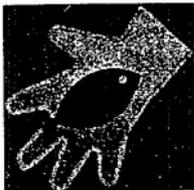


TOWARDS THE INCREASED POLICY RELEVANCE OF FISHERIES RESEARCH

A discussion paper prepared for the Fifth Fisheries Development Donor Consultation
Rome, Italy, 22-24 February 1999



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by

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The views expressed in the report do not necessarily reflect those of either SIFAR or the Food and Agriculture Organization.

Author's note and acknowledgement

The purpose of the paper was, and remains, to stimulate debate and, for this reason, positions taken are somewhat more polemical than might otherwise have been the case.

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The errors which remain are mine alone.

S. Cunningham

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ABSTRACT

The world's fisheries face a multitude of problems. Overexploitation of fish stocks and overcapacity of exploitation systems are increasingly common, as is the inability to conserve essential habitat especially in coastal areas. Many of these problems are symptoms of the failure to recognize and generate the economic value of fish and associated environmental resources. The potential contribution of research to the development of fisheries policy can be assessed in terms of this economic surplus potential.

The paper discusses a number of ways in which research might make a more effective contribution to policy development. Improvements could be made to prioritization mechanisms and to the linkages between research and policy. Consideration must be given to the role of the State in the financing and delivery of research. Research capacity must then be developed in an appropriate fashion.

Some suggestions are also made as to how the performance of the research system itself might be improved. Precise research needs will depend on specific contexts and should flow from the prioritization mechanisms. However, some common problems where SIFAR research support would be useful are discussed, in particular the need for improved fishery management systems and the need to focus research at the correct objectives by distinguishing between goals and constraints.

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1. Introduction

Research has the potential to enhance the benefits associated with fisheries exploitation in a number of ways. The challenge is to ensure that this potential is realised to its fullest possible extent. This paper discusses some key issues that must be addressed in order to increase the effectiveness of research. These issues are:

- Improvement of prioritisation mechanisms
- Improvement of linkages between research and policy
- Clarification of the role of the State: separate financing and delivery
- Development of appropriate research capacity
- Improvement of the performance of the research system

Precise research needs will depend on specific contexts and should flow from the-prioritisation mechanism. It is however possible to identify some common problems where SIFAR research support would be useful and the paper concludes by looking briefly at the following:

- The need for improved fishery management systems
- The need to target correct objectives: distinguishing between goals and constraints.

2. Prioritisation

The basic issue is relatively simple to express, although not necessarily to resolve. Both research personnel and institutional resources are scarce. Like other scarce economic resources, their efficient allocation provides the opportunity to increase social welfare. The major problem is how to ensure that such resources are efficiently allocated. This section focuses on an approach or mechanism through which research prioritisation may be more effectively achieved. This issue is discussed much more extensively in Cunningham and Holland (1997).

Benyon et al. (1998, Table 8.1, p.127) identify priority setting as the most important element in improving the cost effectiveness and efficiency of research expenditure. In second place, they rank making services more user-oriented. However the latter is considered likely to have negative distributional consequences whereas priority setting is potentially positive, depending on policies adopted in particular circumstances.

2.1 The need for an economic surplus approach

A number of methods exist for prioritisation. (For a review see Fox (1987).) Among the most commonly utilised are congruence, scoring, and benefit-cost analysis. Many of these methods have been developed, or have found application, in the case of agriculture. It is important to recognise, however, that the problems facing capture fisheries, and extensive aquaculture (especially the sea-ranching kind of activity), are different to those facing agriculture. By far the most important difference is that whereas in agriculture the principal focus of research is on increasing output, in fisheries output faces a more-or-less immovable natural constraint.

The root causes of many of the problems facing the fisheries sector are to be found first, in free and open access systems and second, in a concentration on physical production as a measure of success both of policy and of research. As the limits to global fish production have been reached, the point has become increasingly obvious that the benefits from fishery exploitation and from fishery research are to be found in increasing the net value, and not the quantity of production.

In order to be able to increase net value, it is necessary to identify those factors which comprise it. The value of fisheries output comprises three elements: consumer surplus, producer surplus and resource

rent. The following three paragraphs discuss the ideas underlying these concepts but since economists have spent centuries refining them it will be appreciated that only the barest of outlines can be presented.

When purchasing any product or service, most consumers would be prepared to pay a higher price than they in fact pay. The difference between the price they are prepared to pay and the price they do pay is called “consumer surplus”. The sum of consumer surplus across all consumers of the good is a measure of the value to consumers of the item purchased.

In a similar vein, most producers would be prepared to supply the good at a price below the current market price. The difference between the minimum price that they would accept and the price that they receive is called “producer surplus”. The sum of producer surplus across all producers measures the value to producers of the item produced.

Finally, a fish resource has an inherent value in the sense that if it were privately owned people would be prepared to pay some amount to use it. This amount is called the “resource rent”, and is similar to the rent paid by the user to the owner for the use of a house. Because in free and open access fisheries nobody collects the resource rent, it is added to the profits of the users of the resource and is the main factor inciting overexploitation and overcapitalisation.

The combination of consumer surplus, producer surplus and resource rent is called economic surplus. In aiming to increase the value of production, policy-makers must necessarily focus on economic surplus. Sustainable economic surplus measures the contribution that the fishery is capable of making to economic and social welfare. It is analogous to the contribution made by other natural resources, such as oil fields or ore-deposits, with the added advantage that it is based on a renewable resource.

The maximisation of social welfare (economic surplus) is a conventional economic goal in resource allocation. It focuses on the efficiency with which resources are used. However, policy makers may pursue other goals, such as to improve the distribution of wealth across society (equity goals) or ensure the long term sustainability of resource systems for use by future generations (sustainability goals). The existence of multiple goals complicates fisheries management requiring managers to trade-off between conflicting goals. It is important to recognise, however, that the ability to achieve these other goals depends on realising the wealth-creating potential of the fishery. Management systems which have tended to ignore the economic surplus associated with the resource in pursuit of other goals have generally performed poorly.

In heavily overexploited fisheries, it is common to find that all three elements of economic surplus are well below their potential levels so that the contribution that fisheries exploitation makes to economic and social welfare is much less than it could have been. First, in free and open access fisheries (and also in poorly-managed fisheries) resource rent will be pushed to zero. Second, if equilibrium output falls as fishing effort expands beyond the maximum sustainable yield (MSY) then both consumer and producer surplus will also fall - in other words, the perverse situation is reached that the more fishing effort expands, the less both consumers and producers derive from it.

Because of the key role played by economic surplus in fisheries exploitation, it is proposed here that research prioritisation should, at least initially, use an economic-surplus approach. This conclusion is reinforced by the broadly-agreed view that research is an ineffective method of achieving non-efficiency goals compared to other measures that are available. As Alston et al. (1995, p. 81) note, “research is a very blunt instrument for the pursuits of objectives other than economic efficiency”. If, therefore, research expenditure is to be targeted at income or wealth distribution goals, other methods of achieving such an aim should be evaluated alongside research.

2.2 Advantages of an economic surplus approach

An economic surplus approach has a number of advantages.

First, it may help to correct some common misperceptions of the economic role that fisheries can play.

