ECONOMICS AND THE THEORY OF GAMES IN SOCIAL ANTHROPOLOGY*

This essay owes its existence to a belief that an injection of awareness of the nature of the mathematical models used by academic economists may help anthropologists to make contributions not only to 'the study of society' in the traditional sense but to economic theory itself. We should remember that economic theory is not simply something that is written on blackboards for the entertainment of students; that same model of 'perfect competition on atomistic assumptions' that seemed so thoroughly bankrupt on the pages of Lionel Robbins' famous text-book (1932) has been transformed into the techniques of linear and concave programming which have been, are being, and, perhaps, will be applied to societies of living human beings. In 1965 Robert Solow remarked that the short-run macro-economic model (used to determine policy in developed economies) was now "pretty well in hand", with no more than fifty years more being needed to "fill in the empty boxes". We shall see later just how wrong this claim turns out to be, and examine also the field of "Development Economics", an enterprise about which anthropologists might be expected to have fewer illusions (see Griffin: 1969). But many anthropologists are no doubt sufficiently sensitive to the taunt of "unprogressive" to follow the lead of Edel (1969) and accept the kind of 'underlabourer' role for anthropology that the much avowed success of economic science seems to allot to other disciplines in its system of patronage. Edel argued that the role of the anthropologist is to put flesh on to the bones of the linear programme by specifying preference functions, in particular using his knowledge of the culture's values to help ensure the consistency of the plan's targets, and by making sure that the engineer's production function is compatible with variables whose structural determinants usually lie outside the economist's orbit of empirical research.

All this raises the much debated questions of what 'Economic Anthropology' might study, where 'the Economy' might be located in a social system, what precisely is the meaning of 'Development', and so on. I hope some answers to these questions emerge in the course of my argument. We may begin with Karl Polanyi's characterisation of modern economic theory as 'the theory of a system of interrelated markets in a monetary economy' (Polanyi 1966: my emphasis). This is precisely what orthodox economic theory is not. The kind of economic model we shall be examining here is that of General Economic Equilibrium. Such models utilise the framework of micro-economic analysis to build up a model of the economy which explicitly takes into account its diversity in terms of goods, tastes, wealth and income endowments to individual economic agents, technological possibilities and so on. Macro-economics can be regarded as a special case of general equilibrium theory where the economy consists of one producer, one consumer and 'the government'. Such models constitute the theory of optimal resource allocation, the theme that clearly constitutes the economic background to the work of Raymond

Firth (see Firth: 1939).

The theory sets out to prove that if all producers in the economy maximise profits as individuals, then the whole economy reaches an optimal position, subject to the preferences of consumers being connected and transitive. (This means that each individual must be able to rank any allocation of goods in order of preference, so that he can express a preference between any two goods and that his preferences are consistent.) An optimal position can be specified at one, or a series of resource allocations such that:

(a) producers obtain maximum revenue;

(b) the outlay necessary for a consumer to secure any allocation preferred to the selected allocation is not less than that needed to secure the selected allocation.

Given further mathematical assumptions (see Koopmans: 1957), a set of relative prices can be computed at which the agents in the economy will act in such a way that they reach the optimum allocation. This is the essence of programming the economy.

Two things need to be noted here. Firstly, whilst the theory can specify an efficient point, it cannot specify the best of all possible worlds. Secondly, in an economy with many consumers and producers, if even one of those producers or consumers fails to behave in the 'rational' manner demanded by the theory, there is no 'second-best' position to which the economy can be moved (see McFadden: 1969). The whole edifice collapses immediately. Since the constraints on the model are very severe, and could not possibly be satisfied in a real economy, one might conclude that planning was futile and the theory ridiculous. The practising programmer, whilst forced to accept the logic of this argument - which he himself helped to construct - can only defend himself by asserting that some kind of control of what's going on is better than none. There we can leave the theory of resource allocation.

