THE AUSTRALIAN JOURNAL OF AGRICULTURAL ECONONICS

VOL. 20 APRIL 1976 NO. 1

CRITIQUE OF THE METHODOLOGY OF AUSTRALIAN AGRICULTURAL ECONOMICS

W. R. STENT

La Trobe University

In this article attention is drawn to serious methodological weaknesses in Australian agricultural economics. It is frankly polemic in style and after a discussion of the philosophies of science of Popper and Kuhn the author argues that Australian agricultural economists ought to be more critical of the assumptions on which current theory is based and should be seeking to establish a new approach to the problems of the agricultural economy which would place more emphasis on social justice than on 'growth and efficiency'.

Science and its Methodology

Economic theory as taught and followed in Australia is, with the exception of Marxist economics, empiricist in philosophy. Empiricists say that the essence of science is that it is based on 'facts'. That is that scientific knowledge is based on experience rather than reason. In this they differ from rationalists, such as Marxists, who assert the pre-eminence of reason and who seek to derive, by deduction, knowledge from axioms whose truth is held to be self-evident and therefore incontrovertible. For empiricists there are two essential stages in the establishment of a scientific theory.

1. The postulation, or establishment, of the basic hypotheses of the theory. This is done by the process of *induction*. Characteristically it is this stage which is least emphasized in theory building even though, as we shall see, it is the more important of the two stages.
2. On the basis of the premises of the theory, through a process of *deduction,* that is by logical manipulation, theorems are derived which, provided the premises are true, are themselves true. It is these theorems which are commonly referred to as the 'predictions' of economic theory.

For rationalists the first stage, that of induction can quickly be overcome. They have their *a priori* truths, or axioms, from which may be deduced their corpus of knowledge. Empiricists, on the other hand, cannot so quickly pass on to their deductive reasoning for, as Hume long ago pointed out

* This paper derives from one presented to the Victorian Branch of the Australian Agricultural Economics Society on April 16, 1975. The author wishes to acknowledge with thanks the many helpful criticisms he received of that paper and from *Journal* referees on a subsequent draft.

> there can be no demonstrative arguments to prove *that those instances of which we have had no experience resemble those of which we have had experience.* (1964:91 Original emphasis)

The best that an empiricist can do given such a situation is to formulate a theory which is in accord with the known facts. It is that process of induction and its associated problems which are crucial to all scientific endeavours even though scientists, at times, try to overlook them. The problems may be formulated in several ways. One way raises the question of how do we determine the truth of a 'law' induced from empirical data. Another asks how do we distinguish between two mutually contradictory theories which are, nevertheless, each in accord with observed reality.

The philosopher Sir Karl Popper believes that he has solved the problem of induction, or at least he believes that he has provided a method whereby the problem may be solved. He emphasizes that to be scientific a theory must be capable of formulation in such a way that it may be refuted (or falsified). If it cannot be posed in a way in which it may be conceivably refuted then that theory forms no part of science, it is metaphysical. Thus he emphasizes *testing* of theses.

> A theory is tested not merely by applying it, or trying it out, but by applying it to very special cases—cases for which it yields results different from those we should have expected without that theory, or in the light of other theories. In other words we try to select for our tests those crucial cases in which we should expect the theory to fail if it is not true. Such cases are 'crucial' in Bacon's sense; they indicate the cross-roads between *two* (or more) theories . . . [such testing] can at most refute or falsify the theory in question . . . then we say that it is corroborated by the experiment. (1965:112).

Thus in a sense Popper is not concerned about the scientific veracity of the orignal hypothesis. He simply insists that it always be held tentatively and open to refutation. Thus he does not accept basic hypotheses as axioms, not requiring proof, but whilst he urges that falsification be sought he nevertheless warns that it should not be too easily accepted. There should be a certain doggedness, or dogmatism.

> This dogmatism is to some extent necessary. It is demanded by a situation which can only be dealt with by forcing our conjectures upon the world. Moreover this dogmatism allows us to approach a good theory in stages, by the way of approximations: if we accept defeat too easily, we may prevent ourselves from finding that we were very nearly right. (ibid: 49).

Popper believes that through this process of conjecture and refutation scientists gradually approach objective truth. He is, it should be emphasized, speaking of scientific method in general and whilst his methodology might seem more applicable to the physical sciences he has at times specifically referred to its applicability to economics. (vide 1972:263).

Thus Popper has provided a logical methodology whereby we may approach objective truth. His methodology is, however, definitely weak at the level of practice and this has been illustrated by Thomas Kuhn who, whilst agreeing with much of what Popper has said, especially denies that Popper's concept of falsification is invariably followed by scientists. Kuhn asserts that

> No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. (1970a:77).

For Kuhn there is no objective truth towards which science is evolving. All that he believes in is that there is evolution *from what we do know* and it is that process of evolution which provides the real task for science. Central to his theory is the term 'paradigm.' A careful critic (Masterman, 1970:61-65) listed 21 different ways in which Kuhn first used the term but in response in 1969 Kuhn listed a narrower range of meaning for the word. For him a paradigm is

> what the members of a scientific community share, *and* then a scientific community consists of men who share a paradigm. (1970a:176).

The community may be large, e.g. all physicists, or it may be small no more than 25, for example those dealing in a specialized area of science.

