

## SOME IDEAS AND FRUSTRATIONS ABOUT FISHERY SCIENCE

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I am going to discuss a collection of ideas and some frustrations concerning fishery science.

I would like to sample a reservoir of ideas that I have collected over some years of association with fisheries science, and I intend to make a provocative presentation. Some of my contentions may be overstated, perhaps even unfairly, but I hope that they get the slings and arrows of outrageous fortune started—from which I will then stand aside. Nevertheless, I think the unfair ones and the overstated ones are not without substance, and I think that some of the points I bring up are downright serious indictments of fishery science.

I will begin with the assumption that there is a body of coherent knowledge that constitutes a field called fisheries science. I accept the existence of such a body of knowledge, but from then on I seriously question everything. As a matter of fact, all fields should be questioned and reexamined, in my way of thinking, and not questioned at the point where they now are but right down at their beginnings. The more I see the educational and research processes, the more I begin to liken them to some sort of austere anechoic halls, well plastered, where young people are led down along the asphalt tile corridors, some tiles inscribed with complex names or symbols, which must be remembered before proceeding. No one gets to tap on the wall early in these corridors to find if there is something hollow back of the plaster that needs examination. I think all fields suffer from this sort of "safe hall" where a leader can be followed to the final vista that he will eventually reveal. There is no chance to see if there are any vistas earlier. I like to rap on those walls and see if they genuinely show the limits of useful knowledge or whether they hide them, and whether they cover some windows and doors that have been closed long ago and which nobody has opened since.

If I might philosophize for a moment, this is the sort of thing to which I refer.

I live out in the country and I have a neighbor who periodically discs his field for some unknown reason—mainly to raise dust as far as I'm concerned. At any rate, I am interested to see how paths form across this newly disc'd field. They form very rapidly; our dogs going over to get handouts, the cats, the rabbits, the coyotes, the horses, and the people. There is an intergeneric agreement; all agree, after the first footprints, that this forming path is a very good place to walk and very rapidly they develop a beaten path. It is clear that the reason for this agreement is that something else had walked there and one is not as

likely to stumble or to fall into some sort of a trap as he would should he try to walk elsewhere. The more it is beaten and well trampled, the safer it is. I think that is a lot like the choices graduate students make and into which faculty lead them—well beaten paths.

At any rate, I would like to start with a serious questioning at the very beginning of these paths and, as I said, this is a sampling of a large reservoir. I have here a list of 14 questionable inherent assumptions in fishery science.

1. *Some steady state of biological, physical, or chemical conditions exist around which there is a normal distribution of perturbations.*

This certainly does not seem to be the case. The assumption is that there are some normal statistics to all kinds of conditions. Rather, there are probably a great number of possible regimes and abrupt discontinuities connecting them, flip-flops from one regime to another; multifarious regimes involving biology or climate, or oceanography, or migrations, temperature, or weather, or combinations of these. Probably one can illustrate this most succinctly by pointing out (which no one ever seems to point out) that the average temperature in Moscow is probably a negative 20°F, at least over the last 100,000 years, and its normal depth is under about 15,000 feet of ice. No treatment of present climatic statistics will even hint this, nor would a statistical study 50,000 years ago have predicted the present. This state is not a fluctuation around some sort of normal distribution, it is an episodic regime.

Ice ages are, of course, very conspicuous episodic regimes. On all time scales, it appears there are these locked-in persistent episodes in which it is either this way or that way or a third way or an *n*th way, and statistics have very little to do with the prediction except over very brief periods. Sardines, for example, are either here or not here. Rainfall follows these persistent trends, in periods of years during which it is high and others during which it is low. Thresholds may suddenly be exceeded, such as depletion of oxygen followed by mass death, whereas a speck more oxygen might have been above the threshold. One must consider these sorts of periods as quite different statistics. Recent changes in rainfall have fluctuated at one level in various parts of the world and then suddenly jumped up to some other level of fluctuation. The frequency of lightning storms in southern California, which occurred once every five years for the previous two decades, has increased about twenty-five times in the last decade. My main point

is that there are no simple statistics in the ordinary sense. There are internal, interactive episodes locked into persistence, and one is entirely fooled if one takes one of these short intervals of a decade or so and decides there is some sort of simple probability associated with it. I would like to return to this latter point because organisms must respond to more than just fluctuations around some optimum condition. Actually, many of their characteristics and fluctuations of populations must be related to these very large alternations of conditions.

Now in the very first step that must constitute fisheries science, the second cliché or inherent unexamined questionable hypothesis, or implicitly accepted assumption that I would like to examine is:

*2. Primary carbon fixation or oxygen evolution—the usual measurements of primary production—are valid measures of primary energy fixation.*

We assume that energy is well measured when we measure carbon uptake or oxygen evolution. Strangely, this implicit assumption has never been adequately checked. Bill Thomas and some others have made some tests of this—not under the extremes of conditions. It is not necessary for a plant always to take up carbon or to evolve oxygen to increase its energy resources. It can absorb photons elsewhere in the cycle of photosynthesis and add energy to the system through nonoxidative photophosphorylation. One thing that has always puzzled me when I consider the ocean is why the inhabitants of the very unproductive regimes are so energetic. One would think that they would be conservative, but the dolphin fish and the flying fish seem to be profligate with energy. It may be that there are energy rich and material poor environments where creatures are effective only when they expend a great deal of energy to capture what little material (and highly energetic material) that is available to them.

