An Intellectual Retrospect *

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To write about oneself is generally pleasurable – to be asked to write about oneself is doubly so. After all, it is not that one is taking oneself too seriously – other people are, and it is likely, so one is inclined to think, that they are right. I see the temptations and shall do my best to avoid them even if here and there I succumb. I will not provide an autobiographical sketch (I had a shot at that elsewhere),[1] but concentrate on my journeyings through economic theory. It is just possible that such an enterprise has the justification that some readers here or there may be stimulated to look at their own intellectual journey and reflect on it. In any case I hope to provide more "theory" than Hahn.

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Before I go to particulars I consider some generalities. Of these the use of mathematics in economics is a favourite topic for general musing. From my earliest days in economics to the present I have found the debates on this matter of little interest or consequence. There is indeed very little to discuss. From the beginning it seems that economic theory has been couched in formal argument, as formal as it was in the capacity of the writer to make it. One need only think of Malthus, Marx, or, say, Bohm-Bawerk. Marshall admitted that he first looked for a mathematical formulation before hiding it from the eyes of the "intelligent businessman" whom he regarded as

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* Contribution to a series of recollections on professional experiences of distinguished economists. This series opened with the September 1979 issue of this *Review*.

[1] Hahn, F.H. (1992), "Autobiographical notes with reflections" in Michael Szenberg ed., *Eminent Economists: Their Life Philosophies*, Cambridge University Press, Cambridge.

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his customer. Since then it is hard to think of examples where theorising was not either explicitly or implicitly mathematical. The injunction that such theories should always be translatable into English is so obviously foolish as to require no answer. After all, what sane person would translate the solution of a differential equation into English and what reasonable person would deny that such equations make a natural appearance in economics?

As I have said, there is little to discuss. There simply is no corpus of non-mathematical economic theory to provide a countervailing paradigm. But there is nonetheless a lesson which has only gradually been borne in on me which perhaps inclines me a little more favourably to the "anti-mathematics" group.

The great virtue of mathematical reasoning in economics is that by its precise account of assumptions it becomes crystal clear that application to the "real" world could at best be provisional. When a mathematical economist assumes that there is a three good economy lasting two periods, or that agents are infinitely lived (perhaps because they value the utility of their descendants which they know!), everyone can see that we are not dealing with any actual economy. The assumptions are there to enable certain results to emerge and not because they are to be taken descriptively.

This view which I have always held has however been severely strained by relatively recent developments, largely by American macro-economists. For instance a recent macro-text starts with the mathematics of Ramsey-like problems for a single agent which seems distinctly peculiar for the macro-enterprise which is to distill something useful out of economic analysis. Of course these macro-economists are not mathematical economists. They use mathematics to be found in texts but are quite unrigorous in their analysis of what would have to be the case for their exercises to be applicable. Nonetheless it looks as if the scientific air of mathematical reasoning has misled them into believing that they are saying something scientific.

Reflecting on this and indeed worrying over what I regard as a misuse of mathematical reasoning, I have come to the conclusion that the latter is not to blame. The blame lies in the first instance with Milton Friedman and in the second with the romantic desire to pass as a "scientist". It was Friedman's doctrine of "as if" and the general babble about the virtues of "simplicity", "beauty" and "elegance" which is largely responsible. If economics were a science with a body of doctrine confirmed by controlled experiments there might be something to be said for the Friedman line. But it is not and, as far as I know, no economic theory has ever been conclusively falsified by experiment, leave alone by statistical inference. To hang on the coattails of a theory of such seeming paradox as quantum mechanics, say, because "it works" will be justified when economists' theories predict correctly to eight decimal places as quantum theory does. Until then, the direct plausibility of our assumptions remains a test a theory applied to the "real" world must pass.

No doubt there are interesting things to be said about the social psychology of economic theorists. They are worse off than cosmologists who are not universally favoured by astronomers. There is a need to demonstrate that the subject is not "soft", etc. I do not know the answer. What I do know is that economists recently have given grounds for the suspicion that they are not economists at all. On the other hand, as I shall argue later, there has in fact been a flowering of applicable economic theory, and much of this is due to economists to whom mathematics is a natural language.

