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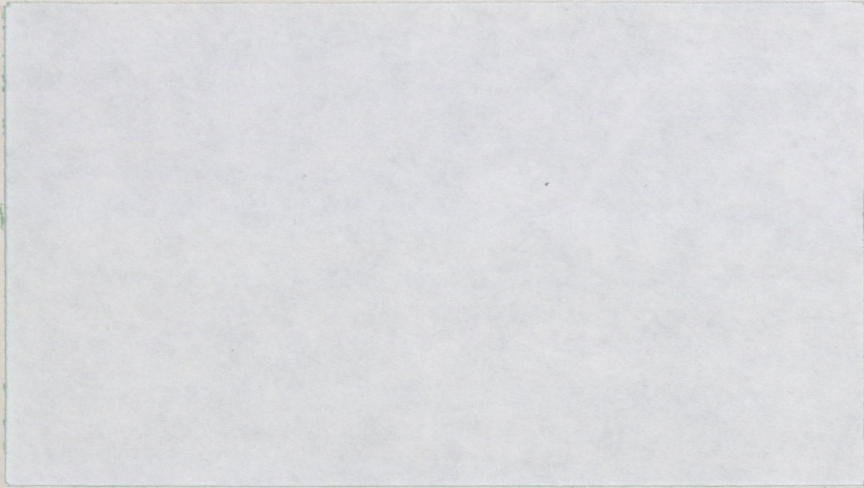
**Investment in Agricultural
Research: Some Economic
Principles**

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PREFACE

This paper by Grant Scobie is aimed at managers of public sector agricultural research programs, especially in developing countries, who are concerned with effective allocation of research resources, aware that little supporting data and analysis is available, and conscious that such analysis can, itself, represent a significant commitment of those same resources. Its objective is to frame a simple series of questions confronting such managers and to draw some guidelines from a vast technical literature for their answers.

The paper grew out of an earlier piece by Professor C. Richard Shumway prepared for a 1982 internal review of CIMMYT priorities for sub-Saharan Africa. We asked Professor Shumway to write for an informed lay audience and to provide a brief summary of what the economist can tell the research manager about attaining more efficient resource allocations. We suggested that the paper give attention to the differing goals which might motivate research, with discussion on how those goals might be weighted. The short Shumway piece served CIMMYT well, stimulating much useful discussion during the course of the internal review. (A slightly revised version was published by Shumway in Agricultural Administration, Vol. 12, 1983, pp. 91-102.)

That discussion and other considerations suggested the possibility of an expanded paper, with the hope that the results might be useful to a larger audience. Here, we were concerned with the potential contribution of models for research resource allocation; to what extent could they aid research decision makers to improve on existing resource allocations?

To do this we contacted Grant Scobie. Through discussions with him, an expanded outline emerged which included the ideas of simple measures as guidelines to research resource allocations, of evaluating private and public sector contributions through research; and of highlighting some of the elements essential to ex-ante research planning. Scobie aimed to produce a "practitioners guide" to research planning.

We think that the paper's thrust is essentially pragmatic. Scobie points the way towards procedures which themselves economize on the resources needed to allocate other research resources. He notes that the adoption rate of formal models is extremely low, a fact which should lead us to question their relevance and effectiveness. While the allocation problem is incrementally complex, Scobie stresses that simple measures can capture the key elements and supplement but not replace informed judgement.

We believe that the paper represents a useful summary of what economists know about the practical aspects of research resource decision making. As with all of CIMMYT working papers, we welcome comments, criticism, or counsel so that the paper might be improved.

Donald L. Winkelmann, Director
Economics Program

"...much has been accomplished by the growth of what is broadly designated as agricultural science. This has been developed with remarkable rapidity during the last quarter of a century and the benefit to agriculture has been great... Much has been accomplished; but much more can be accomplished in the future. The prime need must always be for real research, resulting in scientific conclusions of proved soundness. Both the farmer and the legislature must beware of invariably demanding immediate returns from investments in research efforts. ... In agriculture, effective immediate returns from investments in research efforts. ... In agriculture, effective research often, though not always, involves slow and long-continued effort if the results are to be trustworthy."

Theodore Roosevelt, "The Man Who Works with His Hands," Address to the Semicentennial Celebration of the Founding of Agricultural Colleges in the United States, Lansing, Michigan, May 31, 1907.

INTRODUCTION

Those charged with nurturing, directing and managing agricultural research have a task which is both important and difficult. It is important because the discovery and application of new techniques of agricultural production is possibly the most fundamental and crucial step in raising human welfare in almost any society. Once the link between investment in research and improvements in productivity is acknowledged the task is doubly important in developing countries where agricultural productivity has often lagged.

However the recognition of the importance of research is only a beginning. The task is difficult. The manager is confronted with limited means at his disposal and clamours for research to contribute to multiple goals—better nutrition, greater social equity, more foreign exchange, a cleaner environment, more job opportunities in certain regions, and so the list goes on.

If the questions facing the research planner and director can be broken down into a series of much simpler questions then perhaps some guidance can be offered. The literature in economics, finance, operations research, management science and industrial organization is both vast and rich. It addresses many of the questions that should be answered. How should multiple goals be weighed? How much should be spent on research? How should it be allocated between products or regions? How should the research be organized? What are the expected benefits? How should particular projects be selected.

This literature however, is often obscure; published in odd corners of academe; written in the jargon of its own discipline; addressed solely to others of the same cult; in short, relatively inaccessible. The objectives of these notes are:

- (i) To set down a simple series of questions that face all those charged with the management and direction of agricultural research in the public sector;

- (ii) To draw from this diverse and technical literature some pointers and guides which can be used in framing answers to these questions.

Many procedures and models have been designed to aid decision making, and improve the allocation of research resources. While built on a few relatively straightforward concepts, they have become increasingly complex, difficult to understand and costly to implement. Research managers have responded to these in the same predictable manner as rational farmers have responded to complex, difficult and costly techniques whose contribution was far from clear. As a consequence, their adoption rate has been low. Further development and refinement of techniques to aid research planners and managers should have very low priority. Efforts are needed to understand why the gap is so great between existing "technologies" and those in use. The failure to achieve widespread adoption must be the motivation to examine the techniques themselves. They do contain many of the important elements on which decisions are based. The purpose of these notes is to identify these elements and show that they can be used in a simple straight forward manner; not to produce sophisticated models of research management but to provide some simple guidelines based on well-tried principles.

A number of factors have hindered the use of formal techniques for selecting and evaluating projects in agricultural research. Increasingly research managers have been asked to design and undertake research programs that achieve multiple goals. A simple criterion of selecting the projects with the highest rates of return is no longer perceived as adequate. Attempts to satisfy many goals make the tasks both more difficult and less amenable to formal approaches.

A lack of understanding of basic principles of economics and probabilities creates an understandable reluctance to allow control of a research budget to pass to technocrats. Further, a reluctance to form estimates of possible outcomes and attach probabilities to them can impede the use of even the most simple guides of the allocation of research resources.

Finally, many of the models proposed have been complex, difficult to understand and costly to implement. They require a large investment by skilled analysts and often involve considerable time of managers and scientists. These are scarce resources and the value of the information generated may conceivably be less than the cost.

It should be stressed at the outset that there is no evidence or presumption that quantitative formal models of decision making can replace subjective judgments. It is sometimes argued that whatever their faults and limitations, formal models must be superior to an informal, intuitive approach. The issue, however, is not to choose between formal, structured models and haphazard intuition, but between a quantitative and very imperfect system and informed subjective intuition supported by some relatively simple systematic aids.

WHY INVEST IN AGRICULTURAL RESEARCH?

To provide an adequate answer requires some perspective on economic development. The narrow focus of maximizing the growth of GNP that characterized much of the post-war thinking and policies related to economic development has been substantially altered since the mid-sixties. More recent reviews encompass multiple goals, and often explicitly address the distributional consequences of alternative economic development strategies. An important aspect of these evolving views concerns the role of the agricultural sector.

Until the 1960s, industrialization formed the cornerstone of economic development policy in much of the developing world. Western countries were seen to have generated an increasing proportion of their economic activity in the industrial sector, with a simultaneous decline in the importance of their agricultures. In an attempt to mimic the experience of the developed economics, emerging nations embarked on a deliberate policy of industrialization. An expanding industrial base was seen as a route to rapid economic growth, and to the absorption of apparent surplus labor in the agricultural sector. To achieve their development goals, policies were established which favored industrial expansion, often at the expense of the agricultural sector, frequently

denying the relevance of comparative advantage. In both market-oriented and centrally planned economies, heavy infusions of capital were made in the industrial sector. While some of this investment was met from public and private capital inflows, much was needed from internal sources. As the agricultural sector was the predominant sector of the economy, it became obligatory that resources be transferred from agriculture--in fact, the agricultural sector became viewed as a reservoir of resources from which could be extracted the flows of labor, capital and foodstuffs needed to sustain non-farm economic growth.

Many and varied policy instruments were, and continue to be employed in pursuit of this transfer. Taxes on agricultural exports, land taxes, cheap food policies, multiple exchange rates and protective industrial tariffs represent only a sampling of the devices used to capture the surplus of agriculture and use it as a stimulus to industrial growth.

In large measure this strategy failed. It did not induce broadly based, pluralistic economic growth. Instead a small, highly capital intensive industrial sector emerged, depending for its existence on direct and indirect subsidies and employing little of the expanding labor force. The additional income streams were largely captured by an urban elite whose expenditure patterns did little to stimulate demand-induced linkages to the remainder of the economy. Agricultural productivity tended to stagnate, lending credence to the view that the sector was tradition-bound and unresponsive to prices.

An obvious question arises: was the failure due to faulty conception of the process of economic growth or rather to a missing element? The paradigm itself seems plausible. Sustained economic growth from time immemorial has been typified by a declining importance of the agricultural sector. And rapid increases in real income per capita in Western economies have occurred contemporaneously with a rise in the importance of the industrial sector. However, in every case, technological change in agriculture has been a mainspring of economic growth. From the introduction of irrigated culture some 8000 years ago, the use of a wooden plough (6000 years ago), to the changed farming

practices in Europe in the eighteenth century, to the mechanical and chemical revolutions of the twentieth century, agricultural innovation has been the mechanism whereby the production of food could be accomplished with a simultaneous release of capital and labor for non-agricultural pursuits. So that in an economy where agricultural output and productivity are growing through investment and technological innovation, the resource transfers out of agriculture originally envisaged in the growth policies of many nations do in fact, become a reality.

In the absence of technological change, discrimination against the agricultural sector will lead to stagnation of food production and increasing pressure on food prices, the latter augmented rapid by population growth rates. This has frequently led to government efforts to hold down urban food prices, reinforcing the disincentive to invest in the agricultural sector. In brief, for most countries a development strategy that does not incorporate technical change in the agricultural sector is unlikely to lead to sustainable economic growth. Rather than merely a pool of surplus resources which can be taxed to support non-farm growth, agriculture is increasingly seen as a primary, and crucial, sector in initiating and sustaining economic growth.

There has been a tendency to view investment in research as a relatively new phenomenon. It is sometimes dated from the work of the German chemists in soil fertility in the nineteenth century. Any advances made prior to that tended to be attributed to good fortune--solving a problem simply by accidentally stumbling on its solution. It is doubtful whether this was ever a very good explanation of how advances in agriculture came about. It seems highly probably that virtually all advances occurred as a result of conscious decisions to divert scarce resources to improving productivity. Women (generally acknowledged as the first plant breeders) often selected the seeds from better plants to save for the next sowing. Improvements in crop yields from such early selection were attributable to a conscious decision and the use of time and effort drawn from other activities. Sometimes, of course, simple trial and error was the source, but again the motivation

to improve productivity led to the effort expended in the trial.