There are several examples of land plants upgrading energy in this way. For example, it has been shown that conifers in the north, during wintertime, are actually utilizing light—using it locally within the cells, not for photosynthesis because transport of material is not possible, but rather to increase their existing energy sources to make up metabolic loss and to make their materials more energetic. Thus, if one were to cover a winter conifer with a plastic blanket, and measure carbon and oxygen uptake and evolution, he will be quite wrong in estimating its energy uptake from light because the other routes have not been measured. This process has not been shown in the ocean, but it is difficult for me to conceive that plants in light-rich low-nutrient regions aren't capable of utilizing the luminous energy source by nonphotosynthetic processes, for these phytoplankters are even more sophisticated than other plant cells.

Another assumption is that

*3. Primary productivity is the principal organic input into most oceanic areas.*

I think that this may often be true. But it is clear that other inputs may often dominate, that input from primary productivity may be remote or delayed. These are the sorts of regimes that we see along the California coast, the downstream consumption of phytoplankton or zooplankton from energy elaborated well upstream; these are, of course, dominant influences in rivers and are examples of simple transport, where the primary production mainly becomes available at some distance, as in a grocery store. Other types and extents of transport and delays may also be important. There are apparently strong effects of a direct phytoplankton-to-detritus step in many parts of the sea, detritus which may be later remixed into the suspended particulate food. In Puget Sound, there is a secondary peak after the onset of winter storms from the remixing of dead phytoplankton on the bottom into the waters. We have never evaluated the direct phytoplankton-to-detrital step. A step resembling this seems to dominate the food chain of the Salton Sea. The most surprising results, I think, are John McGowan's recent results at the Equator. I have just glanced at his paper and was astonished to see that he finds the input of dissolved organic material into that region was about 500 g of carbon per m<sup>2</sup> per year. This is ten times local productivity and brings the total productivity, much of which would be unmeasured by ordinary methods, up to as high as that in the Peru Current. Perhaps this bears on the correlation of zooplankton biomass with nutrients, and the fact that that correlation is better than with productivity.

*4. Trophic food chains or webs exist with some substantial selectivity of food by particular species in some five or so rather well defined trophic levels.*

I have devoted some study or speculation to this matter, and what started me thinking (a number of you here have heard this before so I won't take much time) was the strange and most provocative results of Dave Young's thesis work of the uptake of cesium in the Salton Sea. Briefly, the Salton Sea has a more or less linear food chain, with food elaborated by phytoplankton taken up by zooplankton, but very little of the zooplankton is directly consumed. Rather, it becomes detritus, is fed on by worms, and then moves back up to the midwater food chain again. At any rate, in this almost linear food chain, from phytoplankton through zooplankton, worms, and up through several fishes to the top predator, corvina, the concentration of the element cesium increases something like a factor of three in each step, ending in the corvina with a concentration of about eighty times that of the phytoplankton. The concentration factors to corvina, through bairdiella, croaker, and sargo, each step involving an increase in concentration close to a factor of three, result in the

total increase in corvina. Nevertheless, in all these same several species of fish in the Gulf of California, the concentration of cesium over the phytoplankton is close to a factor of sixteen. One would have thought from the Salton Sea results that the concentration of cesium in each one of these steps was an index of their trophic position, and one might think, "Ah! I have a tool. I'll run and measure the cesium in the fish of the Gulf of California and elsewhere around the world and if the total concentration of cesium over that of the phytoplankton is 9 for this one, or 27 for that one, or 80 for another, I should discover where these creatures operate in the trophic chain." But one gets to the Gulf of California and the concentrations are all 16. So perhaps nothing like this happens in the Gulf of California or in the sea in general; all are eating out of the same trough. Perhaps oceanic food webs are so intermixed, with so much knitting and tating and crocheting, food and creatures moving from one place to another, gamete and detrital production, etc., that they are essentially homogenized. Trophic positions then become much simpler with only gross selections of food material being of significance, and with only herbivores, detrital feeders, predators, omnivores and a few others as meaningful trophic types. If so, it explains the factor of 16 and some other things that lead me to my next statement.

5. *The biomass of secondary consumers cannot exceed the biomass of herbivores in steady state.*

Another unexamined cliché, I believe, is that the biomass of secondary consumers cannot exceed the biomass of herbivores, and, correlatively, that the flux of food through the secondary consumers (that is, the predators, the detrital feeders, etc.) cannot exceed the flux of materials through the herbivores. This emerges from a food pyramid concept, and is the basis for believing that much larger fisheries can be supported by fishes low in the food web. This may be true in deep pelagic areas where detritus may be lost to the surface web. It is quite clear, however, that for a food chain in which everything is recycled a great number of times, where there are useful and recovered detritus and reproductive products produced in all steps, this limitation does not hold. In addition, to the degree that there is a direct phytoplankton-to-detrital step, any of these other forms can exceed the herbivorous step even further. I have just published on this subject in *Marine Biology*. It is a very simple concept. The total flux of material through herbivores over a chosen period of time (defined as the time required for the material to progress one trophic step in the web) and their biomass limit can be expressed merely by an input,  $M_o$ , multiplied by a coefficient that reflects the amount of food material that is converted into new living material. Equivalent expressions for detrital feeders and other secondary and tertiary forms can be derived by solving a doubly infinite series of terms,

expressing conversion ( $K_1$ ), irretrievable losses ( $K_2$ ), and retrievable losses ( $K_3$ ). This rather complicated way of going about things, however, reduces to very simple equations, such as:

$$M_m = M_1 K_1, \text{ biomass of herbivores}$$

$$M_d = M_o \frac{K_1 K_3}{K_2}, \text{ for detrital feeders}$$

$$\text{and } M_p = M_o \frac{(K_1)^2}{K_2}, \text{ for full predators.}$$

For coefficients well within the published ranges, consider:

$$K_1 = 0.4$$

$$K_3 = 0.3$$

$$K_2 = 0.3$$

The potential ratio of trophic forms in this noncompeting threesome is:

$$\frac{\text{full predators}}{\text{herbivores}} = \frac{K_1}{K_2} \text{ or } 4/3,$$

and that of:

$$\frac{\text{detrital feeders}}{\text{herbivores}} = \frac{K_3}{K_2} \text{ or } 1$$

Thus the sum of the biomasses of secondary consumers is  $2\frac{2}{3}$  times the biomass of the herbivore in steady state, for quite reasonable conversion coefficients.