A paradigm entails theory but it is much wider than that. In fact Kuhn suggests that it is a 'disciplinary matrix' and it involves—

1. A shared symbolic generalization. Thus for example, all physicists know what is meant by the expression $F = ma$, it is not necessary to define or discuss the terms as such.
2. Metaphysical parts of a paradigm. These are often the devices employed in learning or teaching the paradigm, for example physicists may think of molecules as elastic billiard balls subject to random motion.
3. Values are shared. Especially important in this regard are the values which determine the acceptable accuracy of prediction in experiments. These values are also used to judge the acceptability of whole theories and as to whether they present *puzzles* which may be formulated and solved.
4. Most important of all are the shared *exemplars*. These are the examples which are incorporated in text books and it is the solution of them that forms the basis of apprenticeship to the community. They are the basis of laboratory learning and examination questions (the sort of questions commonly found at the end of text book chapters). It is through these that a student is taught how to solve similar problems (i.e. problems that appear similar and to which an answer is guaranteed). It is also through these, and especially the text books which explain them, that the members of the community acquire what has been called 'tacit knowledge'. That is the knowledge that is *not* directly taught, but rather achieved by osmosis. This is acquired by 'learning by doing rather than by acquiring rules for doing it'. (ibid:191). It is a consequence of the sharing of a paradigm that its members learn to *see things in the same way.*

The central value of a paradigm is that *it sets the problem to be solved.* In addition the paradigm is often implicated directly in the design of apparatus able to solve the problem. Thus the paradigm focuses activity towards a goal, it defines the bounds of legitimate activity and sets the standards by which its practitioners will be judged.

The practitioners within the paradigm are what Kuhn calls *Normal Scientists*. They seek to solve the *puzzles* which the paradigm has set. These puzzles, like jigsaws and crosswords are *believed* to have answers and the paradigm has provided the promise of solving them. Normal science is the actualization of that promise. (ibid:24). It is engaged in what Kuhn calls 'mopping up' work. The paradigm having laid out the area to be covered, the normal scientists fill it in. In doing this they may at times be isolated from socially important real world problems. (ibid:37). Even so, Kuhn sees their work as essential to the advancement of science. Their activities are what Kuhn calls 'puzzle solving' and it is, he asserts, an article of faith of the scientific community that all their puzzles have solutions.

The normal scientist's motivation then is to solve puzzles and in so doing to gain the acclaim of his peers. But in solving the puzzles he must remain within the paradigm; if he goes outside it he ceases to be a 'scientist' in the view of his erstwhile colleagues. However, the rules are not spelled out, the scientists simply rely on tacit knowledge, and just as we know what a chair, a leaf or a game is without actually being able to define it, so members of a paradigm know the rules. (ibid:44)

Under such circumstances whilst the practitioners may know what they are doing, they may not know the basis for their actions. It is only at times of *crisis* or before the development of an initial paradigm that there is debate over the basic philosophy of the paradigm. Once the paradigm is established it is no longer questioned. However, eventually, as normal science proceeds either a discovery occurs or a puzzle is found to be not practicable and as a result the profession becomes aware that nature has somehow violated the paradigm. This is the beginning of a crisis.

The crisis may have traumatic effects on those who share the paradigm for they have been taught that the puzzle can be solved, but, try as they might, they cannot solve it. It is as if there were a problem in 'perception'. This Kuhn illustrates by reference to an experiment in which subjects were shown cards, most of which were normal but some of which were wrongly marked, for example a six of spades in red, or a four of hearts in black. After repeated showing of these cards to individual subjects ten per cent of them were unable to explain what they had seen and, unable to understand what is wrong, became most distressed.

> One of them exclaimed: 'I can't make the suit out, whatever it is. It didn't even look like a card that time. I don't know what colour it is now or whether it's a spade or a heart. I'm not even sure now what a spade looks like. My God!' (ibid:63, 64).

In the same way, Kuhn asserts, the profound awareness of a scientific crisis leads to a professional insecurity. Despite their unease the scientists remain dogged, for, as Kuhn points out, to reject the paradigm is to cease to be a scientist. Besides in science, as in industry, there is a high cost to retooling. (ibid:76). But, *pace* Popper, the scientists do not see these 'counter-instances' as falsifications of their theories. They pay more and more attention to them, possibly offering, halfheartedly, alternative solutions to them. Kuhn emphasizes that the tension that this generates may be enormous. Eventually a new paradigm emerges. It may have been in existence before the crisis or, if subsequent to a discovery, developed after the crisis. In either case it is the crisis which

> simultaneously loosens the stereotypes and provides the incremental data necessary for a fundamental paradigm shift. (ibid:89).

Often the innovators, that is those who provide the new paradigm, will be either 'outsiders' or young men. (ibid:90).

The change is revolutionary and Kuhn likens it to Gestalt shifts (i.e. the different ways in which, say, an ink blot is perceived at different moments) or to political revolutions. The adoption of the new paradigm will be resisted and even though persuasion may be used there will be a partial circularity in the argument of the practitioners of the different paradigms so that the same term will have different meanings and people will talk through each other. (ibid:109).

Thus there arises the problem of paradigm choice. Kuhn asserts that different paradigms are 'incommensurable.' To be commensurate he claims that it would be necessary for different paradigms to share a common language and vocabulary but such, he asserts, do not exist.

> In the transition from one theory to the next words change their meanings or conditions of applicability in subtle ways. Though most of the same signs are used before and after a revolution—e.g. force, mass, element, compound, cell—the ways in which some of them attach to nature has somehow changed. Successive theories are thus, we say, incommensurable. (1970b:266, 267).

It is for this reason that Kuhn claims that paradigms are *never* chosen by comparison with the real world. They are chosen as against each other and whether one or another is accepted is:

1. For old scientists, a matter of conversion. For them, after accepting the new paradigm, the world has become different! It is as if there has been a Gestalt shift.
2. Young scientists simply choose on the basis of persuasion. For them there is no obvious difficulty and no 'conversion'.

