REFLECTIONS ON THE DEVELOPMENT OF ECONOMICS AS A DISCIPLINE

I went to G.L. Wood's 1950 Economic Geography I Lectures and his 1951 Public Finance Lectures (which were given at 8 o'clock in the morning) and, as a fourth year student in 1953, I was at the Funeral Service where the Rev. Irving Benson spoke eloquently about his old friend. In delivering the 1982 G.L. Wood Memorial Lecture, I hope I may honour the memory of a man who played such a significant role in the development of the Melbourne Commerce School.[1]

I

The Nobel Prize for Economics has been given for 12 years. In that time 19 distinguished economists have received it. Clearly the electors have been faced with a stock and flow problem: the need to catch up on those who, had the prize been given before 1969, would have received it long ago, as well as to recognize those whose appointed time has just come. While it would be ill-mannered to quarrel with the awards to those who have received the Nobel Prize - although some have managed to do so - there are some scandalous oversights. The most notable are Joan Robinson, an oversight which still can be redressed, though I doubt that it will be, and Roy Harrod, about whom, alas, nothing now can be done.

Whatever the merits of awarding a Nobel Prize for economics - one recipient has argued that as economics is not a hard science, there should not be a Nobel Prize for it and that he only accepted his because

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1. For an affectionate and appreciative memoir of G.L. Wood, see Mauldon, Economic Record, 1954.

he was told of his award early in the morning before he was properly awake, see Myrdal [Challenge, 1977, p. 52] - it does have the great advantage that the recipients are required to give a lecture at the ceremony at which they receive the award and that other economists are required to write essays about them, in order that the selection committee may judge why the subjects are fit and proper people to become Nobel Laureates.[2] These essays, most of which are published in what is now the Scandinavian Journal of Economics, are the source material for Sections II-IV of this lecture.[3]

In addition, the Nobel Prize winners are required to write autobiographical notes and these also have been sources for the lecture. While these various sources obviously preclude anything approaching a comprehensive cover, they do allow us to discern trends, cross currents, crises, controversies about which many of the best minds in our trade have written. Changing moods also may be discerned as we pass from the relatively optimistic days of the late 1960s to the uncertain, anxious and troubled times of the early 1980s. Aspirations, expectations, even hopes, have all been considerably dampened and this is reflected in some of the later essays. Indeed, considering how deep seated are our current problems, the spate of 'lament for econmics' presidential addresses of the early 1970s now seem, with hindsight, to have been decidedly premature. It should be said, though, that Marshall's "cool heads but warm hearts" are well represented - to date - amongst the Nobel

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2. It makes us speculate how many people may have missed out because of the β^- quality of the essays on them.

3. Sources below identified only by year refer to this Journal.

Laureates. The compassionate, decent, humane personalities that come through the essays, both their own and those of their sympathetic admirers,[4] are admirable. Bent Hansen and Lloyd Reynolds think that their subjects - Jan Tinbergen (1969) and Gunnar Nyrdal (1973) - just as appropriately could have received the Nobel Prize for Peace.[5]

Most of the recipients still see our discipline as the means of analyzing the malfunctioning of our economies - the existence of poverty, inequality, unemployment, discrimination and inflation. Sometimes their vision extends to encompass the world - Wassily Leontief [1974] prepared an input-output model of the world for his lecture. Their object is to obtain an understanding which will allow the lot of ordinary men and women to be improved. Even those who are opposed to discretionary government intervention nevertheless base their case on the damage which they believe will ensue - and has ensued. This is because, on their understanding, the system left to itself - and provided only that economic and political power are widely diffused, and competition is

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4. Possibly the one exception to this is the late Harry Johnson's essay on James Meade, which is certainly the most critical of all the essays. This, no doubt, was Johnson's way of showing his respect and admiration for Meade - that Meade would still emerge as a giant despite the critical evaluations of his work. Audrey Silberston [Economic Analysis and Policy, 1977, 1978] did the same thing for Johnson himself.

