Frontiers in Agricultural Policy Research: Discussion

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I take it as obvious that we as a profession, as well as individual research organisations like the BAE, should think about research priorities. Brian Fisher's views on needs in agricultural policy research is a welcome contribution to this thinking. It is in the nature of such efforts that they will not attract universal agreement. Inevitably there will be debate and argument. This is healthy and desirable.

A Classification

To start my own thinking, I attempted a classification of the research suggestions made by Fisher. Some members of the audience may be interested in what an eminent economist had to say about classification forty-seven years ago.

'Indeed, it may be said that the major part of traditional economic theory consists of classification. Classification is a highly respectable scientific activity of which economists have no need to be ashamed. By referring more to it and less to so-called 'laws', their claim to scientific status, albeit more modest, would be less suspected' (Harrod 1938, pp.392-3).

I venture to suggest that, notwithstanding progress in economics during and since World War II, Harrod's point continues to have validity.

The classification I have come up with is given in Table 1. It shows fourteen specific suggestions for research that I have identified in Fisher's paper. Each of these is classified according to whether the suggestion is:

- theoretical or applied,
- hard or very hard,
- most likely to be carried out successfully in Australia or overseas.

The final column contains the initial thought which impressed itself most firmly in my mind about each research suggestion. There is necessarily an element of judgement in the classification. In relation to this effort, at least, I am happy to accept the invitation of Brian Hardaker in his Presidential address (Hardaker 1985) to 'come out' and confess to a deal of subjectivity. Nevertheless, I found most of the classifications relatively easy to make. In this context the memory that confidence has been defined — by an unremembered wise person — as that marvellous feeling of being on top of a problem that wells up in a person before he understands it, makes me slightly uneasy.

I don't intend to discuss the classification in detail, just to highlight some features:

1. Most of the research suggestions require theoretical work as well as applied work. For the great majority of us, this poses difficulties beyond those arising with research that is strictly applied.

2. It is not surprising, therefore, that I decided most of the research suggested is very hard. None is easy!

3. Perhaps most interestingly, for many, and probably most of Fisher's suggestions, substantial progress is in my assessment more likely to come from the efforts of overseas researchers than from work in our own country. In general, a country with a small number of researchers is likely to rely mainly on other countries for new theoretical knowledge and for the development of techniques that are used in undertaking research relevant to agricultural policy.

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Table 1: A Taxonomy of Suggestions for Research in Fisher's "Frontiers in Agricultural Policy Research"

Research need/suggestion	Research suggested is: Theoretical or Applied	Hard or V. hard	Progress is likely to be made in Australia or Overseas	Comment
A. *Stabilisation*				
1. Comparison of benefits and costs of Australia's stabilisation instruments	Applied	Hard	Australia	Much is known. Need more work on price underwriting and on the buffer stock scheme for wool
2. Effect of functional form on assessment of stabilisation schemes	Applied	Hard	Both	Can it be realistic if it is manageable?
3. Determination of Australian farmers' attitudes to risk	Applied	V. hard	Australia	Agree there is scope for an innovative study
4. Allowance for shifts in supply due to stabilisation	Both	V. hard	Overseas	Neglected for too long
B. *Expectations*				
5. Study of how farmers form expectations	Both	V. hard	Overseas	Challenging
6. Relevance of rational expectations hypothesis	Both	V. hard	Overseas	Has support for rational expectations peaked?
7. Benefit-cost analysis (including study of distributional effects) of improving market information	Applied	V. hard	Both	Could strengthen support for providing information rather than using other assistance policies
C. *Regulation*				
8. Benefit-cost analysis of easing regulations dealing with low probability events (some health, quarantine regulations)	Both	V. hard	Both	Guaranteed to catch a headline
9. Size of savings in resources devoted to rent-seeking due to deregulation	Both	V. hard	Mainly overseas	Very worthwhile
D. *Rural Competitiveness*				
10. Importance of rural assistance through infrastructure subsidies	Mainly applied	V. hard	Mainly Australia	Joint cost problems
11. Effectiveness of different policies for delivering assistance	Both	V. hard	Both	Need to focus on the reason for the assistance rather than on delivery of a given amount of assistance
12. Efficiency cost of raising tax revenue	Both	V. hard	Both	Important. There is increasing evidence that the benefit-cost outcome of expenditure – increasing projects and policies is considerably less favourable if the welfare cost of raising extra taxes is considered
13. Determinants of capital formation in agriculture	Both	V. hard	Both	Out of fashion?
14. Benefits and costs of land degradation – including consideration of differences in private and social time preference rates and of intergenerational equity	Both	V. hard	Both. Overseas in case of theoretical work on discount rates and equity	A growth area

Further Comments

My remaining comments relate to the following matters:

- the demand for and the supply of agricultural policy research,
- suggestions regarding research priorities,
- assessing the quality of policy research and advice.