For instance, the idea is frequently encountered that benefits from fishing are principally related to the activity of fishing. This idea leads to views that in order to ensure that everyone benefits from fish resources, conditions of access must be open. As a result, the fisheries sector frequently plays the role of employer of last resort. However, as is also becoming frequent, the result tends to be an overexploitation of the resource base resulting in fewer long-term jobs - i.e. sustainability must be defined not only with respect to the fish resource but also to the jobs that depend upon it.

Economic surplus requires instead looking at the value of the resource base. Correctly managed, the resource base can furnish a sustainable supply of capital to a community (defined at the level appropriate to the resource under consideration). The community can then invest this capital to the benefit of all citizens and hence all can benefit without necessarily having to be fishers. This approach requires that the sustainable wealth of the resource is first generated and then equitably distributed.

A second advantage of economic surplus is that it provides a common standard against which to compare very different kinds of research so that, for instance, the potential value of cross-cutting research can be estimated, something which can be difficult under other systems. It is forward-looking, focussing on the expected value, rather than the quantity, of output. This may help to correct the concentration on physical production, which has characterised the assessment of fishery performance in many countries. It may also play a role in ensuring that policy-makers and researchers have a similar (if not the same) view of what the researchable problem is about. Tabor and Faber (1998) argue that in agriculture a key problem is that researchers tend to start their analysis of a problem with the natural resource whereas policymakers tend to begin from the social consequences. The result is that solutions developed by scientists may have limited appeal to policy-makers and problems identified by policy-makers may be considered to require little research by scientists. It seems unlikely that this problem of perspective can ever be fully resolved but at least economic surplus would give some commonality.

A third, very important, advantage of economic surplus is that it takes into consideration unpriced resources, that is resources for which markets do not exist. The problem of unpriced resources shows the strength of an approach based on economic surplus compared to, say, congruence methods. The fact that a resource is not traded in a market and hence has no price does not mean that it is of no value. One has only to think of the value of fresh air to realise this. For various reasons, markets may be missing or imperfect for important resources - in the case of fishing the most obvious example is usually the fish stock itself, but the same is generally also true of many supporting resources such as mangroves, coral reefs, seagrass beds, mudflats, beaches and so on. A congruence-based approach which concentrates on the value, or worse quantity, of output will miss some important aspects of resource use because it will not be able to take into account unpriced resources. An economic surplus approach on the other hand can include such resources along with those traded in markets. Various techniques exist to estimate the potential economic surplus of unpriced resources and hence to improve the allocation of research funds to different programme areas.

Even in agriculture, where markets are much more generally present than in the case of fisheries, the use of congruence-based methods has arguably led to too much research on increasing the physical output of different commodities and too little on important unpriced resources, such as the capacity of the environment to assimilate agricultural waste. In fisheries, the failure to use an economic-surplus approach is likely to result in even more serious distortions in research expenditure, and thence in policy.

2.3 The benefits of research in economic surplus terms

In order to identify what research should do, it is essential to clarify what it is possible for research to do. Some of the disappointment with the progress of fisheries research seems based on unrealistic expectations as to what such research could be expected to achieve. In particular, there seems to be a tendency to try to use research as a substitute for policy where difficult decisions are required.

In terms of economic surplus, the main contribution of research lies in the more efficient utilisation of scarce resources, in four main ways:

First, research can help to capture potential resource rents. The kind of research that is likely to be relevant in this context is policy-related, in particular the consideration of management instruments and compliance with regulations. Changes in consumer and producer surplus will also have to be considered.

Second, the more classic kind of research involves enhancing potential resource rent. Rent enhancement comes either through research-induced cost reductions or revenue increases. However, such research will only be beneficial if management systems are adequate. This kind of research will also affect consumer and producer surplus.

Third, research may help to ensure the sustainability of the economic surplus. Relevant research could include for instance preventing habitat degradation, understanding environmental variability and so on.

Fourth, research could play a role in ensuring a preferred distribution of economic surplus. However, the surplus has to be captured before it can be distributed and mechanisms exist for achieving such goals other than biasing research expenditure.

2.4 *Prioritisation as a process*

It is important to recognise also that research prioritisation is a process. In this process, it would seem appropriate to borrow from the satisfying principles which are common in the management of large companies. That is, rather than trying to define and achieve some elusive "optimum", it may be preferable approach the optimum by defining and achieving a satisfactory outcome during each round, and gradually refining the definition of "satisfactory".

A number of desirable features of a prioritisation process can be identified. First, it should be transparent with details of the prioritisation mechanism published, so that the various stakeholders, including researchers themselves, can identify with the process. Second, the process should have continuity, with progress reviewed and priorities revised regularly. Third, the process should be cumulative, with each prioritisation exercise, and hence each research funding round, building on previous results.

The prioritisation process needs to be on-going. Prioritisation should not be seen as a one-off event to be undertaken every X years. Rather it must remain sensitive to changes in both research and policy. Major changes in either area may alter research needs and priorities, and the system must be able to respond to this.

2.5 *Scope of the prioritisation process*

An important decision is where in the research/policy system the prioritisation mechanism should be placed. The answer will depend on the precise institutional arrangements existing in different contexts, but it is clearly important that the mechanism is located at a high enough level in the system. In many cases this means that it should be located at ministry level.

Prioritisation will be required at a number of levels. At the ministry level, one approach would be to organise research priorities on a fishery basis. Each fishery could be assessed according to its economic surplus potential, and the research budget allocated proportionately. This approach may be difficult at first because information will probably be lacking. In some cases, there may not even be agreement on the correct definition of various fisheries in the country. One external benefit of adopting an economic surplus approach to research prioritisation will be the generation of information enabling a more precise definition of fishery management units and an evaluation of the economic importance of each unit. This information can be expected to improve gradually over time and should be of great interest to policy-makers. Information is not the only difficulty however. There will also be a need to distinguish between

research and policy factors underlying the failure to extract potential economic surplus. A difficult subjective assessment will be needed as to whether the ministry has enough information to develop policies in the case of a particular fishery.

Once budgets have been allocated to fisheries, important research topics can be identified. In keeping with the overall thrust suggested here, the key issue will be to identify problems which are preventing the fishery from making the impact in terms of economic surplus *of* which it is capable.

Finally, at the project level, project proposals should be evaluated using the same economic surplus criterion (see section 2.6 below).

Cross-cutting issues can be identified at both topic and project levels.

A decision will have to be made concerning the proportion of the research budget which is to be prioritised. There are a number of reasons why it might be wise not to attempt to prioritise the whole budget. First, in the case of some speculative research, it may be very difficult, if not impossible, to derive a realistic estimate of its expected impact on economic surplus. Such research may aim simply to find out if something can be done, without necessarily worrying about the potential benefits of doing it. If the research is successful, others may or may not see economic or social benefits from applying it. Care must be taken not to over-prioritise the research system so as not to eliminate such curiosity-driven research, which may be extremely important in generating researchable issues in the future. A second reason for maintaining some non-prioritised research is as a control on the prioritisation system. Prioritisation is not a costless exercise and in evaluating such systems, comparison with the results obtained from non-prioritised research will provide an important test. Depending on the results obtained, a greater or smaller proportion of research might be prioritised in the future.

2.6 *Role of researchers in the prioritisation process*

Another question that must be addressed is the role that researchers play in the prioritisation process. In many cases, the role seems too great since research prioritisation exercises often begin (and end) with an audit of the problems as perceived by the researchers and the research institute. Allowing researchers to set priorities has some advantages (e.g. the researchers tend to be close to the problem and can identify research needs) but it probably has more disadvantages. Unless it is part of a clear framework, it makes it difficult to achieve a coherent overall strategy. It also tends to mean that similar (or the same) research programmes continue over very long periods and significant changes in the direction of research become very difficult, if not impossible, to achieve.

