UNIVERSITY OF NOTTINGHAM
School of agriculture
Department of Agriculture and Horticulture

Fourteenth Heath Memorial Lecture

AGRICULTURE, EDUCATION, RESEARCH AND EXTENSION

JOHN L. DILLON
Professor of Farm Management
University of New England

February, 1971

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THE HEATH MEMORIAL LECTURE

WILLIAM EDWARD HEATH was born in Leicestershire in 1906 of a large farming family. All the family have been associated with agriculture and some are now farming in Canada and New Zealand.

He was a student at the Midland Agricultural College and graduated with the degree of Bachelor of Science of the University of London. (The Midland Agricultural College is now the School of Agriculture of the University of Nottingham).

He started work at the Agricultural Economics Department at Sutton Bonington and then moved to the Farm Economics Branch at the Department of Agriculture for Scotland, in due course becoming Head of that Department. During this period he was responsible for an economic survey of marginal farming in Scotland.

In 1947 he was appointed Reader in Agricultural Economics at the University of Nottingham. He played an active part in the School of Agriculture and later was Vice-Dean of the Faculty of Agriculture and Horticulture. In 1951 he was selected to visit the United States of America to study research and teaching methods.

He was particularly interested in all the international aspects of agricultural economics and devoted a good deal of time to lecturing and writing articles on the subject of food and people. He was an active member of the International Conference of Agricultural Economics and of the Agricultural Economics Society.

Although handicapped from his youth by an attack of infantile paralysis he refused to bow to his handicap and shared in full in the whole life of the University. It was a shock to many when he died suddenly at the age of 45.

The Heath Memorial Lecture was established in his memory, largely through the initiative and generosity of past and present students (The Old Kingstonian Association) and of the farmers who appreciated his work in the East Midlands province.

THE LECTURER 1971

After graduating from the University of Sydney in 1952 John Dillon took to the land as a dairy farmer. It was not long however, before the lure of research drew him to the Department of Agriculture, N.S.W. and then, having secured a Fulbright Scholarship and a Research Associate at Iowa State University, to the U.S.A. He completed his graduate studies under Professor E.O. Heady in 1959 and returned to Australia soon to be appointed Reader in Economics at Adelaide University. In 1965 he was appointed to the foundation chair of Farm Management at the University of New England and in 1970 became Dean of the Faculty of Economic Studies at this University.

Professor Dillon has an active interest in the agricultural economic problems in developing countries and in 1966 was consultant to the Chilean Land Reform Corporation and later in 1968 was Rockerfeller Visiting Professor of Economics at the Catholic University of Chile. At present he is a member of an FAO/UNDP Consultant Panel working on Hungarian Agricultural Development.

In 1965 John Dillon was awarded the Edgworth David Medal of the Royal Society of N.S.W. for research and in 1971 the Medal of the Australian Institute of Agricultural Science. He is the author of a number of books on agricultural production economics that have become basic texts in many countries.

AGRICULTURAL EDUCATION, RESEARCH AND EXTENSION

by

John L. Dillon

Professor of Farm Management and Dean, Faculty of Economic Studies,

University of New England

It is both an honour and a pleasure to have been asked by the University of Nottingham to give this Fourteenth Heath Memorial Lecture. The achievements and example of William Edward Heath were most notable. Together with the academic and professional esteem in which Nottingham's School of Agriculture is held, they give me a most substantial feeling of inadequacy in presenting this lecture; feelings which I can say have nagged me in a variety of airport lounges over recent days as I tried to put my scrambled thoughts together in an order suited to this occasion.

The topic of my address — Agricultural Education, Research and Extension — could hardly be broader. Of necessity it cannot be given its due in the time at our disposal. Nor, I must emphasize, will I be talking specifically of the situation in the United Kingdom. Indeed, though some of my forbears left these parts with what we might describe as a cast-iron handshake some 150 years ago, this is my first visit to the United Kingdom and I have only the sketchiest knowledge of your agriculture and its institutions. I will be talking of principles relevant to agricultural education, research and extension in general relative to the world of today. Perhaps some distinction should be made between developed and underdeveloped agricultures in this context, but I think not. Much the same principles — though not the methods of implementation — are pertinent everywhere, I believe. Since much of what I have to say reflects subjective judgement, let me note that I hold the individual as sometimes more important than the State, that I hold any individual just as important as any other, and that I favour efficiency if it does not cause social misery. Most importantly, I should note that I tend to see things more through the eyes of an economist than through the eyes of an agriculturalist.