Despite the simple nature of these equations, they may explain the persistent findings that there are larger biomasses of secondary consumers than there are of primary consumers, without invoking extremely rapid herbivore turnover rate or advection of herbivores or some other mystery.

An elaboration of this approach may allow us to envisage a way to quantify multispecies or "trophic level" interactions in fisheries, probably the single most important needed step in fisheries research and management.

6. *The only substantial aspect of gamete production is reproduction.*

I have been wondering about this for some time. Reproduction is a major metabolic loss from almost all organisms, and a loss of extremely high grade organic material. There are scars on scales, otoliths, and bones that mark the metabolic shock when gamete production or mating was taking place—severe growth discontinuities. In every species, if one can measure growth against age, one can see discontinuities of growth occurring at times of reproduction or even permanent changes at onset of maturity, and very severe changes, indeed. Even paedogenic larvae show such discontinuities. These are shocks to growth or even losses to somatic mass, and these are very large. Clearly, all this great production of reproductive products goes to primitive food. The survival

of gametes in terms of biomass is trivial—that is, the part of the initial egg material that is contained in juvenile tuna is infinitesimal. The reproductive products are consumed by filter feeders, by bacteria, by zooplankton, and in effect there here is a direct predatory step on the adult. One can interpret it no other way. Copepods do eat tuna! Mussels do eat yellowtail! There is undoubtedly a substantial or even dominate flow of material (and a larger flow of energy) downward in the food web through this mechanism, which must be a powerful part of the homogenizing process that I spoke of earlier. This mechanism is an aspect that we have the data to evaluate—Reuben Lasker's and other work—but we have not looked at it broadly in the ocean. Everything is sitting or swimming around throwing reproductive products freely into the ocean in such great quantities as to constitute a very powerful flux that is a predacious trophic step on the adult populations by primitive forms!

7. *Success of a recruitment is related to some optima biological and physical properties of the environment.*

The cliché here is that there is some relatively steady, physical, and biological optimum for a species and that the degree to which the conditions match these optima determines the success of recruitment. This is undoubtedly so in some culture experiments. I think that it has not been demonstrated in nature. Isn't it more likely that survival is always under suboptimal conditions—actual physical or chemical conditions—that suppress the competitors, or at least give the organism greater advantage over the competitors or predators, perhaps even at a life stage other than the adult one. I think this is seen very clearly in the terrestrial realm, where physical or chemical conditions are not optimum for most organisms but rather suboptimal and are ones in which the species has a relative advantage in respect to its predators, competitors, parasites, or pathogens.

I also think that we cannot even define optimal conditions of recruitment as related to any steady state, but rather fluctuating conditions may dominate the optima!

I was very much impressed by Mary Silver's defense on her research on salps, in which she demonstrated there was quite another kind of environmental interaction that could exist in the ocean—one resulting from time sequences rather than with conditions *per se*. Two species of salps seemed to be totally and exactly competing organisms enjoying the same water masses, the same conditions, and the same food, but they possessed an essential difference: one had a sexual resting stage and the other an asexual resting stage, and thus they responded differently to fluctuations in conditions. The one with an asexual resting stage could reproduce extremely rapidly in response to rapid onset or discovery of good conditions, whereas the species

with a sexual resting stage could respond only slowly but in far greater abundance and hence could dominate in slowly improving conditions. So it is the time rate of change of conditions under which it is determined which of these two species will dominate in any sequence. That is quite a different picture from sequences that are merely responses to some optimum condition. It is a picture that I think is not even very well recognized in the terrestrial realm except crudely, such as opportunistic populations following some episode such as a flood or a fire.

As you know, there are plants that have been waiting for just that sort of sudden transient event and take advantage of it. That is why you can buy a jar of fire-weed honey in the Northwest. This plant has jumped in following a fire after waiting for years in small enclaves such as where some fallen tree made space, and then opportunistically has taken over. I think that this is a much more common, continuous, and subtle mechanism of succession in the oceans. One can erect a theory of speciation and competition, defining a set of niches in time sequences of conditions.

I will go briefly into this. Garth Murphy has looked into some aspects of this matter, but I believe that he has not pursued it very far. Let's consider two exactly competing organisms, precisely competing in all ways, but with only two differences in life history. One becomes sexually mature at one time period, let's say a year, and has a longevity of 4 years; the other becomes sexually mature in 2 years and has a longevity of 8 years, not unlike the anchovy and sardine. One can easily see that with a straight probabilistic or episodic fluctuation of annual conditions (some 10 or so steps of good, intermediate, to bad years) precisely affecting both species in the same way, these two creatures will fluctuate together until some critical sequence is encountered, say 4 bad years. Now the two populations will change in their relative numbers according, not to the condition *per se*, but to the duration of these sequences. These two organisms then respond to all conditions precisely in the same way except when mediated through these critical differences in their life histories. Clearly, if there are 4 successive years that are bad for recruitment, the population of one is greatly reduced, but it requires 8 bad years in succession or some combination of that nature to equally reduce the second, but once bad conditions are over and there are some surviving remnants of the populations, the first species can respond to rapidly improving conditions and become dominant. It requires an unusual sequence to bring the second species to dominance once its population has been greatly reduced. So there can be fluctuations and, in a sense, environmental niches (perhaps even involved in speciation) that are not associated with conditions *per se*, but with the statistics or stochastics of time-rates of changes. I believe that models of this sort will be necessary to explain fluctuations and successions of creatures in the

oceans, and changes in their dominance. This seems particularly appropriate to the anchovy and sardine history. There probably is no such thing as a stable climax population.

