ICLARM STRATEGIC PLANNING: ASSESSING THE GLOBAL SITUATION 17 - 19 December 1990

Report of Working Group on

Scope for Research Impact on Living Aquatic Resources Management

I. Session Schedule

Monday 17: 17^{00} - 20^{30}	Working group proper
Tuesday 18: 09^{00} - 11^{00}	Follow-up plenary session

II. Membership

Chairperson	:	D. Pauly
Rapporteur	:	C. dela Cruz
Members	:	V. Christensen; Chua Thia Eng; L. Fallon; C. Farrar; F. Henderson; C. Lightfoot, J.L. Maclean; J.L. Munro; R.S.V. Pullin and G. Silvestre.

III. Terms of Reference

The following terms of reference were provided to the working group by Dr. C Farrar:

"The Working Group should identify those research problems or areas within the broad field of fisheries in developing countries which the trends identified by the first Working Group suggest should be on the international research agenda. Research areas should be included whether or not it appears likely that they will ultimately form part of the ICLARM program, provided that the research involved could contribute to the achievement of ICLARM goals, as in the mission statement, or CGIAR goals as stated by TAC. At this stage, when in doubt an item should be included.

Research areas suggested by the TAC, in the SIFR materials, and elsewhere, should be considered. (Implied or explicit revisions of the description of trends may result from this process.) Specific account should be taken of the likelihood of success in the research, given the present state of scientific knowledge; and the probability that the results will be taken up and applied. The potential or actual demand for the results, and other economic factors also should be considered.

The Working Group also needs to identify areas of further work beyond the mere elaboration of the points in their report.

V. Second Round of Working Group Discussions:

The round of discussions following the definition/identification of research areas involved elaborating upon areas A to F as much as time allowed (which was not much); the major points that emerged from this are recalled here by area (with some unavoidably subjective elaboration by the WG's Chairperson):

A: "Global Trends"

The first key concept emerging from the discussions was monitoring (of trends, of pollutants, etc). It is research on the estimation of the carbon dioxide content of the air, and its subsequent monitoring over more than two decades, and the analysis of old air temperature data and especially of sea water temperature records which provided the key evidence for global climatic changes, and the impetus for global climate models.

The initial monitoring of atmospheric CO_2 was clearly basic research, at the very extreme of a spectrum whose other extreme may involve the repeated measurements of known pollutants e.g. for the very practical purposes of a public health agency. This shows, among other things, that the word "monitoring" has various meanings, which may even include the actual measuring process in the field; the WG however, considered only the analysis and interpretation (usually via some computer model) of (extant) data as "monitoring".

With this proviso, there was a consensus that monitoring of environmental and societal variables is a major research area for the next decades.

For the aquatic world, monitoring the levels of various pollutants (combined with assessment of their toxicities on various aquatic organisms) appears particularly important, given that pollution levels are likely to increase throughout much of the world.

Other important aspects of "monitoring are those related to present (or future) usefulness i.e. the research devoted to ,e.g., the cost-effectiveness of various monitoring schemes (as related, e.g., to the size, and the temporal and spatial frequencies of samples) and to (ii) the appropriateness of monitoring ecosystems in which "nothing" seems to happen, both to obtain background or control information, and to be ready for unexpected (non-linear) changes.

Examples of (ii) are the (present) usefulness of schemes aimed at monitoring (previously) "irrelevant" variables, such e.g. as the CO_2 content of the atmosphere on top of a Hawaiian volcano (see above), the ozone content of the air above Antarctica (of ozone hole fame), or the state of coral reefs, now increasingly threatened by more widespread, more frequent and longer lasting "bleaching events", apparently due to increased water temperatures.

The WG came to the conclusion that what should be done is the appropriate kind of research.

[To the WG Chair, now writing up this report, this conclusion appears, in retrospect, to be too facile, too reminiscent of an S. Harris' cartoon of a lab praising itself for doing "not high tech, not low tech, but the right tech".

It is evident, after one has discussed global climate models, that a third new pillar has been added to the classic two pillars of the scientific method (experiment and theory). That pillar is modelling, a tool that is not strictly an experiment (the climate in climate models is not experimented with) nor theory, since this only tells you how to construct the model, but can't predict their results - at least for non-trivial models involving feedback and/or non-linear effects (note here that our ECOPATH II and ELEFAN models are trivial by this definition).

Clearly, a discussion on the right mix of approach must involve the relative benefits and applicability of experiment *and* theory *versus* modelling, and in future loops of our planning process, the relative importance of these three scientific approaches to an international research center *versus* national development-oriented institutions].