Looking back it seems to me that I have been faithful to a view of the subject which I held soon after I learned something about it. That view is that economic theory is best thought of as a kind of grammar – a way of speaking coherently about complex social events. It is at present not in a state which allows more than qualitative forecasts. Talk of "optimal" policies when meant seriously cannot be taken as such, although they often provide a starting point for argument. Pre-eminently I regard economic theory as an attempt to "understand" rather than to predict and prescribe. Economists have very useful contributions to make to policy debates etc., but their subject does not lead to unique, leave alone decisive and certain, answers. I have always been quite content in this modest role. Many of my colleagues are not, and many call themselves "scientists", a description which, when the term is taken in its common usage, seems to be somewhat presumptuous and premature. Our rather ambiguous reputation may be deserved given these overblown claims.

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Given my mathematical background it was inevitable that Samuelson's *Foundations*, Hicks's *Value and Capital* and Wicksell's *Lectures* should have impressed themselves on me when I first studied economics. I was particularly impressed by the reductionist methodology which attempts to explain and understand economy-wide phenomena through a theory of interacting agents. I was, and am, allergic to "holistic" theories. It was clear that Hicks and Samuelson made too little allowance for uncertainty in their formal analysis (Hicks for instance was content with single valued expectations), and there were quite a few other lacunae, but I was pretty convinced that they were pointing in the right direction.

However, the war had just finished, we were to have a new and fairer world, so instead of turning at once to General Equilibrium analysis I decided to write my Ph.D. on the Theory of Distribution[2] (published 1972). It was, I think, not a bad thesis. I proposed and developed a theory which has similarities with Kaldor's, published six years later. But the similarity ends with the common assumption which we made that workers and capitalists had different marginal propensities to consume. I was not fighting marginal productivity doctrines and developed the analysis almost entirely in a short period disequilibrium context. Indeed, I imagined an economy where changes in distribution brought about market clearing but where further changes would occur if the distribution left profit and utility maximisers dissatisfied. For the long run I had to assume imperfect competition (*à la* Kalecki) and suppose that somehow the degree of monopoly, and so the distribution of income, adjusted to bring about the equality between the "warranted" and the "natural" rate of growth. I do not think that this long run part of my analysis is very convincing. On the other hand the analysis of distribution over the cycle – that is, over sequences of short runs – I believe stands up quite well.

I published two chapters from this youthful work which led to the first of my communications from Joan Robinson, who seemed to approve of the line I was taking. I believe she liked my adaptation of the IS apparatus to the short period distribution problem. Unfortunately I needed to close the model by a theory of the supply of risk-capital which I did in a somewhat crude way by adopting Kalecki's principle of increasing risk. By today's standards the analysis here is somewhat crude and *ad hoc* although it was an attempt to take formal note of the role of uncertainty in determining investment in an imperfectly competitive world.

[2] Hahn, F.H. (1972), *The Share of Wages in the National Income: An Inquiry into the Theory of Distribution*, Weidenfeld and Nicolson, London.

Hicks and Lionel Robbins conducted the oral examination on a fine June afternoon. For some reason much of the discussion was about Wieser and his tombstone!

I was now a doctor and Assistant Lecturer in Birmingham and ready to return to micro-economics and general equilibrium Analysis. In Birmingham I had splendid colleagues including Terence Gorman who certainly is touched by genius. He is an accomplished mathematician and splendidly different from today's young: he produced numerous important results (*e.g.* duality theory of the consumer) but made little effort to publish more than those papers which by his own standards he regarded as definitely worth publishing. It is not obvious that this is a socially useful trait, but it was gloriously unworldly, and it is not surprising that we became lifelong friends.

It was at this time that I first developed some of my most lasting convictions. Amongst these perhaps the most important is and was that equilibrium analysis can only be applicable in economic theory if one can show that there are reliable and speedy feedbacks which ensure that an economy does not stray far from equilibrium. This of course also entailed coming to grips with a possible number of equilibrium concepts. My inclination therefore was to start with a process rather than with equilibrium, although like many others I often succumbed to the relatively easy pickings offered by equilibrium analysis.