5. "...the humanitarian idealism which Nobel wanted to reward has no representative in our profession as fine and noble as Jan Tinbergen. As much as he is respected in the privileged world, he is beloved by the underprivileged. Always at their service, always on their side, always working to improve their conditions, Tinbergen would be an equally worthy candidate for the Nobel Peace Prize" (Hansen). "One could not improve on this as a tribute also to Gunnar Myrdal" (Reynolds).

strong, including letting the losers fall out, at least economically - will provide the best environment for free persons to fulfil themselves. Hayek [1975] is the most articulate proponent of this view. He stresses the limitations of knowledge and of the gathering of information which the market can supersede but no one brain, or combination of brains, can. As Hahn [Lloyds Bank Review, 1982] has documented, Hayek argued "that [while] economically relevant information was highly decentralised [, the price system] successfully aggregates this information so that the economy behaves as if there had been no specialised knowledge in the first place" (p. 16). Hahn adds that "Hayek did not prove this to be so and... only very recently [have] we ... understood the circumstances in which the claim made is correct" (p.16). Nevertheless, Hayek argues his case in a civilised manner, exactly as in The Road to Serfdom of which Schumpeter [Journal of Political Economy, 1946, p.269] said: "[I]t is ... a polite book that hardly ever attributes to opponents anything beyond intellectual error".

II

A continuing theme that runs through the Lectures and Essays is that our discipline is a science, evidently a very superior buzz word. Great stress is put on this, though I suspect that the users do not always mean the same thing, which is hardly surprising with a spectrum that spans from Frisch to Friedman. Frisch thought that we are scientists in the service of people and that all our problems should be stated in such a way that the solution could be set out explicitly. Frisch did not believe in cutting corners in the interest of communicating,

of being understood. (I hasten to add that he was an excellent expositor but that he never minimised the difficulties of a problem by ignoring them). He would range over a number of areas as problems presented themselves in the process of moving towards a solution. His essayist, Leif Johansen [1969], illustrates this trait by reference to Frisch's 1933 paper on the business cycle, "Propagation Problems and Impulse Problems in Dynamic Economics". "This trade cycle model is more refined and comprehensive than several [others] that have become well known ... Frisch's model results in a mixed difference - differential equation ... more difficult to solve than the pure difference ... or pure differential equations ... found in later trade cycle models ... another typical example of the fact that Frisch never chooses the line of least resistance ... did not select the easy way of reformulating his ... models so as to obtain one of these pure forms. Instead, he worked out (in collaboration with mathematicians) a complete solution of the type of equation which arose ... not previously been solved in mathematical literature" (pp. 314-315).

In this respect Frisch differed from Tinbergen, who believes that diminishing returns to complex and refined methods set in very quickly in a social science where the urgency of the problems concerned require a "crash through" mentality combined with relatively crude but effective methods. (Gough Whitlam could have used him on his staff). Thus, Bent Hansen [1969] in commenting on Tinbergen's theory of economic policy, says of it that it "is quite simple but it is concerned with the actual macro-policy targets of politicians in actually existing economies and has proved powerful and applicable to current policy problems ... a

characteristic feature of all the scientific work of Tinbergen ... never much interested in esoteric theorizing for theory's own sake. His preoccupation has always been quantification and empirical application ... as soon as a method "works" empirically he does not waste time on further theoretical refinements..."[6] (p.331).

Of course, there has long been a debate about whether social sciences can be regarded as akin to natural sciences or whether our problems and data are such as to preclude this, especially the problem that the subjects of analysis can find out what the observers have found out about them - and change their behaviour accordingly. (This is an insight which the rational expectations school are currently exploiting for their own ideological purposes of urging non-intervention). Keynes, it will be remembered, thought that economics was a moral science in which intuition and ethics, "introspection and values", played as important roles as so-called scientific methodology. The latter "leads directly counter to the habit of mind which is most important for an economist to acquire". He further argued that "[Economics] deals with motives, expectations, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether it is worth while falling to

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6. Hansen compares Tinbergen's work and approach with the Anglo-Saxon work on welfare economics of the 1940s and 1950s which he considers to be elegant and refined but "sterile and difficult to apply to practical policy problems". One amusing side effect of the Nobel Prize awards is that the Scandinavians seem to be getting their own back on the Brits in particular for the arrogance, the "unnecessary originality", which the British economists displayed in their heyday.

the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth" (Keynes, Collected Writings, Vol. XIV, p.300).