B. Global Effects

The WG discussion of item B suffered throughout from conceptual difficulties in separating this item from item A. Nevertheless, a number of issues were discussed under item B; they all tend to share the feature that they are results or effects of major demographic and economic trends.

One major issue for the WG was the question whether predicted demographic trends for the next three decades necessarily imply falling per capita incomes for the majority of the world's population, and/or a widening of the gap between the "haves" and the "have-nots".

Two trends seem to point in this direction:

i) falling average incomes throughout the developing world in the 1980's and

ii) falling per capita income, food production (and consumption?) in Africa since the early 1960's.

These trends are extremely discouraging. However, as emphasized, by WG member C. Farrar, the CGIAR doesn't assume these trends will continue, and indeed, it is CGIAR policy to assume income to increase in the long term - as can be indeed demonstrated to have been the case even for countries viewed as "hopeless basket cases 2-3 decades ago.

Moreover, even if medium per capita incomes should continue to decrease, the increased number of well-to-do members of the population of developing countries will increase demand for high quality products, in addition to the foodstuff, fiber and other basic products needed by an increased number of poor people.

Capture fisheries are subsystems emerging from the interaction of a socioeconomic and technical system (an extractive machinery coupled with a market), in the context of a legal system (e.g. common property within EEZ's), with a biological system (the aquatic ecosystem producing the organisms that are being extracted).

Understanding fisheries, and providing advice for managing them is thus mainly a matter of dealing with interacting system properties, rather than elucidating, in a reductionistic mode, say a specific biological feature of one of the interacting species.

Thus, "upstream research" must therefore almost by definition include a heavy dose of modelling - and could indeed basically consist of model development, with experiment and theory being less crucial. [This is not to state that one would neither do experiments nor develop new theories; rather, this means that experiments (and field data gathering) and theory become model-driven, just as experiments, have earlier and/or in various disciplines, still tend to be theory-driven. [This theme is further developed in Appendix I which contrasts "hydrography", "biological oceanography", "marine ecology", etc, i.e. relatively unspecific "environmental modelling" as suggested by the Study of International Fisheries Research (SIFR) with focussed "multispecies modelling" as a vibrant area of fishery research in developed countries with a great potential in the developing world.]

Of the six above-listed SIFR priority areas, three (1, 4 and 6) pertain to capture fisheries *sensu stricto*. Of these, # 1 covers the above-mentioned issue of modelling, as well as "population dynamics of early stages of fish stocks". What is hidden under the latter formulation is the "recruitment problem", i.e. the Holy Grail of earlier generations of fishery biologists. This consists of attempts at predicting "next year recruitment" (of young fishes) and hence future yields, based (largely) on an extremely detailed knowledge of the physical and biological environment of the larvae of the studied species, and on the size and structure of the spawning stock.

Although it continues to attract some attention (and funding), this highly reductionistic research program is to a large extent, a failed one - we are, after decades of research, not nearer to predicting next year's recruitment than in the 1950's - and practical fisheries management, such as e.g. for the intensely studied North Sea stocks uses rather costly young fish surveys for yield prediction.

Although their potential may be overestimated, there appear to be presently untapped unconventional resources which may contribute to significantly increase world wide fisheries catches, notably among some oceanic fishes and squids. One problem with these, and the main reason why fisheries for them have not evolved to date, is the lack of gears/technologies for harvesting them economically (e.g. in the case of the very scattered myctophids, or lantern fish, occuring at great depth in average densities of a few g per m^3 of water) or even for catching them at all (in the case of the giant squids, "harvested" only by sperm whales).

These and related issues are pursued further in Appendix II.

Farmers acceptance of aquaculture has been low to date, particularly in those areas in Africa where the need for the supplement for income and/or protein that fish could contribute would be most useful. The political/administrative, cultural, social, economic and other barriers involved here represent a rich area of Policy/Strategy research (which would have, in a number of cases, to be connected with studies of water supply and uses, a critical variable regarding the expansion of aquaculture).

[At this point, the WG discussed the difference of potential between mariculture and inland aquaculture, and more importantly, the isomorphies between farming systems research/watershed management on the one hand and mariculture/coastal zone management on the other.

It was noted that both sets of systems required interdisciplinary, "holistic" approaches, and that such approaches, when rigorously articulated do represent "upstream research". The crux of the matter, though, is the <u>rigor</u> of that research and its <u>generality</u> - topics which the WG did not even begin to address satisfactorily].

iii) <u>Culture-based Fisheries</u>

Culture-based fisheries enhancement represents one of the TAC's areas of emphasis, and is also prominently discussed in the SIFR report. Unfortunately, culture-based fisheries, appear to be different thing to different people, and before profitable areas of research can be identified and evaluated, it is necessary to define the concept, and explore some of its ramifications.