Models of this type clearly make no direct reference to money. Efforts were made to introduce it explicitly, notably by Patinkin (1956). The result of these efforts was to produce yet another theory of a barter economy! To understand this situation we need to look at the classical equilibrium of the economy as expressed in the theories of Léon Walras (1954). Looking at the problem macro-economically, the economy cannot be in equilibrium unless aggregate demand is equal to aggregate supply. In general equilibrium terms this means that all the markets in the economy must be cleared simultaneously; the sellers must sell their goods, the buyers buy as much as they want. This is clearly a case of successful barter. In the optimal resource allocation model a benign planning authority kindly computes a set of prices which enable buyers and sellers to transmit messages to each other about their respective desires. In the Walrasian system a little mechanism called 'tâtonnement' - which literally means "groping" - was introduced to make this possible. Buyers and Sellers come along to the market, but instead of trading with each other directly, they submit tickets to an 'auctioneer' on which they write 'offer prices'. Unless these prices are the same, the auctioneer sends the transactors away to

reconsider their positions. The process continues until an equilibrium price vector is reached throughout the market, and then and only then are the participants allowed to trade. This process of tâtonnement can be seen as a variation on the theme of 'perfect information', and helps us to see that <u>non</u>- tâtonnement processes, where trading is allowed at "false" (i.e. non-equilibrium) prices are those that characterise a monetary economy.

First, it may be helpful to look at the very foundations of the orthodox approach to general equilibrium models with money.[1] A transactor in the traditional economic model is faced with a problem of constrained maximisation. In Patinkin's original formulation this was represented as the maximisation of the utility of a desired quantity of goods and a desired money holding expressed in real terms as purchasing power. The consumer's choices are constrained by the fact that the consumer could not end up, after trading, with a higher value of goods and money than that of his initial endowment, which it was assumed had "fallen like manna from heaven". The problem was that the way these equations were written it was possible to carry out two types of transactions, goods for goods, and money for goods. The result of this is that if some transactors do not wish to hold any money at all, let us say only one transactor wishes to hold money, then money ceases to be used in exchanges at all, and accrues to this single, money-hoarding transactor. The consequences of this possibility are radical, and explain why it has been so difficult to incorporate money into the traditional value theory of 'classical' economics. For, far from satisfying Polanyi's definition of it, modern economic theory has failed to take into account the most basic structural feature of the economies it purported to describe. For as Marx expressed it, every transaction in a pure money economy must be of the form:

Commodity → Money → Commodity (where ' ⋺ ' stands for "is exchanged for")

The existence of the cash nexus in every sphere of economic life, means that a monetary economy must be portrayed by a model which has at least three goods, only one of which, money, is directly exchangeable for both the others. The orthodoxy has rested throughout on the assumption that one should generalise from two-good models (see Clower: 1967), and has thus been unable to produce a monetary model that was distinguishable from the barter world of Crusoe and Friday.

The belated grasping of what should have been a first principle, has led two economists, Clower (1965) and Leijonhufvud (1968), into a critical re-evaluation of the work of Keynes; the orthodoxy, it must be remembered, had since Hicks' 1937 paper been steadily subsuming Keynes as a special case of the neoclassical model, "useful in practice but contributing nothing in theory". It was felt that Keynes' theory rested on very special assumptions about human behaviour, particularly 'sticky wages' and 'the liquidity trap', which were portrayed as frictions within the machine of perfect competition that resulted in periodic malfunction. His book 'A Treatise on Money' was largely ignored. Clower and Leijonhufvud used a general equilibrium reading of Keynes to reinterpret his work as an attempt to construct an economic model based on true monetary foundations, a basis which had been disguised by neoclassical

macro-exonomics and those who called themselves "Keynesians" alike.
In this reformulation, the existence of the Labour market, and the
fact, again unrecognised in Patinkin's equations, that workers were
paid in money, not goods, became the crucial determining factor in
the existence of unemployment disequilibrium states.   Keynes'
attack on 'Say's Law' came to be seen as in reality an attack on
'Walras' Law', the idea that the price system was capable of
clearing all markets in the economy simultaneously.   The familiar
Keynesian idea of 'lack of effective demand' was reinterpreted as
the information problem that results from dropping the assumption
of tâtonnement, the fact that in a monetary economy information
must be transmitted at actual ("false") trading prices.   The
demand for goods of the unemployed worker is "ineffective" because
he demands a money wage; he cannot manifest his demand in terms of
goods on the market, without the services of the Walrasian
auctioneer as intermediaty between worker and consumer goods industry.
It is precisely in the "price-taking" atomistic market that these
services cannot be available.   In the Keynesian (reinterpreted)
unemployment state the 'potential' purchasing power of the
unemployed worker is non-communicable through the monetary medium.
A situation results in which all markets are cleared except the
labour market, where the excess supply of labour (the unemployed) is
equal to the excess demand for money (wages).   It is important to
realise that Keynes' attack on the principle of 'perfect
information' (the dual decision hypothesis) can only be coherently
formulated in a theory of a monetary economy, whose basic principles,
though recognised by historians and sociologists, escaped the
attention of the mainstream of economic theory altogether.[2]
Secondly, we should understand that the Keynesian model is just as
much a model of 'rational' and 'maximising' behaviour as the
orthodox approach.   Where it differs from the latter is in shewing
the limitations on behaviour resulting from the information
situation of the monetary economic <u>system</u> - with decentralised
decision making.