Education, research and extension, of course, are very much interrelated — it's tempting to coin the word 'edresex' to cover their overlapping complex of aims, activities and institutions. But rather than try to cover their conglomerate of activities as a single entity, I will stick to the traditional routine and look at them in somewhat separate fashion.

Education

Around the world these days, education is "in". Probably no other item of consumption has shown such as upsurge of demand and such difficulty of satiation. In general, the more education the better because education is aimed at man and he is the main actor in the world — despite the fact that agricultural scientists often imply soils, plants and animals are the most important entities in agriculture. As Cliff Wharton of the Agricultural Development Council has put it: "Education pushes back cultural prohibitions. It reduces the restrictions of traditionalism and facilitates innovation. It broadens a person's ideas of the possible. It increases the geographic and occupational mobility of people. It makes it easier for a person to think through the problems which he faces and not merely accept them as unchangeable givens."

I would distinguish four areas of education particularly relevant to agriculture.

Firstly, there is the education of youth needed for agricultural production *per se*, be it a question of labouring, tractor driving, farm book-keeping, managing or something in between. Regardless of its level, this education ought to be in terms of principles and applicatory examples, not rote learning.

Secondly, there is the education of the majority (around 90 per cent in many countries) of rural youth who will not be needed in agriculture. They must receive education aimed at facilitating their transfer out of agriculture if the problems of surplus rural labour, too-small farms and rural slums are to be avoided.

Thirdly, there is the education of those providing services to agriculture either via the public purse or commercially. Specific categories of service activities that might be distinguished here are technical research and extension (as typically provided by agricultural scientists, technicians, extensionists and consultants) and financial management (as often provided by banks, accountants, specialist extension officers and management consultants).

Fourthly, there is the continuing job of educating farm leaders and policy makers. That, however, is a difficult proposition. Short of drafting them for night school, it is hard to see how the job might be done!

Let me expand a little with respect to two sub-categories in the listing: the education of farm managers and the education of agricultural scientists. For both, I believe the best school education is a good general education to matriculation level, preferably with emphasis on mathematics and science. I do not believe they should study agriculture as a school subject; the opportunity cost of such study is too high in terms of other studies foregone. This is especially so, given (at least as I believe) that agriculture will be increasingly dynamic in both a technological and a market sense; and that it will become increasingly industrialized. In such circumstances the education of managers and scientists requires an assessment of the intellectual demands that will be placed on these human inputs 10, 20 and 30 years hence. This, of course, does not mean specifying the type of sprays to be used on fruit trees in 1990. The speed of technological advance makes such factual prescription impossible — just as it makes the giving of such prescriptions for today obsolescent tomorrow.

What must be done is to train potential farm managers or scientists at tertiary level in such fashion that they are capable of operating in the dynamic agricultural environment that they will face over their working lives. The minutae of pest control, seed and fertilizer rates are facts of fleeting value today but obsolescent tomorrow. This lesson, I expect, has been learnt in the United Kingdom but in most countries of the world it has not.

Both for potential managers and scientists, professional education must be in terms of principles, not facts. The principles of soil physics, plant physiology, animal nutrition, input-output relations, enterprise planning, systems analysis, industrial management and agriculture's place in the economy, for example, must be taught. A mind trained in terms of such principles is not filled with information of declining relevance. On the contrary it is adaptable to new technology and problems. Technological advances can be absorbed as they come forward, fitting them into the relevant framework of principles and with some appreciation of the nature of the processes involved.

The farm manager of 1990, to succeed, will have to be up to date scientifically, be capable of operating a highly capitalized and industrialized business, and be adaptable in both a technical and an economic sense to the dynamic environment he faces. Rather than being a traditionalist and emulator of others, the farmer of 1990 will need to be a man of initiative and real managerial capability. Likely he will need to have training as complex and sophisticated as agricultural graduates of the 1940's and 1950's. Indeed, as agriculture becomes more industrialized in terms of larger scale of operation and with greater substitution of capital for labour, as well as increased automation, I see more and more farm managers being university graduates with training in both agricultural science and management.

