PN-AAP-233

THE ECONOMICS OF SYSTEMS RESEARCH*

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SUMMARY

There are three broad aspects of Systems Research worthy of particular consideration by professional personnel earning their keep within or about the agricultural research system. I will discuss these three aspects seriatim. The first *is the conjecture that the systems approach constitutes a technological change in our mode of thinking about the world of such magnitude as to imply that we are now moving from one socio-technical age to another. The* second *aspect, a corollary of the first, is the use of the systems approach as a framework for the selection of research programmes. The* third, *which also follows from the first, is the efficiency of conducting research on a systems basis. Many would think of this last aspect of efficiency as constituting 'the economics of systems research', but I believe the systems approach, via the first two aspects I have mentioned, has far wider implications of economic import for agricultural research.*

1. AGRICULTURAL RESEARCH IN THE SYSTEMS AGE

This section draws heavily on the contributions of Ackoff (1973), Ackoff & Emery (1972) and Weiss (1972): to varying degrees, it also reflects the work of Dror (1971), Georgescu-Roegen (1971), Heady (1973), Heilbroner (1972), Hightower (1973), Jordan (1973), Klir (1973), Kuhn (1970), Lowe (1972), Weiss (1971) and Williams (1973).

As a prelude to looking at agricultural research *per se*, we need first to appreciate

* Revised version of a paper presented to Agricultural Systems Research Conference, Massey University, 20–22 November, 1973.

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Agricultural Systems (1) (1976)—© Applied Science Publishers Ltd, England, 1976
Printed in Great Britain

the broad scientific implications of the systems approach. In themselves, these implications are dramatic and far reaching. Before the advent of systems thinking over recent decades, scientists tended to derive their understanding of the functioning of the whole from the mechanical structure of the parts. The scientific mode of thinking about, and endeavouring to understand, the world was in terms of the two concepts of *reductionism* and *mechanism*.

Reductionism implied reducing phenomena to their more basic (and hopefully independent) parts, analysing these parts as independent entities to explain their behaviour, and then aggregating these explanations as an explanation of the phenomena under study. Any decent problem was tackled by reducing it to a set of simpler component problems whose solutions were seen, in aggregate, as a solution to the whole. *Ipso facto*, reductionism abetted and fostered the proliferation of specialised deafness and tunnel vision in the form of independent scientific disciplines. The growth of science via reductionism resulted in the categorisation of phenomena into smaller and smaller classes with each of which was associated a specialist discipline—and, as disciplines multiplied, each increased in depth while decreasing in breadth. At the same time, the narrowing and deepening of each discipline's interests moved it further and further away from the felt problems of the real world.

The idea of mechanism implied that phenomena could be explained in terms of mechanical or machine-like cause and effect relationships. Aided by Occam's Razor, explanations were pared to the simplest 'adequate' causes. The influence of the environment was ignored by developing environment-free scientific laws or explanations in specially designed environments called laboratories (or research stations). Such closed-system thinking led to a view of the world as being both *deterministic* and *mechanistic*. Effects had only direct physical causes and the world was just a machine to be analysed as one might dismantle a car. There was no place for concepts of a teleological (i.e., purposeful) nature such as goals, choices and free will. The teleological view that effects may be due to the purpose they serve was considered to be outside scientific interest. In consequence, as a concomitant to their deterministic and mechanistic view of the world, scientists had no concern for phenomena involving goal-seeking and goal-setting behaviour.

Increasingly since the 1950s, reductionism and mechanism have been seen as an inadequate basis for science in its attempt to understand the world so as to enable purposeful manipulation and control. *Expansionism*, *teleology* and *synthesis* are now being recognised by science as necessary modes of thought in understanding the world. Stated another way, science today increasingly tends to gain an understanding of the structure of the parts from an understanding of the functioning of the whole.

Expansionism is the reverse of reductionism. It denotes the view that all objects and events are parts of larger wholes and emphasises the whole rather than the part by focusing attention on the system as a set of interrelated parts or elements.

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These parts may be of any kind—concepts, physical phenomena, objects, people, etc.—and as elements of a system have the following two properties:

(i) Each part affects the properties of the system as a whole.
(ii) Each part depends for its own properties and for how it effects the system on the properties of some other part(s) of the system.

By virtue of these properties of its parts, a system is an indivisible whole and, in contrast to the reductionist view, is more than the mere sum of its parts in the sense that the system's behaviour is not deducible from the behaviour of its parts considered in isolation. Of course, as we all know, virtually every system we may care to delineate will be a part of some larger system and will itself have parts that can be viewed as systems.

