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SOCIAL LIMITS TO ECONOMIC THEORY

Jonathan David Mulberg
B.A., Dip.

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Department of Sociology
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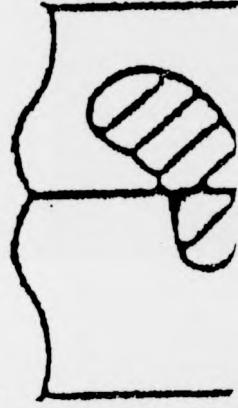


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Social Limits to Economic Theory

Jon Mulberg

Summary

THIS DISSERTATION effects a complete re-orientation of economic theory. It shows how the economic cannot be analysed separately from the political and the social, and lays the foundation for an integrated political economy.

The work examines the philosophical difficulties faced by economists, and re-draws the history of economic thought as a response to methodological dilemmas.

The traditional History of Economics textbooks re-write the history in terms of the contemporary paradigm. This dissertation, by contrast, shows how the philosophical debates have shaped the trajectory of economics, and how the orientations of the schools undergo major changes so as to attempt to deal with the fundamental dilemmas of a 'policy science'. It brings out the 'hidden history' of economics, and shows both how laissez-faire can only be defended by dropping any notion of economic 'science', and how economic theory has an implied political theory. It then considers the debate over political economic theory and the consequences for economic organisation and for environmental disruption.

CHAPTER 1

Introduction

... Economists set themselves too easy, too useless a task if in the tempestuous seasons they can only tell us that when the storm is long past the ocean is flat again. [J.M Keynes]

I. POSITIVE SCIENCE

TO BEGIN with the facts. For many years now, a great number of people have been dying through poverty. This question of poverty appears to be an economic phenomenon. Let us therefore look to the economic textbooks for the solution, or at least for some sort of explanation. On my shelf, I find Laidler [1982], whose book begins with the explanation that "Economics is about scarcity" which looks promising, especially when Laidler continues: "The word 'scarcity' is used here in a special sense: it refers to a state of affairs in which, given the wants of a society at any particular moment, the means available to satisfy them are not sufficient".

This is obviously the very state of affairs we are concerned with! We eagerly read on:

If all desires cannot be totally satisfied, then choices have to be made as to which of them are going to be satisfied, and to what extent. To say that economics is about scarcity then is also to say that it is about choice [op.cit p.1].

Surely there is some confusion here? We have before us the ultimate case of scarcity — starvation — and the economic scientist is talking about choices. Our confusion is soon compounded when later we read "The objects of the consumers choice are goods and services [ibid p.10]."

But the problem is precisely that there are no goods or services! Clearly this approach will not be of use, and indeed a reading of the next 300 pages would confirm this. Yet we would turn in vain to other economics texts for our answer, since they all adopt this 'choice' definition of economics (which as we shall see, was introduced by Robbins in 1932), and this is inadequate for the problem. This is a strange position for a leading social science to be in. It appears to be unable even to contemplate one of the main problem areas of the contemporary world.¹

The theme of our investigation in the pages below is that there is a central dilemma in economics. This dilemma is between the wish to employ the methods of positive science, and the contradictory desire to make contributions to policy questions. We will show that this predilection for a positive 'policy science' cannot be achieved, and we will show how the development of the discipline can be viewed as an attempt to respond to this dilemma.

Our point of departure is the argument that a positive policy science of economics cannot be sustained. In this introductory chapter we will consider the arguments in favour of such a positive science, and we will witness the tendency for the methodological positions to move away from positivism towards instrumental and evolutionary approaches. The two chapters following will show the inability to put a positive economics into operation.

An alternative approach is required, and chapters four and five will look at normative approaches to economic theory. The thrust of our argument will be that it is not possible to develop an economic theory of any substance without also including the political and social. Economic theory, we will maintain, must also contain social and political theory if it is to be of any consequence. We will reject the idea that there can be a positive science of economics that is somehow 'neutral', and will suggest that all economic theory — and the consequent policies derived from it — contain implicit political and social value-judgements. It is for this reason that a normative

¹ An explanation of why economics is unable to consider the question of poverty is contained in Mulberg (1987)

theory of political economy is needed — a theory that explicitly includes social norms and the political structure as well as the questions of production and exchange which are usually regarded as constitutive of economic science. To develop such a theory would be a tall order indeed, but in the last two chapters we will give an outline of some of the possible constituents of such a theory.

It may be well to begin by defining our terms. The term 'methodology' is often used as a grandiose synonym for 'technique'. This is not the sense in which we are using it here. We shall be employing it in its original meaning: the study of the principles and procedures of an organised discipline.² The practise of methodological enquiry is therefore connected with epistemology and ontology, and with the philosophy of science in general. We will not be overly concerned with these areas however, since our case is based upon application of knowledge rather than its acquisition. Nonetheless, a brief summary of the main positions of the philosophy of science will be useful, especially since we are investigating the claims of 'positive' economics. We will therefore give special attention to the philosophy of positivism.

We can begin this task by employing Keat and Urry's tripartite classification of the conceptions of natural science. This classification will come under strain later, but will serve our purposes for now. The authors outline three positions — positivism, realism and conventionalism [1975 p.4]. Positivism they classify as the belief that science consists of the attempt to obtain "predictive and explanatory knowledge" of the world as it exists. This is achieved by theorising the existence of regularities, which enable prediction and explanation. These theories are objectively tested by observations and experiments [ibid p.4].

Realists also believe in science as empirical and objective, but posit a difference between explanation and prediction, the former being primary. This involves going 'beyond' regularities to the causal mechanisms involved, which may well necessitate positing "unobservable entities" [ibid p.5].

² From the Greek: *methodos* 'method' and *logos* 'study of'.

Conventionalists reject science as empirical and objective [ibid]. As a result, the goals of science become a matter of debate, and conventionalists tend not to share common views on what scientific knowledge consists of.

We can see that a belief in objectivity in science is therefore insufficient to distinguish positive science. In addition, the positivist will put forward claims about the content of explanation and its relation to prediction, the process of observation and the role of theory. Fay cites four essential features of modern positivism: the limited account of explanation, the foundation of knowledge in the language of neutral observation, the ideal of value-free science, and the assertion of methodological unity [1975 p.13]. Our interest for the first part of this chapter will be on the first two of these features.

Although much of the work on the philosophy of science has focused upon this last notion of the methodological unity of the sciences, we will not be directly concerned with the problem of monism, but will instead be looking at the possibility of positive social science.

Before we do so, it might be as well to enquire as to the status of the philosophies of science themselves. Positivists suggest that science consists of, amongst other things, explanatory and predictive knowledge of the world. Is this suggestion itself explanatory and predictive? It would not appear to be possible to give a positivist account of what is actually a criterion for science — to attempt to do so is to fall into an infinite regress. Modern positivism has moved away from a theory of scientific meaning towards what Kolakowski calls a "normative attitude" to knowledge, towards the postulation of criteria for the demarcation of science from non-science. But if the positivist philosophy consists in fact of normative criteria, these criteria have to be justified normatively. Why have positive science at all? What does science achieve that — say — religion does not?

Science is often justified in terms of instrumentality — science is valuable as an instrument for problem-solving.³ Positive science is 'a good thing' because it gives

³ The term 'instrumental' is used in many different senses throughout the literature. We will try to distinguish these as necessary.

us knowledge which enables us to improve our lot. This is the point of the exercise. Fay believes that it would be the claim of many social scientists that

... just as the natural sciences have provided men with a certain kind of knowledge by which they can control their natural environment, thereby making it more hospitable and productive, so also the knowledge gained from social science will enable men to control their social environment, thereby making it more harmonious and congruent with the needs and wants of its members [Fay 1975 p.14].

Social science is therefore required in order to obtain instruments for social improvement. The positivists' claim is that two elements within positive science make it uniquely placed to provide these instruments. Firstly, they claim that the value-neutrality of positivism means that only positive science can provide *objective knowledge* of society, and that this is superior to subjective viewpoints. They also claim that the ability to *predict* gives us instrumental knowledge which would otherwise be lacking [Fay 1975 p.21].

The normative justification for the demarcation of science from non-science along positive lines is that it gives us control over our environment. It is able to do this because it gives us both the ability to predict the future and to understand the present in an objective manner.

We will have more to say on the emphasis on objectivity later in the chapter. Our interest here will be in the idea of 'knowledge'. We suggest that the positivist insistence on knowledge as being exclusively based on observation places severe restrictions on the ability to understand and consequently to predict. The justification for positivism in instrumental terms (in terms of instruments for control) tended to suggest that the science would in some manner enhance our predictive ability beyond that of mere chance; which would usually be taken to mean that science could offer us *causal* knowledge. We would justify a prediction by explaining the causes of the phenomena under investigation.

This requirement of causal explanation creates considerable friction within the positivist philosophy, given the insistence on a *limited* account of explanation and of observation being the foundation of knowledge. Although the idea of causality is now

accepted as implied by explanation [Hempel 1966 p.52-53], many of what we would usually regard as explanations involve non-observational entities — viruses, magnetism, electrical resistance, friction or even (from economics) satisfaction. That is to say, we require theories and theoretical terms to develop causal analysis. Of course, the introduction of theories marks a significant weakening of the positivist position, and it is far from clear how they should be treated. Mach believed that positive science should have no interest in theory — these may be a useful heuristic device for the practising scientist, but are of no consequence for the science itself [Caldwell 1982 p.23]. That is, we should restrict ourselves to the discovery of regularities. This appears to make non-trivial science next-to-impossible though, since we would have no method of deciding which empirical regularities to consider, and which experiments to perform. We also need to be able to interpret our observations; the regularities we observe will require theories to explain them [Hempel 1966 p.70].

The modern positivists usually accept the centrality of explanation: "To explain the phenomena of the physical world is one of the primary objectives of the natural sciences" [Hempel 1966 p.47]. In fact, one of the central claims of positivists is the symmetry between explanation and prediction [Keat and Urry 1975 p.11, Caldwell 1982 p.29]. This is because of the form of explanation now adopted, known as the 'covering law' model. Covering laws are conditional statements; given the antecedent condition, we can conclude the explanandum. The explanandum can be predicted given knowledge of the existence of the antecedent conditions, and the covering law can be used as an *a posteriori* explanation of the explanandum [Keat and Urry 1975 p.11]. The explanations can therefore be tested by their predictions. Hempel refers to this form of explanation as *deductive-nomological* [Benton 1977 p.55; Fay 1975 p.32]. Nonetheless, some form of theoretical analysis is still required to enable consideration of the covering laws, since we have to distinguish our causal explanations from accidental generalisations.⁴ To do this we would require in particular

⁴ e.g.

"all the coins in my pocket are shillings

This coin is in my pocket

Therefore it is a shilling" [Hempel 1966 p.56, cf. Benton 1977 p.10]

counterfactual conditionals [Hempel 1966 p.56].⁵ But counterfactual conditionals can seldom be observed without controlled experiment, which is not usually possible in social science.

Perhaps we can get round the problem of conditional statements by placing less emphasis on deductive explanation. The other mode of explanation frequently discussed Hempel calls *Inductive-Statistical*. We can perhaps explain an event inductively by reference to its frequency of past occurrences. We might explain, for example, the fact that inflation rose as unemployment fell by the fact that this has occurred in, say, 90% of all past cases.

It is important to note that this mode of explanation is not a *logical* probability, which is a question of the confirmation of an hypothesis, but is a *statistical* probability, a "quantitative relation between repeatable *kinds of events*: a certain kind of outcome ... and a certain kind of random process..." [p.63].

Given this definition, we may question whether a statistical 'explanation' is possible [Keat and Urry 1975 p.13]. The process is described as random. A poker hand which I deal to myself is not "explained" by its probability; if I deal four aces I would not normally be inclined to "explain" this event to the other players. I was lucky, that is all there is to it. In this sense, explanation is precluded.⁶ What can perhaps be explained is the pattern of repeatable events.⁷ Of course, as the analysis becomes exhaustive, the hypotheses become more like a covering law model: the overall probabilities tend to unity, and the two types of probability outlined by Hempel tend to merge.

The inductive model cannot, then, offer explanations of events. It can enable us to make predictions on the basis of regularities though. This would therefore appear to give two choices: to either break with the requirement of explanation or to adopt a

⁵ i.e.: If not A then not B

⁶ Fey disagrees with this analysis [1975 p.37].

⁷ Fey also argues that statistical explanations of mass events are as much explanatory as deductive models, and that this would maintain the symmetry of explanation and prediction. [1975 p.37b ff.]. Although we do not have space to discuss this question here, we would suggest that this form of 'explanation' is somewhat empty.

weak version of deductive explanation involving theory. Theories are usually regarded by positivists as containing two types of principles: their own internal tautologies and 'bridge principles' [sometimes called 'correspondence rules']. The bridge principles link the theoretical terms to observable entities. It is this that gives a theory its "explanatory power" and its testability [Hempel 1966 p.74]. This is a fairly significant departure from the concept of testability therefore: the theory as a whole is never tested. The only test is of the implications of the theory in terms already (antecedently) available. This makes testing problematic, since we must assume the correctness of the correspondence rules. The explanation as a whole is never tested against observation.

It would appear that the positivist philosophy as a whole cannot be sustained. We must either accept theoretical concepts which violate the principle of observation-based knowledge or abandon the congruency of explanation and prediction.

We have already mentioned the idea of inductive/statistical reasoning that does not offer explanation. In fact we can go either way — predictions that do not explain or explanations that do not predict. Of the latter, the most common form are those known as 'functional' explanations, which broadly consist of explaining processes in terms of the function played in achieving an end-state. We shall see how there is a tendency for social explanation to adopt this form. The main question in an examination of functional explanations is the domain of the theory. As *special* theories these forms of explanation may have some legitimacy, but as *general* theories, and as *social* theories, counterfactual conditionals cannot be supported, and functional explanations of this level tend towards bland assertions of the inevitability of the *status quo*. The whole idea of control, which was one of the reasons for our interest in positivism, is lost when one adopts evolutionary/functional analysis — indeed there is a strong tendency towards deterministic theories. We will come across this mode of reasoning later, in the works of Veblen and of the Austrians.

The alternative route is to view theories as simply instruments to obtain predic-

tions: we are no longer concerned with explanation at all. Theories could be completely false; as long as they generate accurate predictions they will continue to be employed. This methodological instrumentalism⁸ was mooted by Friedman — we will consider it in greater detail later in the chapter. We would note in passing that both these views do not fit in well with the tripartite classification that we mentioned earlier. It is unclear whether methodological instrumentalism is a form of positivism or conventionalism, whereas functionalism appears to straddle the positivist/realist boundary.⁹ As we shall see, this often makes these arguments difficult to perceive, while on the other hand those who present these methodological viewpoints may end up contradicting the philosophical tradition they purport to uphold.

We have seen how the observation that the stipulations of positive science cannot themselves be justified positivistically has opened up cracks that have spread throughout the entire structure. A positive philosophy of science has to be justified normatively in terms of control, yet such control requires causal explanations which contradict the tenets of positivism. We are either forced to admit theoretical concepts and move towards realism, or to move away from positivism either in the direction of conventionalist instrumental theories or towards determinist evolutionary theory.

We will now turn our attention to economic theory, and look at the effect of these problems upon economics. Chapter two will trace how the methodological problems outlined here have determined the trajectory of economic theory from the utilitarians to modern theory. The next section of the present chapter will look at the methodological debates as applied to economic science, or to use Fey's term, the

⁸ It is important to note that the term 'instrumentalism' has a different meaning here to that we used in our discussion of Pay's work. The term can have many different meanings — instruments have different functions. What is meant here is *methodological instrumentalism*, that theories are instruments in generating predictions. This should be distinguished from *policy instrumentalism*, which concerns the use of science to generate policies. The two views are, however, linked [Keat and Urry 1975 p.63].

⁹ Keat and Urry consider instrumentalism as a form of conventionalism, but they note that it has many similarities with extreme views of positivism, and it is easy to move from one to the other. Moreover, they lead to similar forms of activity [1975 p.64]. Hollis and Nell (in their discussion of Friedman) seem to place instrumentalism in a similar position, but they also maintain that pragmatism is a form of positivism [1975 p.4]: they seem to have little space for conventionalism at all. In this, they follow Kolakowski [1972 p.181]. Possibly the more empathetic analysis is Benton [1977 p.68], who suggests that there are different forms of the instrumentalist methodology.

economic "metatheory". The final section of this chapter will show how the metatheory adopted by economists introduces unwarranted limitations on the method and scope of the substantive economic analysis.

II. POSITIVE ECONOMICS

The problems of positive explanations in social science are compounded by the fact that the causes are often motivational in nature. A positivist social explanation would therefore often be teleological — it would explain action in terms of its final objective. This form of explanation was introduced by J.S. Mill in the guise of his means/end dichotomy, which corresponded with the distinction between fact and value. The scientist could consider the best means for a given end, but could make no judgements upon the ends themselves. At the same time, this method of analysis introduced the requisite teleological explanation [Myrdal 1933 p.206].

Myrdal points out a problem for economists adopting this line. If we do adopt the end/means formulation, "we arrive, not at the *laissez-faire* inherent in the philosophy of natural law, but at some kind of 'economic planning' ...". The consequence is a need for justification of *laissez-faire*:

This does not mean that non-intervention may not be looked upon as a more appropriate policy to promote a given end than any alternative. But it is never a foregone conclusion. It can be seen to be the case only after the consequences of all possible courses have been explored. Modern economic liberals attempt to prove the desirability of non-intervention in this manner. By acknowledging that this kind of proof is required, they accept the end-means scheme, and the element of planning inherent in it. [1933 p.207]

The later classics and the marginalist economists were therefore set two tasks to justify *laissez-faire*: to show that their conclusion is empirically-based, and also to show that no non-market knowledge of policy instruments is possible. To complete both these tasks simultaneously was to prove impossible.

Myrdal notes that this dichotomy of means and ends was required as an outgrowth of the philosophy of natural law. Under this philosophy the propelling cause was identified with the final cause¹⁰ — there was a direct connection between is and ought. What 'is' existed because it ought to. The end and means were both known *a priori*. The early classics would therefore have no need for the means/end dichotomy. This only occurred when propelling cause and final cause were distinguished [ibid.], which awaited the development of the utilitarian philosophy, and the distinction was explicit in Mill. He distinguishes between the art of economics — the production of effects, and the science of economics — the classification of causes [1836 p.151]. The utilitarianism of Mill was therefore not yet a full blown positivism, but contained some remnants of the natural law philosophy. Moreover, "It was then still believed" according to Myrdal

that ends can be established scientifically, the only difference being that proof of those ends was required; proof was no longer seen to lie in 'the nature of things'. As before, ends were thought to be a proper subject of research, requiring, however, special methods for their discovery [ibid p.208].

In spite of the fact/value dichotomy, the identification of hedonism with positivism came after Mill; Mill's method was a prioristic. He believed that the entire science of Political Economy "reasons from *assumed* premises — from premises which might be totally without foundation in fact, and which are not pretended to be universally in accordance with it". This has the consequence that the conclusions "are only true ... in the abstract" [ibid p.144 original emphasis]. We must allow, Mill explains, for possible deviations when considering actual cases [ibid p.150]. The procedure is one of deduction from abstract premises with assumptions that *ceteris* remain *paribus*. Indeed, this *a priori* mode of investigation is the only mode for the "moral sciences", of which Political Economy is one. *A posteriori* investigations are only useful as an aid to deduction in that they enable us to verify truth [ibid p.153]. It is

¹⁰ Soreen's 1958 translation uses "cause" for propelling cause and "teleology" for final cause (German *final*). This seems to be an error, although I am happy to be corrected on this matter.

also required to put the science into practice, since abstractions do not hold [p.155]. However, as Blaug puts it, "Mill cannot bring himself to equate a failure to verify a prediction with a refutation of the underlying theory ..." [Blaug 1980 p.65], the discrepancy may be because *ceteris* are not *paribus* [Mill 1836 p.154].

Utilitarian political economy is therefore saved from the possibility of having to test its actions by being imbued with a knowledge of ultimate causes. This line was continued by Cairnes [1875], who re-asserted the position taken by Mill. The economist begins with a knowledge of ultimate causes, but Cairnes insists that these premises are real [1875 p.87]. Political Economy is therefore deductive, and is hypothetical because it is conditional. Verification is aimed only at establishing the existence of antecedent conditions, not at the testing of theories.

The first edition of Cairnes was published before Jevons' *Theory of Political Economy*, and Cairnes only makes a few disparaging comments on the *Theory* in the preface to the second edition. Cairnes had very little time for the mathematics of Jevons, or at least for his view that economic knowledge would be advanced in this way. By the time of John Neville Keynes' essay on methodology [Keynes 1890], mathematics was established as an appropriate technique for the analysis of economics. Furthermore, the philosophy of utilitarianism was proving an embarrassment for reasons we shall see in the next chapter, and by the time of Marshall was virtually abandoned. Neville Keynes' *Scope and Method* was contemporary with the work of Marshall — they were colleagues at Cambridge — and can therefore be said to represent the end of the 'classical' methodology, and is a transition from the classical methodology to the new marginalism.

The inability to operationalise utilitarianism created a need for methodological adjustment to maintain the positivist philosophy of science. The changes that Keynes made were subtle but notable. Firstly, it was no longer possible to avoid empirical input. Induction was now to be used both in the testing of theories and in their derivation. Political Economy "must both begin with observation and end with observation" [1890 p.227]. The matter of *ceteris* not being *paribus* was simply another prob-

lem of this particular discipline [ibid p.233].

There were many other subtleties associated with the gradual abandonment of utilitarianism. Keynes outlines the Senior-Mill-Cairnes tradition of separating the positive science from the normative art of economy, but qualifies this with the notion that the normative art is social [ibid p.75]. The science of political economy was concerned with individuals within society, and since morals were no longer regarded as scientific, Political Economy was a social rather than a moral science [ibid p.88]. Economic human ~~remains~~^{remained} an abstraction, but one which was close to reality [p.124].

This last was fairly illustrative of the nature of Keynes' methodology. In line with the move away from utilitarianism, we no longer have knowledge of ultimate causes *à la* Mill or Cairnes, but an assertion that economic motives are predominant, but with — as Blaug notes — only "casual empiricism" offered as evidence [Blaug 1980 p.55]. The problem stems from the fact that ends are no longer regarded as known, but must be derived. For Mill the abstraction of economic behaviour from all other aspects of behaviour was an irrelevance — all were scientific. Since both ends and means are still known, the definition of economic science is arbitrary, and indeed Mill readily admits that his definition of economic human is arbitrary [Mill 1836 p.144]. For Keynes this is not the case. Since ends are not known, any abstraction may divorce economics from reality. This is why (following Marshall) he is hesitant about the application of the conclusions of Political Economy — *ceteris* may not be *paribus*.

The central problem was how to obtain a social ethic from a positive methodological individualism. This will inevitably break some of the positivist tenets. The utilitarian philosophy — which we consider in more detail in the next chapter — claimed that this could be accomplished because ethics could be measured scientifically. As it became obvious that this could not be achieved, the only method of maintaining positivist analysis and maintain a value-free science was to avoid considering ethics. We shall see in the next chapter that the social ethic cannot be obtained. In metatheoretical terms, the dilemma was between the positivist criteria of

observation and of limited explanation, and the normative justification in terms of environmental control, because the latter places a limit on the minimum scope of positive science: we must be able to derive policy instruments from our social science in order to justify the practise. The positivist dilemma which emerged with the fall of utilitarianism was precisely how to deal with the ethical behaviour which the utilitarians tried to make scientific.

As the positivist methodology came to be established in Britain and much of Europe and the U.S., the methodological problems associated with positivism remained stubbornly resistant to attack. The methodological debates which occurred could be viewed as squabbles over which defensive strategy to adopt to try to protect the positivist position. The first innovation was influenced by the Austrians, who actually never adopted positivism. Robbins's *Nature and Significance of Economic Science* [1935 (1932)] attempted to deal with the problem of abstraction by positing scientific behaviour as a *necessary condition* of economic action. Economic action is instrumental in nature, and consists of adopting means for a given end. However, there is a further requirement for sufficiency — there must be a multiplicity of ends and a scarcity of means. This scarcity will then involve behaviour in the form of choosing between means [1935 p14]. In keeping with the positivist prescription, Robbins suggests that *Economica* is neutral between ends [ibid p24].

The identification of economics with scientific teleology is drawn from Robbins's contact with the Austrian school [Caldwell 1982 pp.103 ff.]. However Robbins appears reluctant to keep the Austrians company along their entire methodological path [ibid].¹¹ He does not embrace praxeology (he claims never to have been able to understand Mises position [1981 p.xiv]) or embrace other a prioristic epistemological positions, but rather attempts to remain within the positivist framework — indeed he accepts Popperian falsification [ibid]. Since a prioristic knowledge of human behaviour is rejected, we must conceive of explanation in observational

¹¹ For a review of Austrian methodology see Hutchison (1981). The discrepancies between the Austrians and Robbins also carried over into the substantive theoretical concepts: see chapter 3 below.

terms. As Hicks notes, valuation is not itself observable [1935 p.87]. What is required is a method of transforming this theoretical term into observational terms which can either be observed or — for Hicks — are intuitively obvious. This is, of course, the essence of the positivist concept of correspondence rules

But the praxeology of the Austrians was a prioristic, not positivistic.¹² It is far from clear what the status of Robbins' rationality postulate is. Robbins himself believes that his sense of rationality is "simply one of a number of assumptions of a psychological nature which are introduced into economic analysis of various stages of approximation to reality" and that "Economic man" is simply an "expository device" which can be used to gain knowledge which can then be applied to real, more complex situations [1935 p.94].

This is not the case though. The assumption of rational humans is not simply a heuristic device that can be relaxed when applying the theoretical analysis to 'real' humans. "It is worth emphasising that the assumption of rationality is not simply another *ceteris paribus* condition. Even if all outside influences were eliminated and all observed values of variables adjusted to fit the theory, irrational behaviour ... would render prediction impossible" [Hollis and Nell p.53]. The original means/end dichotomy of Mill, it will be recalled, was to distinguish between science and art. Following this, Robbins's rationality 'assumption' was also used as a *definition* of economic science. Any behaviour outside of this assumption would be beyond the purview of economics also. "We may put this new qualification by saying that economics is the study of rational economic man" [ibid]. We cannot consider an abstract 'rational' economic human, and then compare with a real 'irrational' economic human, because economic action has been *defined* as rational action. Any irrational behaviour would therefore be regarded as non-economic, and ignored.

The utilitarians tried to deal with morality by reducing it to scientific measurement. What Robbins appears to do is simply ignore this moral element, thereby

¹² Note in particular Von Mises: "The starting point of praxeology is a self-evident truth, the cognition of action, that is, the cognition of the fact that there is such a thing as consciously aiming at ends" [1962 pp.5-6]

rendering what is left 'scientific', but also vacuous: it cannot be applied to or observed in any concrete situation. By identifying the subject-matter with an abstraction, Robbins has made the analysis analytic.

This would, of course, mean that Robbins would not be able to draw policy conclusions from his theories. Curiously enough he seems perfectly happy to admit this — possibly because he is unaware of the analytic nature of his theories. Although in 1935 he believed that "without economic analysis it is not possible rationally to choose between alternative systems of society" [1935 p.155 original emphasis], by 1981 he believed "that all recommendations of policy involve judgements of value" and that "in the application of Economic Science to problems of policy, I urge that we must acknowledge the introduction of assumptions of value essentially incapable of scientific proof" [1981 p. xxxi]. It was the vacuous nature of this "approximation to reality" which prompted Hutchison's critique. Hutchison does not believe that the game is being played according to the positivist methodological rules. The legitimate use of theory to help test definitions was rendered inoperative by tautologous definitions and unspecified *ceteris paribus* assumptions. Hutchison complained that the Robbins definition effectively excludes all facts. He claims that since Robbins excludes the choice of ends, and technical and social considerations from the subject-matter of economics, "All facts, that is, are excluded, for technical, social, and psychological facts presumably comprise that entire possible factual material for the social scientist" [1938 p54]. Furthermore, the 'hypothetical method' of economics resulted in analytic propositions [p.36]. Unspecified *ceteris paribus* assumptions achieved the same result, because they rendered the theory untestable [p.40 ff.]. Indeed, Hutchison believes that this sort of qualification should be reserved for inductive theories [p.46].

What Hutchison is after is — following Popper — economic propositions that are falsifiable. He claims that scientific propositions would 'forbid' the occurrence of some events. This economics does not do. Following from this, prediction was not possible.

There is finally the view to which the fullest and most penetrating discussion of Economic Prognosis led up: that all prognosis of economic events is impossible. It was clearly seen that the prognostic content of propositions of pure theory was nil, but owing to the misconceived notions of exactness and necessity ... all prognoses based on empirical regularities were objected to as in principle inexact and liable at any moment to be falsified. If all prognosis was really impossible, then ... Economics would be ... an idle science but a true one [p.64].

Economics, according to Hutchison, depends on the *discovery* of regularities. He regards it as an empirical science [p.164], but thought that current practise limited the science to tautologies. Furthermore, policy initiatives would necessarily go *beyond* economics: "No one, statesman or individual, can act on purely economic advice. *All state economic policy involves politics* ... only advice and policy which is based on an estimate of the political, sociological, and economic effects *together* of a policy or measure can be sensibly acted on" [p.165].

Of course, the entire problem with the discovery of regularities is the need for theoretical constructs for their understanding. While Hutchison is correct about the emptiness of Robbins' scheme, he offers nothing to put it in its place. Which regularities are non-trivial? On what basis do we judge the social and political effects of policy recommendations? The answers are not apparent, and Hutchison does not enlighten us.

Our argument in the first part of this chapter was that the sort of empirical-based science that Hutchison appears to be advocating could not be sustained. We also noted at the beginning of this section how the positivist formulation leads not to *laissez-faire* but rather to economic planning. This appears to be the consequence of the Hutchison methodology, and was the very antithesis of the orthodox programme. Clearly the charges laid by Hutchison must be refuted. While Knight's bellicose review of Hutchison's book reasserted the Robbins definition of economics along with Austrian praxeology (Knight 1940 p.154), the orthodox response was a move towards methodological instrumentalism. This line is taken by Friedman (1953) and Machlup [1955]. We shall break slightly with the historical order and begin with the latter work, since it directly responds to Hutchison.

We mentioned the possibility of sliding into instrumentalism earlier during our review of positivism. Instrumentalists abandon the idea of explanation, and are concerned only with prediction. Machlup believes in theories, but these have no particular significance — they are, to use Popper's term, conjectures: "No fixed lines can be drawn between theories, hypotheses and mere hunches ..." [Machlup 1955 p.3]. Since all conjectures have equivalent standing, Machlup believes that identifying one as a "fundamental" postulate is an arbitrary process [p.4]. This is especially the case since Machlup does not propose to test any of the hypotheses. Rather, we are to draw empirical conclusions from these hypotheses or hunches, and then to "confront these conclusions" with empirical data. That is, we are to use the principle of correspondence rules [ibid]. We are then to test whether an assumed change leads to the predicted change under certain conditions or not. If so, we can regard the test as a confirmation. However we only test the occurrence of the assumed change, and the correct conclusions. We need not test for the existence of the conditions: "Regarding them, a casual, perhaps even impressionistic, empiricism will do" [ibid]. That is, given a theory 'If A, then *ceteris paribus* B', we can test 'If A then B'. If this occurs, we assume *ceteris* were probably *paribus* [pp.13-14].

However Machlup appears to have gone a stage beyond positivism. He is no longer simply translating theoretical terms into observational terms, which is what bridge principles are about. He is missing out a part of the theory, which is whether the conditions under which the theoretical laws apply actually held during the test. The twist in the argument [to use what later became a cliché] is that these conditions are actually held to be irrelevant: indeed, they may even be knowingly unrealistic. "When a simpler hypothesis, though obviously unrealistic, gives consistently satisfactory results" Machlup contends "one need not bother with more complicated, more realistic hypotheses" (p169, emphasis added). That is, the real test of the theory is the generation of "satisfactory" results, whatever this means. Again, in the *Rejoinder* Machlup states:

We can, to repeat, test empirically whether the outcome of peoples actions is most of the time reasonably close to what one would expect if people always acted as they are unrealistically assumed to act. Again, the 'indirect verification' or justification of the postulate lies in the fact that it gives fairly good results in many applications of the theory [1956 p.488]

If these results are not forthcoming, presumably any *ad hoc* modifications to the "hunches" could be made. Machlup views theory as a "machine" which is "a construction of our mind", and that "Something goes in ... and something comes out" [p12]. The 'mental machine' as it were, can be altered to suit our purpose. Since the theory is only a machine to generate deductions, we would only stop using the machine when a replacement arrives. A theory will only be rejected when a better theory is available [p.11]. The position corresponds to that of methodological instrumentalism.

There is, however, one group of fundamental assumptions which according to Machlup cannot be verified [1956, p.437]. These are the "ideal types" of action and (citing Weber and Schutz) Machlup maintains these postulates of human action, of which the fundamental assumptions of economic theory are one, "are not subject to a requirement of independent empirical verification, but instead to a requirement of understandability in the sense in which man can understand the actions of fellow-men" [1955 p.17]¹³

This view Machlup opposes to what he terms "Ultra-empiricism", of which Hutchison is cited as a practitioner. An ultra-empiricist requires the direct testing of all assumptions [p.7]. Machlup believes he has shown this is unnecessary and undesirable — or even impossible.

Hutchison's most telling reply is that the only 'fundamental assumption' of any note is that of rationality [1956 p.479]. Beyond this the two protagonists appear to be talking past each other, with the debate over "ultra-empiricism" being largely semantic: for Hutchison this is possibly something like the logical positivists, for Machlup it represents Popperian positivism. We have already noted the particular

¹³ Machlup criticizes Friedman on this point [1955 p.17]

difficulties associated with the means/end definition of rationality. This automatically excludes 'non-scientific' behaviour from the analysis. Naturally, it follows that we can analyse this behaviour but our propositions are analytic.

The other response of Hutchison's we could note is the observation that successive generations of economists have attempted to reformulate value theory in testable form. This suggests an unwillingness of economists to accept *a priori* postulates [p.482].¹⁴ We will turn to this trajectory of value theory in the next chapter.

Machlup, then, adopts a sort of 'limited' methodological instrumentalism. Theories are simply mental machines to deduce change, and are really no better than hunches. We should proceed "as if" they were true [1956 p.487]. However there is also a fundamental postulate of rationality which is not subject to verification [1955 p.16].¹⁵ Friedman also believes in methodological qualifications, and uses "as if" formulations of theories, but appears to go further than Machlup. It will be recalled that Friedman's essay antedates Machlup's by two years. Friedman is still concerned to refute both the "empty economic boxes" charge of Clapham, and the charge of tautology by Hutchison and others. His response is to adopt an extreme version of methodological instrumentalism.

Friedman maintains that a theory contains two elements. The first element is that of language, and this Friedman accepts is indeed a set of tautologies. Its function is to serve as a "filing system" [1953 p.7]. Friedman also accepts that "economic theory must be more than a structure of tautologies if it is to be able to predict and not merely describe the consequences of action; if it is to be something different from disguised mathematics" [p.11]. The second element of theory is therefore as "a body of substantive hypothesis", the validity of which is judged by its predictive power [p.8]. For Friedman, this exhausts the possibilities. The only relevant empirical test

¹⁴ The response of Machlup was to suggest that ordinal utility was part of the Austrian heritage [1956 p.491]. We will see the inadequacy of this reply in our survey of value theory.

¹⁵ Although in the *rejoinder* Machlup seems to place this rationality postulate alongside all the other instrumental theories. If a suitable alternative model comes up "then the Assumption will have outlived its usefulness and will be sent to the limbo of 'disconfirmed propositions'" [1956 p.489]. This seems to take Machlup closer to Friedman.

is that of the implications of the theory. Hypotheses do not, Friedman suggests, have assumptions as such [p.14] — the so-called assumptions may indeed be widely inaccurate and yet form the basis of useful substantive hypotheses [p.20].¹⁶ The only relevant point is "whether the theory works", which for Friedman "means whether it yields sufficiently accurate predictions" [p.15]. This completes the task of economics, which is "to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances" [p.4].

This appears straightforward. Nonetheless, Friedman attempts to justify his position. The reason for the lack of relationship between the realism of 'assumptions' and the significance of the hypothesis, he explains, is that an hypothesis abstracts "the common and crucial elements" of phenomena, and is therefore "descriptively false" in its assumptions [p.14]. We therefore are to proceed "as if" the hypothesis were true [p.16 ff.].

Should the hypothesis fail to yield true, meaningful predictions, it should presumably be replaced. The construction of hypotheses is, for Friedman, exogenous. This is a creative act which cannot be formalised [p.43]. He might also add that the derivation of hypotheses is, as for Popper, an irrelevance. If it works it is of use, no matter where the hypotheses came from.

It may well have been Friedman's justification of the instrumentalist position which caused so much confusion in the secondary literature. Viewed in terms of orthodox positivism, the position is untenable. Musgrave points out that Friedman fails to distinguish between three different types of assumption. The main sense in which Friedman is using the term is what Musgrave calls "negligibility assumptions". That is to say, a theory ignores some aspects of a group of phenomena because these aspects have little effect on the conclusions. We may act *as if* they were not there [1981 p.373]. We cannot say from this that the assumptions are unreal-

¹⁶ This appears to be what Friedman is trying to say, especially in the light of the corresponding footnote, where he adds the rider that unrealistic assumptions do not guarantee a useful theory. His phrasing of the point is a bit clumsy though — he almost appears to regard unrealism as a virtue. This was later caricatured by Samuelson.

istic, though. We are not assuming that these aspects are not existent — which would be, as Friedman put it, "descriptively false". We are simply saying that their presence is irrelevant [p.379-80, cf. Melitz 1965 p.40].

This difference is significant because of the other forms of assumptions which can be made. We may also assume a *domain* for the applicability of a theory. The realism of domain assumptions is significant, because it specifies when the theory will "work", and when it will not [p.351]. We will see this clearer later on, when we discuss the difference between testing and prediction. We may particularly note that it is possible to delimit the domain in such a way as to make the theory untestable — which is the opposite effect to that posited by Friedman [Musgrave 1981 p.382, cf. Nagel 1963 p.215]¹⁷

Indeed, we can use a theory whose assumptions are known never to hold. Musgrave calls these "heuristic assumptions" and they are used in employing a method which Musgrave calls "a method of successive approximation" [Musgrave 1981 p.383]. Nagel seems to consider 'ideal types' in this set [1963 p.215].¹⁸ Only in this sense could the use of assumptions be regarded as 'descriptively false'. Yet Friedman has stated that the derivation of hypotheses — including presumably heuristic devices — is not a matter of concern for positive science. Both Nagel and Musgrave believe that Friedman commits an error by not distinguishing between these various senses of 'unrealistic' assumptions. It is not particularly legitimate to claim that the truth of assumptions is irrelevant because assumptions abstract from reality. Mason points out that an abstraction is different to a *false* statement [1980 p.242]. Friedman's justification for ignoring the assumptions of a theory cannot be sustained.

What is not clear is exactly why this justification was thought necessary in the first place. Nagel believes that

...the essay is marked by an ambiguity that perhaps reflects an unresolved tension in his views on the

¹⁷ This of course throws some light on the Hutchison/Machlup debate.

¹⁸ The three senses of unrealistic assumption outlined by Nagel seem to correspond to that of Musgrave [Nagel 1963 pp.214-15].

status of economic theory. Is he defending the legitimacy of unrealistic theoretical assumptions because he thinks theories are at best only useful instruments, valuable for predicting observable events but not to be viewed as genuine statements whose truth or falsity may be significantly investigated? But if this is the way he conceives theories (and much in his argument suggests that it is), the distinction between realistic and unrealistic theoretical assumptions is at best irrelevant, and no defense of theories lacking in realism is needed. Or is he undertaking that defense in order to show that unrealistic theories cannot only be invaluable tools for making predictions but that they may also be reasonably satisfactory explanations of various phenomena ... But if this is his aim (and parts of his discussion are compatible with the supposition that it is), a theory cannot be viewed ... as a "simple summary" of some vaguely delimited set of empirical generalisations with distinctly specified ranges of application [Nagel 1963 p.218, cf. McLachlan and Swales 1982 p.22].

If we forgo the possibility of offering explanation for economic phenomena, and focus solely on prediction, then Friedman's position can be salvaged [Coddington 1972 p.4]. This is the position of Boland [1979]. He suggests that Friedman's position is indeed one of instrumentalism. Furthermore, he claims that instrumentalism cannot be logically assailed, it "is its own defense and its only defense" [1979 p.522], a position accepted by Rotwein [1980 p.1553]. Rotwein does not accept that Friedman does indeed take an instrumentalist stance, however. He points out Friedman's concern with *science*, whereas we could classify the instrumentalist position as a sort of "engineering" approach. Hirsch and DeMarchi also believe that Boland's categorisation "certainly carries it too far. He ... makes it appear as if science is no concern whatsoever to Friedman. This is not true. It seems rather that Friedman passionately wanted to contribute to economic science, because he felt, as Dewey did, that to be useful, economics had to be scientific" [1984 p.783 fn.6].

It is clear that Friedman did not want to go completely down the instrumentalist path. This creates the tension and ambiguity that Nagel perceived. After all, he did title his essay 'The Methodology of *Positive Economics*', in a volume called 'Essays in *Positive Economics*'. This suggests an unwillingness to accept the conventionalist elements implied within instrumentalism: what Caldwell terms a "methodological instrumentalism". This position is ostensibly taken to avoid the problem of theoretical entities. However, it does not solve the problem of theory-laden observation. Friedman seems to regard empirical data as unproblematic, whereas we have seen that the opposite is the case [cf. Coddington 1972 p.9].

In addition the inductive problem is not avoided; a problem which the pragmatist philosophy of John Dewey did attack. Hoover notes an ambiguity in this respect. He cites Boland's argument that "For Friedman, an instrumentalist, hypotheses are chosen because they *are successful* in yielding true predictions" [Boland 1979 p.511, emphasis in Hoover 1984]. Indeed, Friedman himself states that "the only relevant test ... of a hypothesis is comparison of its predictions with experience" [1953 p.8]. This implies that a preconceived theory is confirmed only because it *has been* successful [Hoover 1984 p.790]. But Friedman also states that "the goal of a positive science" is to predict phenomena "not yet observed" [1953 p.7], that is, predictions that *will be* successful [Hoover *ibid*]. We cannot attempt both to predict the future and test the accuracy of the prediction at the same time [McLachlan and Swales 1982 p.21]. The Humean problem of induction was precisely the inability to extrapolate past regularities into the future. The past predictive success of a theory does not guarantee future success.

Yet this successful prediction of the future is clearly what Friedman had in mind, since normative questions and policy conclusions are dependent upon positive economics [1953 p.5]. One notes in this respect the difference in the use of theories between Friedman and Machlup. Machlup appears to adopt a 'two-stage' technique of *testing* followed by *application*. "Once we have confidence in the whole theoretical system" then, he states

we are willing to apply it to concrete cases even where only one of the two 'changes', in the 'cause' or the 'effect', is identifiable in practice, rather than both.

This is not the case while we are testing, however

For purposes of verification of the entire theory, however, we shall have to identify both the phenomena represented by the Assumed Change and the Deduced Change [1955 p.14].

