

Definitions and issues

Throughout the history of economic thought, attempts have been made to incorporate issues relating to institutions¹ and institutional change within the discipline. The most obvious example is that of the American institutionalist tradition of Veblen, Mitchell, Commons, and Ayres. But institutional analysis of various kinds can also be found in the works of classical economists such as Adam Smith and J. S. Mill; members of the German, English, and American historical schools; Marx and other Marxians; Austrian school members such as Menger, von Wieser, and Hayek, Schumpeter; and neoclassicals such as Marshall.

This book concentrates on the two major traditions of institutionalist thought in economics. The first is the American institutionalist tradition that began at the turn of the century and has continued uninterrupted (although with large swings in popularity and prestige) to this day. The second is a more recent development, but one that can be seen as a revival and considerable expansion of the institutionalist elements to be found in classical, neoclassical, and Austrian economics,² elements that had fallen into neglect in the intervening period. The former tradition is now often called the "old" institutional economics, or OIE, while the latter is usually called the "new" institutional economics, or NIE.³

1.1 The old and the new institutionalism

The old institutionalism consists of that tradition of thought associated with Thorstein Veblen, Wesley Mitchell, John R. Commons, and Clarence Ayres, and with the more recent contributions of Allan Gruchy, Wendell Gordon, Marc Tool, and the many others represented in the pages of the *Journal of Economic Issues*. Despite claims to the contrary (Gruchy 1947, 1972), the OIE does not represent a single well-defined or unified body of thought, methodology, or program of research. Within the OIE there are two research programs of major theoretical significance. The first is associated with Thorstein Veblen, and with the development and modification of Veblen's system undertaken by Clarence Ayres. This program is built around the concept of a fundamental dichotomy between the business or pecuniary and the industrial aspects of the economy. This is also expressed in a more general way as a dichotomy

between institutional and technological or between ceremonial and instrumental ways of doing and thinking (Waller 1982; Munkirs 1988). In very sweeping terms, this program focuses on investigating the effects of new technology on institutional schemes, and the ways in which established social conventions and vested interests resist such change. Institutions, then, need not be well adapted to the available instrumentalities, the criterion of judgement usually being some notion of "instrumental value," which in turn is based on a more or less broad definition of "community serviceability." These ideas are often coupled to a view of the structure of the modern economy that emphasizes the political and economic power of large corporate interests.

The second major program within the OIE has its roots in the work of John R. Commons and is now represented by writers such as Warren Samuels and Allan Schmid (Schmid 1978; Samuels and Schmid 1981). This program concentrates on law, property rights and organizations, their evolution and impact on legal and economic power, economic transactions, and the distribution of income. Here, institutions are seen largely as the outcome of formal and informal processes of conflict resolution, the criterion of success being whether the institution has generated a "reasonable value" or "workable mutuality" out of conflict. In some respects, this program is complementary to the Veblen-Ayres approach, which does not explicitly address the judicial and political processes of conflict resolution central to Commons's work, but there are points of tension and even of outright conflict between them. For example, Commons ([1924] 1968: 376, [1934] 1961: 673) himself directly attacks both Veblen's approach and his "cynical antithesis" between business and industry, and this conflict extends into the value concepts used by each (Ramstad 1989). It is also the case that the Commons tradition – owing to its emphasis on transactions, property rights, and organizations – has closer links with the NIE than does the Veblen-Ayres tradition. Nevertheless, as will be seen in later chapters, Commons's approach still differs from anything found in the NIE in significant ways.

It is probably true that the NIE is just as disparate as the old (Coats 1986; Maki 1987; Andersen and Bregne 1992). One major strand is to be found in the work on property rights (Demsetz 1967; Alchian and Demsetz 1973) and common law (Posner 1977, 1981). Another strand is that concerned with public choice processes, including those involving rent seeking and the activities of distributive coalitions (Olson 1982; Mueller 1989). A third important element deals with organizations and includes the agency theory developed from Jensen and Meckling (1976), and work on transactions costs stemming from Coase (1937) and uti-

lized extensively by Oliver Williamson (1975, 1985). Further aspects are provided by game theorists, some of whom use game theory primarily to model action within given institutional situations (Shubik 1975), while others use it in a more ambitious attempt to explain the evolution of the social institutions themselves (Schotter 1981). Many of these elements can be found combined in the institutional economic history of Douglass North (1981, 1990). The new institutionalism has also been defined to include Austrian and neo-Schumpeterian efforts to explain various types of institutional development in invisible-hand or evolutionary terms (Hayek 1967, 1973, 1979; Nelson and Winter 1982; Langlois 1986a).