This is not to argue that good luck has not occasionally been the source of a new discovery. But failure to recognize that, in general, the expenditure of time and effort was the source of improved ways of producing crops and husbanding animals is to risk discounting the importance of those deliberate efforts and attributing them to serendipity. This, in turn, weakens the appreciation of the link between research and improved productivity.

A significant share of the responsibility for generating technological change in agriculture lies in the public domain. For certain phases of innovation, individual producers could neither finance nor capture the benefits of investment in research. However, establishing in principle that agricultural innovation is often an essential element of economic growth leads only to the justification of social investment in research, given the public nature of the product. It provides no guidelines as to the nature or magnitude of that investment.

WHAT ARE THE GOALS OF AGRICULTURAL RESEARCH?

In Section II it was argued that improved productivity in agriculture has always been an important source of economic growth--higher real incomes in the broadest sense. For most countries this continues to be true.

In 1934-38, the yields of grain were identical in industrial and developing countries (1.1 tons per hectare). By the mid 1970s grain yields in IDCs were 1.5 t/ha compared to 3.0 t/ha in the industrialized countries. In 1907-09, Thailand's rice yield was 1.9 t/ha; in 1962-64 it was 1.5 t/ha. Between 1926 and 1964 rice yields in the Philippines rose from 1.2 to 1.3 t/ha. In 1901-05 India's grain yields were 296 kg/ha; by 1961-63 they were 305 kg/ha.

Growth in farm productivity is, of course, a result of many forces,

although, the evidence is sufficiently clear to attribute a good part of that growth to investment in research. IDCs have generally not invested in research to the same extent as the industrial countries for the last 50 years. Investment in agricultural research is typically about half of one percent of the value of agricultural output in the IDCs. This contrast with an investment of over two percent in most developed countries, or about four times the IDC level.

There is some evidence that more rapid growth in farm output is associated with greater investment in research among IDCs. But the contribution of research inevitably occurs with substantial lags, and this obscures the relationship. This is especially so as there has been rapid growth in the real levels of investment in agricultural research in the last decade. Total expenditures on research in 47 developing countries almost doubled between 1975 and 1980.

Investment in research has been an important factor in improving farm productivity, and this is a major source of growth in real incomes. If this is taken as the goal of research then it is a relatively simple matter, at least conceptually, to look at the costs and benefits and allocate research resources for greater economic efficiency. If there are two possible research strategies each costing \$10 m and one raises agricultural output per unit input by 1 percent and the other by 1.6 percent, then a goal of economic efficiency would suggest shifting resources into the strategy with the higher marginal return. If the value of the additional output is \$50 m in the first case and \$80 m under the second strategy, then the second clearly generates a greater rate of return. All of this assumes that the lags involved in procuring the research result are the same, the adoption rates are the same, and the two strategies are equally risky. We shall have more to say about each of these matters in subsequent sections.

Economic efficiency is, of course, not the only goal that might be set for agricultural research. However, as all countries face limited research resources, insuring that they make the greatest possible contribution must be of prime concern. Research strategies and projects

may be selected which do not necessarily generate the maximum net monetary benefits, because of the need to address other goals. But in that event, public debate about the use of scarce resources to achieve those ends will be enhanced if the costs of the alternative strategies are made explicit. Those costs are simply the value of the extra production foregone by selecting a research strategy with non-monetary goals.

It seems generally desirable that the non-economic goals should not be allowed to unduly influence the selection of a research strategy in a organization whose prime function is to contribute to greater economic efficiency in agricultural production. If it is desired to achieve other goals then funds should be directed to those institutions which are appropriate.

The list of these other goals is almost limitless. At some time or another agricultural research has been charged with:

- a. Generating foreign exchange
- b. Saving foreign exchange
- c. Achieving food self-sufficiency
- d. Creating employment
- e. Improving rural incomes
- f. Changing the distribution of income
- g. Increasing the incomes of small farmers
- h. Reducing rural to urban migration.

In some cases these goals can be subsumed into that of achieving maximum net benefits. Consider the objective of producing technologies which are labor using. The widely held belief that abundant labor is available in the rural areas of developing countries frequently leads to the goal that research should produce new technologies which take advantage of these abundant resources. The argument is further bolstered by noting the need to avoid mechanization which would use foreign exchange for imported capital goods, fuels and parts; or by the desire to expand rural employment opportunities by introducing new techniques which demand labour. What is not so generally appreciated is that simply

choosing those technologies which have the highest economic payoff will automatically tend to bias the research strategy to more intensive use of relatively abundant (ie. cheaper) factors. A research strategy that was designed to produce a technology which required large amounts of scarcer (ie. more costly) inputs would simply have a lower expected rate of return. Provided that the prices which are attached to factors indicate their true scarcity and are not prices distorted by existing public policies, then choosing research projects which are expected to have the highest economic return will automatically result in technologies using relatively more of abundant factors.

It should perhaps be stressed that finding the correct price to use for factors is not easy. For example, if the country's exchange rate is overvalued, imported capital goods will be artificially cheap and will not reflect the true cost of using capital; nor will the domestic price received by farmers for export products be the true return. Using distorted prices and costs can create a very distorted picture of where the true returns to investment in research might lie. A country might for example be considering investment in horticultural research. If irrigation water and fertilizer are supplied to farmers at artificially low prices, and to promote employment the industry is given preferential tax treatment, then it might appear horticulture is highly profitable and research would have a high pay off in contrast to some apparently less profitable industry. The point is, however, that the return to research can only be compared when all the prices and costs represent the true not the distorted values of inputs and outputs.

There are legitimate concerns with the distribution of income in many developing countries. In some cases, agricultural research in colonial times was concentrated on plantation crops for export. The benefits were captured by landowners, often expatriate, in the developing countries and by industrialists and consumers in the home countries. Such research made little contribution to the well being of most of the population in the developing world. This lesson serves to illustrate the importance of the commodity mix in determining the distribution of benefits. Research on export crops will tend to generate higher incomes

for the owners of the scarce factors in that sector; specifically land and managerial skills. Perhaps there will become increased demand for labor. Research on domestic food crops, on the other hand, will tend to increase their output and lower the real price of staples facing consumers. This is a powerful mechanism by which research can contribute to improved income distribution.

In the broadest terms the gains from technological change accrue to two groups: those who supply the services used in the production of the output, and those who consume the product. When the product is consumed largely within the country rather than being exported any decline in price will benefit domestic consumers. And if the product is a basic food staple then the lower income groups will benefit disproportionately. This can be illustrated by reference to the accompanying table.

	Consuming Groups	
	"Poor"	"Rich"
Number of People	100	100
Income Per Capita	5	50
Total Income	500	5000
Spent on "Wheat"	400	1000
Old Price of Wheat	10	10
New Price of Wheat	9	9
Cost of Buying Old Amount of Wheat	360	900
Gain in Income	40	100
As a Percentage of Original Income	8	2

This stylized and simplified example is intended to represent a society with an uneven distribution of income. Suppose all food expenditures are for a composite called "wheat" whose price is 10 units. The poor spend 80 percent of the incomes on wheat; the rich only 20 percent. Now let the price of wheat fall to nine units as a result of a research program which improved wheat productivity and increased output by 20 percent. Both groups could continue to buy the same amount of wheat, and they would spend less to do it. The savings represent income they can now spend to buy more wheat at the lower price and to buy more of other goods. In absolute terms the income of the rich group has risen by 100; that of the poor by 40 units. But this simply reflects a larger

base income in the case of the rich group. Income of the poor group has risen by 8 percent--that of the rich by 2 percent. So even if the rich capture a greater absolute gain, the gains are distributed disproportionately in favor of the poor who spend a greater share of their incomes to buy the staple food. In short, a goal of equity is served by first choosing basic food staples as the target of research and then maximizing the net economic benefit from the use of the scarce research resources. The widely discussed trade-off between the goals of equity and efficiency in agricultural research simply does not exist in these circumstances.

None of the foregoing arguments in any way attempt to minimize the serious problems of poverty and income distribution prevalent in many countries. The point is simply that burdening agricultural research with the responsibility for addressing a broad spectrum of social ills is often to seek goals which are simply not attainable using research budgets as the instrument. If there are problems of employment opportunities, migration, health, nutrition, sanitation or land ownership, then each should be addressed with the policies which have the most direct and greatest impact. The risk is that seeking to achieve multiple goals will direct research resources away from the urgent need to continue developing and maintaining new technologies to enhance agricultural productivity. If this research is directed to basic foodstuffs it can be a powerful tool for improving the distribution of income. Increases in food production are an important source of additional income streams for both rural and non-rural households. These lead to more broadly diffused economic growth and expanded demand for off-farm labor. That large farmers have more land and can capture more absolute gains from technological change is more a consequence of a historically determined land ownership pattern, than an effect of agricultural research per se. Attempts to divert attention from the fundamental goal of investment in research, namely to generate widely adopted technologies which improve productivity, would be to overlook the demonstrably favorable impact on income distributions that can and does follow from such investment.

WHO SHOULD INVEST IN AGRICULTURAL RESEARCH?

Traditionally farmers have been the primary source of research in agriculture. Their creativity, risk-taking, ingenuity and continuous search for better methods is still a key element in the process of improving agricultural productivity. But there are limits to their capacity to finance research, and importantly to capture the benefits.

Some types of research produce results for which there can be no market. Information by one farmer about an improved management practice, say a grazing system, might result in significant gains in productivity but the use of this information does not reduce the amount available for another. Other types of research might result in a new implement or input through whose sale a return to the investment in research can be captured. In these instances private firms who produce inputs for agriculture can be expected to engage in research. This source of investment becomes increasingly important with greater dependency on purchased inputs. Freely functioning input markets and an economic environment which stimulates private investment are key ingredients to ensure the maximum possible contribution from research by private firms. To the extent they are multinational firms they can bring the advantages of experience and testing from a large number of settings. Fears of exploitation are best allayed by fostering an environment in which several companies are encouraged to compete.

When the research costs cannot be appropriated through market sales of inputs, some collective action is needed to ensure an adequate investment in research. This can take two forms. In the first place, the farmers themselves might decide to collect a levy on each unit of output and, by pooling these resources, fund a program of research and extension for their particular commodity. Typically, this would occur for export crops where the benefit of the research accrue largely to domestic producers rather than consumers. The cases of both rice and coffee in Colombia are examples of the important role of producer levels for research.

The foregoing paragraphs lead to the conclusion that public research

in agriculture should concentrate firstly on those matters for which the benefits could not be appropriated through market sales and secondly, on those commodities in low-income countries that form an important element of domestic consumption and are not necessarily exported. If the good does not enter international trade then domestic consumers will tend to be the long-run beneficiaries of lower real prices engendered by improved productivity. However, if the good is either imported or exported in significant amounts then the world price will tend to be a primary factor in determining the level of domestic prices. The issue becomes less clear when public policies intervene to insulate the domestic price from world prices. While this matter is of consequence for the distribution of gains, it does not affect the basic proposition that publicly funded research to enhance agricultural productivity in basic food deserves high priorities in government expenditures.

Increasing attention has been given to the role of "spillovers" in agricultural research. This refers to the ability of a region or country to benefit from research undertaken in another area. Investment in research by the international centers is aimed at precisely this issue. Their objective is to generate results which are widely adaptable and thus achieve more efficient use of global research resources. While the potential benefits of research done in other areas can be substantial, evidence exists that they are captured only when there is an adequate indigenous program of research and extension to receive, adapt, test and distribute the findings and materials. If the results of research in one region or in one commodity are likely to be captured by other regions or commodities it may be legitimate to count those benefits when estimating the likely pay off from the research.