Machlup here seems to have somewhat abandoned science in favour of an

"engineering" type approach. He is not, it would seem, concerned with universal conditionals 'and all that', but simply with finding tools that will probably work in the future. Boland and Frazer suggest that methodological instrumentalism is therefore applicable for "short-run or most practical problems" [1980 p.1556]. Yet it is unclear exactly which "practical problems" we are supposed to solve: we do not have much in the way of examples to follow. The snag with an engineering-type approach is that while it may be able to deal well with "negligibility" assumptions, it is particularly susceptible to domain problems. Recall that the Robbins 'rational economic man' was only an "expository device" — an heuristic assumption. We noted that Robbins actually denied the legitimacy of policy applications. Of course, it was Robbins' rationality postulate (which Hutchison calls "The Assumption") which Machlup in particular was at such pains to defend. To use an assumption in an inapplicable domain will, of course, lead to false predictions. And to use an ideal type — an assumption which has *no* applicable domain — as a negligibility assumption is an invitation to theorists to get up to all sorts of chicanery. The Friedman methodology leads to mythology [Mason 1980 p.242].

Indeed, it will be recalled that the main methodological dilemma for the orthodox economists was to derive a positive science — which would inevitably lead to the development of policy instruments — and at the same time show that these tools should not be used for planning. Machlup and (especially) Friedman's methodology appears to lead directly to this planning conclusion. The Robbins rationality assumption was designed to avoid the conclusion that planning is desirable, and to justify *laissez-faire*. In spite of their professed indifference to theories, Friedman and Machlup are actually closely tied to the rationality postulate and the models derived from it as a means of escape from the legitimacy of planning. This is what occurs in Friedman's essay, according to Mason:

The Chicago assumption of 'perfect competition' has the advantage of permitting rigorous — though unrealistic — analysis [Mason 1980 p.242]

and has the effect of

introducing ideological bias through the back door and avoiding the moral problems of unequal economic power [Jones 1977 p.358, cited Mason *ibid*].

The use of rationality therefore breaks the conditions of objectivity:

in short, the scope of positive economics is drawn so as to be a normative defense of neo-classical analysis, and the concept of a testable theory is confused with the untestable agglomeration of neoclassical doctrine [Mason 1980, p.242].

Actually if 'The Assumption' of rationality is dropped, one could be forgiven for wondering what is left. As we mentioned in section one, the point about causal knowledge is precisely that it helps us to increase the accuracy of our predictions beyond that of mere guesswork, and indeed in the same year that Friedman's essay was published, Schoeffler's influential book *The Failures of Economics* pointed out that, in fact, economists were not actually very good at predicting. Similar conclusions had been drawn by Mitchell earlier.¹⁹

Certainly all was not well, yet it is unclear which way orthodox economics could now turn. The use of correspondence rules to transform theoretical terms appears to have serious failings, and the conventionalist consequences of instrumentalism were unacceptable, yet a purely methodological instrumentalism was not valid. Nor did a retreat into a pragmatic 'engineering' methodology help. Samuelson's answer was to turn to empiricism.

The debate began with Samuelson's critique of Friedman. Samuelson maintains an essentially positivist critique.²⁰ The net result is similar to Hutchison's position, that Friedman "is fundamentally wrong in thinking that no realism in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a

¹⁹ See later analysis

²⁰ Samuelson's methodological naivety is noted by Caldwell [1982]. Certainly he has not understood the instrumentalist methodology.

theory or hypothesis" [Samuelson 1963 p.233].

Machlup responded to Samuelson in much the same way as he responded to Hutchison, claiming that Samuelson "rejects *all theory*" [Machlup 1964 p.733 original emphasis]. For Machlup the point is "we never deduce a consequence from a theory alone. We always combine the postulated relationships (which constitute the theory) with an assumption of some change or event and then we deduce the consequence of the *conjunction* of the theoretical relationships and the assumed occurrence" [ibid p.733 original emphasis].

Samuelson does not wish to accept the 'rational choice' theory of Robbins, since his own 'revealed preference' approach did not rely on *a priori* axioms.²¹ However, rather than following Hutchison, Samuelson decides to adopt an extreme empiricist approach.

Scientists never 'explain' any behaviour, by theory or by any other hook. Every description that is superseded by a 'deeper explanation' turns out upon careful examination to have been replaced by still another description, albeit possibly a more useful description that covers and illuminates a wider area. [1964 p.73]

That is to say that "An explanation ... is a better kind of description, and not something that goes ultimately beyond description".

Caldwell views this position as an error [1982 p.194], and certainly an anachronistic empiricist position cannot be sustained. Theories cannot, as Samuelson claims, be reduced to "basic sentences".²² Observational terms are not neutral but are theory laden [Wong 1973].

In addition, the whole justification for positive social science was that it could deliver policy prescriptions. Clearly a science based on Samuelson's metatheory would not be able to do this. In fact, such a science of description would be pointless.

²¹ Although the differences between the theories turn out to be largely semantical.

²² This claim is made by Samuelson after a rather weak attempt at refuting Hempel's contrary position [Samuelson 1963 p.1167]. For a refutation of the empiricist position Samuelson identifies with, see Benton [1977 p.50] and citations therein: "No number of observation statements can ever be equivalent to the statement 'The sparrow is cleaning itself.'" [ibid]

The philosophy of economics has therefore run aground on the same rocks as the positivist philosophy it tried to take on board. The idea of a methodological unity of the sciences runs into the immediate problem that much social science is motivational, and will therefore require teleological explanation. This teleological explanation could not be squared with the requirement of observation and the need for causal explanation to justify the positivist position, leading either to analytic theory which cannot be observed, or to a sort of 'methodological' instrumentalism which is unsustainable — particularly if laissez-faire is to be maintained.

III. POSITIVE SOCIAL THEORY

In fact the need for a justification of the positivist metatheory is something of an achilles heel for the philosophy, for it leads to a weakness in the concept of value-neutrality, which was the cornerstone of the entire programme. Towards the end of the previous section we looked at methodological instrumentalism. This position could be arrived at through an extension of the Popperian critique. Karl Popper's philosophy, based on the refutation of theories, was born out of a dissatisfaction with the philosophy of logical positivism, in particular with the responses to the Humean problem of induction. Popper believes that theories do not obtain meaning 'in themselves', but obtain meaning *a posteriori* through the failure to refute them. This means that the way theories are constructed is immaterial — they could indeed, be conjectures. Popper believed the method of construction of a theory "is irrelevant to the logical analysis of scientific method." [1934 p.31] Unlike Friedman however, Popper did not regard theories as an irrelevance, but thought them a vital part of scientific explanation.²³ It was only the method of construction he was unconcerned with. Popper was explicitly critical of instrumentalism, as he believed that "ordinary

²³ Although Boland and Frazer do argue for an 'alignment' of Friedman's methodology with Popper's philosophy (Boland and Frazer 1983).

language is full of theories" and that "observation is always *observation in the light of theories*" [1959 p.59 n.1 original emphasis].

In spite of Friedman's invocation of falsification [p.23] the positions of Friedman and Popper are different. Popper does not maintain that theories are irrelevant, but only that the method by which the scientist arrives at the theory is irrelevant. Theories themselves are of great importance to science. The reason for Popper's emphasis on theory is fundamental, and rests on the normative attitude to knowledge we mentioned at the start of the chapter. Popper believed that the positivist philosophy should not be regarded as a criterion of *meaning* but as a criterion of *demarcation* between science and non-science. As we noted earlier, the positivist prescription cannot itself be positive.

Positivists usually interpret the problem of demarcation in a *naturalistic* way; they interpret it as if it were a problem of natural science. Instead of taking it as their task to propose a suitable convention, they believe they have to discover a difference, existing in the nature of things, as it were, between empirical science on the one hand and metaphysics on the other [op.cit p.35 original emphasis].

Popper's metatheory was explicitly conventionalist:

My criterion of demarcation will accordingly have to be regarded as a *proposal for an agreement or convention*. As to the suitability of any such convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose must, of course, be ultimately a matter of decision, going beyond rational argument [op.cit. p.37 original emphasis].

This conventionalist aspect of Popper's position is often overlooked, but the conventionalist attitude to metatheory cannot be avoided by the philosophers of economics either. As McLachlan and Swales point out, all the methodological prescriptions of the economists are themselves normative. It is therefore apparent that we require a normative discussion on metatheory before any science can take place. Yet, once we allow this, it is no longer clear why this normative discussion should not form a part of the substantive theories

Suppose that we were to accept that normative statements cannot be rationally discussed. How should we consider a claim such as that positive economics should be judged by its predictive success or that economists should make positive statements, or that if, as economists, we do not try to test our theories empirically, 'we should cease right now giving policy advice' [Lipsey 1975 p. xx1]? We would have to say that these claims cannot be rationally discussed and are rationally no better or no worse than the claims that positive economics should *not* be judged by its predictive success, and that economists should *not* make positive statements, and that whether or not we try to test our theories, we should *not* cease giving policy advice. To be consistent, we would have to say that any pronouncements we made about the methodology of economics were either descriptions of how economists do or might actually proceed or else not intended to be considered as rational claims at all but simply as expressions of personal whim. If this is the case, much methodological discussion seems to us to be pointless, and any, even implicit, methodological prescription not prefaced by a clear statement that such prescriptions cannot be rationally defended is misleading. [McLachlan and Swales 1978 p. 15-16]

Yet it would seem strange to suggest that no dialogue on methodology is possible

— We can see no reason *a priori* for discounting the possibility that normative statements can be rationally discussed in the sense that reasons for and against their acceptance can be offered. [op.cit. p 16]

But this suggests that we should attempt to incorporate analysis of the normative into our theory. "If normative statements can be rationally discussed, the failure to develop a normative economics is regrettable" [op.cit. pp. 17-18].

Our claim here is that a normative metatheoretical position must contain an implicit normative *theoretical* position. If, in the case of positivism, we are to consider a convention for the demarcation of science, the convention adopted will be justified according to a normative evaluation of the 'science' it would yield. We would adopt the positivist criteria for science only if we value the contents of the science which would result. That is to say, the discussion is no longer what is 'correct' or 'incorrect' science, but what is *useful* science.

The problem for a positivist formulation is, of course, that it claims to be value-free. We have already seen how the normative justification of control of the social environment created insuperable problems for the observational and limited explanation aspects of positivism. Our criticism here is that in addition the need for a normative metatheory must result in a positive science contradicting its own criterion of value-neutrality, and that "with regard to social science, there is ... an implied politi-

cal theory as an element in its account of what it means to understand social life'' [Fay 1975 p.15]

The political theory of positivism Fay calls the concept of 'policy science', which consists of a 'set of procedures which enable one to determine the technically best course of action to adopt in order to implement a decision or achieve a goal'. However, the policy scientist is more than an adviser.

Here the policy scientist doesn't merely *clarify* the possible outcomes of certain courses of action, he actually *chooses* the most efficient course of action in terms of the available scientific information. In this regard, the policy scientist really is a type of social engineer who makes instrumental decisions on the basis of the various laws of science — in this instance, social science — which are relevant to the problem at hand [op.cit. p.14 original emphasis].

This view of policy science obviously has considerable implications for political practise, for to the extent that policy can be analysed impersonally and objectively without subjective argument, political debate would be removed from these areas. Furthermore, Fay argues, participation in the political process would be limited to those with requisite technical competence, and also the accountability of the policy-scientist is internal to the peer group.

Fay also outlines the inevitable direction that politics would take. The employment of the policy scientists would create a movement towards centralised government concerned with the planning of the social and the political [op.cit. p.26-27].²⁴ The sum total of these changes is, for Fay, 'the sublimation of politics ... an attempt to eliminate politics as we know it' [op.cit. p.27]. It would be replaced by a 'social engineering view of politics, which is that there is a *correct* way of proceeding in human affairs and that it is the responsibility of the decision-maker to discover what this way is.' [op.cit. p.28]

This result, Fay maintains, is caused by positive social science because of the combined effect of the presumed value-neutrality and the connection between the

²⁴ For an explanation of how this squares with arguments for *laissez-faire* see the analysis below.

concept of explanation and prediction. It will be recalled that the positivist conception of explanation was in terms of regularities, and that this was regarded as the same as a possible prediction. That is, understanding an event consists of knowing what produces or can prevent it, but this is exactly what is required for control. Positive science "constitutes, [the] world from the viewpoint of how one can gain control over it" [op.cit p.40]. Nothing else apart from control is to be regarded as scientific. It is not so much that scientific knowledge enables social manipulation, but that only this is regarded as scientific. "Technical control is a defining element in the scientific enterprise itself" [op.cit p.41]. It is not a question of *how* science is to be applied. Given the form that knowledge takes, there is no other way it can be usefully applied [op.cit. p.39].

The orthodox economics scheme is similar in its view of the exclusion of the political, but has in addition to justify *laissez-faire*. It was this additional burden that lead to the analytic analysis of rational 'economic human'. We shall return to this shortly.

The political theory implied by the positivist analysis could be described as an *end state* conception, the only concern is the efficacy of the means at obtaining the independently derived ends. Under this scheme, we make no other judgement as to *which* means should be used. Myrdal maintains this is an error. He believes that "In any human valuation" means have independent values as well as instrumental values [1935 p.49] and that "means are not ethically neutral". The alternative courses are compared through value judgements. But this causes the collapse of the whole construction — we now have to hypothesise each stage in the argument, not merely the final stage [1933 p.211]. The means/end construction was supposed to allow the objective derivation of policy.

But as soon as we admit that value can be attached to means independently, every single link in the chain of the argument is opened. It follows, first, that the problem can no longer be solved unequivocally even if the end is given.

In addition, we no longer have any guiding principle for eliminating any possible alternative from consideration. We cannot limit our field of enquiry [op.cit p.212]. Generalising becomes complex and difficult [1953 p.50], because "the value premise which has to be introduced in order to allow policy conclusions from factual analysis has therefore to be a valuation of means as well as ends." [1955 p.49]

The 'engineering' approach of matching means to ends must therefore be rejected, because the means must themselves be evaluated. That is to say in place of the Newtonian conception of policy science, we should consider policy in Einsteinian relativist terms as it were. Ends and means are not fixed, but are relative. What we regard as a means is also an end which is to be achieved by a previous means [Fay 1975 p.51]. Similarly the end we are striving for can itself be a means to a further end. This implies that "any given course of action may be either a means or an end depending upon the point of view which one adopts" [Fay *ibid*].

So the various means we have at our disposal will themselves have been arrived at as normative ends. "And, upon reflection" Fay believes

this seems an obvious point. For all political decisions, even those which are seen as means to an end, are social policies, and as such they embody a notion of what people ought to be required or permitted to do to others. No social policy's worth can be solely instrumental because any such policy will require that people interact with one another in certain definite ways, and for this reason it must have a moral value *in itself*'

Fay believes this can be easily shown through caricature of purely technical policy recommendations.

What if it were concluded that the most efficient (i.e. the cheapest fastest, most simple etc.) way to deal with the population problem were simply to exterminate large numbers of some special class of people? The point is that such a proposal could be objected to not simply on the grounds of technical efficiency, but also on the basis on an antipathy to the moral values inherent in it" [op.cit p.52].

Fay concludes that all political proposals — even those of which means to adopt for a given end — are moral in nature [*ibid*].

Because we are unable to separate out objective means from subjective ends, we are unable to abstract 'economic human' from 'social human'. Since we cannot limit our field of enquiry, in this respect the act of abstraction is in point of fact, normative [Streeten 1958 p. xx]. Our choice of question determines the answers (p. xxi). Myrdal outlines one method of abstraction which is sometimes proposed by economists. While admitting the subjectivity of valuations, the problem is split in two. One part is political, and is taken as exogeneous, the other is economic, and is endogeneous and value free. "The division usually runs along the old classical line drawn between income distribution and production. They say, for example, that they disregard the effects on income distribution, or that they presume a just distribution, etc." He adds that "such a separation of a political problem into a political and a purely economic section is, however, impossible" [1933 p.218].

In particular, we shall see that the distributional criteria cannot simply be 'imported' into the theory as an exogeneous variable, because the price theories themselves have distributional implications. This means that at best the theory can only be static, since the distributional pattern can change with each time period. In addition, we will see that the development of a *value* theory as opposed to simply a *price* theory will necessarily involve dealing with distributional issues.

The neoclassical scheme was different from the classics though, in that it required a method of *deriving* social value. Its value theory could not be identified with a price theory: their congruence had to be shown. And because under positivism the abstraction of '*homo oeconomicus*' was non-arbitrary, the separation of distribution from value could not be achieved. These questions will be considered in chapter two, where we look at the attempt to put the positivist philosophy into operation: the chapter will focus on the orthodox *laissez-faire* economic theories. However, as we saw, the positivist philosophy actually tends towards a process of economic planning. Chapter three will outline the market socialist approach to positivist economic planning, and will show that the positivist approach to social science cannot be maintained.

If the positivist means/end scheme is abandoned, then both the conception of 'rational economic human' and the abstraction of economic from social and political action must also be abandoned. Chapter four will look at the American Institutionalists' critique of orthodox economics, and their attempts to include psychological and social elements in their economic theories.

To explain the contexts of chapter five it will be necessary to return briefly to our analysis of positivism. If the political theory implied by the positivist analysis is an end-state conception of politics, the alternative could be described as a *process* conception of politics. We should concern ourselves not solely with the end product of the decision-making process, but with the organisation of the process itself. Since we reject the idea of policy being decided by the policy-scientist, we must hold a conception of a participatory process, and given that we cannot separate out the various elements of human action, we must also consider a theory of political as well as economic human

... according to this theory 'politics' refers to men's deliberate efforts to order, direct, and control their collective affairs and activities, to establish ends for their society, and to implement and evaluate these ends. From this perspective, what is fundamental about politics is the interaction and participation of men according to mutually defined and accepted rules as they engage in this process of creating and administering the laws of their community, which is to say that what is most significant is the involvement of the citizens in the process of determining their own collective identity [Fay 1975 p.54].

It is this process of determining a collective identity that will be the subject of Chapter Five.

Our task then is to show that a positivist conception of economic science cannot deliver on its promise of value-free control over the environment. We will begin this task by showing how the orthodox economists from the 19th century to the present day were unable to put the philosophy into practice

CHAPTER 2

From Utility to Welfare:

The Trajectory of Orthodox Economics

WE SAW in the previous chapter that the achilles heel of the positivist approach to social science was the need for a justification for the metatheory. The only cogent justification lay in the claim that positive science leads to an ability to understand and thereby control our social environment. This, though, necessarily involved a causal explanation of the social world, and explanations solely in terms of observable regularities were insufficient for this. Causal explanations invariably involved recourse to theories.

Positive economics faced extra difficulties, particularly in justifying *laissez-faire*. The idea of a positive economics would seem to lead to some notion of economic planning. *Laissez-faire* therefore has to be justified by showing both why social planning cannot succeed, and how individuals acting in ignorance of an unknown social optimum nevertheless can collectively obtain the best social outcome.

This left the positivists two tasks in order to justify *laissez-faire*. The first task is to explain activity in terms of individual actions. They then had to show how the aggregate of these actions not only arrived at but actually revealed the optimum social outcome. That is to say, the theory must be based on individuals, must be capable of aggregating the individual actions, and must show that only then can the best social outcome be found. What we will see in this chapter is that it is impossible for the positivists to keep all three skittles in the air at once.

In order to put the positivist philosophy into operation the economic theory would require three elements. It would require a psychology so that causal explanation can be derived in order to explain why actions occurred. To aggregate these actions a second element — the theory of price — is needed. But to show that this aggregate is optimal a third element — a value theory — is also required, and it is

this last that causes the tension in what is supposed to be a value-free positive science, because in order to support *laissez-faire* the values must be *subjective*, whereas positivism stresses *objectivity*.¹

This chapter will trace the trajectory of orthodox economics as it tries out different ways of dealing with the dilemma of attempting to obtain objective results from subjective analysis. First we will look at the utilitarian marginalists, who claimed to be able to reconcile value and price through their knowledge of 'ultimate causes'. We will see that the hedonist psychology which this involved cannot be sustained, and the philosophy cannot be put into operation. The utilitarian philosophy gave way to a more orthodox conception of positivism, and in economics the marginalist school was succeeded by the neo-classics. The neo-classics used an objective value theory, but could not then defend *laissez-faire*, at least not without incorporating unsubstantiated psychological assumptions. The later ordinalist theories attempted to dispense with psychology, but in so doing had also to dispense with value theory, which meant that they could not show how the best *social* outcome can be arrived at. But without this ability to arrive at policy conclusions the positive approach seems pointless, and we shall see that orthodox economics arrives at an essentially vacuous theory.

I. UTILITARIANISM

Bentham

One method of deriving an objective social science from a subjective theory is to moot a theoretical approach based on human psychology. This then makes it seem possible to draw policy recommendations from 'evidence' because the ultimate facts

¹ This is not to say that planning cannot be criticised, or that *laissez-faire* cannot be defended, but simply that this would require a non-positivist approach. We will consider one such approach in the next chapter.

of behavior are (claimed to be) known. This is the approach of Benthamist Utilitarianism, which Hollis and Nell refer to as "the triumph of positivism" [1975 p.48]. The avowed aim of the utilitarian philosophy of the eighteenth and nineteenth century was precisely to extend the principles of rational calculation to all aspects of human activity. Morality could be calculated in the same manner as the quantities of physical sciences. Utility was a guide both to what is *and* what ought to be. According to Bentham

Nature has placed mankind under the governance of two sovereign masters, *pain* and *pleasure*. It is for them alone to point out what we ought to do, as well as what we shall do. On the one hand the standard of right and wrong, on the other the chain of causes and effects are fastened to their throne [Bentham 1789 p.11].

There is therefore no distinction between ethical and technical considerations. Left alone, what we want to do is what we will do, and this is indeed what we ought to do. Ethics are therefore objective.

The consistency of the utilitarian moral philosophy will not be discussed here. Our interest is in the *operationalisation* of utilitarianism — the transition from social theory to social science. The ability to operationalise the philosophy is of vital importance to utilitarianism because of the claim for objective morality and measurement. Unless the philosophy can be put into operation, it will not be able to "point out what we ought to do". To show that morality is objective, it is important to show how it can be discovered. The technical cannot be separated from the ethical.

Following on from our analysis of positivism in the previous chapter, we can see that the key element in what we could term the utilitarian methodology is the perceived demarcation between that which is scientific and the non-scientific. We saw that J.S.Mill distinguished means from ends, and that this distinction was used later by Robbins as a defining feature of economic science. Mill's usage differed from that of Robbins, though. For the utilitarians there was no distinction between the scientific and the non-scientific. "The quintessence of utilitarian moral philosophy, which at that time constituted the basis of economic thought" Myrdal maintains "was the

conviction that the will both can and should be rational even with respect to the end towards which it is directed" [1953 p.8].

The question for the utilitarians was not how human values could be examined scientifically, since all values were scientific. Because of the identification of science and morality, the problem appeared to be technical: how to arrive at a social 'maximum' of morality. Bentham's desire was to become "the Newton of the Moral World", and he hoped to develop a method for calculating morals, a "felicific calculus" [Mitchell 1918 p.170]. However the problem is not straightforward. The calculation of a social maximum of utility would require the summation of the net utility of each individual [p.171]. This implies an universal cardinal measure of utility. It is far from clear how such a measurement could be derived. Even though Bentham did not appear to view the principle of measurement as problematic, in practice he could only measure on a nominal scale [p.173].

Bentham actually began to move towards the notion of pecuniary measurement of utility and towards the concept of indifference analysis to find his way out of this difficulty:

If of two pleasures a man, knowing what they are, would as lief enjoy the one as the other, they must be reputed equal ... If of two pains a man had as lief escape the one as the other, such two pains must be reputed equal. If of two sensations, a pain and a pleasure, a man had lief enjoy the pleasure and suffer the pain, as not enjoy the first and not suffer the pain must be reputed *equal*, or, as we may say in this case, *equivalent*

This equality then suggests the use of pecuniary measure:

If then between two pleasures the one produced by the possession of money, the other not, a man had as lief enjoy the one as the other, such pleasures are to be reputed equal. But the pleasure produced by the possession of money, is as the quantity of money that produces it: money is therefore the measure of this pleasure ...

If then, speaking of the respective quantities of various pains and pleasures ... we would understand one another, we must make use of some common measure. The only common measure the nature of things affords is money ... Money is the instrument for measuring the quantity of pain or pleasure. Those who are not satisfied with the accuracy of this instrument must find out some other that shall be more accurate or bid adieu to Politics and Morals [Bentham, in Halevy 1901 p.410-14 cited Mitchell 1918].

It can be seen how in attempting to derive a method of measurement for utility Bentham began to move away from his original idea of the calculation of morality, for by utilising money as a measurement we immediately begin to place a boundary around those human activities which are to be deemed capable of 'scientific' examination. But the whole point of utilitarianism was precisely to get round this problem of demarcation. In the utilitarian political economy there was no real sense in which one could separate non-economic ends from economic means: both were governed by utility:

In other words, the distinction was not one of principle. It was dictated entirely by expediency. That this was the sole motivation was, indeed, sometimes explicitly stated.

It was thus regarded as both natural and desirable that political economists should venture beyond the frontier line (Myrdal 1953 p.8).

Indeed the later classics had little option but to deal with utilitarianism in this way, because they simply did not have the tools to deal with money measurements of utility. Myrdal believed that "the hedonistic trimmings of classical theory show merely a desire to conform to the utilitarian philosophy" (1953 p.80). The actual analysis of the later classics remained much as before. "Not until later did utilitarianism exert its full influence upon economic theory" [Ibid].

Jevons

What the classics lacked in their attempt to obtain money measures of utility was the mathematical tools of the differential calculus. It was only with the work of the marginalists in the late nineteenth century that the method of 'measuring' utility was fully developed. Jevons' *Theory of Political Economy* was an explicit attempt to create the felicific calculus of Bentham [1957 p.xxvi].

The marginalists linked price² with utility by distinguishing between the total

² More accurately exchange value: see below

utility of the entire quantity of a commodity consumed and the 'final degree' of utility: roughly speaking the increment in total utility caused by consumption of the last unit.³ But in order to provide the connection with price a further concept was needed: the notion of diminishing marginal utility. This enabled the construction of the felicific calculus; the market would adjust until the marginal utilities of all goods are equal. If the marginal utility of one commodity was higher than that of another, then the individual would switch consumption to the first good. Of course, the more of this commodity that is consumed, the lower the utility yielded by each incremental unit. Switching consumption to goods with a higher marginal utility therefore lowers the marginal utility. Eventually a position will be reached where all marginal utilities are equal, and there is no incentive to switch consumption. This is an equilibrium position: there is no pressure for change.

This treatment of the Marginalists made it appear possible to find a social optimum. However the new approach to Utilitarianism was more than just an improvement in technique — Jevons' professional pique against Mill was in this respect somewhat misplaced. There had been earlier critiques of the labour theory of value, and several anticipators of the marginal utility technique used by Jevons. It would seem that the reason that the time for this idea came in the late nineteenth century was more as a response to the advent of socialist political economy than any notion of scientific progress [Dasgupta 1985 p.95]. Blaug does point out that Marx's work was probably unknown to Jevons and the other marginalists [1962 p.282], but he also writes that the utility theory itself was not accepted for many years. It could be suggested that the acceptance of marginal utility at the turn of the century — rather than earlier when it was first mooted — was due to the ideological content [Gramm 1988 p.232]. Recall that the utilitarian philosophy was itself a response to the natural-law philosophy of Adam Smith. That the utility theory should provide a philosophical challenge to labour value theory should come as no surprise, but the

³ For an explanation of the differences between various formulations of the differential approach to utility see Mitchell [1969 p.55].

treatment of utility theory itself shows clearly a conservative bias. We will be tracing these developments in due course.

The utilitarian theory of human nature focused theory on individual behavior. The problem that Bentham was struggling to solve was in essence to derive from this an operational *social* theory. The significance of marginalism was that it made the calculation of this utilitarian social theory appear possible. The resultant theory enabled the marginalists to arrive at the same *laissez-faire* conclusions as the classics, but without employing any notion of objective value. Utility was subjective — it was located within the individual actor: "The utility theory of value is primarily an attempt to explain price determination in psychological terms" [Viner 1925 p.123]. However, there is a difference in this approach, reflecting the problems of positive social science. The psychological 'explanation' remains an hypothesis — unlike production, utility cannot be objectively known: "One does not proceed from a knowledge of relative utility to prices of commodities. The procedure seems to be the other way about; when one knows relative prices, one hypothesises that they represent the relative marginal utilities of commodities" [Dasgupta 1985 p.85].

If the methodological impact of utility theory was the movement towards individualism, the theoretical effect was to switch the focus of economics from production to demand [Gramm 1988 p.232]. The problem is how to move from a (subjective) utility to an (objective) market demand. Some of the marginalists (including Jevons) tended to move directly from utility to demand; the former was expressed in money terms, thereby identifying it with the later [Viner 1925 p.125]. Actually, Jevons never really arrived at a fully fledged price theory. He does equate purchasing power with the ratio of exchange [1957 p.81], but he tends to move directly to this from price without showing how the ratio of exchange has been derived. In fact, all Jevons' examples involve barter (cf. Mitchell 1969 p.61 ff.). Price theory proper was only developed after Jevons. Furthermore, his treatment of the derivation of maximum aggregate utility is incomplete in other respects also. His analysis has no time factor present at all [Hutchison 1953 p.44]. Also, because Jevons limits his examples to

barter, he is necessarily restricted to a two-person, two-commodity analysis, and it is unclear whether this can be generalised. Jevons therefore developed his rather ambiguous concept of the 'Trading Body'. This was defined simply as "any body either of buyers or sellers" [1957 p.88]. Jevons did not distinguish between individual traders and groups of traders: "The trading body may be a single individual in one case; it may be the whole inhabitants of a continent in another; it may be the individuals of a trade diffused throughout a country in a third." [p.88]. The actual size of the trading bodies was not a relevant factor in the theory, since "the principles of exchange are the same in nature, however wide or narrow may be the market considered." [p.89].

Jevons' use of the concept of the Trading Body is significant for the operationalisation of utilitarianism in two respects. Utilitarianism involved the derivation of a *social maximum* from the maximisation of *individual utility*. Later analysis showed that this social maximum would not necessarily occur when just two traders were involved; to ensure this maximum there is a minimum requirement of a large number of traders, notwithstanding the other requisites of perfect competition. Game Theory analysis was later to show that the maximum aggregate utility is often not arrived at in bilateral negotiation.⁴ Jevons mentioned some of the conditions of competition in his 'law of indifference' (which gives a simplistic explanation of why a single price accrues for each commodity in a market), but dismisses these as "extraneous circumstances", and also applies them to his two-trader models, where they are not sufficient.

In addition, the concept of the Trading Body tends to beg the entire question of aggregation. Jevons admits that explanations based on an aggregate Trading Body must be derived from individuals [1957 p.89]. He also admits that the differential calculus is difficult to apply to individual behavior, since the functions are not continuous [p.15]. The calculus is only applicable to averages of large groups: these show continuous functions, and the effect of small price changes is negligible [Ibid]. Jevons terms this average the "Fictitious Mean": the average does not correspond to

⁴ The original text was Morgenstern and von Neumann [1944].

any individual. Yet Jevons does not show us how this aggregate function is derived. Again, he switches from utility to demand in a rather disconcerting fashion. If we are only concerned with demand functions then the use of an aggregate function may have some legitimacy, but Jevons was concerned with utilities. In order to be able to derive an aggregate *utility* function, we have to assume that utility is comparable between people. Yet this Jevons denied [Myrdal 1953 p.101, cf. *op.cit* p.87, Stigler p.72]. He states that

The reader will find, again, that there is never, in any single instance, an attempt made to compare the amount of feeling in one mind with that in another. I see no means by which such comparison can be accomplished ... Every mind is thus inscrutable to every other mind, and no common denominator of feeling seems to be possible (1957 p.14).

This problem actually has its roots in the positivist/ policy dilemma we have already encountered. The dilemma is that the admission of interpersonal comparisons leads straight back to the sort of egalitarian conclusions that the labour theory of value led to. The problem is that the concept of diminishing marginal utility could also be applied to money, income or wealth. This would suggest that maximum aggregate utility would be achieved when these were equal, which implies a re-distribution of wealth along egalitarian lines.

This points to egalitarian principles, justifies trade unions, progressive taxation, and the welfare state, if not more radical means to interfere with an economic system that allows so much of the good juice of *utility* to evaporate out of commodities by distributing them unequally.

But on the other hand the whole point of *utility* was to justify *laissez faire* [Robinson 1962 p.53].

Again, the dilemma of price and value emerges. If Jevons is to arrive at a theory of price, based on the hedonist psychology, and to move from utility to demand, he has to accept the interpersonal comparison of utility. This interpersonal comparison would, of course, result in an egalitarian *value* theory.

By denying that utility can be compared between different people, Jevons appears to have avoided this conclusion, but at a cost. As Myrdal points out: "... the

concepts of utility and disutility which the marginalists took over from the utilitarians were intended for interpersonal comparisons'' [1953 p.90]. The abandonment of interpersonal comparisons necessarily involves abandoning ''utilitarian comparisons and the *social* calculus. At this point economic theory parts company with utilitarian philosophy. It aims at a positive psychological analysis, and does not as such intend to be a rationalistic political metaphysics.'' [op.cit. p.87]. The price of denying interpersonal comparisons is the failure to operationalise utilitarianism. We can no longer equate 'what we shall do' with 'what we ought to do', because we cannot compare the results of our actions between people. The social optimum becomes indeterminate.

In fact the question of distribution shows up well the limits that Jevons put upon his exposition of Bentham's theory. If we are not to have interpersonal comparisons of utility, along with the consequent egalitarian distribution, we may ask what distribution theory Jevons has to offer in its place. Here we find the limitation of the theory. Jevons' calculus was only really concerned with commodities and exchange [see Mitchell 1967 p.43]. Bentham was concerned with the entire range of human activities, especially with penal legislation and the legal system [Warnock 1962 p.12]. Of course, as we saw, the whole problem with the Bentham scheme was the measurement of utility. Jevons appears to have somewhat abandoned this general ethical calculus, and limits his analysis to market exchange. He evokes the notion of a hierarchy of pleasures to separate the higher (moral) pleasures from the lower (economic) feelings, and suggested that ''It is the lowest rank of feelings which we here treat. The calculus of utility aims at supplying the ordinary wants of man at the least cost of labour'' [Jevons 1957 p.27]. Yet Jevons appears to believe in the moral calculus, and seemed to believe that distribution was a part of it:

Each labourer, in the absence of other motives, is supposed to devote his energy to the accumulation of wealth. A higher calculus of moral right and wrong would be needed to show how he may best employ that wealth for the good of others as well as himself. But when that higher calculus gives no prohibition, we need the lower calculus to gain us the utmost good in matters of moral indifference. [1957 p.27]

Basically Jevons is passing the buck. He is simply asserting that somehow distribution and ethics can be calculated, but that this is not his concern.

Given that no indication is provided as to how a 'higher calculus' of morals is to work, we are left with something of a vacuum. The whole point of utilitarianism was that all human activity was determinate. Jevons is now saying that only part of this activity is determinate, and also that this part cannot be measured interpersonally while the rest can, since if we cannot make interpersonal comparisons of utility even in the moral calculus it is hard to see how any utilitarian policy conclusions can be arrived at at all. Jevons needs to maintain two different notions of utility: the ethical one which is comparable between people, and the political economy/exchange utility which is not. Jevons makes no attempt to distinguish between these: we have no boundary line between the one and the other. We are asked to distinguish between 'political economy' and 'ethical' aspects of behavior without being given any distinguishing feature.

Without any distinction between the political economy and ethics, it would seem to be impossible to derive any policy prescriptions along Jevonian lines. In fact, Jevons himself originally expressed doubts as to the usefulness of his theory [1871 p.25], but in later editions of the *Theory of Political Economy* he expunged these passages. But separated from its utilitarian roots the theory had very little to offer in the way of practical advice, and a return to the utilitarian philosophy involved accepting interpersonal comparisons, along with the distributional consequences. This was the path chosen by Edgeworth.

Edgeworth

Edgeworth was in some ways far more utilitarian than even Jevons [Mitchell 1969 p.95]. His book *New and Old Methods of Ethics* was part of his attempt to derive a clear mathematical conception of exact utilitarianism [Hutchison 1953 p.109]. He believed that this was implied in the greatest happiness principle [Edgeworth 1953

p.109], and he thought that the "Economical Calculus" as he termed it, was an extension of this. His 1881 essay *Mathematical Psychics* was subtitled 'An Essay on the Application of Mathematics to the Moral Sciences', and was an attempt to formulate this economical calculus. The main title of the essay is of significance. Edgeworth believed that hedonism represented an accurate psychological analysis. Furthermore, the methods of both physical science and "moral science" were identical: "The central conception of Dynamics" according to Edgeworth "is *other-sidedly identical* with the central conception of Ethics", and he believed that

'Mecanique sociale' may one day take her place along with *'mecanique celeste'*, throned each upon the double-sided height of one maximum principle, the supreme pinnacle of moral as of physical science. [Edgeworth 1881 p.12]

For Edgeworth utility was a measurable, observable fact. It was, to use Samuelson's phrase, 'as real as his morning jam'. Like Jevons, Edgeworth's felicific calculus was divided between the economic and the ethical calculus, but Edgeworth's arrangement was not hierarchical. The ethical calculus was in no sense a 'higher' calculus, as it was for Jevons, but is parallel with economics, and it is through the ethical calculus that Edgeworth derives his social value solution. "The Economical Calculus" was to explain

the equilibrium of a system of hedonic forces each tending to maximum individual utility

whereas the Utilitarian Calculus considered

the equilibrium of a system in which each and all tend to maximum universal utility. [op.cit. p.15]

The question of social value and distribution was therefore part of the moral calculus, but this calculus Edgeworth believed to be fully capable of operationalisation. Putting the moral calculus into operation did, however, require the use of inter-personal com-

parisons of utility, according to Edgeworth:

For moral calculations, a further dimension is required: to compare the happiness of one person with the happiness of another, and generally the happiness of groups of different members and different average happiness.

Such comparison can no longer be shirked, if there is to be any systematic morality at all. It is postulated by distributive justice. It is postulated by the population question ... [op.cit. p.18]

Boundary questions were not of the same importance for Edgeworth as for Jevons, since the dual calculus were 'other-sidedly identical'. It will be recalled though, that the need for a moral calculus in Jevons' theory arose out of distributional considerations. We may ask if Edgeworth, who claims to be able to consider the moral question of distribution, accepts the egalitarian conclusion implied by diminishing marginal utility. Edgeworth avoids this conclusion by positing differing "capacities" for the enjoyment of pleasure: the savage societies, the labouring classes and women may well, according to Edgeworth, have "inferior capacities" for pleasure [op.cit. p.77 ff.].

This artificial and arbitrary supposition of Edgeworth shows up well the problems the marginalists had in squaring the radical element of utilitarianism with the conservatism inherited from the natural law philosophy of the early classics. We will see this reconciliation emerge in the works of Marshall, which we will examine shortly, but there were several developments in Edgeworth's works which overshadowed later developments. One of the problems we observed with the Jevonian treatment was that his examples of bilateral trading were not sufficient to derive a social maximum,⁵ and that his device of the trading body involved an inadmissible additive utility function. Edgeworth dealt with the former problem by attempting to specify the requirements for perfect competition.⁶ He also moved from an additive utility

⁵ "He saw, that is, how closely dependent micro-economic maximisation theorems are on the 'perfect compension' postulate. He even went so far as to argue that in a regime of general monopoly (and, presumably of oligopoly) 'abstract economists would be deprived of their occupation, the investigation of the conditions which determine value. There would survive only the empirical school, flourishing in a chaos ...' [*Papers*, vol.1 pp.138-39, cited Hutchison 1953 p.112]

⁶ Edgeworth's formulation involved the ability to recontract: this would ensure an unique equilibrium price [1881 p.18].

function to what Stigler terms a 'generalized utility' function [Stigler 1950 p.77]. For Jevons the utility of a commodity was independent of other commodities. For Edgeworth, the utilities were linked [Hutchison 1953 p.114]. It was in this way that he could address the problem of bilateral trading, which he did through the now familiar device of indifference curve analysis⁷ (he was attempting to show the indeterminacy of this trade, and derived his well-known contract-curve). As Hutchison remarked, it was ironical that the most utilitarian of the marginalists should have developed the concepts that led to the eventual exclusion of utilitarianism from economic thought.

Edgeworth's formulation was dependent upon the moral calculus being operative: it was formulated directly upon hedonism. Later in his life he began to doubt the hedonist psychology in the light of advances in psychology and other sciences [Mitchell 1969 p.94]. Hedonism was no longer in keeping with the spirit of positivism.

From the period of the later classics onwards, economic theory has been out of step with the general advance of ideas. Marginal utility theory attempted to give a hedonistic interpretation of value at a time when psychologists were abandoning hedonism in favour of a more realistic analysis. [Myrdal 1953 p.81]

The early marginalists accepted utility analysis as a fact of common sense [Jevons 1957 p.52, Edgeworth 1877 p.35]. They attempted to operationalise utilitarianism by narrowing the focus of political economy from that of the later classics, but were never really able to derive a distributional theory that was separate from an inoperative moral calculus. They also never properly developed a theory of social value, both of which were essential elements of the classicists' theories.

⁷ These may well have been based on Marshall's in the latter's *Pure Theory of Domestic Values and of Foreign Trade* [Hutchison 1953 p.114].

II. NEO-CLASSICS

Marshall

It was with Marshall that the reconciliation of the classics and the marginal approach was established, and it is Marshall's work that marks the beginning of 'neo-classical' economics proper. This coincided with the fall of utilitarianism as a moral science. It is notable how the most mathematically-inclined of the economists (Edgeworth's background was in Law and English) was also the most sceptical of the usefulness of mathematical analysis.

Though a skilled mathematician, he [Marshall] used mathematics sparingly. He saw that excessive reliance on this instrument might lead us astray in pursuit of intellectual toys, imaginary problems not conforming to the conditions of real life; and, further, might distort our sense of proportion by causing us to neglect factors that could not easily be worked up in the mathematical machine. [Pigou 1925, cited Coase 1975 p.30]

Although he was perfectly able to use mathematical reasoning if and when desired, Marshall was very sensitive to the limits of Mathematics. For Marshall, mathematics was simply a useful language; he advised one of his students to

- (1) Use mathematics as a shorthand language, rather than as an engine of enquiry
- (2) Keep to them till you are done
- (3) Translate into English
- (4) Then illustrate by examples that are important in real life
- (5) Burn the mathematics
- (6) If you cannot succeed in (4), burn (3). This last I did often. [ibid.]

It was important to Marshall that economics should be reflected in practical examples of everyday living. He was not concerned with developing ethical systems of interest

mainly to initiate — although his work did have ethical motivation. For Marshall the idea of measuring ethics, or indeed any social phenomenon, was simply nonsensical. One of his marginal notes in the *Principles* reads "Even common pleasures and pains can be compared only through the strength of the incentives which they supply to action" [Marshall 1890 (1920) p.15]⁸ — a clear indication of Marshall's break with Utilitarianism. Marshall was not concerned with Bentham's 'springs of action', he was concerned with *effect* not *cause*. "It is essential to note" Marshall writes "that the economist does not claim to measure any affection of the mind in itself, or directly, but only through its effect." [Ibid]. He is also explicit in his rejection of utilitarianism [op.cit p.17 fn1]. Typically, the reason for Marshall's rejection of the utilitarian ethic stemmed not so much from abstract methodological considerations as from simple practicalities: utilitarianism did not work. There was no way that Marshall could see of reconciling what is with what ought to be:

Lastly it is sometimes erroneously supposed that normal action in economics is that which is right morally. But that is to be understood only when the context implies that the action is being judged from the ethical point of view. When we are considering the facts of the world, as they are, and not as they ought to be, we shall have to regard as "normal" to the circumstance in view, much action which we should use our utmost effort to stop.