As a result the revolution is usually invisible. This is especially so, says Kuhn, because text books are written in such a way that revolutionary changes are glossed over.

In discussing Kuhn's theory Popper emphasizes that he believes it to be mistaken, even though based on a 'fashionable thesis: the thesis of *relativism*' (1970:56) Popper admits

> that it is much easier to discuss puzzles within an accepted common framework, and to be swept along by the tide of a new ruling fashion into a new framework, than to discuss fundamentals—that is, the very framework of our assumptions. [And that] . . . at any moment we are prisoners caught in the framework of our theories; our expectations; our past experiences; our language. But . . . if we try, we can break out of our framework at any time. (ibid:56).

He agrees that Kuhn's normal scientists exist, but he insists that they are to be pitied. (ibid:52).

Popper has provided us with what might be called 'standards of excellence for science'. He has outlined what he considers to be the essentials of scientific method and insists on the continued critical appraisal both of the framework of assumptions and of the methods of operation within that framework. Kuhn, on the other hand, has told us that scientists just do not work like that. Normally they accept uncritically the assumptions of their paradigm and seek simply to operate within its framework. Popper, in insisting on the need to hold all hypotheses open to testing, directs attention towards the premises of a theory and thus he may be said to focus attention on the critical acceptance of 'laws' arrived at by induction. Kuhn on the other hand in describing the social system in which scientists operate emphasizes that scientists are mainly engaged in deductive work. In that they stand in a long tradition, stemming from the days of the early Greek philosophers, who, Russell tells us, unduly emphasized deduction in their theory of knowledge. This was due, in part at least, to their devotion to geometry which

> as established by the Greeks, starts with axioms which are (or are deemed to be) self-evident, and proceeds, by deductive reasoning, to arrive at theorems that are very far from self evident . . . It thus appeared to be possible to discover things about the actual world by first noticing what is self evident and then using deduction. (Russell, 1961:55).

Such a view is, of course, mistaken. For as Russell elsewhere points out 'All the important inferences outside logic and pure mathematics are inductive, not deductive'. (ibid:209, 210).

Australian Agricultural Economics

In a recent article John Phillips has sought to evaluate resource allocation within agricultural economics research in Australia during 1958 to 1973.

He classified and analysed 383 articles published in the principal relevant journals and concluded that

> There was reason . . . to suspect an undue emphasis on methodology at the expense of both the solution of significant problems and perhaps also at the expense of creativity. (1975:56).

Whilst he did not make his analysis within a specifically Kuhnian framework his results seem to suggest that agricultural economists in Australia are in danger of being locked into a particular paradigm. For, as he said

> Research, probably, is more subject to recursivenes because the practitioners become, or are, teachers and they tend to direct their students along similar paths to their own. This may be particularly so with technique—oriented research. (ibid).

Of course agricultural economics as practised in Australia, as elsewhere, is an applied science but that does not mean that it ought uncritically to be devoted to theories handed down from one generation of technologists to the next. If that were the case then its practitioners would warrant the criticism which Popper made of certain engineering students who

> merely wanted to 'know the facts'. Theories or hypotheses which are not 'generally accepted' but problematic, were unwanted: they made the students uneasy. These students wanted to know only those things, those facts, which they might apply with a good conscience, and without heartsearching. (1970:53).

Such an attitude, Popper asserted, is 'a danger to science and, indeed, our own civilization' (ibid.).

If Australian agricultural economics is to be truly scientific it must be creative and analytic as well as being concerned with the application of well tried techniques to trivial problems and so it needs to be as concerned with the inductive as with the deductive aspects of its science.

Its dominant paradigm has however, since the 1950's, been that form of microeconomics which is best described as 'positive' and emanating from the 'Chicago School' and which places little emphasis on the inductive but greatly stresses the deductive aspects of economics. As a result little discussion takes place with respect to the analytical framework but much emphasis is placed on the deductive tools of mathematical analysis. In this sense Australian agricultural economics conforms well to the model outlined by Milton Friedman in his essay 'The Methodology of Positive Economics'. There Friedman emphasizes that 'Positive economics is in principle independent of any ethical position or normative judgements' and that it is, or at least ought to be, an 'objective' science. (1971:24). He believes that it is the economist's task to formulate a hypothesis which must then be tested on its predictive power. A 'hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted'. (ibid:27). Friedman seems totally unaware that the very formulation of the hypothesis is in fact a normative act, and to that extent in his own terms, non-objective. This point has been well made by Popper who insists that

> observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem . . . A point of view . . . for the scientist [is provided] by his theoretical interests, the special problem under investigation, his conjectures and anticipations, and the theories which he accepts as a kind of background: his frame of reference, his 'horizon of expectations'. (1965:46, 47).

Friedman is not worried about that, nor is he concerned about the truth of the assumptions that are embodied in the hypothesis.
In fact he says that

> Truly important and significant hypotheses will be found to have "assumptions" that are widely inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions . . . To be important, therefore, a hypothesis must be descriptively false in its assumptions . . . the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic" for they never are, but whether they are sufficiently good approximations for the purpose in hand . . . which means whether [the theory] yields sufficiently accurate predictions. (1971:30).

