

FOREWORD TO THE SYMPOSIUM

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As an introduction to the messages which I expect everyone should take with them from this symposium, I think it is fair to state that no matter from what training one approaches fishery science the application of the catch equation $C = qfP$ in open systems is simply inadequate conceptually and therefore inadequate practically. A simple equation with four variables and 4^n , with $n \geq 1$, unknowns can hardly be satisfactory, either intuitively or mathematically. Most of the background for my concern is given in the "What is a Tuna School?" manuscript, but a few more points from other discussions on these topics will help to move the focus from "Sharp's observations and opinions" to those of others, with the explicit intention of placing the onus on you, the ICCAT Scientific Committee on Research and Statistics, as well as other interested scientists, to find out why the conventional assessment methods do not successfully apply to tunas and many other schooling fishes, and how to replace these with monitoring and interpretive techniques that might do.

Each of you knows to varying degrees the conventional techniques, but I have yet to hear or read a discussion of the collective underlying assumptions and their relative compliance in the specific cases of multi-gear, often multi-targeted tuna fisheries with non-uniform and non-random gear behaviours with respect to these target species, in ICCAT's proceedings or anywhere else. Even this uncertainty is overshadowed by the complications or unknowns in regard to the behaviour of the fishermen in response to one another, economic factors (e.g. fuel prices, fish markets, etc.) and geographic situation, and particularly the varying ambient or climate-ocean physical-biological processes.

The primary assumption in fishery science is that all of this variation "averages out" in the short to medium term, to produce a steady state or near equilibrium condition over time. This is clearly convenient heuristic logic, which pragmatically speaking is neither realistic; credible, given what is known; nor defensible as a basis for decision-making. These are strong words, but nowhere near as devastating as the consequences of continuing to employ, without due regard, paradigms in which the dynamics are "averaged out". See Francis (1980) for a more philosophical discussion of the problems. The results of conventional approaches can be unrepresentative of reality. This effective "curve smoothing" through use of averages is of no value to the fishermen or canner-distributors who need to know whether to order expendable items or make a stockpile or inventory to secure against increased demands and/or limited sources.

One of the most difficult problems for a management advisory group, such as ICCAT-SCRS, is to formulate clear objectives. In recent discussions at FAO in Rome, it became quite obvious that there are few "single-audience" managers, and fewer individuals who can be pointed out as "those persons who manage fisheries X or Y". There are even fewer fisheries where management is successfully based on voluntary compliance with regulations which require the fishermen to decide to not catch fish rather than to sort out from catches the unmarketable or illegal individuals, hence no real benefits derive to the resource population from regulation. For example, invoking yield-per-recruit regulations in fisheries where there are limited effects of gear selectivity such as tuna purse-seine fisheries

makes the real proposition simply one of censuring or closing fisheries (i.e. western Atlantic bluefin seine fishery), whereas the trade-off in the Gulf of Guinea small tuna fishery does not warrant any more yield-per-recruit consideration once one decides that the losses of catch at appropriate times and locations are not likely to be made up or even only partially recuperated at a later date.

What this is to preface is simply that once the facts are on the table, i.e. size, abundance and distributions of the catches, the elegance or precision of the models is often irrelevant in comparison to the recognition of the behaviour of the fishermen and/or the fish.

The most pressing problems in tuna and billfish fisheries are not actually how much can we take before the stocks collapse, but more fundamentally what are the stock compositions, distributions and patterns of variation? This implies a working knowledge of growth rates, movement patterns of identifiable population components, and some measure of abundance, apparent or real, which can eventually be interpreted or followed back to the basic stock components. Once these questions are answered, how much to take is a trivial exercise which any "Hewlett-Packard Generation" baccalaureat can solve, given the available mathematical tools. Without this basic knowledge these analyses reside solely in the realm of contestable guesswork.

In my studies I have found no single example of a tropical tuna fishery which, upon rigorous genetic analysis, can be said to comprise a simple single source "unit stock" in the genetic sense (Sharp, 1972, 1978, MSA, MSb; Anonymous, 1976; Sharp and Kane, MS). This is not taken into account by any tuna management body at present. For example, the Atlantic bluefin tuna presents an odd and provocative picture of two nearly or relatively independent geographic phenomena which blend in their overlapping ranges, to suggest that this is another not-so "unit stock". There are numerous other examples, but these are merely "frosting on the cake",

so to speak, showing that far more attention needs to be paid to this basic problem than has been evidenced to date in the Atlantic.

The International Skipjack Year is a good example of a concerted effort toward investigating many of the questions posed above, but I would suggest that in the next few years a less "single minded" approach must be taken, so as to obtain a better understanding of all the relevant species, tropical, temperate and particularly billfish.