Friedman [Journal of Political Economy, 1977] has some challenging views on the issue. He says that he has never accepted that there are fundamental qualitative differences as between social sciences and natural sciences. "In both there is no "certain" substantive knowledge, only tentative hypotheses that can never be "proved" but can only fail to be rejected, hypotheses in which we may have more or less confidence ... positive knowledge grows by reaction to failure ... to predict ... by patching up until someone suggests a new hypothesis that more simply or elegantly embodies the troublesome phenomena ... In both ... no way to have a self-contained closed system or to avoid interaction between the observer and the observed" (p. 452). These reflections, of course, lead to Friedman's account of his triumph over (what he takes to be) Keynesianism - the relation between inflation and unemployment, "an admirable illustration because it has been a powerful political issue ... yet the drastic change ... in accepted professional views was produced primarily by the scientific response to experience that contradicted a tentatively accepted hypothesis - ... the classical process for the revision of a scientific hypothesis" (p.453).

In fact both Friedman and Hayek considerably over simplify the positions of those they are criticising. To Hayek, Keynes's theory "consists in the assertion that there exists a simple positive correlation between total employment and the size of the aggregate demand

for goods and services", see Hayek [1975, p. 433]. To Friedman, the battles with Keynes - to some extent anyway - are empirical arguments over the stability and values of the paramenters of certain key functions of IS/LM models. This, of course, is extraordinary since Keynes's own work was not meant to be confined within the context of a Walrasian model. His essential methodology, partly derivative from Marshall, partly from the classical economists, partly due to his own insights, was meant to escape from a general equilibrium methodology, i.e., a notion of self correction both in the system itself and in models of it, from simultaneity in the interrelationships of the important functions of the system, from including time in the analysis only in so far as it had the characteristics of space.[7] Hayek's characterisation is also quite at odds with the evidence of The General Theory itself, especially the discussions of the nature of the aggregate supply function (Chapter 3), the employment function (Chapter 20) and the theory of prices (Chapter 21), and with the evidence of Kahn's multiplier article of 1931.

It is interesting to reflect on how many of the Nobel Laureates have been either natural scientists or greatly influenced by them. Thus Tinbergen and Koopmans were physicists, Kantorovich was an applied mathematician, and Samuelson was greatly influenced by the Harvard mathematical physicist, Edwin Bidwell Wilson, whom he couples with Hansen, Schumpeter and Leontief to make an Age - so much so that Samuelson has kept up his interest in physics ever since. (For an

7. For Keynes's methods in The General Theory and after, see Kregel [Economic Journal, 1976].

Australian audience we might soberly reflect that John Stone started his intellectual life as a mathematical physicist.) Certainly Koopmans' and Samuelson's contributions reflect their early training and influence - elegance, rigour, preciseness of statement of the problem, belief that they will make break through in the end as they move from the simple cases to more complex ones. Thus Koopmans in his paper at the S'Agaro Conference wrote: "This constraint [the acceptance of the shadowy notion of perfect allocation that is guided by two seemingly opposite interpretations] is adopted on the hunch that aggregation is simpler within it than without it, while what is learned in this way may still be a worthwhile starting point for the study of more complicated situations" (p. 144). This optimism contrasts with Kaldor's well-known attack on neoclassical economics: "It is the hallmark of the neoclassical economist to believe that however severe the abstractions from which he is forced to start, he will 'win through' by the end of the day - bit by bit, if he only carries the analysis far enough, the scaffolding can be removed, leaving the basic structure intact. In fact, these props are never removed; the removal of any one of a number of them - as for example, allowing for increasing returns or learning-by-doing - is sufficient to cause the whole structure to collapse like a pack of cards" (Kaldor, Review of Economic Studies, 1966, p. 310).

