quinn teaching in Juneau ca. 2000.



Pat Sullivan at Cornell ca. 2000.

Ana Parma in Argentina, 2002.

Steven Hare (left) and Bill Clark at IPHC, 2003.