One of the main problems of utilitarianism for Marshall was the undesirable side-effects of the *laissez-faire* policy latent in the orthodox implementation of utilitarianism. He continues:

For instance, the normal condition of many of the very poorest inhabitants of a large town is to be devoid of enterprise, and unwilling to avail themselves of the opportunities that may offer for a healthier and less squalid life elsewhere: they have not the strength, physical, mental and moral, required for working their way out of their miserable surroundings. The existence of a considerable supply of labour ready to make match-boxes at a very low rate is normal in the same way that a contortion of the limbs is a normal result of taking strychnine. [op.cit. p.35]

⁸ References to the 8th. edition (1920) are taken from the Variorum edition, London, Macmillan 1961.

The simple description of social events was not the point of Marshall's work. He justified economic science in the manner that we discussed earlier — it was to improve our lot in life. Marshall refused to accept that the squalor of the poor in Victorian Britain could be squared with the notion that *laissez-faire* led to the best possible social outcome. One of his major concerns in the *Principles* and elsewhere was the abject poverty which the Industrial Revolution left in its wake:

This free competition, or rather, freedom of industry and enterprise, was set loose to run, like a huge untrained monster, its wayward course. The abuse of their new power by able but uncultured business men led to evils on every side; it unfitted mothers for their duties,⁹ it weighed down children with overwork and disease ...

Furthermore the economists were also to blame:

And yet the time at which free enterprise was showing itself in an unnaturally harsh form, was the very time in which economists were most lavish in their praises of it. [op.cit. p.9]

What Marshall seemed to find objectionable was not ethics, but ethics masquerading as science. He also seemed to disregard the idea of a purely descriptive social science. Marshall's aim was to study how to improve the lot of humankind, and this could be best done, he believed, by improving humans themselves. He believed that economic science could show ways in which human nature could be modified for the better [op.cit. p.35]. This was part of Marshall's concern for the plight of the poor.¹⁰

It was for this reason that Marshall was unconcerned with deriving exact theories of transcendental economic 'laws' based on theories of human nature: "Sci-

⁹ This is the first indication of the move away from an *individual* unit of analysis to that of a group unit. Note that this means that *individual* utility can no longer be said to be maximised.

¹⁰ In particular, Marshall held a vague notion of human capital. The improvement of the conditions of the working class would help the material wealth of the country [1920 p.562]. Marshall believed that "The most valuable of all capital is that invested in human beings" [1920 p.564]. The improvement of the effectiveness of the labour force — especially through education [1920 p.212, 562 ff.] — was one of the keys to growth. Indeed, Marshall believed that investment in human capital may itself end class distinctions [Dasgupta 1985 p.118].

ence may suggest a moral or practical precept to modify that nature and thus modify the action of the laws of nature." [1920 p.36] Theory was therefore of little value in itself, but had constantly to show application, indeed Marshall once wrote that "in economics there is 'no theory' to speak of" [Coase 1975 p.28]. Later he wrote to Edgeworth

In my view "Theory" is essential. No one gets any real grip of economic problems unless he will work on it. But I conceive no more calamitous notion than that abstract, or general, or "theoretical" economics was economics proper. It seems to me a very small part of economics proper: and by itself even — well, not a very good occupation of time ... general reasoning is essential, but a wide and thorough study of facts is equally essential ... a combination of the two sides of the work is *alone* economics proper. Economic theory is, in my opinion, as mischievous an impostor when it claims to be economics proper as is mere crude unanalysed history. [cited Coase 1975 p.29; Hutchison p.71]

Marshall was unconcerned with questions of pure theory and with philosophical and methodological disputes. He regarded himself as "midway between Keynes + Sidgwick + Cairnes and Schmoller + Ashley" [cited Coase 1975 p.28]. He simply had no appetite for the debate. He wanted to "obtain guidance in the practical conduct of life", and he required economics to generate pragmatic council as to the best policy.

For Marshall, the notion of measuring the *effects* of utility, and his approach to studying mental states through their manifestations [Principles 8 p.14], was as much a matter of common-sense as utility was for the early marginalists: he uses such phrases as "in ordinary life" and "ordinary usage" several times in his early chapters. It did not, for him, require extensive justification. Neither, Marshall believed, did the definition of economics, although he did move away from the 'wealth' definition of the classics. In the earlier *Economics of Industry* he had been happy to include the idea of wealth:

Those portions of human conduct which are directed towards the acquirement of material wealth, and those conditions of human well-being which directly depend on material wealth, are called *Economic*. The *science of Economics* collects, examines, arranges and reasons about the facts which are connected with the economic habits and conditions of well-being in various countries at various times. [1879 p.3]

Later on he placed more emphasis on the notion of action, and less on wealth. He also emphasised the 'common sense' element more. He began the first three editions of the *Principles* by writing

Political economy or Economics, is a study of man's actions in the ordinary business of life; it inquires how he gets his income and how he uses it

But by the fourth edition Marshall's concern with the idea of social welfare caused him to revise this definition. By then economics was

... a study of mankind in the ordinary business of life; it examines that part of individual and social actions which is most clearly connected with the use of the material requisites of wellbeing.

Marshall's interest was not in material wealth *per se*, but with the material aspects of 'wellbeing' — it was only for this reason that wealth was of importance, and it was this implicit ethic of the 'wellbeing' of society that was at the back of Marshall's endeavors.

The study of "material requisites of wellbeing" required, for Marshall, a fusion of both the psychological approach of the early marginalists and the wealth delineation of the classics. He believed economics "is on the one side a study of wealth; and on the other, and more important side, a part of the study of man." [1920 p.1] What was needed was the analysis of both subjective and objective elements. Writing to N.G.Pierson, Marshall says "The book [*Principles*] was written to express one idea & one idea only. That idea is that whereas Ricardo & co. maintain that value is determined by cost of production, & Jevons & (in a measure) the Austrians that it is determined by utility, each was right in what he affirmed but wrong in what he denied." [Dasgupta 1985 p.104]

We will consider Marshall's value theory in more detail shortly. What is also striking about Marshall's definition, compared to that of the earlier marginalists is the lack of abstraction. We are not dealing with an abstract *homo oeconomicus* which

later economists were to use, nor are we splitting human action into ethical and economic dimensions. We deal "with man as he is; not with an abstract or 'economic' man; but a man of flesh and blood." [1920 pp.26-27] For Marshall, the point was that it is economic forces which mould the character of humans. "For Man's character has been moulded by his everyday work, and the material resources which he thereby procures, more than any other influence unless it be that of his religious ideals" [op.cit. p.1]. It was not that humans are naturally rational, but that in regard to business conduct humans are at their most rational [op.cit. p.20]. Habits and customs are followed, but have themselves arisen from calculation (ibid.).¹¹ However the only generalisable measure of motives, Marshall maintained, was that afforded by money, hence this "is the centre around which economic science clusters." [p.22] Mitchell maintains that for Marshall money measures the "force of motives that control human behavior", and suggests that modern society "is organised about the process of making and spending money incomes", a process which "moulds the minds of people" and

makes them as economic characters substantially different from the denizens of another type of economy. If the theorist could take away the effect of the money economy from the modern civilised nations, he would find that their inhabitants would be very different creatures from what they are. [Mitchell 1969 p.158]¹²

Marshall's theory is therefore objective in the sense that he is concerned with an objective element: that of the *material* aspects of wellbeing. Furthermore, these material aspects have come to centre around money. Although he uses the differential calculus, he is not attempting to measure the same things as his marginalist predecessors. In addition, Marshall is far more cautious about the use of money measures than his predecessors: it is only with "careful precautions" that money can be used as a

¹¹ Marshall's marginal notes are misleading here.

¹² This should be compared with the institutionalist critique of the economic assumptions concerning human action which began to emerge in America about the time of the sixth edition of the *Principles*, with the work of Veblen and Mitchell. We will look at the institutionalist school in chapter four.

measure of the "moving forces" of human lives. We will see here that Marshall only provides scant exposition of the "precautions" we must take when using money measures of behavioral forces, and provides next-to-no institutional analysis of the predominance of money as the focus for modern behavior. The reason for this is that Marshall was not as concerned with market equilibrium as the orthodox economists have tended to be, either before Marshall or since.¹³ Part of Marshall's synthesis of the marginal and classical approach was that he drew no distinction between value and growth (Loasby 1978 p.1). He wanted to incorporate the element of time that was so central to the classics. It was this that led him to insist upon the use of costs as well as demand in value theory: hence his famous dictum about the scissors. Marshall added the concept of diminishing returns to that of diminishing marginal utility to explain how market price was derived. An increase in one factor of production would yield a progressively smaller increase in product. Each extra hour that the labour force worked would result in a smaller and smaller increase in output, the other factors of production (especially plant) being fixed. This meant that while demand price fell with quantity, supply costs rose. At one particular price supply would equal demand: the market would clear.

However this is not the complete story. Costs were, for Marshall, determined differently from demand: the central element in the analysis of supply was time. There were three time-periods, which represented different positions in the variability of output. He outlines the period of "temporary equilibrium", where there is a fixed *stock* of a commodity for sale. This is Marshall's "day". The equilibrium price will be determined by the "higgling and bargaining" of the market, and will represent a market clearing price — assuming that sellers and buyers are equally matched [1920 p.333].

Marshall's main concern though was not with this market equilibrium, but rather where output could vary: supply as a *flow* [cf. Dasgupta 1985 p.104]. One of his mar-

¹³ With the possible exception of Pigou (we are concerned, of course, only with microeconomics).

ginal notes reads "In a stationary state the doctrine of value would be simple" [1920 p.367]. It was the effect of changes over time that interested Marshall. His analysis can be divided into two such time-periods where supply can be treated as a flow: the short period and the long period. During the short period a change in demand will cause a corresponding adjustment in supply and a change in price: an increase in demand will cause price to rise and supply to increase [1920 book V sec.5]. However this need not be an equilibrium price. Marshall did not subscribe to any notion of 'perfect competition' — as Loasby points out, the word does not appear in his index [Loasby 1978 p.5]. Marshall does suppose "free play" of the forces of supply and demand, meaning an absence of combination, and he also supposed "free competition" among buyers and also among sellers [1920 p.342]. Although Marshall envisaged a large number of buyers and sellers, he did not rule out some imperfections in the market. He realised that firms have regular customers and regular suppliers and might not react to short-run changes in market price. Marshall uses an 'oligopolistic' information system [Loasby 1978 p.6]. It is this which brings about a competitive solution — but one which is neither perfectly competitive nor oligopolistic. Marshall believed that the realities of the industrial world would themselves lead to price maintenance. He believed that depressions were caused by a lack of confidence [1920 pp.710-11].

Marshall's analysis of the short run tried to show that the "higgling" of the market should not affect "normal" price greatly. It was his long run analysis that formed the link with the classics, for in the long-run it was costs which were the main variable: firms could adjust their plant to alter output. For Marshall the important element in this adjustment was the notion of returns to scale. Although diminishing returns held for the short run because size of plant was constant, in the long run increasing returns could accrue. For Marshall, this was what led to economic growth: technical changes and also changes in industrial organisation caused by increasing scale. It was this prevalence of increasing long-run returns that Marshall believed was the main justification for contemporary industrial organisation.

This is further underlined by Marshall's treatment of oligopoly. Marshall's conception of the effect of increasing returns upon industrial organisation is that of the industrial 'life cycle'. In the long run many firms presently trading would go out of business: only a few survive to dominate the market. However, these firms will in their turn stagnate to be replaced by newer 'younger' undertakings. By the 1910's Marshall acknowledged this life-cycle analogy was looking somewhat dated, but he did not develop an oligopolistic theory. There is no place for market power in Marshall's analysis once poverty is removed.

In the long-run, the industry will therefore be in equilibrium. It is worth stressing that this is an equilibrium of the *industry*: the firms themselves could come and go: there was no sense in which one could discuss the long run equilibrium position of the *firm*. Again, this is much more like the classics that a purely marginalist analysis.

Marshall's treatment of value and growth is of significance to our thesis for several reasons. Methodologically, the need for consideration of long-run effects mitigated against the partial equilibrium approach. *Ceteris* were not likely to remain *paribus* in the long run. This is possibly why Marshall believed his analysis to be realistic: this was not an abstraction, but 'flesh and blood'.

We will recall the other aspect of Marshall's work, which was his concern to study real humans rather than abstract 'economic human'. This enabled Marshall to justify moving directly from utility to money. All his examples in the *Principles* are of market demand schedules, and he frequently uses the concepts interchangeably [e.g. 1920 p.92-94]. There is nowhere an equation of utilities in terms of price. We therefore require an explanation of how maximum social value is coincidental with equilibrium in cash terms. Marshall uses the device of the *surplus*

Since marginal utility of a commodity diminished with quantity, the demand price for all units except the last will exceed the equilibrium price. Market price, of course, does not vary — the same price is charged for the first unit as for the last. Marshall therefore maintained that consumers gained a *surplus* by trade: they paid

less for the first units of a commodity than they were prepared to pay.¹⁴ By the same token, producers also gained a surplus: they would have been prepared to supply the first few units at a lower price than the equilibrium price, since the supplier is subject to diminishing returns. The total surplus is maximised when supply surplus equals demand surplus — at the equilibrium price. Note that this measure has currency as its unit: it is a direct money measure of satisfaction obtained through trade. Also note that, as such, it implies both interpersonal comparisons and a cardinal measurement of utility. Of course, as soon as we allow for cardinal utility, we run into the diminishing marginal utility of money, and all the egalitarian implications that this holds. By contrast, the measurement of consumer surplus adopted by Marshall is only unambiguous if the marginal utility of money is constant [Dasgupta 1985 p.113], but Marshall maintained that it was only in extreme cases that the marginal utility of money was not constant. This may explain why Marshall had misgivings about always using the competitive equilibrium position as an optimum [ibid.].¹⁵

The exceptions [to a constant marginal utility of money] are rare and unimportant in markets for commodities; but in markets for labour they are frequent and important.

When a workman is in fear of hunger, his need of money (its marginal utility to him) is very great ... and he may go on selling his labour at a low rate ... [op.cit. p.335]

Marshall's *social* value theory depended for the most part on an unrealistic psychological assertion concerning the marginal utility of money. This assumption enabled him to avoid the distributional implications of marginalism.

Marshall did, though, suggest a technical reason for particularly requiring the amelioration of poverty. He fully realised that the vast inequalities of wealth would

¹⁴ Marshall defined the consumers surplus as "the excess of the price which [the consumer] would be willing to pay rather than go without the thing, over that which he actually does pay" [1920 p.124]. This amount will be less the more units are actually consumed, and will be zero at the market clearing price, since the marginal utility diminishes with each additional unit. If it were possible to sell the consumer units at a higher price than the subsequent units, then the producer could take some of this surplus (in a competitive market this is not possible — the consumer would buy elsewhere, at the lower equilibrium price).

¹⁵ Hicks also believes this is a requisite for an industry demand curve [1939 p.26].

mean that it would be most unlikely that the marginal utility of money would remain equal. Furthermore, the mathematical training of Marshall did not allow him to ignore the problems caused by the use of the differential calculus. The concept of diminishing marginal utility and diminishing marginal product rested upon a notion of a small incremental change. It was this that enabled the derivative to be obtained: if the changes were large, the margin becomes indeterminate. Marshall realised that one market where changes are likely to be far from incremental was the labour market. Changes in prices here may well have large effects on 'wellbeing'. The labour market is for this reason exceptional, and disadvantages in bargaining tend to be cumulative.

It was for these reasons (as well as the common-sense observation of distributional problems), that led Marshall to question the marginal productivity theory of distribution proposed by J.B.Clark and later by Wicksteed. This was in fact the formal closure of the system: wages were determined by the value of the marginal product made by the labourer. Marshall never fully¹⁶ accepted the Marginal Productivity Theory. One of the essential elements is that it only holds for constant returns to scale, whereas the entire thrust of Marshall's work stressed increasing returns. Both Clark and Wicksteed's analysis were essentially static — they did not consider long run supply costs as did Marshall. Indeed, Wicksteed believed that the supply schedule was simply an inverted demand schedule:

But what about the "supply curve" that usually figures as a determinant of price co-ordinate with the demand curve? I say it boldly and baldly: there is no such thing. What usually figures as such is merely a disguised and therefore unrecognisable portion of the 'demand curve' ... [Wicksteed 1913 p.261]

Of course, this was far from the case for Marshall: the whole point was that costs were the dominant feature in the long run. This also explains why his advocacy of redistribution did not extend beyond the amelioration of extreme poverty. The essen-

¹⁶ His reaction is ambiguous [cf. Hutchison 1953 p.84 ff.].

tial principle for Marshall was that although the analysis of value may show up imperfections, it leads to growth in production in the long run. Even monopolies could be justified in this way [Dasgupta 1985 p.119]. By increasing the quantity of production — with the worst evils of poverty eliminated — the competitive economy could create a *post hoc* maximum solution, even though there may be *ex ante* inequalities.

Marshall's growth theory was therefore an essential element of distribution also. It was growth that justified inequality (if not extreme poverty). Unfortunately, Marshall's emphasis on long-run growth impales him on the other horn of the positivist dilemma, because as we mentioned at the outset of this chapter a *laissez-faire* value theory must be subjective. The utilitarian theory was subjective, but only at the expense of a realistic psychology. Marshall has rejected the psychology of hedonism, but his emphasis on growth involved, as with the classics, a corresponding emphasis on costs. Costs, though, are *objective*, and since they are objective it is not clear why production cannot be planned. If Marshall's analysis revolves around increasing returns to scale, and if furthermore he only considers the equilibrium of the *industry*, then there appears to be little justification for *laissez-faire*: increasing returns to the industry could be easily served by large state enterprises. Marshall was forced to resort to casual psychological assertions to back up his position, claiming that planning undermined "self-reliant and inventive faculties" and so on.

In addition, we can see that Marshall often resorts to unsubstantiated claims, most notably for the growth element of his theory. He does not explain exactly *how* the possibility of returns to scale will affect technical and industrial change, and conversely why only the market mechanism can achieve this. We are not told why oligopolistic markets would maintain price, and not lead to depressions.¹⁷ We are not told exactly why there should be a rotating elite of dominant firms, or even why Marshall's limited concept of free competition should hold — or what would happen when it does not. Neither is there any but the most casual institutional analysis of our

¹⁷ or as Mitchell was shortly to suggest, business cycles.

"men of flesh and blood" - and no analysis of any other forces *not* measured by money. Furthermore, if "Man's character" is malleable, as Marshall made out, then it can be manipulated, and utility and market demand are no longer guides to material well-being.

By the time of Marshall's death the sort of psychological assertions he invoked would no longer wash as science. Furthermore, Marshall's lack of oligopolistic theory was beginning to show as a major weakness in his theory, as the 'life cycle' analogy was called into question by dynasties of joint-stock companies. His concept of increasing returns was coming under fire from within the profession. And in America his casual institutional analysis was being challenged by the Institutional school.

Pigou

The Marshallian analysis therefore foundered on the same problems that we considered in a methodological guise in the last chapter. If we attempt an objective, refutable analysis à la Hutchison, then we must either accept the viability of economic planning or provide proof as to why planning is not viable. In his attempt to move from static value analysis to dynamic growth — in order to escape the distributional consequences of the surplus — Marshall necessarily invokes the objective cost analysis of the classics, and is therefore not able to sustain the political theory implied in his position. Marshall is unable to escape the value implications of his price theory.

The critiques of the Marshallian approach had very little immediate impact upon orthodox economics. Indeed, Marshall's successor at Cambridge — his pupil Arthur Pigou — was to continue employing the same value theory as his teacher. Pigou was not as concerned with individual behavior as Marshall was however. His aim was to examine the aggregate results of marginal analysis. Aggregate individual utilities were known as welfare, and the analysis of this Pigou called Welfare Economics.

One of Marshall's early definitions of Economics, it will be recalled, concerned

how man "gets his income and how he spends it." Pigou's concern is the National Income, or as he terms it, the "National Dividend". Like Marshall, Pigou only considers "that part of social welfare that can be brought directly or indirectly into relation with the measuring-rod of money" [1920 p.11]. Unlike Marshall though, Pigou does not defend this choice of boundary by institutional analysis, however casual. He simply states that "The one obvious instrument of measurement available in social life is money" [ibid.], and then proceeds without further ado. This lack of caution was to create problems for the analysis. In fact, even Pigou admits that this 'Economic Welfare' cannot be separated "in any rigid way from other parts", and that "The outline of our territory is, therefore, necessarily vague", but he still asserts that "the test of accessibility to a money measure serves well enough to set up a rough distinction." [ibid.]. Indeed, Pigou believed that effects on economic welfare were "probably equivalent in direction ... to the effect on total welfare" and that "in all circumstances the burden of proof lies upon those who hold that the presumption should be overruled." [p.20]

Although Pigou adopted the Marshallian value analysis, he did not adopt the concept of consumers surplus. Instead he went directly to a marginal approach [Blackhouse 1985 p.166]. The marginal analysis itself is fairly standard once the initial concepts are established.¹⁸ Pigou takes the concept of marginal product developed by Marshall and J.B.Clark: this he refers to as marginal private product. The contribution to the aggregate whole of a marginal act of production is the marginal social product. In a competitive economy, the national dividend is maximised when these two products are equal.

The aim of policy would therefore be to ensure that this is the case. There were several reasons for divergence, one of the most important of which were increasing returns to scale. It will be recalled that these formed a major plank of Marshall's value theory. Marshall suggested that a subsidy to industries subject to increasing

¹⁸ "My conclusions ... are identical with those established thirty years ago by Dr.Marshall" (Pigou 1924 p.30).

returns may optimise welfare. Pigou believed that this should form part of policy. However, this notion came in for sharp criticism, most notably from Clapham's jibe that the concepts of constant, increasing and diminishing returns were "empty economic boxes". Clapham did not believe that the concepts were capable of being operationalised: he maintained that it was impossible to tell which industries were subject to falling costs and which were not [Clapham 1922]. Indeed, for the purposes of fiscal policy which Pigou and Marshall proposed, it appears to be necessary to make an almost entrepreneurial *ex ante* judgement as to long term costs.

Indeed, the whole Marshallian value/growth theory, which Pigou also based his work upon, was now being questioned. Unfortunately, it will be recalled that the prospect of growth was used to justify inequalities. Once the idea of growth is removed, egalitarian *laissez-faire* policies lose their justification. And such a justification was indeed necessary under the neo-classical scheme, because of the egalitarian implications of diminishing marginal utility of money. Pigou's treatment is slightly more forthright than Marshall's:

The old 'law of diminishing utility' thus leads securely to the proposition: Any cause which increases the absolute share of real income in the hands of the poor, provided that it does not lead to a contraction in the size of the national dividend from any point of view, will, in general, increase economic welfare. [Pigou 1920 p.91 emphasis added]

There is even less analysis of the limitations of redistribution and intervention here than in Marshall's work however: presumably we are to take Marshall's qualifications and casual psychology *en bloc*. It was through assertions concerning psychology and technical progress that Marshall defended *laissez-faire*, and these problems have been imported straight into the Pigouvian scheme.

The other problem¹⁹ both Marshall and Pigou fail to deal with concern aspects

¹⁹ Pigou suggests that freeloading could also occur when durable capital is leased rather than owned. The tenants may not receive adequate compensation for investment [p.176], in particular this could apply to tenant-farmers [p.177]. He believes that a case could be made for compensation to be paid for investment in capital made by tenants (He notes that penalties are often levied for damage to capital) [p.179], but also that all such compensation schemes have shortcomings.

of material well-being which cannot be judged by the measuring-rod of money. Marshall devotes considerable space to discussing external economies to scale of the *industry*, but he has no mention of general external economies and diseconomies, especially as concern the individual. The problem of what economists now call externalities — where the supplier of goods or services renders a cost or benefit to other (third) parties, who do not, or cannot, receive compensation for loss or submit payment for benefit — was more crucial for Pigou, who was directly concerned with the divergence between individual and social maxima. If the costs of production are not internalised, but rather passed on to the community, then the private product will be higher than the social product, similarly, if it is not possible to collect payments for goods which give universal benefit, these goods may not be provided: the marginal social product is higher than the private product. In these instances the marginal social product will obviously vary from the marginal private product. Sidgwick's example of the latter is that of a lighthouse: this would bring substantial benefits to shipping, but it is not possible in many cases to collect payment from them [Pigou 1920 p.186]. Pigou gives various other examples, such as pollution and loss of amenity.

The problem with externalities is that often it is far from clear that these are measurable by money — in fact Pigou himself observes that few real-life examples are capable of monetary measurement:

If we were to be pedantically loyal to the definition of the national dividend ... it would be necessary to distinguish further between industries in which the uncompensated benefit or burden respectively is and is not one that can be readily brought into relation with the measuring rod of money. This distinction, however, would be of formal rather than real importance, and would obscure rather than illuminate the main issues. I shall therefore, in the examples I am about to give, deliberately pass it over. [Pigou 1920 p.185]

Pigou's position is somewhat unclear: if there is a problem here he should attempt to deal with it. His tone ("pedantically loyal ...") seems to suggest that he regards the non-pecuniary externalities as either exceptional or unimportant. He believed that, in spite of the non-pecuniary nature of many externalities, fiscal measures should prove

sufficient to deal with the problem. It was not until much later that the possibility that the "measuring rod of money" may be faulty became widely debated. Hutchison remarked that the acceptance of externalities put the concept of marginal social net product in a rather dubious position:

Any summation even of the more strictly economic 'products' into the single 'social product' of an economic act is bound to be more or less arbitrary, but would have nothing 'illegitimate' about it if the result emerging from this use of 'the measuring rod of money' was significant for a particular purpose. But under the one relevant heading where a wider range of clear practical examples is given, the introduction of the "measuring rod of money" ... is held to obscure rather than illuminate the main issues. [Hutchison 1953 p.293]

At any event, the imposition of taxes or subsidies on 'externalities' itself has distributional implications: fiscal policy itself presumes a net equitable level of distribution. Pigou disregards these distributional implications because he seems to regard the matter as trivial, but subsequent events have suggested this is far from being the case, and it has become a matter of importance where the incidence of taxation or subsidy for externalities should lie. Again, the distributional aspects of value theory cannot be ignored, as indeed the limitations of the neo-classical value theory cannot.

It had become clear that it would not be possible to separate out the marginal utility from its distributional implications. Jevons and Edgeworth tried to do so, but were forced to rely on an unoperational 'scientific' utilitarianism. Marshall and Pigou tried to deflect concern with distribution by incorporating growth into their value theory, but this was no more operational than before, and was looking less and less plausible as the 1930's approached. The 30's were to herald a movement away from the analysis of material well-being towards a purely subjective analysis, and the complete separation of distribution from value. The price of these changes was to propel economic theory further and further into an analytic shell.

III. ORDINALISM

Pareto

The movement towards this state of affairs actually began contemporary with Marshall in the works of Vilfredo Pareto. Pareto discarded the utilitarian philosophy in favour of positivism. He also refused to adopt the marginalist partial equilibrium approach, preferring instead Walrasian general equilibrium. He says of Marshall that "Marshall has not yet managed to grasp the idea of economic equilibrium ... He adds nothing remarkable to our knowledge" [cited Cirillo 1979 p.15]. He believed that the study of economic units in isolation from each other caused the imputation of incorrect causal relationships. One had to consider equilibrium in terms of the economy as a whole [Tarascio 1966 p.109]. We noted earlier how Edgeworth was moving towards the notion of interdependent utilities. Pareto, following Walras, extends this into a concept of equilibrium simultaneous throughout all exchanges. Pareto also used Edgeworth's device of indifference analysis. However the indifference curves had a completely different meaning for the two theorists. Edgeworth, it will be recalled, believed very strongly in the utilitarian philosophy and cardinal utility. Pareto, on the other hand, rejected all notions of utility. The indifference curves for him were factual data [Mitchell 1967 p.412]. Pareto believed that it was necessary to go beyond hedonism:

Subsequently it seemed to me that one could go one step further. I was worried about that *pleasure* and that *pain* which had to be measured, because in reality, nobody is capable of measuring pleasure. [cited Tarascio 1966 p.410 original emphasis]

Although Pareto still had some notion of utility in his *Cours* [1896], in the *Manuel* the use of indifference analysis enabled him to break away from utility alto-

gether [Cirillo 1979 p.21]. In fact, by the turn of the century Pareto had broken with the idea of value entirely. The whole notion of value, he believed, was metaphysical:²⁰

... there are people who think that the new economic theories have been produced to explain value. Far from it! I am looking for something very different from the metaphysical reason of value [1901, cited Tarascio 1972 p.410]

Pareto's theory was one of price, not value — it was objective and positive: "I look for a theory which may include and present economic facts" [ibid.]. For Pareto, the economist should focus only on the objective data of choice [Tarascio 1972 p.411 ff.]

This would appear to be naive empiricism, but Pareto is not attempting to explain these facts. His concern is limited to pure economic theory. His objective is

only to search for the uniformities that phenomena present, that is to say their laws, without having any direct practical usefulness in mind, without concerning himself in any way with giving recipes or precepts, without seeking the happiness, the benefit or the well-being of humanity or of any part of it. [Pareto 1909 p.2]

We have seen that Pareto has eschewed both utility and value, and that he is not concerned to investigate 'wellbeing'. His sole stated concern is with uniformities. We may therefore wonder if he has any time for questions of distribution. Without a notion of value it would not seem possible to consider distribution, and indeed Pareto excludes distribution from his economic analysis. He believes that distribution is a question of social ethics [Cirillo 1979 p.43]. The 'ophelimities' of two people cannot be compared: they consist of heterogeneous quantities [Pareto 1916 sec.2130]. This has the corollary that it is not realistic to talk of a unique optimum for a commodity. Pareto distinguishes between a maximum *of* the community and a maximum *for* the community [op.cit. sec.2129]. There may be many such maxima: we cannot compare

²⁰ And also, of course, dispensable: cf. Robinson [1962], who also believes that 'value' is metaphysical, but believes that metaphysics is a necessary complement to science.

one with another without considering distribution.

In fact, there is a severe restriction on the movements to general equilibrium which can be considered. Since the economist cannot consider distribution or re-distribution, only movements that are not to the detriment of any individual are permissible; even if the social gain overall is positive, a loss to any individual would involve a re-distribution. This condition leads to what is now called a 'Pareto optimum' position. The originator of the condition did not view the ensuing optimum with anything like the contemporary enthusiasm though. For Pareto, this condition simply represented the boundary between economics and sociology:

Movements of a first type, P, are such that, beneficial to certain individuals, they are necessarily harmful to others. Movements of a second type, Q, are such that they are to the advantage, or to the detriment, of all individuals without exception ...

Consideration of the two types of points, P and Q, is of great importance in political economy. When the community stands at a point, Q, that it can leave with resulting benefits to all individuals, procuring greater enjoyments for all of them, it is obvious that from the economic standpoint it is advisable ... to move on from it as far as the movement away from it is advantageous to all. When, then, the point P, where that is no longer possible, is reached, it is necessary, as regards the advisability of stopping there or going on, to resort to other considerations foreign to economics — to decide on grounds of ethics, social utility, or something else, which individuals it is advisable to benefit, which to sacrifice. From the strictly economic standpoint, as soon as the community has reached a point P it has to stop. (Pareto 1916 vol.4 sec 2129)

The condition for 'Pareto optimum' is therefore in no wise an exclusive decision-rule for a society, and it is not really used in this manner. It is simply a statement of the limitations of pure economic theory, given the conditions which Pareto placed on economic theory.

We might hold that this limitation places extraordinary restrictions on policy formation. It would be nigh impossible to only promote policies which affected nobody for the worse. Of course, as we saw, Pareto was not concerned with economic policy. Pareto's exclusive concern as an economist was with pure economics, precisely what Marshall believed was a waste of time. The point was that in Pareto's view the economist, *qua* economist, could not give "guidance in the practical conduct of life", to use Marshall's phrase. The practical conduct of life involved socio-

logical elements as well as economic, and concerned non-logical as well as logical actions. The economist only considered logical actions (and also all logical actions were economic in nature), and viewed non-logical actions as beyond her brief [Parsons 1937 p.185 ff.]. In order to derive a realistic policy science, the non-rational must also be analysed. In fact, Pareto makes explicit the analytic nature of 'rational economic human'. Economics, he maintained, abstracted from real 'flesh and blood' humans entirely, so that "the individual may disappear; we do not need him any longer to determine economic equilibrium." [Tarascio 1972 p.415] Unlike the later welfare economists, who ironically made such extensive use of the concept of 'Pareto optimum', Pareto himself "removed the facade to expose so-called 'economic man' for what he was — neither economic nor man" [Ibid]. What Pareto wanted was a complete *social* science: a science of all human behavior. He therefore attempted to construct a sociological supplement to his pure economic theory [Tarascio 1967 p.670, cf. Samuels 1974 p.8].

This sociology was to consider the non-logical aspects of action which Pareto believed pervaded human life. He realised that the economy as well as the polity is affected by systems of power [Samuels 1974 p.88]. Economic policy analysis therefore had to go beyond the market [op.cit p.198]. Market choice was only part of economic policy [op. cit p.185]. What Pareto hoped to do was to put sociology on the same footing as economics, and thereby derive a complete policy science.

Unfortunately Pareto's positive sociology never really gets off the ground. He attempts to use the same notion of maximisation and equilibrium simply transported into the social sphere, where they are not operative.²¹ In retrospect, Pareto's work can be seen as another attempt to square the circle of objective social value, based on a notion of transcendental human nature. For the early marginalists, this theory of human nature could be found by a science of 'utility'. For Pareto, an invariable positive science of society is posited. Neither can be put into practice.

²¹ For an outline of Pareto's sociology see Aron [1970]. Pareto's sociology actually appears to be more an exercise in political theory, in which case a positivist philosophy is an inappropriate guide. We shall be considering our own approach to political philosophy in chapter five.

In spite of Pareto's strictures of the need for a sociological supplement to economic theory, the use of indifference analysis and the Paretian conditions for equilibrium were eagerly taken up by the later 'welfare economists' of the inter-war period, as was the idea that distribution is an ethical, not an economic, consideration. However, as we have seen throughout, ethical elements are essential in a value theory, and this theory cannot be separated from its distributional implications. The effect of replacing the neo-classical value theory was to move towards a behavioral- and therefore analytic- definition.

Robbins

This movement towards behavioralism began with the new definition of economics developed by Lionel Robbins [Robbins 1935 (1932)]. This change in definition, which has become common currency throughout the discipline since the 1930's, was couched in positivist terms. All traces of the discipline's utilitarian past were discarded: legitimacy now lay in the claim to 'scientific' truth in positivist terms. Economics was no longer to look at wealth, as the classical economists did, or *material* welfare, as did the neo-classics. Robbins' definition concerned itself with behavior, with 'rational choice'. In fact, Robbins considers not Marshall's 'men of flesh and blood', but rather an abstract, 'rational' human. All that is necessary for a science of behavior, Robbins asserts, is for actors to be able to choose between given alternatives. As long as these choices are transitive (and it would be strange for an *individual* to hold intransitive preferences), then it is possible to show (by the same indifference analysis as used by Pareto) that an equilibrium can occur at which each individual's utility is maximised. Robbins replaced a cardinally measurable utility with an ordinal utility, based on the idea of rational choice.

Robbins's definition (and, as we shall see, much of his substantive analysis) reflects his Austrian background, and is a curious mixture of positivism and Austrian praxeology. Although his theory is couched in behavioral terms, Robbins explicitly

denies that economic theory can be based solely on observational concepts [1935 p.87]. He believes that

After all, our business is to explain certain aspects of conduct. And it is very questionable whether this can be done in terms which involve no psychical element. It is quite certain that whether it be pleasing or no to the desire for the maximum austerity, we do in fact *understand* terms such as choice, indifference, preference and the like in terms of inner experience. [ibid.]

Robbins also specifically denied interpersonal comparisons of utility. Again, this affected the discipline boundary, in that it effectively excluded statements concerning re-distribution [Cooter and Rappoport 1984 p.521]. The move to ordinalism therefore appears to neatly sidestep the distributional conclusions of the diminishing marginal utility of money. However the cost of this move was high.

Ordinal utility theory gives the appearance of allowing us to reach similar conclusions to Marshall, but without the distributional implications attached to the theory.²² It also laid the foundation for a positivist, behavioral economics

Robbins assembled the elements of a new conceptual framework by joining together the scarcity definition of economics, the positivist conception of method, and the ordinalist view of utility. The only piece missing from the modern view was a behaviorist interpretation of ordinal utility, and that was supplied by others. [Cooter and Rappoport 1984 p.523]

Hicks

Hicks and Allen [1934] attempted to redefine Marshall in terms of ordinal utility and behavioral concepts. Hicks denied the legitimacy of any notion of quantitative utility [Hicks 1939 (1946) p.42, cf. Hicks 1975 p.220]. Although accepting the notion of utility, he denied this is a measurable phenomenon in the sense of cardinal measurement. Hicks believed that "the facts of *observable conduct* make a scale of prefer-

²² Although, as we shall see later, ordinal utility and indifference analysis tend towards static theories.

ences capable of theoretical construction ... but they do not enable us to proceed from the scale of preferences to a particular utility function." [Hicks and Allen 1934 p.6 emphasis added]

Hicks claims that ordinal utility can be derived from observed behavior, whereas cardinal utility cannot. Hicks therefore proposes to "undertake a purge, rejecting all concepts which are tainted by quantitative utility, and replacing them ... by concepts which have no such implication" [1946 p.19]. Marginal utility was therefore replaced with marginal rate of substitution (between two goods), diminishing marginal utility by diminishing marginal rate of substitution and so on. Note though that the justification for the move to rational choice was not the same for Hicks as for Robbins. For Robbins, choice was justified by appeal to introspection. For Hicks the justification is that the ordinal ranking can be observed in behavior.

The Hicksian methodology can be viewed as a move towards a more positivist justification of economic 'science', and away from the psychologism of earlier schools.²³ But, as we saw in chapter one, the appeal to introspection was not an optional extra. The use of an abstraction as a definition automatically precluded any empirical input into the science. The Hicksian formulation is no more observational than the old cardinal theory was, since observation is precluded almost by definition. Samuelson believed that the concepts could not be justified without recourse to psychologism: the law of diminishing substitution, for example, is not an observational term [Samuelson 1938, cited Wong 1978 p.49]. 'Indifference' is not an observational term either. The switch to ordinalism cannot be justified on positivist grounds and indeed, as Wong observes, Hicks rather changes tack in later writings:

In Hicks [1939] the rationale for revising Marshall's theory is changed. The revision is not defended on the grounds that an ordinal utility rather than a cardinal utility function can be constructed from the facts of observable conduct, but on the grounds that only an ordinal concept of utility is necessary for

²³ cf. Cooter and Rappoport: "John Hicks and R.G.D. Allen ... supplied the technical basis for a behaviorist account of consumer theory. Their article reconstructed consumer theory by isolating and developing those parts that did not rely on cardinality ... A concept that described mental impulses (marginal utility) was replaced by a behaviorist concept (marginal rate of substitution)." [1984 p.523]

the explanation of consumer behavior:

and he cites Hicks:

Pareto's discovery only opens a door, which we can enter or not as we feel inclined. But from the technical economic viewpoint there are strong reasons for supposing we ought to enter it. The quantitative concept of utility is *not necessary* in order to explain market phenomena. Therefore, on the principle of Occam's razor, it is better to do without it. [Hicks 1939 p.18 cited Wong 1978 p.34, emphasis in Wong]

Hicks has therefore changed the justification for replacing cardinal with ordinal utility from his original claim that it provided a more realistic, observable concept to the weaker (and somewhat trivial) claim that it provides a scientifically neater theory.

Samuelson

The cudgels for observability were taken up by Samuelson. Samuelson's aim was to devise a new theory of consumer behavior which succeeds in "dropping off the last vestiges of utility analysis" [Samuelson 1938 p.62]. He believes that the ordinal utility theory does not go far enough in severing all links with the concept of utility. Samuelson wishes to move towards a stronger positivistic economics — he believes that the theory must be based on purely observational concepts. Samuelson therefore proposes to employ the behavior of consumers as *data*. He purported to show how the same conclusions as in the ordinal utility approach could be reached by observation of the choices made by individuals. Given a price and a budget constraint, we could simply observe the choices made by individuals. Samuelson maintains that rational individuals given the same axioms of choice as in the ordinalist construction, can be observed as "revealing" their preferences in their choices. Given relative prices and a budget constraint, we could simply observe that the consumer has a preference for those choices made over all other viable choices. No recourse to utility need be made.

Does this new construction solve the problem of a policy science? Firstly we would observe that prices and distribution are given. To derive policy these would, it seems, have already to have been settled. One presumes that price would be explained in a manner similar to the Marshallian short run analysis, with the curves drawn from observation. Distribution, however, is not clear.

Indeed it is not clear that Samuelson is providing a new theory, since in order to derive the theory we still are required to accept both transitivity over time (otherwise we have a trivial answer — the consumer chose what s/he preferred) and the other aspects of rationality [Wong 1978 p.58]. As Sen observes, these axioms are in fact identical with those of ordinal utility

Faith in the axioms of revealed preference arises, therefore, not from empirical verification but from the intuitive reasonableness of these axioms interpreted precisely in terms of preference ... if the theory of revealed preference makes sense it does so not because no psychological assumptions are used but because the psychological assumptions used are sensibly chosen [1973 pp.3-4, cited Wong 1978 p.59]

In fact, as Wong notes

Samuelson seems to move towards a non-observational ordinal utility in his derivation of indifference curves. Being indifferent is the opposite to holding a preference. Indifference curve analysis therefore involves assumptions of individual preferences and a limitation of the means to obtaining these preferences. This is already presumed by the ordinal utility approach — Samuelson's theory does not differ substantially [Wong p.73].

Samuelson was correct in his belief that ordinal utility contained non-observational concepts, but wrong in his judgement that these could be avoided. Moreover, the notion of scientific explanation that Samuelson seems to be adopting is very much the strong, Humean conception of regularities. Certainly Samuelson excludes any possibility of causal explanation. It is "not an explanation in the sense that it explains why one bundle [of goods] were bought and all other bundles were not" [Wong 1978 p.61]. This is in accord with the stated methodological position held by Samuelson, that explanation is constituted by descriptions [Samuelson 1964 *passim*].

Samuelson does not provide a description though. Following the ordinal utility theorists, he actually provides a corollary of an abstraction from human behavior. The concept of rationality is never defined in operational terms. Inconsistent behavior is re-defined in terms of changing tastes, the wish for variety or some such. In addition, as Sen showed in his well-known 1976 article, by redefining the boundaries of egoism, any individual can be made to appear self-seeking [Sen 1976 *passim*].

This leads on to a major criticism of the ordinalist school, which is shown up well in the Samuelson formulation. The whole point of utility theory was to derive a theory of *value*. Meek suggests that the elimination of cardinality in favour of ordinality also eliminates value theory [Meek 1956, cited Gramm 1988 p.238]. What we are given instead is a *price* theory. Preferences are just preferences which we can neither judge nor compare. The rationality assumptions have removed any vestige of a maximand. It is no longer clear what the objective of ordinal utility is. Myrdal had earlier criticised Fisher on the same point:

When hedonism is abandoned, utility, subjective value, satisfaction, pleasure and pain, etc., must be defined in terms of observable choice (*Wahlhandeln*). This is done by Irving Fisher. But he, unlike, for example, Cournot and Castel, does not try to give up the subjective theory of value. He abstains from psychology and yet retains a purely formal, behaviorist utility and value theory.