It became clear very early on that agents taking intertemporal decisions had to take a view of the future, in particular of future prices. This clearly would have to form part of an equilibrium definition. At this time Patinkin was developing his monetary theory and in an early version he omitted expected prices from his excess demand functions although he included the interest rate. I wrote a note pointing out the theoretical difficulties of this, and Patinkin handsomely acknowledged that a change in formulation was required. Much later I showed that in Patinkin's formulation there was nothing to ensure a positive equilibrium exchange value of money.[3] This demonstration left Patinkin unmoved although the problem which I had raised gave rise to quite a large literature which is not yet complete. For my part it started an ongoing interest in the problem

[3] Hahn, F.H. (1965), "On some problems of proving the existence of an equilibrium in a monetary economy" in F.H. Hahn and F.P.R. Brechling eds., *The Theory of Interest Rates*, Macmillan, London, pp. 126-135.

of incorporating a monetary theory into general equilibrium analysis. On which I shall say more a little later.

The difficulty with showing a tendency to equilibrium was and is, of course, that we have no agreed theory of adjustment. In my first attempt at this enterprise I considered the simplest case of a sequence of short period equilibria. Price here always clears markets, but correct expectations are not stipulated. I investigated a simple question: would the presence of uncertainty improve or worsen the prospects of convergence to long run equilibrium? I showed that there would be an improvement. But of course I took over the naive expectational assumptions of cobweb theory. The question, now that we know more, is still a good one. For instance, we have arguments to suggest that uncertainty leads to some inertia. This may or may not be stabilising, and it may or may not affect the lag structure of responses to mistakes or new information. It certainly is a matter worth pursuing.

Much later I was caught up in the investigation of tâtonnement stability. To start with we were all concerned with stability in the small and so with local linear expansions. I managed to prove a number of theorems. Arrow and Hurwics then introduced economists to Liapounov functions and global questions could be tackled. But it was clear that we were playing with toys. No one had made a connection of a tâtonnement with an actual process of price change. There were two difficulties: no one was allowed to trade at "false" prices and no actual agent could change prices (because perfect competition was postulated). Negishi and I[4] managed to overcome the first objection in a pure exchange economy. To allow trade we postulated that any rationing was on the short side of the market. This postulate was later taken over by the Franco-Belgian builders of "fix-price" models.

Since we still needed the auctioneer and could not allow for production, we had not got very far. But I learned an interesting lesson. In a pure tâtonnement, to obtain stability we needed to restrict the form of the excess demand functions; when we allowed trade, that became unnecessary. In other words the simplification "no trade at false prices" turned out not to be a simplification at all. This taught me circumspection in praising simplicity in models. We know

[4] Hahn, F.H. and Negishi, T. (1962), "A theorem on non-tâtonnement stability", *Econometrica*, 30, pp. 463-469.

that scientists and mathematicians like simple and elegant theories. But they seem to know how to avoid falsification by simplification. The ancient Greeks had a simple but false theory of the elements, and quantum theory is not exactly simple. Indeed "simplicity" is a quite complicated notion and I confess to considerable impatience when silly models are defended by virtue of their simplicity, or indeed by the argument that without such simplification "nothing could be said". If at a certain stage of knowledge nothing can be said without drastic simplifying falsification, then perhaps we should keep quiet.

While I am on this methodological point I give another example. Solow's growth model was one in which a single good could serve as capital or be consumed. He showed that every equilibrium path converged to the steady state. When I investigated the same kind of problem with many capital goods, the steady state turned out to be a saddle point, and there were then many divergent equilibrium paths over finite time. The gain from dropping Solow's simplification was that expectations were brought into the picture and played a central role. The response of "Chicago economists" was to simplify more than Solow had done: they now assumed that the economy followed an equilibrium path over infinite time because a representative agent maximised a Ramsey integral. This gave the transversality conditions and so ruled out non-converging paths. Expectations (in a stochastic sense) were assumed correct over the infinite future. We now know of many reasons why an economy will not follow an infinitely efficient path, so that even as "as if" this particular simplification is a non-starter. But it cannot be that I am alone in believing that these authors have "simplified" economics away so that very many – perhaps most – of the problems which have engaged our subject cannot even be considered with this simplification. It is amusing to find that these economists often forget what they have done. Having modelled an economy as following an optimum path, they then announce as a separate discovery that public policy cannot improve it!