8. *The nature of the relationships of recruitment to spawning stock in a single species is such that a specified fishing mortality can be accommodated, and that "maximum sustainable yield" or other yield estimates can be made.*

Without question, that statement constitutes the basis of management, and one of the principal efforts of fisheries science, and I think it is highly questionable. It is a mystery to me how we can look at fisheries as a single species model without examining its disabling defects and recognizing the necessity of developing multispecies models to emulate conditions. I would be hard put to accept as sane a farmer who would plant his crop without considering the number of weeds or employing a hoe or harrow to do something about them. I've been emphasizing the deficiency of single species models for a long time. I believe that it is the single most damaging cliché in fisheries science. The basic argument, of course, is that one is enabled to take some increment of the population because of the inverse relationship of year class recruitment and spawning stock, which putatively compensates for the fishing mortality. The plots of model are typical (Figure 1). You all know about this and we always hope we are operating on the starboard side which says, as the spawning stock is decreased by some increment, the year class recruitment increases by at least a compensating amount. Of course, it is not clear that we are free to take this increment, and I have always questioned whether that increment taken out really would otherwise have expended its life in vain or whether it would be fending off competitors, feeding predators, testing limits, widening the range, and/or doing other necessary things for the health and survival of the total population.

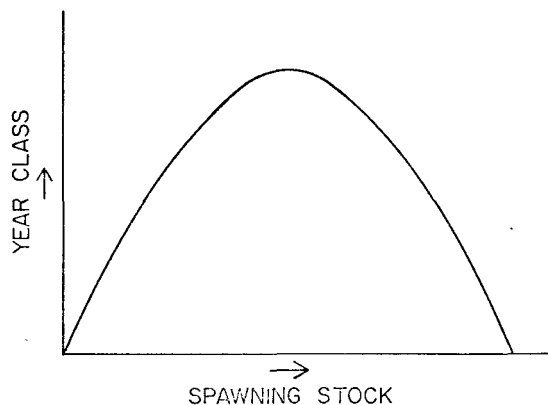


FIGURE 1. Typical model of year class recruitment and spawning stock relationship.

When we look at the data for the sardine during the years that it was here in abundance, points for the sardine fall very nicely in this sort of a relationship and appear to define the righthand branch of a classical curve (Figure 1). But later, when the sardine began to decrease, there was no appearance that it started to flip over on the left or dangerous side of the curve. Rather, it became a series of points seemingly defining parts of two smaller curves, in two steps. Surprisingly, when I took each one of the points on the sardine population and multiplied them by the appropriate ratio of the populations of sardines and anchovies, all of the points were brought into a simple line (Figure 2). Reconstructed, anchovies plus sardines fell on a curve, but not one species alone. I've never really understood why this wasn't clear *prima facie* evidence that a multispecies model is a necessity for understanding this, and that we must manage fisheries as multispecies or trophic level operations.

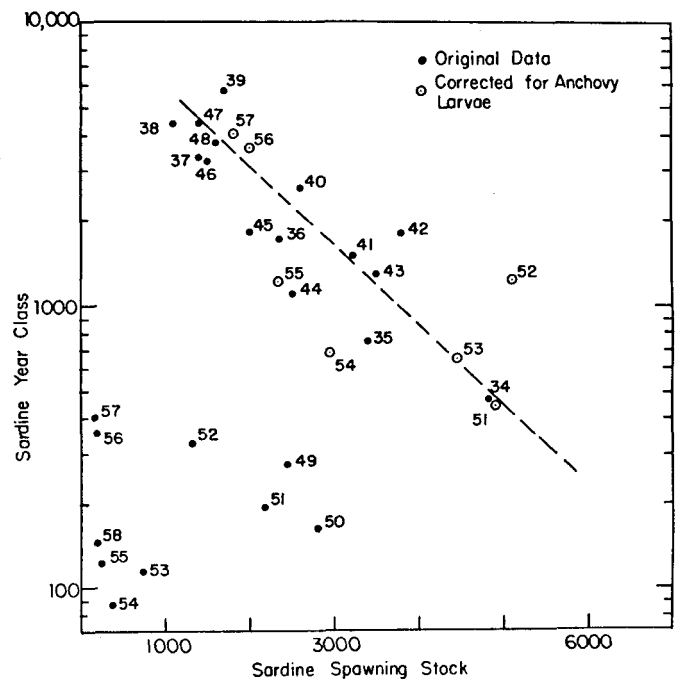


FIGURE 2. Sardine spawning stock and year class recruitment.

I am surprised how little we have considered the possibilities of a strong response of competitors when some population is fished. I believe that such responses are very real. For example, perhaps we stimulated the porpoise population by our heavy harvesting of yellowfin tuna and similar fish over the last several decades. It is conceivable that they are in symbiotic relationship, but they ostensibly are in direct competition, judging by behavior—porpoise feeding above and tuna feeding below on the same assemblage. If one looks at it this way, it is perhaps most inadvisable to spend our substance trying to understand how you get the yellowfin tuna out from

underneath this competitor without damaging it. If the yellowfin tuna could speak, I would think it might well say: "What do you stupid people think you are doing? We already are in an almost hopeless position with this profligate air breathing creature that takes all our food and now you are trying to figure out how you can take us from under them without hurting them. We'll be totally overwhelmed." Similar arguments could be made for the hake vs. silver salmon. Along this line, it may be that we want to be hauled kicking and screaming and agreeing to the Russians' taking all the damn hake they can possibly find, since they are clearly in close competition, at least with adults of silver salmon.