In regard to the problems of interpreting "rates" in the several fishery contexts, I believe that too little effort has been made throughout the tuna-billfish community to resolve the questions through direct measurement methods. My introduction of Dr. Brothers to the yellowfin tuna ageing problem in 1976 was strictly fortuitous, but now some five years later we have a fair idea of just what growth rates are in the eastern Pacific. We can also say that a few fish, collected and analysed from each fishery will be far more informative than the hundreds of hours of data processing and "cohort definition" implicit in modal progression studies, which has been far too frequently given priority over direct methods since the direct methods became available.

Since about 1970, I have been trying to get an idea of what a "cohort" really is in the eastern Pacific yellowfin fishery. The problems of their identification and trying to follow any sort of patterns in growth and distribution are really confusing. When both Pat Tomlinson and Walter Ritter-Ortiz spent over a year chasing through the eastern Pacific Ocean samples by region and area it became clear that the cohort, per se, is a poorly defined entity at best. The only traceable "cohorts" were available within any sampling area for a very limited time, they might be traced to adjacent areas or just disappear from the records for up to one year (only identifiable in this case by tag returns or some such corroborative data).

In the genetic data it was obvious from the start that samples with age-size distributions including more than 15-30 cm range could be shown to have cycles of transferrin A allele frequencies which were semestral in age-length-growth relation (Sharp, 1972, MSA). It was also clear that the old X-Y categories of Hennemuth (1961) were inadequate to explain these semestral patterns and I re-invented a Semester A - Semester B label to categorize time of entry of 40 cm fish into the fishery. As a carry-over, it was also obvious that the recruitment at 40 cm did not necessarily have centrally located peaks in abundance in the semesters A or B. The recruitment and preceding spawning could occur at any time - continuously during the year, but A and B were generally distinguishable genetically.

Bob Francis and I tabled in our energetics paper the "recruitment groups" by area and semester of original recruitment cohort (by definition) and my point in including that table appears to have eluded everyone (Sharp and Francis, 1976). The areas are such that there is nearly no chance of fish migrating between the north and south (based on summarized tag recovery data) and area 5 or the middle zone where the fishery is nearly continuously active and most productive is really a zone of overlap in abundance for either or both of the extreme areas, plus "its own" general production. Quite clearly Table 1 of Sharp and Francis shows that if you can believe that there are at least 3 separable regions of reproduction, which the gonad data and genetic data would suggest, then the "recruitment" is not particularly stable within each area (Sharp, 1980a). This is seen in comparison of the Northern A semester cohorts caught in 1966 (7 769 tons) and 1970 (39 197 tons). We integrated recruitment years to make the table compact, but even at that a regional difference of 5X in catch, if this represents abundance at all, would suggest much greater differences in annual recruitment on this regional basis. Of course this does not begin to compare to the >300X variation in catch of Japanese sardines in the last decade or forty

year period between peaks, but this pattern is what results from an opportunistic approach to reproduction in what I term oceanic-nomad species in contrast to typical pelagic species with more localised or homing spawning behaviour (Sharp, 1980a).

The manuscript entitled "Colonization: opportunism in the ocean" is included in the report and supporting documentation of a workshop I convened in Lima. This manuscript has as a basis the Hjort-Lasker early life history microcosm paradigm. It also benefits from my genetic, physiological and behavioural comparisons of the tunas as portrayed in the "Physiological Ecology of Tunas" (Sharp and Dizon, 1978), and considerations regarding the evolutionary differentials in cosmopolitan and homing or localized species, and the intergradations between these types of fishes. In the colonization manuscript I point out that the low levels of population abundance fluctuations in tunas is a likely result of their constant search and sample strategy wherein only size similar, common history, hence common condition individuals are involved in any given reproductive event. This is doubly interesting as it appears nearly irrefutable now that sibship in tuna schools is high, from which one can infer that each similar size sibling group is a potential colonizing unit which itself has a major component of individuals from the same larval-survival group or "cohort".

In the Lima Workshop on Effects of Environmental Variation on Survival of Larval Pelagic Fishes (Sharp, 1980b) it was made abundantly clear that the necessary conditions of successful reproduction of fishes can be broken into several classes which, due to their definite hierarchic sequencing, can or need to be treated as "pre-conditions". I will get to the meaning of that in short order. The first stages which must be met by larval fishes is abundant appropriate food. That results only from a physical process which must precede the biological events. This physical event in coastal

areas is typically upwelling. The optimal upwelling processes appear to have a set of finite limits in time; they should pulse on a frequency which promotes primary production and sequential maturation of the phytoplankton and zooplankton community and still not be so strong and widespread in area that all of this production is lost to the offshore open ocean. This implies that they are also constrained such that the upwelling should not be too strong and too long. Also any destratification which might occur on the short term, needs to be re-established at regular intervals such that the food species (particles) are in high density or contagion. The physics of the process is receiving lots of attention and Andy Bakun's regional upwelling indices show that there are indeed discontinuities on the short term and over relatively concise regions of these events.