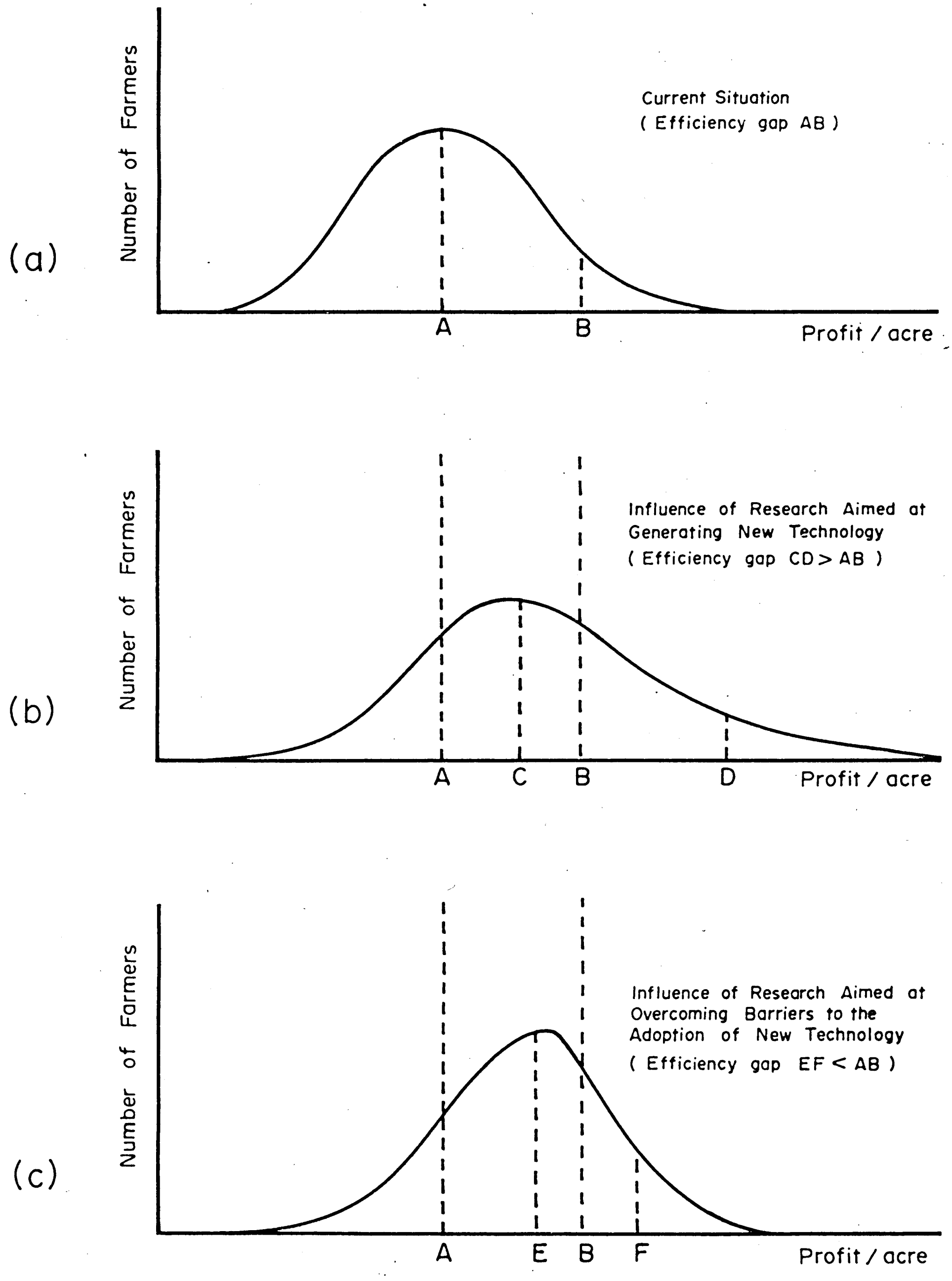
Most of the above comments are subjective. To make reasoned policy decisions on education, we need information on its costs and returns since in neither the public nor the private sense is education costless. To get it, we have to forego something else. Presently, however, factual data – let alone research – on education for agriculture and the education of rural youth around the world is pitiably small.

Research

I want to make two comments about agricultural research. The first consits of some hypothesizing in the type of research strategy that might be followed. The second is of a more methodological nature on how research is carried out.

Overall, around the world, too much of the limited supply of public agricultural research resources is devoted to research aimed at the development of new technology; too little support is given to applied research aimed at making new technology operational; and far too little support is given to economic and behavioural science research aimed at the efficiency of farm business decisions, the government policies under which agriculture operates and the human element in agriculture.