In submitting research proposals, researchers should be asked to indicate the expected contribution of the research to sustainable economic surplus. They should also assess the probability of research success, both in deriving useful results and in seeing them implemented in policy. Great care needs to be taken however as to how this is done. Obliging researchers to evaluate their own research in economic terms will of course provide an incentive to exaggerate the likely benefits and minimise the likely costs. Project evaluation will have to deal with this issue, but at least the evaluators will know where the researchers expect to make their contribution and the realism and importance can be assessed. There will also be an issue of credibility for the researchers themselves, particularly concerning future funding exercises.

2.7 *Time scale*

It must be recognised that the power of the status quo will make it difficult to alter, at least quickly, the way in which research priorities are set. Research planning rarely, if ever, starts with a blank sheet. Research institutes exist with their staff. They have programmes underway, with budgets linked to these programmes. Past decisions will dictate which researchers are currently employed and the kind *of* work that they are doing. For these reasons, care will have to be taken to ensure that prioritisation does not

simply become an exercise to justify continued work on existing themes. On the other hand, care must also be taken if large-scale changes in research priorities are to be introduced since there may well be negative consequences for both current and future research (in the latter case via the impact that increased uncertainty might have on future recruitment to the profession). The problem is the more serious, the longer a research institute or programme has been running and will be especially acute where budgets are limited or declining since the introduction of new areas of research will necessarily imply the suspension of others. Some re-training may be possible but there is clearly no guarantee that the existing researcher profile will be adequate for whatever new conditions exist.

It would be naive to think, therefore, that research programmes could be quickly changed in a fundamental way. Continuity must be allowed for. It is for this reason that it will probably take ten years or more for the results of a major change of emphasis to appear. The inertia tends to be such that full-scale research prioritisation may not be required, merely ideas of changes in direction. Before committing itself to carrying out a full prioritisation exercise, an institution (research institute, funding body or even government) needs to be clear about its ability to change things in accordance with the prioritisation findings. Otherwise it could easily become a very negative exercise simply demonstrating that what is currently being done is incorrectly focussed but that nothing much can be done about it, demotivating researchers and undermining confidence in the research process.

2.8 The importance of efficient fishery management

The main argument of this section is that, due to the characteristics of fisheries, research prioritisation should be developed around the principle of economic surplus. It must be recognised, however, that such an approach can only increase the *potential* benefits from research.

If the sustainable economic surplus potential of the world's fisheries, and the benefits from fisheries research, are to be *realised*, then efficient fisheries management is essential. The failure to develop such management systems has resulted in a situation where the resource rent dimension of economic surplus has driven more and more of the world's fisheries to overexploitation. Successful fishery research, especially of the cost-cutting variety, will simply make this problem worse in poorly-managed fisheries.

3. Interface between Research and Policy-making

If research is to influence policy then simply undertaking the research may not be sufficient. Some investment will be required in transmitting knowledge as well as in perfecting it. As Laurec (1989, p. 12) argues “*raffiner année après année les conclusions, en déplorant rituellement qu'elles ne soient pas utilisées traduit un manque de réflexion sur le rôle de la recherche et son insertion dans le système social et économique*” (to refine conclusions year after year whilst ritually deploring the fact that they are not used is indicative of a lack of thought about the role of research and its position in the social and economic system).

One problem facing fisheries researchers, at least under the institutional arrangements currently common in fisheries, is that research frequently highlights the need to limit production and make difficult political choices between users. Such research is not likely to be very welcome politically, especially because increases in production are so often used as a measure of the success of fishery policy. There is a need, where possible, to present research results in a politically-acceptable manner, including research on how to implement essential, but potentially unpopular, policies in ways that do not amount to political suicide. There is also a need for research to contribute to changing society's views as to the form in which benefits are derived from fisheries. If the success indicator can be changed from output to value then there is no reason why it cannot increase monotonically without endangering the resource base.

Tabor and Faber (1998) consider in detail the problem of moving from research to policy change. Drawing on a number of case studies, they present some general conclusions. Experience in Ghana shows that

“involving policy makers in setting the research agenda for Governmental research bodies has proven to be especially effective in feeding policy reform. Bringing research results to the policy makers (rather than bringing policy makers to research funding presentations), through seminars and discussions tailormade for the policy makers, has also proven effective” (Chapter 1, p. 3 of internet version). This conclusion reinforces the point made earlier concerning the need for a research prioritisation process to be undertaken at the appropriate points in the policy/research system.

Agricultural research experience in Kenya is also illuminating. The Kenya Agricultural Research Institute (KARI) produces policy briefs, which are distillations of research results for the attention of policy makers. Although supportive of this approach, Tabor and Faber (1998, Chapter 1, p. 3) advocate that it may need to be reinforced with “policy advocacy efforts”. However, this seems a dangerous course for researchers to follow. It seems preferable to follow the Ghanaian strategy if possible. Identifying researchers too closely with policy has a number of potentially adverse consequences for the future, particularly concerning the perceived impartiality (or not) of research. There is also a clear risk that researchers will be identified with policy failures which have nothing to do with research. (In fact a little later Tabor and Faber give the example of the Philippines where researchers are now being sued when their results “adversely effect (sic) corporate interests” (ibid, p. 4)). Researchers also have only a partial view of the policy problem. It is the role of the Government to integrate wider considerations into policy development.

From the various case-studies, Tabor and Faber attempt to formulate some key recommendations for developing an effective interface between research and policy change. This is clearly a difficult problem and much more work will be required. Successful solutions will depend mainly on local conditions so it is not surprising that drawing out general principles is a tricky process. However, only some of their recommendations relate specifically to the interface. Others are more general applying either to research or to its management. Rather than presenting the recommendations in the same way as Tabor and Faber, the section below attempts to organise them in terms of their focus.

Interface

The following recommendations relate specifically to the idea of an interface between research and policy change. The first group relate to the research system itself whereas the second relate to the policymaking users of research results.

- *Research leaders need to understand policy change. process.*
- *Research results need to be presented in a friendly form to policy makers.*
- *Research institutes should be located in capital city because distance is a barrier to communication.* This hypothesis needs verification on a case-by-case basis. It seems an extreme step, and counter examples can certainly be found where distance does not seem to have been a problem - e.g. the Lowestoft fisheries laboratory in the UK. There must also be some danger that geographical proximity may create too cosy a relationship between the Ministry and its institute.
- *Research institutes should ensure that they have an effective relationship with the media as a way of disseminating their messages.*
- *Research needs to forge links with appropriate NGOs since these often have a good idea of what research is required to push policy forward.* The choice of appropriate NGOs may be tricky, especially where they are in conflict with the Government.
- *NGOs themselves may be important catalysts for change., building appropriate networks and identifying the kind of reform needed.*
- *Involve policy makers early on in setting the research agenda so as to improve their understanding of the problem and their commitment to finding and implementing a solution.*

This is probably the single most important point. The priority-setting process is one of the key elements in the interface between research and policy provided that it is set high enough in the system.