Research can be directed to improving efficiency in both the production and processing of agricultural commodities. This means that the appropriate mix of private and public research in both sections has to be found. In many cases innovations in the processing section will be of a type whose benefits can be appropriated by the innovating firms. However, public research may well be needed to fund research on such matters as grading standards. Of course, producers themselves may have

the incentive to support research (in either the public or private sector) to generate new processing methods which expand the demand for the farm product.

In many instances it is the basic or scientific research rather than the applied technological research which has no direct market. The results of research on the effect of certain management practices on mineral levels in the grazing animal would not generally be "marketable", in the way that the results of a research program to produce more effective animal remedies, farm implements or weed killers can be marketed. There will be a natural tendency for the more basic types of research to be undertaken in the public sector.

HOW MUCH SHOULD BE INVESTED IN RESEARCH?

Establishing that there is both a legitimate and crucial role for public investment in agricultural research does nothing to address the question: "What is the optimal level of expenditures?" It is surely unnecessary to belabor the point that to derive a complete answer to such a question is a difficult and complex task. Yet investment decisions and budget allocations are being made by research managers and politicians continually. At least some attempts might be made to draw on simple principles as a guide in developing an answer.

Studies of past investments in agricultural research have yielded high rates of return--a figure of 40-50 percent annual return is not uncommon. While there are clearly problems in the measurement of both the benefits and the costs and in the models used, there are now sufficient studies covering many commodities in many countries that there is little doubt about the general conclusions. Certainly there have been failures, although the literature is not replete with accounts of such incidents. But few if any of those who have examined these studies would deny that the return has been very high.

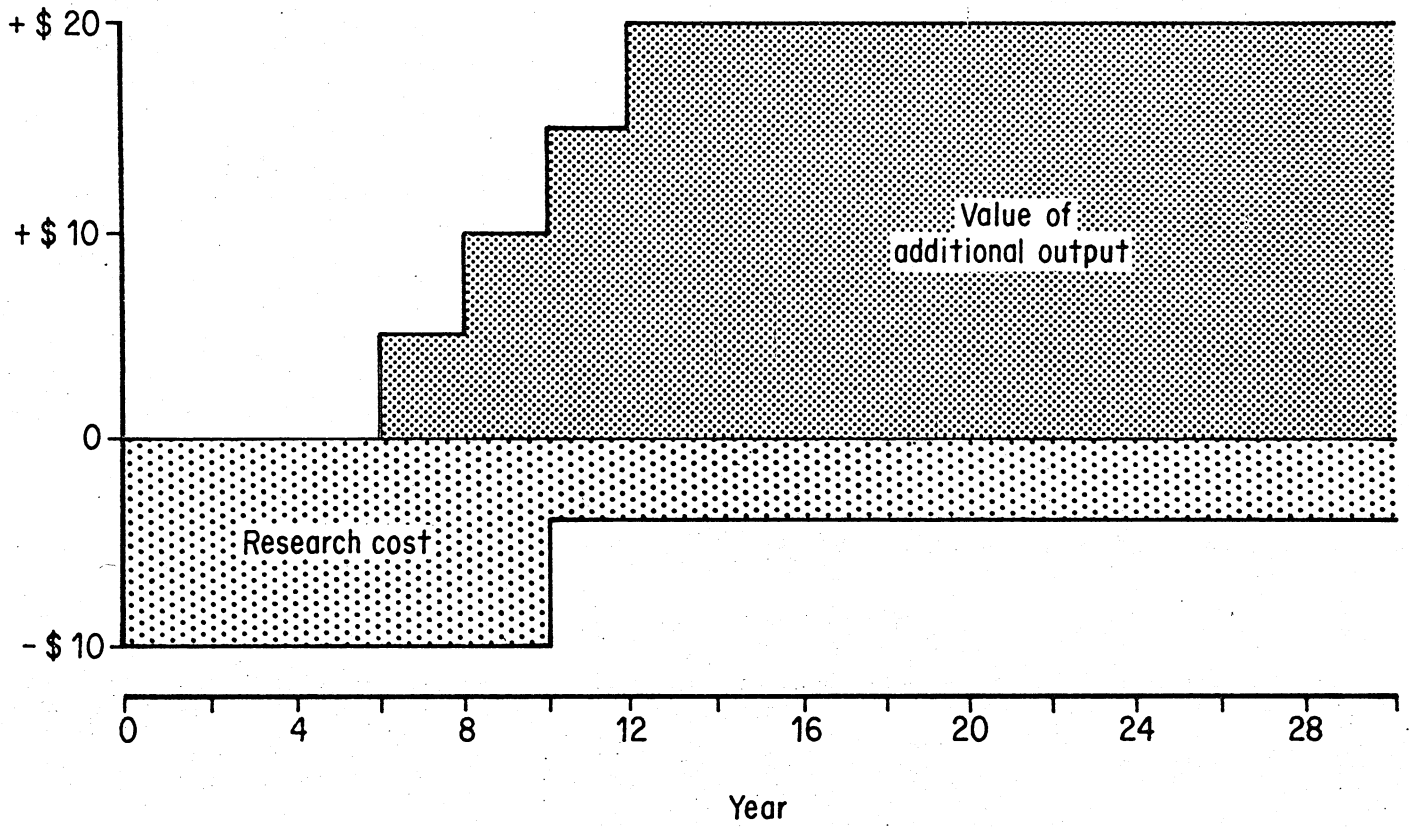
This is at the same time both reassuring and disturbing. It is reassuring because it suggests that the informal, subjective mechanisms

which have been used to assign funds to research have performed consistently well. The informed judgment of research managers and scientists who are knowledgeable of local circumstance and responsive to the demands of client groups has been the most valuable tool for allocating research resources. And furthermore, it is unlikely to be supplanted by any mechanistic models of resource allocation. While a vast array of quantitative armory has been developed, it neither can nor should be a substitute for the creativity and judgment of informed participants. A formula, however rigorous in a mathematical sense, may give an impression of pseudo-objectivity. It cannot, however, disguise the fact that certain important elements are inevitably subjective. There remains the possibility that in making those judgments some relatively simple guidelines can be used as aids. The disturbing feature of the very high recorded rates of return is that we have foregone opportunities by underinvesting in agricultural research—we should have done more and driven the return down to rates comparable with alternative investments.

There are a number of key elements in the problem of deciding on how much should be invested in research, say at the national level. The first of these concerns the timing:

- (a) How long will it be before there are any results which can be adopted?
- (b) How many years after the initial release will it be before the maximum level of adoption is achieved?
- (c) How long do the results continue to contribute to output?

Figure 1: Flow of Benefits and Costs from an Agricultural Research Program



These points are illustrated in Figure 1. In the case illustrated there is a six year lag, full adoption takes six years and the research results have a productive life of 24 years terminating totally in year 30. In general, the results will not be particularly sensitive to which year is selected as the terminal year unless the contribution of the research is predicted to have a life of only a few years. A variety whose disease resistance will predictably breakdown after two years is a case in point. Here, however, concern is with the broad level of total national investment.

The research costs are shown in two phases:

- (a) The investment period.
- (b) The maintenance period.

The costs may be computed to include extension as well as research costs. Not all research results will require maintenance, but estimates of up to 30 percent of the total budget for maintenance of existing productivity gains are not atypical. In some cases it may be appropriate to allow for the contribution to decay over time, in much the same way as the capital investment in a building or a machine would be depreciated.

The essential elements of a simple benefit-cost analysis are now in place. The final matter concerns discounting. It is evident from the figure that the costs and the returns occur at different points in time. Investments in research today generate results and contributions to output at various times in the future. A dollar given up today is not fully compensated by a dollar received in, say, 10 years time. In order to compare the streams of benefits and costs it is necessary to express them all in terms of the same year. It is quite an arbitrary matter which year is selected. Convention usually dictates that the first year (or the present year) be selected. In this way, all the costs and returns are computed in the terms of present values. Formally

$$B = b_t (1 + r)^{-t}$$

and

$$C = c_t (1 + r)^{-t}$$

where $B, C =$ the present value of benefits and costs (a single monetary value),
 $b_t, c_t =$ the value of benefits and costs in each year,
 $r =$ the discount rate.

Now the values of B and C combined can form a single index of the return to the investment. Two forms are commonly used; the difference, or the ratio. The difference

$$N = B - C$$

is called the Net Present Value (NPV) of the project or the research program. The ratio

$$R = B/C$$

is simply called the Benefit-Cost Ratio. The Net Present Value is a monetary amount (eg. \$156 m) which if invested at the discount rate (r) would generate a stream of returns equivalent to those generated by the research program. The Benefit-Cost Ratio is a pure number (say 3.2), which indicates the investment generates a return of 3.2 dollars for every dollar invested. Sometimes a third way is used to express the results. An Annual Average Return (AAR) is computed which simply says that the program generates returns which are equivalent to receiving an annuity of say \$12 m each year. This is found by simple converting the Net Present Value to an equivalent annuity. If the discount rate is r , it simply gives an annual amount which would make one indifferent between receiving a lump sum (NPV) today, or receiving a constant annual return (AAR).

All these methods (NPV, B-C Ratio and AAR) require an estimate of the discount rate. This should reflect the rate of return that could be earned if funds from the agricultural research programs were invested in another activity. Alternatively, it is the return given up, or the opportunity cost of those funds. If the next best alternative would be to invest the funds in foreign bonds at 12 percent or highway construction at 10 percent or education at 15 percent, then 12, 10 or 15 percent is the relevant discount rate for the agricultural research program.

It often happens of course that determining this rate is not a simple matter and a suitable discount rate may not be readily at hand. An alternative method of constructing a single index of the returns to the investment in research is the Internal Rate of Return (IRR) generated by the project and given by that rate (i) which satisfies

$$\sum_{t=0}^T b_t (1+i)^{-t} = \sum_{t=0}^T c_t (1+i)^{-1}$$

where T is the total life project.

The Internal Rate of Return is the interest rate that a savings account would have to pay in order to generate returns equivalent to the benefits of investment in agricultural research. It is widely used concept and one that is readily understood. With the widespread use of business and even pocket calculators its burdensome computation (which is done iteratively) is no longer a barrier to its use.

Consider the problem of deciding on a level of investment in research for crop production in the developing market economies of the world. All crops were converted to "wheat equivalents" using their caloric values and the basic quantity (Q_0) valued at the world wheat price (P_0). For example,

$$Q_0 = 550 \text{ m tons of wheat equivalents}$$

$$P_0 = \$135 \text{ per ton}$$

Research was assumed to take 10 years to produce results, five years to reach full adoption and to last indefinitely. Maintenance costs were set at 30 percent of the investment costs. As interest focussed on the return to investment in international agricultural research, it was assumed that it contributed 15 percent of the total growth in output due to all research in DMEs. With these assumptions the following Internal Rates of Return were found:

<u>Level of Investment</u>	<u>IRR</u>
\$ m	%
179	10
75	15
26	20

The important result to be noted is the decline in the rate of return to research as more is invested. In general it will be true that expanding any capital investment, including that in agricultural research, will result in smaller increments to output and so a lower rate of return. Of course at very low levels of investment, research establishments may be too small for efficient use of overhead facilities, and greater productivity could be associated with increased investment. But once these economies of scale have been reaped, the extra or marginal return is likely to decline with expanded investment.

This raises the problem of how to specify an appropriate relation between investment in agricultural research and growth in output. It is through this relation that the declining marginal return to research is introduced. The problem is addressed further below. In the meantime, an extremely simple procedure can be used to generate an approximate result.