Too often agricultural research is posed in terms of the question "Under what conditions will it work?" rather than in terms of the question "Under what conditions will it pay?" In this regard it is pertinent to think in terms of an efficiency gap in agriculture defined as the difference in economic productivity between the average farm manager and, say, the top 10 per cent or so of farm managers. In rough diagrammatic form this existing efficiency gap is represented or may be conceptualized by the location and length of the interval AB in figure (a). Relative to this efficiency gap we can distinguish two types of research. On the one hand there is the typical and prestigious type of research aimed at developing new production technology. Such research can make no contribution to closing the efficiency gap. Of itself, it can only enhance the gap. Indeed, by producing results faster than


(a)
Number of Farmers
Current Situation
(Efficiency gap AB)
A
B
Profit / acre
(b)
Number of Farmers
Influence of Research Aimed at Generating New Technology
(Efficiency gap CD > AB)
A
C
B
D
Profit / acre
(c)
Number of Farmers
Influence of Research Aimed at Overcoming Barriers to the Adoption of New Technology
(Efficiency gap EF < AB)
A
E
B
F
Profit / acre

the average farmer can digest them, such research is a cause of the gap. Relative to an initial efficiency gap AB, such technology-induced widening of the gap is represented by the distance CD of figure (b). In contrast, we can distinguish research aimed not at generating new technology, but aimed at understanding and overcoming the barriers that retard the average farm manager from performing as well as he might in the face of an already-given technological array. Such research can only reduce the efficiency gap. A diagrammatic representation is given by the distance EF of figure (c).

Given these two avenues of research, we have a choice as to what mix of gap-increasing and gap-decreasing research we might select. This choice of research strategy will then control the size of the efficiency gap in just the same way as our use of the Hot and Cold taps to control bath temperature. Alternatively stated, relative to limited research resources, what size efficiency gap between actual and potential performance do we want? Having chosen the gap, what does it imply about the distribution of our research and other resources?

Obviously the choice of the best-sized efficiency gap to have and its feasibility is a problem for economic and sociological research. It must depend on the costs and benefits involved and on farmer attitudes. Without such information we can only conjecture. I am convinced that present gap is typically too large. One of smaller size could be more profitable. The technological carrot would still be there for the efficiency leaders; and at the same time, there would be gains from not having to wait so long for new technology to be adopted.

Most importantly, we would not be expending funds blindly on an oversized stockpile of new or potential technology. The manufacture and inventory of new technology would be worked more efficiently. Instead of producing new technology indiscriminately and overstocking the technological warehouse with a fast inflow and a dribble of outflow, we would be keeping a more rationally chosen stock of new and potential technology in reserve and at the same time be paying more attention to balance the inflow of the warehouse.

On the research side, therefore, I feel we need a far better balance between, on the one hand, research aimed at the soil, plants and animals which the farmer uses and, on the other, research aimed at understanding and appreciating the farmer himself, and the business and social environment in which he has to operate. After all, the farmer — not the soil, plants or animals — has the most important role in agriculture.

Let me now turn to a methodological aspect of agricultural research. Recent years have seen the budding, if not the flowering, of a new approach to statistical decision known as Statistical Decision Theory or Bayesian Statistics. In contrast to the reliance of classical statistical procedures on objective probabilities and significance levels, the new approach emphasizes subjective probabilities and economic significance and subsumes hypothesis testing under the general heading of decision making under uncertainty. As yet these developments have had little or no impact on agricultural research and extension. Rather than being based on the economic significance of experimental results, recommendations to farmers from agricultural research continue to be based on statistical significance or, as I prefer to put it, the magic of asterisks. In themselves, significance tests have absolutely no economic orientation and imply researchers and farmers operate in an economic vacuum. There is simply no economic logic in the mechanistic use of the magic numbers 0.01, 0.05 and 0.1 as if these particular levels of significance came to us as a statistical postscript to the decision rules Moses received from on high. What these significance levels do reflect, of course, are the purely self-oriented cautionary motives of the researcher and his desire for the false goal of scientific objectivity.

The essential differences between the Bayesian and classical approaches are, first, that while the classical approach relies only on objective probabilities derived from experimentation or sampling, the Bayesian approach also takes into account the subjective probabilities of the researcher or decision maker about the hypotheses under question; and, second, while the classical approach uses level of significance or chance of a Type I error (i.e. the probability of rejecting the *status quo* when it is true) as a decision rule, Bayesian analysis uses the maximization of expected profit or minimization of expected loss as the decision rule. In short, Bayesian analysis is economic in its orientation and, through its use of subjective probability, takes account of all available information. Classical analysis, in contrast, has no economic orientation and ignores subjective prior information. Since the idea of introducing subjective elements into research is anethma to most agricultural researchers, we should look for a moment at subjective and objective probabilities.