Of course I am not blameless myself. A good deal of my work has accepted the simplification of perfect competition and the absence of increasing returns. I have already noted (following Arrow) that this stops formulation of a proper theory of price change. But it also commits us to theories without quantity signals. For instance an increase in demand has to be reflected in changes in prices before it can induce a supply response. Casual empiricism confirms this to be false. The directors of Fiat may wish to predict various prices but it is clear that the bottom line is "how much can we sell if we set this price". In my view the perfect competition simplification has had rather disastrous effects on macro-economics. This is particularly true when it comes to the labour market where setting the money wage is equivalent to setting the product wage.

While I could not resist the ease of perfect competition theorising, I think that I never took the results as applicable economics. Much of the work simply arose from the discussions amongst theorists. Nonetheless I do hold the view that if one firmly keeps in mind that one is a good many stages removed from actual economies, the perfect competition hypothesis has been fruitful in answering the purely intellectual question – can an economy in which all decisions are taken by self-regarding agents be orderly with no greater array of information signals than prices? In the preface to the book which I wrote with Arrow this view point is elaborated.

Nonetheless I have continued to feel an urge to go beyond this simplification. I followed the trail blazed by Negishi[5] in my work on non-Walrasian theory and conjectural equilibria. This is another of the small fraction of my work I continue to have some pride in. I shall not give details here but comment on the lessons which it has for me.

In traditional general equilibrium theory an economy is described by the preferences and endowments of agents and by the available technological knowhow. But we need also to know something about agents' expectations and beliefs, leave alone their information and the institutions in which they operate. This almost surely entails also some information on the economy's past. My work on conjectural equilibria constitutes only a partial improvement. But I could show that equilibria – that is consistent actions of rational agents – could be sustained by beliefs – conjectures – which need only be locally correct. What this means is that the almost Marxian determinism of general equilibrium theory which deduced inevitable outcomes (or set of outcomes), from the "objective" description of the economy was sidestepped. Recently I was delighted to find game theorists on the same track (Fudenberg and Levine,[6] Kalai and Lehrer[7]) in which they find "self-confirming" equilibria.

[5] Negishi, T. (1960), "Monopolistic competition and general equilibrium", *Review of Economic Studies*, 28, pp. 196-201.

[6] Fudenberg, D. and Levine, D.K. (1993), "Self-confirming equilibrium", *Econometrica*, 61, 3, pp. 523-547.

[7] Kalai, E. and Lehrer, E. (1991), "Private beliefs equilibrium", mimeo.

I have returned to this topic a number of times and I find my conviction strengthened that the market is not just a "veil over reality" but is part of it. Recall the first "Fundamental Theorem of Welfare Economics" to see what I mean by "veil". It is not just externalities and public goods which cause difficulties here, but the fact that the economic world is partly what it is because the perceptions and theories of agents are what they are. (For simple confirmation attend a bankers' discussion on inflation.)

One of the objections to conjectures being part of the theory is "that anything can be the case". That is somewhat of an exaggeration, but it is true that many more states are compatible with equilibrium than we are accustomed to allow. Far from finding this alarming, I welcome it, since I have always doubted that history follows an inevitable path. But there is a more important consideration. As game theorists have found, even rather orthodox, hyper-rational and "hyper-informed" agents can give rise to many equilibria. There are, in spite of numerous attempts, no decisive ways of choosing between them so that even this approach does not escape the "anything can happen". This alerts one to the obvious fact that the process of learning and adjusting – indeed history – will need to be brought into the story. But processes themselves will need to be invoked in the account of equilibrium. For instance, if a process never reveals a certain kind of information, then the equilibrium associated with it cannot have actions and states depending on such information.

All of this has convinced me that a history and adjustment process-free economic theory is not on. I am happy to see that others have reached the same conclusion.

I am also happy to find a pleasing consistency in my own thinking. The view which I have sketched was already in a youthful paper[8] (1952), reappeared in a better elaborated form in my Cambridge inaugural[9] (1974) and seems to have been in the background of many later papers. I know that there is a quip concerning consistency, but I find that it leaves me cold.

[8] Hahn, F.H. (1952), "Expectations and equilibrium", *Economic Journal*, 62, pp. 802-819.

[9] Hahn, F.H. (1974), *On the Notion of Equilibrium in Economics*, Inaugural Lecture, Cambridge University Press, Cambridge.