Leontief's essayist, Robert Dorfman [1973], also has praised him as a scientist, especially as an applied one who cut through the practical difficulties of applying a theoretical idea - his great contribution. "There is a dominant theme that runs through Leontief's four decades of professional work ... that economics is an empirical and

applied science ... The only valid test of economic research is its empirical significance and its practical implications ... the clue to his discovery of input-output economics ... It was Leontief who first saw the practical potentiality of an input-output table and who learned how to really put one together" (pp. 430-31). Dorfman contrasts Leontief's achievement with what he describes as Sraffa's "very elegant independent discovery of the theoretical basis of input-output analysis" (p. 440). "Sraffa," he says, "was content to present some interesting and important logical relationships" whereas Leontief kicked on to show "how they could be measured ... confirmed ... and applied" (p. 440). This is an extraordinary howler, since Sraffa's ofject was entirely different to that of Leontief. Sraffa provides a critique of the supply and demand theories and points the way forward for a reemergence of the surplus theories of the classicals and Marx as applicable to modern problems, in the process solving some logical puzzles. Sraffa has made quite clear his understanding of the distinction between theory and measurement and has stressed that the former must be logically watertight, not only in order to be consistent but also in order to be able to survive. Theory is as important for sorting out conceptual issues and approaches as for providing theorems. This quibble apart, though, I certainly agree with Dorfman and applaud Leontief's philosophy which is very similar to those of the late Eric Russell and the late Wilfred Salter, to whose work I have always tried to pay tribute.

Associated also with the nature of the scientific procedures of economics, we have recently had the absorbing essay by Melvin Reder [Journal of Economic Literature, 1982] on Chicago then and now and the

unique characteristics of Chicago methodology. Reder sums this up in one proposition and four assumptions which need must form the basis of any hypothesis used to tackle a specific problem - if it is to be acceptable to Chicago. (The pages of the J.P.E. in recent years record how well this describes what is likely to get through their refereeing sieve. Except for a brief interlude in the late 1960s, early 1970s, under the more liberal and open-minded editorship of Robert Gordon, now safely moved away to Northwestern, the Chicago model as described by Reder is repeated over and over again.)

The Chicago view - "Tight Prior Equilibrium" in Reder's terms - "is rooted in the hypothesis that decision makers so allocate the resources under their control that there is no alternative allocation such that any one decision maker could have his expected utility increased without a reduction occurring in the expected utility of at least one other decision maker". To make the model yield testable hypotheses, we need to add four assumptions:

(1) "most individual transactors treat the prices of all goods and services that they buy or sell, as independent of the quantities that they transact;

(2) the prices at which individuals currently agree to transact are market clearing prices that are consistent with optimization by all decision makers;

(3) information bearing on prices and qualities of all things bought and sold, present and future, is acquired in the quantity

that makes its marginal cost equal to its price; information is treated like any other commodity;

(4) neither monopoly nor government action (through taxation or otherwise) affects relative prices or quantities sufficiently to prevent either marginal products or compensation of identical resources from being made approximately equal in all uses" (p. 11).

We may wonder as well what Chicago made of Herbert Simon's receipt of the Nobel Prize in 1979. Simon is most suspicious of the applications of the hypothesis of the heroic rational economic person maximising under constraints of formidable and daunting odds. He says that his contribution to economics is to change economic person into a sort of Woody Allen satisficing in an environment characterised by uncertainty, and a great deal of ignorance and anxiety. His Woody Allen is content to find the first sharp needle in a haystack that will do for the job in hand, rather than to continue to search for the sharpest, see Baumol [1979, p.76].