It might seem that economics was now in the process of
undergoing a revolution which would at least make it useful for
dealing with modern economies.   But once these apparently curious
assumptions like tâtonnement, or the idea that workers might receive
their wages in milled steel, are dropped, the difficulties of
constructing a mathematical theory of the economic system multiply
considerably.[3]   A major programmatic statement of the limitations
on system-building in economics was provided by Von Neuman and
Morgenstern (1953).   Their classic work not only supplied the
foundation for the theory of games but sketched a perspective for
the past and future of economics which should be of great interest
to anthropologists.   Indeed in the work of Fredrik Barth (1966) we
have explicit recognition of this.   Since like most formal
theories, game theory has been used for flag waving rather than for
serious analysis, it is not surprising that the result of this
interaction should be a total distortion of the original arguments,
and a set of conclusions which seem derisory.

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What exactly is game theory?   Von Neuman and Morgenstern argued that economic theory had been too ambitious in trying to set up general systems of universal application and should face up to the difficulties of handling mathematically even the limited problems of which we had adequate empirical knowledge.   In particular they argued that mathematical economics in the marginalist tradition had largely been concerned with a 'pseudo-maximisation' problem, that of maximising two functions at once.   Their theory of games was offered as 'a modest contribution' to economic science.   In the light of their own programme, it is perhaps unfortunate that many of the popularisers of game theoretic notions have insisted on trying to make the edifice seem so vast and portentious that only disillusion and retreat have resulted from its application.

Game models can be classified as strictly competitive and non-strictly competitive.   Into the former box go zero-sum and constant sum games.   In games of this type one player gains at the expense of the other.   Zero-sum games are the limit case where "winner takes all".   Where the number of 'players' is restricted to two, zero-sum games have solutions, and provide the players with normative rules of how to play.   In the case of non-zero-sum games, or zero-sum games with more than two players, solutions tend to be neither general nor in many cases plausible.   This is a pity, for it is just here that the theory gets interesting since it deals with phenomena like collusion, side payments and open communication between players.   As work in the field proceeded, the limitations of mathematical analysis became only too clear, as it was discovered that even apparently simple n-person games sometimes neither had solutions nor shewed in advance that they lacked solutions.[4]

I should make it clear that game theory is limited by the information situation.   We do not need perfect information but we do need complete information.   The player of poker who discards some cards has made a move.   Another player knows that he has made a move, but does not know which cards have been discarded.   Bluffing in a game of poker characterises the game as one of imperfect information, unlike chess where all the moves made up to a certain stage in the game can be observed by the other player.   But in both types of game players must have full knowledge about all the payoff values of the game that can result from any given strategy available to them.   In other words they must be able to assign probabilities to the outcomes. Next we need to formalise the concept of a strategy.   Games can be written down in two ways, extensional form and normal form.   The latter is more economical.   We represent the game as if the players moved simultaneously rather than in sequence and can write the result down in matrix form.   The structure of the matrix usually tells us a player's optimal strategy.   Of course, the matrix must be able to take account of the fact that playing the game will quite probably alter the value of the payoffs and enlarge the number of strategies as the players proceed;   it must therefore be comprehensive, which means, in the case of 'real-world' games, that we have to go quite deeply into the environment of the game and the way the environment may be affected by play.   (In the real world, for example, a game may start with the players behaving in a strictly competitive way, but after a number of moves they may be in a position to collude, which further modifies the environment, and so on.)