Money

I have already noted my interest in monetary theory – in particular the problem of embedding it in general equilibrium or economy-wide models. It was soon borne in on me that we had no theory of transactions and so no way to discuss the role of a medium of exchange. Of course it had always been understood that the device of fiat money facilitated exchange, but no formal account of exchange – certainly in the context of general equilibrium theory – existed. I took the opportunity of the Walras-Bowley Lecture[10] to study this matter (1971). I was only partly successful.

Many economists think of the exchange process in terms of search (*e.g.* David Gale). Since search is costly it seems clear that middlemen who provide information where to exchange what can make a profit and will arise. Indeed looking at any modern economy the importance of shops, wholesalers, brokers etc. can be seen with the naked eye. In my view then a modern theory of exchange must in the first instance be a theory of mediation. In my Walras-Bowley Lecture (which was overly mathematical and overly concerned with existence) I hit on what still strikes me as a useful distinction between goods: "Named" goods were in the hands of named agents. Mediators transform these into "anonymous" goods – that is, goods the utility of which is independent of the name of the owner. Mediators were endowed with a mediation technology. Unfortunately I could not take account of the obvious increasing returns to mediation.

Fiat money is now a device to make mediation more efficient and, indeed, possible. My interest has always been to study the circumstances in which fiat money could survive – as an asset. In this I differ from others – most recently Kiriataki and Wright[11] who essentially are producing a theory of the origin of fiat money. To do so they again have to resort to models of search. Sometimes it is part of these theories to characterise the good(s) which will serve as means of exchange. These theories are fine, but they do not engage my interest. Theoretical "as if" histories are not my cup of tea. Nonetheless there has been work (Madden,[12] Ostroy-Starr,[13] etc.) which has made precise the famous "coincidence of wants" problem and these are certainly worth having.

[10] Hahn, F.H. (1971), "Equilibrium with transactions costs", *Econometrica*, 39, pp. 417-439.

[11] Kiriataki, N. and Wright, R. (1993), "A search theoretic approach to monetary economic", *American Economic Review*, 83, 1, pp. 63-78.

All of this treats the problem pretty abstractly. As Grandmont has noted, as long as agents attach a non-zero probability to money having a positive exchange value in the future, money will have a positive exchange value. Once again it is expectations and beliefs which need to be invoked.

Taking these basic expectations for granted, one wants to be more precise about the demand function for money. Friedman maintained that it was a stable function. This may or may not be so, but it still remains to specify it. Keynes's three-fold division is still the best. In some of the best papers (Baumol) on the transaction demand, transaction costs figure prominently, as they should. But I think more attention should be given to ways of economising on transaction balances (*e.g.* delaying payment and possibly credit cards). But it is the liquidity demand for money which we now understand better than before. Hicks' suggestion, that it can be regarded as an option value (in the face of possible new information), has been followed up. But once again it is the "lock in" effect of transaction costs which is important.

Tobin of course wrote a classic paper on the speculative demand for money, and since that time the finance literature has increased our knowledge. But doubts are beginning to emerge over the classic treatment of uncertainty. It now seems unlikely that the von Neumann-Savage treatment is descriptively satisfactory. We shall have to await developments. But it is clear that some of the work on asset pricing and portfolio choice neither corresponds to what we observe nor is some of it theoretically acceptable. Thus the modelling of asset markets by means of an infinitely lived Ramsey agent with rational expectations seems perverse. This procedure traces the course of prices when no trade in assets takes place. There can of course be no difference of information between agents nor, for that matter, of risk aversion. It is a paradox that so many papers are written which neglect almost every aspect of a decentralised economy and describe the latter as if it were under the optimum control of an exceptionally

[12] Madden, P.J. (1975), "Efficient sequences of non-monetary exchange", *Review of Economic Studies*, 42, 581-596.

[13] Ostroy, J.M. and Starr, R.M. (1974), "Money and the decentralisation of exchange", *Econometrica*, 42, 1093-1113.

able and long lived (!) planner. At no stage have I been tempted to follow this line which became popular when Pontryargin provided economists with a "cooking recipe" for solving problems in the calculus of variations.