Such a (systems) mode of thought leads naturally to synthesis or the *systems approach* whereby parts or phenomena to be explained (or understood or manipulated) are seen as part of a larger system or systems. Explanation then proceeds in terms of the role or function of the part in the larger system(s). System performance must therefore be judged not simply in terms of how each part works separately, but in terms of how the parts fit together and relate to each other, and in terms of how the system relates to its environment and to other systems in that environment.

The systems approach has obvious implications for the organisation of science and research. Where reductionism implies the specialised deafness of disciplines, expansionism and synthesis imply openness and 'teamness' through interdisciplinary endeavour. Only such a holistically-oriented approach can lead to the capturing of adequate understanding of a system for purposes of improving performance. Despite the traditional disciplinary organisation of science, the world does not come to us in a disciplinary form.

There are also profound methodological implications of the systems approach. As soon as we recognise that physical systems are embedded in, or interact with, social systems, we recognise that science—conceived in terms of understanding phenomena for purposes of manipulation—can no longer be free from value judgements. Social systems involve not merely the interactions of physical forces but also contests of will arising from the purposiveness of behaviour of animate elements in the system. Even without contests of will, teleological considerations imply a non-determinism and capriciousness of behaviour that can only be captured by subjective judgement and not by objective fact. Within the systems approach, therefore, value-free science will inevitably be diluted with some value-laden elements.

At the same time, if—as has been hypothesised by Ackoff (1973)—we are leaving the Machine Age and entering the Systems Age, the traditional hypothetico-deductive method of science, with its emphasis on cause and effect, will not have the field to itself. Of course, the traditional method (i.e., in the fashion of those great 17th century thinkers such as Sir Francis Bacon and the editors of many agricultural

science journals, the old (supposedly sequential) routine of (i) observation, (ii) hypothesis, (iii) deductive prediction, and (iv) performance testing based on the cult of arbitrary levels of statistical significance) has never really been applied in strict sequential fashion (Medawar, 1967). The teleological or *means–ends approach* must also be recognised as a valid scientific method. Social systems or systems with social effects, because of their intrinsic purposiveness or the purposiveness of a significant part of their environment, will increasingly require a means–ends approach to research. By this I mean the stipulation of a target and assessment of alternative policies as to how the target might be achieved given whatever initial conditions may be specified. Such a normative research orientation implies both an expansion of knowledge (how to get what we want) and problem-solving (how to be better off). To force it into the hypothetico-deductive frame is tortuous and artificial. Far better to recognise it for what it is—a teleologically based and valid approach to research involving goal-seeking and goal-setting systems. By their nature such systems are of a higher order than passive and reactive systems.

In the sense that lower-order systems are used as instruments by higher-order systems, systems may be classified (Ackoff & Emery, 1972; Jantsch, 1973) from lower to higher order as:

Passive: a system that is incapable of reacting to its environment (e.g., a clock).

Reactive: a system that can display different behaviours in different environments but only one kind in any one environment (e.g., a servomechanism).

Goal-seeking: a system that can respond in different ways in any one environment, these responses having the function of producing a particular outcome which is its (exogenously given) goal (e.g., an unmanned mooncraft).

Goal-setting or purposeful: a system that can change its goal in constant environmental conditions or can pursue the same goal by changing its behaviour in different environments, i.e., it displays will by selecting goals as well as the means by which to pursue them (e.g., a manned mooncraft, a farm, the agricultural research system).

A further implication of all this is that physical scientists, by virtue of their concern with lower-level (i.e., passive and reactive) sub-systems, will increasingly have their terms of reference set by social (including biological) scientists concerned with relevant higher-level social systems of a purposive nature. In turn, social scientists will have their terms of reference set by politicians employed by the overriding purposive system. Science will certainly have reduced opportunity for operating in a spirit of divine right, although it probably never did quite operate this way.

Without doubt, I have used a broad brush in the above sketch of how I and some others see science presently progressing. Personally I believe a shift from the Machine Age to the Systems Age is fact, not conjecture. Only on this basis is it possible to rationalise the many straws in the wind within and about science—straws that include, for example, a shift away from inference to decision, an increasing appreciation of the inescapability of subjective judgement, the scientist's plain-

tive cry of 'What are we here for? To solve problems or do research?', a developing awareness that to use resources in one piece of research means something else cannot be done, the growth of interdisciplines (such as cybernetics, information theory, operations research, computing science, etc.) concerned with purpose, the growing use of task forces, a concern with National Science Policy and the social function of science, and an increasing recognition that Man does not live by bread alone.