Demand and supply considerations in research

These comments are not directed to the question of how resources should be allocated between activities which initially benefit producers and those which have their first impact on consumers. Rather they are concerned with some issues relating to the deman ford and supply of publicly funded agricultural policy research, and in particular with highlighting the significance of two conflicting interpretations of the motivation of governments. The distinction between these interpretations — which is sufficiently important to warrant regarding them as separate paradigms – is relevant in thinking about demand and supply of publicly funded research, as it is in seeking to understand government decisions on industry regulation and assistance.

Ruttan (1982, 1984) puts the view that while the demand for agricultural production research is derived from the demand for technical advance in agriculture, the demand for social science research, including research in agricultural economics, is derived from the demand for institutional innovation. Innovation in commodity marketing arrangements and in finance markets are examples. It is widely accepted that the rate of return from investment in scientific research for agriculture is very high (e.g., Ruttan 1982; IAC 1977). Ruttan argues that the return from social science research is also very high in the U.S. and elsewhere. On a public interest interpretation of the activities of government, this suggests that society would be willing to pay for extra investment in agricultural economics research.

If social welfare depended entirely or substantially on the productivity of resources, governments and their agencies behaving in accordance with public interest considerations would wish to direct extra resources to uses giving very high rates of return.

However, the supply considerations that are important in making decisions on research if the emphasis is on maximising social welfare include the skills, experiences and preferences of our researchers. The starting point for important pieces of research is often that an individual *believes it is worthwhile.* This belief is likely to come from a very individual process of introspection. People responsible for setting research priorities, if they are wise, will recognise the importance of the individual's affinity with (which is closely related to his enthusiasm for) the project or program on which he is concentrating during working hours and on which his subsconscious mind may be performing miracles while he is asleep. As always, supply and demand are both important.

The opposing private interest view of research is captured in the following quotation from Hadwiger:

'Political scientists and politicians recognise that effective support and opposition came not from the whole of "society" but from specific small sectors. Even the occasional wide or "diffuse" support for research provided through the ballot box is usually elicited from rather specific publics such as farmers, conservationists and environmentalists. Close observers of political behaviour are likely to regard references to "society's goals" as naive or self-serving' (Hadwiger 1982, p. 75).

According to the private interest paradigm the demand for policy-oriented research will reflect the pressures upon the Commonwealth or State Government concerned, and especially the pressures experienced by the Minister for Primary Industry or Agriculture. The level and pattern of effective demand perceived by governments or Ministers will change with changes in the

ability of various groups (e.g., wheatgrowers, irrigation farmers, consumers) to deliver political support in return for government funding of research. The supply of publicly funded research under the private interest paradigm can be expected to reflect costs not in terms of the net social returns from activities foregone, but in lost support from groups expecting to be affected adversely by the research and in the political opportunity cost of funding research rather than something else. This general approach can be applied to the allocation of a given agricultural policy research budget between programs or projects as well as to the determination of the aggregate research effort.

Which paradigm — public interest or private interest — best describes the allocation of resources to agricultural policy research in Australia? If we accept Ruttan's view that society's rate of return from rural policy research is very high, it might be thought that the public interest paradigm could not apply. A government concerned with maximising social welfare would allocate extra resources to activities yielding high rates of social return, thus driving the return down.

There are problems with this argument. One problem is that the social rate of return from other uses of scarce resources of a type used in agricultural policy research may also be very high. It is not clear, for example, that research-induced institutional changes in such areas as transport, health, education, public enterprise, labour markets and capital markets would generate lower rates of social return at the margin than does research into agricultural policy. Another problem is in the choice of a measure of social welfare. Not everyone agrees that an activity's contribution to social welfare is given by its rate of return without reference to distributional effects. Research that yielded a 50 per cent rate of return with the benefits accruing mainly to farmers with high incomes and/or net worth would be judged by some criteria to contribute less to society's welfare than research giving a 10 per cent rate of return and accruing mainly to farmers or others with low incomes/wealth.

If the level and pattern of rural research activity was determined entirely by a government's assessment of political costs and benefits there would be a point to this Conference session only to the extent that it had potential to influence that benefit-cost calculus. Such potential does exist (see Standen 1983). But Fisher appears to be thinking in terms of a public interest motivation of high level decision makers. With that attitude it is easier to be confident that better identification of relative research payoffs will result in productivity-increasing reallocation of research resources than it is if politicians and research managers are influenced strongly by costs and benefits to themselves in terms of power and status.

Research priorities

I take the view that Australia would be unlikely to make large advances in many of the research areas identified by Fisher with any feasible reallocation of resources. I do accept, however, that there needs to be more effort devoted to investigating the relative merits of different stabilisation policies in the Australian context. We still do not know whether woolgrowers' revenues have been increased or reduced as a result of the wool buying and selling activities of the Australian Wool Corporation. The assessments of Quiggin (1983) and Monday and White (1984) leave it unclear whether the in vogue price underwriting schemes can be supported on grounds stronger than that they are probably better than the alternatives. Also in the stabilisation category, I suggest that work on the use of futures markets by marketing boards is a neglected high priority area. Possibilities for options markets for agricultural commodities also merit investigations in an Australian context (see Gardner 1981).