- *Scientists should be seconded to policy-making bodies as a way of building institutional bridges.* Note however that this recommendation does not remove the need for Ministries etc to have their own "translation units" - discussed further below. In fact such units would be the obvious home for seconded scientists, as well as for the body responsible for the prioritisation mechanism.
- *Involve scientists in regulatory oversight bodies.* This may increase the impact of research but care needs to be taken that it does not politicise researchers.
- *Politicians should use scientists in their advisory circle.* Again, the risk of politicisation is clear.
- *Policy-makers should experiment, to develop new ways to bring research (and the community in general) into the policy-reform process.*

Research quality

Top of the list of recommendations put forward by Tabor and Faber is the need for research quality.

- *Importance of "good science": scientific process must be professional, credible, honest, independent, without political interference.*

Although this is a crucial point, it clearly does not relate specifically or even mainly to the interface problem.

Research management

A number of recommendations seem to relate more to the way in which research is managed than to the interface itself.

- *"Research that focuses on problems that are urgent and have a clear, devastating impact on particular communities are most likely to attract the attention of policy makers"*

For this recommendation to work, a number of things are required of the way in which research is organised. First, the priority-setting process and/or the research community have to predict future problems because it will usually be too late to do much research once the problem has reached a level where it has a devastating impact. Second, there needs to be some assessment of the potential payoff to research. Urgent and devastating problems do not necessarily have a high research payoff even if they attract the most attention from policy-makers. Third, if there are a series of urgent problems some mechanism will be required to prioritise them and choose between them if research funds are limited. Fourth, the idea has overtones of research fire-fighting. It may be impossible for research to avoid some of this but it should not dictate the whole agenda.

- *Need to ensure that incentives for researchers to wish to influence policy.*
- *Scientists should see policy makers as a user group of research results.*

These two recommendations relate to the general issue of motivation and incentives facing researchers (see section 8 below). Of themselves they do nothing to put into place an interface between research and policy, although they are likely to increase researcher demands for such an interface.

- *Opportunism on the part of the research community. Be ready for unexpected opportunities to influence policy.*

Once again the recommendation is worthy but does not seem to relate specifically to the interface idea. It has often been said that the role of research is to prepare for the next crisis since, it is only in times of crisis that real progress can be made, especially in matters of institutional reform where a strong status quo often has to be overcome. As John F Kennedy pointed out (April 12, 1959), "when written in Chinese, the word 'crisis' is composed of two characters - one represents danger the other represents opportunity."

- *Protection for scientific whistle blowers who reveal weaknesses or problems in the system.*

This is a very difficult issue. The rationale underlying such a recommendation is easy to understand. However, if the interface between research and policy comes to depend on legal provisions such as this then urgent attention *would* have to be paid to the whole process.

One general comment is that these recommendations seem to place much of the onus on the research community. However, given that public funds are being spent on research, more might be expected of the policy-making process itself to move towards research.

One way to do this would be to establish what might be called Research Liaison Units. Probably it would be best to locate these in the relevant ministries. They would have a number of functions. Most importantly, they would prepare, in collaboration with research institutes, policy briefs as "translations" of research results. They would also house the body responsible for research prioritisation. The membership of these Units should be given careful thought. The idea of basing them in ministries would be to try to increase their potential policy impact and hence the policy impact of research. At the same time, they should not be seen as purely ministerial bodies but should involve representatives of different stakeholders, particularly when researchable problems must be identified.

The establishment of such units may help to resolve the problem that too frequently policy questions are simply pushed down the line to the research centre for "advice". This problem is far from affecting only developing countries but appears to be endemic wherever there exists a sectoral nationally-funded research institute which depends on the Fisheries Ministry.

If a Research Liaison Unit is established, it needs to have a sufficiently high profile so that researchers operating outside of the sectoral research institutes and with an interest in seeing their research results applied to policy feel motivated to communicate their results to the Unit. In other words, the Unit should provide the principal entry point to the policy process. This would place the Unit in an important and potentially powerful position so careful attention would need to be paid to its management: to whom does it report and how is its performance to be assessed and monitored?

Consideration could be given also to the use of a contractual relationship between this unit and research deliverers, including centrally-funded institutions. Some decisions will have to be taken about the level of base-funding that should be given to a centrally-funded institute, with contractual funding making up the rest. There is no reason why all research should be put through the central research institute; it is unrealistic to expect it to have all the necessary expertise. Use should be made of Universities etc., locally and also of foreign institutes. The management of research undertaken by foreigners will require careful consideration, especially concerning data archives, and the ministry unit could also be responsible for this.

Special attention may have to be paid to the relationship between the ministry and public sector research institutes. Research needs to anticipate medium to long term problems but it is difficult to do this if the ministry continuously uses public sector research institutes for short-term technical advice because its own technical divisions are underfunded. It might be argued that short-term advice is one output of previous medium-to-long-term research plans but clearly if the quantity of advice solicited becomes too great, short term considerations come to swamp the medium to long term. Moreover, there is a tendency to assign the best personnel to dealing with these short-term problems. There is no simple solution to this problem since both the ministry and its research institutes are subject to short-term pressures. However,

seeking the funding of public-sector research institutes by the private sector could easily make matters worse since the questions facing the private sector are likely to be dominated by short-term considerations, especially in overexploited fisheries. An obvious way forward in theory is to set up a unit designed specifically to deal with issues of diffusion of research results and advice, a kind of after-sales centre. The problem in practice is whether the Institute can effectively limit the size of this function.

If it is not possible to create a Research Liaison Unit within the Ministry, then the onus will (continue to) be placed on the research side, especially public sector research institutes. In this case, the institutes will have to ensure that they have the necessary capacity to develop a dialogue between research and policy.

4. The role of the State in the Funding and Delivery of Research

A key issue is: what are the appropriate roles of the public and private sectors in fishery research? There is no universal answer to this question.

Generally speaking there are strong arguments for the public sector to fund some research but the arguments tend to be weaker in favour of public sector delivery.

The arguments in favour of public funding for research rest on market failures of various kinds. Thirtle and Echeverria (1994) identify three major failures:

- inappropriability - research is likely to have public good characteristics (see Table 1) so that social gains exceed private gains leading to under-investment in research by the private sector
- uncertainty - since it is not possible to insure against uncertain events, risk-averse firms will tend to under-invest in risky research
- indivisibilities - increasing returns to scale may lead to monopoly power, with the classic result that output (of research) is less than socially optimal.

Table 1 presents the standard classification of goods used by economists. Knowledge is non-subtractable: whether it is in the public good category or the toll good category depends on excludability. This in turn depends on institutional arrangements which differ from country to country. The private sector can be expected to attempt to develop brand loyalty as a way of retaining research benefits but the most important factor is likely to be intellectual property right (IPR) legislation. It is important to note that goods and services can change categories as institutional arrangements change-for instance, fisheries in developing countries that were regulated under traditional management systems were effectively private goods (albeit communally managed). With the breakdown of traditional management they have often moved to the common pool category. On the other hand, moves to implement individual quota systems tend to move fisheries towards the private goods category.

Table 1. The economic classification of goods

	Excludable	Non-excludable (cannot prevent free-riding)
Subtractable (or rival)	Private goods	Common-pool good (e.g. unregulated fish stocks)
Non-subtractable Consumption by one person does not change total available for others	Toll goods (e.g. private roads)	Public goods (e.g. street lighting)

The more research is basic, defined by Thirtle and Echeverria (1994, p. 32) as "the disinterested pursuit of scientific knowledge without a specific technological objective in view", then the more private research is expected to be inadequate, and the more public support will be required. This theoretical case for

public funding is supported by empirical evidence showing very high rates of return to such research investments.