The central theorem of game theory is called "min-max" (see Von Neuman and Morgenstern 1953:   sections 13-17).   The 'payoff' a

player gets depends on which strategy the other player adopts, and in a game with two possible strategies and two players, one of the players may gain £10 by playing his first strategy if the other player plays his first strategy, and may lose £20 by playing his first strategy if the other player plays his second strategy. "Min-Max" tells the players how to minimise their possible losses, and they select that strategy which ensures minimum loss whatever strategy the other player adopts. The values that represent minimum loss (maximum 'security level') for each player are termed the 'max-min' and 'min-max' values of the game. Clearly in the case of some games the max-min value may be the same as the min-max value and these games are called 'strictly determined'; they possess a 'saddle-point'. In games where max-min does not equal min-max, a saddle-point exists if players are allowed to play what are termed 'mixed strategies'.
If a game does not have a saddle-point neither player can guarantee minimising his losses and there ceases to be an optimal strategy. The mixed strategy solves this problem, although it is almost impossible to make it sound plausible heuristically by means of a qualitative argument. The reader should imagine that the players select their strategies by means of a random device. The key point to grasp is that mixed strategies follow with perfect logic from the initial axioms of the Von Neuman and Morgenstern theory (op. cit.: sections 9-10). In effect, the player does not choose a strategy, but plays all possible strategies and chooses only the probabilities with which he is going to play them, thus introducing, in a sense, an infinity of available strategies.[12] What one has to decide here is whether, on Von Neuman and Morgenstern's premises, any quantitative result might arise from such a theory. Certainly one side effect of the theory of games has been several suggestive theorems in learning theory, and quite a number of ideas about information processes. But game theory in the formal sense, whatever its metaphoric contributions to other disciplines, has now been fairly fully incorporated into the framework of orthodox theory; nor is this surprising if one remembers that the min-max theorem is the formal equivalent of a linear programming problem, which was indeed expressed in min-max form in Von Neuman's paper: "A model of General Equilibrium" (1945).

Game theory's most serious limitations are revealed precisely in those fields where it might become most interesting. One example is what, at first sight, looks like a simple two-person co-operative game. Co-operation enlarges the set of possible payoffs for both players; they can both be better off, which is the reason for the initial co-operation. The problem is, 'how do they split the spoils'? There is quite a literature on the solution to this game, simple though it is as a sociological phenomenon. There are two basic approaches. One is to examine the question of the strength of the two participants. Obviously, if the game takes place more than once, the threat of a refusal to co-operate next time round is a powerful one, even if one player has the power to enforce his decision. Alternatively, even if one is able to force his decision, and the other announces that 'he won't play any more', a compensation which is just big enough to make it worth his while, that is which enlarges his payoff beyond the limit of the non-co-operative game, may encourage him to co-operate once again. Obviously there are limits to what can happen that seem, in abstract, quite plausible, but we cannot determine the solution with certainty from a mathematical description of the game. The other approach is to specify a 'fair division' of the

spoils, so that with the introduction of an arbitrator a unique solution can be defined. Many of these approaches cannot be reconciled with the Von Neuman and Morgenstern axioms, and those that can seem open to the objection of implausibility or are based on excessively restrictive assumptions. In particular, some solutions of this type suggest that the best course for the players might be to deceive the arbitrator by disguising their true preferences. These kinds of suggestion often came out of game theoretic discussion, and are sometimes themselves susceptible to game theoretic analysis - one might term it "the theory of the optimal lie". They sometimes have a certain amount of real explanatory power; one example is provided by the question of the behaviour of decentralised plant managers in the Soviet economy, where the theory of bilateral monopoly was found to account quite comprehensively for certain biases in the input/output figures the enterprises were sending back to Gosplan.

But in the last analysis, game theory has proved of limited utility in economic, sociological or political analysis. True, it serves as a good metaphor for making work of theoretical triviality seem more portentious than it is. I am thinking particularly of "Stratagems and Spoils" here, but I will deal with that in the last section of the essay. It is also true to say that if one searches hard enough one will find phenomena that could be handled by formal game theory. But game theory scarcely ever provides any qualitatively new results, and on that record it must be judged, although it has done much to clarify and sophisticate some older results (see Luce and Raiffa: 1957).

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I hope that in the light of what I have said the debate on maximisation theories now seems a most curious sort of undertaking. Those who embraced the economics of Lionel Robbins, far from using economic theory as an explanatory device, seemed to be groping around for some of its basic assumptions, which are now summarised as 'convexity properties'. Without convexity properties the mathematical model collapses for mathematical reasons, but as a theory it would surely have been more barren than it is if that is all it had to say. As many critics have pointed out, the study of 'economising behaviour' disperses the economy into every aspect of social life, with results that are plainly ludicrous. But a 'formalist' position does not have to rest on so tenuous a basis. It is still, in principle, possible to bo beyond 'economic theory' as we now have it, and construct formal models of 'primitive economics'. But such a programme would encounter the same difficulties that make modern economics what it is. Certainly we can write down useful little pieces of symbolism for heuristic purposes as Steiner did in his "Notes on Comparative Economics" (1954), but I think it most unlikely that one could at the moment get better results than orthodox-style economic theory in terms of global models, and we have seen just how unsuccessful such models have been. Formalism, in the mathematical sense, must proceed piecemeal, if its results are not to be totally trivial, and in saying this I am only following in the tradition of Von Neuman and Morgenstern. Yet at the same time, I think that mathematical models