If the current value of output (either national crop production, or that for a particular commodity) is taken as the base, and multiplied by the reduction in unit costs due to research, and the extent of the industry covered, then

$$b_t = V_o \times A_t \times R_t$$

where

- V_o = value of output in year 0
- A_t = the adoption level in year t
- R_t = reduction in unit costs due to research

For example, let the value of crop production in a given country be \$300 m, and assume that investment in agricultural research and extension would produce results applicable to 75 percent of the crop. Further, unit costs would be reduced by 15 percent; ie. with current input levels

output would rise by 15 percent. As a result

$$b_t = 300 \times 0.75 \times 0.15 = \$33.75 \text{ m}$$

If the research program is being funded currently at \$5 m per year and produces results with a five-year lag and a further five years until full adoption, then the stream of benefits and costs are:

Year	Benefits	Cost
	\$ m	\$ m
1-4	0	5
5	6.75	5
6	13.50	5
7	20.25	5
8	27.00	5
9	33.75	5
10-20	33.75	2

Such an investment implies an Internal Rate of Return of 42 percent. The key element of this approach, and in fact all approaches to estimating the return to research, is the economic "size" of the industry and the reduction in unit costs. The size is conveniently captured by the current value of output; the effect of the research on productivity can never be more than a subjective estimate, although past results may be useful guide.

This approach assumes that in the absence of improved productivity engendered by the contribution of research to productive knowledge, the output of the industry will be constant. If the industry is expected to grow at some rate, say g , then the benefits from any given amount of research will be greater. Formally they will be given by

$$b_t = V_0 (1 + g)^t \times A_t \times R_t .$$

Naturally, the benefits of any given amount of research expenditure will be greater in larger, growing industries.

As noted earlier, the extra (marginal) contribution of more resources invested in research is likely to be diminishing. Ideally one would like to be able to specify the relation between growth in output and the level of research investment. This relation involves many subjective elements however. At best, some bounds can perhaps be used. These are useful to give some estimate of the additional output which might be expected to flow from different levels of research investment. Figure 2 shows a possible relation.

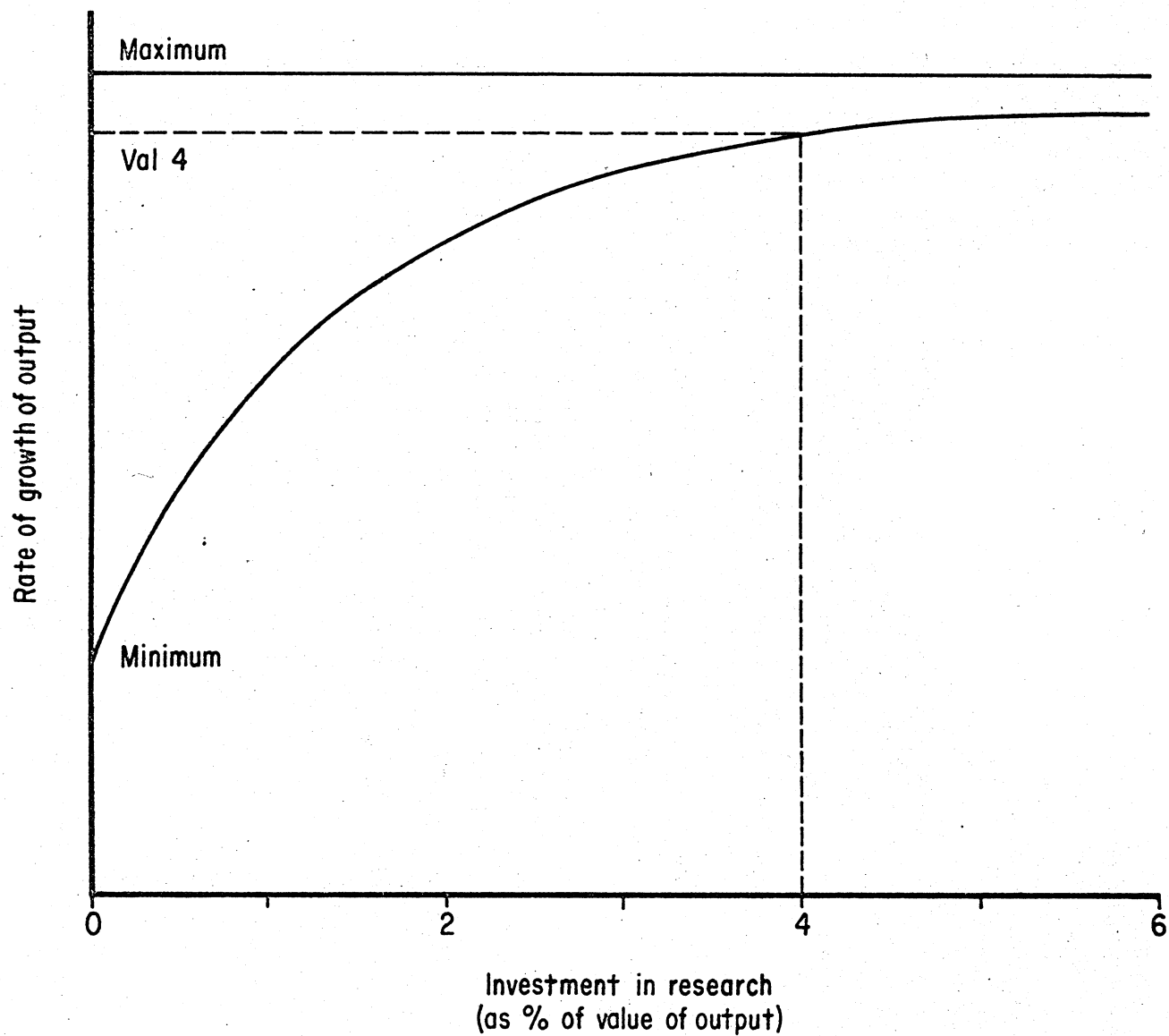
If there were no investment in research, output is assumed to grow at some minimum rate (MIN). With very high levels of investment output grows at a rate approaching some maximum (MAX). An equation describing this is given by:

$$G = \text{MAX} - \frac{(\text{MAX} - \text{MIN})}{(1 + (R/V))} e$$

where G is the rate of growth of output, and R/V is the proportion of the value of agricultural output spent on research. The scale proposed in the figure is intended to be indicative of the range of values typically encountered. Rapid rates of agricultural growth have been associated with expenditures of around one percent or perhaps as high as two percent of the value of agricultural output in developing countries. The functional relationship can be easily solved for the parameter e, by inserting values for MAX and MIN (6 percent and 1.5 percent would seem to be typical values) and selecting a value of growth when four percent of value of output is invested on research (90 percent of the MAX value of 5.4 is probably a reasonable estimate).

Knutson and Tweeten built a model to determine the optimal growth rate of the US agricultural research budget. Their model is built around an estimate of the marginal productivity of research, ie., the contribution of investment in research to the growth of agricultural productivity. The first step is to use historical data for the output of the agricultural sector, and subtract the inputs. Their residual is the growth in productivity, or output per unit input. This productivity is then used as the dependent variable in a regression model

Figure 2: The Relationship Between Investment in Research and the Growth of Output



which has research expenditures, extension expenditures, weather and educational level of farm operators as explanatory variables. From this it is possible to isolate the marginal contribution of research to productivity.

Demand is assumed to grow (due to population, income and exports) at a rate of 1.5 percent p.a. For a given rate of increase in research investment (say 30 percent p.a.) the regression equation can be used to find the growth in productivity that this level of research would imply. Then the change in output is calculated, reflecting the growth in productivity engendered by investment in research. Suitable lags are introduced and, after valuing the extra output and netting out research expenditures, the rate of return to investment in research can be determined. In an application to the US it was found that a 3 percent p.a. increase in research expenditures would imply an internal rate of return of 36 percent.

It is abundantly clear that there is uncertainty surrounding estimates of future benefits and costs. This arises from a number of sources:

- (i) Will the research be successful?
- (ii) How long will it be before results are available?
- (iii) How widely applicable will they be?
- (iv) How rapidly will they be adopted?

A basic approach often adopted is simply to discount the benefit by an estimate of the probability of success (P_s) and so obtain the expected present value of benefits:

$$E[B] = P_s \times B_t (1 + r)^{-t}$$

or if the probability is intended to reflect say uncertain adoption rates, then it might vary with each year so

$$E[B] = P_{st} \times B_t (1 + r)^{-t}$$

where P_{st} is the probability that new technology will be available in year t .

Suppose a research program is contemplated so C_O and C_N are the costs with the old and new reduce harvesting costs, by the development of a mechanical harvester. If costs were \$78 per ton for manual harvesting and \$18 with a machine which would take 10 years to develop and have a useful life of 7 years then, for a crop of 23,000 tons, the discounted value of the benefits (at $r = 6$ percent) with no uncertainty would be

$$B = \sum_{t=10}^{17} (78 - 18) \cdot 23\,000 (1 + 0.06)^{-t} = \$5 \text{ m.}$$

On this basis, a research program to develop a mechanical harvester could afford to spend up to \$5 m, if it was certain that by year 10 a successful machine could be developed. When P_t is less than 1, then clearly the benefits, and the justifiable research expenditure, will be lower.

In some cases it may be better to maintain the probability estimates separate from the value of the benefits. In the above example the value of the benefits of a successful research program are calculated to be \$5 m. The probability of having a successful machine in less than 1, 2, 3, 4, 5, 6...years could be calculated separately. Information about how risky an investment might be tends to be lost when the probabilities are multiplied by the benefits. For example, suppose a project would generate benefits of \$10 m but the probability of success is only 0.1. This is the same expected benefit as a project which would generate \$2 m with a probability of success of 0.5. However, research planners may well prefer the second project.

Further, the probability measure is not just the probability of success or failure. The uncertain elements will in general have a whole range of possible outcomes each with their associated probability. In many cases the outcome of research is not just a success or a failure but may lie between these extremes by generating new knowledge which, while

less than hoped for, is still significant.

This leads naturally to the notion of a distribution of expected benefits or alternatively, of expected rates or return. As a general rule it is probably sufficient to screen projects simply on their estimated net benefits or upon their estimated rates of return. However, some additional information is provided by the spread (or variance of returns). Two poultry research projects were evaluated by Grieg, who derived the distribution of the net benefits, and the cumulative probabilities. The latter concept allows the research manager to ask: "What is the chance that this project will generate net benefits of less than \$2 m" or "What is the chance that this project will fail to generate sufficient benefits to cover the cost of the research project?"

Grieg investigated the potential returns to a research program to develop a vaccine against infectious bronchitis (IB) in poultry. He elicited subjective estimates of the probability distributions of the main uncertain parameters by judiciously questioning research scientists. Typical of the uncertain elements were:

- (i) The probability of an IB outbreak in this batch of birds;
- (ii) The age of the birds at the start of the outbreak;
- (iii) The severity of IB, both before and after research as reflected in mortality, and weight gain.

The use of subjective assessments of the probability distributions was based on the simple questions. For example, in the case of mortality the questions would be structures as follows:

1. What is the typical (or modal) level of mortality in birds?
2. What is the highest mortality rate that is feasible?
3. What is the lowest mortality rate that is feasible?

The use of this three-point (or triangular) distribution is a convenient way to summarize information about uncertain outcomes and incorporate its uncertainty in a quantitative analysis of research benefits. The scientist need only specify the minimum (MIN), most likely (MOD) and maximum (MAX) values that the uncertain element could adopt

(see Figure 3). This allows a cumulative distribution function to be derived which gives the probability that the element will have a value greater than some specified level, eg., what is the probability that after a successful research program the mortality rate in a poultry flock would exceed two percent? This distribution can then be used to derive a histogram of the present value of the research program (Figure 4), by drawing repeated samples (a technique known as Monte Carlo simulation). Of course, we could have as many uncertain elements as appropriate.