Ranging between 0 and 1, probabilities are weights or indices assessing the chances of occurrence of uncertain events. Objective or empirical probabilities are defined as the limit of a relative frequency. Since a limit implies an infinite series of observations, this is not an operational definition. It is an abstraction that can never be verified. This unverifiability, in itself, is not a difficulty since the probability concept may still be useful — just as the unreal notions of points and lines in geometry are useful. However, because the theory relates to infinite sets of observations, there is a logical difficulty in applying it to finite sets of observations. In particular, probability statements about single events (will the coming season be good or poor?) are excluded by the objective frequency approach. Most users of the objective approach get round this difficulty by assuming that a finite set of observations is good enough to estimate the limit. In doing so they make a subjective judgement and so, in fact, are using subjective probabilities. As well, in applying objective frequencies based on finite historical sets of observations to future occurrences, they make a subjective judgement that the structure of the situation has not changed, i.e. that the future will be like the past. For these reasons, the unqualified use of what are thought to be objective probabilities might be described as the inefficient and ignorant use of subjective probabilites chosen in a lazy mechanical fashion.

The degree of belief or strength of conviction an individual has in a particular proposition is the subjective probability of that proposition for him. It is a personal assessment corresponding to the odds at which he would just be prepared to bet on the occurrence of the event. People may therefore differ in their subjective probabilities about the same event. For example, what chance do you think there is that I will still be talking 20 minutes from now?

Although there is controversy over the use of subjective and objective probabilities, their supporters agree that each type must follow the axioms and rules of probability theory as a mathematical concept. There is, however, one theorem — known as Bayes' Theorem — which is of particular relevance for the subjectivist. By this theorem he is able to revise his prior probabilites in the light of additional objective information (i.e. experimental or other sampling evidence) to obtain revised or posterior probabilities about the events (hypotheses) under consideration. Further, Bayes' Theorem may be applied over and over again as further information comes to hand, the posterior probabilities of one stage being the prior probabilities of the next.

The argument for making use of subjective prior probabilities via Bayes' Theorem is that they introduce relevant extra information into the analysis of a problem. This (perhaps costless) extra knowledge would be ignored by objectivists who place complete weight on the likelihoods obtained from empirical experimentation. The objectivist approach might be summed up as saying "We must force ourselves to forget what we believe in order to be completely rational or scientific". Such an argument has no economic content. Worse still, it implies that by acting otherwise, one loses "scientific objectivity" an identification that agricultural scientists on the treadmill of status and acceptance by their peers cannot easily give up, even though it often leads to what might be described statistically as Type III error, i.e. when the analyst delivers a carefully computed solution to the wrong problem.

To illustrate some of the ideas expressed above, let me present a simple example, fuller discussion of which may be found in an article in a forthcoming issue of **The Farm Economist.** Suppose an agricultural consultant or extension officer is faced with the problem of whether or not to recommend the spraying of barley crops in his region in the coming season with a new herbicide. He knows yields can be adequately represented by a normal distribution. In the previous season a trial of the herbicide was conducted on nine representative areas of barley in the region. This gave the following results:

	Sprayed	**Not sprayed**
Mean yield/acre	30 cwt.	26 cwt.
Standard deviation/acre	3 cwt.	3 cwt.

The results for the unsprayed treatment accord with local yield experience. Relevant financial data is that spraying costs 75 new pence per acre and the net profit from each extra cwt. of barley produced is 25 new pence. To cover its cost, spraying must therefore increase the mean yield per acre by at least 3 cwt. The mean break-even yield with spraying is therefore 26 + 3 = 29 cwt. per acre. On the subjective side, based on his personal assessment of current seasonal conditions and his knowledge of yields with the herbicide in other regions, the extension officer or consultant expects the mean yield after spraying this year to be around 32 cwt. per acre with a fifty-fifty chance that it will be between 28 and 36 cwt. per acre. This implies the prior normal yield distribution has a mean of 32 and a standard deviation of 5.97 cwt. per acre. He believes expected break-even yield will be unchanged at 29 cwt. per acre.

