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BELIEFS AND VALUES IN AGRICULTURAL ECONOMICS RESEARCH*

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Attention is drawn to the values that are often kept hidden but condition what research agricultural economists do. It is pointed out that the economic theory, which guides most research, is not value free and that therefore the lack of debate about theory amongst Australian agricultural economists may be unhealthy. In a world with uncertainty, the research process is likened to learning, leading to a suggestion that Bayesian procedures provide a more satisfactory conceptual framework for the process of enquiry than does the conventional hypothesis-testing approach. While difficulties in the practical applicability of Bayesian methods are understood, they do provide insights which permit some of the more inappropriate uses of hypothesis testing to be recognised. The view of research enquiry as learning also leads to an appreciation of the desirability of a more honest acceptance of the inherent subjectivity of the process. Finally, attention is drawn to the need for vigilance over professional standards in research.

That we should practise what we preach is generally admitted; but anyone who preaches what he and his hearers practise must incur the gravest moral disapprobation.

Logan Pearsall Smith
Afterthoughts

In this paper my aim is to examine some of the ways that beliefs and values of Australian agricultural economists impinge upon the nature and quality of the research we do, sometimes with undesirable consequences. I shall argue that the risk of bad consequences is increased by the tendency to value specious objectivity, whereby beliefs and values of researchers are kept hidden. I am therefore advocating greater honesty in research as well as the need for us to look more critically at the beliefs and values we carry with us, often without recognising the forms of behaviour they impose on us.

Value Judgments in Research

Prior to the main part of my argument, I aim to dispose of the familiar but, I believe, largely irrelevant distinction between 'normative' and

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'positive' economics. The notion of a positivist, value-free economics seems to me to be unrealistic. Certainly, there are well-known difficulties with normative economics that limit what we can confidently say, as economists, about how the world should be. Social welfare is not objectively measurable while the Pareto principle is too weak to resolve most of the interesting and important policy questions. On the other hand, there is really little difference between positive statements that 'if action A is taken then B will result' and conditionally normative statements that 'if C is the objective then D is the best action'. The difference between the two positions is small because, if positive economic research is to be useful, it needs to be cast in terms of selecting between realistic policy options. In other words, as Rose (1948) argued, values are implicit in the choice of research topic and in the type of conclusions sought, and this is no less true when the work is set in an overtly positivist framework.

The inevitable intrusion of values into our research is not, of itself, a matter for concern. What is of concern is our frequent unwillingness, even inability, to recognise these values and their impact. Australian agricultural economists tend to share a common view of 'economic reality' and of the desirability of *laissez faire* solutions to economic policy issues. Marx (1970, p. 21) argued that: 'It is not the consciousness of men that determines their being ... it is their social being that determines their consciousness'. It seems we should ask ourselves to what extent our shared views reflect the socio-economic conditions under which we live rather than the results of our individual, independent searches for 'truth'.

It is not only what we might call the 'social' values of economists that affect what they do; private objectives are important too. The beliefs and preferences of individual researchers in relation to the prevailing reward system clearly condition behaviour (McLaughlin 1970). The 'publish or perish' environment in universities and some other organisations may drive people to undertake 'publishable' rather than 'useful' research—the recent dissatisfaction with the contents of our *Journal* may be evidence of this effect—while the pressure on postgraduate students to generate an 'acceptable' thesis or dissertation is another consideration affecting which kinds of research get done.

In an uncertain world, the choice of a research topic is a risky decision. Insights from decision analysis lead to the expectation that most agricultural economists, like most other people, are risk averse and therefore will select research topics that are not too risky in terms of the operative system of rewards. Topics that might yield dramatic results but with a high degree of associated risk will be shunned (Ladd 1979). Such behaviour, sadly, will inhibit work on radical ideas or approaches that could produce major advances in our discipline, tending instead to perpetuate what Kuhn (1970) called 'normal science'.