A value from each distribution is drawn at random and combined with similarly drawn values from the other distributions. These values are used to calculate a NPV (or IRR) of the research. By repeating this operation a whole distribution of NPV values is obtained which itself can be plotted as a cumulative distribution (Figure 5). This can be used to ask "What is the probability that the NPV is greater than zero? than \$100 000? etc."

The use of these distributions can be extended to compare two research strategies or projects. Two research projects on broilers (coded IB and LW) were comparing using the Monte Carlo technique described above. The outcomes are depicted in Figure 6. Immediately one observes that there is no chance of the IB project having a negative return in contrast to the LW project which, given the uncertainties, may produce a negative return.

It is not difficult to use estimates of the contribution of research to productivity, together the "economic" size of the industry. When combined with appropriate lags and timing this is sufficient to generate an estimate of the net benefits, or of the rate of return. Furthermore, it is a relatively straightforward matter to identify some of the main uncertain elements and develop probability distributions each of these types of information can be of use to the decision maker. However, combining them in a simulation model of a typical farm, and then aggregating to the level of industry in order to generate distribution of the returns, is not a trivial task. In many cases, one or two man years of a researcher with considerable analytical skills in economics,

Figure 3: A Triangular Probability Distribution Constructed from Three Points.

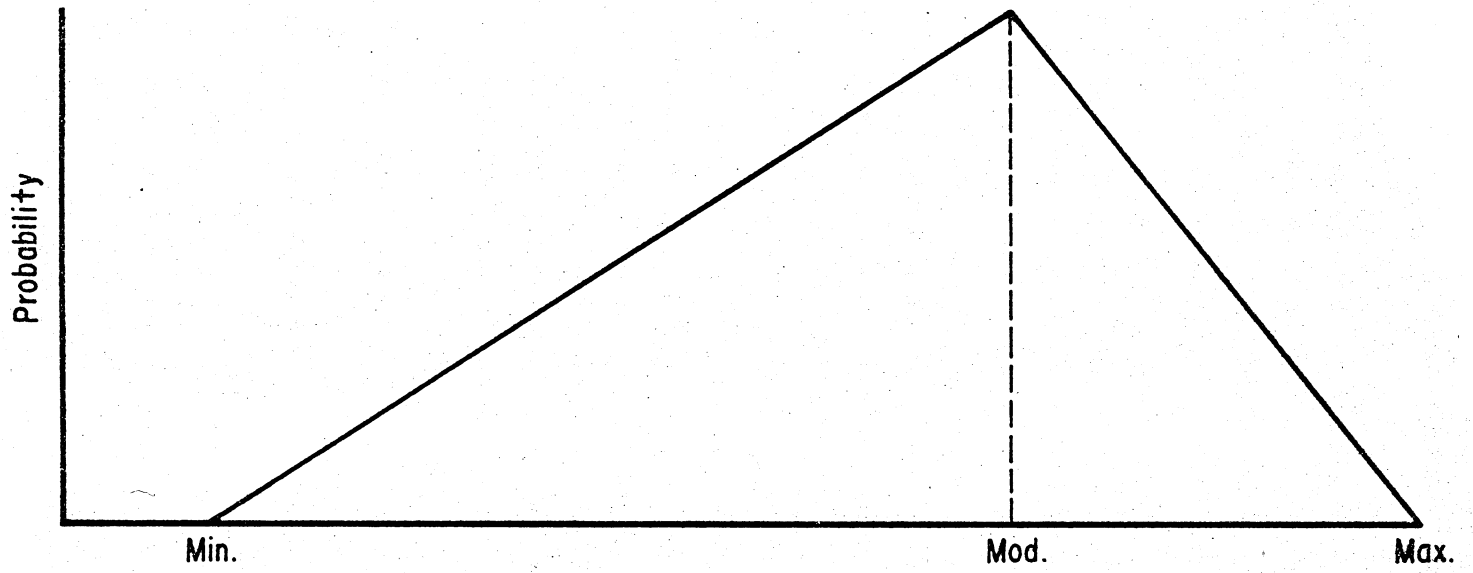


Figure 4: Histogram of Net Present Values of a Research Program.

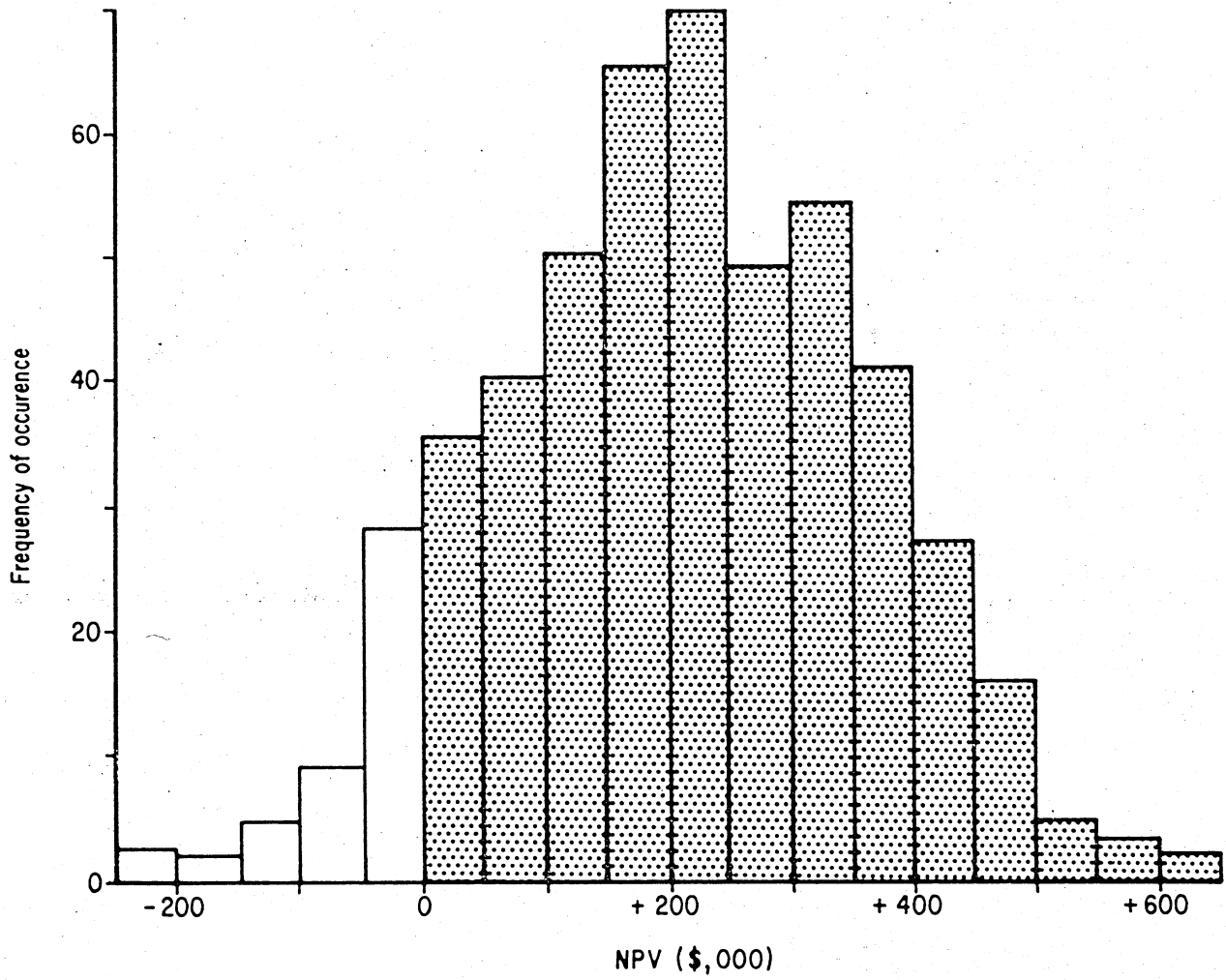


Figure 5: Cumulative Probability Distribution of Net Present Values.