What of agricultural research in the Systems Age? I believe the systems approach, viewed as a new and improved technology for our research, has substantive implications in three directions. These lie in the areas of professional training, research organisation, and research directions. To me, these are the three major macro-economic aspects of the agricultural research system. Decisions made between and within these three areas will be the major controllable influences on how well agricultural research serves whatever purpose it is supposed to serve. Just what this purpose may be I leave until later.

Professional training

In terms of the professional training of agricultural scientists, the systems approach obviously implies the need for an appreciation of systems concepts and procedures of research, particularly in relation to the agricultural system and its environment. As Klir (1973) emphasises, purposive elements within and without the system need to be stressed because they will dictate the relationship of the professional to the system and his role in it. Training must also aim at versatility so that the future professional is not tied to a disciplinary straightjacket.

That Man—not cations or nodules or rumen flora or crop varieties or livestock species or dollars—consummates the system must be a basic text. Along with this must go an appreciation of lower-order systems as instruments of higher-order systems. Teleological concepts and the means–ends approach to research should be introduced as a necessary corollary to the inadequacies of the old hypothetico-deductive method of research as a means of assessing goal-seeking and goal-setting systems.

Disciplinary emphasis in training must be seen to be justified largely as a necessary prior background for interdisciplinary teamwork—and this message should also be transmitted to those being trained. Professional training, moreover, should not be in terms of disciplinary majors but in terms of systems majors—not biochemistry, soil science, agricultural economics and such like, but the crop–soil system, the plant–animal system, the production system, the communication system, the rural political system, etc., not forgetting the agricultural system *per se*.

The difficulty, of course, is that the systems approach is new and somewhat unfamiliar, while the disciplinary approach to training is old and familiar. Just like any other piece of new technology, albeit as yet in short supply, it will be adopted

gradually with the usual staging process from innovators to late adopters. Nor would I expect the rate of adoption as a technology for professional training to be rapid. After all, academics are perhaps the most conservative purposive elements in the agricultural system! In addition, the systems approach to training—unlike much other new technology—is not something with an easily quantified benefit-cost ratio. It lies outside the price system. With cost-return qualifications to be entered later (Section 3), economic justification of the systems approach resides in its inherent logic as an approach to understanding and, *ipso facto*, in the inadequacies of reductionism and mechanism.

Despite all these difficulties to adoption, in the end I would see a significant shift in the system of professional training. Instead of the present typical pattern of a broad (one or two-year) training base leading only to disciplinary specialisation in later years, I expect the development of patterns involving an initial (one-year) introduction to the systems approach combined with basic disciplinary foundations, followed by a (two-year) period of disciplinary specialisation, capped off with a (one or two-year) bringing together of different disciplines in the context of some relevant agricultural system.

Research organisation

Little needs to be said on the need for shifts in the organisation of research. What is required is a structure which will facilitate a synthesising, integrative, team-oriented outlook rather than one that is analytical, compartmentalising and disciplinary. As Heady (1973) (p. 941) puts it, in talking of Land Grant Colleges in the USA: 'Over time the tendency has been for disciplines to dig deeper moats around themselves and to retreat further into their departmental bastions; while physically adjacent, their deepened discipline barriers prevent simultaneous attacks on the major facets of relevant problems. In fact, furtherance of the discipline typically is taken as more important than the solution of people's problems. This situation could be changed by administrative structures that give problem sets [systems] as much control over resources as the disciplines now have. Administrative control could be viewed in the context of a matrix where the rows are problem sets [systems] and the columns are disciplines.'

With a systems focus, sole reliance on disciplinary units burrowing ever-deeper into virtual non-problems must inevitably go by the board. The systems approach immediately leads to recognition that the overall agricultural system is a purposive one involving physical, biological and social parts, and that it operates within an environment having significant purposive components. Goal-setting elements within and without the system will dictate the need for multidisciplinary groups to work as *systems teams* on problems they (not the scientists) judge to be significant. Moreover, unlike traditional disciplinary units, these interdisciplinary teams will not be permanent. They will need to be formed, reformed and regrouped as required for the solution of particular problems at particular times in the context of the

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overall agricultural system or of particular sub-systems. Their membership, too, will be more diverse than that typically represented by the disciplinary cubbyholes of agricultural research institutes. Recognition of the key role of purposeful elements in the system or its social environment will lead both to enhanced recognition of the role of extension and to the participation of management and social scientists (particularly economists, sociologists and political scientists) as members of many research and/or extension teams. Far more so than at present, there will be a natural vertical integration of research and extension activities. Further, the higher the level of the system under study, the more will the hypothetico-deductive method of study be complemented—and eventually dominated—by the means-ends approach.