Last fall, while fishing in Barkley and Nootka Sounds on Vancouver Island, I was impressed by the organization of the salmon fishermen to avoid making sets where there are large numbers of dogfish. By radio, it is announced where salmon can be taken in the absence of these small sharks. At the same time, I find that trolling herring for salmon is no longer practiced, for any bait attracts dogfish. All of these small tactics are clearly effective in stimulating the dogfish population vis-à-vis the salmon. These tactics are no more than an exacerbation of every fisherman's efforts—from bent hook fishing to tuna purse seining—to conduct his methodology to maximize his catch of the preferred species, with an overall effect of maximizing the populations of undesirable species. Surely here is another example of maximization in the small constituting minimization in the large, and another example of the necessity for management at the multispecies level.

*9. The deep benthic populations are adjusted to a food source derived from a sparse rain of highly refractory terminal debris.*

Most of you have seen the photographs and motion pictures of the very active creatures of the deep ocean floor, clearly fed by windfalls of large pieces of higher class food. I didn't bring along the pictures of the great sharks in deep water off southern California, finally captured in three sequences of motion pictures. I consider them quite spectacular. The study of deep benthic populations has raised a number of mysteries—I'll mention one of them.

We find very many active fish attracted to the bait on the ocean floor under areas of very low surface productivity, and very few and inactive fish on the bottom attracted to our baits under areas of highest surface productivity, the first being the North Pacific gyre, the other being the Antarctic. One possible explanation of this is that in the very low productivity areas, the midwater population is so sparse that an occasional input of carcasses or fall of fragments completely overwhelms the capacity of the sparse midwater population, which is quite unable to cope with the fall and it continues to the bottom. However, in the regions of higher productivity, perhaps the midwater population has greater continuity and can per-

haps clean up material on the way down. Another possibility is that old or ill individuals of the large migrant species die when crossing the oceanic deserts. After all, land deserts are known for their vultures and jackals and skeletons! These situations are quite opposed to what one might have thought.

*10. The metal levels in marine organisms will reflect the levels of input and most of these levels will increase successively in higher trophic positions.*

With the interest in pollution, there has been a lot of talk about heavy metals and their uptake by marine organisms. One of the untested assumptions of this is that the metal levels in marine organisms will reflect the levels of input or concentrations and most of these will increase at higher trophic positions. I have already shown in a cesium example that they may not increase in the higher trophic positions. They might be all the same. Even more surprising is the Southern California Coastal Water Research Project finding that where benthic fishes are living on contaminated sediments, where some metals are 100 times what might be called normal concentration, of the 11 metals that they analyzed, all were lower in the livers of fishes living on highly contaminated sediments than they were in the same species at great distances. This is true except for perhaps one metal, iron, which is somewhat reduced in regions of high contamination and seems to be slightly increased in fish. This is a very surprising result. It is known that the fish were actually inhabiting these areas because their content of other contaminants such as DDT correlates very well with the levels in the sediments—so they are inhabiting those sediments in some sort of equilibrium. The metals, however, trend in precisely the opposed direction and with no relationship with what might be considered the trophic positions.

There are several possible explanations. For example, one that preserves the effect of contamination is that some unanalyzed metal may be concentrated in the fish. Then because of a general toxicity, the animal adjusts by generally excreting all metals in greater than normal quantities.

The explanation I suggest is that there is a great deal of food material in these regions. Most animals in nature are more or less starving to death. Perhaps the organisms in the regions of these contaminated sediments, because of the amount of organic material put in, are growing very rapidly, and the metals they do take up are essentially diluted by the new tissue. A relationship that has never been clearly pointed out is that, if one defines a concentration factor as the increase in some trace element in relationship to tissue in a single trophic step, the concentration factor cannot be greater than the reciprocal of the coefficient,  $K_1$ , that I defined earlier. That is because even if all the metal is absorbed, if the creature isn't losing any of it, it still has to be diluted by the material he is laying down in growth. So, the fast growing and

presumably efficiently growing organism will show a lower concentration than the slower growing organism, and this may be the reason for the lack of response to these metals. The metals are being diluted by growth. Victor Vidal is going to check this. There are some hot springs off Mexico that are putting forth elevated metal levels and obviously not making more food available unless they are killing local organisms. If the metals are presented along with no extra food, then the local organisms might show elevated metal levels.

*11. Plankton nets sample the population of fish larvae over some size range.*

Under this point, I get back to an old friend of mine. This is the plankton net sample of fish larvae over some size range. Some time ago, I proposed that plankton nets and larvae of anchovies and sardines were interrelated in such a way as to give one a larger and more important answer than just populations. They told you much more! They told you something about the growth rates, at least the rate at which the growth rates were changing in these larvae, and they told you something about mortality.

I proposed and put forth the data that the plankton net in regard to two species, the anchovy and sardine over a range of sizes, when hauled at night did in-

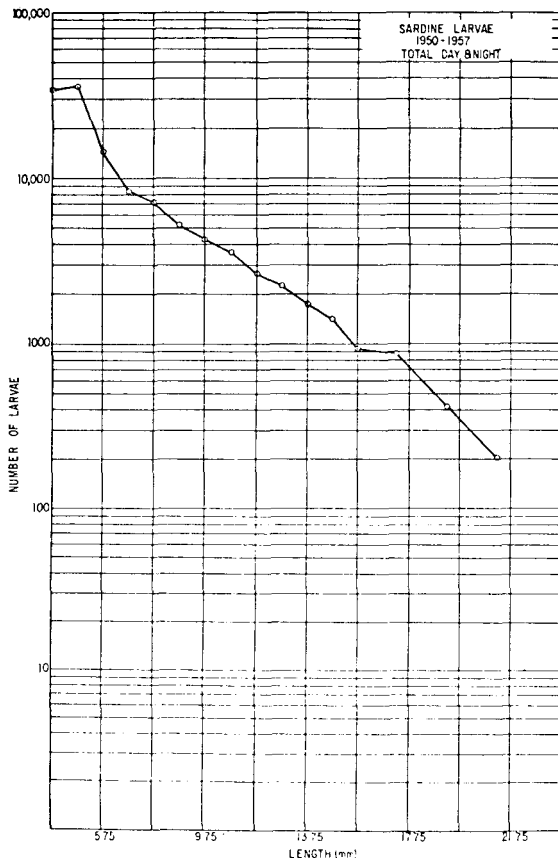


FIGURE 3. Size frequency for total day and night caught sardine larvae, 1950-1957.