In the general area of regulation, Fisher's third category, there is scope for important applied research work. In the last two years the IAC has recommended substantial deregulation in the wheat, sugar and dairy industries. It will always be true that the capacity of the market — when it is allowed to operate freely — to induce efficiency-increasing adjustments will outstrip economists' ability to anticipate and measure them.

But illustrative studies of the effects of deregulatory policies on particular groups of producers through such effects as reductions in marketing costs and ability to sell to higher priced markets would be very useful. In the case of the sugar industry there remains a need for comprehensive examination of the effects, including distributional effects on sugar producers and processors, of existing and alternative institutional arrangements.

A comment is in order on the suggestion that more research is needed on the effectiveness of different policies for delivering assistance. The discussion of this point smacks a little of the notion that the amount of assistance to be provided has been predetermined, with policy evaluators left to decide the best forms. Adherents of the private interest approach to regulation hold that both the amount and the form of assistance depend on the various — largely political — factors entering into the demand for and supply of regulation (see Sieper 1982). Within the public interest approach to intervention — into which category Fisher's paper more readily fits — the idea of using instruments that are efficient for achieving the intended objective is a fundamental one. Conventionally, it has been accepted that once the source of market failure is identified the most efficient policy is determined: it is the policy which removes that inefficiency without creating other inefficiencies elsewhere (e.g., Johnson 1965). Of course the situation is more complex in the real world where first-best rules may be inappropriate partly because, as Fisher recognises, revenue-raising involves efficiency costs. It appears though that a good deal of discussion about effectiveness in the delivery of assistance stems not from issues of second-best but from a vagueness concerning the objectives of policy.

One instance of this is to be found in a BAE submission to the latest IAC inquiry into the wheat industry (BAE 1983). There the Bureau claims that "providing assistance to reduce risk or increase research is usually preferable to subsidising inputs or returns to growers" (p.73). But this is only so if the "usual" justification for government intervention is market failure due to risk or externalities associated with research. If the justification is provided by the "low cost" nature of an activity, as appears to be so in the case of the BAE argument in relation to assistance for wheatgrowing, the first-best policy is output-based assistance. This has the desired effect of encouraging all farmers to produce more wheat, regardless of their attitude to risk. This is not true of policy measures directed to reducing risk. There are possible qualifications to the superiority of assistance on output when the welfare costs of revenue raising (Parish and McLaren 1982) and second best issues are considered, but these complications were not introduced in the BAE's discussion.

Finally, since it is virtually certain that increased effort can be devoted to favoured research only by cutting back on other areas, (a situation not easily reconciled with a simple public interest approach — maximisation of the returns to investment — in the presence of the high returns to rural policy research accepted by Ruttan), where should the cuts occur? Fisher is silent on this. One class of research that is in my view not likely to pay its way is research into the effects of overseas policies on the welfare of *overseas* producers and consumers. It is hard to see why overseas governments (regardless of whether they are motivated by public interest — interpreted in a national and not a global sense — or private interest considerations) should change their policies to Australia's advantage because of Australian research showing that the policies impose a cost on the foreign country or countries concerned or on certain groups (such as consumers). Agricultural policies in the EEC, the U.S. and elsewhere have been formulated and/or maintained in the knowledge of *domestic* estimates of their effects on efficiency and on income distribution. Australian research is unlikely to add to the information regarded as relevant by overseas decision makers. Further, Australian research will be represented overseas, especially if carried out in the public sector, as biased research produced as ammunition for Australia's international negotiators. Australia may well promote its cause overseas more effectively by publicising the results of research undertaken in the country concerned, though that also is not an activity from which a worthwhile return could be expected.

Critiques of policy research and policy advice

It is important that agricultural policy research, like research in other fields, be subject to independent assessment. The knowledge that other people are likely to notice and speak up if one produces sloppy or inconsistent work is a necessary condition for a competitive and efficient policy research and policy advising sector. I believe that this view accords with the spirit of the report by Sir John Crawford which formed the basis for the legislation setting up the IAC (Crawford 1973)! Elsewhere I have examined critically the argument in more than thirty IAC reports on rural inquiries (Edwards 1980). In a recent thesis Wonder (1984) examined the BAE's submissions to IAC inquiries for consistency in the economic arguments regarded as relevant and in the application of economic arguments. Significant inconsistencies were identified. This is a pioneering and important piece of research which should be read by everyone — not just people in the BAE — who has the task of preparing a policy argument on a rural issue. Of course, the work of policy analysts and advisers in other institutions, such as Commonwealth and State Departments of Agriculture, the National Farmers' Federation, the Australian Wheat Board and the universities should also be subjected to critical examination.

Conclusion

Brian Fisher has performed a very worthwhile task in setting out a considered position on needed research in agricultural policy in Australia. To suggest that his contribution is best viewed as a stimulus to much-needed discussion on priorities in rural policy research — and on the processes for determining the level and pattern of research — rather than as a final research agenda is not to detract from the value of the paper nor, I am confident, to view the paper differently from its author.

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