The Working Group only initiated this, and the few keywords noted here by the rapporteur were: genetics of culture organisms, of diadromous fishes, conservation of genetic diversity, ranching, protected areas management, location-specificity of solutions, stocking, recruitment and habitat enhancement - all tantalizing clues to a discussion which could not be conducted due to lack of time.

Indeed, there was time only to agree that culture-based fisheries are indeed one area where the impact of research may be most decisive. Such research would draw expertise now monopolized by specialized groups e.g.fisheries scientists (e.g. for population dynamic models) and aquaculturists (e.g. for production of stockable fingerlings), to mention only two obvious disciplines.

D. <u>Post-harvest losses</u>

[This account is based only to a small extent on the Discussion of the Working Group; the bulk of the text below consists of material assembled by the Chairperson].

E. <u>Women in Fisheries</u>

A vast body of empirical evidence exists which shows that women, almost everywhere, and particularly in developing countries, while contributing hard work to the welfare of their families, suffer from low incomes and reduced access to food, health and educational resources. There is also a growing body of evidence showing that this inequity, and the increasing "feminization of poverty" presently occurring in (both developed and) developing countries represents powerful constraint to development.

This, and certain developments in the developed (donor) countries have resulted in a strong pressure for institutions involved in research for development (e.g., the CGIAR) to deal constructively with "gender issues:. However, the temptation for these institutions to minimize the problems alluded to above and/or their impact on their own activity is extremely great, and this often results in lip service being paid to this issue, or in desultory action being taken. This is dangerous, for two reasons:

- o it may objectively harm these institutions' work; and
- o it will eventually lead to political pressure being applied to these institutions.

The world of developing countries fisheries of the research and development institutions dealing with this world abounds with illustrations for the above statements. However, the WG did not include as a member any professional with expertise in gender issues, and this issue will have to be followed upon as part of another element of our Strategic planning process.

F. <u>Biodiversity</u>

Biodiversity involves (i) genetic diversity <u>within</u> species (ii) high number of species and (iii) the integrity of the ecosystem supporting (i) and (ii).

The present global assault lead to reductions of (i), (ii) and (iii) - contrary to the widely held belief that biodiversity involves only high number of species (i.e. only item ii).

Maintaining a modicum of biodiversity in the next decades must involve a considerable amount of research ranging from basic ecology to park and reserve management: areas about which far too little is known.

This research might have among its highlights:

- o effects of selective exploitation, and of non-extractive uses;
- o size and spacing of reserve areas,

as well as, hopefully, better focussed and more conclusive studies on the perennial issue of "stability" in response to stress, or changes in boundary conditions.

[Time ran out for the WG to explore this theme in details].

Appendix I: Notes on Modelling of Fisheries System

Fishes are usually not seen, counted or weighted before they are caught, and hence, fishery science has from the very onset, at the turn of the last century, depended on indirect inferences - of the mathematical and statistical kind - for quantifying vital statistics of fish populations.

Thus, some of the earliest application of mathematics and statistics (outside of the physical sciences) are those of fishery biologists.

An analogous phenomenon occurred with computers, which fishery biologists have tended to use earlier than other biologists.

Nowadays, notwithstanding some frightfully mathematical texts in theoretical ecology, it still is fishery biologists which tend to build the most awesome models. The reason for this is not only the one alluded to above - the need to indirectly estimate e.g. the size of exploited populations, but also two additional, interrelated facts:

i) fishing biologists usually have lots of field data with which to parameterize their model;

ii) because of the open access nature of fishery resources, these must be managed by public agencies which also generate publicly available data.

The most important step in the development of fishery biology as a quantitative science came through the publication as a quantitative science came through publication of "On the Dynamics of Exploited fish populations" by Beverton and Holt in 1957. In this book Beverton and Holt presented the basic concepts of single species fisheries assessment together with descriptions of its references.

The single species models have mainly been applied using the virtual population analysis (VPA), a method developed in 1965 by J.A. Gulland to reconstruct fish populations from catch data. The VPA has been applied with varying degrees of success on a large number of stocks especially in temperate waters. The main conclusion that can be drawn from these exercises is that the VPA can generally be applied with success to stocks with limited fishing pressure if the management options that are considered do not change the interactions between stocks notably. For stocks with high fishing pressure or with a marked influence on or from other stocks the use of single species models is problematic.