do need to be introduced into this field more than any other. In the last stages of functionalism, quantification and models that seek to make it rigorously possible, have become a dominant concern. The anthropologist is in a unique position to examine critically the central concepts of development economics, particularly the notions of "the subsistence economy' and 'economic surplus' that form the basis of the theoretical work in that field. But he will achieve nothing by applying his critique in an 'ad hoc' and unsystematic way. The Development economist has no qualms about locating 'the economy' in another culture, and he will ask the anthropologists precisely the kinds of questions to which Edel's paper seeks to provide answers. If the anthropologist does answer those questions, it is my opinion that he will be denying the validity of what is most useful in the tradition of theory that sprang out of "Argonauts of the Western Pacific" through Mauss rather than through Firth. In fact he will even by denying the validity of the fundamental proposition established by Polanyi (1957), Dalton (1961), and Godelier (1968), among others, that "the anthropological perspective forbids us to describe the economy without showing at the same time its relation with the other elements in the social system." (Godelier op. cit.). So simple a proposition, almost a paradigm statement of the functionalist perspective, implies that we need to do more, much more, than answer the economists' questions. We must ask ourselves exactly what we have discovered about the nature of 'primitive economies' and examine its implications. The result of such a review should be a denial of the economists' questions, and their substitution by more useful ones.

First of all, we know that primitive economic transactions do not correspond with the notion of 'barter' as envisaged in economic theory. In particular, we have been able to establish, following the classic statement of Mauss (1950), that money in its modern form is a means of annihilating social relationships. Mary Douglas' paper "Primitive Rationing" (1967) is a very useful general statement on the question of primitive currencies and pseudo-monies. Polanyi's work on the 'archaic' economy of Dahomey (1966) serves as the most dramatic example. Here we have a highly centralised society, utilising a system of 'economic planning', in the sense that economic decisions are made explicitly in an institutionalised manner. Yet in Dahomey the use of cowrie shell as a standard of value institutionally similar to a modern currency, but expressing a system of social positions, necessitated a radical financial policy. The stabilisation of the transformation rates ('price ratio') between commodities became a necessity in Dahomey because once all social positions had been given a quantitative expression in terms of cowrie - formally equivalent to the introduction of money - the status (and therefore administrative) system could only avoid disruption by a kind of financial management that would seem to militate totally against the western theory of resource allocation. Dahomey is simply an extreme case of the phenomenon observed in those societies featuring 'spheres of exchange', for example the Tiv (Bohannan and Bohannan:1970) and the Fur (Barth:1966), where the native economy fights a rear-guard action against threats to its status system from the introduction of European currency. Douglas describes the presence of three rates of exchange for raffia cloth in the Lele economy in these terms. From this kind of data, a very interesting feature of the 'primitive economy' begins to emerge. Prices are administered by the institutional framework, through the creation of scarcity. Control on