The second pre-condition is development of a phytoplankton-zooplankton community with appropriate species (in the workshop we decided *esculent particles* was an appropriate term). The third pre-condition is that these *esculent particles* should be stratified into aggregates of appropriate density to stimulate feeding by the individual fish larvae as they hatch. From there on out their survival becomes the complex predator-prey problem, etc. which we are not even close to resolving given our present sampling tools and basic ignorance of invertebrate predators. They must have their first meal or there is no survival.

It is necessary now to back up a bit to the place where the adults of the various species enter the picture. For continuous successful reproduction they must be tuned or adapted to the cycles of "good and bad" physical variation. If the typical pattern of climate includes a high probability of several years of "bad conditions" then only fish with lives of "several years plus one" can really be expected to survive.

The fish in this sense are pre-adapted to the physical-climate of the systems in which they are abundant (you do not have a year added to your lifetime because your young did not survive). So the "tuned" individuals

are the only ones who are sustained through the long term. The adults must invest their reproduction energy wisely, and from Hunter and Goldberg's (1980) observations on batch spawning in *Engraulis* one could estimate that since they appear to spawn sequentially in 10-15 day cycles and the proportion of ripe individuals is relatively small at any one time (e.g. 7 to 10 percent) then their strategy is to continuously enter eggs into what must be a highly variable environment. One strategy is to salt, more or less continuously, eggs into a smallish region where, on-the-average, the pre-conditions will be met on an annual basis.

Another strategy is to be more exploratory, i.e. not migrant per se, but not homing either - where the species is more nomadic within a finite range. I think that the mackerels and *Scomberomorus* species are good examples. All species have a portion of their population with this behaviour or there would only be one universal species.

The next step is to become totally opportunistic in reproduction as I feel the cosmopolitan species need to be. The problem here is how to locate appropriate larval habitat in a patchy system of pre-conditions. Indiscriminant sowing of eggs into the oceanic environment does not seem likely to be the best strategy. Scombrid larvae are busy courting predators with no qualms regarding cannibalism. They must, however, have relatively nutritious (large) food in fair abundances if they are to survive. The open ocean systems must provide this scenario which differs only in a few details from the general picture previously presented. The plankton community should be slightly more developed, providing larger prey for the larvae, and since the scombrids do grow rapidly, there must be lots of it. As much of the larger zooplankton which tunas thrive on are themselves voracious predators there must be some particularly gruelling times in trying to get from the egg to post-larval stages. Where this is best accomplished is in a freshly maturing plankton patch with few predators. The open ocean seems a perfect opportunity given that production can be initiated, and located.

The interesting conclusion which can be drawn from the results of studies in laboratory rearing and at-sea sampling of scombrids, as well as from an understanding of their feeding energetics and the relative distributions and abundances of appropriate particles in the water column, is that larval scombrids sampled from the surface or within the mixed layer, well above the thermocline, are very likely in negative energy balance and must either find their way or be transported by convection to the dense aggregations of appropriate food organisms, or they will indeed starve to death. In warm tropical waters (>26°C) this will be a very rapid, short term phenomenon. In areas where the thermocline is shallow, the opportunities for encountering stratification and significant aggregations of appropriate food should be far greater than in areas with well mixed upper layers and therefore very deep thermoclines. This set of considerations may well explain the absence of large numbers of larvae except in shallow thermocline areas, i.e. eastern ocean boundaries and near shoals or islands.

Another set of questions that constantly re-appears are those about relations between temperature, development time and predation. Point 1) the absolute temperature is nearly irrelevant, although intermediate to cool temperatures within a larval fish's tolerance promote better survival for the individual larvae for metabolic reasons; Point 2) predators in warmer habitats follow all the thermal laws and hence invoking Q_{10} , a bleary but identifiable biological principle, the log of a predator's activity increases in proportion to the increased temperature; their activity-metabolism is a cubic function of the activity, hence they are disproportionately voracious in the warm habitat, cancelling nearly all benefits a larval scombrid might derive from developing rapidly from egg to larval stages in the warm environment. Point 3) the larvae upon hatching becomes a victim of the energetic spiral as in point 2 and there-

fore is generally better off by "cooling it". It wouldn't take much to show all of this scenario is true, as the pieces have already been worked out in various forms. The following table gives the essence of the problem.

Table ...