Similar conclusions are reached by Alston *et al.* (1995, p. 12) who suggest that where the objective is to correct a market failure, the public sector should focus its support more heavily on research that has a high social payoff but which the private sector has relatively little incentive to support, for example, socio-economic research or base-line research where the benefits may be particularly difficult to estimate and where it may be very difficult to prevent free-riding. However, they also note that government intervention in the provision of R&D should not crowd out private investment, giving three alternative conditions which must apply before government intervention should occur:

- (i.) that incentives are such that markets fail to produce socially optimal amount of research or
- (ii.) that economies of size and scope in research threaten the competitive structure of markets or
- (iii.) opportunities exist for exploiting the 'complementarities' between research, education and extension.

They suggest that these conditions are more likely to apply in developing countries than others and hence the argument for government intervention in the provision of research is likely to be stronger.

Along similar lines, Troadec and Christy (1990) argue that the private sector can undertake technological advance but relies on the public sector for research relating to institutional change. This division is not hard and fast, sometimes for example because of risk, public sector funding of technological research may be justified, and sometimes the private sector may invest in institutional research, if it promises profitable changes for them. There may also be infant industry arguments for public research funding in developing countries.

Even if there are some strong arguments in favour of publicly-funded research, the issue of research funding is increasingly under the spotlight. In discussing agriculture, particularly in sub-Saharan Africa, Benyon *et al.* (1998, p. 1) write “research and extension (R&E) systems ...have typically been dominated by donor-dependent centralised public sector institutions widely perceived to be underfunded and underperforming”. Much the same is true of fisheries research.

They identify three reasons why there is now growing interest in finding alternative ways of financing and delivering R&E:

1. “The global reappraisal of the role of the state and (the) associated ideological shift towards economic efficiency and market-based solutions for resource allocation and service provision” (*ibid.*).
2. Current policies of structural adjustment and fiscal restraint make it unlikely that resources can be increased.
3. A general perception that R&E has failed to deliver successful innovations in African agricultural technology systems.

The first two reasons certainly apply also to fisheries. The third has to be interpreted rather differently in fisheries, at least in the case of capture fisheries, where the failure has been to deliver exploitation and management systems capable of generating fisheries wealth on a sustainable basis. If anything, successful technological innovations have been rather too frequent in fisheries in situations where adequate management institutions were either absent or deficient.

Benyon *et al.* (1998, p. 2) also highlight the difference between financing and delivery of research. This crucial distinction is frequently overlooked or ignored because States seek to achieve multiple objectives through research. In particular, there is a tendency to want to achieve high-level research and training simultaneously but these objectives often conflict in practice with the result that neither is achieved as effectively as it might be.

Because they focus on financing, Benyon *et al.* identify a 2 x 2 matrix to describe the financing and delivery issue. This matrix is presented in Table 2 below.

Table 2. A model of research financing and delivery (developed from Benyon *et al.*, 1998)

Delivery	Public sector	Private sector
Financing		
Public sector	1	3
Private sector	2	4

Traditionally, cell 1 has dominated, especially where the public sector is funding its own research institutes (e.g. a Fisheries Ministry funding a research institute under its control). In terms of developing away from cell 1, there seems to be much interest in cell 2 where public sector research institutes can (and are increasingly expected to) fund themselves directly through contracts with the private sector. One potential drawback of this approach in terms of the concentration on the short-term has already been mentioned.

When considering delivery of research, it may be useful to develop the above matrix somewhat (Table 3). For instance there is frequently an important difference between domestic and foreign research institutes.

Table 3. A modified model of research financing and delivery

		Delivery		Domestic		Foreign	
		Public	Private	Public	Private		
Domestic	Public	1	5	9	13		
	Private	2	6	10	14		
Foreign	Public	3	7	11	15		
	Private	4	8	12	16		

Traditionally, research financing and delivery has been dominated by cells 1 and 3. However, Table 3 makes it clear that there are many options that could be considered. It seems that this domination arises at least in part from the mixing of objectives: research and training (or development). However, research is a tradable service just like any other and the law of comparative advantage applies just as strongly. It may well make more sense for a country to consider cells 9, 11, 13 and 15 where the quality of the research output is the only consideration. Issues of training and development may be better dealt with as such.

Table 3 could easily be developed further for example to distinguish between public sector research institutes and universities. Research planners may well find it worth while to develop a table such as this looking first at the relative amounts of funding going to different kinds of institutions and second comparing this with the research payoff. Budget allocations could be altered according to the marginal efficiencies of research investments, although account would also have to be taken of the abilities of different institutions to undertake different kinds of work

5. Research Capacity

Once the issues of funding and delivery of research have been separated, questions concerning research capacity naturally arise. In particular, who (public and/or private sector) should develop such capacity and to what extent?

The research capacity of developing countries is substantially less than that of developed countries. As a consequence, it generally seems to be taken as self-evident that it is in the interests of developing countries to invest in research capacity. World Bank et al. (1992) gave a high priority in fisheries research to the strengthening of national research capacity in developing countries and the Asian Fisheries Society Special Publication Number 7 (Davy, 1993) deals extensively with this issue.

However, a first question is whether a particular country should undertake its own research at all in a given area. If a publicly-funded priority research area were identified for which no local expertise existed, then there would be the following options:

- It could be decided that the research simply cannot be done.
- It could be decided that the research cannot be done at present and that investments should be made in research capacity to ensure that it can be done at some point in the future.
- It could be decided to import the required research services.
- A combination of options 2 and 3 could be used whereby expertise is imported to deal with the immediate problem whilst providing some training in order to develop local expertise. Many projects involving international organisations have this element of "technology transfer", although how successful they are is a matter of debate (see for instance Schultz, 1980b, who argues that western experts are an expensive drain on research resources).

The important point, however, is that, if it is accepted that research expertise is like any other good or service, a country could recognise that it did not have a comparative advantage in this area and simply import its requirements. Given the large amount of such expertise available in developed countries and increasingly in developing countries, it should be possible to import at reasonable cost. This approach may make much more economic sense than attempting to develop capacity, especially in situations where governments do not have the funds to provide the infrastructure to support research skills once they have been developed. If governments do decide to develop national research expertise then there is a need for them to be committed to the activity in the long run if the investments made are to pay off.

There are a number of reasons why a government may prefer to develop its own research expertise rather than rely on that which is available through the world market. One reason may be strategic: that the potential research results are of sufficient national importance that it would not be sensible to allow foreign interests to derive them.

There may also be an argument in favour of government intervention in the development of local research skills in terms of domestic market failure (e.g. public or merit good arguments). Research may be important both as a source of skilled people for the economy as a whole and as a source of employment for skilled people preventing a leakage of such skills from the economy. Clearly if this is to work the package available locally must compete with that available internationally, otherwise a "brain drain" is likely to occur and the country will have spent some of its scarce resources to train labour for developed economies. One practical difficulty is that many developing countries are unable to justify the high salaries necessary locally to retain staff and hence the best researchers often drift abroad or into other sectors of the economy. Where it is the latter, there is at least a gain to the economy as a whole.

There may also be infant industry arguments in that the developing countries could have a dynamic comparative advantage in research (or at least some dimensions of it) yet be uncompetitive simply because they have a time lag compared to developed countries. As a result developing country governments could justify favouring their institutes in the allocation of research contracts. However, this argument would not be sufficient to justify public institutes since private research institutes could be equally favoured.