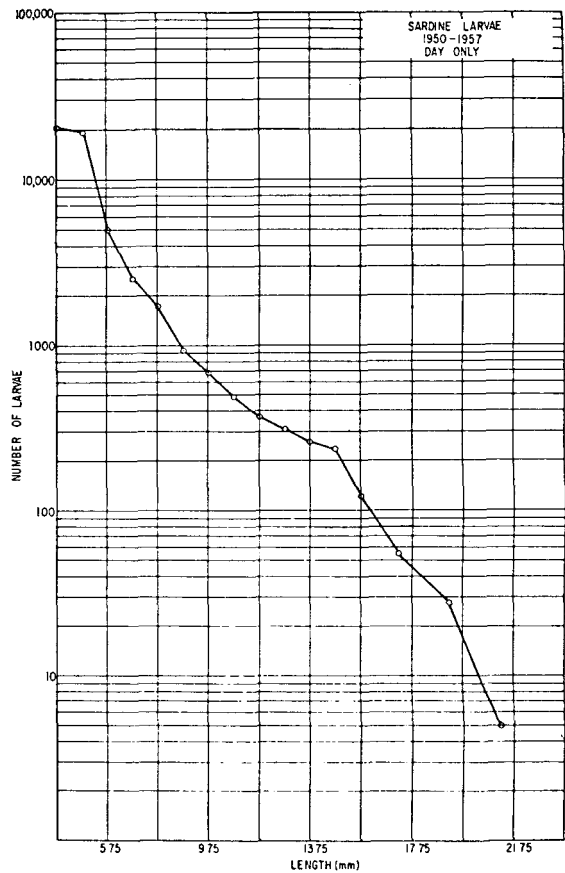


FIGURE 4. Size frequency for day caught sardine larvae, 1950-1957.

deed sample the population, but when hauled in the daytime, it sampled a portion of the population that was a measure of the portion that was going to pass on! In other words, the day catch was some measure of mortality. These data seem to be as strange as some of the data in atomic physics, and one either has to accept them or explain them away.

The plankton net hauled during the day, samples a portion of the population that is equal to the proportion dying! If this is really true, it is a very powerful entrée. We have two powerful pieces of data: relative growth rates and relative mortality between years among different species. Yet, I must say, I put this paper out and thought that I would receive some real criticism. But it disappeared into what I have been calling the anechoic chamber of fisheries science! I've never had anyone say: "You are all screwed up, for this reason or that reason" or "Gee! What a great idea, I see that you are correct, obviously you are correct and now let us incorporate it as a vital tool in fisheries science!" I got neither, and I really don't understand why either I didn't get hit over the head or somebody didn't take this and use it. Since I have you captive and have only two more points to make, I'll spend just a moment showing again how this works (as I have shown in both Science and CalCOFI Reports).

This is the typical size frequency diagram for sardine larvae (Figure 3). There seems to be very little relationship between the slope of this curve, which you might interpret as mortality, and the measured survival of year classes. However, if one breaks these size frequency diagrams into catches during the day and catches during the night and normalizes them for the numbers of such catches, there are two surprising transformations.

The day catch is even steeper (Figure 4). The feature that took me 6 years of self-persuasion was that the day curves are the first derivative of the night curves—a very strange result, and when the decrease in the night curve across an interval is compared to the related total catch during the daytime, there is an absolute 1:1 correlation (Figure 5). In other words, the day catch is the missing part of the population as measured by the night curve. So in some strange way, the daytime curve is measuring the mortal fraction, and it is not that it merely has been subtracted from the totals.

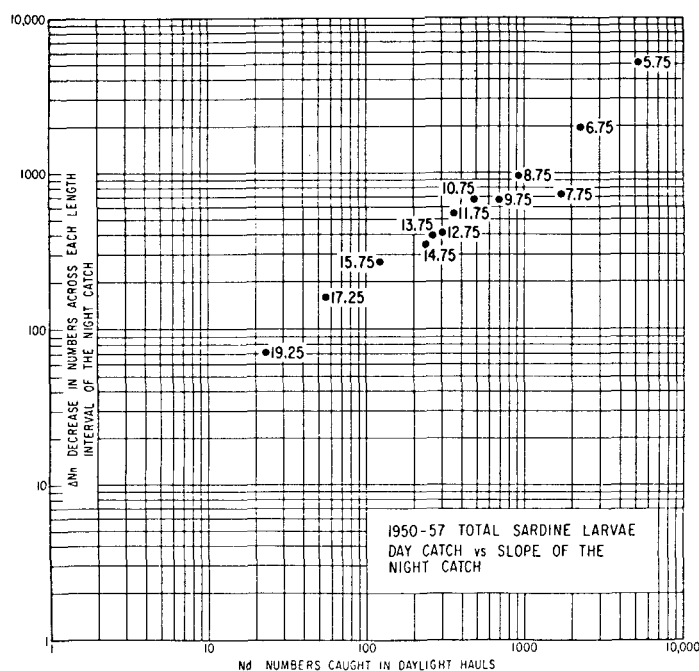


FIGURE 5. Total sardine larvae day catch vs slope of night catch, 1950-1957.