Thus, for Friedman, the importance of a hypothesis is simply its capacity to predict and for him simply to predict is to explain. That is an extraordinarily narrow view of explanation which in economics requires the seeking out from the complex set of 'economic' events some sort of regularity of occurrences of such events so that it may be possible to test hypotheses describing their causal ordering. Thus, whilst Friedman is content to say that it does not matter if his assumptions are untrue and that all that matters is that the predictions be true, his critics would emphasize, with Nagel, that

> if by an assumption of a theory we understand one of the theory's fundamental statements . . . a theory with an unrealistic assumption is patently unsatisfactory; for such a theory entails consequences that are incompatible with observed fact, so that on pain of rejecting elementary logical canons the theory must also be rejected. (1971: 51).

Probably the two most basic assumptions of positive economics are the assertions of the existence of universal scarcity and a Walrasian equilibrium.

The first assumption is embodied in Robbins' definition of economics as being the study of 'human behaviour as a relationship between ends and scarce means which have alternative uses' (1935:16). In fact for many economists the existence of universal scarcity has become an article of faith. Even so, as Joan Robinson and others have pointed out, at the very moment that Robbins was enunciating his definition the world was suffering not from universal scarcity but from a plenitude of unutilized resources, and especially of labour. Nevertheless this assumption has led to positive economists being unswervingly committed to growth. Their pursuit of that goal has often seemed unquestioning and so, for instance, Harry Johnson can happily write

> there is likely to be a conflict between rapid growth and an equitable distribution of income; and a poor country anxious to develop would probably be well advised not to worry too much about the distribution of income. (1970:683).

Clearly Johnson equates 'growth' with 'development' but for others growth which does not enhance social justice cannot be so equated. (Each point of view is, of course, equally a value judgment).

The second assumption of positive economics is so fundamental that it is rarely stated. It remains implicit and unquestioned even though, as Blaug has pointed out

> nearly all economics nowadays *is* Walrasian [i.e. equilibrium] economics. Certainly, modern theories of money, of international trade, of employment, and of economic growth are general equilibrium theories in simplified form . . . All of modern macro- and micro-economics can be viewed as different ways of giving operational relevance to general equilibrium analysis. (1968:587, 88).

One most important consequence of the equilibrium assumption is that prices have become central to economic analysis based on the assumption. They are considered to provide the most important descriptive information about the economy and are assumed to have been determined by 'supply' and 'demand' according to the well known equi-marginal principles of neo-classical economics.

It is through the manipulation of these 'laws' of supply and demand that the practitioners of positive economics seek to analyse economic problems. Further it is because of the importance of prices in this paradigm that their fluctuations tend to define 'economic problems'. Thus the methodology is essentially deductive, rather than inductive and the positivists' claim to being scientific lies not in their full application of scientific method but in their assertion that they test the truth of their theories through their predictive value. That is they assert that their deductive reasoning leads to conclusions which can be tested through their predictive value. However, even the application of this criterion is not as simple as Friedman and others might suggest. Thus Coddington has pointed out that positive economists claim that they return to the real world to test their theories but that their method of doing so is 'silent on the crucial and controversial point of *what* is to count as evidence' (1972:9). Thus they seem to be unaware that so called 'facts' do not have an existence separate from the observer. To repeat Popper, 'observation is always selective'.

> Another critic has made an even more telling point in asserting that the failure of positive economics . . . is to be seen particularly in the fact that no major economic hypothesis has yet been successfully refuted. (Katouzian, 1974:282).

This he asserts is partly due to the hypotheses being formulated in a way that they are 'strictly speaking irrefutable' (and therefore in Popper's terms 'metaphysical' or 'non-scientific'). However, Katouzian believes that the main reason for economists failing to reject theories because of the inability correctly to predict

> is the faith of the economists, including the applied economists, in their own 'better judgment' when a major hypothesis is refuted by tests which are then admitted to use inadequate information or techniques (ibid).

It is for these reasons and especially because positive economists place little emphasis on the truth of their assumptions that I believe their methodology cannot be termed scientific in Popper's terms. However, in their deductive activities positive economists seem to behave like the puzzle-solving normal scientists of Kuhn and it may be that in his terms positive economics may be considered to be 'scientific'.

The effect of failing critically to examine the assumptions of their methodology has led agricultural economists in 'developing' countries often to give inappropriate advice. This, one American professor of agricultural economics claims, is due to the fact that

> Western-trained economists are market minded to a remarkable degree . . . [so that] When thought moves from microeconomic theoretical concepts to agricultural development policy . . . the prescriptions for policy take the form of accepting competitive price theory as the basic rationale for development policy from which are derived the conditions which need to be met by institutional innovations if markets are to function effectively . . . [Thus] the market problem in this approach to agricultural development virtually

> is resolved into stating the conditions which need to be met if "package programs" for modern "inputs" are to be feasible in terms of costs and returns. (Parsons, 1974:738).

He insists, and I agree with him, that such an approach is to put the cart before the horse. If policy is to be aimed at the development of the market then this requires

> that the development of economic systems be seen from the primary viewpoint of the achievement of an economy as a system of human organization—a system of power, authority, and stabilized procedures. (ibid:739).

Hence if agricultural economists are to be truly adaptable they must learn to think critically about their assumptions. That is they must learn to think inductively as well as deductively.