There may be important non-economic arguments. Research may be seen as an index of economic and social development and the view may be taken that it is right that countries should undertake this activity for themselves rather than relying on others to do it for them. Whether this argument makes sense in

particular contexts will have to be decided by individual governments, although it is clear that no country is going to be able to fund all the research that it requires domestically. Rather, research takes place in an international context. Perhaps the view from the developing countries is simply that they should be participants in this context rather than customers of it.

In developing a rational policy response, it is well to keep in mind the fundamental rule that policy measures should aim directly, or as closely as possible, at the source of the problem. This would tend to mean that research priorities should be set and executed as efficiently as possible and that developing research capacity should be treated as a second independent problem.

If a decision is taken to develop public sector research institutes then the government will also have to take a series of decisions about investments that it is to make in such institutes. Such decisions will include size (do they have a critical mass?), funding (do they have sufficient funding to operate effectively?), employment conditions (do researchers have security and incentives to function effectively?) and discipline coverage (will the necessary expertise be available in the future?). A research system based on companies and universities tendering for work would obviate the government from the need to make such decisions.

In conclusion, in the discussion of research in developing countries, emphasis has often been put on the development of local capacity. The economic arguments in favour of such an approach are not clear. There does not appear to be any real link between public funding of research and the development of research capacity. Where such a link arises, it seems to do so because an implicit assumption is made that local research capacity must be used. It is argued here that this assumption is specious and that research should be funded according to priority and ability to undertake the work. If developing country institutes have a comparative advantage they can be expected to emerge over time. If a decision has been taken to develop such institutes this can be implemented independently of research priorities (except, perhaps, where there are budgetary conflicts).

6. Co-ordination Needs

Where governments have decided to develop their own directly-funded research institutes, research performance can often be improved by increasing co-ordination between different institutes. The various institutes and other institutions producing research output (e.g. universities) tend to work independently and as if one another did not exist. Objectives and priorities are left to Directors of Research Centres or to individual scientists themselves. As a result, work programmes tend to be somewhat haphazard, sometimes corresponding well to the needs of the sector, but often reflecting personal scientific interest or funding availability. It is not uncommon to find therefore that the knowledge base is greatest for a relatively less valuable species. For instance, pelagic stocks present particular scientific challenges because of their great variability, and as a result many scientists have developed a research interest in such species. It is comparatively rare to find, however, that such species are the most valuable ones in a country's fishery portfolio. When the fisheries ministry comes to develop management plans, it may well find that relatively (or even absolutely) little is known about the most valuable species with the most pressing management needs.

Since both human and material research resources tend to be in short supply, a co-ordinated approach amongst the various research bodies is of the utmost importance to ensure that scarce resources are not wasted. There is a need to develop a short-to-medium term plan for research activity at the same time as the government is preparing its development/management plan for the fisheries and aquatic resource sector. Such a plan is likely to favour applied research, multi-disciplinary studies and co-ordinated inputs from different research bodies. There will be a need also to consider the long run development of research capacity.

In seeking to improve the performance of research institutes through increased collaboration and coordination, an important factor to consider is information exchange and the required infrastructural investment (especially libraries and new computer developments (e.g. internet)).

7. Improving Performance of Research Institutes themselves

Putting into place a coherent research prioritisation process will improve the performance of the research system. Developing a more adequate interface between research and policy-makers should help to increase the policy impact of research. The next question that must be considered is what might be done to improve the performance of the research institutes themselves. This is a question that is difficult to answer at a general level; the answer depends on the particular circumstances of the research institute being considered.

Nonetheless, a recent evaluation of IPIMAR (Ministério da Ciência e da Tecnologia, 1997) identifies a number of measures to improve research performance that seem to have quite general applicability. The following suggestions are adapted from the report.

a) Promote influx of new ideas and approaches by:

- Recruiting young scientists of high calibre in areas that the Research Institute should develop
- Linking up and exchanging staff with appropriate national and foreign institutes
- Promoting publication of findings in journals
- Ensuring that staff can at least read English at an appropriate level given the importance of English language journals
- Establish networks to exchange reprints where journals are too expensive

b) Enhance capacity to formulate and conduct multi-disciplinary research by:

- Involving all scientists-in the elaboration and evaluation of the master programmes of the Research Institute
- Participating in national and international programmes and projects

c) Enhance systematic evaluation of research units, programmes and staff by:

- Making clear the evaluation criteria and linking these to the prioritisation process
- Use external evaluation systems for the Institute's programmes
- Maintain and develop contacts with relevant international scientific bodies.

However, there is perhaps a need to be careful not to generate unrealistic expectations of research institutes, particularly in developing countries. As Tollini (1998), has said of agriculture (and much the same seems true of fisheries) “Most agricultural research systems in developing countries today have only developed capabilities for very basic [research]...They rarely evaluate the economic impact of their experimental results. Yet these institutes are now increasingly being requested to deal with problems related to the sustainability of the production capacity and environmental conservation. They are called upon to focus more on food security, poverty alleviation and gender issues. They are asked to move upward in a scientific spiral when they have not been able to even close the loop at the base of the first spiral”.

8 Researchers: Incentives and Evaluation

Care needs to be taken to ensure that incentives faced by researchers do not run counter to the interests of research funders. If the primary goal is to influence public policy so as to contribute to sustainable economic development, then ideally researchers should face positive incentives which pull them in this direction. They definitely should not face incentives which push them in some other direction. Unfortunately, for many researchers the main incentive is to publish, preferably in class-1 journals, since this clearly enhances professional status. Changes in research prioritisation will have to take into account incentives at researcher level.

There is also the related problem of the evaluation of researchers. The difficulty has to be faced of distinguishing between good research which for some reason did not have the economic impact hoped

for and poor research. Moreover, researchers need to feel confident to be able to indicate when a particular line of work is not giving the desired results. This problem of reporting negative research results is important because of the various biases in the system in favour of only reporting positive results, most obviously in the publication of papers. In order to provide the right kind of incentives, clarification is needed concerning the evaluation criteria to be used both for research programmes and for researchers.

The previous sections have suggested some general ways in which the policy relevance of research might be improved. The remainder of the paper considers first some important research problems which seem to face many countries and second the role that research might play in addressing some significant problems.

9. Need for Research into Encompassing Management Systems

In many countries and in many publications the small-scale fisheries sector is treated as a world apart which must be managed separately from other fisheries and even from other activities in the coastal area. Management systems currently available may leave little choice but such an approach seems likely to place the small-scale sector at a disadvantage with respect to other sectors. Much research is currently focussed on the themes of co-management and especially community-based management. However, great care is required in deciding that locally-based management is a more appropriate response than centrally-based. Bardhan (1996, pp. 1354-5) argues that "one should resist the temptation to romanticise the value of the local community as a social and economic organisation... On the equity side, it is important to keep in mind that in situations of severe social and economic inequality at the local level, decentralisation can be highly inadequate in helping the poor. It may be easy for the local overlords to capture the local community institutions...". He goes on to argue that such capture may be more difficult at a national level whilst at the same time the poor may find it more economical to organise themselves nationally. Centralised policy co-ordination may therefore be more appropriate depending on the particular circumstances.