But why retain psychological concepts without psychological content? What is the purpose of an analysis which is intended to prop up the theory of price and which, apart from small improvements and terminological changes, is identical with the old theory? Marginal utility theory proper had at least an objective; it purported to be a psychological explanation of price formation. But what is the point of the new theory of choice which claims to be non-psychological?

Myrdal maintains that economic explanations must of necessity involve a psychological formulation:

The comprehensive modern non-hedonistic value analysis seems to have been constructed in order to replace marginal utility theory. The latter was intended to provide a psychological explanation of price formation. When hedonism became discredited psychological explanation generally suffered a loss of reputation. But the flaw in the marginal utility theory was not that it endeavored to explain economic phenomena psychologically, but that hedonism could not explain them. The new school tries to salvage the hedonistic model by stripping it of its psychological content. Its concepts are formal and 'purely economic'. But its theoretical model is not likely to provide a very happy formulation of the specifically psychological problems of economics, for in so far as it formulates them at all, it does so hedonistically. [Myrdal 1929 p.99]

The ordinalists use of rationality actually abstracts any psychological content from economics: "Rational economic man is not an actual man. He is, rather, any actual man who conforms to the model to be tested." [Hollis and Nell 1975 p.55]

Welfare Economics

The positive economic science has come to the impasse that we discussed in the Introduction. If positivists are to base their theories on conditional observational statements, then we must be able to test when the conditions have held. The ordinalists — and especially Samuelson — cannot do this. The concept of rationality is being used both as what Musgrave earlier referred to as a heuristic assumption and also as a domain assumption. All the behavioral elements have been assumed away in the strictures of rationality. Indeed the professed belief in the justification of theories through observation was completely at odds with the welfare economics movement post-1930. This — as Hutchison complained — saw a veritable plethora of abstractions and unrealistic assumptions.

These become compounded when we try to move from the individual to the social utility. The essential methodological problem of orthodox economics, it will be recalled, was to derive a social theory from an individualist basis, if it is to develop an alternative to planning. Marshall attempted this by the concept of consumers' surplus, measured in monetary units.²⁴ Since this was a cardinal measure, the social 'well-being' could be derived by simply aggregating the surpluses. Obviously by definition ordinal utility could not be aggregated in this way. By the late 1930's most welfare economists were moving towards Pareto's scheme of general equilibrium, using the 'Pareto criterion' of optimality, but employing indifference analysis as a derivation of ordinal utility.²⁵ Welfare economics became a study of the conditions

²⁴ Hicks attempted to reconstruct the theory of consumer's surplus in his article. For a critique see Samuelson [1947].

²⁵ Recall that Pareto took his indifference curves simply as data. "As we have seen, by the time he wrote the *Manuel*, Pareto had adopted Edgeworth's indifference curve techniques. However unlike Edgeworth he did not derive the curves from an existent cardinal utility function. On the contrary, he worked his way from indifference maps to the preference function. In this manner

under which a Pareto-efficient allocation is possible.

What was originally mooted by Pareto as an example of the *limitations* of pure economics was now to be used as an *objective* of economic theory. The entire thrust of Pareto's thought was based on the idea that social policy could not be derived from purely economic analysis — Pareto believed a sociological supplement was required. It will be recalled that Pareto's economics was limited to the presentation of "uniformities". He explicitly eschewed explanation or practical usefulness — it was a purely synthetic exercise. Yet the ordinalists are embracing Pareto's exposition of the limit as a major tool for positive economics. The discipline consequently became more and more abstract and unrealistic.

This can be immediately seen in the conclusions concerning social utility which were arrived at. Social utility is only maximised under conditions of what were called 'perfect competition'. This concept was considerably different to Marshall's 'free competition' in that the axioms were impossibly strong. Perfect competition involved first and foremost a very large number of buyers and sellers in the market, such that no one seller or buyer can influence price. All economic agents are therefore price-takers. Further to this, there must be free entry to the market for suppliers and perfect knowledge of all present and future market conditions. All commodities must be divisible, and the product of the market homogeneous. Furthermore, government interference with the market (tariffs, subsidies and so on) are presumed not to occur.²⁶

Clearly, these assumptions do not hold in any real-world market. The theory is synthetic and vacuous and unlike the Marshallian analysis totally static in nature:

The analysis is about a stationary economy in which everything exactly repeats itself from one period to another. In such a society all decisions are made only once; once made they are relevant for all time because the things ... decided upon ... remain constant for all time. Everything is assumed to take place at a point in time — no time elapses between one event and another; hence there is no future to be uncertain about. In such a timeless world, though there is no uncertainty about the future, there

he did away with the need of considering any measurability of utility and interpersonal comparisons of utility as well." [Cirillo 1979 p.100]

²⁶ For a full list of the conditions for Pareto-efficiency see Nash [1969 p.28]

might yet be imperfect knowledge about one another's actions at the present time; but this, too, is assumed away by supposing there is perfect knowledge [Nath 1969 p.11].

Furthermore, unlike the partial equilibrium approach of Marshall, the general equilibrium approach of Walras is not only static but instantaneous. The optimum conditions must obtain simultaneously for *all* markets. It is not the case that the optimal conditions are a goal towards which we may strive: the analysis of Lipsey and Lancaster showed that it is 'all or nothing' as it were. If there is a departure from the optimal conditions in one part of the economy, the next best ('second-best') situation may involve further departures elsewhere. For example, if we have a large monopoly in one industry, it would not be possible to attain a Pareto-optimum: it is possible to make everyone better off. The second-best situation may involve even further imperfections — say for the sake of argument a monopsonist trade union. The second-best situation is not attained by conditions as close as possible to the ideal:

... there is no *a priori* way to judge as between various situations in which some of the Paretian optimum conditions are fulfilled while others are not. Specifically, it is *not* true that a situation in which more, but not all, of the optimum conditions are fulfilled is necessarily, or even likely to be, superior to a situation in which fewer are fulfilled. [Lipsey and Lancaster 1956 p.144]

But even if we suspend disbelief, and accept the assumptions of perfect competition, it is still not clear that we can derive social value conclusions from this analysis. Recall that the ordinalists are attempting to obtain a maximum social utility from individual utilities. Since these individual utilities are held to be non-comparable, and since distribution is not considered, the best that the ordinal economists can achieve is to consider situations where unanimity occurs: where any change in the economy lessens someone's welfare. This is clearly a maximum: the position is clear-cut. It was soon realised that it would be impossibly restrictive to limit economic analysis to these cases — indeed as we have seen, the conditions were never meant to be applied. Kaldor's solution for extending the scope of what became known as the 'New Welfare Economics', was based around the idea that it would

always be possible for the gainers to overcompensate the losers in any situation where total welfare is increased [Kaldor 1939]. Given this possibility of compensation, the economist need not be limited to changes where no-one is worse off. Kaldor did not believe it to be the task of the economist to suggest that compensation should actually take place. This is a question of distribution, which economists were forbidden to answer. As long as compensation *could* be paid, an improvement could be said to exist. The question of who should actually obtain the fruits of this improvement is, it seems, to be left hanging in the air.

Let us briefly recap the argument, before showing how the position we have arrived at leads to a complete impasse. The use of ordinal utility, originally mooted as an improvement in 'scientific' method, was in actuality no more scientific than the old cardinal utility theory, although it was considerably more restrictive. The need for perfect competition to obtain maximum social welfare meant that economists could not incorporate any form of market power or state intervention into their scheme. The ideological backdrop to the adoption of ordinalism was the need to avoid questions of objective 'well-being' and of distribution. Yet as we have seen, the compensation principle upon which the new welfare economics relied could not entirely distance itself from the question of distribution. This anomaly was soon to be picked up by Scitovsky, although it appears, as usual, in a technical guise.

Scitovsky criticises the compensation principle for containing an implied distributional bias in favour of the status quo [1941 p.393]. All that is being proposed by Kaldor, Scitovsky points out, is that the redistributive effects of a change should be corrected. Scitovsky then shows that this leads to a paradox. Consider a change (say in the relative prices of various commodities) which leads to an improvement in some people's welfare but leaves others worse off. Let us call the old position A and the new one B. The compensation principle states that a change from A to B is only a social improvement if it is possible for gainers to compensate the losers and still gain. Let us assume this is the case. B is therefore better than A, in Kaldor's sense. So far so good.

Now, Scitovsky argues, consider reversing this change, moving from B back to A. If it is possible for the original losers to give compensation back to the original gainers to compensate them for 'loss' they are now incurring — so that we arrive back at the original starting point — then, following Kaldor, A is better than B! We have a contradictory result, because we cannot choose any "starting point". Moving from A to B is an improvement, and moving back from B to A is also an improvement, since we cannot make any judgement about the respective distributions.²⁷ We cannot say which way compensation should go — compensation to the losers from change or bribes to the would-be gainers to maintain the status quo — and recall that we are still only considering the *possibility* of compensation!

Scitovsky suggests that the economist must abandon the idea of deriving policy conclusions without considerations of social justice [op.cit p.393]. This appears also to be Little's position. Neither appears to be able to bring themselves to suggest the abandonment of the scheme. Scitovsky decides to simply add one more qualification to the already long list to obtain a social optimum from the market mechanism: we can only make welfare judgements if two-way compensation is impossible. Quite how this is to be operationalised is unclear.

Given the problems of this analysis another method of proceeding was advocated. It was suggested by Samuelson and others that we should posit directly a social welfare function, which would include a variety of ethical judgements from outside economics [Samuelson 1950]. It is for this reason that he is apparently happy to forgo any notion of welfare in his economics — this notion is to be 'imported' into economics, as it were.

In a sense we have come full circle, for we began by looking at the idea of measuring ethics, and of deriving social policy from them, and we have traced two centuries' worth of efforts to put this into effect. We should not therefore be surprised to find that the ethical judgements which can be so imported are, as it turns out,

²⁷ This was, of course, the distinctive element in the Pigouvian analysis. Pigou was happy to consider distribution, up to a point.

remarkably restrictive — in fact, they must amount to a dictatorship over economic choice in order for the social function to be constructed. Arrow [1950] showed that even if *individual* preferences are defined as transitive by definition of rationality, the resultant social preferences need not be — unless, of course these 'preferences' are imposed. The social utility function cannot be derived.

Sen suggests that it is the "informational restrictions" of welfare economics that cause the impossibility of transitivity. Sen argues that this "can be seen as resulting from combining a version of welfarism ruling out the use of non-utility information with making the utility information remarkably poor (particularly in ruling out interpersonal utility functions)" [Sen 1979 p.539].

We have seen, then, that the move to ordinal economics, even though it is unjustified and results in a synthetic theory, fails because it cannot derive a social value theory which is independent of social distribution. The game is simply not worth the candle. We have no theory of social welfare or even any realistic theory of individual value. We have lost any idea of 'well-being'. In fact, as Sen points out, it is impossible within the framework of welfare economics to even distinguish a rich person from a poor one!

Can we identify the rich through the observation that they have more utility than the poor? Not in the Arrow framework, since interpersonal comparisons are not admitted. Perhaps as those with a lower marginal utility of income? No, of course not, since that will go against *both* noncomparability and ordinalism. Can we then distinguish the rich as those who happen to have more income, or more consumer goods (nothing about utility need be said), and bring this recognition to bear in social judgement? No, not that either, since this will go against welfarism ... since this discrimination has to be based on non-utility information.

Sen believes that this is connected with the Arrow impossibility theorem

'Social utility', in Leonnef's characterisation, as 'a function of utility levels', without interpersonal comparisons robs us of our ability to 'tell' effectively the rich from the poor. It is this peculiarity of traditional welfare economics in insisting on both that social judgements be based on utility information *and* that the utility information be used in a particularly poor form, that can be seen as paving the way to inconsistency or incompleteness — and thus to impossibilities. [Sen 1979 p.544]

The move to ordinal utility and 'rational economic human' results in an abstract theory devoid of practical application. This militates against the very purpose of positive social science. We argued in chapter one that a policy science is justified by the possibility that it will yield instrumental knowledge. Contemporary ordinal economics precludes precisely this knowledge.

The fundamental problem of orthodox economics has not been solved. In order to arrive at a positivist social policy either we must show some psychological knowledge of human desires, which was the aim of the utilitarians, or we must investigate objective, empirical activity, as with the neo-classics. The former was simply unoperational, but the latter led to the social policy of planning that is inherent in the positivist approach. Since the positivist approach is pointless unless social policy can be derived, it appears that we must either surrender the policy of *laissez-faire* and embrace positivist planning, or ditch positivism if *laissez-faire* is to be maintained. We will consider both options in the next chapter.

CHAPTER 3

1930's Market Socialism

THE INABILITY to obtain a positivist subjective economic theory leaves us with two choices. We can either remain true to the positivist philosophy and accept the possibility of economic planning, or we can maintain a subjective theory at the cost of abandoning our claim to positivist science. In this chapter we will look at the 'English debate' on Market Socialism which, although the protagonists did not always realise it at the time, was directly concerned with this question. The 1930's market socialists attempted to base a socialist theory on the models of welfare economics we outlined in the last chapter.¹ This position was criticised by the Austrian school, who dismissed not only the socialist models, but also their positivist, orthodox basis. While their critique appears cogent, the move from positivist to normative theory carries with it the condition that an account be made of the political elements in the theory, and the attempts of the Austrians to abstract from political and economic power renders their substantive theory no more plausible than the orthodox economists'.

I. MARKET SOCIALISM

It will be recalled that the argument of the previous chapter was that the abstraction of distribution was a response to the distributional implications of the early marginalist and the neo-classical analysis. Of course the result of arguing for the removal of distributional questions from economics is that economists cannot evaluate the different distributional schemes put forward by different political persuasions. Furthermore, welfare economists appear to be reliant upon the polity to derive such a scheme, since they have no distributional theory of their own. This abstraction of

¹ Although the early market socialists were also influenced by Marshall.

distribution — which we suggested was behind the move to ordinalism — also led to the problem of deriving a maximum social welfare from individual ordinal functions. We saw several authors — notably Samuelson and Bergson — suggesting that we consider directly a social welfare function. Again, if we are dealing with this function directly, and it is not derived from individual behavior, it is not clear why a market process is required for production and distribution. In principle, it would be possible to directly meet the requirements of the social function. Even if we concede the marginalist school may have established that a social maximum can be derived from individual actions in the market, this does not in itself show that a similar feat cannot be achieved through planning. This demonstration was attempted by Pierson at the turn of the century. The main point of his 1902 article was that a planned economy would still require a monetary valuation of goods and services. Only if the state can supply everything in plenty will this need disappear.

As long as the communist state can supply each person with what he wants, no trading will arise...but when this is no longer the case trading is inevitable [1902 p.75].

Trading will inevitably involve markets and pecuniary evaluation — Pierson's 'Problem of Value'. He also maintains that an income distribution which is independent of labour productivity is inconsistent with free choice of labour. "If the state is to provide us with all our needs, then it must dispose of all labour at its own discretion, otherwise its task will be impossible. It must be in a position to place us where our work is required and it must not be so far influenced by our wishes that they interfere with its plans" [p.48].

If, however, we are to distribute income according to productivity, then monetary calculation is, Pierson believed essential [p.80 ff.].

This analysis was reinforced in the orthodox literature by Barone.² Barone was a follower of Pareto, and his 1908 article was an investigation, on general equilibrium

² Pareto also made some contributions to this question, although as we have seen his work is equivocal.

lines, of the possibility of calculating the various coefficients of production in a collectivist state. Barone concludes that such a valuation would be impossibly difficult, and would involve solving a vast number of simultaneous equations [qv. Hoff 1938 p.222].

Both these articles remained relatively obscure until Hayek's reproduction of them in 1935. The author whose work did succeed in provoking a response was Von Mises, not least because of his polemical and mocking style:

When Marxism solemnly forbids its adherents to concern themselves with economic problems beyond the expropriation of the expropriators, it adopts no new principle, since the Utopians throughout their descriptions have also neglected all economic considerations, and concentrated attention solely upon painting lurid pictures of existing conditions and glowing pictures of that golden age which is the natural consequence of the New Dispensation [1920 p.88].

We will deal more fully with the work of Von Mises shortly. Mises critique of 'physical unit' or non-pecuniary planning, and his criticism of the lack of economic analysis in Marxism, led socialists to attempt to rectify this. The 'English debate' centred around the possibility of retaining pecuniary measurement of goods and the use of money and market within a socialist state; the impossibility of non-market socialism was conceded. Indeed, even Lange's ironic praise of Mises ('a statue of Professor Mises ought to occupy an honourable place in the great hall of the ministry of socialization ...') did contain the admission that "it was his powerful challenge that forced the socialists to recognise the importance of an adequate system of economic accounting to guide the allocation of resources in a socialist economy" [1938 p.57].

Lange believes that the Marxist theory is insufficient by itself, although it remains a necessary component of political economy

The Marxists claim to superiority for his economics is that 'bourgeois' economics has utterly failed to explain the fundamental tendencies of the development of the capitalist system

which are the increase in the scale of production leading to monopolies, intervention and economic imperialism, economic instability and a revolt against the economic system, and so on [1934 p.190]. However Lange believed the Marxian theory was insufficient

But this superiority of Marxian economics is only a partial one. There are some problems before which Marxian economics is quite powerless, while "bourgeois" economics solves them easily. What can Marxian economics say about monopoly prices? What has it to say on the fundamental problems of monetary and credit theory? ... And (irony of fate!) what can Marxian economics contribute to the problem of the optimum distribution of productive resources in a socialist economy

The point for Lange is that the two theories are operative in complimentary 'spheres':

Clearly the relative merits of Marxian economics and of "bourgeois" economic theory belong to different "ranges." Marxian economics can work the economic evolution of capitalist society into a consistent theory from which its necessity is deduced, while "bourgeois" economists get no further than mere historical descriptions. On the other hand, "bourgeois" economics is able to grasp the phenomena of the everyday life of a capitalist economy in a manner far superior to anything the Marxists can produce [ibid. p.191].

The two sciences are valid for different domains. Marxian theory provides an historical explanation of the development of the political economy, whereas 'bourgeois' economics provides a useful tool for everyday economic administration. There is, Lange suggests, a functional difference between the two theories:

This difference is connected, of course, with the respective social functions of 'bourgeois' and Marxian economics. The first has to provide a scientific basis for rational measures to be taken in the current administration of the capitalist economy ... the social function of the latter has been to provide a scientific basis for long range anticipations guiding the rational activity of a revolutionary movement against the very institutional foundation of the capitalist system.

Lange believed that the concepts of neo-classical theory would be of great use in the socialist economy

But in providing a scientific basis for the current administration of the capitalist economy 'bourgeois' economics has developed a theory of equilibrium which can also serve as a basis for the current administration of a socialist economy. It is obvious that Marshallian economics offers more for the

current administration of the economic system of Soviet Russia than Marxian economics does, though the latter is surely the more effective basis for anticipating the future of capitalism.

and in this respect

Modern economic theory, in spite of its undoubted 'bourgeois' origin, has a universal significance [p.191]

The response to the critique of Mises was to attempt to develop market-based socialist models along neo-classical/welfare economics lines.³ Indeed, one of the first contributions to the debate over market socialism — Fred Taylor's American Economic Association presidential speech [Taylor 1929] — was concerned primarily with a justification of consumer's sovereignty. Following the methodological stance of the marginalists, Taylor abstracts the question of distribution from the domain of the economic. Taylor believes that distribution under private enterprise is entirely an institutional matter.

First, on the basis of a vast complex of institutions, customs and laws, the citizen adopts a line of conduct which provides him with a money income of greater or lesser volume [1929 p.42].

The income is then used to demand goods from the producers, who "promptly submit to the dictation of the citizen in this matter" [ibid.].

Taylor proposes to mirror this system in a socialist state. He begins with the assumption that

the authorities of such a state would have honestly and earnestly endeavored to fix incomes so that they represent that distribution ... which is called for in the interest of citizens generally and of the group as an organic whole.

³ Bradley [1981 p.23] and Vaughn [1980 p.542] both note what Bradley terms a "retreat from a pure advocacy of Marxist socialism to a compromise watered down with 'competitive' institutions". In fact this was an explicit admission by Lange.

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³ Bradley (1981 p.23) and Vaughn (1980 p.542) both note what Bradley terms a "retreat from a pure advocacy of Marxist socialism to a compromise watered down with 'competitive' infusions". In fact this was an explicit admission by Lange.

This being the case, the state should submit to their claims in the market:

This socially correct system of incomes being assumed, it necessarily follows that the judgements reached by citizens with respect to the relative importance of different commodities would be virtually social judgements, and the resulting commodity prices would be prices which expressed the social importances of commodities [p.44].

Taylor then goes on to establish the optimal pricing level. Recall that the state is required to follow consumer's sovereignty. Taylor therefore suggests that "A single consideration is decisive. That price which equals resource-cost is the only price which would be consistent with the income system supposed to have been already decided upon" [p.49].

The key factor is therefore to determine the cost of resources. This involves a measurement of the value 'imputed' to factors. Taylor believed that this could be achieved by constructing factor-valuation tables based on initial estimates. The key point is the feedback mechanism offered by market pricing. If the estimate is in error, either surpluses or shortages of the factor will result [p.52-54].

Taylor's article was to form the basis of many of the models of the early market socialists.⁴ Dickinson's 1933 scheme was similar in many respects. He also accepted that consumer goods would be priced on free markets, and proposed the use of marketing boards to distribute goods through markets. But Dickinson additionally maintained that it would be possible to derive the various pricing and output coefficients directly

...once the system has got going it will probably be unnecessary to create in this way within the framework of the socialist community a sort of working model of capitalist productions. It would be possible to deal with the problems mathematically, on the basis of the full statistical information that would be at the disposal of the supreme Economic council [1933 p.242].

The plausibility of this scheme was bolstered by Dickinson's belief that "under

⁴ Useful summaries of the early market socialist models can be found in Vaughn [1980] and Vaughn [1981].

capitalism, demand schedules are apt to exist in the realm of faith" [p.240]. It is possibly for this reason that Dickinson was content to attempt the same problem that Barone despaired of. He did have second thoughts on this mathematical solution though, and abandoned the idea in his 1939 book *The Economics of Socialism*. By then the debate had reverted to the 'trial and error' method outlined by Taylor.⁵ Taylor's method was adopted by Lange when he re-joined the debate in 1936, in reply to Hayek's intervention [Hayek 1935a and 1935b].

Lange believed that the trial and error method overcame the problems of computation of equations raised by Hayek [1935b] and also by Robbins [1933].⁶

Neither would the Central Planning Board have to solve ... millions ... of equations. The only 'equations' which would have to be 'solved' would be those of the consumers and the managers of production. These are exactly the same 'equations' which are 'solved' in the present economic system and the persons who do the 'solving' are the same also. Consumers 'solve' them by spending their income so as to get out of it the maximum total utility; and the managers of production 'solve' them by a method of trial and error, making (or imagining) small variations *at the margin*, as Marshall used to say, and watching what effect those variations have either on the total utility or on the cost of production.

...there is not the slightest reason why a trial and error procedure, similar to that in a competitive market, could not work in a socialist economy to determine the accounting prices of capital goods and of the productive resources in public ownership.

Lange maintained that the feedback mechanism was sufficient to enable equilibrium prices to be determined.

Any mistake made by the Central Planning Board in fixing prices would announce itself in a very objective way — by a physical shortage or surplus of the commodity or resource in question and would have to be corrected in order to keep production running smoothly. As there is generally only one set of prices which satisfies the objective equilibrium condition, both the prices of products and costs are uniquely determined [1938 p.82].

Furthermore, Lange asserted that price did not need a free market to be operative. All that was required was "terms on which alternatives are offered". Given a preference

⁵ Although the similarity of the two schemes was pointed out by Hayek [1935b], Dickinson actually mentioned the problem of 'linked' prices requiring a general equilibrium approach himself, but did not draw out the corollaries of this [Dickinson 1933 p.240].

⁶ Although this was something of a red herring, as we shall see presently.

scale and a quantity of resources, the economic problem of choice was soluble. Lange also believed that this trial and error method is analogous to the functioning of the market under capitalism, that "The Central Planning Board performs the functions of the market" [ibid.] and even claimed that the method is the same as that outlined by Walras [Lange 1938 p.70].

Lange attempted to construct a working model of pure competition by instigating managerial rules for productive plants. The first instruction was for the managers to produce at lowest average cost. The second was to alter output so that marginal cost equals price. This last rule also applied to the industry as a whole [1938 p.76]. This is only desirable, though, if the managers act as price-takers — in Lange's terms the "parametric function of prices" must be retained. Only if prices are given would the managerial rules be sufficient, because only then would the perfectly competitive market results be imitated [p.80-81].

Lange noted that producing at lowest average cost leads to the marginal productivity of factors — measured in money — being equal. On this basis Durbin suggested an alternative set of rules based on what he claimed was a blend of Marshallian and Pigovian analysis. He believed that the solution to production coefficients should be found by equating the marginal products of all factors [1938 p.141-62]. Durbin was not satisfied with the exclusive use of this rule though, since the marginal productivities would only be estimates [p.143]. He therefore includes a managerial rule based on Marshallian analysis — that at the point of equilibrium, the average cost will be at its minimum. At this point marginal costs equal average costs. Durbin's rules were to equate marginal productivity and also minimal average cost with price [p.144 ff.].

Durbin's analysis was designed to provide a more realistic set of policy proposals than the equation-solving system of Dickerson [p.140]. However, the entire idea of providing managerial rules was questioned by Lerner, perhaps because of his familiarity with Austrian theory [Bradley 1981 p.26]. Lerner views it as a mistake to attempt to recreate perfect competition, but believes that instead we should provide

an analysis of how to achieve the same results. He begins his 1937 article by stating

This article is in the main a protest against the developing tradition, in approaching the problems of socialist economies, of starting from the consideration of competitive equilibrium, instead of going direct to the more fundamental principle of marginal opportunity cost. This approach is not only subject to methodological criticism as indirect and cumbersome, but it is a fertile source of actual error deriving from unrealised implications of the static nature of competitive equilibrium [1937 p.253].

That is to say, we can arrive at a simple conclusion:

Price must be equal to marginal cost. This is the contribution that pure economic theory has to make in the building up of a socialist economy [1937, p.270].

The significance of Lerner's intervention is to attempt to include the notion of opportunity costs into the socialist models — the marginal costs were marginal *opportunity* costs [p.254]. The concept of opportunity cost is designed to introduce a dynamic element into the static model: his costs rule includes long-term costs. Lerner criticised the solutions of Lange and Durbin for focusing on short-term costs.

II. AUSTRIAN CRITIQUE

Although these concepts of opportunity cost and dynamics are crucial for the debate, the fact is that these have different meanings for the various protagonists. The Austrian Theory was radically different to the neo-classical framework within which the market socialists were working, and the Austrian critique was aimed as much at neo-classical theory as at the market socialist use of this theory. The difference in the two approaches was not grasped by the socialists and the two sides talked past each other, so to speak, as a result.

The confusion was not helped by some rather loose terminology on the part of the Austrians, in particular Mises' claim that "as soon as one gives up the conception of a freely established monetary price for goods of a higher order, rational production

becomes completely impossible." [Mises 1920 p.104] Mises' point was not that production would be impossible under socialism, but rather that such production would not be — in Mises' terminology — rational. Nonetheless, Lange and others believed that they — and also the solutions of Pareto and Barone — had successfully refuted Mises' claim, and that Hayek's subsequent defense of Mises' argument signalled a "retreat" from the claim that allocation under socialism is *impossible* to a position that such allocation is *impractical* [Lange 1938 p.62]. But the whole point of the Austrian critique was that the General Equilibrium models of Pareto and the neo-classical works of Marshall and Pigou — while they may be logically valid — were an irrelevance to real-world economic activity. The market socialists were criticised for using an abstract model as a blueprint for an actual working economy [Vaughn 1980 p.543].

In order to understand the Austrian critique of neo-classical and general equilibrium theory, it may be helpful to examine the differences in the use of economic concepts. It will be recalled from the last chapter that it was Lionel Robbins, described by Vaughn as "at least partially in the Austrian tradition", who moved the definition of economics from an objective to a subjectivist basis with his 'choice' delimitation. Unfortunately, this definition only succeeded in grafting Austrian concepts into neo-classical analysis, resulting in bastardised versions of the Austrian concepts. Of particular importance to the present argument is the notion of costs. The point of the Austrian concept of opportunity cost is that it is entirely subjective, and can only be known by the individuals concerned. It cannot be measured in terms of money outlays, or by any other objective measurement [Barry 1984, p.37]. The Austrians would be contemptuous of the assumption of 'perfect knowledge' used by general equilibrium economists as a condition for market equilibrium. The whole problem is understanding entrepreneurial behavior with imperfect knowledge [cf. Barry 1984*passim*]. Indeed, the Austrians were also critical of the orthodox concept of equilibrium. Mises maintained that equilibrium is a stationary condition, and that "To assume stationary economic conditions is a theoretical expedient and not an

attempt to describe reality" [1922 p.163].

The Austrians were dismissive of the entire welfare economics/general equilibrium structure precisely because they viewed it as static and unreal. For the Austrians, the central problem for economics was the analysis of dynamic activity through time — the analysis of economic change [Lavoie 1985 p.65]. Mises stated that "the problem of economic calculation is of economic dynamics; it is no problem of economic statics". That is "The problem of economic calculation is a problem which arises in an economy which is perpetually subject to change, an economy which every day is confronted with new problems which have to be solved" [1922 p.139]. Under perfect competition there would not be any economic problem, since the assumption of perfect knowledge assumes complete efficiency [Bradley 1981 p.27]. In fact, for Hayek, the theory of perfect 'competition' presumes that no competition as such can ensue

what the theory of perfect competition discusses has little claim to be called "competition" at all and ...its conclusions are of little use as guides to policy.

This occurs, Hayek believes, because the current theory begs the questions set by the older theories, as a result of which

if the state of affairs assumed by the theory of perfect competition ever existed, it would not only deprive of their scope all the activities which the verb "to compete" describes but would make them virtually impossible. [Hayek 1946 p.92]

The key concepts of the Austrian theory were competition and entrepreneurship. The stationary equilibrium cannot consider these concepts, because it is precisely entrepreneurship and competition that cause economic change. The market is "not merely an allocative device, by which factors are assigned to their most important uses, it is a discovery procedure through which economic agents try out new techniques, experiment with different uses of resources and exploit new opportunities" [Barry 1984 p.42].

So, far from accepting general equilibrium as a normal condition, the Austrians believed such a situation to be impossible [Lavoie 1985 p.65-108]. What they were concerned with was the tendency towards equilibrium — the 'equilibrating' process [Bradly 1981 p.26]. While Marshall's 'market clearing' price may be reached, the price set for general equilibrium will not be attained [Lavoie 1985 p.109].

The Austrian critique was not that the market socialists employ general equilibrium theory incorrectly, but that this theory is itself redundant [Barry 1984 p.54]. Any model based on general equilibrium/welfare economics would be rejected by the Austrians. Although Mises' 1920 article dealt largely with a moneyless economy, his writings do contain enough to suggest a critical attitude.

Mises believed that a moneyless economy would only be possible under primitive exchange conditions [Lavoie 1985, p.60]. He thought that exchange and markets would soon begin to reappear for consumer goods, even if these were originally rationed:

The beer-drinker will gladly dispose of non-alcoholic drinks allotted to him, if he can get more beer in exchange, whilst the restorator will be ready to give up his portion of drink if he can get other goods for it.

Furthermore money could even re-appear:

The principle of exchange can thus operate freely in a socialist state within the narrow limits permitted. It need not always develop in the form of direct exchanges. The same grounds which have always existed for the building-up of indirect exchange will continue in a socialist state, to place advantages in the way of those who indulge in it. It follows that the socialist state will thus also afford room for the use of a universal medium of exchange — that is, of money [Mises 1920 p.91-92].

Mises did not view this development as a particular problem for consumer goods. The real problem was in capital goods. Mises claimed that these would therefore be impossible to value, since they are not subject to exchange. That is to say, since the value of factors is unknown, costs cannot be calculated — at least not in any common unit of money. [Vaughn 1980 p.539]. This would mean that "any economic

system of calculation would become absolutely impossible" [Mises 1920 p.109].

The whole problem for Mises is that equilibrium values are unknown. This results in a constant flux of economic action, and in the necessity to *anticipate* events in an uncertain future. This interpretation of present data, and the anticipation of future events, is a function performed by the entrepreneur in Mises' theory. He believed that this function must be performed in all economies [Murrell 1983 p.95]. Of course, the static equilibrium model removes this function.

The entrepreneur "anticipates what future conditions hold and is penalised or rewarded in his quest for profits". It is the reward for "alertness" to consumer requirements that is the relevant function for the Austrians and is the dynamic equivalent of the 'trial and error' process. It is this constant need to change and adjust production decisions that is at the heart of the Austrian dynamic theory. [Bradley 1981 p.30]. It was from this that Mises' distributional theory is derived. Mises' believed that the entrepreneurial ability that was such a vital function of the economy was unevenly distributed among the population. What is important for Mises is that the best anticipators are brought to the fore. The market performs this task by the profit or loss obtained in production and exchange. There were two aspects to this. The first was that the selection process required an incentive, since the production process involved risk. But just as important was the fact that the incentives — the process of reward — placed capital resources with those who were best able to make use of them. The market acts as a filter for processing entrepreneurial ability, only allowing those with proven ability to retain access to capital funds. The point of the market mechanism is not to obtain the objective equilibrium conditions, for such objective knowledge is impossible, but rather to select those whose speculative abilities are most developed, and to provide incentive and resources to encourage their speculation [Murrell 1983 pp.95-96].

Mises maintains that this lack of an incentive scheme under socialism is detrimental

In an economic system based upon private ownership of the means of production, the speculator is interested in the result of his speculations in the highest possible degree. If it succeeds ... it is *his* gain. If it fails, then, *he* is the first to feel the loss. The speculator works for the community, but he himself feels the success or failure of his action proportionately more than the community. As profit or loss, they appear much greater in proportion to his means than to the total resources of society

whereas

under socialism it is quite different. Here the leader of industry is interested in profit and loss only so far as he participates in them as a citizen — one among millions. On his actions depends the fate of all. He can lead the nation to riches. He can just as well lead it to poverty and want (Mises 1922 p.206).

There are two elements to this argument. The first is that the market system gives an incentive to follow the society's wishes. The second is that the risk — although magnified — is also localised. Only the entrepreneur's speculation stake is at risk. Under Socialism, Mises believed, neither aspects held. The managers of the state enterprises had no incentive to follow the demands of the market, while at the same time the effects of their decision were universal.

There is another, subtler, argument raised by the Austrians concerning the state ownership of production. This concerns a possible socialist defence against the Austrian charge that the businessman must obtain an incentive to follow the market, and that this would necessarily involve personal risk. It could be suggested that the separation of ownership from management in most modern corporations tends to give this notion the lie — few of the decision makers in modern companies are risking their own capital. If this is the case, it is not clear why the same managers could not simply be employed to manage state corporations. It is due to this separation that the Austrians place great emphasis on capital markets ... it is the capital markets which keep private managers in line ... the owners of private capital can shift resources from unprofitable to more profitable ventures and thus put poor managers out of a job. Where profit or loss no longer serves as an objective test of managerial success, as it likely would not under socialism, it becomes exceedingly difficult to weed out inefficient managers." [Vaughn, 1980 p.548] Hayek drew on this analysis later.⁷

⁷ Halm was also concerned that the 'rate of accumulation' would be entirely arbitrary, since

We can see therefore that Mises would certainly reject any model based on static equilibrium, since this precludes the consideration of economic change. As Lavoie remarks

Neoclassical equilibrium models, of which the perfect competition model is one variety, are attempts to include all alternatives in a given framework — that is, they presuppose a world of Robbinsian optimisers who have preset adjustment ready to meet any change that is anticipated under a given framework. There are no genuinely new ideas, no alertness to unimagined alternatives — in short, no genuine changes. [Lavoie 1985 p.107]

Yet this is precisely the model that Lange *et al* are proposing [Murrell 1983 p.97].

There are never any changes considered in these models:

When Lange explains the "trial and error" technique, one is immediately impressed with the banishment of change in his system. Such statements as "so the process goes on until the objective equilibrium is satisfied and equilibrium finally reached" remind one more of natural scientists in the laboratory than applied economists checking inventory. [Bradley 1981 p.28]

All these elements of Austrian economics were misinterpreted in the 1930's debate. The interpretation of the claim by Mises that socialism was "impossible" was misunderstood by a market socialist school derived from a general equilibrium tradition. In fact it seemed to Lange that Mises was almost advocating an institutional view of economic action [1938 p.62], presumably since it appeared that the general equilibrium logic only applied under capitalism and not under socialism. Of course, Mises' claim was that the logic of general equilibrium was inapplicable to any economy.

The confusion was probably not helped by the inclusion in Hayek's collection of critical articles [Hayek 1935] of the essay by Enrico Barone, *The Ministry of Production in the Collectivist State* [1908]. This essay, it will be recalled, was in the Paretian mould, and showed that a calculation of the coefficients of production in a socialist

any interest rate would be arbitrary. There is no non-arbitrary method to determine the level of industrial investments [Halm 1935 p.161 ff.]. This affected the possibility of developing new production techniques so as to lower costs. See the argument later.

state would require the calculation of a vast number of simultaneous equations. The point of its inclusion was presumably to substantiate the claim that the operation of the Dickinson/Taylor models was wildly impractical. The decision to include the essay was unfortunate in two respects. Firstly, far from accepting the impossibility of a planned economy, the market socialists actually cited the Barone article as a proof of the possibility of the functioning of a socialist economy. Related to this, the article also caused attention to be diverted into what Lavoie calls the 'computation' debate, a debate over the possibility of computing the vast quantity of data [p.90]. This was only a minor part of the problem, Lavoie suggests. The main point of both Mises and Hayek was the 'calculation' argument — that the data to be input into the equations is not available [Vaughn 1980 p.545]. That is

to at least Mises and Hayek, if not also Robbins, the problem was *formulating* the equations — not solving them. In a world of complexity and continuous change, the central planners would lack the knowledge of the coefficients that go into the equations [Lavoie 1985 p.91].

Hayek views the economic problem as "not merely a problem of how to allocate 'given' resources" but thought that

It is rather a problem of how to secure the best use of resources known to any of the members of society, for ends whose relative importance only these individuals know. Or, to put it briefly, it is a problem of the utilization of knowledge which is not given to anyone in its totality. [Hayek 1945 p.78]

The economic problem, for Hayek, was how to co-ordinate the information about economic variables which was dispersed among individuals. He regarded the price system as "a mechanism for communicating information" between individuals, which therefore enables social co-ordination: "in a system in which the knowledge of the relevant points is dispersed among many people, prices can act to co-ordinate the separate actions of different people in the same way as subjective values help the individual to co-ordinate the parts of his plan" [p.85].

The reason that the socialist planners cannot draw up their equations is that the

economic data available to any individual is always incomplete. "The peculiar character of the problem of a rational economic order" according to Hayek

is determined precisely by the fact that the knowledge of the circumstances of which we must make use never exists in concentrated or integrated form but solely as the dispersed bits of incomplete and frequently contradictory knowledge which all the separate individuals possess [p. 77].

This information could only be revealed through the market.

Of particular relevance to an understanding of the assertion of decentralized and incomplete information is Hayek's view of the commodity. For Hayek, each commodity is only a 'type', and variations in quality, time and location and a myriad of other factors all contribute to differentiate the usefulness of ostensibly identical commodities, so that "Two technically similar goods in different places or in different packages cannot possibly be treated as equal in usefulness for most purposes if even a minimum of efficient use is to be secured." In fact we "would have to treat the existing body of instrumental goods as being constituted of almost as many different types of goods as there are individual units." [1935b p.209]

Vaughn refers to this as the problem of product specification [1980, p.546]. Each 'product' is capable of an infinitely large number of minute variations in availability and quality. In a sense, each object is a different 'good'. The information as to which 'goods' are available and which goods are required is therefore never complete, but is the subject of continuous discovery [op.cit. p.545].

It was for this reason that the entrepreneur was required in a rational economic system, and why Hayek believed that a planning board could not act as a replacement. It was the constant effort to find out how goods could be *altered* that for Hayek was the relevant feature of business. The planning board could never differentiate goods in this way.

That the price-fixing process will be confined to establishing uniform prices for classes of goods and that therefore distinctions based on the special circumstances of time, place and quality will find no expression in prices is probably obvious. Without some such simplification, the number of different commodities for which separate prices would have to be fixed would be practically infinite. This

means, however, that the managers of production will have no inducement and even no real possibility, to make use of special opportunities, special bargains, and all the little advantages offered by their special local conditions, since all these things could not enter into their calculations [Hayek 1940 p.143].

It was this element of differentiation that was crucial, because it introduced changes in the economy. The reason there could be no equilibrium in the market was because there could never be a fully specified product. Consequently, Hayek was not concerned with studying economics in general equilibrium. His concern was to study disequilibrium and economic change. The static equilibrium models were therefore inadequate.

In fact the market socialist models regarded the disequilibrium of capitalism as a disadvantage. It was this constant change that made the task of the Central Planning Board so difficult. As each unit of each commodity was in a sense unique, having different qualities, location, availability and other contingencies, the price of each unit would need to be treated separately. Since the knowledge of the units constantly changes, these prices would constantly alter. The CPB would have to constantly monitor an infinite number of prices.

The other consequence of the employment of static equilibrium models employing 'given' choices was that it was unable to consider the development of new products, and the other advantages of economies of scale⁸ [Bradley 1981 p.28] In this respect the static equilibrium economy may well involve lower welfare and higher costs than the dynamic disequilibrium economy, which could develop new production methods to lower costs

The point where these differences materialise in the market socialism debate is precisely in the concept of costs. The Austrian notion of cost was opportunity cost — the value of alternatives foregone. The point — as was mentioned earlier — was that these costs were *subjective*, and could be known only to the individual who bears them. This is because evaluating the opportunity cost involves knowledge of the

⁸ The reader will recall Marshall's extensive discussion on economies of scale.

value of all possible alternatives, and these alternatives, as we have just seen, are infinite. The knowledge of alternatives is therefore always incomplete, and is always diffused throughout the population, all the members of which have incomplete localised information of the different 'goods' which are available and which may be wanted. [Hayek 1945 p.88] Only the individual knows the opportunity cost of their choice, because only they can judge the value of alternatives foregone. This cost, moreover, will instantly change as the knowledge of alternatives changes.

The concept of cost used in the equilibrium models from which the market socialist models were drawn was not subjective, and in fact is only nominally opportunity cost. The general equilibrium approach uses "objective opportunities displaced by a course of action" rather than a subjective approach. The costs are indeed often measured in money terms, which again presumes an objective measurement: it would be possible for any individual — including the socialist state managers — to objectively measure alternatives foregone. This, for the Austrians, is an error. Costs cannot be measured objectively [Lavoie 1985 p.16].

Of course, both welfare economics and the general equilibrium socialist models have as a central requirement the measurement and knowledge of costs [Barry 1984 p.36]. However, to the Austrians, costs are not measurable, and are known only to "the man on the spot" [Bradley 1981 p.33]. The use of marginal costs as a pricing rule would therefore be regarded by Hayek as impossible — no planning or marketing board could possibly know the marginal costs, since only Hayek's "man on the spot" is aware of all the possible alternatives. Knowing the relative money price of already existing alternatives is insufficient. It is also necessary to know other possible alternatives (which maybe a change in a pedantic detail, such as location or packaging) which do not yet exist, but which could be delivered [Lavoie 1985 p.102]. Although the socialist managers could make decisions between the 'given' alternatives — production is not impossible in this sense — there is no real way of knowing whether better alternatives exist. Hayek's contention is that the market mechanism offers the chance to see if this is so, since new alternatives are constantly generated.