Ross Parish has stated that in his opinion Australian agricultural economics has made a major contribution to the Australian economics profession as a whole through its having 'a heavy emphasis on econometric research and mathematical techniques of analysis'. (1969:4) That is it is through its deductive approach that it has made its major contribution. Certainly there seems to have been very little emphasis on induction. One way of testing the relative importance of the two aspects of scientific method to the profession is to analyse recent writings in the professional journal and I have attempted to do this by broadly classifying the 86 articles which have appeared in the six volumes of the *Australian Journal of Agricultural Economics* commencing with Volume 12 No. 1. I consider that 27 of the articles cannot readily be classified as either 'inductive' or 'deductive'. They are either descriptive of current market situations (such as Bieda's 1970 article 'Future Australian —Japan Trade Relations' or Sturgess' 1968 review article on the Wool Board's Second Report on Marketing) or descriptive of the current stage of the development of the profession. (As commonly occurs in Presidential Addresses such as those of Parish, 1969, Lloyd, 1970, Harris, 1971 or Burns, 1973).

I have classified five articles as 'inductive'. Each of these questions in some way the uncritical application of conventional theory. Three (those of Penny, 1968, Mellor, 1969, and Baker and Bhargava, 1974) discuss the institutional framework of 'traditional agriculture' and in so doing cast doubt on the direct application of the prescriptions of 'conventional' agricultural economics in L. D. C.'s. One article (Standen, 1972) discusses the applicability of theoretical concepts of efficiency and welfare to the problem of 'farmer welfare'. The fifth, that of Francisco and Anderson (1972), discusses the risk aversion behaviour of a group of pastoralists and points out that the range of utility functions experimentally determined

> indicated a diversity of utility-maximizing stocking rates ranging from about 15 to 25 acres per sheep with most differing substantially from the rates that maximize expected monetary value. (1972:91).

I have classified the remaining 54 articles as 'deductive'. Most accept entirely uncritically the assumptions of neo-classical economics that producers are profit maximizers and that provided the efficiency criteria of welfare economics are achieved social welfare will be maximized. Certainly no other criterion of social justice other than that implied by the 'Pareto criteria' is ever suggested. Most (37) employ the methodology of neo-classical economics, i.e. supply and demand analysis, in their description of particular economic phenomena (for example price movements on the effect of the imposition of output quotas) or in their discussion of agricultural policy. In additional several articles (14) are devoted to the development, or popularization, of new techniques of analysis such as simulation studies or spectral analysis. Such articles are clearly entirely deductive in approach. One article, (Chisholm *et al.*, 1974) seeks to extend neo-classicism into a 'new' era, that of Pollution and Resource Allocation. That, too, is deductive and in full accord with Kuhn's concept of the articulation of paradigms to new areas of interest (cf. Kuhn, 1970a:29).

Despite the outpourings of the Cambridge, England, School which have been critical of the concept of the aggregate production function none of the 32 articles which I have classified as 'deductive' and which have employed the concept have in any way sought to justify its use. One article (Anderson and Powell, 1973) is, admittedly, somewhat critical of the validity of statistically estimated farm production functions but it nevertheless proceeds, on the basis that they are valid, to determine whether economies of size exist in Australian agriculture. Another (Johnson, 1971) which in effect points out the practical impossibility of establishing aggregate production functions nevertheless concludes that his

> approach [which employs them] has considerable heuristic value in drawing attention to the kinds of problem involved in formalizing production function relationships. (ibid:158).

Only those who are lost in their deductive techniques could consider such a teaching method worthwhile!

Problems of Induction in Agricultural Economics

Having illustrated that little emphasis is placed on questioning the theoretical framework within which Australian agricultural economics operates I now wish to turn to a consideration of some of the problems of induction associated with that framework.

One of the major problems associated with the estimation of causal relationships between economic variables, such as occurs in production functions, may be illustrated by the attempt to represent such a relationship functionally in the following terms

$$Y_t = f\ (Y_{t-1}, X_{i,t})$$

where Y_t is the value of the variable at time t and Y_{t-1} at time t_{-1} and $X_{i,t}$ ($i = 1$ to n where n is indefinite) represents the values at time t, of the n other economic variables which affect Y_t.

It is quite clear that if Y_t can be readily quantified so too can Y_{t-1} It is not, however, at all clear that all the X's may be equally well quantified, nor is it clear that they can all be known!

The mathematical function $f(\ldots)$ itself may well be highly complex and, as Freebairn has recently said,

> may include continuous and discontinuous functions, equalities and inequalities and linear and complex functions ... [and that] it is

> unlikely that the true underlying causal relationship will be known. (1975:5).

All of this points to the extremely complex task of formalizing economic relationships. The problem of doing so has been discussed by Carl Christ who has pointed out that

> In actual econometric practice it is typically impossible to find a sufficiently specific model or maintained hypothesis that we can believe with certainty ...
> In this situation the economist has several choices. He can give up ... He can somehow choose *one* reasonable form of the equation, on grounds that are in part neither theoretical nor empirical, and use it with relevant data to draw inferences. Or he can try out several different theoretically reasonable forms, in a sort of experimental fashion, confronting each one with relevant data, and then choose among them after he sees how well they fit the data. (1966:7, 8).

As is well known Christ has written an excellent econometric text book and so, like all normal scientists when confronted with the sort of situation he describes, he continues to operate within his paradigm. It therefore should not surprise us that in this case he opts for the third choice on his list, and in so doing he suggests that

> it offers some more or less objective grounds for preferring one form of an equation to other forms that are about equally reasonable from a theoretical standpoint. (ibid.).

Thus the scientifically 'objective' basis of his 'deductive' methodology becomes eroded to a situation in which it is simply 'more or less objective' and indeed the whole 'experimental method' becomes one based on *a priori* restrictions and assumptions. Thus the mathematical function chosen as the basic model for analysis is selected from the multitude available on *a priori* and pragmatic grounds, as indeed are the variables included within it. All the X variables excluded from the function and all the estimated parameters within the function are considered for the purposes of prediction to be constant and are termed 'structural'. Having so termed them they, and all other aspects of the 'structure' of the economy in which they are embedded, are disregarded.