As in the area of training, transition to a systems-oriented basis of organisation will not be easy. It will have its costs and, particularly while professionals trained in the systems frame are in short supply, its difficulties.

Research directions

As soon as we look at a piece of agricultural research either in the context of the higher systems in which it is embedded or in terms of the lower systems which it influences, we can see implications which go unrecognised in the traditional disciplinary approach to research. The reason is that optimisation or improvement to merely a part of a system or a sub-system cannot be presumed to lead to enhanced performance of the overall system—indeed it will generally result in overall sub-optimisation. I can speak most easily of such effects in the socio-economic spheres of productivity, growth, security and equity but similar implications must also exist in the physical and biological spheres. However, given Man's dominant importance—either as a producer or a customer—in relation to the agricultural system, it is implications in the socio-economic sphere that are most important. Let me therefore concentrate on the social sphere.

Traditionally, agricultural research has been production oriented. [I ignore the fact that some agricultural scientists have carried out pure research when they were being paid to do agricultural research. In the farmers' heaven, I expect their sin to be seen as grievous.] The goal and the result have been either the same output from fewer inputs or more output from the same inputs. To no significant degree has our research been oriented to encompass aims of social justice for farmers or domestic consumers. Benefits in the main have gone to well-to-do producers. The poor and disadvantaged, whether inside or outside agriculture, have been neglected, if not positively disadvantaged. In the USA, for example, the development of cotton varieties specially adapted to the Southwest, along with the development of irrigation there, led to a much more efficient production of cotton. As a result, cotton production shifted westward and whole communities in the Southeast were left with unemployed farmers and workers, not to mention shopkeepers and parsons. Many of the displaced migrated to city ghettos in search of non-existent

jobs for which they lacked training anyway. The net result was that a piece of agricultural research which was excellent from a disciplinary point of view generated great socio-economic inequities and added further to already overcrowded city slums. As some have argued, it is indeed a tenable view that the terrible riots of a few years ago in Watts, California, were basically caused by the 'success' of production-oriented research which led to the displacement of agricultural labour in the USA—for example, the mechanical cotton picker, hard tomatoes, etc. The results of such research gave too much to the wrong people and ignored significant (human) costs.

The first part of my argument is that the systems approach can provide an indication of where research is needed—not just in the trivial sense of being able to say to a plant physiologist that we need an estimate of such and such a coefficient, but in the far more significant sense of recognising disadvantaged regions or groups and problems of farmer and rural equity or security that lie outside what has been the traditional focus of research interest. We need to recognise—as a systems approach emphasising purposive higher-level systems makes explicit—that there is more than a material product to agriculture. It may be that in the future the question to be answered will be 'How much would it cost, how much are we willing to pay and where should the money be spent in order deliberately to foster a less economically efficient system of agriculture whose social values add greatly to our entire national quality of life?' That question is already a live one in the USA. Its evaluation will certainly need a systems approach.

The second part of my argument is that the systems approach provides a logical and workable procedure for obtaining a guide to the likely ramifications of research.

Williams (1973) (p. 92) suggests an example: 'Of course we need new wheat varieties . . . but such plant breeding programs may be improved if we can see the problems of wheat growing as problems of farmers in particular environments trying to adapt to changes. To do this we should explore the potential for new varieties to meet particular specified needs, the role of new crops, the scope for changes in farm size, and the related problems of access to capital to do this, as well as the changes in the biological environment, and in educational systems and social structure, which these adjustments may involve.'

Some of these effects will be good, some will be bad. Some will occur within the higher-order or related systems. Some will be significant, others not. But unless they are foreseen and appropriate steps taken where required, either in the form of redirecting the research or providing some form of cushioning for those to be disadvantaged, agricultural research will fall into disfavour. Moreover, as economic progress proceeds, the interdependencies of the socio-economic system will also grow, but at a faster rate. This will increase the likelihood that the gains and losses from new production technology will not be shared equally by society's members. So the problem of assessing the possible effects of such research is likely to become more difficult and the need for a systems approach more urgent.