This is what Hayek means when he claims that socialist managers would lack the necessary information to derive production decisions.

The Lange/Taylor trial-and-error model and especially the Lerner [and Durbin] marginal cost rule were therefore rejected by Hayek as inoperative. The rule would simply be unenforceable. "What is forgotten is that the method which under given conditions is the cheapest is the thing which has to be discovered anew, sometimes almost from day to day, by the entrepreneur" [Hayek 1940 p.196,qv Lavoie 1985 p.142,170]. It would be impossible to know whether each decision took advantage of a possible change or not. Whereas the socialist models assume that these costs can be objectively measured, the Austrians rejected objective costs. Even Lerner's 'opportunity cost' was an equilibrium concept, and therefore remains static [Bradley 1981 p.26, qv.Lavoie 1985 p.140] in spite of his criticism of Lange's model as "reactionary and static" [cf. Hoff 1938 p.218] Furthermore, all the models deal with standardised commodities, whereas in fact almost all commodities have infinite differentiation. This results in the need for constant price changes, which would be impossible for the market socialist models to deal with.

If in the real world we had to deal with approximately constant data, that is, if the problem were to find a price system which then could be left more or less unchanged for long period, then the proposal under consideration would not be so entirely unreasonable. With given and constant data such a state of equilibrium could indeed be approached by the method of trial and error. But this is far from being the situation in the real world, where constant change is the rule [Hayek 1940, p.188].

Hayek also pointed out that the trial and error method was actually very similar to the mathematical model. The prices of goods were all interconnected, and no one price could be changed in isolation: "Almost every change of any single price would make changes of hundreds of other prices necessary" [Hayek 1935b p.214].⁹ The method of trial and error, Hayek believed, would therefore soon break down. There would be an enormous number of errors caused by the same number of trials, and no way of

⁹ Actually the problem of interconnected utilities was noted by Roper in his 1931 book [see Hoff 1938 p.126]

knowing which trial caused which error. The trial and error method only applies to prices individually — it cannot be used to obtain the *general* equilibrium.¹⁰ Indeed, the entire process depends upon the assumption that no other change occurs whilst the trial is proceeding — that *ceteris* remain *paribus*. That is, it involves a static economy. As Barry puts it ‘‘the socialist model was, in effect, an experiment in comparative statics’’ [1984 p.55].

There are also good managerial reasons for the market socialist systems to break down. Lange especially insisted that the socialist managers must treat the market clearing price as parametric: each producer must act as though they were unable to influence price [Lange 1938 p.70]. In practice, it is most unlikely that the managers would act in this way, particularly if it runs contrary to their personal interests [Vaughn 1980 p.548]. It is entirely possible that the managers will use what amounts to a monopoly position to raise prices and reduce output (and therefore, of course, reduce costs), particularly since Lange’s rules were to apply to the managers of the entire industry as well as the individual producers. It would be possible for the state undertakings to simply exploit their monopoly position [Hoff 1938 p.125]. In fact, the marginal-cost/trial and error method is less a model for socialism than a device for state marketing. Indeed, Lange was aware of the general nature of marginal-cost pricing, and accepts that the Taylor model is not specific to socialism but can also be used in a totally centralised economy [1938 p.90]. He mitigates this by asserting that ‘‘such a system would scarcely be tolerated by any civilised people’’ [ibid. p.45]. It is unclear whether Lange is here relying on some Marxian analysis of the development of a socialist state, such that some correct political ‘form’ is presumed to come into existence.¹¹ Certainly, no analysis of political structures is given, and without this, toleration seems rather beside the point. It is unclear how effective political action is supposed to be taken. Humphreys describes Lange’s model as somewhat ‘‘politically

¹⁰ We can see that this is actually a partial equilibrium approach to a general equilibrium problem.

¹¹ It is worth remembering that Lange is the only participant in the 1930’s ‘London debate’ to explicitly endorse the Marxist theory of history.

innocent", and points out that the political structures may be undesirable or unrealistic [1977 p.109].

Furthermore, it is far from clear how the managerial decisions could be challenged. There is no real method of obtaining information about the responsiveness of managers to market demand. Bergson points out that

...Lange nowhere provided any criterion for judging and rewarding managerial success. The rules themselves, it is true, might be viewed as such a criterion, but in order to gauge and reward success on this basis the Central Planning Board would have to probe deeply into the cost and other internal records of individual production units. This would vastly increase the CPB's responsibilities, which it is a cardinal concern of the competitive solution to limit. The failure to establish any practical success criterion for managers, it was held previously [Bergson 1948 p.220] represents a major deficiency of the competitive solution and one not easily repaired [Bergson 1967 p.657].

There is no real method of ensuring that the production rules have been applied. In addition, the industrial managers will have less profit incentive, and even greater monopoly power than most of their capitalist counterparts (ibid. p.658).

This managerial critique is also raised by Kornai. He suggests that the Lange/Lerner general equilibrium-based models abstract from the institutional setting.

The people at his Central Planning Board are reincarnations of Plato's philosophers, embodiments of unity, unselfishness and wisdom. They are satisfied with doing nothing else but strictly enforcing the 'rule', adjusting prices to excess demand.

Kornai, with his experience of the Hungarian economy, believes that this conception is desperately wide of the mark

Such an unworldly bureaucracy never existed in the past and never will in the future. Political bureaucracies have inner conflicts reflecting the divisions of society and the diverse pressures of various social groups. They pursue their own individual and group interests, including the interests of the particular specialised agency to which they belong. Power creates an irresistible temptation to make use of it [Kornai 1986 p.1726]

It will be recalled that it was precisely because of the need for judgements on

managerial decisions that Mises emphasised capital markets. Further to this, Hayek points out that since it is near impossible to judge the performance of managers, a central bank, which Hayek regarded as the minimum requirement for any semblance of a socialist economy, would have very little knowledge as to which industries and enterprises to commit funds to. There is no automatic allocation mechanism, which was the function that the Austrian believed the profit and loss of the market performed, nor was there any 'track record' to judge the success of the present going concerns [Hayek 1935a p.20 cf. p.232 ff.].

III. NORMATIVE THEORY

The 1930's market socialist models were the first systematic attempts to directly apply the pure economic theory of the welfare economists to a hypothetical economy. The attempt failed because the welfare economics theory is an inadequate approach to the analysis of real-world economies. The Austrian critique was a critique of the unrealistic approach of the orthodox theory from which the market socialist models were derived [Barry 1984 p.54 cf. Murrell 1983 p.102]. The models of welfare economics — of an unchanging world in which rational optimisers act instantaneously with perfect knowledge — was regarded as of no practical consequence by the Austrians. It was the orthodox theory itself, not the market socialist application of it, that was the main point of the 1930s debate.

The Austrian critique was in essence a critique of the application of orthodox economic theory. The debate outlined in the previous chapter was methodological in nature, and concerned the validity and scope of the orthodox theory. We saw that in order to remain logically valid whilst satisfying various methodological imperatives — that the theory should be positive and yield a social conclusion from the analysis of individuals — the scope of the theory had to be drawn so narrow that in the end it was rendered virtually analytic. The market socialist blueprints of the 1930's lay

firmly within the scope of this orthodoxy, and represented an attempt to apply the models of the welfare economists to achieve a particular politically inspired outcome. The Austrian claim was that this market socialist scheme was inoperative because it was derived from a largely analytic or unrealistic theory.

In fact, the Austrian economists readily conceded that once one accepted the welfare economics/general equilibrium theory, the market socialist position is not readily assailable. It was this that Mises meant when he said that there was no economic calculation in the static state. Under the static conditions presumed in the orthodox theory, the calculation problem posed by Mises and Hayek would simply not exist [Bradley 1981 p.28]. Indeed, one may well hold the view that Lange has adequately refuted the sceptical conclusions of Barone

it cannot be denied that as an answer to the computation argument, Lange's demonstration is an impressive accomplishment, and indeed it would seem that neoclassical economists who doubt the workability of socialism face a difficult task in responding to this demonstration. If the equilibrating process of real-world capitalism is explained by recourse to a Walrasian auctioneer, it is not clear why a planning bureau could not similarly function as a coordinating agent [Lavoie 1985 p.122].

Indeed, criticising the Lange/Lerner socialist models on the basis of the computation difficulties alone is insufficient. As Lavoie goes on to point out, the *computation* argument is dependent upon the *calculation* argument, which necessarily involves a rejection of general equilibrium theory:

Some might argue here ... that Hayek is showing that Lange's answer was ineffective not only against the calculation argument but also against the computation argument. However, Hayek's objection holds only if one rejects (as I believe Hayek did, at least implicitly) the Walrasian explanation of how capitalism works. For any neoclassical theorist who takes his Walrasian auctioneer seriously, Lange's formal analogy argument shows quite plausibly that his central planning board can do as well at finding a general equilibrium configuration of prices as the auctioneer could. Thus no doubt Lange would have responded to Hayek that if the planning board could not find the right prices, then neither could capitalists. Within what I have called the 'neoclassical' perspective, this response would be unanswerable [Lavoie 1985 p.168 n.12].

This general equilibrium function performed a methodological function in economic science though, as outlined in the previous chapter. The pure theory of

choice enabled the orthodox economists to maintain a positivist, neutral science of society. The economic science was to show how the society was to be organised in order to accrue maximum benefit. The positive science was required in order to show how much (or how little) control over social affairs was necessary. The science was to be individualistic to show how centralised control was undesirable, and a 'rational economic human' was posited to show how economic value was subjective.

This entire scheme can now be seen to have fallen through. The essential insight from the 1930's debate was the need for an evaluation of costs: that the measurement of costs must be *imputed* from values. If value is subjective, then costs are also subjective [Bradley 1981 p.33], and neither are capable of pecuniary objective measurement.¹² The choices facing the 'rational economic human' were infinite in number, and any talk of preference sets should be dismissed. The theory therefore does not show how social value could be derived. The orthodox theory was accordingly not able to make any comment upon how a society should obtain maximum benefit. The *raison d'être* for positive economic science has disappeared. It no longer provided instrumental knowledge of how to improve our lot.

Moreover, as we noted in chapter one, the very notion of a policy science is not politically neutral. The resultant theory will inevitably result in a particular view of the political process. It was this dilemma of positivist policy that the orthodox economists were unable to solve. Indeed, the entire thrust of the Austrian methodology is that the sort of objective economic science which the marginalists were hoping to achieve was impossible. They rejected the idea of prediction in the social sciences, claiming that the sort of regulations and laws which characterised positivist scientific activity did not exist in the social world [Barry 1984 p.35]. Hayek in particular denounced 'scientism': the use of physical science methods in social science. [cf.1955 pt. one], precisely because he believed that it inevitably denied the social scientist the sort of individualist basis that he viewed as an essential element of politi-

¹² It is worth noting that Marshall's scheme was different: he was looking at "well-being". He believed this was capable of some measure of objectivity.

cal economy.

With the denial of positivist social science, the sort of hedonistic psychology which formed the basis of orthodox theory becomes redundant. The Austrian economic psychology — as put forward by Mises above — moved towards a Darwinian explanation of social structure. The capacities of individuals are deemed to be unequal: those 'best fitted' to handle economic decision-making would eventually obtain the necessary economic power to do so. Note that the evolutionary mode of explanation is different to its positivist predecessors. There is no longer an equivalence of prediction and explanation. The explanation of evolutionary theories is always retrospective. Furthermore, the denial of objective economic theory means that the boundary between economic and social/political action disappears — indeed the Austrian economists are renowned for their forays into the disciplines of political theory and philosophy.

This is not to say that we need accept the substantive theory of the Austrians. We may well maintain that this school is no more realistic than the orthodoxy. Again, we can derive our critique from the methodological roots, as we did with orthodox economics. Although the Austrians rejected many of the positivist tenets, they nonetheless retain a methodological individualist approach, and with it the concept of the 'invisible-hand' explanation of the derivation of the optimal social outcome from the individual actions. This leads the Austrians to retain a psychology based on rationality, and also to retain the notion of causal explanation. We will see that these prove to be incompatible under Austrian theory.

The point of departure for the Austrians, it will be recalled, was their conception of entrepreneurial activity. This broadly speaking consisted of speculative action based on localised knowledge. Because the knowledge of production possibilities and opportunity cost was unavoidably localised, it was necessary to encourage and reward entrepreneurial activity. It will also be recalled that the Austrians placed great emphasis on capital markets. It was these that enabled entrepreneurs to run very large corporations 'by proxy' as it were: by employing managers.

Further to this, we also noted the accompanying psychological assumption made by the Austrians. In order to derive a distributional theory from this analysis, it is necessary to assume both that business success is largely a result of some sort of undefined entrepreneurial 'capacity', and that this capacity is inherent, and unequally distributed among the population. Only then is it possible to show, as Mises attempted to do, that distribution will settle into an optimal pattern. Mises' analysis was that given this unequal entrepreneurial talent, it was indeed functional for assets to be channeled to those who were best able to employ them.

Unfortunately, no causal link is established between the cited cause — entrepreneurial talent — and the cited effect — business success. It is necessary for the theory to demonstrate that success in the market is due primarily to entrepreneurial talent, rather than market power or simply luck. This demonstration is markedly absent from the analysis and remains a presumption. Without this demonstration the theory moves away from an Adam Smith-type 'invisible-hand theory' and towards a Darwinian style of evolutionary theory which neither explains nor predicts. This can be seen most clearly in Mises' treatment of distribution. In essence, this is an evolutionary theory, which states that the market acts as a filter mechanism to move resources away from bad entrepreneurs towards good ones. The actual manner in which the market does this is not specified, as a result of which we are unable to show that distribution is indeed optimal. There remains simply an assumption that those in some way 'best fitted' to receive wealth did indeed receive it. In order to explain that the distribution is optimal, it has to be shown that markets do indeed respond mainly to entrepreneurial activity.

In fact, it is far from clear that this is the case. The possibility that market power may affect the optimal outcome is not considered by the Austrians — they simply presume that the market will filter out 'inefficient' monopolists. But their evolutionary system must have some sort of starting-point. Given the sort of vast inequality of wealth which has been in existence for hundreds of years, it is hard to see how the pecuniary evolutionary process envisaged by the Austrians is to be got going.¹³ It

¹³ Even Hayek distances himself from uncritical support of the present institutional set [1935a]

would not be at all fanciful to suggest that the market is manipulated by the already powerful rather than the market being a mechanism to re-allocate wealth in a functional manner.

Much of the theory rests on the vaunted capital markets. The theory presumes free entry and exit of firms, and that entrepreneurs are able to obtain capital for speculation: the capital markets are 'open-ended'. It also presumes capital is mobile, and will pursue whichever areas will lead to greatest profit. In point of fact, present capital markets clearly reinforce market power, and access to funds for speculation will be denied to those who do not already have collateral. Indeed, the instability and constant crises of financial and capital markets gives an air of unreality to the Austrian theory.

Apart from the evolutionary aspect of the Austrian psychology, we can see that it also retains a notion of rationality. All *institutional* influences are ignored. This, combined with the evolutionary/Darwinian psychology, results in the Austrian 'explanations' being *post hoc* in nature. The theory has ceased to explain *why* events have occurred, and as a result can never be proven wrong — the economist is always wise, but only after the event. The theory has, just like the orthodox economics, become closed. Should markets not work in the required manner, the Austrians assume it is due to institutional barriers, or because individuals are not being entrepreneurial or some such. The Austrian theory is therefore no more capable of application than the orthodox theory was.

We must abandon the attempt at deriving our social economic theory from a methodological individualism, and we must abandon our psychology based on individual rationality. Later we will consider the Austrian political theory, and will also consider particular problems, such as the environment and the legal framework, on which the mainstream economists are unable to shed any light. In the next chapter we will look at the institutionalist critique of positivism and rationalist psychology, and

p.22-23], but offers no re-allocational scheme to remedy the faults. Indeed, none would be possible under the Austrian theory — presumably the 'evolutionary' type distributional theory is presumed to cover this.

consider the employment of social psychology and the pragmatist conception of science in economic theory.

CHAPTER 4

American Institutionalism

IN THE previous chapter we saw that orthodox economics could not derive the sort of social policy conclusions that were necessary in order to justify the positivist metatheory. The use of orthodox theory by the 1930's market socialists, while legitimate, failed because the orthodox theory itself could not be sustained. In particular, the concept of rationality employed by the orthodox economists was static, and was therefore not able to consider questions of dynamic economy. The Austrian critique was that economic values — and therefore imputed costs — were totally subjective in nature and cannot be known to any planner. Furthermore, they believed, the interaction of rational individuals led to a distinct order which it would be unwise to attempt to alter.

We suggested that in fact it was near impossible for such an aggregate social order to evolve. In addition, the Austrians retained the concept of rationality. Both of these elements — rationality and aggregation — were attacked by the American Institutionalists. They maintained that human action was not determinately rational, but was influenced by social culture and habit, and that individual action was insufficient to derive an optimum social order. They also attempted to revive the idea of economic planning by trying to base their value theory on objective technological/scientific criteria. We shall see in this chapter that although the critique of individualism and rationality is correct, the renewed calls for planning solutions cannot be sustained.

The American institutionalists, in so far as they can be called a distinctive school of thought, are certainly a much 'looser' school than any of the others we have looked at. In so far as the authors have a common thread, it is that they view the economy as a sub-system of an evolving socio-cultural system, and also that they believe that scientific and technological advance is a primary force in this evolution. They would not accept the rational self-seeking model of conventional economics.

Instead, they would claim that the behaviour of humans in society is specific to each epoch [Gruchy 1987 pp. 2-3].

We can identify two main streams of thought in the disparate school of institutionalist thought, representing two different interpretations of social institutions. We shall consider first of all the more deterministic wing of Veblen, Mitchell and Ayres, who invoke the pragmatist philosopher's conception of institutions as habits of thought, followed by the 'purposive' theories of Commons, who interprets institutions as deliberate social control over individual actions while employing to a greater or lesser extent a similar rationalist conception of human action as the conventional economists. We shall argue that both interpretations are necessary. Both in this chapter and in the chapter following, we will suggest that while the rational economic conception of behaviour is inadequate, we must hold, and work towards, a non-deterministic model of self-expression. This model, we will suggest, will invariably involve non-market, deliberate social action.

I. DETERMINIST THEORIES

Veblen

We saw earlier how the utilitarian formulation of individual and social action had metamorphosed into rational-choice explanation by the 1930's. This theory was also used by the market socialists as the basis of their models. Meanwhile in America, contemporary with Marshall and just preceding the English market socialism debate, Thorstein Veblen was developing a social psychological critique of orthodox economic theory. His critique led to many theoretical revisions which now form part of the orthodoxy, but his work was in itself heterodox and radical, and led to a distinct socio-economic theory.

The utilitarian hedonist psychology was individualist and transcendental. The utilitarians conceived human nature as given, and derived economic laws which they considered immutable. Veblen, by contrast, insisted on the historical relativity of all social science generalisations, because of the malleability of human nature. Furthermore, human nature was an *endogenous* variable, not a 'given'. It changed as a result of the normal functioning of the social system.

The basis of Veblen's critique was the employment of hedonistic psychology. Veblen joins together the classics and marginalists in this; he regarded the marginal utility school as a "specialized variant" of the classics [Veblen 1909 p.155].¹ They may have provided a "sufficient reason" for human behaviour, but not an "efficient cause";² they have not postulated a causal explanation. Economic explanations were given in teleological terms, not cause and effect [op.cit p.158]. The fact was that, for Veblen, "each generation of economists had taken for granted the very things that most needed proving — the 'preconceptions' they took over from the prevalent world outlook and the accepted institutions" [Lerner 1948 p.20]. The Classics and marginalists ignored the social and cultural effects of society upon the humans which create that society.

A gang of Aleutian Islanders slushing about in the wrack and surf with rakes and magical incantations for the capture of shell-fish are held in point of taxonomic reality, to be engaged on a feast of hedonistic equilibration in rent, wages and interest [Veblen, cited Lerner 1948 p.20].

Since human behaviour is regarded as an universal datum, all that is left for the social scientist to do is to classify this behaviour — taxonomy. All economic behaviour is "hedonistic equilibration" in one form or another. Economics makes no allowance for the possibility of changing human nature. In the "received formulations of economic theory", according to Veblen

¹ Nabers points out that both the classics and the neo-classics supported the status quo for the most part. Certainly not much in the way of new policies were derived for business or for the state, and the analysis of the relation of the individual to the economy was similar [Nabers 1958 p.78]. See also chapter 2 above.

² i.e. propelling cause.

the human material with which the inquiry is concerned is conceived in hedonistic terms; that is to say, in terms of a passive and substantially inert and immutably given human nature. The psychological and anthropological preconceptions of the economists have been those which were accepted by the psychological and social sciences some generations ago. The hedonistic conception of man is that of a lightning calculator of pleasure and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. He is an isolated definitive human datum ... [Veblen 1898 p.73].

For Veblen, the psychological theory of the hedonists was outdated, as indeed was their conception of science. He rejected the scientific basis of teleological explanation. Modern science in Veblen's time was Darwinian. Veblen criticised the Classics and Marginalists for not positing a theory of the evolution of human nature. This would provide a causal explanation of human action. The acceptance of the present social norms by economists as "intrinsic to the nature of things", Veblen suggests, "limits their inquiry in a particular and decisive way. It shuts off the inquiry at the point where the modern scientific interest sets in" [Veblen 1909 p.161]. If Bentham wanted to be the 'Newton of the moral world', then Veblen wanted to be the Darwin of economics. Anthropology was to be replace physics, and the concepts of equilibrium and calculus had no part in Veblen's scheme. Instead, he wanted to search for the reasons behind the presently existing social norms to explain what the orthodox economists took for granted.

Veblen believed that human nature underwent an evolutionary change along Darwinian lines [Copeland 1958 p.50]. In place of a human nature modelled on rational thought, Veblen posited thought as conditioned by habit, social conventions and social experience. Veblen replaced the hedonism of the utilitarians with the social psychology of the pragmatists. The term used by the pragmatists to describe habits of thought and social conventions was *institutions*. It was with institutions that Veblen was primarily concerned. He was interested in cultural evolution rather than biology, and unlike the "social Darwinists", he was unconcerned with biological attributes of individuals. He regarded biological traits as universal [McFarland 1985 p.97, Lerner 1948 p.210]. The important element for Veblen were the effect of the institutions upon basic human nature.

Veblen held that although primary instincts were of importance as a determinant of human nature in the early stages of cultural evolution, by the later stages these were dominated by the institutional structures [Rutherford 1984 p.332]. The instincts were the fixed "prime movers" of human action. Veblen mentions three such instincts: the drive towards the provision of welfare for both family and society (Parental Bent), a drive towards explanation of the world (Idle Curiosity), and a drive towards technological improvement (Instinct of Workmanship).³ In Veblen's schemes, the social institutions acted and (to an extent) reacted upon these instincts.

The heart of Veblen's social theory is the "bitter and unending struggles that go on in the cultural arena". One such struggle is the conflict between animistic and matter-of-fact explanations, between superstition and science. Science weakens the cultural institutions, which will attempt to overcome or divert the results of inquiry. [Lerner 1948 p.24]. Since the institutions are the result of habits of thought, they are antipathetical to matter-of-fact inquiries. And the members of the social order who gain from the institutional system would obviously have a vested interest in conserving the order: their class was called the "Vested Interests".

The conflict between superstition and science emerges in other forms. Of particular interest to Veblen, and the link between his social and economic theory, is the clash between the workmanship instinct and the pecuniary institutions. The emergence of modern civilisation had altered an instinct for well-being to one of a predatory culture [Veblen 1899 *passim*]. When this leads to individual esteem being judged by reward, property is no longer possessed for use, but as a mark of status. Goods take on what later came to be called "positional" attributes [cf. Hirsch 1977]. Goods are only of value in so far as they are not possessed by others. This makes

³ See Veblen [1914b p.25, 1918 p.5], Nabers [1958 p.84] and others put the predation among the instincts, possibly because of the use of the term in the *Theory of the Leisure Class* (Veblen also refers to the "instinct of sportsmanship"). This was an early work however, and contains some loose terminology. Veblen makes several references to predatory culture and predatory habit (cf. Veblen [1899 p.7,14 and 36], see also chapter headings: 'Dress as an expression of the Pecuniary Culture', [op.cit. chap viii], 'The Technology of the Predatory Culture', [1914 chap iv]. The concept of a predatory instinct would also contradict the non-predatory nature of savage societies mentioned in the *Leisure Class* [p.24] and the *Instinct of Workmanship* [p.101] [Rutherford 1979 p.122].

wealth, *by definition*, socially scarce:

In the nature of the case, the desire for wealth can scarcely be satiated in any individual instance, and evidently a satiation of the average or general desire for wealth is out of the question. However widely, or equally, or "fairly" it may be distributed, no general increase of the community's wealth can make any approach to satiating this need, the ground of which is the desire of every one to excel every one else in the accumulation of goods [Veblen 1899 p.32].

By changing human nature from a concern for welfare to positional wealth and predation, the vested interests can legitimise their position in the social order. By creating social scarcity they can then legitimise business activity. What is actually for Veblen the creation of waste is seen to people under the pecuniary institutions as a vital social function.

The creation of waste enables business to draw upon "intangible assets", assets which have no relationship to material productive sources. They represent free income to the businessman. Industry, on the other hand, employs only tangible assets: "mechanically productive capacity" [1919b p.69]. This difference concerns one of the main areas of Veblen's framework.

The conflict between workmanship and the pecuniary institutions manifests itself in a conflict between business and industry. Business attempts to sabotage industry by providing a check upon the productive capacity of industry, and by keeping prices high. The main weapon of industry was technology. It was progress in technology — the "industrial arts" — that provided the challenge to the old institutions. The owning groups respond to the challenge by reasserting the existing principles: the assertion of vested ideas by the vested interests [Lerner 1948 p.25]. There is therefore a 'lag' between the state of the industrial arts and business. The outcome of the cycle is uncertain, there is no suggestion of technological determinism [Rutherford 1984 p.331].

The main aim of business, then, is to take an ever expanding and productive industrial complex, and ensure that nonetheless a condition of scarcity exists. This is achieved through non-market means. We have mentioned one of these — the instiga-

tion of positional wealth. Following from this is the production of conspicuous waste. This is Veblen's concept of the 'Leisure Class': that conspicuous expenditure and leisure are taken as signs of well being. The Leisure Class — those whose wealth precludes the necessity of work — dominate the cultural norms of the society. Standards are judged against this norm: the 'standard of living' is judged against this cultural level which is set in relation to the leisure class. Patterns of consumption of the "common man" are emulative of the leisure class — dress, entertainment, food, drink, housing and so on. The vested interests — business — utilise the productive capacity of industry for profit by producing articles of waste.

Veblen stressed that this wastefulness may well be invisible to the participants. "Ordinarily", he believes "his motive is a wish to conform to established usage, to avoid unfavourable notice and comment, to live up to the accepted canons of decency in the kind, amount, and grade of goods consumed, as well as in the decorous employment of his time and effort" [1899 p.155]. The tastes that the orthodox economists took for granted were, for Veblen, created by business and vested interests.

In contrast to the position taken by the Austrians, which we outlined in the previous chapter, Veblen believed that there was actually a drive in industry towards standardisation, which was advantageous. It was business which created the desire for individualised goods, because the uncertainty which this created enabled the businessman to extract profit [1904 p.28, cf. Nabers 1958 p.88]. Veblen believed that the uncertainty which according to the Austrians formed the entire problem of economics was in fact deliberately created by the business class. This uncertainty was heightened by periodic cyclical crises, caused by expansion of credit [1923 p.91 ff.]. This led to a rapid expansion of industrial capacity, and then to overproduction and glut [ibid.]. The cyclical tendency was often curbed through the "business-like sabotage" measures of restriction of output, and unemployment [op.cit p.97].

There were other methods of diverting production into waste. One method that was to assume considerable significance in the coming years (and has maintained that

significance up to the present) was the use of patriotism and militarism to serve business ends. Veblen was concerned not only with the Leninist analysis of imperialism. He also believed that the use of nationalism was an intrinsic part of the operation of the vested interests [1917 *passim*]. The use of militarisation was held to be a good illustration of the duality of business and industry. On the one hand the industrial arts were vital to the successful prosecution of warfare

... modern warfare not only makes use of, and indeed depends on the modern industrial technology at every turn ... but it draws on the ordinary industrial resources of the countries at war in a degree and with an urgency never equalled. No nation can hope to make a stand in modern warfare ... without the most thoroughgoing exploitation of the modern industrial arts [op cit. p.310].

Yet the business interests which the state were to represent were antithetical to the machine process of industry. There is a dilemma. The vested interests are reliant upon technological development, but this very development works against the vested interests. However in the case of war, the change may sometimes be so sudden as not to allow the development of new institutions [ibid.].

Veblen seemed to regard the actual production and distribution of artefacts as unproblematic. It was only the business interests that create the need for a pecuniary valuation of goods [1904 *passim*]. Similarly, the "laws of distribution" posited by the orthodox economists were simply reflections of the institutions of private property. The orthodox theory served to legitimise these institutions, indeed they formed part of the institutions themselves [see Nabers 1958 p.88]. The orthodox economists confuse value with price. The medieval notion of the "just price" was replaced by a notion of competition. The "natural order" was replaced by the "invisible hand" which had the advantage of making the benefit to the community an irrelevance to the individual. All that the orthodox theory has achieved is to explain pecuniary exchange. Progress is implied, but cannot be shown. Waste is outside of the domain of the theory.

In summary, business exercised a cultural control over the common man which camouflaged the real nature of the social system. Orthodox economic theory was a

part of this cultural control. Veblen's theory was, in this respect, somewhat similar to that of Marx. Indeed, in many ways Veblen's theory can be viewed as a supplement to Marx [Gruchy 1972 p.27]. In this light, we will consider Veblen's theory of the state and his class theory. Veblen was less concerned with the *means* of production than with the *ends* of production. His class cleavage was between those who benefited from the product set — the "vested interests", and those who had no stake in affecting this set — the "common man". Veblen turns the separation of ownership and management into a critique: it is the absentee owners who create the requirement of pecuniary measurements of production. Business controlled industry. The division was

not between those who have something and those who have nothing — as many socialists would be inclined to describe it — but between those who own wealth enough to make it count, and those who do not. [Veblen 1919b p.161]

The key factor was those who were of the "kept classes", who had a

legitimate right to get something for nothing, usually a prescriptive right to an income which is secured by controlling the traffic at one point or another. [ibid.]

Veblen's concern was not so much the Marxian concept of "one capitalist driving out many", but rather the idea of a dynastic class of vested interests, who manipulated culture and law to protect their vested interests.

Just outside of the vested interests are their "agents", who will act for and protect the vested interests proper. Notable among these are the clergy, the state and the military. Also of interest is that Veblen puts the labour unions among the agents of the vested interests [cf. Pluta and Leathers 1978 p.127].

It will be recalled that one of the cultural controls that Veblen believed was exercised upon the common man was that of nationalism. This was possible because, for Veblen, the state was one of the agents of the vested interests. The government was the institutionalised means of physical coercion in any society, and would protect

the existing social order [Hunt 1979 p.130]. It would act on behalf of the business class, and would also attempt to preserve itself, hence Veblen's term *dynastic* state. The state-run concerns would also invariably reflect the business interests, most notably the education system (hence his work *The Higher Learning in America*, subtitled 'A Memorandum on the Conduct of Universities by Business Men' [Veblen 1918]). Veblen did not regard it as particularly desirable that the state should increase its intervention in the economy, or undertake public expenditure, he regarded the latter as "ceremonial".

Let us return briefly to Veblen's view of orthodox economics. We discussed earlier his claim that the scientific methodology of orthodox economics was outdated. Veblen also claimed that the theory itself was outdated. It may have had empirical reference in the eighteenth century, but had no reference in the twentieth [1909 p.151, cf. 1919b p.162]. Veblen rejected the classical and marginalist concept of perfect competition. This was no longer applicable to modern business. The economic world of Veblen was one of monopoly, oligopoly and large corporate groupings. It should be reasonably obvious that this is a necessary pre-requisite for the level of influence Veblen ascribes to the "business class". The completeness of Veblen's system may also explain why he did not attempt to formulate his own system of political economy. His outlook, unlike virtually all the other theorists we have considered, was one of pessimism. Even the hopes he did have were to be proven somewhat false. Veblen placed great store in the machine process, and in the engineers as a force for change. This has palpably failed to occur.

In spite of the fact that Veblen did not systematise his socio-economic system, we can gain some idea of his thought through his critique. The first step in analysing Veblen's theoretical scheme is probably to attempt a separation of his psychology from the social psychological theory. It has been noted that while Veblen attacks the psychology of the utilitarians, he tends to replace this with his own assertions of instincts. A far more consistent approach would be to reject psychologism altogether [Rutherford 1984 p.333]. The use of instincts is an obvious target for critics, and is

generally regarded as the weakest element of Veblen's work [Pluta and Leathers 1973 p.133, Ayres 1958 p.25]. It can also lead to racism, which Veblen tended towards [op.cit. p.132, Hunt 1979 p.118]. The concept of instinct is not necessary to the social theory. "One could abandon the psychologism in Veblen's work and still maintain a perfectly cogent non-psychological theory. Indeed, the social psychological concept of institution leads exactly to this point." [Rutherford 1984 p.345] The reason for the instinct concept was mainly because of Veblen's fascination with Darwin [cf. op.cit. p.333]. Darwin was cautious about using evolutionary theory in the social sphere however. Veblen's use of Darwin gives no significant insights [Hunt 1979 p.118].

Veblen appeared to regard a theory of value as unnecessary. Value was a question of scientific use, not of pecuniary measure. Pecuniary valuation was simply a business class institution, and was not required for serviceable production for the community. Veblen believed, to use Zingler's phrase, that there was a contest of "vendability versus serviceability" [1974 p.227]. The value of commodities was measured in technical terms by the service to society it performed — by its contribution to the continuity of the life process [op.cit. p.224]. The economic value was *socially* determined:

Private utility may reflect only the whim or caprice of the individual, whereas social serviceability reflects what is fundamental to human survival. Social or brute serviceability is a matter of the 'mechanical' and 'chemical' qualities of goods that make them useful for enhancing 'human life on the whole'. In other words social serviceability is a matter of science and technology and the ultimate criterion of value is not subjective personal utility but science and technology. [Gruchy 1972 p.23]

Veblen appears to maintain that if production was standardised then the industrial system would be able to produce adequate quantities of goods. This is especially the case if the emulative expenditures were not in existence. Indeed, Veblen viewed most human wants other than that for survival as largely social phenomena [Zingler 1976 p.223]. The whole concept of scarcity, central to the Robbins definition of economy, was for Veblen created by the vested interests [cf. Nabers 1958 p.96-98].

The entire question of consumer sovereignty, which was the key issue in the debate on market socialism, was regarded as non-existent by Veblen. Nabers's analysis is interesting. In reply to Frank Knight's objection that a rejection of consumers sovereignty has dangerous political implications for a democracy [Knight 1920 pp. 518-20], Nabers suggests that

Veblen would have replied (1) consumer sovereignty is a myth derived from an acceptance of the hedonistic calculus; (2) in any event the gains to be made from the application of the canons of serviceability for society as a whole would outweigh any other possible loss and (3) to the implied question of who is to make the decisions for individuals he would ask "Who makes them now"? [Nabers 1958 p.99]

On the production side, Veblen posited three sources of inputs the availability of which determined the limits of community production: the size of the population, the natural resources, and the state of the industrial arts. The last is of primary importance. Veblen believed that the capacity of industry set the limits for population [Veblen 1919b p.293 ff], and also that "The state of the industrial arts determines what natural materials will be useful as well as how they will be made use of" [1923 p.63].

The major source of production was the industrial arts. These industrial arts were for Veblen a *public domain* factor: — he referred to them as a "joint stock of knowledge" which belongs to the community. It was impossible to factor out the level of productive efficiency of an individual unit, as the marginalists had suggested. Veblen viewed the factors of production as public goods [Nabers 1958 p.103].

We would add that Veblen would also have rejected Knight's notion of a democratic political system, since the political is dominated by the business class. Consequently, Veblen did not undertake extensive analysis of the state, or of the political system. Since the cultural controls affected the behaviour of the common man, the vested interests would not be simply demolished. Business would continue to control industry, even though industry had no need for business. Unlike Marx, Veblen did not believe the state would wither away.

Veblen believed — correctly or otherwise — that Marx had used an unscientific Hegelian philosophy in his belief in the inevitability of a socialist antithesis to capitalism. Veblen wanted to change or update Marxism into a "neo-Darwinian" theory. Although Veblen saw the economic as in a state of constant flux, this was not due to any "inner contradictions" of the social system. Neither was there any pre-ordained goal for history [cf. Pluta and Leathers 1978 p.133, Lerner 1948, p.35]. There was no inevitability concerning the Veblenian analysis, indeed he was considerably pessimistic. In a way, Veblen was Marxism *sans* materialism and determinism. The cleavage between workmanship and predation is similar to the Marxian class analysis [Hunt 1979 p.122]. In practice, many radicals use a Veblenesque class scheme [Pluta and Leathers 1978 p.128].⁴ Furthermore, aside from the common belief in the centrality of the economic, we can see how the "exploit" of Veblen's theory would only be possible in a class society, after technology and tools had made production efficient [Hunt 1979 p.122]. Veblen rejected the labour theory of value though; he considered it a leftover from the natural-rights philosophy of the classics [Veblen 1906 p.278]. And although the conflict between the common man and the vested interests is very similar to the labour/capital clash, the two theorists did have different emphasis: Veblen on the social-psychological, Marx on the economic-legal [Hunt 1979 p.126].⁵

Veblen's work gives a clear example of how the cultural critique of institutionalism took it beyond the realm of orthodox economics. By positing the manipulation of wants, Veblen undermines the entire rationality approach of individualist economics. Even if equilibrium is attained and markets cleared, and even if maximum 'satisfaction' is attained, this need not be construed as a social optimum, since these wants are created artificially.

At the level of critique, Veblen's work is powerful. It is when Veblen attempts to construct his closed, Darwinian theory that the project weakens. As we have seen,

⁴ In fact, many social scientists also use similar classifications.

⁵ For a discussion of Marxian critiques of Veblen see Simlich and Tilman [1982]

evolutionary theories tend towards determinism. Darwinian theory replaced teleological explanation with

a fully materialistic explanation in terms of 'opaque cause and effect': evolution has no course, and no consciousness guides it — or even affects it. Veblen clearly wants to translate this idea into economics. Taken to its logical conclusion, of course, that would mean eliminating all consciousness from economic explanation, creating an economic analogue of Skinnerian behaviouralism. [Langlois 1989 p.273]

Veblen is in effect positing a deterministic theory, in which all behaviour is socially determined. This may be the reason that Veblen reached such pessimistic conclusion. He has left no room in his theory for conscious action. All behaviour is controlled — there is no way of breaking out of the vicious circle. We should reject such a fatalistic theory, not least because Veblen does not consistently employ a strictly materialist approach [op.cit. p.275]. He frequently employs conscious actions on behalf of the vested interests — such as the employment of nationalism and war, for example. His critique is as much concerned with the conscious use of power as with the inevitable consequences of structure, which is in contrast to a structuralist, deterministic theory along Darwinian lines.

Where methodological questions arise within substantive economic theory is in the theory of value, where they take on a technical guise. Veblen views value as entirely a technical problem: value consists in the technical uses to which a commodity can be put, and is measurable by physical science. Unfortunately this value theory is inadequate. Without a common unit of account, there is no way to compare the values of different commodities. We cannot say whether, for example, land should be used for industry or agriculture, or any other comparison of different commodities. Such comparisons require at least a unit of account to balance production across the board. This was the scheme put forward by Clarence Ayres, whom we will discuss shortly. We will maintain that some notion of subjective valuation is still necessary unless the accounting prices are to be arbitrary.

Veblen's scheme replaces the money-based conception of objective value used

by the 1930's market socialists with an objective value theory based on the material properties of commodities. As such it is open to many of the criticisms of the socialists we outlined in the previous chapter: in particular that no change in circumstances could be accounted for. Veblen's value theory is actually as static as that of orthodox economics, and the engineers whom Veblen believed could plan the economy would run into exactly the same problems as the socialist Central Planning Board.

Mitchell

If Ayres can be considered to be attempting to develop Veblen's objective value theory, Mitchell can be viewed as attempting to reconcile subjective and objective values. He thought that it would be possible to objectively 'understand' the subjective basis of price. The link between Veblen and Mitchell has often been misunderstood, possibly since Mitchell's main work was ostensibly on business cycles and his books were highly quantitative in content — indeed he was one of the first economists to use statistical data extensively. This has led to his work being dismissed as empiricist [cf. Starr 1983], especially by book reviewers [eg. Koopmans 1947]. There is a structure to Mitchell's work, but it is implicit and not codified. His acceptance of Veblen "has been more complete than those are aware who have read his books but not his essays"⁶ [Tugwell 1937 p.234]. Tugwell believes that the orientation of Mitchell's work was provided by Veblen: "If he has been an observer, he has always known what to observe" [ibid.].⁷ The criteria for deciding what to observe are seldom stated though, and have to be pulled together from a variety of sources.

One such source, which although not directly concerned with methodology strongly suggests non-empiricist sympathies, is Mitchell's *Types of Economic*

⁶ See Gruchy [1972] p.45

⁷ cf. Gruchy [1972] p.45: "Mitchell believed that detailed, quantitative studies of the business cycle would enable economists to have a better understanding of the qualitative aspects of the money economy. He felt that in the early decades of this century there was more to be gained from quantitative than qualitative economic analysis. His inquiries might be 'more intensive and tamer' than Veblen's broad speculative studies, but in Mitchell's opinion this was the way to advance beyond Veblen on the path that the latter had staked out."

Theory, his seminal work on the history of economic thought. Mitchell lectured at Columbia in this subject for 24 years,⁸ which suggests more than a passing interest. It rather suggests a concern for questions of methodology. We shall see that this is indeed the case.

Mitchell did not reject all theory *per se*. Few academics would spend 24 years analysing something they reject outright. What Mitchell was concerned to attack was certain types of theory. Mitchell realised that theory is necessary for the generation of working hypotheses and for effective thinking [Wolfe 1939 p.212]. What were not useful, Mitchell believed, were deductive systems based upon untested and unsound assumptions, and "pure" theories which were completely synthetic. The confusion arises because historically orthodox economic theory evolved as hedonistic, and Mitchell implicitly associated deductive theory with hedonism [ibid.]. It was hedonism which Mitchell was opposed to, and it was this which led to his interest in the history of thought. He was against the "conscious use of assumptions contrary to fact", especially when economists use them "not in the purely logical development of their untrue assumptions" but rather attempt the "use of these assumptions to facilitate comprehension of the complex conditions of reality" [Mitchell 1910 p.109]. Hedonism was such a false assumption, although the hedonism was not to be always explicit: Mitchell believed Fisher's work to be "Bentham's ideas under new labels" [ibid. n.28], and he viewed Marshall's work as an attempt to move away from hedonistic concepts [1916 p.145]. For Mitchell, the whole idea of an economics based on "the mechanics of self-interest" is misconceived.

Nonetheless, he believed that the development of economic thought consisted precisely of theories of this nature. Marshall, Mitchell believed, simply qualified his use of hedonism in an essentially hedonistic theory, "reserving an air of reality by sagacious qualifications of the conclusions" [Mitchell 1910 p.910]. Marshall's theory remains based on the pleasure/ pain continuum, expressed as differentiable function.

⁸ The two-volume publication *Types of Economic Theory* [Mitchell 1967] was edited from Mitchell's lecture notes for this course.

"Marshall based his whole system upon a theory of value. He thought of economic conduct as governed by two opposing sets of motives the force of which can be measured in money" [Mitchell 1944 p.213].