Finally, the values of those who fund research must have an important impact on what research gets done. The significance of this influence in agricultural economics, with our heavy reliance on 'industry' funding, is obvious. It has been instructive to see the shift towards more research on agriculture in developing countries following the establishment of the Australian Centre for International Agricultural Research, despite the bureaucratic cost involved.

The Use and Abuse of Theory

Neo-classical economic theory provides an intellectually seductive and appealingly comprehensive framework for exploring aspects of the functioning of modern society. It is, as I have already noted, the theoretical stance seemingly shared by most (or, at least, by the most articulate) Australian agricultural economists.

Theory is important in guiding economic research enquiry by suggesting how economic phenomena are likely to be related. Given the difficulty of experimentation in economics, theory becomes especially important as a means of drawing inferences about causation from observed associations among economic variables. Yet there is a danger in setting theory above observation in this way. At the heart of neo-classical theory lie some *assumptions* about human behaviour. It seems that few neo-classical economists (and few Australian agricultural economists) are prepared to attempt, or even to consider, direct empirical tests of the validity of these assumptions. Friedman (1953) goes so far as to argue that economic theory should be judged, not by the validity of its assumptions, but solely by its predictive power.

Critics of neo-classical economics, such as Balogh (1982) have attacked both the unsustainability of its underlying assumptions and its poor predictive power. The issues that have been raised are seen by some as representing a 'crisis' in economic theory (see, for example, Kristol 1981). It is not my aim to show whether the perceived crisis is a real one; I am not so ambitious. What does concern me is the apparent lack of debate about these issues among members of our profession. I am fearful that we may be too complacent.

Cole, Cameron and Edwards (1983)¹ distinguish three main schools of economic thought according to the assumptions made about the source of value. They call these three schools: (a) subjective preference (that is, utilitarian) theory, (b) cost-of-production theory, and (c) abstract labour theory, according to the assumptions made about the source of value. These three theoretical viewpoints, they argue, can be related to the conservative, social democratic and socialist political positions, respectively, corresponding, in turn, to the interests of capitalists, managers and planners, and worker, respectively. If these authors are right, it seems strange that our profession, with its predilection for neo-classical economics founded on subjective preference theory, has seemingly aligned itself with the conservative politics of the capitalist class. One might have expected to find more agricultural economists espousing theories on the political left, especially given the interest in welfare issues in rural policy and the growing emphasis on agricultural development in the Third World.

Politics aside, it is clear that alternative theoretical viewpoints carry with them associated value judgments. As Walker (1943, p. 213) argued:

... the body of doctrine which is usually taught as economic theory is only superficially neutral. Upon examination it will be found to owe

¹ These authors entitle their book *Why Economists Disagree*. I was tempted to call my address 'Why Australian agricultural economists don't disagree' to emphasise my concern, shared by Stent (1976), about the seeming conformity of theoretical viewpoints within our profession.

its form largely to value judgments as to what is desirable; and many of its verbal positive analyses carry normative force in practice. It may also be argued that academic teaching of conventional theory, despite and to some extent because of its apparent 'impartiality', helps to train the moral sensibility of pupils in a particular direction.

It is bad enough if we are deceiving ourselves and our students about the values embedded in the theory we espouse. The error may be compounded by the effect of theory on what work gets done and, more significantly, what gets published. How many econometric analyses never see the light of day because the signs of some of the key estimated coefficients are 'not as predicted by theory'? McCloskey (1983, p. 494) makes this point and also recalls the view of Kuhn (1970) that those who share a particular paradigm tend to disregard or discount any evidence that contradicts their preconceptions. If theory, chiefly the neo-classical theory, is raised in the minds of many Australian agricultural economists to the status of irrefutable truth, there is a real danger that the profession will reject evidence or arguments that are in fact highly relevant in a world where market failure and imperfect information are the rule rather than the exception.