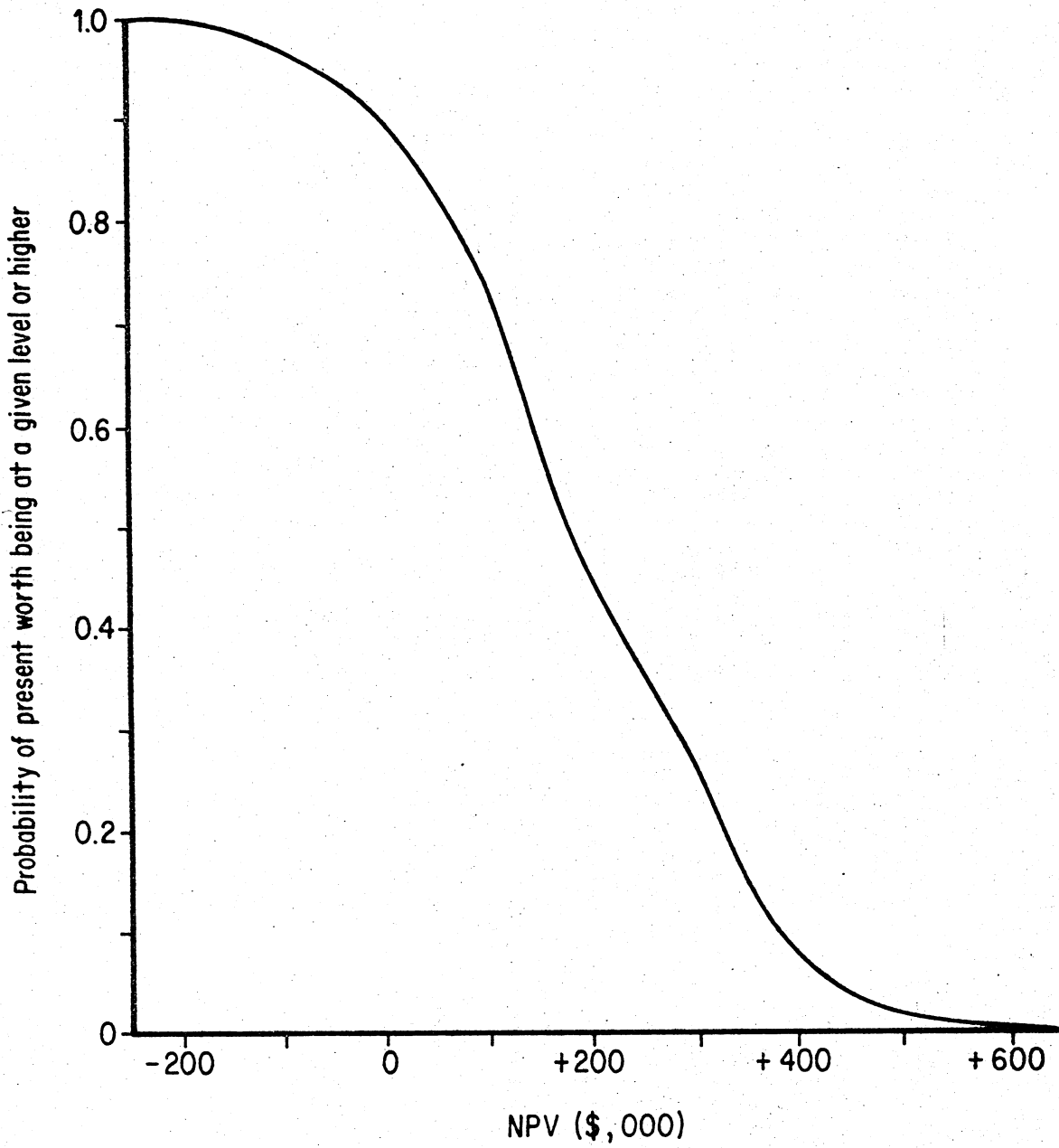
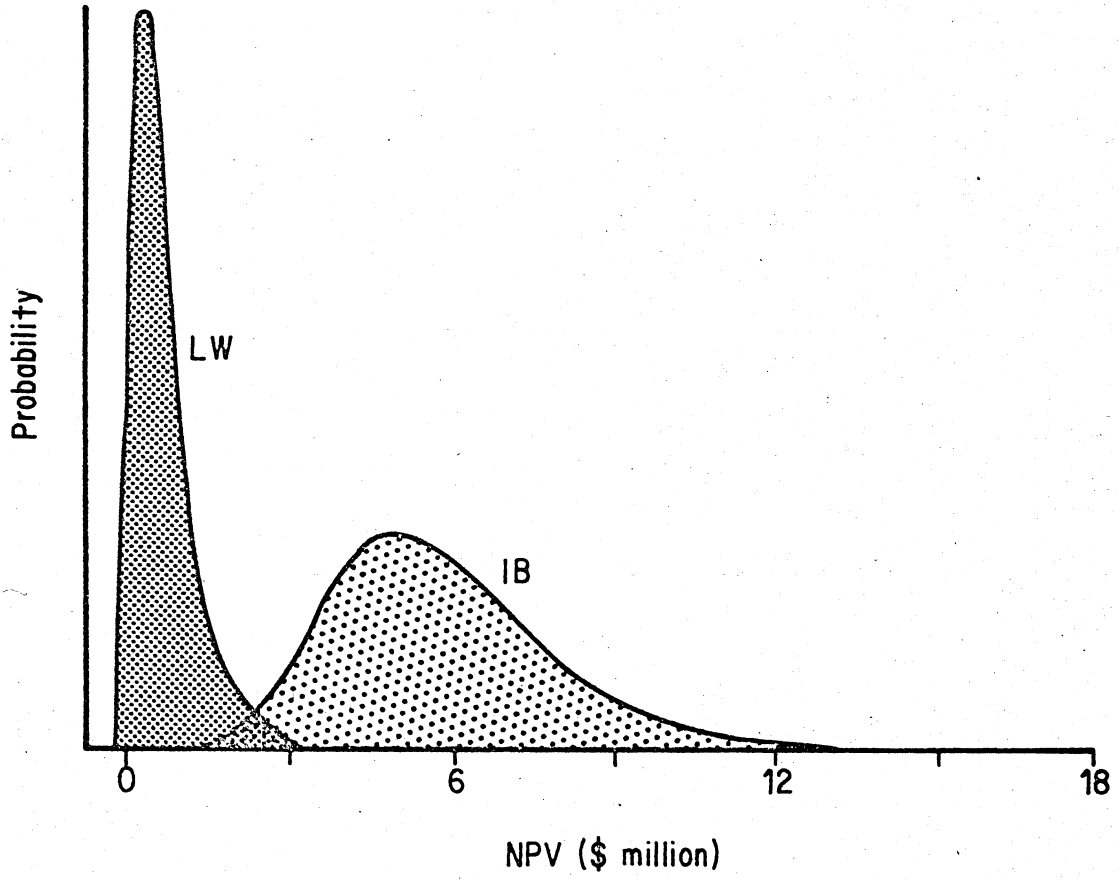


Figure 6: Probability Distributions of the Net Present Values of Two Poultry Research Programs.



statistics and decision theory would comprise the minimum resources needed. Clearly, there will be circumstances when these are not available. More importantly, the cost and delays may simply not be justified. From the time alternative research strategies or projects were clearly identified until estimates of the distribution of returns to each were available one or two years could elapse; the value of the analyses as an aid to decision-making would probably be very small. For this reason alone, simpler (albeit cruder) but more timely analyses may often be more appropriate.

There is clearly a trade off between more costly elaborate models and the accuracy of the forecast of prediction of returns to research. Elaborate models are expensive to construct and generally are not readily transferrable from one research project to another as each has its own special features. They can, however, be expected to give more accurate predictions. This trade off and the notion of an "optimum" degree of accuracy is conveniently depicted in Figure 7.

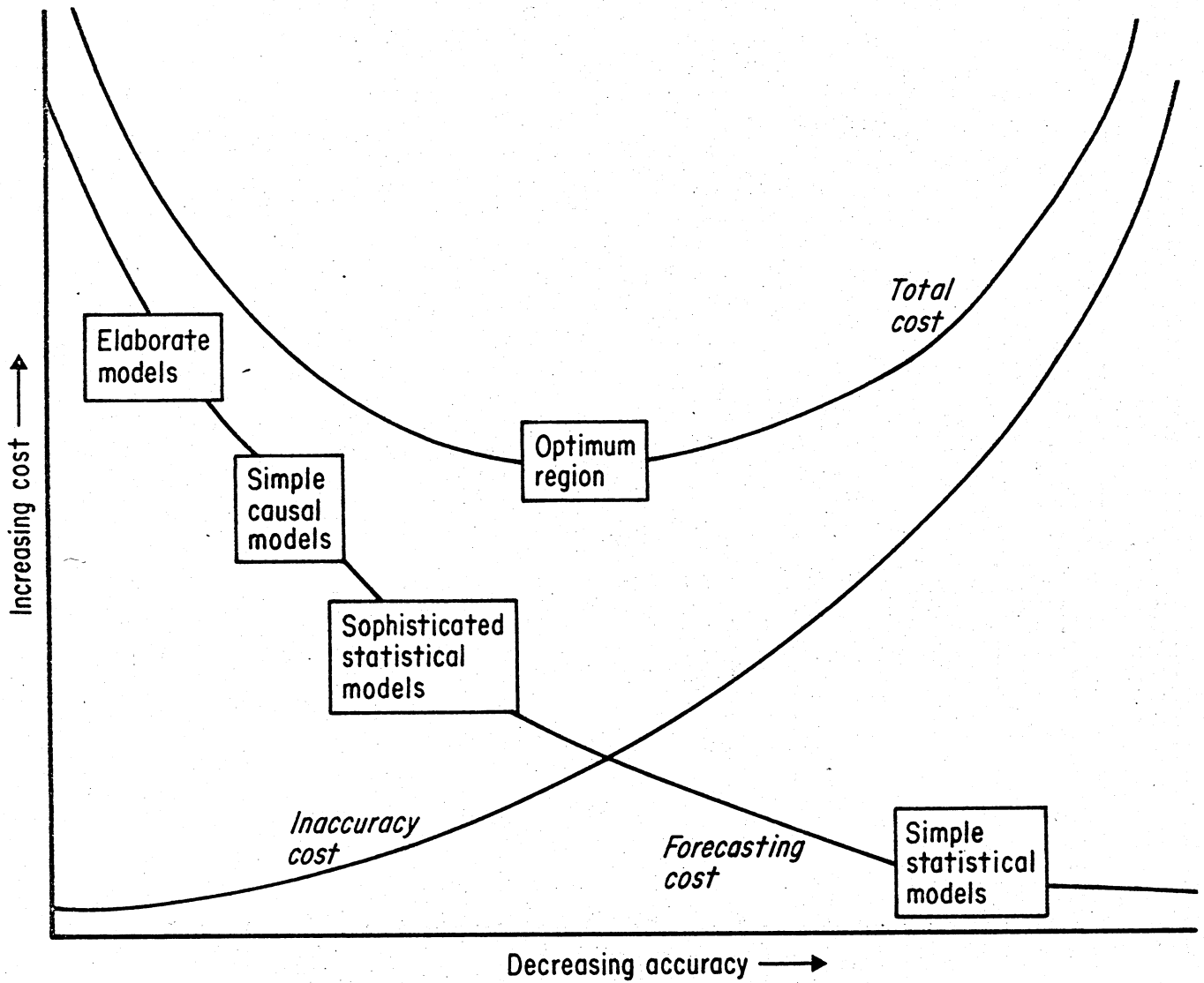
HOW SHOULD A RESEARCH BUDGET BE ALLOCATED?

In this section are discussed methods to allocate a research budget between zones or commodities or projects.

Perhaps the simplest and most useful initial step in allocating research budget is to exploit the concept of the "size of the industry." For example, if there is a total research budget of \$100 m to be allocated across commodities, an initial assignment would be based on the share of each commodity in the total value of agricultural output. The research budget would then be assigned as follows:

Commodity	Value of Output	Share	Research Allocation
	\$ m		\$ m
Wheat	300	0.26	26
Rice	150	0.13	13
Cassava	200	0.17	17
Cotton	100	0.09	9
Cattle	200	0.17	17
Poultry	150	0.13	13
Horticulture	50	0.05	5
Total	1,150	1.00	100

Figure 7: The Trade Off Between the Accuracy and Cost of Estimating the Net Benefits to an Agricultural Research Program.



In Figure 8 the various commodities would lie along a 45° ray from the origin. It is possible that, because of the difficulty of the research task or its cost, different products may lie on other rays; this matter is pursued below. Actual research budgets can be compared to this by constructing an Index of Congruence (C) between the actual budget shares and those suggested by the economic size of the industry. Formally the index is given by:

$$C = 1 - \frac{\sum_{i=1}^n (A_i - S_i)^2}{\sum_{i=1}^n S_i^2}$$

where A_i = the total share of each commodity in the present research budget.

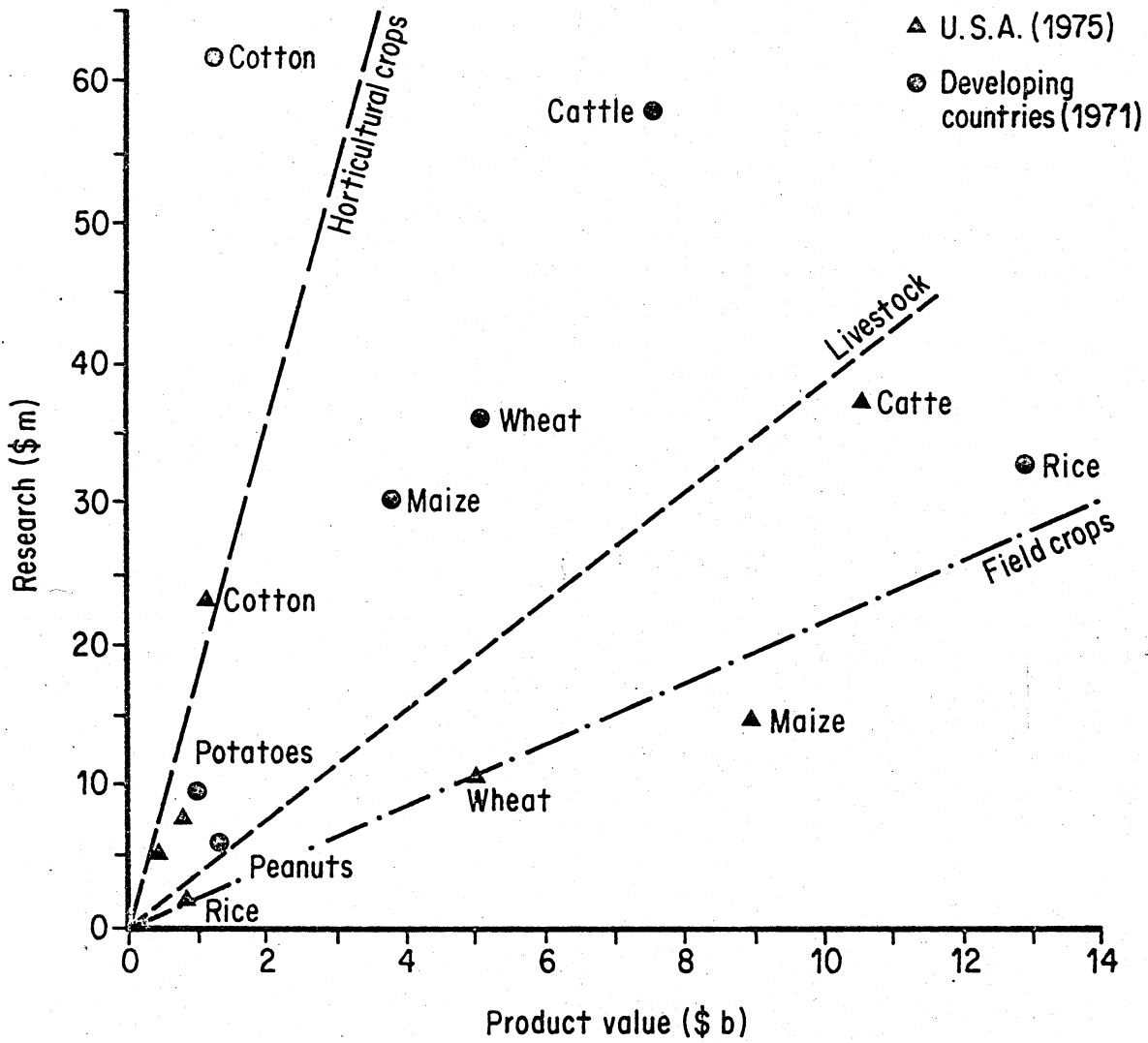
S_i = share of the commodity in the total value of output.