For Mitchell, this form of unrealistic postulate could never lead to realistic conclusions. The views of Mitchell and Marshall on the relationship between theory and data differed. Marshall had also called for quantitative work, but he believed the kinds of market forces were already marked out. Quantitative analysis was supposed to give an objective account of these forces — indeed Mitchell thought they formed the entire scope of Marshall's economics. However Mitchell believed that Marshall's model of two opposing motives was obsolete, and Mitchell did not search for a statistical measurement of these forces [Homan 1928 p.139]. Rather he wanted economics to turn to psychology for an explanation of motivation. He believed psychology would give no credence to hedonism. Indeed, the point of his 1912 article 'The Backwards Art of Spending Money' was precisely that observed consumer behaviour, and especially economic behaviour within the family unit, was clearly not governed by rational self-interest [Mitchell 1912 *passim*].

For his analysis of human motivation Mitchell turned to the institutional psychology of James and — especially — McDougall. It was socialised habits which evolved from repeated usage which Mitchell believed lay at back of social behaviour.⁹ If economic rationality did exist, it was this rationality which had to be explained and this by explaining human nature. Rational actions were only a limited part of human activity [Homan 1928 p.182]. For Mitchell "human nature is conceived, not as a ready-made something taken over at the outset, not as a *postulate* whose consequences must be developed, but as itself the chief subject of investigation [Mitchell 1910 p.111].

The major institution which governed economic rationality was money. It was "the use of money" which was one of the main rationalising institutions.

⁹ McDougall treats pain and pleasure as modifying influences on instinctive behaviour [Mitchell 1910 p.103].

It gives society the technical machinery of exchange, the opportunity to combine personal freedom with orderly cooperation on a grand scale, and the basis of that system of accountancy which Sombart appropriately calls "economic rationalism". It is the foundation of that complex system of prices to which the individual must adjust his behaviour in getting a living. Since it moulds his objective behaviour, it becomes part of his subjective life ... Because it thus rationalises economic life itself, the use of money lays the foundation for a rational theory of that life. Money may not be the root of *all* evil, but it is the root of economic science [1916 p.157].

Far from the neo-classical notion of money being used to measure motivation, money *becomes* the motivation in Mitchell's view. It is precisely because the individual is socialised into the use of pecuniary rationality that this is applicable to economic activity. It is not the case that money gives the economist a guide to satisfactions. It gives little guidance to anything at all except current institutional practice. By the same token, if we wish to study this institutional set, then the study of the money economy is he believed the only objective way of so doing. [cf. Vining 1951 p.114].

It was the concept of money which is one of the links between the history of economic thought and Mitchell's work on business cycles. Although the work appears to be positivist and even empiricist, there is a definite framework for his investigations, which was institutional and Veblenian in nature. We have already seen that Mitchell had criticised the use of false assumptions in orthodox theory. We have also seen that he criticised the avoidance of the institutional role of money. Of course, the combination of these two aspects was necessary for the equilibrium model of orthodox economics to hold.

Mitchell therefore considered data pertinent to cyclical fluctuations, as an antithesis to equilibrium. The data were part of a quantitative study which he believed to be objective, but he did have a "framework of preliminary ideas" which he employed to guide his search for data. We can view this as his theory.

However objective we might wish to make such a study, it was nevertheless essential to have some framework of ideas into which to fit the data. In short, there must be some test of relevancy. In setting up this test, Mitchell displays an implicit theoretical position. For he prefaces his investigation by a brief statement of his view of the operation of the economic system. And in this statement, negatively or by neglect, he may be said to discard adherence to any orthodox view of economic process, while, positively, his descriptive analysis rests upon the distinction, taken explicitly from Veblen, of the

distinction between business and industry [Homan 1928 p.166].

The main aspect of this dichotomy of industry and business which Mitchell believed was the main institution of contemporary activity was the use of money. "not the use of money as a medium of exchange, but the fact that economic activity takes the form of making and spending money incomes" [Mitchell 1913 p.21]. This meant that there was no direct connection between the well-being of the community, which was determined by the production of useful goods, and the pecuniary-motivated behaviour of the individual units — the family and the firm.¹⁰ Useful production functioned according to the dictates of businesses which were connected by an "endless chain of interlocking indebtedness" [Homan 1928], which determined business action. It is the "pecuniary aspect of economic activity" which must be dealt with, and fluctuations are problems of the fluctuating prospects of profits [ibid.].

The price system is the means whereby the pecuniary behaviour is coordinated. Mitchell viewed it as an endless and continuous chain, without any logical beginning or end [Mitchell 1913 p.31]. Because of this interconnectedness of business, the outlines of the system remained stable, even if the individual units changed [Vining 1951 p.116]. But because each unit was only motivated by profit, Mitchell did not believe any overall coordination would be achieved. "In detail, then, economic activity is planned and directed with skill, but in the large there is neither general plan nor central direction." [Mitchell 1913 p.38] The money economy gives no assurance of a desirable distribution nor of smooth functioning; this may be upset by uncertainty.

This, then, is the theoretical framework which Mitchell moved in. Having established that the postulates of rationality and motivation used in orthodox economics were wrong, he then went on to consider the question of equilibrium. "The whole bearing of his study, in fact, tended to discredit the notions of normality and of an

¹⁰ Mitchell used the same units of economic activity as the neo-classical authors (Vining 1951).

equilibrium of forces" [Homan 1928 p.174]. Mitchell believed that his review of business behaviour belied any notion of normal or static conditions.

But a review of business annals never discloses ... a 'static' or 'normal' state ... On the contrary, in the real world of business, affairs are always undergoing a cumulative change, always passing through some phase of a business cycle into some other phase ... In fact, if not in theory, a state of change in business conditions is the only 'normal' state [Mitchell 1913 p.86].

Using his Veblenian framework, Mitchell disproved the logical consequents of the orthodox model. This was the relationship between the master Veblen and his pupil Mitchell.

Veblen's method of demolishing systematic theory is to cast doubt upon its postulates. Mitchell appeals to his mass of facts and distils them into a convincing picture of economic process which lends no support to the conclusions of systematic theory. He thus appears, not as a parrot of Veblen, but rather as his complement, proceeding from a different angle and with a wholly different technique, to complete the discrediting of the scheme of thought in which economic theorists have framed their systems. [Homan 1928 p.176]

Veblen's theoretical framework could be ignored or dismissed by orthodox economists. Mitchell's factual examination of the conclusions was harder to avoid. However the fact that the link between the two was often not made facilitated this avoidance as did the fact that Mitchell did not offer any ultimate "cause" of business fluctuations. They were simply a function of the actions of business in the money economy. His was a "static" study in the sense that his concern was not to trace the evolution of institutions, but rather to describe cyclical fluctuations within the domain of the present institutional set. Institutions are fixed rather than variable [Homan 1928 p.173].

The other way in which economic theory could avoid analysing money as an institution was simply to take preferences as data, and not to seek to instill any valuation theory into this data. This was the line of Pareto, and later of Samuelson. Mitchell classifies this as pure theory. He wrote in 1916 that

It is clear, at once, that this type of theory eliminates the problem of valuation from economics. That is, it does not concern itself with the way in which men find out what relative importance different goods have for their purpose. Instead, it assumes that this process of valuation has been completed before they come to market ... [1916 p.150].

Later, Mitchell became more critical of this pure theory. "First and most obvious, it meant that economic theory moves on a more superficial level than in its more confident days. The theorist works with data he does not profess to explain" [1944 p.215]. Mitchell also believed that price theory was insufficient for an explanation of economic behaviour. The Benthamite value theories may have been incorrect, "But the policy of dispensing with a theory of value seems to me unhappy and unnecessary. When economists deal realistically with human behaviour, they find themselves needing ... to understand how people evaluate goods" [ibid. p.216].

Mitchell believed that economics should be able to give guidance as to future conduct. Pure economics was a futile indoor sport [Wolfe 1939 p.214]. He would not accept the notion of the economist as 'disinterested observer'. Economics has the moral purpose of obtaining rational control for societal welfare. This was never explicitly discussed however [op.cit p.219].¹¹ Defining welfare is of course the central problem of economics. Mitchell leaves welfare undefined. Nonetheless, the task of considering social welfare, however hazy the concept may be, determines the nature of economics.

The motive is now not idle curiosity but the "instinct of workmanship". And from this point of view, understanding is something more than hindsight or knowing the genetic causation of things. It now takes on the complexion of foresight or a certain teleological character.

The issue, therefore, involves no questioning of our need of factual knowledge. It lies in the significance or meaning of the knowledge we have or can get. And in this frame of reference meaning has an instrumental or value complexion. For all welfare economists ... the meaning of meaning must lie in the sphere of values [op.cit p.225].

Wolfe criticised Mitchell for not bringing out the significance of the means/end

¹¹ cf. Vining *op.cit. passim*. Vining suggests that this is in fact the main motive of all economic theory.

relation [ibid.]. The criticism is valid, but much of the relation is implied in Mitchell's articles. Mitchell also believed that only factual work could further the science.

Much of the critique of Mitchell comes from the fact that he did not propose a system to replace the orthodoxy he criticised. He appeared to favour economic planning, which he saw as an inevitability [ibid. p.210]. His main concern was to gain objective data from psychologists as to the actual motivation of humans. However, he is not very clear as to what exactly we would do with this data. It is unclear how motives would relate to welfare [ibid.]. Mitchell seems to cling to the Veblenian notion of instincts and believes these could be uncovered. Veblen believed that institutions functioned to raise the desire for consumption and thereby artificially create shortage. No economics would be necessary if industry could run unhindered. Mitchell is ambiguous as to how far along this road he is willing to travel. Without a definition of value or welfare, it is not possible, as we have seen throughout, to derive policy recommendations.

... it is a little difficult to see how welfare economics, which assuredly is without frame of reference unless it discovers or posits some human aims, can be much advanced by generalisations, no matter how illuminating otherwise, which have no reference to the purpose of the human behaviour on which they are made [op.cit p.217].

Mitchell has provided us with an objective critique of equilibrium theory. He isolated the question of equilibrium by reference to Veblenian social theory. However, he then wished to derive an objective replacement theory which would enable the economist to prescribe policy. As we have seen throughout, the search for an objective basis for value remains doomed to failure. Even if Mitchell were to uncover the institutional basis of market demand, it is far from clear how we are then to proceed. To say that market value is not objective may be correct, but does not in itself give an objective account of value which is required for non-arbitrary economic planning.

Ayres

This objective foundation for planning is precisely the basis for Ayres' theory. Ayres believed he could go 'beyond' the values given by price to some notion of 'real' value given by science and technology. Ayres scheme was that prices could be adjusted to account for these real, social values.

Ayres began his system with a critique and rejection of classical and neo-classical price theory. He drew a distinction between value, price, and capital, which he believed were the essential components of the "classical tradition". Although these three concepts were irretrievably interconnected they were nonetheless distinct. Price itself was only the guise in which economic problems emerge (Rostow 1976 p.76). While price has no significance in itself, in the classical scheme it is used as a catalyst "by the action of which capital and value were combined" [1944 p.37]. The key element of orthodox theory for Ayres was the way in which a concentration on price theory enabled the neo-classics to derive a value for capital (and labour). The neo-classics confused capital funds with capital goods (Walker 1980 p.651). By equating these two distinct elements, neo-classical theory was able to explain the growth of capital goods by the accumulation of capital funds, which they suggested occurred through abstinence or saving [Ayres 1944 p.50, cf. Walker 1980 p.651]. This also provided a moral justification for "the great inequality of income which is one of the most conspicuous features of capitalism" [Ayres 1944 p.52]. (In fact Ayres interprets the classics as justifying inequality on the grounds that it leads to capital accumulation [op.cit. p.53]). Once we understand the real role of technology this justification will be abandoned [Ayres 1953 p.284].

The confusion of capital funds and capital equipment made it appear that the growth of funds was required before industrial growth could occur, whereas for Ayres "industrial growth is a consequence, not of non-consumption, but of technological development. No community has ever starved itself into prosperity" [ibid.]. The level of economic wealth is determined not by the accumulation of funds, but by the level of technology [1944 p.283, cf. Walker 1980 p.653]. It was technological

growth that, for Ayres was the most important force in civilization [Walker 1979 p.521]. An understanding of economic growth required not an understand of market mechanisms, but rather an understanding of technological development.

Since Ayres rejected the neo-classic notion of price as the focus of value, he must also reject the Robbins means/end dichotomy. Ayres follows Dewey in this, and bases his theory of value upon the concept of 'continuity'. Value is a synonym for continuity which is the human "life process", the "continuity of civilization" [Ayres 1944 p.221 cf. Rutherford 1981 p.658]. The continuity of experience is the basic scientific postulate of both physical and social behaviour [Ayres 1944 p.91], and it is technology which enables the continuity of civilization to occur. Value is therefore instrumental, and located in technological knowledge (Chung 1978 p.119). Whatever causes technological development is valuable [Rutherford 1981, p.659]. The criterion of every economic judgement is "keeping the machines running" [Ayres 1944 p.223]. Value is objectified within technological instruments [Rutherford 1981 p.658]. "True values are not a matter of individual taste or institutional conditioning, but a matter of demonstrable efficiency in the maintenance and production of the technical continuum and the life process" [op.cit. p.660]. Questions of value are simply technical questions of choosing the right tools, as it were. Value is objective and transcendental.

Ayres is a technological optimist. Whereas for Veblen institutions can be either servicable or disservicable, for Ayres all institutions are ceremonial in nature. They are formed as a mixture of conventions, myths and social mores [Rutherford 1981 p.660]. These institutions serve to hide true value, and to assign 'rank through ritual', thereby maintaining the social order [Walker 1979 p.524]. The aim of institutions is to convey the social order of the past into the future. They are therefore in opposition to technology, which is concerned with future development. Institutions constitute a cultural brake to technological change [Rostow 1976 p.76]. The processes of technological and economic development therefore required large changes to the institutions of society [ibid.].

Ayres dichotomised human behaviour. The main force for progress was technological-based behaviour. This was held back by institutional/ceremonial behaviour. The price economy was one of these ceremonial institutions, and Ayres held that price bore no relation to social value. Ayres, following Veblen, believes that the industrial economy is more fundamental than this price economy [Gruchy 1972 p.97], in fact the price economy is viewed as simply a power system. 'Real' value is determined technologically and, as for Veblen, is capable of measurement by physical science. Prices can then be adjusted to reflect these 'real' values, and the economy planned accordingly [Gruchy 1972 pp. 97-109].

It was for these reasons that Ayres thought the level of finance capital to be an irrelevance. Progress occurred through technology, not through investment. The development of western societies is due to the "triumph" of technology and rational behaviour over ceremonial actions and institutions. Ayres believed [1944 chap ix]. He considered that the key to progress was to cut away the institutions which dragged back the progress of technology. Ayres believed that "the technological revolution is itself irresistible" [Ayres 1944 p.xxiv], and would lead to an improvement in the welfare of society.

In his interpretation of technological development, and his interpretation of value, Ayres could possibly be said to be extending institutional theory along Veblenian lines. With his analysis of institutions he begins a significant departure from the Veblenian position. Veblen considered that the vested interests held sway over scientific development. The institutional set was dynamic and evolutionary. In Ayres work all institutions are imbecile. He replaces the Veblenian institutional lag with his concept of institutional *drag*, implying that technological progress is inexorable and drags all institutional behaviour behind it. This raises the possibility — or even probability — of short-run triumph of technology over institutions, thereby completely changing the conclusions of the theory. Veblen believed only wholesale replacement of the institutional set could achieve maximum social welfare. Ayres' theory makes it possible for reform to achieve this. In contrast to radicalism and

pessimism. Ayres harbours liberalism and optimism [McFarland 1985 p.100].

Indeed, Ayres definition of the concept of 'institution' borders on the tautological. Veblen interpreted the concept somewhat loosely, using the term for ceremonial practice, customs, widespread social habits, and culture among others. Of these, the usual usage was social habits, the first in the list is the least central [ibid.]. Yet Ayres takes ceremonial practices as his exclusive meaning. The technology/ institution dichotomy may then be tautological: all institutions are bad [ibid.].

Anything that hinders economic development is by definition an institution in Ayres' work. If a habit of thought or behavioural pattern that we would call an institution contributes efficiently to the process of production, Ayres would call it a technological activity [Walker 1979 p.535].

Ayres's use of tautology is typical of deterministic theories. Ayres scheme is indeed deterministic. By his use of the concept of technological drag, his view of technology as a self-generating process, and his view of institutions as ceremonial, Ayres creates a technological over-determinism (op.cit. p.522). The tautological aspect of Ayres's definition of institutionalism is necessary to protect himself against a charge that some institutions may be beneficial to technological growth. If they are beneficial, they cease to be institutions.

There is another charge which can be raised against Ayres, which is harder to dodge. Deterministic theoretical systems must be functionalist in nature. If the determining element can be shown to be dysfunctional, then the determinist system begins to break down. Ayres was not really able to cope with the fact of the employment of technology for unarguably evil uses. Ayres argued that these uses were the result of institutional forces. Yet if this is the case, then institutional value-judgements are apparently of major importance in the development of particular technologies [Rutherford 1981 p.668].

Clearly, however, the recognition that the evil consequences of some uses of technology result from institutions should entail the recognition that the machines are ethically and morally neutral in themselves with respect to all uses, and that what are considered to be their beneficial effects are also determined by institutions. Uses of technology that are judged harmful in some or all respects are the result

of decision to employ technology in those ways. Beneficial consequences of technology result from value judgements about what is beneficial ... The value judgements are the outcome of the social institutions that operate to select objectives and to define problems. Science and technology enable the achievement of the objectives, but do not select them, as can be inferred from the different uses to which science and technology are put in different societies. The conclusion must be that Ayres was not correct in supposing that there are universally true moral values which are objectively determined by technological operations, and that he was unable to demonstrate scientifically that culturally relative values are harmful. This rejection of his theory of value is a logical corollary of the rejection of his theory of the antithetical roles of technology and institutions in the process of economic development. [Walker 1979 p.534]

The technological determinism of Ayres's theory is therefore in doubt. We can also see that his value theory is open to doubt. The essence of his value theory was that value can be identified with technological progress: the "instrumental" theory of value. This was to replace the classical/neo-classical value theory [Chung 1978 p.118]. Since technology is a positive determinant of all human values, these values can be objective. Ayres does not allow for differences over values — all choices are the same as choices between tools [Ayres 1944 p.212-13]. Science can give us objective analysis of value judgements. This is "an example of the naturalistic fallacy that statements of obligation can be derived from statements of facts" [Rutherford 1981 p.669]. Without showing that science can make values objective, Ayres's theory falls.¹²

We arrive at the same place as we departed in 1789. Pre-determined theory is held to be possible because value can be objectified. For Bentham this possibility was because values were based upon calculation. For the marginalists values could be measured by price. For Ayres science equates with 'real' value. Both attempt to show how what appear to be subjective valuations are in fact objectively measurable. Neither of them can be operationalised.

In fact Ayres, far more than Veblen, appears to be heading down a parallel path to Bentham. This movement is accentuated by his conception of the "unity of value". He maintains that all values — not only commodity valuations — form part

¹² The Knight/Ayres debate does indeed seem to be a confusion, as suggested by the titles of the articles. If morality becomes objective, then indeed no moral judgements need be made. The difference is over the content of value theory, not over the inclusion of ethics in economics.

of value systems which can be compared.

True values are trans-cultural ... because they are interrelated. All are manifestations of the same process, the life process of mankind. All knowledge is related to and conditioned by all other knowledge, and all skills are mutually contributory. Good health, freedom from disease and famine, is contributory to the acquisition of skill and knowledge; and the acquisition of knowledge and development of skill are contributory to good health.

All true values define and fortify each other. [Ayes 1961 p.167]

Ayes also extends this analysis to such values as freedom and equality, and even aesthetic "excellence". These were also capable of technological expression [op.cit. p.34 and relevant chapters]. The parallels with Bentham are clear — everything is reducible to scientific measurement. Unfortunately, in actuality these elements are not measurable in this way, and to claim this is possible remains faintly absurd. It seems worth pointing out that some of the "growth of knowledge" has been distinctly unhealthy; although Ayres would no doubt claim that this was due to institutions, not technology. But such reasoning smacks of *post-hoc* rationalising. 'Bad' technology is imbued with ceremonial practices, 'good' technology is not. By contrast, *ex-ante* judgements of how disparate technologies are to be deployed is required if planning is to succeed.

The idea that technology is itself neutral should be rejected. It is never possible to separate social from technological change.¹³ There is no sense in which technology can be objectively judged. Again, the idea of objective value cannot be realised.

II. PURPOSIVE THEORIES

Commons

The other wing of the institutional school concerned itself less with the replacement

¹³ See references in Dickson [1974] and Rose [1976]. Both Marxists and Greens would reject the idea of neutral technology; see for example the works of Habermas, Illich and others.

of orthodox rationality than with analysing the social outcome and offering policy proposals to supplement and reform the results of orthodox analysis.

Commons¹⁴ responded to the prospect of radical overhaul by developing a reformist theory. We shall see that Commons did not attempt a reassertion of the neo-classical approach, but rather tried to alter this analysis to accommodate the boundary extensions that he believed the discipline badly needed, and which fitted in with his early 20th century Christian ethics [Starr 1983 p.110].

Commons's avowed purpose was to cure the inadequacies of economics, both in theory and practice. He was "trying to save capitalism by making it good" [Commons 1934 p.143 cf. Dugger 1979 p.369]. This would be achieved not by a rejection of orthodox analysis, but by providing an institutional "supplement" [Rutherford 1983 p.722]. "The problem now" Commons believed, "is not to create a different kind of economics — 'institutional' economics — divorced from preceding schools, but how to give to collective action, in all its varieties, its due place throughout economic theory" [Commons 1934 p.5]. In Commons's view, orthodox economics was necessary but insufficient to provide an adequate analysis of behaviour [Commons 1936 p.242, Starr 1983 p.118].

Commons also did not dichotomise theory and practice. Rather, his approach was inductive: he attempted to generalise from his experience. Much of his writings are taken up with references to case studies and personal anecdotes as examples. He also believed in active reform, and his work should be viewed as bordered by the possibilities of what he viewed as possible reform within his stated ethical guidelines. Commons' reform method "transcended the rather artificial normative/positive dichotomy", instead of which "he was always practical, which means that he investigated what *could* be rather than what *should* be" [Dugger 1979 p.370]. This meant taking existing practice as his starting point, but he also believed that the ethical inputs to his analysis were that of the society under scrutiny. Reform must be based on the values of society [ibid.].

¹⁴ and also J. K. Galbraith

Commons's first major work was his *Legal Foundations of Capitalism*, completed in 1923. Most of his main concepts were developed by this time. However, Commons's writing style is by common consensus the most unintelligible of all western economists. The 1934 work *Institutional Economics* was a restatement of this theory [Ramstad 1986 p.1090], the earlier exposition of which many people had criticised "to the effect that they could not understand my theories nor what I was driving at" [Commons 1934 p.1]. *Institutional Economics* did little to solve this problem, however, and *The Economics of Collective Action* was an "attempt to explain in simple terms the essentials of a complex system of thought" [Parsons, preface to Commons 1950 p. v]. The references below are taken in the main from *Institutional Economics*, which was his major work of exposition.

We saw previously how the attempt of orthodox economists to arrive at a social value function faltered when they attempted to aggregate the individual functions which they posited. If Commons is to successfully supplement orthodox theory, he must either add a supplement or find an alternative to the orthodox methods of deriving a social value. Commons's response was to contend the boundaries of economics to embrace the political and the legal. These became necessary constraints upon individual activity. Commons defined an Institution as "collective action in control, liberation and expansion of individual action" [1931 p.644]. Social value was not derived from individual values, but rather formed the constraints within which individuals may act. These constraints were necessary, but were also interactive and dynamic, and the change in institutions was what caused the social evolution.

The initial point of departure from orthodoxy of Commons was the necessity for law in economic relations. In the orthodoxy law and ethics

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were necessarily excluded, because the relations on which the units were constructed were relations between man and nature, not between man and man. ... Neither statute law, nor ethics, nor custom, nor judicial decision had anything to do with either of these relationships; or rather, all these might be eliminated by assuming that ownership was identical with the materials owned, in order to construct a theory of pure economics based solely on the physical exchange of materials and services.

The latter was done. This identity of ownership and materials was accepted as a matter of custom, without investigation. It was assumed that all commodities were owned, but the ownership was

assumed to be identical with the physical thing owned, and therefore was overlooked as something to be taken for granted. The theories were worked out as physical materials, omitting anything of property rights, because they were 'natural' [Commons 1934 p.56-7].

For Commons the crucial omission of orthodox economics was the concept of the transfer of *rights*. "Although we speak of the price of commodities", Commons believed that "what we actually value in the market is the legal rights to these uses or services, namely property rights. Physical control follows legal control; legal control is strategic and valued" [1950 p.151]. What was involved in exchange was not the exchange of materials, but the exchange of property rights to economic goods:

If the subject-matter of political economy is not only individuals and nature's forces, but is human beings getting their living out of each other by mutual transfers of property rights, then it is to law and ethics that we look for the critical turning points of this human activity [1934, p.57].

The distinctive feature of political economy is the exchange of property rights. Commons maintains that *ownership* is the relevant question, and this involves human interaction. It also involves *social* action, in the form of legislation and judiciary. Ownership implies, in the modern world, law. This transfer of title is different to the act of physical delivery, which is part of the labour process of transportation. Property rights requires the existence of a state. "The individual does not transfer ownership. Only the state ... by operation of law as interpreted by the courts, transfers ownership by reading intentions into the minds of participants in a transaction." [1934 p.60].

Commons distinguished between institutional economics and engineering economics. The former was concerned with the transfer of property rights, and involved human to human relations. Engineering economics was the relation of human to nature, and concerned only the "physical attribute of use-values" [1934 p.256]. The engineer would go on producing indefinitely unless restrained, producing even to the point where goods were over-supplied. The production of use values — wealth — is distinguished from value, which is the scarcity value of wealth. Wealth

had a double meaning; the engineering meaning of output (use-value) and the business meaning of income (scarcity-value). Business is necessary to regulate the output, such regulation being a feature of all societies [ibid.].

Orthodox economics confused efficiency with scarcity. Technical efficiency must be supplemented by collective action in order to maximise real value. Efficiency and scarcity are complementary, and, Commons maintained, set limits to each other [op.cit. p.259].

Scarcity was the factor which Commons (following Hume) suggests requires the institution of property rights. Abundance would mean that there is no need for or possibility of title or ownership [op.cit. p.237, 1925 p.371]. Commons does not reject the Robbins' definition of Economics, but suggests it is insufficient. Scarcity was one of the requirements of economic analysis, since it explained the need for ownership, but the analysis of the legal and ethical institutions which enabled property rights to exist was also necessary.

The classics and neo-classics derived economic order from the 'invisible-hand' mechanism. Commons, however, is maintaining that the scarcity mechanisms of the market are only partly causal of the economic order: there are intervening parameters of institutions. There is no presumption of Adam Smith's 'harmony of interests', in fact, the opposite is presumed. Commons believes that conflict would inevitably ensue, and that 'harmony is not a presumption of economics, it is a consequence of collective action designed to maintain rules that shall govern the conflicts'. Commons expressly denied the concept of the 'invisible-hand': individuals' self-seeking would not lead automatically to the best social outcome. The imposition of collective order is required. Given that political economy involves transfer of legal title, it becomes a requirement for a sovereign state to exist, to enforce the legally binding contracts. The state monopolises sovereignty; the 'extraction of violence from private transactions' [1934 p.684, 1950 p.74, cf. Rutherford 1973 p.727]. Other forms of power and other sovereign bodies also exist: both economic and moral power may be employed by other 'governments' than the state government. There

is a hierarchy of governments in the political economy [Commons 1950 p.75].

Law, ethics and custom also provide, for Commons, a necessary *security of expectations*. Because exchange involves exchange of title, this requires a legal duty concerning *future* behaviour, a concept Commons terms *futurity*. Because of this, it is possible for legal title to be negotiated: credit, debt and goodwill become negotiable. Property need not be limited to tangible objects; intangible goods are also tradeable [1950 p.30].

To summarise, Commons designated five broad principles of economic action: efficiency, scarcity, sovereignty, futurity, and one other not yet discussed: working rules or customs [Parsons 1942 p.347]. These rules were the mixture of ethics, custom and collective action which has evolved which defined the rights and duties of individuals. An organised form of collective action Commons termed a "going concern" [Rutherford p.723]. The working rules of the going concerns may involve moral, as well as physical or economic sanctions. Furthermore, the institutions were themselves changeable by individuals, although to what extent Commons believed this possible is a matter for conjecture. We will return to this.

Economic activity for Commons took the form of what he called *Transactions*. This contained a bargain struck between parties of varying legal relationships concerning ownership rights. Commons identified three types of transaction. Bargaining transactions were between legal equals. Either party has the ability to withhold from the bargain. This form of transaction was connected with scarcity. The form connected with efficiency was the Managerial Transaction: a transaction between manager and managed. This was a relation between legal superior and inferior: the worker had less right to withhold. There are no available alternatives to the bargain — the "limits of coercion" are less [op.cit. p.725]. Rationing transactions involve sovereignty: the imposition of transactions by superior authorities, such as taxation.

The working rules of the society may constrain the negotiations within the transaction. For Commons, the key rule upheld by the judiciary was that of "reasonable value":

The key word for Commons was 'reasonable', reasonable value, cost, price, wage, profit and so on, and he always viewed expectations of future economic activity (transactions) in terms of prevailing notions of equity and justice. The ultimate criterion of reasonableness with respect to bargaining and division of economic power, income and wealth was what the highest courts would accept in the final analysis. The specific manifestations of that social or collective criterion could be altered smoothly and continuously as the system evolved via a continuing re-interpretation of the law by the courts. [Zingler 1974 p.335]

The use of legal or economic coercion which was unreasonable would be disallowed. It should be noted that Commons viewed his method of analysis as universal and applicable to any form of society. A Communist or Fascist society would simply have different proportions of the three types of transaction, different institutions and different working rules [1934 p.7].

Change in institutions also would come about through transactions of individuals with going concerns. In particular, political parties held a key part in the control of the sovereign power of the state in democracies. Commons distinguished between Routine Transactions and Strategic Transactions, which establish customs and working rules of for the routine transactions. The system is therefore evolutionary [1934 p.656].

This evolution was not, however, of the same nature as that posed by Veblen. Commons interpreted Darwin as positing a dichotomy of natural and artificial selection: of purposeless and purposeful selection [1950 p.91, 1934 p.657]. Commons believed that it was possible to engender "moral selection", to direct the trajectory of social evolution. This he regarded as the distinctive feature *vis à vis* Veblen [ibid.]. The evolutionary theory of Veblen was expressly that of natural selection.

We saw above, though, that this aspect of Veblen's theory was in fact its weakest part, and was not necessary for his social psychological critique. Commons also believed that Veblen effectively abandoned natural selection, in that his workmanship instinct introduced a purposefulness into the theory [1934 p.661]. Commons main point of contention was that Veblen ignored scarcity and the need to regulate the efficient production of industry [1934, p.673 ff]. Veblen also misunderstood, Commons believed, the consequent relationship between managerial and bargaining tran-

sactions [ibid.]. There is also a more trivial claim that Veblen considered the actions of the wrong "going concern": Commons claimed that Veblen's experience at the 1901 Industrial Commission was too narrow, and that his own perusal of Supreme Court decisions since 1810 were superior data (1934 p.660).

To an extent the debate on scarcity, which is the main point of contention, is never joined. The question is whether the regulation of industry is a service to the community. Veblen believes that it is not, Commons that it is. The matter of the power of the Supreme Court relative to business is an important but secondary issue. Veblen's contention was that the business interests use cultural institutions to mould the "needs" of socialised individuals: to create scarcity. If it were not for these institutions, no scarcity would exist. This argument was not met by Commons.

In a way, the question of sovereignty is not contradictory to Veblenian analysis. The concept is derived by Commons in an almost game-theory manner.¹⁵ Sovereignty is required to solve the inevitable conflicts occurring from bargaining transactions, which themselves occur because of the transfer of legal title to scarce commodities. The sovereign state functions by working rules which enable it to decide upon reasonable behaviour. Given the institution as the fact of scarcity, the regulators of industry may well need regulation themselves. It may well be impossible or impractical to practice untrammelled *laissez-faire*. But all this is to say that it is in the best interests of business to organise a sovereign state. While the state may be sovereign to an individual going concern, it is far from clear that it is immune from control by the collective vested interests.

On the face of it, the idea of judicial control of the economy as a cure-all for the market mechanism appears naive. Raising a critique of Commons is quite problematic though, due to his obscure style. In particular it is unclear whether Commons is proposing an analytical technique or a theory of political economy. The orthodox theory, at least in principle, purported to show how economic order is derived from individual self-seeking. It is far from clear whether Commons is offering a similar

¹⁵ cf. chapter five

theory, or is offering typologies as to what to consider when investigating any given society. Zingler suggests the latter:

Commons was not the theorist that Veblen was. Rather he was a conceptualist. He developed ways of viewing economic phenomena, not theoretical constructs that had predictive value. (1974 p.336)

At times he suggests that his own concepts could be supplemented or replaced by others [Parsons 1942 p.347]. It is also hard to apply the idea of political neutrality to Commons's theory in quite the same way as to orthodox economics, since the legislature and judiciary are expressly endogenous.

This is indeed the weakness of the system as a theory. It is by no means clear either why the courts should be primary nor why they should adopt "reasonable value" as a criterion — unless, that is, "reasonable value" is identified with judicial decisions. This would then amount simply to a revision of orthodox theory which points out the need for legal institutions to enforce contract or duty, which is precisely how the theory has subsequently been incorporated into conventional economics. We shall return to this in the next chapter.

The importance of the institutional economists to the matters which we are concerned with here is not so much the answers they suggest as the questions which they raised. The questioning of the entire conception of rational economic human raises a powerful critique of both the orthodox and Austrian formulations, and the idea that even market transactions have a social, political and legal basis is a matter that *laissez-faire* economists could not ignore since it suggests that, in a sense, '*laissez-faire*' economics is a contradiction in terms — markets could not function if left alone. We shall look at the response of 'free-market' economists to this problem in the first part of the next chapter, where we will also show the shortcomings of this reply. But given that we have rejected the technological determinism of Veblen and Ayres, and the associated notions of planning, one could wonder where we can go from here. We will maintain that calls for planning simply pass all the problems on to

the polity, and in order to show how the polity can deal with them we would have to develop a non-deterministic political theory to run parallel with our theory of economy. The rejection of the concept of rational human, we will suggest, is an important first step in developing such a theory. These matters will occupy us in the second part of the next chapter.

CHAPTER 5

NEW DIRECTIONS

IN METHODOLOGICAL terms the American institutionalists raised two challenges to orthodox economic theory. The first, connected more with Veblen, was a psychological critique of hedonism. It was a denial of the entire notion that the objective welfare of individuals corresponds to their subjective wants. The question of how the subjective utility functions of individuals could be maximised was therefore an irrelevance, since these functions did not correspond to the objective well-being of the individual; whose wants were not exogeneous, but were manipulated by the major players in the political economy.

In this critique, maximising 'utilities' would not correspond to an optimal outcome even at the individual level. The other strand of criticism concerned itself with showing that even if individual utility functions were to be taken as given, an optimal social outcome would not result from individuals following their own self-interest. Mitchell believed that business cycles were caused by the social institution of money; Ayres that technology needed to be accounted for in the calculation of the social optimum, and Commons suggested that social institutions needed to be deliberately designed so as to restrain individual action, in particular by legal institutions.

There was also by the 1960s a growing doubt amongst orthodox economists themselves concerning the structure and method of the discipline.¹ At about the same time a growing body of literature arose which attempted to reconcile the orthodox theory with an analysis of institutions, and it is these theorists, whom we shall call the 'new institutional economists',² that we shall look at here.

The acceptance of the concept of the institution marks the final abandonment of

¹ 'There is, we are regularly told, a 'crisis' in economic theory. Indeed, over the past 20 years this crisis has been almost permanently evident because it seems to have been the favourite theme for the Presidential Address to the Royal Economic Society and the American Economic Association.' (Whynes 1984 p.3) For a listing see Nelson and Winter (1984 p.3).

² The term was coined by Oliver Williamson. The NIE are not to be confused with modern 'neo-institutionalists', who are followers of the American Institutional tradition: see Dugger (1983 fn. 1) and the discussion below.

the traditional or 'classic' positive approach to economics. We shall see that the idea of the congruency of explanation and prediction, which was the hallmark of positivism, has necessarily to be abandoned in favour of a functional or evolutionary mode of explanation. Furthermore the demarcation of the economic and the political, the hallmark of orthodox economics, must also be abandoned. If it is accepted that the institutional set can have a non-negligible effect on economic behaviour, then the *laissez-faire* economists has to establish the primacy of the economic, and cannot simply ignore the political. Without an explanation of the social and political, there can be no explanation of the economic, given the existence of institutions. The value-neutrality of *laissez faire* economics, it will be recalled, rested upon the idea that we could have a science of 'human nature', and also that individual actions would lead naturally to a social optimum without further interference. With the adoption of institutions, it is accepted that the social and the political do influence the economic actions of humans, and therefore a science of 'human nature' must account for the political and social institutions also.

A further consequence of the abandonment of classic positivism is that the debate on policy (the derivation of which was the whole point of a neutral social science)³ has now shifted to a debate over the best institutions. We will divide the protagonists into two main schools, which we could call the New Institutionalists and the New Market Socialists, although the latter would also include many authors from within the Green movement. The new market socialists attempt to combine a measure of liberal principles into socialist arguments whilst rejecting the statism and the emphasis on production that was the hallmark of the older socialism and of the 1930s market socialist models. We will look at this school of thought in the second half of this chapter. In the first half, we will look at the work of the new institutionalists. These authors accept the need to include institutional analysis into the liberal framework. But we shall see that in the final analysis the original framework is retained.

³ See chapter one.

I. NEW INSTITUTIONAL ECONOMICS

The new institutionalists accept the need for institutional analysis, but the main thrust of their argument is that these themselves tend to evolve from individual action. They wish to retain methodological individualism, but feel that the orthodox analysis is inadequate.

Much of the work of the new institutional economists was derived from what Nelson and Winter call "a sense of general malaise afflicting contemporary microeconomic theory". They wish to move "beyond the intellectual territory claimed by modern general equilibrium theory", which will as they put it "require a theoretical accommodation with one or more of the major aspects of economic reality that are repressed in general equilibrium theory" [1982 p.5]. The school attempts to deal with what Williamson calls the "human factor" of economic organisation [1975 p.2]. Schotter views his work as "a first step in an attempt to liberate economics from its fixation on competitive markets as an all encompassing institutional framework" [1981 p.1].

In this respect the new institutional economics can be seen as a response to the old or American institutionalism, in that the the orthodox account of rationality — of *homo oeconomicus* — was now perceived as inadequate. Nelson and Winter point to the assumptions of perfect knowledge:

As theoretical representations of the problems faced by economic actors increase in realistic complexity and recognition of uncertainty regarding values of the variables, there is a matching increase in the fears of anticipation and calculation and in the clarity of the stakes imputed to those actors. Never is such a theoretical actor confused about the situation or distracted by petty concerns; never is he trapped in a systematically erroneous view of the problem; never is a plain old mistake made. It is a central tenet of orthodoxy that this is the *only* sound way to proceed; recognition of greater complexity in the problem *obligates* the theorist to impute a subtler rationality to the actors. [Nelson and Winter 1982 pp.8-9]

whereas Schotter raises a neo-Austrian critique of perfect competition

The neoclassical agents are bores who merely calculate optimal activities at fixed parametric prices. They are limited to one and only one type of behaviour — that of acting as automata in response to the auctioneer" [1981 p.150].

or as Langlois puts it

In this theory, the discipline of the market consists entirely in limiting the discretion producers have in setting the prices they will charge. One competes not by taking action but, in a real sense, by being unable to take action" [1986 p.8].

The new institutionalists were also influenced, as were the later American institutionalists, by the literature on imperfect competition and on monopoly. Firstly, as Langlois points out, the reasons why a monopoly should emerge (and by extension an oligopoly) are never explained [op.cit. p.9]. Furthermore, the imperfect competition models require an understanding of the conduct of the firms in order to examine how close the market outcome is to the social optimum [ibid.].

If the new institutionalists were influenced by their American predecessors, they were influenced somewhat selectively. The response largely ignored the Veblenian psychological critique and instead took up his (in our view discredited) employment of an evolutionary methodology. The other aspect of the old institutional critique which was accepted into the new models was Commons' definition of institutions as a deliberate restraint on individual activity, and his attempt to incorporate legal institutions into this concept. However the new institutionalists on the whole did not follow Commons in attempting to investigate deliberate, intentional restraint. Their concern was more to show how *unintentional* institutions could evolve. That is to say, although the new institutionalism acknowledged the force of the old institutionalists' arguments, "it is perhaps fair to say that this modern institutionalism reflects less the ideas of the early institutionalists than it does of their *opponents*." [op.cit. p.2] The new institutionalism can best be viewed as complementary to orthodox microeconomics, rather than conflicting with it [see Williamson 1975 p.xi], and in this respect is antipathetic to the original American formulations.

Game Theory

We will not attempt an exhaustive survey of the wide-ranging literature in this field, but will instead try to give a general outline of the school, and will draw on illustrative texts to examine the inadequacy of the new institutional response. Their task was to reconcile institutions with orthodox microeconomics. As we mentioned, this consisted in essence of a denial of the need for institutions of the sort Commons specified: a need for intentional imposed restraints, for "Collective action in control of Individual Action". Rather, as illustrated by Schotter [1981] the new institutionalists attempted to show how institutions could emerge unintentionally or spontaneously, 'by human action but not by human design'. This form of institution Menger referred to as 'organic'. They were derived from the same maximising behaviour as other economic activity, and helped allocate resources in an optimal fashion. Organic institutions arose from "the selfish interaction of a myriad of individual economic agents each pursuing his own self interest" [Schotter 1981 p.4].⁴ In fact, while Veblen's criticisms of orthodox economics' complete omission of institutional analysis had considerable force, Menger was a bad target for him to pick. Menger was actually greatly concerned with the questions of institutions [Langlois 1986 p.4]. Indeed, Menger believed that the "most noteworthy problem of the social sciences" to be how "institutions which serve the common welfare and are extremely significant for its development came into being without a *common will* directed towards establishing them", and that "The solution of the most important problems of the theoretical social sciences in general, and of theoretical economics in particular is thus closely connected with the question of theoretically understanding the origin and change in 'organically' created social institutions" [Menger 1883 p.147, cited Schotter 1981 p.4].

It is these 'organic' institutions, as opposed to the 'pragmatic' consciously designed institutions, that the new institutional school, following Menger, are

⁴ Although Schotter and, of course, Menger are Austrian rather than neo-classic, we will show that the game theory approach actually collapses into an orthodox position. We will consider the Austrian normative theory at greater length in the second part of this chapter.

attempting to derive. Thus Schotter, eschewing Robbins' definition of economics defines economics as

the study of how individual agents pursuing their own selfish ends evolve institutions as a means to satisfy them. [op.cit. p.5]

Schotter's work attempts to show how these organic institutions could evolve through the use of game theory. The game theory technique models the results of social interaction directly, by positive 'payoffs' for each combination of decisions made by the economic actors.⁵ Of particular note are those games where the pursuit of self interest apparently does not lead to the social optimal.