Let me show you something else more astonishing (Figure 6). The other test is to sum up all these intervals of daytime catch and add them successively to the night catch. This reconstructs a nonmortal population! The only possible explanation of this is that the mechanism of mortality, not the cause of mortality, is some sort of a predator which the net quantitative models in day catches. One can show that the larvae caught in the day are active, so they are not dying, only they are representative of those that will die. Perhaps they are merely disoriented, less physically alert, or starving, or they may be solitary larvae, as E. H. Ahlstrom suggested one time.

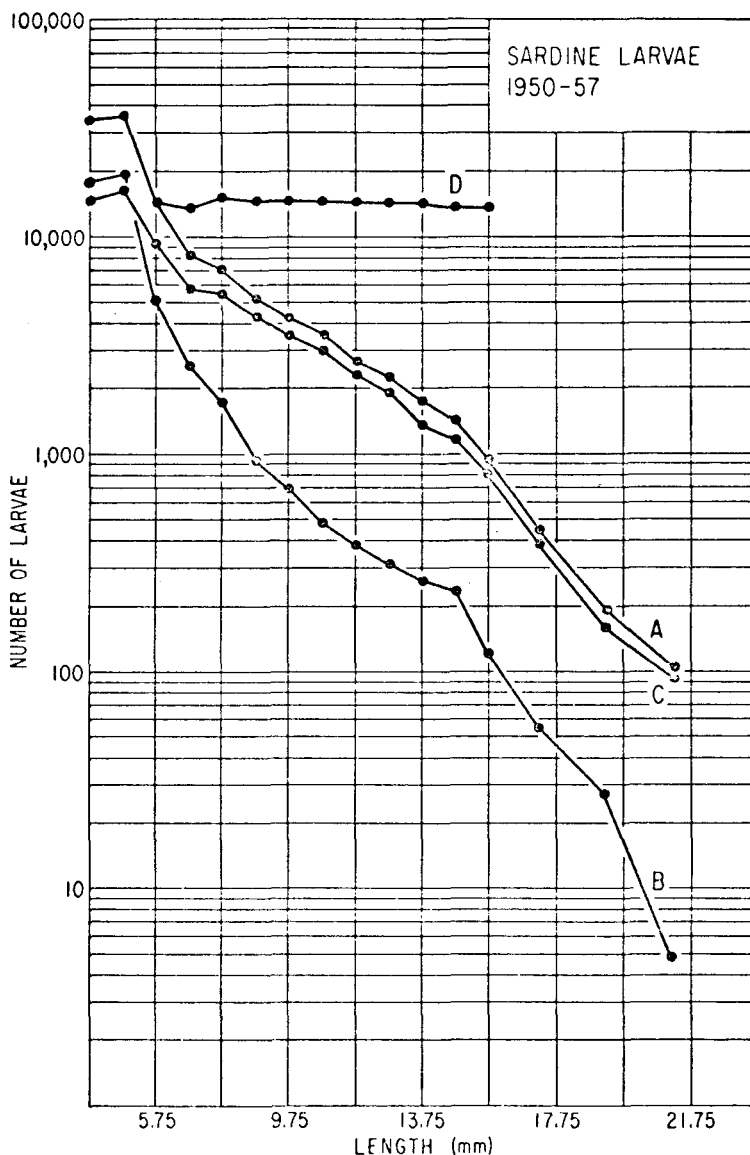


FIGURE 6. Sardine larvae numbers vs length. Curve A is the total catch; Curve B are those larvae taken in daytime hauls; Curve C are those taken in nighttime hauls; and Curve D is a successive summation of the day curve added to each value of the night curve (this should reconstitute the population as it would be sampled in the absence of mortality).

Another fact, curve D (Figure 6) of the reconstituted nonmortal population for different years, either ascends slightly or descends slightly or is level. If the rest of this is true, then the only explanation for a slope of the reconstituted population is that growth rates are changing with length and hence the length of time that larvae spend in these length intervals has changed.

If one interprets these slopes as relative growth rates, and compares the anchovy and the sardine using the graph of relative growth rates (Figure 7), the years 1956 and 1952, the only 2 years advantageous to the sardine, are clearly separated, all the rest being advantageous to the anchovy. If one also plots these relative growth rates as defined by the



slope of the nonmortal curve against recruitment, all points fall on line (Figure 8). One of these days, I would like to hear someone say on this entire thesis that I presented so long ago: "You are absolutely nuts, this is an artifact of the way you handled the data" or "Gee! This is great; let's get at it. This is the way to determine what the success of a year class is going to be, by looking at these larval analyses." But

I have shot this around into the anechoic field of fisheries science, and there apparently is no way to tell if it is alive or a dud. I plan to fuss more about some of these theses. I'm getting too old to wait any longer.

12. *The stocks of fish can be appraised only through sampling of the fish at some stage (including eggs) by direct catch (including tagging studies), acoustics, or other "counting" methods.*

This I think is important, but I am not going to spend much time on it.

First, I think there are some other things to do in acoustics. If we examine very low frequency sound, a fish is an absolutely unique object—a soft place in the water if it has a swim bladder. It alters a great region of the water and the properties there are more profoundly changed. In the case of high frequency, all that the presence of a fish school does is give a reflection, but at low frequency, sound is moving at half its normal speed within the school. This is a very profound effect that isn't true in bulk for high frequency sound. Thus if we transmit sound of very low frequency through a region of fish schools (fish with swim bladders), the transmission of some of the signal would be uniquely and strongly retarded, perhaps quantitatively!