To circumvent these problems so that more reliable advice can be given for the problem stocks, various extensions of the Beverton and Holt model, taking species interaction into account, have been suggested. The most advanced example of this is the multispecies VPA model of the North Sea developed and maintained by the Multispecies Working Group of the International Council for the Exploration of the Seas (ICES).

The latter theory offers tantalizing clues that it should be possible to manage fisheries ecosystems in non- taxonomical basis i.e. without reference to specific single species and without catch data being available on a per species basis. It also appears that particle size theory may help resolve or at least contribute major advances to the famous "recruitment problem".

"Solving the recruitment problem" i.e., finding the reason(s) "why stock vary" from year to year was seen, until the development of multispecies modelling, as the major challenge for fisheries biologists. It appears now that the way the recruitment problem is formulated makes it impossible to be solved, i.e., it is an intractable problem. Dozens of factors influence the recruitment and only in the extreme few cases where the factors are few and the effects hence foreseeable have successful predictions emerged. Particle size theory may help resolve this problem. Further, we note that for size, the elements of a single species theory structure model applicable to fishery science has recently been has formulated by a DIFMR scientist. This model is rapidly gaining acceptance among leading fisheries scientists in Europe and North America as a complement to and refinement of MSVPA and related approaches.

It seems that ICLARM would be very well positioned to pick-up these various threads, and in cooperation which scientists from advanced institutions, develop a size-structured theory of fishing suitable for implementation in tropical developing countries.

This last element (that the theory be suitable for application in tropical developed countries) is very important because the scientists in developed countries' institutions cannot be expected to sustain their research efforts (e.g., merging particle size theory and multispecies theory) to the extent that this becomes applicable and useful (e.g. in the form of software) for tropical developing countries.

ICLARM has conducted research on this through the development of length based stock assessment and of software implementing this methodology, as well as through the development of a tool for constructing of comparable ecosystem models, the ECOPATH II software. The next version of ECOPATH II (due in May-June 1991) will include an implementation of Phalanx analysis and thus elements of particle size theory.

For ICLARM to become a recognized leader in this field, however, an expansion of the number of staff working on modelling would be necessary, as well as an intensification of their contact with developed country institutions.

Fisheries systems modelling should also be conducted at the level of socioeconomic and policy research. Interesting developments in this areas have been the attempt of Drs. Jacqueline Macglade and Peter Allen to develop models for describing the complex and variable behavior of fishermen faced with spatially and temporarily varying fishing stocks. Their model could probably be made to explain the often puzzling behaviour or artisanal fishermen in tropical developing countries.

Appendix II Unconventional marine living resources

When confronted with the food shortages caused by growing human populations and needs, and limited scope for expansion of traditional agricultural systems, many government officials, managers and large segments of the lay public see the world's oceans as our next major source of food either from extended capture fisheries or mariculture. This view is problematic, for three reasons:

i) The oceans are already newly fully exploited, and provided in the late 1980's about 80 million tonnes per year;

ii) Sustaining this harvest in the face of present fishing practices and lack of efficient management will be difficult;

iii) Mariculture, or sea farming requires secure sites, and must rely on inputs, as does any farming enterprise, that are in limited supply, and for which it will compete with capture fisheries (for sites) and/or land-based agriculture (for some inputs). Also, most mariculture operations actually consume protein as they feed on soya-based pellets, anchovies, sardines and other cheap fishes to high-priced species such as salmon, seebass or grouper, generally with losses of about 90% of the protein fed.

Approximately 90% of the world's marine fish harvest stems from "shelves" i.e. the shallow areas with high primary production adjacent to the continents down to 200 m. All major shelves of the world are now exploited, with serious overfishing problems reported from most of them (e.g. North Sea, Sunda Shelf), the few exceptions possibly being parts of the Patagonian, Antarctic and Sahul shelves.

Attempts to identify "nonconventional" marine resources have involved among others:

- lantern fishes (fam. Myctophidae), of which billions of tonnes may occur in the bathypelagical zone of the world's oceans, but generally at very low concentrations (in the order of 1 gram per tonne of water), precluding their commercial exploitations given present technologies;

- oceanic squids, whose biomass and production appear to be very high (based on sperm whale stomach contents), but which are extremely difficult to catch given present technologies, which would be difficult to market (if caught).

- the antarctic "krill" *Euphausia superba*, the key trophic link for the fishes, penguins, seals, and whales of the antarctic ecosystem, now considered for protection, and in any case not exploitable by (protein-) poor countries;

- fishes or invertebrates in OTEC plants (Ocean Thermal Energy Conversion), driven by artificially upwelled, nutrient-rich deep waters), and which would require investments that would make the products so expensive as to be out of reach of people now unable to secure sufficient protein.