Focussing on economic surplus would suggest that, rather than considering who exploits the resource, policy should create conditions within which the best utilisation of the resource (from a social perspective) can emerge. Encompassing management systems are required wherein those who can exploit the resource at the lowest opportunity cost are able to do so. In developing countries, this may well mean the smallscale sector since, as pointed out by World Bank, (1992, p. 15), the sector has many advantages: it is labour-intensive, consumes less fuel, generally uses more selective gear and is less dependent on imported equipment and materials. The small-scale sector's capital is owned locally, often by the fishers themselves. And because the small-scale fishers depend on resources adjacent to their communities, they have a greater self interest than large-scale fishers in management of the fisheries. Sometimes however resource and market conditions may mean that the advantage lies with the larger-scale sector. What is important is that management systems exist such that it is the resource-users themselves who decide what is the appropriate scale of activity.

Although some such systems exist, it is questionable to what extent they could be used to develop management frameworks that are capable of encompassing both the industrial and small-scale sectors. Research is urgently required in this area. Given the likely widespread applicability of results, there would seem to be a strong case for international funding and co-ordination. If left to purely national funding this area of research is likely to be underfunded because the benefits from research in one country spilling over into other countries will be ignored. This kind of externality will not be taken into account in a national priority setting context and suggests an additional role for bilateral and international agencies.

10 Equity and Equality

Research is also required into how to design equitable management systems. In developed countries the most successful management systems seem to be those based around individual transferable quotas. In

designing such systems, many economists have taken the standard line, that the initial allocation of quotas is irrelevant on efficiency grounds. Provided that quotas are transferable, the market will quickly correct the initial allocation to ensure that the rights are exploited efficiently. There is now clear evidence that such management schemes do generate efficiency, but it is also clear that in the process vast amounts of, arguably national, wealth have simply been given to a few fortunate individuals who happened to be fishers at the right time. Such handouts raise questions of both intra- and inter-generational equity. What is most interesting however is that they now appear also to raise questions related to the efficiency of the management systems. Experience in Iceland, and elsewhere, suggests that as the general public come to realise the value of what has been given to the fishers, increasing disenchantment with the management system sets in. It appears therefore that equity is an important issue not only for its own sake but also because it may determine what are sustainably efficient outcomes.

In developing countries where fishery resources represent an important potential for capital accumulation, the issue of equity is likely to be of even greater importance in developing appropriate fishery policies. This area also requires much more research.

The issue of equity versus equality has always been important. An important dimension of the debate hinges on whether it is sufficient (assuming that it is possible) to achieve equality of opportunity or whether the aim should be equality of outcome. The debate is certainly not new: as long ago as the 4th century BC, Aristotle argued that “equality consists in the same treatment of similar persons”.

The worldwide development of national lotteries certainly indicates a general acceptance of inequality of outcome, at least under certain circumstances. For instance, in the UK national lottery (and in many such lotteries) there is effectively only one major prize: the jackpot which is in the order of \$10 million a week. Although a significant number of people denounce the lottery, the much more significant number who participate clearly reveal their preference for extreme inequality of outcome, presumably on the grounds that there is equality of opportunity.

The important point is that in developing efficient management systems, the equity implications will have to be considered at the outset. It will be extremely difficult to correct the situation once efficiency has been achieved.

11. Other Objectives as Constraints on Ability to Achieve Economic Surplus

It has been argued above that economic surplus is at the heart of the problems and the potential gains associated with fisheries exploitation, and that for this reason both policy and research should focus on this concept. However, it is clear that policy-makers can and do pursue objectives other than economic surplus. One way to look at these other objectives would be as constraints on the ability of the system to achieve economic surplus. This will give an idea of the cost to society of trying to achieve non-efficiency goals using particular policy instruments. The section below very briefly discusses three non-efficiency goals which are often very important in policy debates. It is recognised that the issues go far beyond what is discussed here. The aim is to focus on some important issues that need to be considered and which might be the topics of future research.

11.1 Gender issues

Williams and Nauen (1996) identify the important role played by women in West African fisheries, especially in trade, marketing and processing. Given that increasing net value-added is the main way forward in fisheries exploitation, this is clearly an important role. A number of researchable constraints can be identified, in particular the adequacy of capital markets. It may also be useful to carry out extension activities concerning fish handling and preservation, especially on the use of ice, and the importance of quality.

Given the importance of the above issues, they may well top the list in an economic-surplus based evaluation. However, even assuming that they did not, it would remain a legitimate policy choice to argue that equity issues are of such over-riding importance that research must focus on issues of relevance to women so as to improve their position and role within the fishery system. It must be recognised, however, that this could impose significant opportunity costs in terms of other research that will no longer be carried out. But perhaps the key question is whether a sustainable, long-term improvement can be expected. First, will the research results be adopted? And second, will women be able to hold on to the gains made? In more general terms, is it realistic to expect research to be able to run counter to broad socio-cultural factors that go beyond the fishery sector?

11.2 Poverty

Many users of fisheries resources in developing countries are relatively and absolutely poor. Development projects and agencies focus on the poor as a target beneficiary group but this raises many difficult questions.

A first question that must be addressed is whether a meaningful definition of poverty can be developed. Ravallion (1996) reviews attempts to measure poverty pointing out that the standard approach, the headcount index (the proportion of the population below some arbitrarily-defined poverty line), is seriously flawed, not least because if those below the poverty line become poorer, with no changes elsewhere, then the index will not change, even though it is intuitively clear that poverty has worsened. This flaw was noted by Sen (1976, quoted by Ravallion), yet the headcount index remains the most popular measure because, according to Ravallion, it has proven difficult to develop a simple alternative.

A second question is whether the poor will be able to retain any gains that they may make. It is necessary to investigate the extent to which any gains in productivity will be expropriated either through rents or through price changes. Bardhan (1996, p. 1348), makes a similar point when discussing attempts to provide subsidised credit for the poor, arguing that “wealthy borrowers have often appropriated the credit subsidies meant for the poor”.

A third question is how can fisheries policy best help the poor? Often there seems to be an assumption that the poor must be helped to become better exploiters of fisheries resources. However, the best way to help the poor may not be to help them to become better fishers, nor to encourage them to become fishers at all, but to use the wealth of the fishery to create alternative employment opportunities for them. In this view, poverty alleviation is considered to be a macroeconomic, and not a sectoral issue (save in the exceptional case of a very large fishery sector). Attempts to improve the incomes of the poor in the fisheries sector alone seemed doomed to failure because incomes in the fisheries sector are linked to those in the rest of the economy. If a policy successfully improves incomes in the fisheries sector, the relative attractiveness of this sector will increase, attracting more fishers and restoring the equilibrium position where opportunity incomes are earned. The new equilibrium can emerge via a combination of price and quantity effects. The problem is that quantity effects in particular are likely to run counter to conservation goals. Increased numbers of fishers will generally mean lower fish stock sizes.

Much emphasis is currently being put on the “sustainable livelihoods” approach as a means of achieving poverty eradication (see e.g. Carney, (1998)). Singh (in Carney, p. 205), emphasises that sustainable livelihoods is an approach and that it would not make sense to talk of sustainable livelihoods projects. Presumably therefore it is important to identify projects which enhance the features of sustainable livelihoods (Singh identifies five major ones: coping with shocks, economic efficiency in production, social equity, environmental soundness and ability to continue once support is withdrawn). It will be evident from what has been said above that the view taken in this paper is that research can most contribute to sustainable livelihoods via the impact that it has on the economic efficiency of production.