Schotter identifies two main types of game. The most well known type of game is the 'prisoners dilemma'.⁶ Although neither actor has an incentive to change from the sub-optimal scenario in equilibrium, since the payoff if the other protagonist does not change is lowered, both would ideally like the other to change.

A 'coordination problem', on the other hand, is where some form of coordination will always increase *both* actors' payoffs, although not always equally.⁷

The actors have preferences, but there is also an incentive to forgo their preference.

⁵ The model is usually simplified to a 2-player, 2 strategy model.

⁶ The following examples of a prisoner's dilemma game are taken from Rutherford [1989]: This matrix shows the basis of the prisoner's dilemma game.

		A	
		Co-operate	Don't Co-operate
B	Co-operate	5, 5	0, 8
	Don't Co-operate	0, 8	2, 2

There is no possibility of communicating or contracting between the players. Each protagonist benefits most if she refuses to co-operate while the other does, so that while the best joint strategy is to co-operate, the result of each single round is that the worst joint strategy — mutual non-cooperation — ensues.

⁷ This is an example of a co-ordination game:

Both of these situations appear to lead to a solution which is sub-optimal. The prisoner's dilemma appears to lead to a sub-optimal equilibrium, whereas the coordination problem suggests that no unique equilibrium exists [op.cit. p.23].

What Schotter suggests is that economic actors *repeatedly* dealing with these problems — what he calls 'supergames' — will eventually evolve 'rules' which enable the optimal behaviour to occur. These may take the form of social conventions, which are self-policing, or the economic actors may *agree* to the convention being policed externally [p.11].

Schotter calls the method of showing how institutions can arise the 'state of nature' approach. This technique considers a pre-institutional state of nature, and shows how institutions could have emerged without being imposed.⁸ The institutions which could subsequently emerge would then be *endogenous* to the model rather than given [p.21].

The institutions emerge without any form of external coordination, or any agreed cooperation [p.28]. The only information required for the economic actors is the alternative actions available and the possible payoffs. These are called the 'rules' of the game, which also define how much the player 'knows' about previous actions in the supergame [p.110]. The institutions arise as a *solution* to the game problem. They are not part of the behavioural *rules* of the game [p.29]⁹ The institutions are not

		A	
		Co-operate	Don't Co-operate
B	Co-operate	3 3	0 0
	Don't Co-operate	0 0	3 3

Unlike the prisoner's dilemma there is an incentive to find a co-ordinated solution: the problem here is simply to ensure that the protagonists co-ordinate their action. This is because both the pay-offs are equal pairs, but this need not be the case: in some situations a 43:34 payoff may occur, in which case considerable bargaining may ensue.

⁸ Schotter points out the similarity of this approach to that of Nozick (1975). See also the analysis below.

⁹ Schotter differs in this from, for example, Maron Shubik [see Mirowski 1986 p 248].

imposed, but are unplanned agreements [p.155]. The institutions are needed to supplement the market.

Social and economic institutions are informational devices that supplement the informational content of economic systems when competitive prices do not carry sufficient information to totally decentralise and coordinate economic activities.

That is, institutions help the coordination process as *per* Hayek.

... social institutions convey information about the expected actions of other agents when these actions are not perfectly coordinated by prices and consequently create incentives for such coordinated activity [p.109].

The inequality which persists under the present social institutions is not justified directly by Schotter, as with the other Austrians. Rather he simply suggests that nothing could (or should) be done about it, since redistribution would involve a *planned* institutional change. His justification of inequality — why we should *agree* to inequality-preserving institutions — is Hobbesian. We should agree because unequal social institutions are nonetheless preferable to no institutions at all

The fact that the rights assignment may be arbitrary and preserving of an unequal distribution of income merely reflects the fact that the equilibrium achieved with property rights is preferred by all agents to the state of conflict that would result if all property rights were removed, but not necessarily preferred by all agents to the state that might result under another set of property rights [p.44].¹⁰

Schotter is dismissive of the idea that institutions may be planned though. It would not be 'a good thing' for these unequal social institutions to be replaced by planned, equal ones since, following Hayek, such institutions only reflect the preferences of the planner. [p.21] Consequently, the only governmental intervention should occur when "empirical study shows that the equilibrium institutions are sub-optimal, such as with some externalities" [p.156].

¹⁰ Schotter is here following Buchanan [1975].

Schotter, following the Austrian school, attempts to show how social and political institutions can evolve through a need to supplement the information system of the market when it is insufficient. The problems (games) which occur are the result of insufficient information being available to the actors to find an equilibrium solution. This is similar to the position that we saw Hayek take earlier.

Transaction-Cost Analysis

The problem which Williamson [1975] raises is that, in his view, market prices are very infrequently adequate information for exchange, and he believes that institutions evolve less as a supplement than in order to by-pass the market. He is somewhat more orthodox than Schotter though, in that the rationality problems with which he is concerned are not so much *calculational* as *computational*. Recalling the 1930s socialism debate we looked at earlier, the problem for Williamson is not so much that the market does not contain sufficient information (the calculational problem) as that the individual cannot process the information quickly enough. This is what H.A. Simon referred to as *bounded rationality* [Simon 1959]. Simon suggested that the computational ability of human actors was limited and could never be sufficient to successfully compute the optimal strategy in order to constantly maximise utility (or welfare or whatever). Simon distinguished in his later works [Simon 1976] between *substantive* rationality which is precisely the sort of goal-oriented behaviour of the orthodoxy, and *procedural* rationality, which considers behaviour as a *process* of reasoning. The study of behaviour under procedural rationality is concerned not with the goals of action, but with the process by which decisions on the action are taken. Simon had earlier suggested that, given the impossibility of maximising utility, a more realistic analysis of human behaviour is that

Economic man is a *satisficing* animal whose problem solving is based on search activity to meet certain aspiration levels rather than a *maximizing* animal whose problem solving involves finding the best alternatives in terms of specified criteria [Simon 1959 p.277].

However this 'satisficing' aspect of bounded rationality has been largely ignored by the new institutionalists,¹¹ who have instead focused on the costs of acquiring information. It is in this sense that it is used by Williamson [1975]. For Williamson, it is problems of bounded rationality caused through uncertainty that are of relevance, together with the belief that transactions are costly.

Williamson also differs from Schotter in that he explicitly evokes J. R. Commons and his concept of the *transaction* and attempts to use it to explain the growth of large firms through vertical integration which bypasses the market, thereby (according to Williamson) economising on both bounded rationality and transaction costs.

Williamson, building on Coase's work on transaction costs [Coase 1937] maintains that the transaction — the passing of legal control — would occur either in the market or the firm depending on which was more efficient. The contracts involved in specifying transactions, he held, were complex and therefore costly to administer. These costs would vary both within the market and within the firm. The relative efficiency of the internal structure of the firm would effect the outcome of the internalisation of the transaction. The main thrust of the argument is that the cost of drawing up and executing complex contingent contracts is high, and as a result firms tends to adopt the most efficient internal organisation — which Williamson suggests is hierarchical — and to integrate vertically to by-pass the market and cut costs [pp.3-9, p.254].

The reason for the high costs is not simply a function of the market, Williamson believes, but rather due to the factors in the business environment which are often left out of orthodox economics — uncertainty and what Williamson calls "small numbers exchange relations" (i.e. oligopsony and oligopoly). However, it is not these market conditions alone which cause the competition and enforcement of contracts to be costly, but rather their combination with the aspects of rationality that the orthodox ignores which explain the high cost of complex, contingent contracts.

¹¹ See the argument below.

These aspects were bounded rationality and what Williamson calls 'opportunism' [p.9], which he defines as "self interest with guile" (i.e. lying) [p.26]. It is the combination of these which caused problems within market transactions. Lying during a market transaction would be self-defeating if there were a large number of bidders in the market [p.27]. By the same token bounded rationality would not be problematic unless there was uncertainty [p.22].

Williamson, then, is attempting to explain market structure by reference to the very areas which orthodox analysis leaves out. Furthermore, Williamson does not even assume homogeneity of product, and he also points out that even if competitive markets for homogeneous products may have ensued at the initial contracts the renewal of contracts may be for an heterogeneous product supplied by a monopolist or oligopoly [pp.27-28]. But while these elements may show why markets fail, Williamson has also to show why internalising the transactions should be superior, and why such internalisation should necessarily involve hierarchy.

These factors occur because of what Williamson calls *information impactness*. Because of the existence of opportunism, often the information required for a transaction is, according to Williamson, distributed unevenly between the parties, or alternatively the cost of acquiring the necessary information for a contingent contract is prohibitive [p.32]. The costs of such a transaction may then be lowered through vertical integration [p.255]. In addition, Williamson believes that bounded rationality can contain both computational and linguistic elements [p.254]: knowledge may be difficult to fully communicate. Both these elements, Williamson implies, are distributed unevenly among the population. It is for this reason that hierarchies emerge, since "those whose rationality limits are less severely constrained than others are natural candidates to assume technical, administrative, or political leadership positions ..." [p.24].

The analysis of hierarchies is therefore taking on a distinct evolutionary aspect. The relevant abilities for leadership and management are unevenly distributed, and the most cost-efficient organisation is where these people 'rise to the top'. Indeed,

this aspect is actually similar to the Mises account of entrepreneurial talent that we mentioned earlier. Williamson believes that egalitarian or democratic decision making structures — what he calls 'peer-groups' — are inefficient compared to hierarchies. He first of all points to the inefficiency of a fully democratic decision-making structure where everyone is consulted about all decisions. Obviously, this process would be far too slow for use in business. But even if a 'centralised' decision-making structure was agreed (perhaps by rota), this would come under increasing strain through "differentials of bounded rationality", that is, different ability levels [pp.45-47]. Given self-interest, it can be seen that a hierarchy will eventually evolve¹²

Williamson is therefore critical of attempts by government to intentionally intervene in the functioning of the resulting integrated firms and large conglomerates since these represent the most efficient method of lowering transaction costs and ensuring sufficient management [op.cit.p.159-60, cf. Dugger 1983 p.106]. Williamson's concern is to show, like Schotter, how a socially optimal solution can emerge even when markets fail to supply this, and how institutions are evolved to supplement the market mechanism. Williamson's model is also evolutionary, since it outlines the emergence of institutions, and proposes a selection process (cost-efficiency) whereby the 'best-fitted' institutions are sifted out. In fact, generalising from these authors, we can see how the explanations of the new institutionalists must tend towards functional-evolutionary modes of explanation, precisely because with their rejection of the orthodox 'rational economic human', and with their insistence on explanations of social end-states which are unintentional (this being justified because insufficient information emerges for rational choices to be relevant), no predictive element is left in the theory. There is no hedonist-style 'theory of human nature' which can be employed as a predictable element within the theory. The marginalist theory could predict social optima because 'human nature' was known and human behaviour was known. This enabled the congruence of explanation and pred-

¹² Williamson does suggest that some peer groups may be desired 'for themselves', presuming the loss of efficiency is not too great [p.55].

iction, which we saw in chapter one to be the hallmark of positivism, to be maintained. Here prediction is denied — all explanations are *post hoc*.

By removing the predictive elements in economic theory, and thereby positing explanatory theories which have no predictive element (and therefore no possibility of enabling social control), the new institutionalists ensure *post hoc*, evolutionary/functional explanations.

Evolutionary Theories of the Firm

We can see this in the work of Nelson and Winter [1982]. These authors explicitly adopt an evolutionary model in their work.¹³ Their theory "emphasises the tendency for the most profitable firms to drive the less profitable ones out of business". Their quibble is not with the concept of self-seeking, but rather with its application to individuals [p.51], and with the idea of perfect knowledge and of automatic profit-maximising. Their operative concept is that of organisational profit-seeking [p.31]. They have a "partial accord" with the ideas of competition and equilibrium [p.32], but believe by focusing narrowly on the concept of perfect competition and equilibrium models the orthodoxy goes too far. What Nelson and Winter wish to look at is the competitive *process* [ibid.].

The basis of their evolutionary theory is that most business behaviour follows regular and predictable patterns. These patterns of behaviour they call *routines*. It is these routines that play the same part in the economic evolutionary process that genes play in the biological theories of natural selection. It is the routines that enable firms to adjust to changes in the environment [p.134 ff]. These routines are classified into three types.

1. Decisions concerning fixed quantities of factors (similar to Marshall's 'short-run')
2. Decisions on periodical change in capital — investment.

¹³ Nelson and Winter's methodology is not Darwinian but Lamarckian, stressing adaptation and 'inheritance of acquired characteristics' [p.11].

3. Routines which modify decisions in class (1) over time.

The first class of routines are the day to day "operating characteristics" of the firm. The second group provides the stochastic elements in the model, since investment is linked to profitability. Profitable firms will therefore expand, at the expense of unprofitable firms. The third category of routine Nelson and Winter call "searches". These are characterised by a "routine-guided, routine-changing process". The changing of routines enables firms to 'mutate', to use a biological analogy. The authors believe that this model of decision-making is sufficient to explain the dynamic evolutionary outcome of a competitive market. As they explain it, the current operating characteristics determine factor inputs and outputs. These decisions determine factor and product prices on the (given) market, which therefore determine profitability, which in turn affect investment. A new set of input and output levels are thereby generated from the new level of capital stock. All the while the operating characteristics are being slowly changed by the firms' search routines [pp.14-19].

The organisational routines are similar to individual skills, according to Nelson and Winter. They believe that much of this skill is personal and tacit, and is not transferable, and cannot be widespread [p.76 ff]. It is for this reason that firms' reactions to a dynamically changing environment differ.

Critique

While much of Nelson and Winter's book is concerned with specific descriptions of the development of these routines, these need not detain us here. Our criticism of the evolutionary methodology will be at a more fundamental level. We can see how even Nelson and Winter — who are closest to the orthodox views of rationality — are relying upon *post hoc* explanation. There is no attempt at explanation of *which* routines are likely to be profitable, but merely something of an assumption that these exist. Similarly, in game theory there is no mention of just *how* organic institutions emerge through supergames, but only that such a process would be socially

optimal.¹⁴ Exactly *what* abilities are to be found in the leaders of hierarchies is not discussed by Williamson. The new institutionalist attempt at *post hoc* explanation is actually a *post hoc ergo propter hoc* fallacy, and by offering such a fallacy as a cause for events, they tend to offer undue justification for the status quo.

In fact, the new institutional economists use a very particular form of causal explanation. They explain not so much the historical emergence of institutions as the '*raison d'être*' for these institutions to exist [Langlois 1986 p.21]. They offer a *functional* explanation of institutional development — Williamson, in fact, often 're-interprets' historical institutions in logical terms [Dugger 1983 p.102]. But functional explanations have to be couched in terms of social benefit. The problem faced here — as with the positivism of the orthodox economists — is to show how this social optimum can be derived through methodological individualism. Either the historical institutions must be shown to be irrelevant for present choices, or the functional institutions must be traced from some sort of pre-institutional 'state of nature' [Rutherford 1989 p.302], as Schotter sets out to do.¹⁵

Unfortunately the presumed 'state of nature' is no more natural than the post-institutional world, which we can see very clearly when examining game theory. Recall that Schotter's neo-Austrian formulation was that the rules themselves were not regarded as institutional: the institutions emerged as the solution to the game problem. Nonetheless, game theory does take some rules or institutions as given. The notion of the supergame, for example, itself presumes that the game is capable of repetition [op.cit p.305]. The supergame concept is inoperable if a non-optimal response is catastrophic [p.308]. Furthermore the entire structure is not so much abstract as artificial:

Game theorists sometimes become so enamoured of the mechanics of the theory and the single-minded determination of their players to win that they lose sight of what any game-theoretic problem presupposes: the arena in which the players are to compete or cooperate ... although one can investigate with

¹⁴ See below on 'learning'.

¹⁵ Nelson and Winter actually seem closer to the marginalist position of simply abstracting from institutions.

game theory the dilemmas possibly faced by two prisoners, one should not expect from such a theory an explanation for why escape or insurrection is not part of the strategic space ... the arena of any interactive game is partly determined by resources and technologies, but the social norms that pervade the atmosphere are an equally important characteristic of that arena [Field 1984 p.703].

Furthermore, the supergame must posit a constancy among the economic actors [Mirowski 1986 p.252]. Indeed, the 'rules' of the game, as we noted, posit very stringent restrictions on the information the players are presumed to obtain.

Players are assumed to 'learn' from past players of the game but this learning is constrained to a very small sub-set of experience: they are allowed neither threat strategies nor to be different from other players and cannot remember past the last immediate play of the game [op.cit. p.251].

In fact, no real 'learning' is ever done. "Schotter, like many other latter-day Austrians, shies away from explicitly discussing *learning*, as opposed to the transmission of a discrete and seemingly prepackaged commodity called *knowledge*, because the former suggests a social process, whereas the latter conjures up the grocer's dairy case" [Mirowski 1986 p.252, cf. Field 1984]. Game theory abstracts from the entire social and cultural elements of individuals, complete with customs, language and *rules* [Field 1984 p.688 and passim]. In fact, if game theory is to be consistent, these elements should also be explained in terms of efficiency. These 'basic' rules must be endogenised into the model. This it does not do [Field 1984 pp.695-96]. In fact, the individuals in the game theory models are just as abstract as the orthodox *homo oeconomicus*.

In effect, Schotter defines the 'problem' to be so straight forward and unambiguous that only one choice can be made; it is not so much learning as it is mechanism ... It is difficult to maintain that this model transcends the passive cooperation of the zombies found in conventional neo-classical general equilibrium [Mirowski 1986 p.252].

So in fact the economic actors never change throughout the supergame. In addition the objectives and the environment are also given in the rules, and held to be constant. Actually no economic game is played at all and no problem solved: the rules,

as for the orthodox theory, determine the outcome in advance.

Given the fixed actors with their fixed objectives and fixed rules, the analyst (and *not the actors*) pre-reconciles the various sets, insists the pre-reconciled outcome is the one that will actually obtain and calls this a solution ... The process in which the actors take part is irrelevant, because the deck has been stacked in a teleological manner. [op.cit p.253 original emphasis]

And what is more, since the models assume this constancy, they cannot consider historical change [Mirowaki 1986 p.255], which is precisely to say that they are unable to show the evolutionary adaptation to a changing environment which Schotter began by claiming is a necessary requisite of economics. In fact, the new institutionalist school relies upon essentially the same fundamentals as the orthodoxy it hoped to supplement [Hodgson 1989 p.249]. It still holds the same hedonistic approach, using a self-seeking and abstract individual, and may be no more dynamic than general equilibrium analysis. We have just seen how game theory, like general equilibrium, models an unchanging world. Similarly, Williamson actually has a notion of rationality not too dissimilar from that of the orthodoxy, particularly because he misunderstands Simon's concept of the bounded rationality [Hodgson 1989 p.254]. Simon's point was that because human computational ability is limited, agents cannot maximise, and therefore strive for acceptable minima. The problem for Simon is not scarcity of information but of the complexity and quantity of information [Hodgson 1988 p.81]. The problem of information is not therefore simply that of its cost.

... 'satisficing' does not amount to cost-minimizing behaviour. Clearly, the latter is just the dual of the standard assumptions of maximisation; if 'satisficing' was essentially a matter of minimising costs then it would amount to maximising behaviour of the orthodox type.

But this is clearly Williamson's interpretation:

... Williamson adopts the orthodox, cost-minimising interpretation of Simon and not the one which clearly prevails in Simon's work. In Williamson's work 'economizing on the transaction costs' is part of global, cost-minimizing behaviour, and this is inconsistent with Simon's idea of bounded rationality. Whilst Williamson recognises some of the informational problems, the fact that the cost calculus remains supreme in his theory means he has not broken entirely from the orthodox assumption of

maximisation [op.cit p.254].

Williamson therefore holds a similar notion of rationality to that found in orthodox models. He also employs the general equilibrium static concept of cost when he considers transaction costs. As we saw in chapter three, the entire problem of costs was that these were subjective. Yet in Williamson's treatment, they appear to be objective — in fact, they have been given primacy. It is unclear why this should be so.

Following the lead of George Stigler's classic article [1961], search and information costs could be accommodated alongside, and treated similarly to, other costs in a probabilistic framework. In this approach information is treated just like any other commodity, and subject to the marginalist rule that its composition is optimal when the marginal cost of information and acquisition is equal to its expected marginal return [Hodgson 1988 p.201].

The 1930s 'London debate', it will be recalled, identified this question of costs as a pivotal issue. The whole argument stemmed from the Austrian critique that the minimal cost of production was unknown; it was therefore impossible to judge *a priori* how economic actors would behave. If the costs of production were objective, it was then conceded that production could in fact be centrally planned [cf. Hodgson 1989 p.80]. A similar criticism is raised by Dugger, who points out that in the Williamson model it would be possible to extend the analysis to the state.

Although not emphasised by Williamson this advantage of internalisation over contracting out could be applied to numerous government purchases ... The state should not contract out to the private sector for goods and services so readily ... Yet, it must be noted, Williamson does not emphasise these public policy implications, though they are of immense practical import. This monumental omission in his transaction cost analysis yields a glimpse into his philosophical orientation. Although he views the firm as an efficient transaction-cost economiser he does not extend his vision to include the state. [Dugger 1983 pp.99-100]¹⁶

¹⁶ It could presumably be suggested that the argument concerning hierarchies could be extended to the political sphere, whereby competing governments attempt to economise on 'bounded rationality' by instigating hierarchies. Williamson mentions that his approach is well suited to the analysis of non-market organisations: this 'social Darwinism' would be an extension of that. Certainly, the domain of the transaction-cost model has not been specified. See also Olson [1965] for an extension of economic concepts into the polity.

Returning for a moment to the Austrian critique, it will be recalled that the *value* of markets in capital was to ensure control over management. Yet Williamson necessarily maintains that this control is inadequate due to "information compactness" [p.142]. Yet without even the market or the state as a regulator, it is far from clear what is to stop corporate power running amok.

If not anti-trust and the market, if not the public purpose and planning, then what? Is any kind of social control over corporate power needed... [Dugger 1983 p.107].

Indeed, any notion of power is non-existent in both Williamson's formulation and the new institutionalists in general [op.cit. p.108]. At no time do the market institutions coerce or influence either individual actors or the social or political institutions. The new institutional economics attempts to entirely remove the possibility of power — all multi-national conglomerates are serving the public interest by minimising costs, legal institutions emerge because they are the most efficient at distributing property rights, and optimum government will necessarily emerge by rational choice. The entire programme takes on an air of unreality — try telling 'em about 'efficient property rights' in Soweto. What the new institutional programme amounts to is an attempt to endogenise institutions [Rutherford 1989 pp.301-02, Hodgson 1989 p.252],¹⁷ thereby eliminating all possibility of power. All decisions are deemed to be the result of rational action. Nonetheless, the new institutionalists only consider J. R. Commons treatment of institutions — collective action as a restraint. They do not look at the Veblenian psychological critique of rationality, but rather take the view that

— the individual can be 'taken for granted': To put it another way, the individual, along with his or her assumed behavioural characteristics, is taken as the elemental building block in the theory of the social or economic system [Hodgson 1989 p.250].

¹⁷ Langlois points out that Coase and Williamson come from a more neo-classic (sic) position, and as such do not attempt this endogenisation of all institutions [1989 p.292]. We would view this as a weakness rather than a strength though. The question of legal and political institutions and the lack of conceptualisation of power are glaring omissions in the transaction-cost analysis.

The possibility that the individual behaviour may be malleable is never met by the new institutionalists, and in this sense "the assumption of the abstract individual which is fundamental to classic liberalism is fundamental to the 'new institutional economics' as well" [op.cit p.252], as it is for the Austrians [op.cit. p.253]. They are not therefore able to consider conspicuous expenditure, cultural manipulation, the production of waste and the other machinations of the business class that Veblen discusses. Douglas North, for example, recognises and attempts to accommodate the need to include 'ideology' in economic analysis. But North's concept of ideology is not that of Marx or Veblen. His concept is endogenous and evolutionary. Ideology is "derived from the experiences people have. If all the people had the same experiences, we could expect that their perspectives about the world around them would be the same" [1984 p.35]. The sole role of ideology was as a method of deciding on the fairness of institutions [ibid.].¹⁸ No exogenous manipulation of ideology is considered.

In effect, the new institutionalism is more deterministic than the old institutionalism it seeks to replace. If the new institutionalism is indeed attempting to offer a supplement to the orthodox analysis it has to explain how a social optimum is obtained from the individuals acting selfishly. In evolutionary theories, which we have seen the school invariably adopts, this necessarily takes the form of an efficiency argument [Langlois 1986 p.21]. But as noted before, this is not an explanation of the *origin* of an institution but "at best an explanation of the persistence of the institution and only then if the feedback mechanism is specified" [Langlois 1989 p.293-94]. Our critique earlier was precisely that the feedback mechanism is indeed unspecified. Their theory is Darwinian in nature: the explanations always fall short of a complete specification of the reason of maintenance of an institution. As a result the school becomes simply apologists for the status quo.

¹⁸ North is also happy to extend the subsequent 'fair' contracting to an analysis of political institutions. Since no other influences on decisions are brooked, this appears legitimate. A more radical approach would suggest that it is ideology that affects our experiences, rather than vice versa. The contracting approach then looks far more sinister.

The orthodox analysis purported to show how a social optimal could be derived from individual actions — this was the idea of the 'invisible hand'. The new institutionalists have replaced the 'positive' economics of the orthodoxy with an 'evolutionary' functional explanation which is derived from *post hoc* observation. As we mentioned above, methodologically this involves moving to theories which are explanatory but not predictive. But as we saw in chapter one, the justification for a *science* of society was precisely this predictive nature of scientific thought. It was the ability to control and manipulate the social arena to advantage that was the *raison d'être* for the entire enterprise. The basis of the orthodoxy was that it was able to show how the 'best' policy could be determined; it was a tool for the policy scientist to use. Now that prediction has been rejected, and only *post hoc* explanations are being mooted, there is no possibility of policy being derived at all — even a policy of *laissez faire*, since no explanation of the origin of present-day institutions is given.

What is lacking is what Langlois calls a 'compositional' principle. We need to show how individual behaviour links together to form an aggregate result [1989 p.281]. He believed it was carelessness concerning this principle that caused errors in the work of the new institutionalists [p.293]. Our critique would grow further since we saw earlier that the economic composition principle — the derivation of the social optimum from the individual action — consisted in the theory of value. What is lacking in the new institutional economics is a value theory.¹⁹ Although the analysis may show how institutions could be organic, it can no longer show that these organic institutions would be optimal nor that the market complete with organic institutional supplement would be optimal.

When push comes to shove, the new institutionalists simply give us the flip side of the same methodological individualist coin as the orthodox economists. The orthodox analysis tried to ignore the social and the political by abstracting it from their purely economic theory. This resulted in an unrealistic and virtually useless theory.

¹⁹ This is Dugger's criticism of Williamson (1983 p.110), but we would suggest it is generalisable.

The reason that value theory is essential to this scheme is that it enables them to arrive at a social optimum from their individualist analysis. The new institutional economists accept that the exclusion of the political and social is not sustainable but they attempt to reduce them to economic activity. Again, this will manifest itself in value theory, which is either ignored (Williamson, Nelson and Winter) or the values are given (Schotter). None actually meet the problems raised by the theories of market failure which motivated the new theories. The orthodox economists gave us an unrealistic theory which showed how a social optimum could be derived. The new institutionalists have chosen the other path, and attempted to give a realistic theory, but in so doing they cannot show that the outcome will be socially optimal. When they attempt to do so, their theory loses touch with reality, and often becomes orthodox in nature.

II. NEW MARKET SOCIALISM

The quest for a positive science of economics has ended in failure. We showed in chapter two that the utilitarian philosophy of science was impossible to put into practice. The alternative positivist economic theory — the idea of a planned economy — was shown in chapter three to also be inoperable. Furthermore, in chapter four we have shown how the economic cannot be separated from the political and the social. The Veblenian critique was that individual wants are socially manipulated, and it is therefore futile to demonstrate how aggregate satisfaction can be maximised by the market: both are created by the vested interests. In Galbraith's phrase "... it is the process of satisfying wants that creates the wants ... the individual who urges the importance of production to satisfy these wants is precisely in the position of the onlooker who applauds the efforts of the squirrel to keep abreast of the wheel that is propelled by its own efforts" [Galbraith 1958 p.149]. Furthermore Mitchell and Commons showed how the market was itself dependent on the social institutions of

money and also of law and property rights. It is therefore not the case that the problems of economy are indeed universal for all societies, as was claimed by Robbins and others. The economic is itself defined by the society, and to claim that the 'economic problem' is universal is in fact reification.

With the inability to separate the economic from the social and the polity, the possibility of economics providing a policy science is also removed. It is not the case that the economic scientist can inform us which policies will best promote our political ends, since the operation of 'the economy' will vary according to the institutional set. Our interest should centre on this institutional set, not only upon the economy. What the optimum institutional set would consist of will be the question considered in the remainder of this chapter.

It is worth recalling from chapter one that the justification of positive social science was precisely that it did offer the possibility of policy science, the ability to control social life. This was possible because of two facets of positive science: the ability to predict and the provision of objective knowledge. We have shown that the predictive ability of economic science is so limited as to be useless. It is then unclear if the gaining of objective knowledge by itself — which would amount to simple description — is of consequence, since we cannot use this knowledge for policy. Our investigation has gone beyond positivism, and must be conducted in normative terms. We have to reintroduce the possibility of normative *political* debate, which was removed with the acceptance of positive social science, back into the policy discussion.

Freedom

It should be noted at this stage that this imperative for political, subjective analysis does not in itself preclude a policy of *laissez-faire*. It simply requires a political justification of the policy. Indeed, the most successful defence of *laissez-faire* has been conducted in precisely these terms. The point of departure for the 'New Right', derived from Austrian theory, is precisely that the market will maximise the *freedom* of individuals. In this view it is precisely the fact that the market allows the indivi-

dual 'free choice' that makes the market allocation preferable to any collective allocation [Hodgson 1984 chap.3]. The information concerning the wants and desires of individuals is necessarily dispersed throughout the economy, and any interference with the freedom of individuals to exchange will, they suggest, lead to a sub-optimal allocation of resources. Central planning, redistributive taxes and for some even legal tender are all unwarranted infringements on individual liberty.

The social institution of the market is therefore justified not only in terms of efficiency, but because only through markets can individuals obtain 'freedom'.²⁰ That is, the free market obtains a moral justification [ibid.]. We would note though, that a particular conception of freedom is being mooted here. This conception of freedom is often known, following Berlin [1969] as *negative* freedom. A person is free when she or he is "not subject to coercion by the arbitrary will of another or others". [Hayek 1960 p.11]. Freedom is simply the ability to act "according to our own decisions" [op.cit. p.12]. Only deliberate infringements on the right to make our own decisions is relevant in the denial of freedom; the ability to procure the desired results of our decisions is not pertinent to this view of freedom.

In this sense 'freedom' refers solely to a relation of men to other men, and the only infringement on it is coercion by men. This means, in particular, that the range of physical possibilities from which a person can choose at a given moment has no direct relevance to freedom [ibid.].

The point of this conception of freedom is to attempt an ethical justification of distribution. The inability to consider distribution adequately was one of the main failings of orthodox economics. Here *laissez faire* distribution is justified on the grounds of individual liberty. What is important for the libertarians is not whether I am able to give effect to my desires, but that I am able to make my own choices: "The question of how many courses of action are open to a person is, of course, very important. But it is a different question from that of how far in acting he can follow his own plans

²⁰ Although the freedom argument invariably conflates into arguments concerning efficiency. As we will see below, both negative and positive freedom are required in any realistic conception of 'the good life'.

and intentions ..." [op.cit. p.13].

The New Right often call upon a difference between freedom and ability. Simply because I am unable to climb Mount Everest does not reflect on my freedom to try. Conversely I am not free (at present) to walk in Downing Street, even though I am able. Ability is regarded as a natural obstacle, and there is no obligation to remove such a natural obstacle. The only relevant denial of freedom occurs when I am intentionally stopped from doing something I am able to do [see Miller 1989 p.33]. The results of market exchanges are viewed as precisely such a natural element, and are claimed to be the *unintentional* result of action, and therefore not a restraint on liberty. The outcomes of market exchange can be viewed as no more unjust than the consequences of the weather [Plant 1984 p.54].

The 'New Right' would also deny that moral questions can be given an objective basis. The point about market exchange, they would claim, is that it requires only a subjective view. The only commitment is to the exchange procedure — no *end-state*, such as equality, is being morally justified. Individuals are following their own values in the market [op.cit. p.58]. It is for this reason that equality is claimed to be a denial of liberty [Held 1989 p.26].

The American institutional school posed a significant challenge to this libertarian position, for it suggested that markets were themselves coercive, and that social institutions could be formative of individual liberty. In particular, J. R. Commons' use of institutions as "collective action in control, liberation and expansion of individual action" [Commons 1931 p.644] would be of special concern for the libertarians, since it destroys the neat causal link between individual freedom and the absence of coercion. Commons was suggesting that some level of coercion may be necessary for individual liberty to exist.

The reason for the emphasis of the 'New Right' on organic institutions is precisely that they wish to show that such institutions can occur through choice: no *coercion* need be involved. Our previous analysis showed that the development of these organic institutions was unlikely. We need not therefore reject intervention in the

market, nor deliberate restraint on individual action. The question is what intervention and what restraints would be best.

We may also wish to query the exclusive reliance on negative freedom of the New Right. Indeed, we may well ask why we should have any interest in negative freedom at all. What is it about coercion which is so repugnant to us?

The answer must surely be that, if we are free from coercion, we are then able to live a life shaped by our own desires and preferences and not those of another, and that is part of what the distinctively valuable features of human life consist in. However, in order to realize what is valuable about liberty, we have to be able to pursue values of our own, and to do this we have to have abilities resources and opportunities — that is to say, some command over resources so that we can live life in our own way [Plant 1989 p.65].

We would be hard put to maintain that, for example, an unemployed miner in South Africa has obtained a superior state of affairs than, say, a conscript in a well-maintained army, simply because the soldier must obey orders and the miner is not given orders (although there may be other considerations). In fact, very few libertarians would hold rigidly to a notion of exclusive negative freedom, and most would accept a right to positive freedoms in extreme cases. It would be perverse to regard maximum liberty to be the sole criterion for judging the plight of the starving children of Africa or of the homeless of New York and few of the New Right would do so. Most would concede that there is some obligation to interfere with property rights in cases of catastrophes.²¹ Yet as Miller points out, limiting this obligation to extreme cases is arbitrary. It is not made clear why a right to a minimum command over these resources should be confined to life-and-death situations [1989a p.44]. To limit the right to resources to extreme cases, or only to basic resources is arbitrary. We cannot say which resources are basic and which are not.²² We have to accept that all resources can contribute to the effective freedom of individuals [Abell 1989 p.84].

²¹ If a protagonist does insist on maintaining an ethical argument regardless, then as Miller and others have pointed out it is impossible to *prove* this argument wrong. All we can simply say is that it is ludicrous or perverse.

²² Some formulations of the Austrian theory stress the notion that ownership of resources is irrelevant, since access is the relevant aspect. This presumes in particular completely free access to credit markets, which is simply ludicrous.

Although we should accept the need for negative freedom, we should not rely exclusively upon it [op.cit. p.83]. The choice is not between market or state. Rather we should view the two types of freedom — negative and positive — as complementary, and attempt to derive the optimal balance [Miller 1989 p.25]. It is this balance that we will look at now.

We saw in chapter three that attempts at central planning — even using markets — foundered because they did not recognise that goods were produced in order to satisfy unknown and inscrutable wants. This aspect of the exchange mechanism is beneficial and should be defended. The market mechanism is an efficient method of coordinating decentralised decision-making [Estrin and LeGrand p.1]. Markets are “an efficient way of producing and distributing a very large number of mundane items, from tomatoes to transistor radios ...” [Miller and Estrin 1986 p.3]. Furthermore, we should take seriously the liberal emphasis on individual choice.

Markets give their participants a certain kind of freedom. They tend to expand the range of choices that may be made, and they give each person a variety of partners with whom to deal ... [Furthermore] if I am buying for cash, I have no need to explain or justify a request for a large consignment of salami, and this freedom to arrange my personal life in the way I happen to prefer is one whose value should not be under-estimated. [op.cit. p.4]

which is to say that “people on the whole should be left to determine their own idea of the ‘good’ and indeed of the ‘good life’” [LeGrand and Estrin p.7]. Furthermore, *competitive* markets tend to disperse personal power over the positive freedoms which we held to be important, so that we do not have to deal with petty bureaucrats [Miller and Estrin 1986 p.4] or even discriminatory practices.

Public provision of positive freedoms should not be regarded as an end in itself, but rather justified either in terms of comparative efficiency [Miller 1989b p.31] or on the grounds that ‘the good’ in this particular instance goes beyond the limitations of money measurement. We should be especially wary of confusing the allocative results of exchange with the distribution effects of inequalities of wealth and income. As we mentioned in our earlier discussion, the exchange function of markets already

presumes a given distribution; it was in attempting to theorise distribution that both orthodox analysis and Austrian theory failed as complete social theories. It should be realised that 'it is important to distinguish between the unjustifiable consequences of markets *per se* and those consequences which arise as a result of the different endowments which people bring to production' [Abell 1989 p.80]. That is to say that we must bear in mind that the lack of distributional theory in the conventional economic approach results in the need for a political decision concerning distribution as a preliminary to establishing a socially optimal allocation of goods and resources through the market. Only after the question of distribution has been adequately decided will the market allocation system deliver an optimum solution. By the same token, we must be careful not to confuse problems caused by an inadequate distribution with those which are the consequences of market allocation.

Market Limitations

Market mechanisms can have many unjustifiable consequences though, and require the existence of J. R. Commons — style institutions in order for their efficient operation. We should stress the idea that markets are in no sense natural or 'free', and that the concept of markets being somehow separate from the polity is reification. In a sense referring to 'the Market' (using the definitive article) is incorrect — all markets are set up by the society, and require a large set of laws and decisions by the polity and society for their operation, all of which alter the characteristics of the market exchange mechanism. We have already mentioned the need for a distributive mechanism to supplement the exchange mechanism of the market, and we will be examining this issue in greater depth shortly. We have also touched upon the relatively uncontentious desire to ensure a minimum provision of facilities for the abolition of poverty and to provide minimum standards of health and welfare. That is to say, we do have some notions of end-states which we wish to achieve. Although it may be possible to achieve some of the desired end-states through the market, the entire argument for markets becomes dubious when the user of the resources has

insufficient information to make informed judgement on the good or services provided.²³ Health is the usual example cited, although there may be others.²⁴ As Miller points out, such goods and services will invariably require public monitoring or regulation.²⁵ As the need for this becomes greater, the difference between public and market provision becomes smaller, and in the end there may prove to be very little difference between a good public system, and a constrained private system [Miller 1989a p.317].

We have seen other aspects of the market which may be undesirable. We have already mentioned the problem of induced wants and the conspicuous expenditure raised by Veblen. We would reject the strict determinism of Veblen though. Veblen appeared to regard individuals as completely determined by institutions which were controlled by the vested interests. It was this that enabled him to suggest an evolutionary approach to economics, which we viewed as untenable. We also rejected his view that it was *only* this institutional control which was the cause of scarcity. Furthermore, even if overall scarcity may not ensue, the decision of what should be produced remains, as we saw in chapter three, problematic.

What we can gain from Veblen is firstly that the economy can not be studied in isolation. We will need to widen our horizons to include both the cultural and the political, to establish to what extent 'wants' are formulated, and to control power over institutions. But although we may consider individuals as to some extent malleable, we should also consider that basically people are self-determining, and that subject to suitable regulation we should take their expressed wants as achieving some correspondence with welfare. After all, we cannot read minds. Unless we are to presume knowledge of individual wants — and then plan for these *à la* Veblen — then we are left with markets, problematic though they may be [Miller 1989a p.146].

²³ It may also be the case that the requisite information may need to be the subject of regulation to ensure adequate distribution.

²⁴ The subject of health also raises the problem of non-commodity conceptions of 'the good'. See the argument below.

²⁵ Or even banning, as in the case of hard narcotics.

We have also come across the problem of externalities, which will require regulation of one form or another. It is not possible to separate this regulation from the question of distribution though. The regulation of externalities through political control or through taxation will have severe distributive effects, and it is not possible to make meaningful decisions concerning the level of regulation without some notion of individual valuations of the externality, which will vary according to initial distributions of income and wealth. If, for example, we are considering restricting the use of pesticides in farms through either taxation or direct regulation (legislation or some licensing scheme), we would suggest that in the short-term at least, agricultural produce will rise in price.²⁶ This will obviously have a far greater effect on the poor than on the rich, and our view as to the level of restriction may well be affected by our view of the equitability of distribution. Recall that the question of allocation is separate and subsequent to the political question of distribution. Unless the distributive *status quo* is equitable, the consequences of market allocation cannot be optimal. Indeed, adopting a piecemeal approach to the optimisation of the environmental sector in isolation may well run into fairly severe second-best difficulties, and may be deemed to lead to a sub-optimal strategy for maximum social value.

This conclusion is not affected by the discussion on property rights. The argument of Coase was that externalities occurred because of inadequate articulation of property rights — individuals were unable to trade in the external 'commodities'. He suggested that the end result of the existence of property rights in externalities would be that the externalities are consumed at a quantity and price given by the market, regardless of the initial distribution of property rights. If I have a right to clean air, then a producer who wishes to pollute the air will need to offer 'compensation' to me for giving up this right: that is to say, my right is traded. A breach of contract (such as polluting without payment) would be actionable in the courts. This trading will occur.

²⁶ This is assuming that farmers are adept at maximising short-term profit, which seems a reasonable assumption to make. There are those who would suggest that such a move as restricting pesticides would actually have no effect on short-term profitability, but this author does not believe that we should be involved in telling farmers how to turn a profit.

and will produce an end result, irrespective of the initial distribution of property rights, which although important, is nonetheless arbitrary [Coase 1960]. All that the allocation of property rights will affect is which party has to pay for the right to control the common good.

All this is simply to say that the granting of property rights over externalities is a question of distribution which as yet we have no adequate account of. Furthermore, our view of the final result remains dependent upon the equitability of all property rights, not simply those in externalities — in fact, this is even more obvious than before. The discussion presumes that the resources brought to the bargaining are distributed fairly. It also makes the limitations of the analysis more obvious, in that some of the 'trades' that are supposed to occur seem ludicrous. It would appear that people are supposed to trade with nuclear power firms, for example, their chance of catching leukaemia, or their portion of clean air with a factory owner. It is far from clear, in any case, exactly how the individual rights are to be aggregated for trading. The property rights approach to externalities appears to rely upon the conception of organic institutions which in our view is discredited. Otherwise, it appears that all contracts for clean air are to be negotiated separately, so that my air can be polluted whilst my neighbour's air must be kept clean. In fact, we have gone around in something of a circle in the argument. Regardless of the (arbitrary) distribution of property rights in externalities, it would still seem impossible to arrive at an aggregate social pecuniary value of the externality.²⁷

It should also be realised, following from this invocation of property rights, that there are many aspects of rights over property. We may wish to separate the right of possession from the right of use, or the right of use from the right to alienate (right to the capital). There is no *a priori* reason to support full ownership of rights as opposed to other combinations [Honoré 1961, cited Miller 1989a]. This means that the partic-

²⁷ I presume it would be possible for the purposes of our examples to imagine some sort of entrepreneur who offers all the residents near Windscale (or Sellafield as it is now known) a contract to expose them to a risk of radiation, and who then negotiates with British Nuclear Fuels; I am sure the model could be made valid. Nonetheless, the model remains bizarre.

ular forms of property rights must themselves be decided upon by the polity, even before we invoke the market exchange mechanism. Recall, as Commons pointed out, that it is the legal transaction which we are concerned with, not the physical act of exchange. The polity has to make a meaningful decision as to what legal rights will be exchangeable, as well as who will have initial rights to resources.