Another important method is one where estimates of populations of pelagic fish are made using fish scales in the sediments of varved cores in the Santa Barbara and Soledad basins. These records very closely follow the present known history of the sardine and anchovy, the build-up of the anchovy and the fall-off of the sardine numerically, and independently of the fisheries data.

The point is, if we can do that much with fish populations in the sediments in these limited areas, what could we do if we actually put down sedimentation collectors? There are only a few places where these sediments are naturally preserved, only two along this coast. But if we were to put down collectors, on a grid and properly deployed above the ocean bottom, we could very closely estimate not only fish populations (an inescapable conclusion), but also what water masses have been present, what phytoplankton, and most particularly, what shifts in climatology of the California Current have occurred. Since in a very readable way, one can relate back into the remote past (as part of this statistical or episodic type of distribution we have been discussing) back for the last 2000 years, why can't we do it at present?

13. *Enforced primitivism in fishery methods is a viable management strategy.*

An incredible anachronism is the way in which we conduct our fisheries. I was astonished to see the huge progress in Washington and British Columbia in the trolling fishery since I fished in those latitudes. They now all carry radar and radar targets to keep

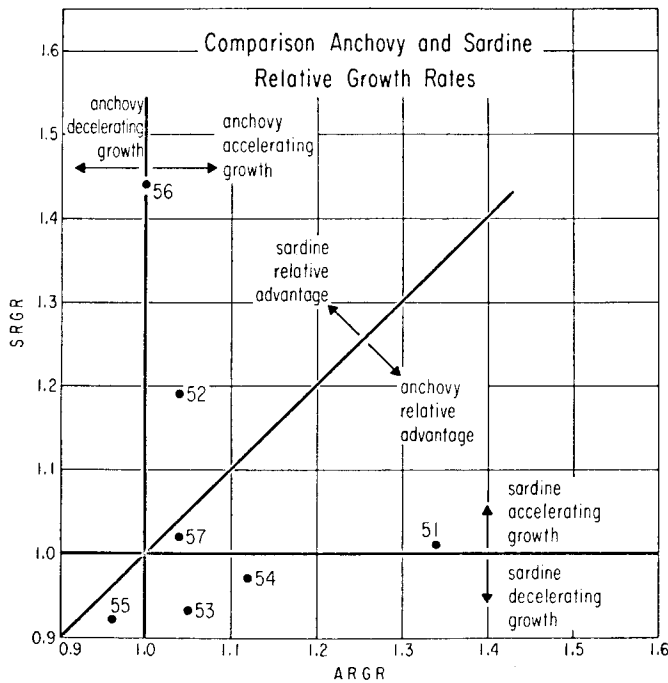


FIGURE 7. Comparison of anchovy and sardine relative growth rates.

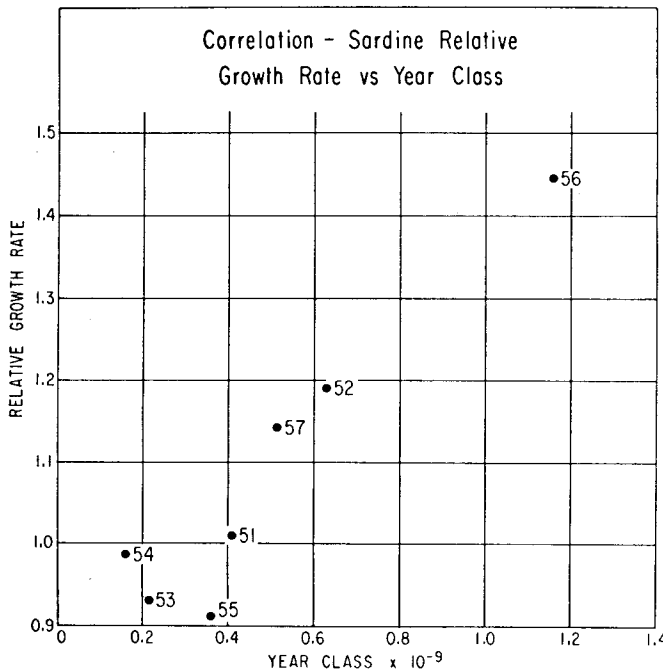


FIGURE 8. Sardine relative growth rate in relation to year class.

from getting run down by the Japanese and Russian mother ships. That's the big technical advance in the last 40 years. Otherwise, it's still little men pulling nets and little men pulling trolling lines in the midst of this particular highly technical society. We are the only nation in the world that insisted up to recently that a great fishery be conducted by such inadequate craft that the boats were built with handles on the bottom to hold on to when these miserable things capsized.

How in the world do we manage this? One hundred years ago, the advanced technology of taking all the allowable catch salmon off these great rivers of the North was well known: fish wheels and traps. These fishermen now could be partners in an industry carefully managed for the take of salmon. They've been outlawed. In attempting to improve their lot, we fiddle around making better corks or cords for these nets so that the fishermen can sit out in the middle of the harbors and dodge the Japanese ships. Only those fisheries, such as the tuna fishery, that have largely escaped regulations have been successful.

14. *Lack of scientific understanding limits the development of some important existing or potential fisheries.*

That is true. We have developed part of an anchovy fishery, now limited by social misunderstanding; but I will submit that reasonably successful fisheries seem to be the ones that little is known about (menhaden, king crab, tuna, etc.), and the least successful fisheries, the ones most is known about (anchovy, hake, and halibut).

Well this is a short list out of a big inventory. I think we have to have serious study not only of the few indictments by one person who is only somewhat peripherally associated with fisheries research, but a full panoply—the total inventory must be immense—it makes me shudder to think of it. I think we should have a series of colloquia, perhaps a summer study, truly to evaluate and get down to brass tacks on these clichés, these dogmas, these facts and fictions in fisheries science, and perhaps elicit some echoes from the silent halls of this field.