The predictive criterion emphasized by Friedman in his discussion of positive economics provides little assurance of the scientific respectability of the methodology when it is employed in practice. Christ agrees with the maxim that 'the acid test of an econometric model or equation is its ability to predict (or even to describe) data that were not used in its construction and estimation' (ibid:546). However, in discussing the applicability of this criterion he emphasizes that the model must be confronted with an entirely new set of data. That is the model is tested in such a way that it adequately describes the original (sample) data and also the new (test) data. If such a test proves unsatisfactory then the conclusion is

> either that a structural change occurred between the times [of sample and test] ... *or* (at least as likely) that the [original] equation is inadequate as an explanation of the combined period ... In either case ... the equation does not represent a theory adequate to explain

the behaviour of *both* the sample period and the prediction period in one general framework with a constant structure, and that therefore it may be desirable to revise [the] equation by including other variables, by changing its form, and so forth. (ibid:554).

As Christ admits 'there is an endless variety of possibilities' and it is just because of this that the practitioners of this form of economics need never experience a Kuhnian crisis. They are able to continue, throughout their professional lives, constantly estimating and re-estimating equations and variables all of which are chosen on subjective, *a priori* grounds which are themselves never criticized. Even so they seem loth to admit that basic flaw in their claim to be 'objective', instead many seem to reserve to themselves the right to criticise other economists for their overt subjectivity. Thus, for instance, Freebairn, in comparing formal econometric models with informal models used for forecasting says of the latter that

the greater importance of subjective judgment and less reliance on quantitative estimates of the effects of changes in explanatory variables would seem to increase the chances of personal biases having a marked effect on the forecasts and of a failure to impose consistency constraints on the final forecasts. (1975:20).

In so saying Freebairn seems to be totally unaware of the subjective basis of 'formal econometric models', for otherwise he would realize that to compare the 'importance of subjective judgments' is in fact meaningless. Thus for example it is a subjective judgment to include, or not, a structural parameter related to the imposition of a trade embargo on an export crop and yet if such a parameter is omitted from the model and then an embargo is subsequently imposed the model will be useless no matter how elegantly were its endogenous variables estimated. The whole point is that the predictive accuracy of a model will depend entirely on the hypotheses on which it is based. It is for this reason that predictions based on the 'guesstimates' of those who know an industry well are likely to be better than those derived from a model constructed by an expert in multivariate analysis who nevertheless has little experience of that industry. That this is in fact so has by no means escaped the notice of the Australian practitioners of the art who are aware that forecasts made by the Bureau of Agricultural Economics on the basis of 'informal' models have often been more accurate than apparently more 'sophisticated' models. (cf. Freebairn, 1975:20-25). This is of course not surprising. However, instead of recognizing that their self-set task is impossible and that they are in a Kuhnian crisis, the econometricians continue, as normal scientists, to practise within their old paradigm making only minor changes to their apparently useless methodology.

Is a new approach possible?

All models of reality must be abstract, and to the extent that choices are made as to what variables are included or excluded from them they are necessarily subjective. However, subjectivity does not necessarily entail falsity. It is a matter of philosophic debate as to whether 'reality' is knowable or not—indeed it is a matter of debate as to whether there is such a thing as 'objective truth'. Kuhn seems at times to suggest that there is no such thing, that all we can hope for is different slants on events. These are achieved through different paradigms which successively give valid, but different, insights into the real world. In the sense that he sees science as being evolutionary but never finally coming to the 'knowledge of truth' he differs from the ancient Greeks of whom Russell wrote—

> [to them] mathematical knowledge appeared to be certain, exact, and applicable to the real world; moreover it was obtained by mere thinking, without the need of observation. Consequently, it was thought to supply an ideal, from which every-day empirical knowledge fell short. It was supposed, on the basis of mathematics, that thought is superior to sense, intuition to observation. If the world of sense does not fit mathematics, so much the worse for the world of sense. In various ways, methods of approaching nearer to the mathematician's ideal were sought, and the resulting suggestions were the source of much that was mistaken in metaphysics and theory of knowledge. (1961:53, 54).

In many senses conventional economists are like those ancient Greeks. They are captivated by deductive techniques. It seems, moreover, that they will not abandon their narrow methodology. For them the 'medium has become the message' and instead of questioning the philosophic basis of their method they simply seek continually to refine their techniques. Kuhn would suggest that they will eventually be confronted with a crisis which will lead to the adoption of a new paradigm. I am not so sanguine. He has emphasized that the scientific paradigm not only provides the methodology but it also provides the goals of scientific endeavour and thereby defines the 'legitimate' tasks of the scientist. Certainly this would seem to be so in the profession of economics. The dominant paradigm of Australian agricultural economics has made price determination of central importance and it has provided the tools of supply and demand analysis as means of solving the 'puzzle' so defined. It has further defined the legitimate methodology whereby hypotheses are framed in such a way that they can be 'tested' by their (quantitative) predictive ability. But just as the paradigm defines 'puzzles' so it also, according to Kuhn, increasingly directs the attention of normal scientists to insoluble puzzles. Thus once more prices take on a central role. The paradigm has provided the 'promise' of being able to forecast future market prices but as yet it has been unable to attain that goal, no matter how sophisticated its methods. In fact, of course, it is unlikely that it will ever be possible to forecast with significant precision prices determined under free market conditions. Demand will always remain unpredictable even though in some areas supply may not. (However even to suggest that forecasts of supply may become more accurate in agriculture is to be highly unrealistic in the case of commodities dependent upon the weather for their production).