Research and Research Methods

Research as learning

According to McCloskey (1983), economists have two attitudes towards research enquiry: the official or explicit attitude and the unofficial or implicit one. The official view, based on the so-called 'scientific method', McCloskey calls 'modernism'.

Modernism promises knowledge free of doubt, metaphysics, morals and personal conviction; what it delivers merely renames as Scientific Method the scientist's and especially the economic scientist's metaphysics, morals, and personal convictions. It cannot, and should not, deliver what it promises. Scientific knowledge is no different from other personal knowledge . . . Trying to make it different, instead of simply better, is the death of science. (McCloskey 1983, p. 488.)

McCloskey goes on to argue cogently that modernism is obsolete philosophy, is unworkable, and is not followed by economists or other scientists. Instead, he argues, we have a workaday unofficial rhetoric by which we judge what is reasonable and acceptable and what is not. I am wholly convinced by his conclusion that our work would be better if we were to make these implicit standards more explicit.

It is in the nature of economics (and of the human condition generally) that we can never be really sure about anything, yet we are cursed with a cult of certainty through self-deception. It seems to be ingrained in human nature to want to know the unknowable. Religion, which is found in almost every society, is based on the suspension of disbelief or on 'faith'. We find the same reliance on faith in everyday life. Political parties win power through the faith of voters in promised social and economic change which, in truth, can seldom be achieved. And how

many of us will admit, even to ourselves, the very low levels of accuracy we can achieve in economic forecasting.²

Even recognising the pervasiveness and inevitability of uncertainty and the impossibility of 'falsification' required for the hypothesis-testing 'scientific' approach to enquiry, we nevertheless need some way of 'knowing what we know'. A plausible approach is to view one's state of knowledge about any proposition in terms of a probability, lying between zero and one, that can be shifted towards one end of the range or the other in the light of new evidence, but that can never rationally reach either extreme.

Such a view of knowledge immediately suggests the use of Bayesian procedures to systematise the process of learning through the accumulation of evidence. This is not a novel proposal and, for example, Burks (1977) provides a comprehensive, formal treatment of the logic of empirical enquiry based on Bayesian probability revision.

Although I find the idea of a Bayesian approach to the accretion of knowledge appealing, I know that there are many practical problems in attempting to use Bayesian statistical methods in practice. Nevertheless, a more widespread adoption of a Bayesian perspective might lead us to a more sensible use of statistical hypothesis testing.

Richard King, in his Presidential Address to the American Agricultural Economics Association in 1979, has pointed to the tendency to place undue emphasis on the null hypothesis in econometrics; what he calls the 'village idiot' hypothesis. It is not realistic to suppose that, for example, the elasticity of demand for Australian wool is zero, so why test the proposition? Why not test for difference from the much more interesting value of 1.0, or better still, as King argues, a value obtained from a previous study? It seems wrong, even improvident, that attempts to estimate such coefficients are so often made as if no prior knowledge existed, when there commonly exist both a priori understanding and earlier empirical results on which analysis could be based. An approach with at least elements of the Bayesian method would allow us the better to 'stand on the shoulders of those who have gone before' in moving towards a better understanding of the world.

A related criticism of the arbitrary way in which significance testing is often performed is pointed out by Officer and Dillon (1968) and by Tweeten (1983, p. 551). Seldom is any attempt made to incorporate into the tests what might be known of the costs of type I and type II errors. True, these costs will seldom be easily assessed and there may be much subjectivity in assigning to them even orders of magnitude, but not to do so implies some valuations that might be obviously inappropriate. At

² Studies of economists' forecasting accuracy are surprisingly sparse. Back in 1962, Samuelson claimed that the best economists achieve an accuracy in forecasting directions of change in macroeconomic phenomena of 77.5 per cent while 'run-of-the-mill' economists using zealous analysis of the latest data might achieve 70 per cent. The toss of a coin would, of course, yield 50 per cent and, given the positive serial correlations for many economic phenomena, simply predicting the continuation of the current trend would raise the score, according to Samuelson, to about 65 per cent. More recently, Freebairn (1975) showed that five-year forecasts of output of Australian agricultural industries based on formal models were not significantly more or less accurate than forecasts based on naive time series projections. See also Hutchinson (1977).

the very least, the costs of the two types of errors should be considered, if not quantified, implying a more thoughtful approach to hypothesis testing.