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Through an interactive cyclical approach of looking at needs and likely effects, required programmes (i.e., bundles of projects) can be specified relative to system goals. Overall goals will be given explicitly or implicitly by the political system but here, too, choice can be assisted via broad systems appraisal. Such an approach, under the name of Planning, Programming and Budgeting (PPB), led to the specification in 1967 of the four broad goals presently recognised by the US Department of Agriculture (Bayley, 1971; Fedkiw & Hjort, 1967). These goals are stated as follows:

(1) Income and Abundance—to achieve a sustained and balanced agricultural abundance with fair income for our farmers.
(2) Growing Nations—New Markets—to provide new markets for our food, feed and fibre and to help growing nations win the war on hunger.
(3) Dimensions for Living—to expand the dimensions of American living and specifically to wipe out undernutrition in America.
(4) Communities for Tomorrow—to build livable and healthy communities for tomorrow by revitalising rural America and restoring the rural–urban balance.

These four basic missions or goals are divided into subcategories as follows:

(1) Income and Abundance
    - Farm income
    - Agricultural marketing and distribution system
(2) Growing Nations—New Markets
    - Food for freedom
    - Export market development
    - Agricultural development
    - International agricultural services
(3) Dimensions for Living
    - Diets and nutrition
    - Health and safety
    - Education and training
    - Services for tomorrow
(4) Communities for Tomorrow
    - Community development services
    - Housing
    - Public facility and business expansion
    - Resources protection and environmental improvement
    - Recreation, wildlife and natural beauty
    - Timber

These subcategories are further subdivided into programme elements and it is only at this level (and lower) that research categories of the traditional type (e.g.,

livestock improvement, grazing management) begin to appear. This is an interesting reflection of the fact that sub-systems serve as instruments of higher-order systems and that, as I have already noted, the ends of purposive higher-level systems will set the terms of reference of scientists working on lower-level sub-systems.

2. THE SYSTEMS APPROACH TO PROJECT SELECTION

My thinking on this topic has been influenced by the contributions of Davidson (1973), Fishel (1971, 1973) and Lifson (1972).

My remarks will be limited to applied (i.e. real!) research on agriculture and to *ex ante* appraisal of such research. Some work has been done on *ex post* appraisal of agricultural research, generally indicating handsome first-order financial returns but neglecting social costs and second-order financial effects. For example, Schmitz & Seckler (1970) estimated a gross return of 929% on US research and development expenditures that yielded hard tomatoes suited to mechanical harvesting. They also noted that such mechanical harvesting displaced 19 million manhours of labour annually, the economic and social costs of which need also to be taken into account. However, such work is of no assistance to *ex ante* choice between research alternatives. The real problem is the *ex ante* one. That it is a significant problem is indicated by the spate of recent conference proceedings (Davidson, 1973; Fishel, 1971; OECD, 1970) and other literature (see Anderson, 1972) concerned with the allocation of resources to agricultural research. The essence of the problem is limited funds in the face of virtually unlimited research possibilities. The basic question to be answered is: 'Which projects and at what levels?'

Discussion on particular procedures such as benefit–cost and cash flow analysis aimed at answering these questions constitutes a not-insignificant slice of the recent management science literature. There are even two journals (*R & D Management* and *Research Management*) concerned with nothing else. Obviously I am not going to solve the problem of choice between alternative projects here. But, as evidenced by the work of Fishel (1973) and Pinstrup-Andersen (1974) I am convinced that the systems approach offers the opportunity of providing more complete and more reasoned answers than other procedures. While these other procedures (reviews and a listing of some relevant literature are given, for example, by Gilchrist (1973), Morgenthaler (1973), Pearson (1972) and Ritchie (1970); see also Parts IV and V of Fishel, 1971) may be utilised within a systems approach, in themselves they are inadequate relative to publicly-funded research (although they may be satisfactory enough in a purely commercial context). Why? Because in not taking a systems approach to appraisal, they inevitably give only a partial assessment.

As a sub-system of the (purposive) national system, the agricultural research system should basically be a hierarchical one with decisions relative to national

research goals being fed downwards—which is not to say, of course, that there should not be opportunity for upward feedback and suggestion so that goals can be conditioned by the perception of means. At each hierarchical level the decision on 'what to do' should come from above. This should then lead to the taking of a 'how to do it' decision which is passed down to the next level as a 'what to do' order. To the extent that a research system is not organised in this management fashion, it can only be meeting national goals by accident!