III

One obvious theme that emerges from reading the Nobel Prize essays is how technical the discipline has become, both in economic theory and in econometrics. The person most responsible for the mathematisation of economic theory is Samuelson - apart from The General Theory, Value and Capital and the Foundations are the two books

most often mentioned as key influences. The philosophy of the Foundations is directed towards establishing operational theorems as well as exploiting the unifying influences of the two great principles of the Foundations - maximisation or minimisation under constraints and the correspondence principle, the interconnection between statics and dynamics. In his Nobel Prize Lecture, Samuelson [1970], Samuelson reaffirms his faith in his youthful enthusiasm for such an approach, his disdain for literary economics, as opposed to his appreciation of the preciseness and rigor of the mathematical approach to theory. He does argue that the theory is more important, that mathematics is merely the agent (J. Willard Gibbs' saying, "Mathematics is a language", is on the title page of the Foundations) and, as I have argued elsewhere, Samuelson is no longer regarded in the trade as a mathematical economist, merely as a theoretical one.

In arguing this case, Samuelson seems not to have heeded - much - Boulding's review of the Foundations [Journal of Political Economy, 1948]. There, Boulding assesses the limitations as well as the advantages and the achievements of the mathematical method in a social science. One point that he makes (which is echoed in Shackle's assessment of Keynes's mode of theorising by use of language) is that mathematics can be a limited, one-dimensional mode of thought, unsuitable for dealing with complex structures. "[I]t makes very little sense to say "let Hamlet equal H and Macbeth equal M" (p. 236). He adds that it may be "an understatement to say that mathematics is a

language, for, while it is probably true that all mathematical expressions can be translated into "literary" language ... it is not true that all "literary" expressions can be translated into mathematics ... [Boulding knows] of no mathematical expression for the literary form, "I love you"," (pp. 236-37). Shackle (see Harcourt, Journal of Post Keynesian Economics, 1981, p. 144) has referred to Keynes's flair for choosing words which are evocative of the multi-dimensional concepts and sets of ideas that he is analysing - he mentions "sentiment" as a good example. By insisting that truth comes only in the guise of a mathematical model, we may so desiccate the factors that we are trying to include in our models that they become very poor vehicles for interpreting the processes at work in our economies.

Moreover, our ability to communicate both within the boundaries of the trade, and outside, to policy makers and ordinary citizens, is greatly reduced - a danger which Boulding clearly foresaw in 1948. Having admonished those 'literary' economists who "from wilful egotism ... refused to acquire that modicum of mathematical training which yields clearly increasing returns", he speculated that "the greatest danger [may now be] from the other side. The mathematicians themselves set up standards of generality and elegance in their expositions which are a bar to understanding". (p. 247) - they make a virtue of what is clearly a vice. I have, for example, on my desk at the moment, a collection of the important papers on the theory and econometric practice of the influential rational expectations school, edited by Lucas and Sargent [1981]. Even in the Introduction, incomprehension

sets in after the first couple of pages because the writers insist on using highly technical language. This contrasts with the ability of a Hahn, say, to communicate the gist of technical economics in an easily understood way, to discuss the conceptual issues involved, as he does in his "Winter of Our Discontent" paper [1973] and his Fred Hirsch Memorial Lecture [1982]. We must add that the very best mathematicians who have become economists do not try to mystify. As they are not overawed by mathematics as such, they are happy to take an independent look at economics as a separate discipline and to use the mathematics which they believe to be appropriate for it. Many of them are convinced that it is not a discipline which needs highly sophisticated mathematical models; they take almost a Marshallian stance, using simple models and often preferring pictures to squiggles, arithmetic to algebra, orders of magnitude as well as qualitative signs, as the most effective means of understanding and communicating. I think here of A.K. Dixit and Bob Rowthorn, Peter Warr and Ian Steedman as good examples. But it has to be said that they flow against a current of not-so-convinced practitioners, those who are still dazzled by mathematics as such and who pick problems as much for their mathematical tractability as for their economic importance.[8]

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8. This is not to say that so-called realism should be the overriding test for all classes of economic problems. Often important conceptual problems can be discussed and analyzed within the bounds of distinctly "unrealistsic", that is to say, highly abstract models. This has been recognised at least since the time of Ricardo and is reflected in some modern debates, for example, the controversies in capital theory, and those about whether the existence of surplus labour in the sphere of production is both necessary and sufficient for positive profits in the sphere of distribution and exchange in joint production models.