Classical analysis ignores the subjective information of the extension officer and looks only at the experimental results. Assuming a critical level of significance of 5 per cent, the trial results imply a recommendation of not spraying. To recommend spraying based on significance at the 5 per cent level, a trial yield of 33.94 would have been needed. The bald statement "not significant at the stipulated level" is as far as the classical analysis goes towards assisting the extension officer in his decision problem.

In the Bayesian approach, account is taken of both the available subjective information and the experimental evidence. Taken together and making use of Bayes' Theorem, the experimental and subjective information imply that the yield after spraying has a mean of 30.4 and a standard deviation of 2.68. These results imply that spraying should be recommended since it would yield an expected profit of (0.25) (30.4) – 0.75 = £6·85 whereas expected profit without spraying would be less at (0.25) (26) = £6·50.

Note that without bringing in the question of additional subjective information, any sane farmer looking only at the experimental evidence would have decided that **expected** profits were higher with spraying than without, despite the lack of statistical significance. His best bet financially, based only on the results of the trials, would be to use the herbicide.

The advantages of using the Bayesian approach over the classical approach might be summarized as follows:

First, in most statistical decision problems facing applied researchers and field workers in agriculture it is possible to estimate expected profit functions. Based on the opportunity cost of alternative decisions, these profit functions set out the conditional consequences of making different recommendations. Because farmers are concerned with the financial outcomes of adopting new or different technologies, expected profit assessments are essential for a complete analysis leading to a decision. The classical approach of only concentrating on Type I error, where the value of permissable error is ruled by convention and is often quite arbitrary in relation to the decision problem, goes only a small way to analysis for decision. In this sense, significance testing is a hoax serving not the farmers' interests but the conservatism of the researcher.

Second, in basic research (i.e. research without any direct managerial decision implications), it is typically impossible to estimate expected profit functions under alternative hypotheses. In such situations, rather than merely describing one hypothesis as significantly different or not from another at some arbitrary significance level, a summary of the research data in terms of its sufficient statistics would be more helpful to anyone who wishes to use the research findings for consequential decisions involving his own subjective probabilities.

Third, because field research in agriculture is generally expensive, decisions often have to be made with little empirical data. In such situations the Bayesian use of subjective information in a well defined and consistent way must lead to better decisions. Traditionally, of course, what has happened is that researchers and/or their statistical advisers have brought in their own fudge factors in rather haphazard ways.

Of course there are disadvantages to the Bayesian approach. In general it is more complicated and as yet has not been reduced to the cook-book stage corresponding to the use of t tests, F tests, R^2 etc. in classical analysis. Against such complications, however, it must be said that the Bayesian approach does not automatically imply Type III errors!

Extension

The above comments on agricultural research methodology are really implications of extension back to research. What of extension itself? I believe extension (however conducted) has to get four types of knowledge across to farmers:

1. Knowledge about new inputs
2. Knowledge about new techniques of production
3. Knowledge about how to economize in production and marketing
4. Knowledge about how to get out of agriculture.

Knowledge about new inputs and techniques are the technical aspects of farming. The third is the economic aspect of management. With the increasing industrialization of agriculture this is likely to be the most difficult task confronting farm managers. It involves not routine tradesman-like skills that once learnt are always on hand, but rather the decision making function of evaluating and choosing between alternative strategies. To be efficient in an economic sense, the farm manager has to use logical processes of thinking through problems himself as alternative choices continuously arise under a bewildering array of situations. And in doing so, of course, he has to subjectively assess the likelihood of alternative risks and take account of his own risk preferences.

To date, extension around the world has typically concentrated largely on technical information. Knowledge about economizing, for example, has not received its fair due, and little attention has been paid in extension to farmers' risk preferences. What is best for one farmer, may not be best for another.

To a degree, however, such problems of extension contain their own seeds for solution. As the poorer farmers get squeezed out of agriculture and as the industrialization of agriculture proceeds, extension will lose its justification for being a free good. Managerial advice, like managers and physical inputs, will more and more become a purchased input to the production process supplied, at a price, by commercial suppliers. More and more, I would expect to find a re-allocation of public funds around the world (especially in the developed countries) away from extension as we have known it in the past towards a welfare role of assisting non-viable producers out of agriculture. In turn, these various hypotheses have academic implications for both teaching and research, implications which I will not attempt to assess. I hope, however, that while such developments will inevitably lead to a more balanced emphasis on the social, economic and physical aspects of agriculture, they will also enhance the prospects for co-operative interdisciplinary research and teaching. Without such co-operation, agriculture must be ill-served and the disciplines serving agriculture must be less efficient than they would otherwise be.

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