A corollary of a willingness to view research enquiry as 'learning' is the desirability of laying bare concealed subjectivity. By having the courage to be honest about subjective judgments embedded in the research method and by willingly discussing these judgments with others, the quality of the enquiry is improved. McCloskey (1983, p. 483) rightly appeals for more examination of the rhetoric of economics, defining rhetoric as 'the art of discovering warrantable beliefs and improving those beliefs in shared discourse'.

Professional standards in research

The other side of the coin of honesty in research is the need to look to our professional and ethical standards in data collection, analysis and reporting. While evidence is hard to come by, I am fearful that, as a profession, we are too often careless of these matters. Costs of data gathering are high and cost pressures may lead researchers into unjustifiable corner-cutting. For instance, it is my experience that there are some data that cannot reliably be obtained from farmers on a recall basis. Yet increasingly I seem to read reports of studies based on just such recalled 'data'. Only rarely does one come across studies in agricultural economics in which data are meticulously compiled by participant observation or by direct measurement. Nor are there many published reports of surveys where respondent bias is assessed and reported or where 'triangulation' is used to improve data quality, that is, using two or three approaches to measuring the same datum.

If we judge the attention that agricultural economists give to the accuracy of their data by how they report results, the conclusion is dismal. Often strings of trailing digits are reproduced directly from computer output with obviously spurious accuracy. Inconsistent numbers of significant digits for different coefficients similarly evidence a disregard for considerations of accuracy (King 1979). I commend to everyone, and especially to the *Journal* Editors, King's recommendation to use scientific notation to sidestep the seeming untidiness of the conventional notation when reporting numbers of different size with a consistent number of significant digits.

Another consequence of rising costs of data gathering is that it becomes attractive to 'farm' what data are available more intensively, or to concentrate on hypothetical examples rather than real situations. Mathematical models get fancier and fancier but more and more irrelevant because 'the model becomes the message' with no regard paid to the quality of the information upon which it is based—a process that one economist is said to have described as 'engaging in illicit intercourse with beautiful models' (cited in Reynolds 1971, p. 314).

To quote another economist:

Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able

to advance, in any perceptible way, a systematic understanding of the structure and operations of a real economic system.

It may be a surprise to note that this outburst was by Wassily Leontief (1982) whose Nobel laureate in 1973 was awarded in large part in recognition of his contribution to economic modelling.

Other pressures create other problems, and the 'publish or perish' syndrome has generated some causes for concern. Plagiarism seems to be worryingly on the increase in universities and is not an offence confined to students. In my term as co-editor of the *Journal* one paper was submitted (not, I am pleased to say, by a member of our Society) that was wholly plagiarised; a fact that we happened to uncover only by good fortune.

The same may be true of what Charles Babbage, the father of computing science, writing as long ago as 1830, called 'hoaxing, forging, trimming, and cooking' (p. 175)—dishonest modification of data or results for personal advantage or to prove a point. Surprisingly often, attempts to replicate a piece of work, using the same data set, fail. It hardly affects my argument that some of the contradictions no doubt arise from careless data entry or checking, or from faulty computer software that is too readily accepted as infallible (see, for example, Boehm, Menkhaus and Penn 1976; Nash 1979). Such errors aside, there remains a worrying doubt that some economists may 'fix' data or results to meet the standards of referees or examiners, or to support their personal viewpoints.

Concluding Remarks

It seems appropriate to bring these rather disparate remarks to an end, if not to a conclusion, by highlighting what I see as important items on the agenda for the future healthy development of our profession.