Attempting to alleviate poverty by focussing on the poorest groups suggests that the structure of income can be changed. Table 4 is interesting in this respect. It presents some (rather old and somewhat

controversial) data on income inequality across countries. What is noteworthy is that regardless of income level, from the least to the most developed countries, income distribution has a quite remarkable similarity. As measured by the Gini coefficient, it is apparent that income inequality initially gets worse as development proceeds but as higher income levels are reached the level of inequality falls (but is very far from removed). Similar evidence exists concerning the development of individual countries. One striking feature is the way in which income inequality is reduced: it is not by the poorest segment becoming wealthier, but by the very wealthy becoming poorer (relatively). In fact, the relative position of the poorest 20% of society is actually worse in the richest countries (5% of GDP) compared to the poorest countries (7% of GDP). Similarly the position of second quintile (21-40%) does not change significantly across the groups. Most of the gains go to the middle groups (41-80%). The group from 81-95% improve their position early on and then broadly maintain it.

It is important to note that all these comparisons are in terms of relative positions. There are important differences also in absolute levels: the very poor in the first group will be very poor indeed so that the fact that they receive relatively more than those in developed countries is likely to be of little comfort.

Given that income inequality is a pervasive feature across both time and space, it would appear to have an important economic role. It also looks likely to be extremely difficult to eradicate. The evidence presented in Table 4 suggests that programmes which concentrate on poverty alleviation, and especially on assisting the very poor, face a formidable challenge. .

Table 4. Average income shares by country group according to GDP per capita in 1965 dollars (from Thirlwall, 1994, pp.33-34)

Some evidence concerning income inequality							
GDP per cap	<20	21-40	41-60	61-80	81-95	96-100	Gini Coefficient
<100\$	7.0	10.0	13.1	19.4	21.4	29.1	0.419
\$101-\$200	5.3	8.6	12.0	17.5	31.6	24.9	0.468
\$201-\$300	4.8	8.0	11.3	18.1	25.7	32.0	0.499
\$301-\$500	4.5	7.9	12.3	18.0	27.4	30.0	0.494
\$501-\$1000	5.1	8.9	13.9	22.1	24.7	25.4	0.438
\$1001-\$2000	4.7	10.5	15.9	22.2	25.7	20.9	0.401
>\$2001	5.0	10.9	17.9	24.1	26.31	16.4	0.365

The apparent difficulty of changing the structure of income distribution would seem to emphasise the importance of raising the overall growth rate of the economy. In this context, once again, the fisheries sector can most contribute as a creator of wealth, which could, if desired, be invested to enhance the lot of the poor, most obviously through supply-side policies (funded by resource rents) such as education and training programmes for the poor. As Bardhan argues (1996, p. 1345, emphasis added) “in the more recent literature on poverty there is a better appreciation of the fact that market-driven economic growth need not be inconsistent with poverty alleviation. In many countries, in East and Southeast Asia in particular, fast outward-oriented growth has been associated with substantial reductions in mass poverty. In general, *the most important way for economic growth to help the poor is by expanding their opportunities for productive and remunerative employment.*” Simply adding more and more people to a fixed resource base does not seem to achieve this expansion, taking the wealth and investing it does.

11.3 Employment

The widespread situation, of free and open access to fish resources has led to the fishing industry being seen as the employer of last resort in many countries. In MacKenzie's famous observation (1979)

“fishermen are not poor because they are fishermen, they are fishermen because they are poor”. Improving the economic performance of the fishing industry often involves reducing the exploitation rate and this may mean the loss of some employment. It is important to note however that employment issues relate more to the way in which the fishery is managed than to the level to which it is managed. Within the limit to the exploitation rate imposed by the biological resource constraint, some increase in employment can usually be achieved by requiring that the fishing industry use less than the best available technology, but quite why one sector of the economy should be condemned to inefficiency in this way is usually not justified. One clear policy guideline which emerges from an employment objective is not to subsidise capital since otherwise standard substitution effects can be expected. Regrettably, many countries have paid subsidies to their fishing industries to modernise, with predictable adverse effects on the quantity of labour employed for a given exploitation rate, and the exploitation rate for a given quantity of labour. If employment is an important goal then it may be useful to promote research which creates employment. However, careful consideration should be given to the long run consequences of such decisions.

12. Conclusions

This paper discusses a range of issues concerned with increasing the policy relevance of fisheries research. Some important research issues are also considered.

The paper highlights the crucial importance of economic surplus, especially its resource rent dimension, to an understanding of the process of overexploitation and overcapitalisation in fisheries. It is the failure to extract or capitalise resource rent which underlies the overexploited state of many of the world's fish stocks and the overcapitalised state of most of the world's fishing fleets. Moreover, the naturally-limited physical productivity of fish stocks means that increases in the economic and social benefits of fishery exploitation are to be found in increasing the net value, rather than the physical level, of production.

Due to the importance of economic surplus, it is suggested in this paper that both fishery policy and fishery research should be based around it. The contribution that can be expected from research can be measured in terms of economic surplus - research can help with its extraction or capitalisation, it can help to increase its potential and it can help to increase its sustainability. Research could also be used as a way of targeting particular beneficiaries, such as the poor, but research is a rather poor method of achieving non-efficiency objectives, and at the very least other methods of achieving the same result should be considered alongside research.

The paper suggests a number of possible mechanisms to increase the policy impact of fisheries research. First, priority-setting mechanisms need to be reconsidered (or implemented) to improve the allocation of public expenditure. Such mechanisms should focus on the economic surplus potential of different fisheries. In evaluating the expected pay-off to individual pieces of research, focus should again be placed on the expected (or potential) contribution to sustainable economic surplus. Researchers themselves need to be persuaded to evaluate their own research in these terms. However, notwithstanding this argument, some consideration needs to be given to the issue of pure curiosity-driven research (or basic research) since the focus on sustainable economic surplus may tend to favour short-term research with a more certain payoff. There will be a need therefore to assess the expected pay-off in terms of different risks associated with different projects.

There is also a need to improve the interface between policy and research. Perhaps the most important element is to involve policy-makers more fully in the determination of priority setting. However, it would seem useful also to consider establishing a formal institutional arrangement to deal specifically with the interface problem: what have been called here “research liaison units”.

The issues of financing and delivery of research need to be separated. There are strong economic arguments in favour of public support for some research, based on its public good nature. However, arguments for public delivery of research tend to be rather weaker and appear to be based on a mixture of objectives.

Careful consideration should be given to the issue of whether public or private, domestic or foreign research institutes should carry out different pieces of research. A related question is what kind of research capacity it is appropriate to develop.

Consideration should also be given to improving the research system itself, including improving the performance of individual research institutes and enhancing co-ordination between their activities, and providing incentives for researchers which are consistent with the objectives of research funders and policy makers.

The paper concludes by discussing some key issues that appear of general relevance in determining research priorities and in linking research to policy. First, it is suggested that, where possible, fishery management systems should be broadly-based, and in particular small-scale fisheries should be brought within the scope of a general management system rather than being considered as a special case. Second, it is argued that equity may be an important issue in the design of sustainable efficient management systems. Third, it is suggested that non-efficiency goals, such as gender, poverty and employment, should be addressed as constraints on the ability to achieve economic surplus, at least where research is concerned.

13. References

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