The 'how to do it' decision is a problem of risky choice. Any such problem involves a choice between alternatives whose payoffs are uncertain in that they depend on what state of the world prevails after a choice is made. As judged by the decision-maker, these different possible states of the world will each have some probability of occurrence. For example, the 'what to do' instruction may be to investigate the eradication of a certain insect pest. The 'how to do it' alternatives to be investigated might be a chemical pesticide, a biological control system, a sterile-male technique, or some combination of these. Each of these research alternatives will probably have quite different payoffs according to their costs and benefits and to which state of the world eventuates. For each alternative there will thus be a subjective probability distribution of payoffs. The best alternative will be the one whose probability distribution of payoffs is most attractive to (i.e., most preferred by) the decision-maker. Note that in such a decision problem it is impossible to avoid judgement. To varying degree, judgements just have to be made as to: (1) what alternatives are available or should be considered; (2) what states of the world may prevail; (3) what the chances of these states are; (4) what constitutes the costs and benefits contributing to the payoffs; and (5) which probability distribution of payoffs is most preferred in terms of the goal(s) that generated the decision problem.

Any decision problem can be put in the above context of alternative choices and possible outcomes under different eventualities. Such a paradigm renders explicit how rational decisions are implicitly made and can be used as an aid to decision-making.

Consider now how the systems approach can contribute to the problem of choice between research alternatives. In doing so I presume we have available a model, however tentative and poorly specified, of the system relevant to our research decision problem. In general this system will be some sub-system of the overall agricultural system. Certainly the pre-research model we have to work with in making our choice between research alternatives will not be complete (in the sense of what we would like to have) since our problem is to decide what further research to undertake so as to enhance performance of the system. If we already knew all about the system, we would have no need to carry out any research! What the model can do adequately, however, to the degree of detail judged relevant, is specify our state of current knowledge about the system.

By reference to the model we can then specify possible action (research) alterna-

tives by a process of synthesis. By its nature, the systems approach applied to the systems model indicates possible points of control or manipulation or adjustment relative to the goal(s) of interest. To what extent this control, manipulation or adjustment can be undertaken and how it will actually influence performance is the question of research embedded in each alternative. One alternative, of course, may be to design a completely new system and another should be to do nothing. Further, the specification of alternatives must include consideration of budget constraints. Of itself, no one alternative can involve a research cost in excess of the available budget. To the same end, a set of complementary projects should be appraised initially as a single alternative.

Solution or running of the pre-research model of the system relative to each research alternative provides an estimate of the size (magnitude) and dimensions (dollars and non-financial) of the outcomes for each alternative. Outcomes may involve many things: e.g., farmer incomes and their distribution, lower food prices and consumer savings, import savings and export earnings, farmer satisfaction with higher yielding crops and animals, displacement of farmers, closure of schools, stress on cities, promotion of researchers, etc. Some of the difficulties of measurement of costs and benefits are discussed by de Veer (1971). These outcome estimates for each alternative will be answers to questions of the type: 'In the various dimensions of relevance, what net system outputs would be expected under the possible states of the world that may occur?' The states of the world across which the outcomes may lie will be a compound of two factors: (i) the stochasticity of the system's operation as determined by its internal relationships and its environment (either because it is that way or because we are uncertain) and (ii) the degree of success that may be attained in manipulating or restructuring the system via each alternative line of attack. This latter question of degree of success, of course, indicates that particular research alternatives should not be specified merely as, for example, 'a chemical pesticide project' but as 'a chemical pesticide project using $\$x$ over y years'. In this fashion a number of like projects varying in intensity and time span could be specified as relevant alternative actions. The question of degree of success also involves questions of real-world effects and their time pattern, not just success in research. As de Veer (1971) emphasises, adoption in the real world is a crucial factor and hence implies considerations of communication and extension.

Having used the systems approach in this fashion of synthesis to generate alternative research projects, followed by solution to specify the probability distribution of outcomes for each alternative, it only remains for the decision-maker to choose the preferred probability distribution of outcomes. The corresponding alternative, by definition, will be the optimal project choice.