Habitat Temperature	Activity Index	Respiration/Feed index
31	1.414	283%
30	1.320	230%
29	1.231	187%
28	1.149	152%
27	1.072	123%
26°C	1.000	100%
25	0.933	81%
24	0.870	66%
23	0.813	54%
22	0.758	43%
21	0.707	35%

If we take the 26°C isotherm as a likely centre-point for egg larval hatching and development, and using the requirements in food/respiration at this temperature as a baseline, the effects of temperature alone on activity level can be indexed as a simple function of Q_{10} are shown to range from 41.4% above the 26°C value with a five degree Celcius ambient temperature increase, or conversely to be only 70.7% of the 26°C values with a five degree decline. However, the energy expenditure based on activity alone in a continuously swimming larval or post larval fish varies as the cube of the swimming speed. As one can quickly see, this results in dramatic food/respiration differences.

Another look at some dichotomies which present themselves, particularly as regards tunas, is needed. I will not make this into another book by following up the ones I have thought about, but one is a real puzzle.

Adult tunas have virtually nothing ecologically in common with their eggs and larvae. The adults are buffered against environmental variations over tremendous ranges by their physiological and morphological adaptations and size. The eggs and larvae are not so fortunate - they are tied to

thermal and local requirements which are quite restrictive. The eggs and larvae must be "placed" into appropriate environments. The adults have no small scale limitations, they travel hundreds of kilometres in days in search of appropriate requisites. How do the adults decide what and where is an appropriate larval habitat? It does not appear to be explainable by the homing sense, as "home" is a relatively large system for many tunas (and other species). In fact, adult bluefins appear to poorly tolerate the regions where their eggs and larvae need to be placed. They apparently cease eating and run on their "stores", much like salmonids. The whole ecological question of r selection versus K selection falls to pieces when you are dealing with species which start as 1 mm spheres in a variably productive, patchy environment, and subsequently grow to hundreds of kilos with the remarkable mobility and buffering adaptations previously mentioned. The entire concept of modelling or representation of the numerous interactions in this group needs to be reconsidered and overhauled. The problems begin with the basis of their survival, not their mortalities as is the general approach. If we knew their requirements, we might then learn what to look for, and how to make prognoses rather than hindcasts.

The message in the Hjort-Lasker paradigm is that it is not the "average" egg, larva or juvenile that survives, it is some small number in comparison to the total that find themselves placed well out on the tails of overlapping distributions of the necessary pre-conditions. On the average, these conditions do not occur except for short periods in small subsets of the potential habitat of the various species. Man's influence is trivial in contrast. We just muck up the species evolutionary investment by trimming the potential investment in the environmental-climatic system within which they are trying to survive. From this viewpoint one can even argue that the concept of "surplus production" is nonsensical, but I will leave that topic for another time and another place.

Well, you will ask, just who then is doing the "right thing" in the study of populations of pelagic schooling fishes? I will recommend that everyone read soon the section starting on page 65, entitled Factors Effecting Recruitment in Parrish and MacCall (1978) where one can read for oneself the potential from multidisciplinary applications in studying scombrid fishes and other pelagics.

I would also suggest that Csirke and Caddy (in press, MS) have given new light to the problems of production models and equilibrium-based approaches. They give far more intuitively satisfying methods for solving for sustainable yields without requiring effort data (for either of the axes, and certainly not both as in the CPUE-based models). Their second manuscript discusses the relations between r, the rate of increase in numbers; M, natural mortality; F_{msy} , in the conventional Schaeffer model this is roughly equal to natural mortality; and the F_{mbp} , the maximum biological production based on the logistic where $Z = M + 2F_{msy}$, which indicates the least debilitating or healthiest place to be on the yield curve since at F_{mbp} "the population is at its most productive and competitive". From their evaluations they have concluded that "with the logistic model, the point MSY occurs when the total production of the population is already declining", i.e. is beyond MBP. Another important implication of their evaluation is that "if" natural mortality is primarily due to predation that in fish like tunas which make such fast and great changes among prey-predator fields, natural mortality will vary greatly with size. Also if one considers the multi-species complex in an area like the Gulf of Guinea, if there are food limitations in the early stages, as studies indicate (Sharp and Francis, 1976; Kitchell et al, 1978), then fishing (man's predation) a considerable number of the small tunas will actually improve feeding efficiency and individual growth, resulting in increased Y/R in the long term.

Now how does one arrive at this set of conclusions? The first step is to begin studying the fish, in their environment. To have become nearly

totally dependent upon "body counts" rather than the relevant survival of the pelagic species, particularly the nomadic oceanic species, has left a great void in the necessary information to describe what these resources are doing and what to expect from them. It is moot that there indeed is an "average", but that it tells one very little because it only occurs "instantaneously" not necessarily even twice in any data set needs to be understood. The five following examples of typical functions shows this point. Each example looks very like some common measure of fish behaviour, abundance or characteristics.

I would suggest that everyone begin to re-assess their contributions to this field, and decide for themselves whether the data set which they have available to them is adequate to base a management decision upon. I believe that in all honesty we should settle down to the real problems, information acquisition, before confining the study of tuna fisheries to the same end as so many others. Gunnar Saetersdal (1981) has documented this end-point all too well.

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