Commodity	A_i	S_i	$(A_i - S_i)^2$
Wheat	0.32	0.26	0.0036
Rice	0.10	0.13	0.0009
Cassava	0.09	0.17	0.0064
Cotton	0.14	0.09	0.0025
Cattle	0.25	0.17	0.0064
Poultry	0.08	0.13	0.0025
Horticulture	0.02	0.05	0.0009

In this example the congruence is relatively high. Perfect congruence would imply $C = 1$. Some indication of the levels of congruence in agricultural research budgets is shown below.

Region	1948-54	1969-74	1948-74
Northern Europe	0.838	0.924	0.866
North America	0.954	0.935	0.853
Oceania	0.878	0.925	0.915
Central America	0.592	0.776	0.695
Tropical South America	0.890	0.908	0.973
West Africa	0.668	0.619	0.686
South Asia	0.779	0.906	0.837

Figure 8: Congruence Between Research Investment and the Value of Output.



For example, a research center may be faced with the problem of allocating its research budget for a particular commodity across various regions. The following example is for sorghum research by ICRISAT. The institute has a responsibility for sorghum research in seven regions and is faced with the problem of how to allocate the research budget across these regions. Rather than just look at the value of output as the sole criterion, a total of 10 criteria were included reflecting concerns with both efficiency and equity. For example, per capita income in each area was used and highest priority given to the lowest income area. The lowest priority area was given a value of 0, the highest a value of 100, and the remaining regions expressed as a percentage of the highest priority region for the criterion. The criteria for scoring the region are listed in Table 1.

Once more than one criterion is used it is necessary to have some manner to make tradeoffs. This step is clearly subjective. However, it can be formalized by supplying a set of weights for the criteria in order to combine them in a single index. As there is no one single weighting scheme which is the "correct" one in any sense, four different sets of weights were applied. The last is the single criterion of the share of output used in the previous examples (see Table 2). The values of the index for both sorghum and pearl millet are given in Table 3. In the case of sorghum, allocation on the basis of the share of output would require much more attention to the Americas, whereas other weightings would lead to a more even distribution of the research budget with no attention on the Americas. In the case of pearl millet, all four criteria rank West Africa as the single most important region and the 1980 allocation of ICRISAT's resources was consistent with this. It is of interest to compare weightings based on equity (C) and the region's share of output (D). Both give prime emphasis to West Africa. Because of the importance of the crop and income and population, there is no conflict between efficiency and equity goals in research. In the case of basic staples this will frequently be the case. They will contribute a major share in the value of output and, because of their importance in the diets of low income groups, research directed to them can satisfy a goal of equity.

Table 1. Allocation criteria

Criterion	Highest priority	Justification	
		Efficiency	Equity
1. Income	Lowest income		X
2. Income growth/income	Lowest ratio		X
3. Population	Highest population	X	X
4. Population growth	Highest growth		X
5. Crop production growth trend	Lowest growth		X
6. Current food status (calories, protein, fat intake)	Lowest intake		X
7. Crop contribution to food status	Highest contribution		X
8. Regional contribution to SAT crop production	Highest contribution		X
9. Yield stability (R^2 of trend lines)	Lowest stability	X	X
10. Man/land ratio	Highest ratio	X	

Table 2. Weights used to derive alternative Congruence Indices

Criterion	Ad hoc	Efficiency 2	Equity 2:	Region's share of
	(A)	Equity 1 ^a	Efficiency 1 ^b	SAT production 1: Others 0 (D)
Income	1	1	1	0
Income growth/ income	1	1	2	0
Population	2	2	2	0
Population growth	1	1	2	0
Crop production growth trend	1	2	1	0
Current food status	2	1	2	0
Crop contribution to food status	2	1	2	0
Regional contri- bution to SAT crop production	2	2	1	1
Yield stability	1	2	2	0
Man/land ratio	1	2	1	0

^a Gives twice the weight to the efficiency criteria.

^b Gives twice the weight to the equity criteria.

Table 3. Congruence of various Index values for sorghum and pearl millet with actual ICRISAT research resource allocation in 1980

Crop/region	Index values (%) using as weights ^a :				ICRISAT 1980 principal scientist equivalents allocated
	Ad hoc (A)	Efficiency 2: Equity 1 (B)	Equity 2: Efficiency 1 (C)	Region's share of SAT production 1: Others 0 (D)	(%)
<u>Sorghum</u>					
India	22	22	18	35	35
E. Africa	19	19	20	10	10
W. Africa	18	15	17	15	51
S. Africa	14	15	17	2	0
Other Asia	12	13	12	1	0
N,C,S. America	0 ^b	0	0	34	4
N. East	15	16	16	3	0
Total:	100	100	100	100	100
<u>Pearl Millet</u>					
India	21	19	17	35	42
E. Africa	14	0	14	7	4
W. Africa	22	19	20	50	54
S. Africa	16	17	19	3	0
Other Asia	15	18	16	3	0
N,C,S. America	0	13	14	2	0
N. East	12	14	0	1	0
Total:	100	100	100	100	100

a. See Table 2 for weights used for each criterion in Table 1.

b. Regions having the lowest initial weighted index value are set to zero and each remaining region's percentage share of the SAT total index is calculated and presented in this table.

It was noted above that different commodities may require different research intensities; Figure 8 bears this out. Three distinct rays are evident, based on the data for the USA: field crops (such as wheat, maize, and rice), livestock, and horticultural crops. Data for developing countries are also shown. The same general pattern is repeated, whereby those commodities with larger values of total output ("economic size") are assigned larger research budgets. Note however that crops such as cotton appear to require higher research budgets per unit of output in both regions. Investment in maize and wheat research is higher per unit output in the developing countries. This is probably due to recent growth in the research budgets preceding the lag in the contribution to output.

Congruence with economic size is undoubtedly the simplest possible technique for allocation of research budgets across commodities or regions. It ensures that the average contribution of research is comparable in all commodities. However this is not necessarily the crucial element for efficient allocation of resources. What is important, is the return on an additional unit of investment. If this differs widely across commodities, then a case can be made for reallocating the funds toward those commodities which have higher expected marginal returns. The congruence model assumes that the opportunity for research to generate new knowledge to enhance productivity is equal across all commodities. An alternative way of thinking about this concept is to assume that nature is equally plastic (or niggardly) in yielding her secrets regardless of the commodity. Clearly, the amount of past research and hence the existing stock of knowledge about a commodity will often influence how difficult or easy it is to generate new knowledge.

A second assumption in the congruence approach is that the value of the new knowledge produced by research is proportional to the value of output. But this ignores the value added in processing, or the cost of particular inputs. In some cases, if research is directed to a particular factor (fertilizer, irrigation water) then the share of that input in total costs may be the appropriate measure for allocating the

research budget across commodities. Likewise, the monetary value of losses from various pests and diseases may be a useful guide to allocation of research effort in an insect or disease control unit.

SOME CASE STUDIES

Australia (Edwards and Freebairn)

This study estimates the benefits of research which lowered the per unit cost of production, (through improved productivity) by 10 percent. The value of B (present value of total benefits) was calculated assuming that the research produces benefits which reach full adoption over a five year period and last for 30 years. The results are useful for exploring how sensitive the estimates are to different assumptions.

Two classes of products are examined:

- (a) Those for the domestic market in which there is no significant foreign trade (apricots, potatoes, poultry, fluid milk);
- (b) Those which are exported (barley, sugar, wheat and wool).

In the case of the domestically consumed products, the present value of the benefits was estimated for three different levels of the discount rate and two sets of elasticities. The elasticities refer to the response of supply or demand to a change in the price. For example, a demand elasticity of -0.3 means that a 10 percent rise in the price will cause the quantity demanded to fall by 3 percent. A supply elasticity of 2.2 means that a 10 percent rise in the price will lead producers to offer 22 percent more output. Table 4 illustrates three important results:

- (a) The allocation of research funds is closely related to the economic size of the industry (the concept of congruence).
- (b) The results are quite insensitive to different levels of the supply and demand elasticities.
- (c) The benefits vary substantially depending on the discount rate with which is used. High discount rates penalize projects with

returns extending well into the future.

Table 4. Australia's Gross Benefits from Research which Reduced Unit Costs by 10 Percent in Four Domestically Consumed Goods

Industry	Economic Size ^a	Elastic Supply and Demand ^b			Inelastic Supply and Demand ^c		
		Discount Rates					
		0.5	0.10	0.15	0.05	0.10	0.15
	\$ m				\$ m		
Apricots	10	15	9	6	14	9	6
Potatoes	91	134	80	54	130	78	53
Poultry	184	272	163	110	263	158	107
Fluid milk	266	393	236	160	381	229	155

a Value of output: average 1975-76 to 1977-78.

b Supply = 2.2; Demand = -1.2.

c Supply = 0.2; Demand = -0.2.

The effect of the growth rate of the industry on the growth research benefits was considered (see Table 5).

Finally, if the effect of the cost reductions is lagged over shorter or longer periods the value of the benefits will differ. In the Table 6 it is seen if 10 years elapse before the full adoption of cost reducing technologies total benefits for poultry would be \$162 m. However, if that lag were reduced to five years the gross benefits would rise to \$197 m. The difference of \$35 m is an indicator of the amount which it would be worth spending on reducing the lag. Returns to investment in research are highly sensitive to the lag, and they can be increased greatly by more effective mechanisms for transmitting research results. The close link between research and extension is an important determinant of the returns to the investment in research.

Table 5. Australia's Gross Benefits for Research Which Reduced Unit Costs by 10 Percent in Domestically Consumed Goods; Three Different Growth Rates^a

Industry	Low growth	No growth	High growth
		\$ m	
Apricots	8	9	10
Potatoes	70	78	87
Poultry	158	158	197
Fluid milk	206	229	225

^aUsing a discount rate of 0.10 and the inelastic values for supply and demand given in Table 4.