the rates of transformation between commodities is thus exercised in
a way that corresponds to a kind of 'planning', but a planning whose
object is society and whose organising structures are plainly homo-
loguous with the organising structures of society in general.
What economic theory and economic anthropology both lack is a
completely adequate theory of price determination. It is well
known, as Marx pointed out (1938: chapter XIX), that whilst modern
economic theory of the kind I have outlined calls itself 'Theory of
Value', (see Debreu: 1959), it tends to be very good at explaining
price adjustment and fluctuation in the short run, but relies on the
determination of price by cost in the tradition common to Marx and
Marshall for its long-run model (see Godelier: 1968: II :S). In
the case of the economic formations of primitive societies, we
clearly need a more sophisticated approach than this. Steiner
pointed out that one could not begin to understand a primitive
economy without appreciating that there were transformations of
commodities that created value in excess of use-value or production
cost. The Potlatch is perhaps the supreme example. Here is a
transformation in which maximum value was created by the
annihilation of the use-value of an object, by its physical
destruction. Steiner's formulation also incorporates a phenomenon
of conspicuous consumption, the increase of value by arranging
objects in a ritual way, so that the sum of the use-values of the
commodities taken individually was exceeded by the prestige-value of
the ordered aggregate. Now whilst one could deal with this
situation in an orthodox formal model by including the commodity
aggregate as a new commodity, to do that would be to destroy rather
than enhance our understanding of the phenomenon. One could not,
in any case, include the destroyed copper as a new commodity since it
has left the system of circulating prestations completely. It is
possible, no doubt, to construct a formal model of the Potlatch in
terms of a strictly competitive game (similar to oligopoly), but
here again the result would be misleading. To capture the full
structure within which the contestants make their moves, one would
have to take into account not only a complex pattern of threat, bluff
and risk-taking but also the overall framework of credit, access to
the system, and the effects of particular moves on the flow of
resources within the system as a whole. Here we are likely to be
near, or perhaps beyond, the limits of our present mathematical
competence.[5] Secondly, behind the potlatch lies the more general
question of the basic structure of primitive economic formations, in
particular the question of distribution.

One of the most striking features of 'primitive economic
organisation' is the way in which competition for status is often
kept sharply separate from the question of the organisation of society
at the 'subsistence' level. Restrictions on the convertibility of
goods between spheres, restrictions on the alienation of property,
most notably land and one's own person, the principle of
redistribution and the specification of rights of access to the means
of production, all these conditions control distribution within the
"substantive" economic infrastructure, whilst scarcity and
competition - one might borrow Lévi-Strauss' use of the term 'entropy'
here - are restricted to a secondary level of activity and
circulation. The impact of money on this kind of 'dual economy'[6]
must ultimately bring about the deconstruction of the entire social
framework.

This observation brings in not only the question of the impact of the colonial economy, which has been intensively studied, but also the evolutionary and historical aspects of the theory of comparative economics. Polanyi suggested that the market system plus money owed its origins to the effects of the technological innovations of the 18th century, with the attendant increase in risk in capital accumulation, and the pressing need to ensure the maintenance of adequate supplies of raw materials. This theory seems to be defective both historically and as an explanation. The transformation of land and labour into pseudo-commodities had taken place over two hundred years earlier in England. What we really need to examine is the break-down of feudal relations themselves; we have to account for that radical transformation somehow, and I offer a tentative hypothesis. The essence of the prestige economy is monopoly of the means of obtaining status. If, in any 'dual economy' type of society a group does not have access to the coupons essential to obtain prestige goods, they may be able to break into the infrastructural economy by exploiting the scarcity of _imported_ goods; if they can establish a new set of transformations outside the prestige sphere, and secondly utilise that framework of transformations to create their own standards of prestige, thus introducing marketability into a social relation that had previously been subject to social control, number is clearly introduced into an economy which had previously been dependent on quality, and capital accumulation becomes possible. The importing merchant, the archetypal entrepreneur, cannot base his trade on the principle of reciprocity, since his own social position is undefined. The 'monetary revolution' may thus be seen to be an event of the same quality as the neolithic revolution, and it was against such dangers that the archaic economy of Dahomey stood firm. So simple an hypothesis is clearly historically inadequate, but the 'evolutionary' perspective may serve to illustrate the apparent resilience of the primitive economy to the exploitation of 'potential surpluses'. It also tends to suggest that 'money' needs to be rather carefully defined, since its 'unit of account' function seems to precede its 'exchange function' in time - contrary to the economists' emphasis - and it can fill that function without becoming the universal standard and liquid unit that constitutes a modern currency.

The lesson for the development economist is clear. Rather than complain of 'inelastic prices' of the kind Mary Douglas discusses, he would do well to ponder on his assumptions and the effects of his actions. We are faced with the basic category problem that Marx discussed in his brilliant "Introduction to the Critique of Political Economy" (1968), of rebuilding our conceptual apparatus from the ground up. What is needed seems to be something approaching a theory of 'Social Development' rather than a 'Development Economics', and the recognition of this fact should lead to a reconsideration of the notions of "subsistence" and "surplus" that lie at the bottom of modern development economics. Social optimality as defined and possibly in a sense achieved in a primitive society is clearly not necessarily related to efficiency of production in the substantive economy.[7] This leads us into the thorny thicket of the relationship between 'development' and 'modernisation'; fortunately at least some of the unfortunate recipients of 'development' are able to work out their own solutions to this question without the intervention of what Thomas Balogh once termed the "goodie-goodies".