Compared with other approaches to project selection suggested, the systems approach has four broad advantages. First, it provides greater completeness in the sense of bringing possible alternatives and the dimensions and directions of their consequences to attention. It is less likely to err in allowing for the opportunity cost

(i.e., foregone opportunities) associated with the choice of any particular alternative. Second, it facilitates and helps to make explicit the judgements that are unavoidable in project selection. Third, it is goal-oriented and recognises the hierarchical purposiveness to which the research system should respond as an instrument of the overall agricultural and national systems. In this sense, the systems approach recognises that it is more important to have the 'right' objective than to make the 'right' choice between alternatives. While the wrong alternative merely means something less than best will be chosen, the wrong objective means the wrong problem is being studied. Fourth, the systems approach to project selection can be applied at any desired feasible level of sophistication or detail. At the simplest it might involve back-of-an-envelope scribblings; or it might involve the most complex specification of a model reflecting current knowledge, together with detailed synthesis of alternatives and analysis of outcomes using discounting, simulation, Bayes' Theorem, decision trees, multi-attribute utility functions, etc. I will not, however, endeavour to go into all that here. It is the orientation (philosophy) that is important, not the details of sophistication (technique). Nor will I attempt to explore the complexities of multiple-goal situations or interdependencies between alternative projects. Like the assessment of commensurable, incommensurable and intangible benefits, these are difficulties to be faced under any selection procedure. By the logic of the systems approach, they will be best brought to attention and handled in a systems format. Sufficient to say that there are pointers to the resolution of such difficulties.

3. EFFICIENCY IN SYSTEMS RESEARCH

As a basis for discussion, let me first outline what I mean by efficiency and by systems research.

Efficiency

Efficiency can only be defined in reference to goals and presumes that gross benefits exceed gross costs, however measured. If we have only a single goal, we are efficient when we achieve the goal at least input cost, or, if input cost is constrained, we achieve the greatest possible degree of goal attainment with the limited inputs available. Relative to multiple goals, efficiency prevails when input costs are distributed efficiently relative to each individual goal subject to the constraint that aggregate satisfaction across the goals cannot be increased by shifting inputs from one goal to another. By their nature, goal-seeking and goal-setting systems will aim to be efficient. Note, too, that the *ex ante* efficiency of stochastic systems can only be considered in probabilistic terms.

Since most scientists (including some economists) are confused about efficiency in the sense that they think it has only a monetary or physical connotation, I must

emphasise that in my view neither the goals nor the input costs will always necessarily be measured in money or physical terms. In many situations non-monetary goals will be relevant (although it will generally be useful to know their financial opportunity cost) and inputs will often involve non-priced activities. As a result, efficiency may sometimes be a matter of judgement. Both goals and costs may involve incommensurate, if not intangible, aspects so that personal judgements are inescapable and what seems most efficient to one decision-maker may not be so to another. Given these difficulties, in discussing the efficiency of agricultural systems research, I will be implicitly referring to whatever goals are specified by the purposive nature of the research system and the systems which it serves. These system goals have often gone unrecognised by researchers who, at least in the past, have tended to misinterpret the overriding goal of agriculture as maximum physical production and to emphasise personal goals of scientific status in their disciplinary peer group.

Systems research

This, I feel, is best defined (after McGrath *et al.*, 1973) as the process of studying a system through: specification of relevant system, system performance and environmental variables; determination of the existence, degree and form of relationship among these variables; and use of this information to arrange or redesign the system so that it operates optimally (i.e., most efficiently) with respect to its objectives. Such a process implies a closed-loop cycle of four research stages in each of which the three mutually-dependent research functions of model development, information collection, and information synthesis are carried out. The first research stage emphasises development of a research model so as to enable delineation of system performance requirements; the second aims at the synthesis and interpretation of information in order to assess the design consequences of the performance requirements: the third the development of amended or redesigned models and the fourth the evaluation of the amended system's performance. The need for a multidisciplinary team approach is implicit in such a research process. Without a suitably diverse team, it would be impossible to adequately handle the research functions of model development, information collection and information synthesis for any agricultural system worth researching. Further, insofar as the benefits of agricultural research will often hinge on farmers' actions, systems research teams should involve personnel with an orientation to farm management and extension—even farmers themselves in some cases—and not just those traditionally labelled as agricultural scientists.

Given the above definitions of efficiency and of systems research, efficient systems research implies:

(1) that the gross benefits, however measured, of the research exceed its costs; and

(2) that the distribution of effort and resource between the four research stages and across the three research functions within each stage should be such that the research goal cannot be better achieved by some other allocation.