This is particularly significant when we look at the other limitation of the market which was implicit in our discussion. Both the orthodox 'externality' and the neo-Austrian 'property rights' treatment of 'commons' hinged on the notion that everything has got a price. This has not actually been demonstrated. There may well be what Miller refers to as non-commodity elements or conceptions of 'the good'. We may well regard, say, cruelty to animals in this light or the promotion of the arts. Health and education may also be viewed as bordering on this. These non-commodity elements may involve interference with the market or with property rights. We may wish to limit the right to dispose of pets as the owners wish, or the right to bargain over health. The somewhat incongruous Austrian treatment of 'commons' has obscured this difference between commodity-based and non-commodity elements of 'the good', because it blurs the usual distinction between what is tradable and what is not. We may simply regard it as bizarre that, for example, a picturesque view or beautiful architecture should be regarded as commodities,²⁸ and absurd that they should require a monetary value. We might well suggest that clean air or public health contribute to 'the good' in their own right, and reject as perverse any suggestion that they should be tradable in the first place.

It is important to note the distinction between these non-commodity elements and our previous conception of end-states. We are not setting out, say, the preservation of picturesque views as an end in itself. Different people may have different conceptions of which non-commodity elements contribute to 'the good life'. What we

²⁸ Although, of course, a room with a view may be regarded as such. The point being made is not that picturesque scenery may not be the subject of rights, but that many people would regard the trading of rights as perverse. This results in these rights being largely ignored by the market, as is argued below.

are suggesting is that the market exchange mechanism will either not take cognizance of non-commodity wants, or more probably will price them disproportionately high, since these non-commodity elements would not be worth much in the market. Miller points to the market for part-time labour as a case in hand — the ability to work part-time is usually bought at a premium [1989a p.94-5]. Similarly, a high premium may be charged for job satisfaction, or even for the lowering of health risks at work. We may well need to interfere with the market to provide such elements of 'the good life'; indeed, many people would suggest that this is the major function of the polity.

Given this lack of provision of non-commodity elements, and also considering the problem of induced wants, we may well decide not to follow the received wisdom of orthodox economics and attempt to maximise production (as measured by money). Rather we will wish to obtain a mix of commodity and non-commodity elements. In particular, political economists such as Gorz would stress the increase of leisure as a major part of policy [Gorz 1975, 1980]. Others stress the need for individual 'development' [Hodgson 1984 p.31], which for authors such as Ivan Illich would necessarily involve the rejection of most of the consumer and capital goods presently produced. It also reinforces the requirement to base distribution on some other footing than the contribution to the production process.

It is in the sphere of distribution that the main failing of *laissez faire* can be said to occur. What we must concern ourselves with is ensuring that individuals obtain sufficient resources "in order to enter markets in an effective manner" [Plant 1989 p.66]. We must be concerned with "the conditions of freedom for the individuals who enter markets and with ensuring that these conditions embody in their institutional form the highest degree of freedom of choice" [op.cit. p.68].

This emphasis on freedom of choice was, of course, the original right-wing justification for *laissez faire*. One of our critiques of the libertarian emphasis of negative freedom was that the existence of such a social order required the derivation of social institutions — money, legal rights in property, technical knowledge and innovation and so on. We then discussed the possibility of these institutions being derived

unintentionally or 'organically'. We will now turn to another criticism of the invocation of organic institutions which will lead us to a basis for distribution. This concerns the emphasis placed on the spontaneity of organic institutions, on the fact that they lead to *unintended* consequences, which suggests that such institutions would not be coercive.

In the first place, it is far from clear why the intentions of others should be relevant to my freedom. If my freedom to act is curtailed then it remains curtailed whether or not this occurred by accident or by design, indeed the distinction is not always clear cut. Much depends on whether we believe the outcome, even though unintentional could have been reasonably foreseen. The implication of the libertarians is that because outcomes are unintended, nothing can be done about the result, but if the outcome of the social order is predictable, then we can influence the consequences and we may be regarded as being instrumental in the final outcome. [see Miller 1989a p.35]. Plant suggests that although we may be unable to foresee which individuals will be worse-off from the market exchange mechanism, we can foresee that the poorest *group* will derive least benefit [1989 p.65].

The relevant questions are therefore not what outcomes are intentional, but which we should act upon. Miller suggests that we must hold an idea of "moral responsibility" to answer this question. We must have some notion of "what people can reasonably demand of one another". Causal analysis is insufficient. We also need some ethical judgement about behaviour [1989a p.35]. Constraints on action [or inaction] can be justified morally; the debate is really over moral responsibility (*op.cit.* p.36).

Turning to the distribution of wealth and income, we can conclude that the distributive consequences of *laissez faire* cannot be justified on grounds of freedom, since the results are broadly predictable and can obviously be influenced by government action. We therefore require a moral justification of our distribution set. As Miller terms it, we must defend distribution in terms of *justice* [1989a p.58ff]. Indeed, we need to do this even to defend *laissez faire*.

Even complete *laissez faire* is a governmental economic policy; and capitalist governments escape ideological responsibility for its full consequences only by fostering a naturalised or prescriptive conception of property rights

whereas if we conceive of property rights in an instrumental fashion, the state becomes the "locus" of questions of distribution [Dunn 1986 p.37].

The question of distribution cannot be regarded as a technical question, as we saw in chapter two. We must regard the matter as an ethical question of justice, which pervades all areas of economic activity. Since the question of distribution is ethical, it cannot be decided through an exchange mechanism — the market — but must ultimately be regarded as the responsibility of the polity.

Participation

We must therefore reject what in chapter one Fay termed a 'policy science'. What is required to answer the ethical questions of distribution, to resolve disputes over externalities, to promote non-commodity elements of 'the good' and to regulate instances of market failure is what he called a participatory theory. We need to put forward a theory of politics. We should not assume as Commons did that the legal and political structures will automatically dispense an appropriate justice [see Miller 1989a p.227], not least because this justice is far from straightforward. We cannot separate distribution from market, or consider an equitable distribution (or even an equal distribution) without an effective exchange mechanism [Miller 1989b p.32].²⁹ Although the state may be the *locus* of distribution, and should accept responsibility for the distribution, it is not a *solution* to distribution. [Dunn 1986 p.38]. The task of the state is to set up the framework of the market so as to achieve the optimal balance between economy and polity.

This is especially relevant if we believe that the polity extends beyond the state apparatus. We may well hold the view such that

²⁹ A similar point was made by Little [1950].

Politics is a phenomenon found in between all groups, institutions, (formal and informal) and societies, cutting across public and private life. It is involved in all the relations, institutions and structures which are implicated in the activities of production and reproduction in the life of societies. It is expressed in all the activities of co-operation, negotiation, and struggle over the use and distribution of resources which this entails... Thus, politics is about power, about the forces which influence and reflect its distribution and use, and about the effect of this on resource use and distribution ...

This means that individual autonomy or independence can actually only be achieved through political participation.

If politics is understood in this way, then, the specification of the conditions of enactment of the principle of autonomy amounts to the specification of the conditions for the participation of citizens about the use and distribution of resources in relation to affairs that are important to them (i.e. us) ... We should strive towards a state of affairs in which political life — democratically organised, is an essential part of all peoples lives [Held 1986 pp.25-26].

This suggests participation in and democratisation of both state and market [Paine 1986 p.54]. Furthermore, since we must consider both commodity and non-commodity elements, or as Paine terms it, "a socially just distribution of 'life chances'", these being "not just income and assets, but also non-monetary components of the quality of life", we cannot make *a priori* judgements on the relationship between the polity and the economy, but this must be continually re-assessed as part of the participatory process. We need to constantly monitor the political economy to ensure neutrality between commodity and non-commodity elements of 'the good':

... a neutral framework cannot be specified in advance of knowing something about the conceptions of the good that people actually hold, and this is a contingent matter. Since the point of the framework is to balance competing claims on social resources, broadly conceived, we cannot sensibly say anything about its shape until we know what claims it has to accommodate ... Furthermore, since conceptions of the good change historically, the appropriate framework cannot be designed in a once-and-for-all manner. We need a political forum in which new demands can be heard, and the framework revised accordingly [Miller 1989a p.96].

We would therefore reject any idea of a 'blueprint' for the optimal political economy. Not only will the conceptions (and realisations) of 'the good' constantly alter, but "there is no simple way of measuring more equal rights nor participatory decision-

making, and more social justice" [Paine 1986 p.55]. Nonetheless, we would hope to be able to show that a participatory political economy would be feasible, and the main elements we would expect it to consist of.

Most 'communitarian' (as opposed to libertarian) political economists, following Nove [1983], would place an emphasis on individual and co-operative enterprises. These would be supplemented by state utilities where needed and by quasi-autonomous state enterprises. The models usually allow for some level of employment, varying from small scale enterprises (Nove), through to informal employment only [Breitenbach et al., 1990].

Before we make a brief examination of these models, we should note that the emphasis on small-scale production is more than just an attempt at breaking market power. We should regard it as a necessary part of participation within the economic process. The co-operative format both extends democracy to the workplace, and gives workers a direct reward for the progress of their firm. Not only does the emphasis on cooperation and small-scale production reduce as much as possible the alienation of the workers from the production process caused by large production lines and a hierarchical management structure, but it also is likely to increase productivity through the workforce having a greater incentive for the profitability of the firm. Although we will not have space here for a full investigation of co-operative economics, we will briefly mention several questions concerning the model of competing small-scale co-operatives which would require resolution.³⁰ The first which we have already mentioned is the possibility of hiring labour. If the economy is not to revert to a capitalist system, this must be curtailed or restricted in some way — some would even abolish it altogether.

A more awkward organisational problem concerns investment. We can identify two forms of investment strategies; mutual investment or external investment. The former is prevalent in the U.S and U.K.; members pool resources and are the

³⁰ For a full investigation of the economics of co-operatives, see Vanek [1970] and [1975], and the references in Miller (1989a p.83).

exclusive owners of the firm's assets. The latter form of investment is based on the Yugoslav model; all capital is leased at market prices,³¹ in other words all firms have a hundred per cent gearing (Estrin 1989 p.172).

The difference between the two models is not only one of finance. The real question is of the property rights in capital. In the high-g geared model the co-operative members have rights of use of capital, but do not have the right to dispose of it or to depreciate it. In the variable geared model, which historically had to compete with limited liabilities, the capital is owned by the Co-op. The Co-op itself is not necessarily 'owned' by anyone though. Although Co-ops are by definition managed democratically by the members (one member one vote), in some models a variable share holding is allowed, which gives members individual rights to the capital (finance by non-voting shares, as it were. These are the Co-operative Producer Federation rules). The alternative (Industrial Common Ownership Movement rules) is to allow only a 'membership fee' to join the Co-op; no additional equity is allowed and all assets are held collectively (Estrin 1989 p.173).

We will consider the variable gearing model first. This has the same full ownership rights as present private limited liability companies since the Co-ops are in competition with them. The fact that profits are shared equally among all members may well act as a restraint on the raising of equity though, since raising equity necessarily involves the dissolution of profits among a larger membership, resulting in a tendency to under-capitalise. In addition, co-operative members may be reluctant to risk both income and capital should the firm fail [Miller 1989a p.87]. This places the co-operatives at a disadvantage compared to limited-liability companies. [ibid.] It may also lead to problems raising loan finance, as the commercial banks would be unwilling to lend to small producer co-operatives to the same extent as private companies [Vanek p.455]. This may well occur through more than prejudice, since the higher the gearing the higher the risk involved.³² This may well make the establishment of a

³¹ One of the main problems with the Yugoslav economy is thought to be that the interest rate was well below market price, so that capital was quickly swallowed up in inefficient ventures (Miller 1989a p.14).

³² Although many large companies presently have gearing well in excess of 100%

hundred per cent geared companies near impossible if they have to compete with private companies, particularly since the larger companies would be better suited to risk spreading.

This suggests that a large co-operative sector cannot emerge from the present market mechanism in competition with limited-liability companies. A move towards a largely self-managed economy will only come into existence as a result of deliberate political intervention and will invariably be highly geared: the polity will probably adopt a one hundred per cent gearing model. This means considerable changes in company law, to restrict the forms of companies allowed to self-manage firms, but more importantly investment would require some method of organising the banking sector. Opinions differ as to whether some form of state-run enterprise or oligopoly should be envisaged if the investment banking sector should be private (or substantially private), or precisely what mixture of these should exist.³³

Other areas for political decision will arise over sectors where economies of scale are likely to occur and indeed judgements will be required as to which these are. The *technical* question of the scale of production (as opposed to the economic question of risk) would probably not be a problem in general. As Miller points out, it is feasible for co-operatives to retain the size of the individual *plants* that at present pertain under private firms. And we have already dismissed the transaction cost approach of Coase and Williamson. There may be particular areas which will require large-scale operation or investment though — in particular, technological research and development may well require large financial commitments. We would be wrong to rely exclusively on co-operatives for all production, and a variety of labour-managed organisations could be expected to emerge (labour/capital partnerships for example, or even co-operatives with both producers and consumers). It should also be restated that maximising production is not necessarily the object being pursued. Maximising investment may therefore not be of primary importance: the level of production will be a political decision.

³³ See Breitenbach et al. [1990] p.50-51, Miller (1989a p.310), Extrin (1989 p.187-90).

The appropriate market structures are not likely to emerge through the present market system, but will require a drastic alteration of the legal foundations of the market by the state. The state will also need to arrive at a decision as to how the inevitable inequalities which will emerge from a market-based economy will be handled: how much inequality should be tolerated, and how excess inequality should be stopped. The state would also be responsible for meeting that element of positive freedoms that are to be based on need, for the provision of public goods and the regulation of externalities. There will, of course, be a requirement for a judiciary and an executive — the latter especially if investment or even Keynesian style demand management is to be regulated.

Having shown that polity and economy are necessarily inter-linked, and having considered the form of economic organisation which we believe to be optimal, we can now go on to look at the polity. This will necessarily involve some consideration of our view of *homo politicus*. Our critique of libertarian political economy centred around the idea that the pursuit of rational self-interest was not sufficient to ensure the derivation of institutions. If we are to suggest that the democratic polity is to install such an institutional set, we have to maintain that the individuals are guided by a different motivation. As Schumpeter amongst others has pointed out, to simply move rational self-interest into the political sphere solves nothing — in fact, the lack of a budget constraint will mitigate against proper political management (see Schumpeter 1942).

Miller refers to this rationalist conception of political behaviour as *interest aggregation*. The participants are viewed as attempting to promote their individual interest or the interests of the group they represent,³⁴ and to guide necessary collective action towards this end. There will of course be many such competing interests, and in this view politics is a process of aggregating these interests into one [Miller 1989a p.254]. In this respect politics is analogous to economics, in that it acts as a

³⁴ This need not be rational self-interest in the narrow sense of orthodox economics. They may, for instance, attempt to seek to further a religious cause.

method of deriving an aggregate conclusion from individual wants.³⁵

Miller contrasts this with what he calls *dialogue*. Individuals enter the political process not to press their individual interests, but to voice opinion on "matters of general concern". They then enter into dialogue in which they attempt to persuade others, but can also be persuaded in turn. Viewpoints are constantly altered until a consensus emerges [op.cit. p.255].

A fairly stock response to a dialogue view of politics is that it is naive, but we could also venture a criticism of the liability of the interest-aggregation view. We might actually wonder more about the concept of self-seeking: it would seem most likely that total self-seeking would destroy virtually all modern societies — it would certainly destroy any democracy (among other things, no one would vote [Downes 1957]). Indeed, the orthodox economists have to indulge in strange contortions of the rationality concept involving "direct utility" and such like to explain the commonplace non-self actions such as parenthood, charity and so on.³⁶ It would be hard to avoid the idea that some level of altruism must exist at present.

Interest-aggregation politics will invariably contain all the same problems of its economic sibling. Unless all interests are equally represented the process will not be optimal. Also, the promotion and bargaining process measures the cost of the failure of the lobbying, rather than the quality (value?) of the interests. At any event, the aggregation procedures will suffer from all the social choice problems we outlined at the end of chapter 2: intransitivity and the other problems of Arrow's impossibility theory [op.cit. p.257]. In fact, such a mode of politics may well require a considerable amount of coercion to enforce the decisions reached — exactly the opposite of the original New Right claim [Miller 1989a p.25].

There are two aspects to the conception of politics as dialogue. The first concerns the individual as a social being, willing and able to take part in communal

³⁵ Hence Hodgson's term 'economics of politics'. The process actually described by Miller is sometimes referred to as 'log-rolling', whereby the parties bargain until a compromise position is reached which commands a majority.

³⁶ For a critique along these lines see Sen [1977].

activity. The second element is that the dialogue process is itself likely to result in superior decisions being made; "The aim of dialogue is not simply to allow participants to air their views, but to reach the best possible decision on matters of general concern" [Miller 1989a p.285].

The aim of the economy could be seen as providing the necessary security to enable people to develop and follow altruistic aims [see Abell 1989 p.88]. We would also suggest that a major concern would be to free resources to enable the equality of opportunity for participation in the political process, in terms of time and human development (education, training and so forth). It should be noted here that the conception of the altruistic individual does *not* involve any notion of 'sacrificing the individual wants for the greater good' or some such. Our conception is of *community*, where "each person identifies with the social group in the sense of seeing both his origins and his destiny as bound up with those of the group as a whole" [op.cit. p.229]. The individual should also not be viewed as entirely formed by the community: the community attachment is partial.

This conception goes beyond the idea of passive 'traditional' ties, but is a positive notion that we can organise and control the social world. [p.235].

This leaves us two areas for consideration. The first is what is to form the basis for this community — what is to form the group cohesion, and why will individuals identify with the group? The second consideration is what the limits should be on the community ties, and which matters should be regarded as individual affairs.

If we are to suggest the individual will be able to identify their well-being with that of the community, then this community will require a point of reference. "If a community is to make a claim on my allegiance, it must represent a distinct way of life; there must be something about the community and its members that makes it *my* community" [Miller 1989a p.231]. Nonetheless, the different communities will also require non-communitarian links between each other (otherwise the communities would not be distinctive), and also be of a size sufficient to exert control over their environment. This suggests that there is a play-off between small communities with a

greater participation and tighter ties, or larger groups with more control and internalised links, but a weaker sense of identity [op.cit. pp.231-36]. The dilemma is to decide the balance.

Miller believes that the common identity has to be based on nationality and citizenship. He points out that only distribution at this level would be equitable given regional variations, and that this distribution will necessarily require an ideology based on the nation-state: he does not believe that the links between smaller communities would be strong enough to invoke agreement to redistribute wealth [op.cit. p.237]. He also believes that, at any event, current identities are actually national — regardless of the lack of substance to this national identity — and that we must start with the existing identities [p.239].³⁷ This would be a point of contention between Miller and many in the Green movement, whose vision of community is more federal.³⁸ We may in this respect wonder if Miller would be able to limit the sphere of national identity in quite the manner he wishes. Recall that the community was only a part of the individual identity. We would be keen to protect areas which need not be planned from interference by the polity: we are not proposing a totalitarian state. But given Miller's appeal to national citizenship as a focus for community, it may be difficult to avoid nationalistic tendencies emerging (that is, an unswerving allegiance to the nation, together with an hostility towards other nations and towards minorities, and so on). Miller recognises this problem, but does not show how it can be avoided.³⁹

Indeed, the main problem for a community-based political economy may well be what Miller calls the problem of toleration. While wishing to limit the community

³⁷ In addition, the Green movement, because of the environmental standpoint, would place greater emphasis on the 'international'; inter-community relationships.

³⁸ Given this appeal to empirical evidence, we may wonder what idea of 'nation' Miller has in mind. Most of the existing states have severe disputes over the question of nationality. It could be suggested that this problematic question is as much an issue as the other questions we have dealt with.

³⁹ He actually likens the connection between nationality and nationalism to that between a football fan and football hooligans. Reading this during the 1990 World Cup it seems an unfortunate example — combating nationalist-based football hooliganism is very much a topical problem.

identity, it may be difficult to cope with cultural and political beliefs which threaten the sense of communal identity. Miller points out, for example, that political parties may well act for sectional interests, and we may wish to discourage political parties for this reason. [1989 p.288]⁴⁰ More problematic may be the existence of sub-cultures, which in the extreme could replace the sense of community which the polity is based around. Such subcultural allegiances would then tend to shift the form of politics from dialogue back to interest aggregation. This may be a particular problem regarding ethnic identities, which can lead to a dominant culture [op.cit. p.280] Religion may also prove problematic, as might some aspects of personal culture. It is far from clear what or indeed whether action should be taken regarding such diversity of culture.

The participatory political economy should be able to move beyond the present inequitable and iniquitous society by refusing to base its institutions around the concept of rational economic human. In doing so, it may find that the calculation of the irrational will prove its sternest test.

⁴⁰ Miller makes no mention of trade unions, but the analysis could be extended to them.

CHAPTER 6

CONCLUSION

I

WE HAVE demonstrated how the supposedly technical questions of economics cannot be separated from normative questions of society and polity. We have shown how all economic theory, along with the positivist metatheory, necessarily has an implied political theory within it. It is not the case that economics can inform us of the correct policy to follow in order to achieve a given set of political and social objectives, because economic theory itself makes presumptions concerning the legal, social and political institutions which are regarded as optimal. In particular, questions of distribution cannot be separated from questions concerning value.

The point of a neutral, value-free social science is that it enables us to manipulate the social environment to our advantage. Unless the positivist theories are able to yield such policy conclusions there does not appear to be any justification for adopting the positivist approach to social science. It is this argument that is at the root of Popper's methodology of scientific explanation. It will be recalled that Popper explicitly stated that the positivist approach to science could not be inferred from an examination of the subject-matter, but was a proposal for a convention to demarcate science from non-science. The question is less whether Popperian positivism is 'correct' than whether his proposal would be useful. We have suggested that the proposed convention may not be useful in social science because social science deals with questions of motivations which may not be susceptible to observation and falsification.

Our special concern is, of course, with economics. The particular problem which the orthodox, positivist economists had to deal with, both in the substantive economic theory and in the positivist metatheory, is that the sort of teleological, means/end

approach which is necessary to examine motivation objectively contains a tendency to yield results favouring economic planning. This means that *laissez-faire* has to be justified objectively, and the task of justifying both positivism and *laissez-faire* proved to be impossible. The basis of the problem is that a justification for *laissez-faire* involves a subjective theory based on a methodological individualist approach, whereas the justification for positivism requires a social, objective policy conclusion. Orthodox economics has never managed to reconcile these two.

In the meta-theoretical sphere, the attempts at reconciliation usually involved some weakening of the strict observational aspects of positivism, such as a claim to have insight into 'human nature' as with utilitarianism, or Robbins' claim to theorise 'economic human'. Neither was able to derive a social conclusion from their theories. The utilitarian theory was incapable of being operationalised. The Robbins rationality postulate led to an analytic theory, since economic action was identified with 'scientific', means/end action. We cannot draw policy conclusions from such a theory.

Nonetheless, the rationality postulate cannot be avoided if *laissez-faire* is to be defended on positivist grounds. The methodological instrumentalists actually remain closely tied to the position of Robbins. If, as for Friedman, theory is an irrelevance and all that counts are the results — the accuracy of predictions — it is not clear why economic planners should not be in a position to employ these predictions directly for policy recommendations. The point of Robbins' rationality postulate is to focus on *subjective* valuations as a defense of *laissez-faire*. Friedman's methodology concerns an *objective* testing of results, which cannot be used to defend *laissez-faire*. Not surprisingly, the track record of economists¹ on prediction is not very impressive.

The teleological, means/end approach to social science also presumes that the relationship between (objective) means and (subjective) ends is fixed. In fact this need not be the case — ends can also be a means for a subsequent end, and by the same token means may be viewed as previous ends. This results in the possibility

¹ As opposed to statisticians

that the means may not themselves be neutral, but can have values attached to them independently.²

This was not a problem when the means/end scheme was originally formulated by Mill. The utilitarians believed all values were capable of measurement. This scheme could not be implemented, for in order to derive a social utility from the individual utilities a unit of account was required, which effectively meant that utilitarianism could only be operative in so far as utility could be measured by money.

This of course meant that non-monetary utilities were ignored, and that utilities that were directly social ("commons") could not be compared with the utilities of individuals. We cannot move from the economic to the wider social framework. Furthermore the problems in deriving an aggregate optimum within the sphere of the economic itself are insurmountable. In particular the utilitarian philosophy, and the marginalist economics derived from it, contains a particular theory of distribution which tends towards egalitarianism. In order to defend *laissez-faire* the philosophy had to be weakened, initially by denying the interpersonal comparisons of utility, and later by switching to ordinal utility, ostensibly to move to more observable concepts. In both cases, the abandonment of the unit of account resulted in an inability to derive a social optimum from individual subjective actions.

The only group of economists to come close to deriving social conclusions from the individual analysis could be said to be the neo-classics, but even they could not find a way out of the problem of obtaining an objective, *laissez-faire* conclusion from subjective analysis. In effect, by concentrating on long-term objective costs and directly on social value the neo-classics avoided the problem of subjective value altogether, but having avoided the whirlpool they are then impaled on the rocks of economic planning: if the social optimum is measurable objectively then planning is justified.

The use of ordinal utility appeared to rescue economics both from charges of using non-observational, non-positivist concepts in their subjective theory, and from

² See also the concept of intermediate goods below.

the need to consider the egalitarian effects of interpersonal comparisons of utility. Unfortunately the theory then becomes static in nature, and no longer allows us to draw policy conclusions. In addition, the social optimum becomes indeterminate. At any event it is far from clear that the new ordinal utility is any more observable than the old cardinal utility.

Again, some welfare economists proposed that we considered the idea of social utility directly, but could not explain why some form of planning could not then take place. If *laissez-faire* is to be defended, this cannot be done on the grounds of positive science. The 1930's 'London debate' showed this up well. If we are to posit social welfare functions, or objective costs imputed from objective values, then it is indeed possible to devise centralised planned schemes to allocate resources correctly. The 1930's market socialists were simply taking the orthodox economists at their word. If the mechanism of the market did show up incorrect pricing by shortages or surpluses, then there is no real need or function for the entrepreneur. Similarly, if the producer knows in advance that maximum revenue (and the socially optimal strategy) will accrue when production is altered so that marginal cost equals marginal revenue, the producer is acting simply as a manager, and can equally well be employed by the state.

This position is unassailable within the orthodox theory. In order to attack the idea of planning the idea of an objective social optimum has to be jettisoned. The Austrian critique was precisely that the static theory which the welfare economists put forward was of no use whatever in considering real-life economic questions, which concerned reactions to *change*. No change ever occurs in the orthodox theories — all possibility of change has been eliminated from the analysis. Each economic actor has a given set of alternatives, has perfect knowledge, all market equilibria occur simultaneously and so on. Indeed, for a discipline supposedly based around choice, few decisions seem to be made.

Of course, the move towards static rational-choice theory was caused by the need to demonstrate objectively how a social optimum is derived, and much of the

weakness of orthodox theory occurred because of the attempt to avoid ethical, distributional theories. The Austrians realised this task was impossible, and met the question of objective theory head on. Their's was a normative approach, and they were quite prepared to deal with questions of distribution and the legal and political structure. Our criticism of the Austrians is not that dynamic questions and normative questions of distribution and law fall outside the purview of economics, but that the answers that are put forward are incorrect. While their critique of static orthodox theory is valid, their attempt to derive a social theory from individual rationality lacks cogency, and degenerates into a social Darwinist defense of the *status quo*.

For the Austrians, the real problems of economics revolved around responses to change, in particular the developments of new goods. The orthodox conception of a choice between *given* alternatives would appear to them not to address any questions of real-economy whatsoever. The Austrians would refuse to accept that the subjective utilities of individuals could ever be known at all. Each individual item of an economic good would, they believe, have a unique combination of elements — be it location, quality or even packaging — which makes it desirable to the individual. The Austrian view of the entrepreneur is therefore different to the 'managerial' view of the orthodoxy. The entrepreneur is viewed as a prime mover, as it were, an innovator of new goods and a taker of risks.

This critique of the orthodoxy seems cogent, particularly when one considers the production of capital goods where minor variations in quality, delivery or location can be crucial. The problem is moving from this critique to a general social theory. The Austrian theory seems to require an equal access to capital funds in order to explain inequality in terms of entrepreneurial talent. This implies unlimited credit (c.f. the concept of perfect knowledge of the orthodoxy), which is clearly nonsensical. Without this element, the Austrians are unable to deal with the possibility of market power, since the assumption of unlimited credit had the function of eliminating the possibility of any barriers to market entry.

One of the hallmarks of Social Darwinian theory is that it only gives *post-hoc*

rationalisations of events. We can see this in the Austrian theory when we ask what exactly the entrepreneur *does* to justify inequalities of income — especially between different entrepreneurs, since one would presume that the risks involved would even out after time. The Austrians — especially Mises — posit some idea of entrepreneurial "talent", but without ever specifying what this talent is to consist of. It appears that the unequal rewards are used to show the existence of entrepreneurial talent, rather than the talent justifying the inequality.

If we are to embrace normative theory and deny the desirability of an objective economics, then we must also reject artificial boundaries to academic disciplines and accept the notion of an integrated social theory. The Austrians are unable to successfully obtain such a social theory from their rational individualism. As the American Institutionalists pointed out, the individual actions were themselves determined socially, either through the need for socially constructed rules or through social and culturally determined habits of thought. They claimed that the use of rationality postulates and the attempt to derive all social conclusions from the individual actions rendered economic theories based on methodological individualism inoperative.

While this critique is correct, we would again criticise the substantive theory derived from it. Many institutionalists revert to an objective basis for value, which is untenable. Veblen's critique of rationality, for example, is played far too strong. While we would accept that human nature should be regarded as malleable, Veblen's theory is somewhat deterministic. At times he seems to suggest that the wants of individuals are almost totally artificial. Certainly the notion that without the cultural manipulation of the vested interests no shortage of goods and services would exist seems unjustified. Without such shortages there would, of course, be little need for economic theory.

Veblen was of the view that the entire economy could be run by engineers. The Austrians showed clearly that the problems of economy were not technical questions that could be solved by engineers. Mitchell went a different route to try to obtain an objective basis for value. He looked to psychology to give an objective account of

market forces. This, again, seems to suggest an unwarranted belief in determinism. Once this objective knowledge has been secured, Mitchell seems to believe, we would be in a position to undertake planning. Our argument here is that such knowledge cannot be made objective, but is subjective and inscrutable.

This is the reason why Ayres' scheme is also doomed to failure. Although more realistic than Veblen's system, the valuations needed to adjust market price would simply be non-existent, since these values are subjective. Ayres has underestimated the problems of such planning. Ayres claimed that "good milk makes healthy babies", implying that questions of the quality of, for example, milk can be solved objectively. Good milk may indeed make healthy babies, but only if it can be transported to them, packaged and stored correctly and so on. This is the 'calculation problem' which we considered in chapter three. Furthermore commodities, as the Austrians so emphatically maintained, may be of identical quality but still not be homogeneous: any variation in location or availability would be sufficient to differentiate between them. Milk available at noon is a different economic good to one available in the evening, particularly for, say, working parents. It is unclear how the market is to be 'adjusted' to take all these (unknown) variations into account.³

Ayres certainly oversteps the mark in extending the idea of objective value to political and aesthetic values. This is a re-run of Bentham, and such a system cannot be operationalised. But nonetheless we should attempt to give some account of the non-economic structures in our social theory. This is the failing of J.R.Commons. While he is correct to point out the legal backdrop of market transactions (and indeed this contribution is of great importance), he is simply naive in imagining that the judiciary — or indeed the legislature which makes the laws the judiciary are to enforce — is a completely neutral body. As Miller pointed out, we cannot simply assume that correct laws will be enacted. If we are to consider normative political economy, we must attempt an account of all areas of social theory.

³ It is also not always clear what does make well babies. Ayres was writing before the days of Thalidomide etc.

The contributions of the American Institutionalists were nonetheless significant. Firstly they raised the entire question of rationality and the malleability of human nature. They also brought the question of technology into the frame. But possibly the most significant contribution was to show that even a so-called *laissez-faire* economy was reliant upon a legal structure. Given this, it becomes far from obvious why one set of property rights rather than another should be enacted. The so-called 'laws' of market forces were themselves the results of legislation.

It was for this reason that the New Institutionalists tried to show how such legal and political institutions could be derived from the actions of rational individuals. While the theories professed to offer a more realistic approach to economic theory, they actually deliver a similar analytic analysis and conclusions which are just as vacuous as orthodox economics.

The very notion of a 'policy science' has within it an implied political theory. Fay referred to this as the "sublimation of politics", in that the notion of technical solutions to social problems would exclude non-technicians from participation in the decision-making process. That is to say that the very concept of positive social science is politically loaded, and mitigates against participatory democracy. A rejection of positivist social "science" appears to imply an account of such a democratic theory. We would however reject the libertarian political theory. We noted in particular how Hayek attempted to provide a moral justification for *laissez-faire* by suggesting a limited morality which excluded involuntary regulation. We suggested this limitation was unwarranted and arbitrary, and that it led to perverse conclusions. We would argue that both the negative freedom of Hayek and also the positive freedom of access to resources is necessary for the effective freedom of individuals.

We have seen that the exchange mechanism requires a legal backdrop, and we have also noted the malleability of human nature. The question is not whether we should 'interfere' with the market, since "the market" is a social institution in the first place. The theories and programmes we should work on would involve showing

how individuals could identify with a political structure which will be able to establish and adequately control an exchange mechanism is possible. Of particular interest would be the areas where pecuniary measurement of 'the good life' or of 'the good' is not possible, such as 'commons', non-commodity elements, commodities and services where the contracting parties lack relevant information and so on. Of vital importance would be political decisions on distribution. These are the sort of questions a participatory democratic political economy would face, but we would also wish to investigate the manner in which individuals would identify their own welfare with that of the community: we must attempt a sociological account of community. This of course also implies that we examine the boundaries that could be placed on such a community both geographically and culturally (what should be tolerated), and also that we consider the links between such communities. One major link will, of course, occur through the exchange of goods and services. These are the tasks we face.

II

Some of the conclusions we have arrived at have been borne out by contemporary events. The present author does not view it as coincidental that both the rise of the Green movement and the falling from favour of centrally-planned economies have occurred at the same time. Both can be viewed as a reaction to the failure of positive economics to offer solutions to the problem of finding the socially optimal allocation of resources. The problem of environment shows up well the shortcomings of orthodox economics (and cannot be adequately dealt with by the Austrian formulation), and also the trajectory of the movement has highlighted the perceived link between the economy and the political and social institutions. This was mirrored in the changes in Eastern Europe, and the realisation that central planning was literally not able to deliver the goods reinforced the view that the positivist planning conclusions

have failed. Unfortunately, the link between planning and positive science, and the link between orthodox economics and planning have not yet been made.

The failings of centrally planned systems are legion, and have been well documented by Alec Nove and others.⁴ The USSR system in particular was doomed to failure, as the centrally controlled economy was planned in terms of physical units. In many ways the system resembled that proposed by Veblen. Without the use of some common denominator or unit of account, and without even the slightest hint of deference to consumer sovereignty, the entire enterprise was doomed from the start:

Basically, the central plan gives exclusive emphasis to production targets in terms of single quantities. As a result, the individual enterprise is often ignorant of precise requirements. For example, plan-fulfillment targets in terms of square meters in the textile sector can lead to the production of an excessive amount of thin, fragile cloth. The firm tries to achieve the target in quantitative terms, knowing or not knowing that the cloth is too thin. If the target is expressed in terms of weight, the tendency will be to produce cloth that is too heavy. [Hodgson 1984 p.102]

The central plan was invariably overwhelmed by the necessity of such detail:

Thus when window-glass was planned in tons it was too thick and heavy; so they shifted the plan 'indicator' to square meters, whereupon it became too thin. Common sense tells us that glass should be thick or thin according to the circumstances of its use, but such detail is not and *can not* be within the cognisance of the central planning organs. Aggregation is a 'must' if next year's plan is to be drafted before the end of the century. [Nove 1980 p.6, cited *ibid.*]

Democracy is not relevant to this problem: "Such distortions could occur in the most democratic of centrally planned systems." [*ibid.*]

At no time do the producers in the economy take cognisance of the consumers. Furthermore, at no time are there any cross-valuations between different commodities: each quota is of necessity given in isolation. No adjustment of production or supply is possible. This is, as we suggested earlier, of particular significance in the production of capital goods. Stories of shipments of machine-parts with incorrect threads, or of goods being stranded in the wrong part of the country are common-

⁴ see e.g. Nove [1980], and Nove [1984].

place. Because there is no exchange mechanism, the planners require an impossibly vast array of technical knowledge to make the plan workable.

In a sense, the responses of the orthodox economists to environment consists of adding a planning supplement to their theory.⁵ By assuming everything has a price, the problem of environment appears to be a technical question of so-called 'externalities'. The problem is reduced to that of allocating 'external' costs and benefits to individuals.

The technical solutions usually revolve around treating non-traded goods as if they had been traded and allocating a price to them [Ball 1979 p.68], or alternatively adjusting the market price to a 'correct' level by taxation. This last approach especially is reminiscent of Ayres: the market is retained, but prices are adjusted to make the technical value equal the pecuniary value.

Ayres, though, was proposing these adjustments throughout the economy. The orthodox economists are proposing such alterations to market prices as a supplement to the market economy. This makes the boundary between market and plan arbitrary: there appears to be no reason for incorporating environment and excluding other non-commodity conceptions of the good life. It is also far from clear how the distributive effects of the price adjustments should be dealt with, or even why the planners should start from the distributive *status quo*. Indeed, if trade is not necessary for the valuation of goods, then it could be asked why the economist should require market valuation at all. Why stick to the planning of externalities?

Since the valuations of the planners are actually arbitrary, the figures that have been put forward by such methods as Cost-benefit Analysis and shadow pricing have frequently been somewhat strange. The official price of life in Britain in the 1970's was 9000 pounds, which meant among other things that a plane was 'worth' more than the passengers- a pilot who is in difficulty should presumably ditch the passengers and save the plane [see Ball 1979]. The actual use of CBA appears to

⁵ For a more detailed analysis of environmental issues in the light of the analysis presented here see Mulberg [1990].

reflect the values of the planners rather than any objective values, but its use does serve an ideological function, in that it legitimises these political values by giving them the sheen of 'scientific' objectivity.

This ideological function has been recognised and rejected by many within the Green movement. The movement has linked the political structure and the need for participatory democracy with the need to account for non-traded conceptions of 'the good' since its earliest conception, and much of its polemic explicitly links the disregard of non-commodity elements of the good life with cultural manipulation and economic and political power.

These factors tend to suggest that the days of formalised economic analysis based upon narrow conceptions of rationality are numbered. It has long been recognised that the world of perfect knowledge inhabited by the orthodox economists bore little relation to reality, but we have seen above that the theory is incapable of relaxing the increasingly stringent assumptions that are necessary for social policy to be derived. The orthodox theory is analytic in nature, and cannot be used to obtain policy recommendations - even if the legal and environmental boundaries of the theory are ignored. Such a science cannot be justified. Indeed, it is very notable how the 'right-wing' politicians invariably invoke Austrian theory in preference to the orthodoxy when they attempt polemical justification of their political policies, a good example of this being the former British Prime Minister's invocation of negative freedom as a defence of *laissez-faire*, and her denial of the very existence of such a thing as "society". Apart from anything else, there appears to be a clear perception of a political theory behind the policy recommendations, and a belief that the economics involved is not neutral.

Our critique also suggests the need to move away from the recent trend towards quantitative analysis. The explosion in quantitative work can be said to have largely resulted through the influence of Keynes and to an extent Mitchell. Our criticism here - and also it should be noted in the works of the Austrians - is that such research is of no interest unless the political questions of valuations are dealt with, and these,

as we have said throughout, are not technical questions. Hayek, indeed, has been critical of the very idea of macroeconomic indicators.⁶ Again, such matters as obtaining a single indicator of inflation appear to be technical questions, but are actually questions of the aggregation of individual valuations into a social value - that is, like value theory, they are political questions in a technical guise; and indeed the answers are frequently manipulated by those with political power.

If mainstream economics and quantitative analysis are to be replaced, what is to replace them? The main thrust of our argument above was that the key area is the question of aggregation — of moving from individual to social value. This question of is central to models of market socialism, and also to questions of environment, which is why this dissertation has been titled after the seminal work of Hirsch [1977]. Hirsch showed that, in contrast to the theories of both the orthodox economists and the Austrians, the process of aggregation of individual actions into a social system is far from straightforward, and can lead to undesirable results. Following a Veblenian approach, Hirsch claims that many goods are what he terms "positional" goods, which obtain value only in so far as they are not possessed by others. Such goods by definition cannot be widely distributed, and can in fact be regarded as having an absolute scarcity. These positional goods can only be distributed by ration. The production of such goods therefore involves distributional questions, and must be decided upon by the polity: the use of a purely economic approach will be increasingly insufficient to analyse the production of these goods. The particular significance of the concept of positional goods lies in its application to the environment. Elsewhere [Mulberg 1987] I have argued that the reverse analysis also holds, and that all goods which are absolutely scarce are also socially scarce. Environmental resources fall under this heading. As the scarcity of environmental resources grows, they will increasingly take on the character of positional goods, and as such will not be adequately allocated by the exchange mechanism. The conclusion of this line of

⁶ As a consequence, we might expect to see increasing contradictions between the basis for policy and the polemical justifications by right-wing politicians.

argument is that considering how market prices of environmental resources should be adjusted is a wholly inadequate approach, and that the analysis of positional environmental resources requires political and social theory as well as theories of economy.

Questions of aggregation also arise in other guises. We have looked at applications of game theory where the theorists have attempted to show how an optimum aggregate (social) solution can be obtained, and saw that these were far from cogent. What is more cogent is Hirsch's claim that often individual actions are actually second-best decisions caused by reactions to other individuals. Hirsch develops this argument into a concept of *defensive*, compensatory expenditures. The defensive actions are not valued in themselves, but are undertaken as a cost to compensate for other, undesirable actions. Leipert believes that "to an [ever] increasing extent, GNP contains transactions which as *such* cannot be given a positive value and whose activity lies only in their function of repairing damage brought about by economic activities." [Leipert 1986 original emphasis]

We can see this when we consider the concept of *intermediate goods*. This is a corollary of the problem we raised earlier in the chapter concerning the relativity of means and ends. We consume many goods not as ends in themselves, but as a means to obtain the final good. One buys petrol to travel, but a rise in the consumption of petrol caused by traffic queues does not increase welfare. The payoff to Hirsch's analysis is firstly that the aggregate measurement of welfare is not straightforward and may even be impossible, and also that economic policy must be considered at the *social* level.

This last may well prove to be the more wide-ranging change in political economy. If we have necessarily to conduct analysis at the level of the social and of the polity, we must give an account of these. The need for a normative theory means that the economist must, as we saw in the previous chapter, venture into the field of political theory. Furthermore, in order to account both for the political structure and also for the activities within the exchange mechanism - such as the manipulation of demand and the production of waste - the political economist must also venture into

the arena of social theory.

We must explain both formal and informal structures in order to examine the input of individuals into the social processes which will determine the allocation of resources and the rules of exchange, decisions which can only be made at the aggregate level. This implies a movement away from quantitative, formal *economic* analysis and towards traditional *sociology*, dealing with such questions as family, alienation and community.

We have shown in the pages above how any attempt at considering economic theory will invariably run up against social limits. We would also suggest that in order to examine these social limitations, the economist must also become a sociologist.

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