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Finally, I want to examine the work of Fredrik Barth and his disciple, Professor Bailey. Barth (1966) attempts to set up an epistemology and a new type of model for social anthropology. Attacking structural-functionalism for accepting form as a datum, he argues for the construction of 'generative models', from which he hopes to derive the form of social institutions from patterns of social interaction. He also makes the extraordinary claim that the operations of his models are 'logical', in the same sense that game theoretic models are logical, adopting explicitly what he takes as the Von Neuman and Morgenstern paradigm that: "The logical operations whereby forms are generated should mirror actual empirical processes which can be identified in the reality which is being analysed". (Barth 1966).[8] Quite how his work does correspond with this paradigm rather escapes me. Firstly, the operations in his 'models' are certainly not 'logical' (in the sense relevant to axiomatic systems), and indeed sometimes not even plausible. Secondly, despite pretensions to "methodological rigour" what Barth actually does with his 'models' - especially that of unilineal descent systems - seems more reminiscent of Gluckman and Fortes than Von Neuman and Morgenstern. Even if we separate Barth's programme from his performance, the difficulties of even approaching the analysis of a single social institution are immense, especially in the present state of our mathematical techniques. This is not to argue against trying, quite the contrary, but anthropologists should realise that the limitations of mathematical economics represent as much the limitations of mathematics as the ideological limitations of economists.

But Barth, of course, does not even try. The result of this is that he struggles vainly at the intuitive level and makes exactly those mistakes which the mathematical theories were designed to correct. His 1967 paper, "Economic Spheres in Darfur" fails on its own terms, since although he manages to formulate a linear programming problem - without seeming to be aware of the fact - he lacks the tools to carry his argument to a useful (and logical) conclusion. Far worse than this, since it leads to incorrect results, is his 'idée-fixe' that consistency in social values might be explained with reference to the collective "groping" of individuals in individual "transactions". The argument here (Barth : 1966) is further confused by his failure to distinguish sufficiently clearly 'value' in the sense of 'preference' and 'value' in the sense of 'exchange rate'. But the major mistake was his rejection of the "particular formalism" of the theory of games; either the theory of games or the theory of non-tâtonnement processes would have shewn him (rigorously) that his collective "groping" was more likely to lead to unstable 'values' in the sense of exchange rates and inconsistent patterns of revealed preference. In particular, when talking of social values, he argues that the process of transactions would eventually establish transitivity of social preference. This extreme assertion is clearly contrary to the 'possibility theorem' derived by Arrow (1966) as indeed are all attempts to derive a unique and consistent social ordering from individual preferences in a situation where the choice involves more than two alternatives (and this is a matter of formal logic). Barth's only escape from this dilemma would be to argue that there was complete unanimity, as he seems to be suggesting when he speaks of 'imitation'. But the generative role of transactions then disappears into the Kantian categorical.[9] In fact Barth's 'model', far from explaining the generation of consistency

and similarity, might logically work in precisely the opposite
direction. We can see here quite clearly that Barth's "methodological
rigour" is a sham, and it is the very absence of that quality from his
work that makes it so desperately inadequate to its own avowed
objective.

Professor Bailey is at least more honest. He confesses in
"Stratagems and Spoils" (1970) that he is unable to understand formal
game theory. Unfortunately, he then continues to spice his work with
allusions to zero-sum games, a concept which ought to be irrelevant to
his argument. In a sense it is highly relevant; for, like Barth, had
he been able to understand formal game theory, he would have been
better able to appreciate the limitations of that form of theoretical
construct, the model based on individualistic social interaction, as
an explanation of historical and sociological phenomena. Despite its
pretentious sub-title, Bailey's book turns out to be a series of very
poor metaphors, designed to dignify an otherwise trivial form of
intellectual parasitism. For it is by now becoming clear that it is
not merely the mathematics that limits us in this case, as Bailey
seems to imagine, but the whole conceptual apparatus of individualistic
models that is inadequate to the task in hand.[10] When we read that
"since social change is worked out through the actions of men and their
failure to act", it can thereby be reduced to a series of games which
will but rarely be capable of solution (and therefore seemingly low on
explanatory power), the hollow ring of 'trendyness' becomes unbearable.