Freebairn urges that we press on to the goal: all we need is a 'greater understanding of the underlying processes generating the variables to be forecast' and a 'greater understanding of, and ability to forecast, movements in international demand for Australia's exports' and all will be well (1975:26). Of course whilst there remains an apparently endless array of possible deductive techniques within the paradigm its practitioners will never be forced to admit of a Kuhnian crisis and a consequent need to reject their paradigm. Thus articles will continue to pour forth and conclude, in my mind pathetically, as they nearly all do, with the disclaimer that 'care should be taken in drawing policy conclusions from the equations' and at the same time suggest that further work in the same area is required.

I turn once more to the dominant paradigm of agricultural economics. If I were to follow Friedman I ought possibly to reject it entirely for it has not, in my opinion, fulfilled the predictive requirements which it has itself imposed. However, it is not for that reason that I reject it. I could await the crisis which Kuhn assures us always terminates a paradigm's effective life. But as I have explained I doubt that that will arrive for a very long time and anyway I agree with Kuhn when he points out that even when the crisis arrives many normal scientists can never bring themselves to abandon their old paradigm. In my assessment of the worth of a paradigm I choose not to employ the criteria of Friedman, rather I choose the following:

1. The premises of the theory must be true. That is they must not be at variance with observed reality and therefore they must constantly be subject to the sort of scrutiny which Popper recommends. It is not, in my opinion, at all satisfactory to forget one's assumptions and then hurry on to deduce a theorem on the basis of untrue premises.

2. The values of a paradigm must be in accord with my ethical values. The values of a paradigm will especially be revealed by its goals and if I do not accept the goals of a paradigm as being morally good then I shall reject it.

We need not quibble about the necessity of choosing between paradigms on the basis of moral standards. All paradigms necessarily contain moral values, at the very least these are associated with their basic truth but possibly even more importantly with their goals. Insofar as we choose between different paradigms we evaluate the (moral) worth of their different goals. When I consider positive economics I find that I must reject it for two reasons.

1. I deny that its assumptions of equilibrium and scarcity are universally true. If some of its predictions have been correct they have been so purely fortuitously. Of course the probability of accurate predictions being made on the basis of false premises is very low and we know that even the most sophisticated models employed in Australian agricultural economics have not been especially good.

2. I reject the values of the methodology and in particular I reject its goals. Positive economics is concerned with, and only concerned with, prices and allocation theory and in concentrating on these it places all institutional and structural factors into what Streeten calls 'the adapted *ceteris paribus* and automatic *mutatis mutandis* pound'. (1968:1946). Instead of being concerned with such factors the principal aims of positive economists are the prediction of prices and the maximization of growth. I don't wish to discuss growth in this paper but shall instead concentrate on prices. They, as is well

known, reflect, in an uncontrolled market, the dollar voting power of the participants in that market. Only rarely do they give an indication of the social reality lying behind the market. As Harry Johnson has himself pointed out

> the market rations supplies of consumer goods among consumers; this rationing is governed by the willingness [the ability?] of consumers to pay, and *provided the distribution of income is acceptable* it is a socially efficient process. (1970:683, my emphasis).

It is just that distribution of income which I believe to be crucially important. For the positivists prices are simply the signals employed in the allocation of scarce resources. Their fluctuations have no 'human' consequences. However, it is because of the influence of prices on incomes that I am not prepared to accept a system which happily permits the sort of violent fluctuations in price which have recently occurred on the cattle and sheep markets and which have had, and continue to have, disastrous consequences for a large number of Australian farmers and their families. As far as I am concerned such ought not to occur and it is essential that steps be taken to ensure that they do not. However, such problems are of no concern to the positivists. They simply keep on refining their mathematical models of supply and demand analysis which remain predicated on the concept of scarcity. That emphasis has turned the eyes of the profession away from distribution to growth. Thus we have become blind to local gluts and surpluses when agricultural and animal products are destroyed and quotas are placed upon their production even though elsewhere people may be in dire need of them.

I believe that Australian agricultural economists ought to seek a new paradigm in which to work. I believe that our best brains should direct themselves not to the ends defined by the existing positivist paradigm but rather to other, difficult but nevertheless tractable problems of agricultural political economy. This would require the development of a new paradigm. Its aim would be to understand the real workings of our economy not for the purpose of making mechanistic 'predictions' but to 'explain' the working of the economy through establishing the causal priorities of events within the economy. The establishment of a new paradigm of this sort requires that the economy be addressed not from the point of view of positive economics but from an entirely different point of view. The new paradigm would be deliberately policy oriented and its ultimate goal should be to achieve what might loosely be called 'social justice'. The economy would be seen to be subject to the purposeful intervention of man rather than to be apparently guided by Adam Smith's 'invisible hand'. Prices would be looked at from a 'human' rather than a 'market allocative' point of view. Thus the concept of the 'just price' would once more be a subject of debate and rather than being assumed to be equal to that determined in some mythical world of perfect competition it would be defined with respect to social goals. Thus under the new paradigm the structural and institutional features of the economy would come to the fore—poverty, unemployment and distribution of wealth and economic power being considered as valid matters for professional economic enquiry rather than the narrow range of price determination central to the existing paradigm.