Table 6. Impact of Different Adoption Lags on Gross Benefits to Research

Industry	Years to Full Adoption		
	1	5	10
		\$ m	
Apricots	11	10	8
Potatoes	104	87	70
Poultry	232	197	162
Fluid milk	305	255	206

^aUsing a discount rate of 0.1, inelastic values for supply and demand and high growth rates in the absence of research.

In general, the benefits of research in these domestically produced commodities will accrue to consumers, while in the case of traded goods producers will tend to be the principal beneficiaries. This contrast is illustrated in Table 7.

Table 7. Distribution of Benefits Between Consumers and Producers for Domestic and Traded Goods

Industry	Good	Consumers	Producers	Total
			\$ m	
Domestic	Apricots	5	5	10
	Poultry	99	99	198
Traded	Barley	12	336	348
	Sugar	17	656	673
	Wool	2	1100	1102

The benefit that a country captures from its own investment in research will depend in part on the "spillovers," i.e., the extent to which other regions and countries are able to borrow the results and reduce their unit costs. The effect on Australian benefits is shown in Table 8. Clearly, the optimal level of investment in research will be reduced quite substantially if, as a result of the research results, unit costs are reduced in countries which are competing suppliers on world markets.

Table 8. Gross Benefits to Research Reducing Australian Unit Cost by 10 Percent in Traded Goods^a

Industry	Reduction in unit cost among competing suppliers as a result of Australia's research		
	0%	5%	10%
			\$ m
Barley	511	429	347
Cheese	83	75	67
Sheep meats	404	363	322
Sugar	953	778	604
Wheat	1538	1225	913
Wool	1394	1105	817

^aUsing a discount rate of 0.05, inelastic values of supply and demand a 5 year lag and low rates of growth.

United States: Maize (Easter and Norton)

This study addresses two questions:

- (a) What information is required to estimate benefit cost ratios for future research expenditure?
- (b) Can, then, this information be used in the simple useful way that is not misleading, and does not require much time and analytical skills?

Table 9 sets out the information required. Several assumptions are made:

1. The discount rate is 10 percent.
2. Area sown remains the same before and after research.
3. Quality is not affected.
4. Price is constant at \$2 per bushel.
5. The probability of success is 0.8.
6. There are lags in adoption, as shown in Table 9.
7. The benefits end in the year 2000.
8. The research only has its effect in the north central region (for this reason there is not expected to be any significant decline in price).

Table 9. Information Needed to Estimate Returns to Two Types of Maize Research

Item	Unit	Improved Biological Efficiency	Crop protection
Scientist years	Number	2.5	3.0
Cost per scientist	\$,000	77	72
Base area	m.acre	55	55
Base yield	bu/acre	89	90
Change in yield by the year 2000	%	2	2.5
Research lag	Years	7	10
Adoption pattern:			
Year 1	%	30	30
2	%	50	60
3	%	75	80
4 and later	%	75	80

Yields are expected to rise by 2 percent by the year 2000, ie. (89 x 2 percent) or 0.11 bushels per year over 16 years. If the probability of success is only 0.8 then a total gain of 1.4 bushes per acre is expected by the year 2000. After a seven year research lag, 30 percent of the area is expected to benefit, and in that year have an increase in yield of 0.09 bushels on each of the 55 m area for a total increase in output of 4.5 m bushels. At \$2 per bushel this would represent gross benefits in that year of \$9.6 m. The stream of benefits is estimated in this manner and then reduced to its present value (B) by discounting at 10 percent. Likewise, the research costs of $2.5 \times 77 = \$0.2$ m per year until the year 2000 are discounted (C) and their ratio (B/C) gives the number of dollars of benefits generated per dollar invested in research. These values are shown in Table 10, both for the initial set of assumptions (see above) and for variants of them. This confirms that even under very conservative assumptions the benefit-cost ratios would be very high.

Table 10. Benefits Generated per Dollar Invested in Research

Assumptions	Improved Biological Efficiency	Crop Protection
1. Initial assumptions	137	118
2. With longer lags (10 years)	117	102
3. With lower probability of success (0.5)	86	74
4. With yield increases 50 percent lower	69	59
5. With all three less favorable assumptions	37	32

IRRI - Rice Research in Different Ecological Zones

Much of the early work on the International Rice Research Institute was focussed on irrigated rice culture. However, an attempt was made to estimate the optimal distribution of the research budget across a number of ecological zones. This attempt encompassed the following key elements.

1. Time taken to solve the problem (the research lag).
2. Expected benefits (Q x V) per ha.
3. Probability of success (P).
4. Area affected (A) in has.
5. Direct costs of using the new technology (C).
6. Changes in intensity in production.

so that

$$N = B - C = \sum_t [(Q_t \times V_t) \times P \times A_t - C_t] (1 + r)^{-t}$$

It was assumed that the probability of success, the research lag and the direct costs would be the same for each environment. Allowance was made for expected changes in the distribution of production in the absence of research. Irrigated culture was expected to increase relative to rainfed. This is analagous to allowing for different growth rates in various countries. By using a value of \$100 per ton the gross benefits were calculated (see Table 11).

Table 11. Estimation of Gross Benefits to Rice Research By Ecological Zone

Ecological Zone	Area in S. and S.E. Asia		Research Productivity		Gross Benefits	
	1970	1990s	Yield	Intensity	\$ m	%
	%	%	t/ha	crop/ha/yr	\$ m	%
Irrigated 4-7 mos	17	22	1.2	0.3	6400	32
7 mos	11	14	0.9	0.2	2725	14
Rainfed 4-7 200 m	30	25	0.8	0.4	4760	24
7 mos 200 m	3	3	0.8	0.3	296	1
Intermediate deep	15	12	1.0	0.3	1590	8
Deep	8	8	0.5	0.3	550	3
Dry	10	10	0.5	0.3	879	4
Arid, high temperature	4	4	1.5	0.5	2247	11
Long day, low temperature	1	1	1.3	0.3	203	1

The potential use of results such as these in guiding budget allocations can be seen from Table 12, which lead to a series of questions about the desirable level of funding for dryland research.

Table 12. Budget Allocation Compared to Expected Pattern of Benefits from Future Research

Zone	1977 Budget		Benefits from Future Research
	\$ m	%	%
Irrigated	2.2	41	46
Rainfed	1.7	31	33
Deepwater	0.3	5	3
Dryland	1.2	24	5
Other	n.a.	n.a.	13

The problem of deciding on projects within a commodity employs the same principles. The same set of questions must be addressed for each project.

- (i) What is the research lag?
- (ii) Which are the uncertain elements?
- (iii) What are the probabilities associated with these uncertain elements?
- (iv) How rapidly will the results be adopted?
- (v) How widespread will be the adoption?

The distribution of net benefits or rates of return from alternative projects can then be computed.

The problem of determining the optimal funding level is always a dynamic one. One does not have to decide that a particular project should be funded at a certain level and, having made the decision simply wait for the outcome. Each year there is a chance to reassess the decision, to review the progress, and incorporate new estimates of the likely benefits and costs. Some of the new information will come from the project itself and from trials on farms of preliminary results. Some will come from changes in costs and prices external to the project. The dynamic and interactive nature of the research management problem serves

to emphasize that informed judgment supported by some elementary estimates of likely payoffs is an appropriate strategy. The following bibliography includes many examples of project selection models. The literature is replete with such tools--but a search for useful applications yields a very small number. The simple principles of discounted benefits and costs together with uncertainty, on which most models are ultimately based, have been highlighted in this paper.

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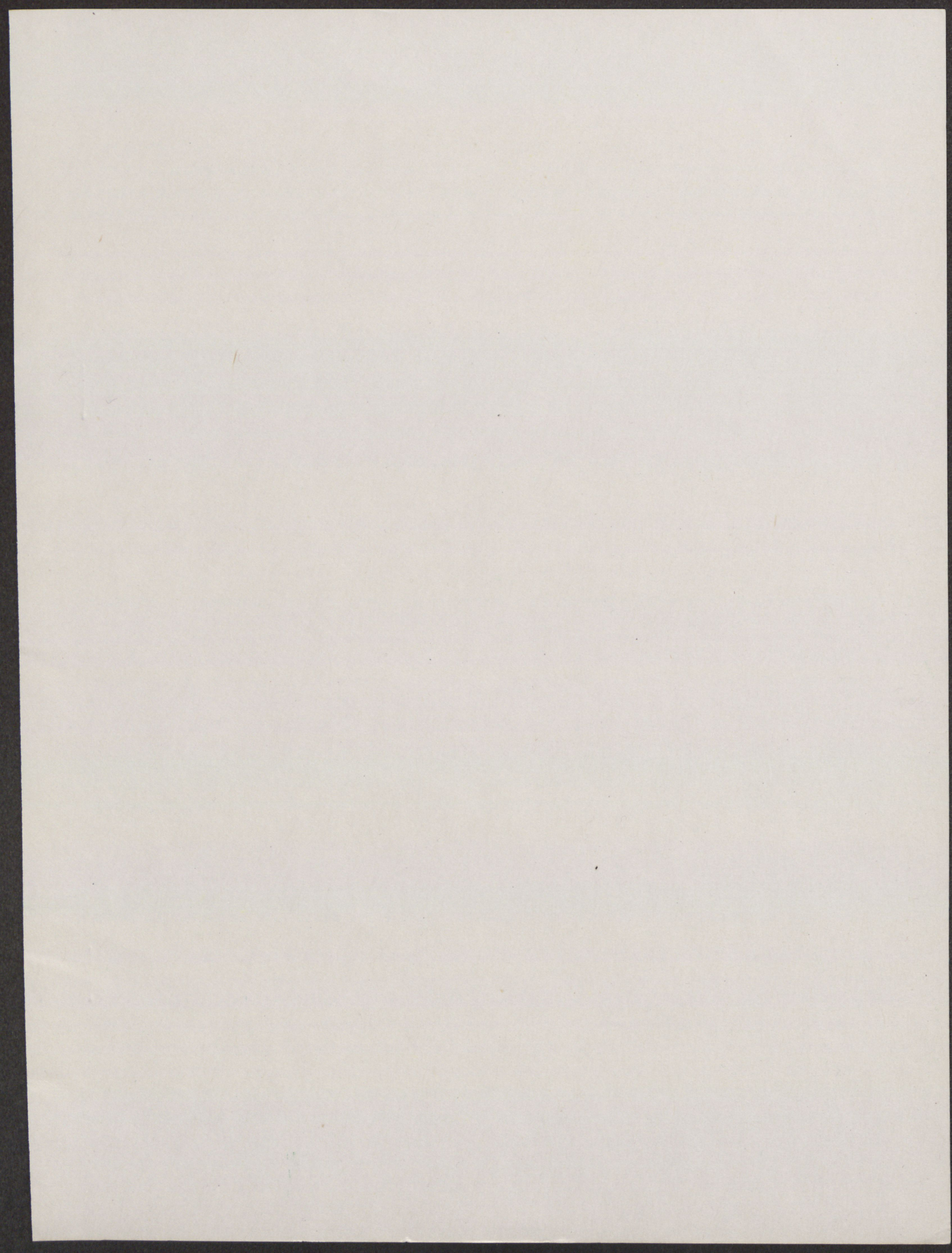
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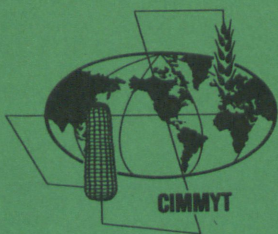
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