We must conclude from the sad experience of these two writers
that Gluckmanesque "naivity" can only lead to abysmal failures and the
ridicule of other disciplines. In particular, making another
discipline's mistakes all over again seems a sorry achievement for a
life's work. Only a full and informed grasp of the successes and
failures of other disciplines will make it possible for anthropologists
to pursue their own data to the level of theoretical adequacy.
Economic anthropology has long been in the grip of a mythological view
of economic theory, a view from which it must be emancipated if it is
to make the fresh and distinctive contributions to science that this
essay has suggested lies ahead of it. But awareness must be strongly
tempered with criticism; for if the anthropologists' results end up
looking like those of orthodox economic theory, "we may be sure that
they are wrong."

John Gledhill

Notes

*   This essay is a revised version of a paper read at Mr. Ardener's
     Tuesday seminar in Queen Elizabeth House, Oxford, during the
     Hilary term 1971.

1.   This approach rests on Clower (1967), although, as I remark, he
     is only 'rediscovering' an observation of Marx. See also, Von
     Neuman and Morgenstern (1953) 2.2.1.

2.   Economists are particularly blind to the results of other
     disciplines. In attempting to 'explain' why money enters the
     general equilibrium model at all, they usually resort to pseudo-
     evolutionary speculation. Here is an example:

"To lend intuitive color to our story, suppose that all individuals in our barter world live on a wooded island (perhaps in company with the odd snake and tiger) and must seek out other individuals as and when they wish to engage in economic transactions. We need not conceive the society to be primitive in an anthropological sense; on the contrary, we may suppose that institutions for the protection of individual limbs, lives, property and the sanctity of exchange contracts are as highly developed as might be desired by the most ardent believer in laissez-faire..." (Clower: introduction to Penguin readings in Monetary Theory, 1969).

The ignorance among anthropologists of the nature of economic theory, and in particular, its extreme limitations, is, of course, equally serious.

3. Even the existence of the orthodox competitive equilibrium requires qualitative (i.e. topological) mathematical argument of some complexity. See Von Neuman (1945), and Koopmans (1957). Leijonhufvud's quite detailed book is non-mathematical.

4. See Shapley and Shubik (1969). The significant result of this paper was that the games that were needed for the theory of perfect competition did have determinate solutions. These results are all concerned with what are termed 'n-person inessential games', i.e. those games in which it does not pay a player to join a coalition. The basic theorem - 'the core of the economy' - states that when the number of economic agents reaches denumerable infinity, no one of them can affect the price at which a transaction is made. In the 'old-style' theory this vital number was expressed merely as 'many'. Shapley and Shubik's "balanced games" comprise the core. One might conclude that mathematical analysis in this field was therefore confined to the trivial, though the rigorous delimitation of triviality is clearly important.

5. Nevertheless, it is important to try to make some progress in this area. The Potlatch is just such an example of the possibilities of making a successful attack on limited and definable problems, with a view ultimately to achieving a more general understanding of the nature of primitive social formations in the global sense. When examining the Potlatch we should be conscious of the larger phenomenon of which it is an exemplification, in order to guide our questioning of the data in the most fruitful direction.

6. My use of this term has, of course, nothing to do with its use in Development economics.

7. In a real sense "planning" is much easier in the 'substantive' primitive economy, precisely because of the simplification of the information problem which I have tried to show characterises the economy in which transactions must be carried out through a true monetary medium. Primitive economies are not characterised at the infrastructural level by uncertainty as to the actions of economic agents, though when making decisions with respect to the ecological environment they (like us) are faced with the uncertainty of nature. The economic behaviour of native populations is puzzling to the planner precisely because it is more structured than he imagines, and structured in a way to which his preconceptions leave him blind.

8. Compare this statement closely with Von Neuman and Morgenstern (1953) Section 4.1.3.

9. Suppose that transactions constituted a learning process by which every participant discovered the values of others. 'Imitation' cannot explain why a certain value is selected as the norm. If the 'majority view' triumphs, then logically, there must be a minority whose values differ. Furthermore the isolated transactor could not know which was the 'right' value, without the intervention of a mechanism like 'tâtonnement'. See Arrow (1966) for an examination of the Kantian alternatives. We should also note that Lévi-Strauss' ('le Cru et le Cuit':1964) accepts Ricoer's characterisation of his work as 'a Kantianism without a transcendental object.'

10. This implies that mathematical argument in this field would have to take a different form if it is to be possible at all. The real danger of a book like Bailey's is that its effect is actually to suppress the results of the formal work, and restore a measure of credence to results that are rigourously untenable.

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