I believe that Australian agricultural scientists have for too long operated as 'normal scientists' within an outmoded paradigm. As such they have dismissed those operating outside their paradigm as 'unscientific'. In that sense they are subject to Joan Robinson's criticism of 'pure theorists' who

> sometimes take a supercilious attitude to 'structuralists' or 'institutionalists'. They prefer a theory that is so pure as to be uncontaminated with any material content. (1971:142).

It is necessary, I believe, for Australian agricultural economists to remove themselves from their ivory towers and once more confront the data of the real world. But if they are to change their paradigm they will need to confront those data unblinkered by positivism and prepared to participate in a process of induction whereby the new 'laws' of the new paradigm will emerge.

References

Anderson, J. R. and Powell, R. A. (1973) 'Economies of Size in Australian Farming', *Aust. J. Agric. Econ.* Vol. 17:1-16.

Baker, C. B., and Bhargava, V. K. (1974) 'Financing Small-Farm Development in India', *Aust. J. Agric. Econ.*, Vol. 18:101-118.

Bieda, K. (1970) 'Future Australia-Japan Trade Relations', *Aust. J. Agric. Econ.* Vol. 14:150-169.

Blaug, M. (1968) *Economic Theory in Retrospect*, (London, Heinemann).

Breit, W. and Hochman, H. M. (1971) *Readings in Microeconomics*, (New York Holt, Rinehart and Winston).

Chisholm, T., Walsh, C., and Brennan, G. (1974) 'Pollution and Resource Allocation', *Aust. J. Agric. Econ.*, Vol. 18:1-21.

Christ, C. (1966) *Econometric Models and Methods*, (New York, Wiley).

Coddington, A. (1972) 'Positive Economics', *Canadian Journal of Economics*, Vol. 5:1-15.

Francisco, E. M. and Anderson, J. R. (1972) 'Chance and Choice West of the Darling', *Aust. J. Agric. Econ.* Vol. 16:82-93.

Freebairn, J. W. (1975) 'Forecasting for Australian Agriculture', Paper presented to 19th Annual Conference of Australian Agricultural Economics Society, La Trobe University.

Friedman, M. (1971) 'The Methodology of Positive Economics' in Breit and Hochman (1971) pp. 23-47.

Harris, S. F. (1971) 'Change in Agriculture: The Relevance of Agricultural Economics', *Aust. J. Agric. Econ.* Vol. 15:119-129.

Hume, D. (1964) *A Treatise of Human Nature* (London, Everyman).

Johnson, H. (1970) 'The Market Mechanism as an Instrument of Development' in Meier, G: *Leading Issues in Economic Development*, (Oxford University Press). 681-687.

Johnson, R. W. M. (1971) 'Aggregation of Micro-functions to Obtain a Whole-farm Production Function', *Aust. J. Agric. Econ.*, Vol. 15:151-160.

Katouzian, M. A. (1974) 'Scientific Method and Positive Economics', *Scottish J. Polit. Econ.* Vol. 21:279-287.

Kuhn, T. S. (1970a) *The Structure of Scientific Revolutions*, (Chicago University Press).

Kuhn, T. S. (1970b) 'Reflections on My Critics' in Lakatos and Musgrave (1970) pp. 231-278.

Lakatos, I., and Musgrave, A. (1970) *Criticism and the Growth of Knowledge*, (Cambridge University Press).

Lloyd, A. G. (1970) 'Some Current Policy Issues', *Aust. J. Agric. Econ.* Vol. 14:93-106.

Masterman, M. (1970) 'The Nature of a Paradigm' in Lakatos and Musgrave (1970) pp. 59-90.

Mellor, J. W. (1969) 'Production Economics and the Modernization of Traditional Agricultures', *Aust. J. Agric. Econ.,* Vol. 13:25-34.

Nagel, E. (1971) 'Assumptions in Economic Theory' in Breit and Hochman (1971) pp. 48-54.

Parish, R. M. (1969) 'Some Thoughts on the Role of the Agricultural Economics Profession in Australia', *Aust. J. Agric. Econ.* Vol. 13:1-7.

Parsons, K. H. (1974) 'The Institutional Basis of an Agricultural Market Economy', *Journal of Economic Issues,* Vol. 8:737-757.

Penny, D. H. (1968) 'Farm Credit Policy in the Early Stages of Agricultural Development', *Aust. J. Agric. Econ.,* Vol. 12:32-45.

Phillips, J. (1975) 'A Subjective Evaluation of Resource Allocation within Agricultural Economics Research: 1958-1973'. *Review of Marketing and Agricultural Economics* Vol. 43:52-56.

Popper, K. R. (1965) *Conjectures and Refutations,* (London, Routledge and Kegan Paul).

Popper, K. R. (1970) 'Normal Science and its Dangers' in Lakatos and Musgrave (1970) pp. 51-58.

Popper, K. R. (1972) *Objective Knowledge,* (Oxford University Press).

Robbins, L. (1935) *The Nature and Significance of Economic Science,* (London Macmillan).

Robinson, J. (1971) *Economic Heresies,* (New York, Basic Books).

Russell, B. (1961) *History of Western Philosophy,* (London, Allen & Unwin).

Standen, B. J. (1972) 'Evaluation of the Efficiency of Resource Use on Farms and the Welfare of Farm People', *Aust. J. Agric. Econ.* Vol. 16:34-44.

Streeten, Paul P. 'Economic Models and their Usefulness for Planning', Appendix 3 in Gunnar Myrdal, *Asian Drama,* New York, Pantheon, 1968.

Sturgess, I. M. (1968) 'The Wool Board's Second Report on Marketing: A Review Article', *Aust. J. Agric. Econ.* Vol. 12:16-34.