First, on theory, I see a need constantly to review the relevance and robustness of our theoretical framework; to be prepared to modify and extend it, possibly to accept its overthrow by an alternative, better theory (Kuhn 1970; Stent 1976). As Johnson (1963) reminded us, neo-classical theory should not be preached as if carved on tablets of stone. We should recognise that other theories, whether from economics or from other social sciences, can provide insights into problems that concern us. For example, one does not have to be a Marxist to gain something from the literature on production relations nor a protagonist of the Austrian school to benefit from ideas about market processes. It would therefore seem valuable to make deliberate efforts to expose students of agricultural economics (that is, hopefully, all of us) to a broader range of theoretical viewpoints than is commonly represented in our teaching programs, our conferences and our *Journal*.

Those who direct others in research or who control research funds need to be alert to the dangers of perpetuating 'normal science', that is, work within the dominant paradigm. 'Young Turks' need to be given a share of resources and encouragement if our discipline is to evolve and progress. The need is all the greater because of the stagnation in staffing that limited budgets have imposed on universities—supposedly the 'power houses' for ideas.

On values, I can best summarise my thoughts with an extract from Warren Musgrave's (1976, p. 142) Presidential Address:

... it is clear that the profession has been moving into a more complex world. In that world our analysis will be more difficult and probably less elegant ... Increasingly agricultural economists will have to be aware of the nature and content of welfare economics and of the crucial role value judgements must play in economic analysis.

On research ethics, it goes without saying that we must always strive for the highest professional standards, especially those who are in positions to oversee and influence the work of others. Perhaps we should contemplate the introduction of a code of ethics for our profession. On the other hand, I am sceptical about the value of written rules of behaviour:

Either an academic polity has that common commitment, in which case no published rules are necessary; or it lacks that common commitment, in which case no published rules can save it (Hook et al. 1977, cited by Powell 1983).

I have argued that one reason why research ethics are under threat is the tendency to view subjectivity as socially unacceptable behaviour. I should like to see more people 'coming out' as self-confessed subjectivists. By recognising that subjectivity is inevitable and by making it explicit, we are in fact being more professional, not less.

On modelling, I suggest that mathematical and econometric modelling needs to be recognised for what it is—an art form. Strings of equations do not, of themselves, imply any special perception by the researcher of the nature of the real world. Nor does the fact that some parts of reality have been quantified assure better understanding. Unquantifiable aspects and omitted features of the real system may be more important than what it has been possible to include in the model (Hayek 1977, Ch. 2). It follows that models should be viewed with scepticism and interpreted with caution. In economics at least, they can never substitute for human decision making; at best, they can be an aid to choice. We should try, in our work and in our *Journal*, to keep models in proper perspective.

Finally, in our contributions to debates on policy issues, we should aim to segregate beliefs from preferences, and should fight the cult of certainty. That means we must be brave enough to admit to our own imperfect knowledge and to expose the imperfect understanding of others. Exposing uncertainty, identifying differences in beliefs about uncertain events, exploring the scope for reducing differences in beliefs by gathering more information or by sharing information already available, and segregating differences in beliefs from differences in preferences between protagonists, should all improve the quality of debate on policy issues. That should lead to more rational choices which surely is something for which, as a profession, we should be striving.

The lesson to draw from the history of science is a humble attitude towards knowledge. It is rational to doubt. As Feynmann puts it:

I can live with doubt and uncertainty. I think it is much more interesting to live not knowing than to have answers which might be wrong (cited in Anon. 1981).

The trouble is that so few people seem strong enough to live with doubt. Hayek (1979, p. 176) calls the twentieth century an age of superstition because, he says, it is outstandingly a time when people imagine they know more than they do. My appeal to agricultural economists is to be more humble about what we claim to know in recognition of the frailty of both our theory and our methods, and to be more honest about what we don't know. Away with superstition and pretence!

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