

Requiem for Fisheries Research: Decision Criteria for Budget Allocation

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EDITOR'S NOTE: This paper is modified from comments presented at the AFS Annual Meeting in Vancouver, B.C., Canada, September 14-17, 1977. Your Editor solicits alternative points of view through submissions to the section "Letters to the Editor." It takes at least two to debate, so here's your chance.

My position is that a requiem is in order—a requiem for management, probably, but certainly not for research. A perceptive student of fish population dynamics would conclude that the fisheries research community is alive; it perhaps shows a bit of stress in several population parameters, but at least the population is successfully recruiting new year classes. What more can we ask of a population, whether it be one of sardines or scientists?

Conversely, management, under the banner of the "Harvard Business School," exhibits the characteristics of an unhealthy population—an unstable and unpredictable population structure. The management tools that worked well for General Motors and the Military seem to be inadequate when applied in fisheries agencies. If we are, in fact, going to lament the dead, let's start with management.

What appears to be a conflict between researchers and managers is not really a conflict at all—it is a minor skirmish. Surely, life is a bit uncomfortable for some of us in research—

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the budget justifications, the "payoff" questions, the incessant reporting of progress, the declining base budgets, and the exponentially increasing paper work. But, in the long run, the conflict between research and management has been won—by research. But, by winning the proverbial battle, we may have lost the war. Let me further develop this last thought.

Researchers have been highly successful in perpetuating the kinds of research that we feel are necessary if not essential expenditures of public funds. Each of us can think of examples, more often than not examples from our own research shops, where research programs have been traumatized because of policy and budgetary shifts; but, by and large, we have progressed rather unmolested. In fact, the mark of a fisheries researcher who "has arrived" is often that he is able to carry out studies of his choice and not someone else's.

Conversely, from a manager's view of the world, the central problem is how best to allocate finite funds and personnel to achieve organizational goals. Stated in another way, if you have only a buck to spend, how would you spend it to get the most payoff. It seems simple enough in concept.

Even though the budget allocation process is a critical management problem, it is frequently underrated by those not principally involved. Scientists are often the most myopic in budget allocation issues. And, in fact, some scientists are, by and large, downright antagonistic to budget allocation in principle. "Just send me the money and let me get on with the job at hand," or "Don't bother me with justifying my research effort again; I've already done it 100 times" are commonly heard refrains in nearly all research laboratories. After a few drinks with fellow researchers the budget allocation process is typically described in less charitable terms.

Further, an almost universal view among scientists is that research (particularly that in their own field) is inherently "good" in its own right. The premise that "research ought to be funded on its own scientific merit" is a rallying cry of those feeling "done in" by the agency budget allocation process. Conversely, to the manager, research is merely one tool available to meet agency goals. Other tools might consist of a service "delivery" system, public relations, fish production, or any number of other alternative uses of agency dollars.

Scientists—for that matter all feeders at the public dollar trough—will continue to find themselves in severe competition for scarce dollars. Is this not a situation that is desirable from a fish and wildlife conservation view of the world? The competition for dollars should be based on the payoff, not on maintaining an existing physical plant of bodies or buildings.

Surely, we need a reservoir of scientific talent within the country, but what price is society willing to pay to maintain the research community in the face of alternative uses of those dollars? The current budget allocation compared to what the general public would allocate is very skewed toward heavy

involvement in basic fisheries research. It is only within the scientific community that heavy support for basic research is found.

There clearly has been a recent trend towards rigorous documentation and quantification of potential research benefits derivable from public expenditures. Stated another way: What will John Q. Taxpayer get out of "buying" a piece of proposed research? The trend toward "management by objectives" has further resulted in every proposed expenditure, including those for research, being measured against the scale of "how will this proposed expenditure move me, as a manager or as an administrator, and the organization, towards meeting specified agency management goals?"

However, many scientists and some managers have a nagging concern that society and fisheries-oriented agencies may be sacrificing long-term advancement for short-term expediency. This concern leads to several questions: (1) who should decide which fisheries problems or areas are to be funded and "researched"; (2) should publicly funded research be tied to specific or at least identifiable solutions to problems; and (3) how much can the scientific community (within and without government) be influenced in terms of research direction.

The answer to the question of who should decide research priorities is simplistically "agency administrators and managers." They are the ones who set priorities for funding—at least at a macro level. Because an administrator has only a finite funding level, he must put the dollars where the payoff will be or is at least perceived to be. While it is easy to poke fun at "irrelevant" research projects, there are always countless examples of major breakthroughs from equally "irrelevant" research. But, put yourself in the role of a manager and forget any bias you might have to research. Where would you put your dollars to have the most payoff? Recognize, too, that you are in competition for public dollars with other agencies, not just other uses of the money within your agency.

The answer to the second question is "all funded research ought to be aimed at solving management problems," at least for a fisheries-oriented agency. As much as anyone in

research would welcome a long-term funded project without specific objectives and end-points, this situation will rarely occur; in fact, fisheries researchers will likely feel even more constrained in the future. The budget process is becoming a public policy arena. As it becomes a public forum, researchers *per se* will have less influence. Even now we see a rebellion within certain segments of the public over closed-door allocation of public dollars to certain special interest groups.

The third question, the amount of research direction possible, largely deals with human nature and the nearly limitless capacity of scientists to work on "pet" projects under nearly any written budget justification. On one hand, this situation may be looked upon as bad management because scientists cannot be better directed *through* funded research. Conversely, such a capability assures that we will never eliminate new and unanticipated avenues of fisheries research.

There must be nothing more frustrating to a newly appointed official in a governmental agency than to realize fully the inertia within his own organization. The built-in funding constraints that face everyone associated with agencies, particularly those of the civil service variety, can be extremely depressing. If not controlled, the overhead and base funding associated with laboratories and other scientific endeavors can easily eat up the budget of any agency. Scientists will think up ways to get an increasingly larger percentage of the budget under *all* allocation systems. The percentage may go down in the short run, but the long-term trend will be up.

In conclusion I return to my initial hypothesis that there should not be a requiem for fisheries research, but rather a requiem for management. Agency managers have not been able to bring effectively scientific and research expertise to bear on solving fisheries management problems through the budget allocation process. The research community is alive, well, and thriving. However, there is a new set of rules that we all must learn and these involve additional paper work and other administrivia; but, in the final analysis, most of us still get our funds, still do our research, and still assume that we didn't get quite enough of the total budget. 