Of course the project of a monetary theory requires us to think of an economy in which there is trading at every date – a sequence economy as Radner called it. (Radner influenced me greatly. I regard his work as amongst the best in economic theory over the past fifty years.) The simplest model which is not confined to two periods is the overlapping generation model. Money here is an asset only when we model its exchange role explicitly (as Solow and I have done). When I first studied it,[14] I was delighted to find that there were rationally expected inflationary equilibrium paths with a constant stock of money. (I had, unknown to myself, been preceded by David Gale.) This was just the sort of "bootstrap" result which I had always believed to be lurking in even the simplest models. Soon, the indeterminacy of the equilibrium path came to be recognised (see also Geanakoplos and Polemarchakis[15] and the "bootstrap" element became more obvious still. Sunspot equilibria completed the story. Here was confirmation of my steadily held view, (Keynesian really) that "reality" included beliefs, expectations and agents' theories.

This is not just a matter of pure theory. Recognition of this feature has profound practical consequences. For instance, Mrs. Thatcher persuaded much of the British public of monetarist doctrine. The result was (and is) that increases in demand which might well have led simply to increased output led to self-confirming price rises. Once that leads to "Banker Power" we finish up with 3 million unemployed. The economic theories of economists and pseudo-economists have considerable real effects, so that it is not unimportant to understand what one is doing when one theorises. I am convinced that there is one thing one is not doing: discovering a unique cause and effect relation. One of the most valuable services an economist can render is to emphasise both the fragility of economic knowledge and the range of logically possible consequences of actions.

[14] Hahn, F.H. (1982), *Money and Inflation*, Basil Blackwell.

[15] Geanakoplos, J. D. and Polemarchakis, H. M. (1986), "Walrasian indeterminacy and Keynesian macroeconomics", *Review of Economic Studies*, 53, 755-779.

While I think that we have not yet managed to formulate a truly integrated and satisfactory monetary theory, I do think that in broad terms we understand both the role and possible pathologies of money. Inflation is one of these. Even though the pure theory of inflation locates its costs only in the economising of money balances (and so increased transaction costs), this theory is based on assumptions which cannot be accepted. There has of course been work on the costs of inflation. It does not suggest that the containment of inflation should be the overriding aim of policy. I have nothing new to say on this. But I would wish to emphasise that the aim of zero inflation is not one which can be supported by anyone even mildly informed on the working of the price mechanism.

Many of my remarks have, I believe, gained support from the recent literature on "incomplete financial markets". This is not the place to go into technicalities. But I note that this literature pays no attention to mediation, no attention to asymmetric information (which Radner took to be the prime cause of incompleteness), and so to moral hazard. It is also silent on how we are to deal with the motives of firms when, almost surely, shareholders will disagree as to the best action. It is also supposed that all uncertainty is exogenous (so that price uncertainty is due to "state of the world uncertainty"). Once again we are faced with unrobust simplification. The technical results produced *are* interesting as such, but seem of little economic relevance. This is especially true of the welfare-economics of missing markets. What has somewhat alarmed me is not the first insights by Cass, Geanakoplos and Mas-Colell but the large literature which has followed it which refines the technical insights but takes no notice of the large and difficult terrain which needs to be traversed before it all makes some economic sense. The perfect foresight assumption (all agents know prices as a function of the states of nature) seems particularly objectionable.

Summing up

I have only touched on those matters which seem most important. But the reader will have, I hope, perceived the main milestones of my odyssey. I can sum them up as follows: I believe that much of the theorising post war was eminently worthwhile and required. But I always took it to be in the nature of an overture. I am disappointed that I, and others, have found it so difficult to make a start on the opera. I am even more disappointed that so few realise that a start has yet to be made.

There have of course been impressive gains in knowledge. There is no doubt that game theory transformed our approach to many old problems. But the theory itself has probably got off on the wrong foot by continuing with assumptions designed to give answers which, on further consideration, turned out to be questions. Nonetheless it seems the one branch of our subject which is making genuine attempts to get to grips with some of the issues I have raised (learning, for instance), and one can be hopeful. But an economy-wide picture still seems in the far future.

My guess is that the age of theorems may be passing and that of simulation is approaching. Of course there will always be logical matters to sort out, and our present expertise will not be totally obsolete. But the task which we set ourselves after the last war, to deduce all that was required from a number of axioms, has almost been completed, and while not worthless has only made a small contribution to our understanding.