IV

When we look at the recipients of the Nobel Prize,[9] we see that three great issues of economics - Keynesianism versus Monetarism, the problems of development and the rise and rise of econometrics - are well represented. As to the first issue, a conjecture arises as to whether the electors are reacting to, or are trying to influence, the course of the debate itself. Thus Friedman received the prize in 1976 when Monetarism as a theoretical challenge was probably at its strongest.[10] But then, in recent years we have had Meade and Ohlin (1977), Klein (1980) who pioneered big econometric Keynesian macromodels, and, now (1981), James Tobin who, with Samuelson, Solow, the late Arthur Okun (who surely would have got a Nobel Prize, had he lived), and George Akerlof, are the outstanding American Keynesians.[11] What is significant

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9. I have neither the space nor the time to discuss the contributions of Myrdal, Lewis and Schultz to our understanding of the process of development, only to recommend to you their essays and their humanitarian concern for what are, after all, the most pressing problems of our age (or of any other, for that matter) and I have not the competence to discuss the econometric contributions.

10. Hayek was coupled with Myrdal in 1974 but his award was not so much, if at all, for his challenge to Keynes as for his broad philosophical and all round contributions - the nature of economic knowledge, the case for economic freedom, the relationship between society and its institutions, especially its legal ones. Indeed, his essayist, Fritz Machlup, thinks that Hayek's Pure Theory of Capital [1941] is his finest work, see Machlup [1974, p. 509].

11. I leave to one side the American branch of the post-Keynesian school - Paul Davidson, Donald Harris, Jan Kregel, Hyman Minsky, Edward Nell, Sidney Weintraub - not because they are not important but because they are not going to figure prominently in either central mainstream debates nor, alas, in Nobel Prize awards. For a discussion of Post-Keynesianism, see Harcourt [Thames Papers in Political Economy, 1982].

is that the recipients have addressed themselves to the policy questions of inflation and unemployment in open economies or, in the case of Tobin, how to model effectively the processes in such economies, in order to have a suitable basis for appropriate policy. Tobin [1981] is concerned to supplement the Keynesian income and expenditure accounts with explicit asset, liability and flow of funds statements, in order to bring out the Keynesian view of the interrelationships between the real and the monetary sectors of the economy.

Explicit in all these essays is the view that economies are not self regulating mechanisms, that they do not tend to settle at optimum positions from the points of view of the individual citizens of the economies concerned, so that there is a role for active, discretionary intervention in economic life. Increasingly this is being extended from traditional fiscal and monetary policies to related policies in labour markets, a situation forced on policy makers and the profession alike by the decline in competition in most markets and the experience during the Long Boom of higher levels of activity, employment - and growth - than when Keynes wrote. Of course, such intervention is not acceptable to either Friedman or Hayek, both of whom have a commitment to the market, to the use of the price mechanism, both on philosophical and, particularly in the case of Hayek, epistemological grounds. Hayek draws the following conclusion: "If man is not to do more harm than good in his efforts to improve the social order, he will have to learn that in this, as in all other fields where essential complexity of an organised kind prevails, he cannot acquire the full knowledge which would make

mastery of the events possible. He will therefore have to use what knowledge he can achieve, not to shape the results as a craftsman shapes his handiwork, but rather to cultivate a growth by providing the the appropriate environment, in the manner in which the gardener does this for his plants" (Hayek, [1975, p. 442, emphasis added]). Keynes wanted us to be humble folk like dentists - he obviously had good teeth. Hayek now wants us to be gardeners. I prefer Hayek's solution and Keynes's arguments.