This, of course, is a mouthful of a statement and begs a number of questions. First, it assumes adequate specification of the research goal. In my view this is the responsibility of a higher echelon in the agricultural system and should come down to the research team as a 'what to do' instruction. Moreover, this instruction should include an initial specification of the total resource to be used over some defined time period. Second, the statement assumes knowledge of costs and benefits. Both of these will be uncertain—markedly so on the basis of experience. However, if the project has been selected on rational grounds of expectation (as discussed in Section 2), we must assume its benefits are expected to exceed its costs. Third, appropriate delineation of the relevant system is assumed. This is no easy problem, choice of system boundary being one of the most important determinants of the scope and intensity of the systems research effort and yet one on which there are few guidelines. Like others, this decision must involve judgement. What is sure is that agricultural research needs to be conceived and appraised in a context extending beyond traditional disciplinary boundaries. Fourth, the statement implies that it is possible to estimate the gain from marginal units of investment in each systems research function at each research stage. This, of course, is impossible because quantitative data will not be available and the outcome of each activity will be uncertain. Lacking precise data, all that can be done is to make judgements. In terms of hindsight, these judgements will not be error-free. They should, however, go some way to the avoidance of gross errors—such as, for example, the predilection of some researchers to over-invest in the development of design models and of others to over-invest in the collection of information for the evaluation of the design model's performance.

From a research management point of view, systems research demands a systems approach, i.e., an approach based not on disciplines or functions, but on goals. The systems approach to management has come to be known as PPB—or Planning, Programming and Budgeting—and has been discussed in an agricultural research context by a number of authors (Bayley, 1971; Fedkiw & Hjort, 1967; McGregor, 1971; Puterbaugh, 1971, 1973). PPB stands in direct contrast to the traditional approach of budgeting research on an incremental basis with a bit more or a bit less this year than last year. Under PPB the orientation (if not the actuality) is to zero-base budgeting. Each year every budget item stands on trial and there is no presumption that last year's budget provides the baseline guide for this year's (Hannah, 1973). With PPB, budget planning involves the determination of objectives, the evaluation of alternative courses of action and the consequent authorisation of select programmes specified in terms of a budget and a time-expenditure pattern. In terms of research management, PPB implies the programming of

approved goals into specific goals and activities, followed up by the design and implementation of organisational units or teams to carry out the approved programmes under regular review arrangements. As Schick (1971) puts it: 'Traditionally, budgeting has defined its mission in terms of identifying the existing base and proposed departures from it: "This is where we are; where do we go from here?" PPB defines its mission in terms of budgetary objectives and purposes: "Where do we want to go? What do we do to get there?" The environment of choice under traditional circumstances is incremental; in PPB it is "telestic".'

In parallel to the purposive and hierarchical nature of the agricultural system, the decisional process under a PPB approach to research management is disaggregative downwards. Policy at the top constrains choice at the bottom. In contrast, the traditional (incremental) approach gives a flow of budgetary decisions that is upward and aggregative, with a consequent research pattern that is exceedingly sluggish in response if at all guided by top-level goals.

So much for the efficiency and economic management of systems research *per se*. From an economic point of view, how does the systems approach compare with the presystems approach? In my judgement, used rationally the systems approach must be much better. For a given outlay of research resources, it must yield a greater return. My reasons for this belief are that with a systems approach:

(a) Research is more purposeful, there is less danger of working on the wrong problem, and there is a greater chance of recognising and responding to research needs and opportunities.
(b) Better research management (e.g., PPB) is facilitated.
(c) Agriculture is recognised for what it is—a hierarchy of systems with a purposeful nature.
(d) Much can be done more efficiently than otherwise (e.g., model rather than real-world experimentation) (Anderson & Dent, 1972; Sturgess, 1972).

4. SUMMARY

Having covered a wide range of territory, let me reiterate my main points.

I believe the systems approach constitutes a new technology for viewing the world. In recognising the purposeful nature of much of the world, it has substantive implications for science. Expansionism, teleology and synthesis must be admitted as valid elements of scientific methodology—and science takes its place as an instrument of higher systems. As a corollary, there are important implications for agricultural research in the areas of professional training, research organisation and research directions. Decisions between and within these areas will largely determine how well agricultural research serves its masters.

In particular, the systems approach has a role to play in the allocation of

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agricultural research resources. It recognises the agricultural research system as an instrument of higher purposive systems and provides a framework both for bringing research alternatives to attention and for their assessment.

Finally, I have argued that the systems approach to research will generally be more efficient than the traditional disciplinary approach. Not only does the systems approach provide a better view of the agricultural system, but it also facilitates purposeful research management in terms of goal achievement and choice between research alternatives.

ACKNOWLEDGEMENT

I am grateful to J. R. Anderson, K. D. Cocks, D. Godden, I. Greig, J. O. S. Kennedy and D. B. Williams for critical comments, used and unused.

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