As with the OIE, in some respects these programs complement each other, but differences and disagreements do exist. Douglass North criticizes both those who take a *purely* rent-seeking approach to government activity and those who limit their analysis to alterations in contracts occurring *within* a given basic institutional framework. North also argues forcefully for a much fuller recognition of the importance of notions of fairness and ideology in institutional change (North 1984, 1986). Agency theory and the transactions cost approach of Williamson have differences too. Williamson (1987a, 1990) points to differences in the basic unit of analysis and in the *ex ante* focus of agency theory as opposed to the *ex post* approach of transactions cost economics. Most agency theorists are also more orthodox in their assumptions concerning maximizing behaviour than is Williamson. More fundamentally, Langlois (1986b) argues that the NIE *should* concentrate more on spontaneous, invisible-hand, processes. He claims that certain key problems with the NIE stem from its "neoclassical core," and argues that the NIE should move in a more Austrian direction (Langlois 1989: 291–294). It is certainly possible to distinguish between a more neoclassical and a more Austrian "wing" to the NIE. The vast majority of the work in the NIE belongs in the former category (Eggertsson 1990). The latter would include Langlois's outline of a program, Hayek's work on institutions, Nelson and Winter's neo-Schumpeterian evolutionary economics, and, possibly, some of the work in game theory.⁴

Given the nature of these bodies of work, one might question the usefulness of the simple two-way division into old and new institutionalisms. Indeed, in some cases it will be necessary to make finer distinctions – into Veblen-Ayres, Commons, neoclassical, and Austrian divisions, for example – but for many purposes the Veblen-Ayres and Commons traditions can be grouped together in contrast to both the neoclassical and Austrian. This can be seen most clearly in the criticisms that each levels at the other. The new institutionalist, whether neoclassi-

cal or Austrian, complains of the old institutionalists' lack of theory; tendency to argue in holistic terms rather than in individualistic terms; use of a "behaviouristic" rather than a rational choice (or intendedly rational choice) framework; failure to give sufficient emphasis to *economizing* as "the main case"; and failure to appreciate the importance of unintended and evolutionary processes in institutional development, as opposed to processes of collective decision making and institutional design (Seckler 1975; North 1978; Schotter 1981; Coase 1984; Williamson 1987b; Ault and Ekelund 1988). The OIE is thus portrayed as descriptivist and anti-formalist, holist, behaviourist, and collectivist. Old institutionalists also reject individualistic welfare criteria and are more interventionist, favouring greater government involvement to correct institutional failures. Of course, some of these labels apply better to some old institutionalists than to others, but it is hard to deny all truth to the characterization, particularly when old institutionalists willingly apply many of the same labels to themselves (Gruchy 1947; Wilber and Harrison 1978; Dugger 1979a). It seems that what new institutionalists see as faults, the old claim as virtues.

The old institutionalists, and those more sympathetic to their position, make the opposite set of criticisms of the new. They argue that its theory is often too abstract and formal; that it sometimes adopts an extreme, reductionist, version of individualism; that the individual is seen as an overly rational and overly autonomous being, constrained, but not otherwise influenced by, his institutional and social setting; that orthodox welfare criteria are not appropriate for appraising institutional change, and that a complacent attitude prevails concerning the efficiency characteristics of markets and of institutions that emerge spontaneously (Mirowski 1981; Field 1981, 1984; Dugger 1983; Dow 1987; Hodgson 1988). The NIE is thus portrayed as more formalist (particularly in its neoclassical and game theoretic manifestations), individualist, reductionist, orientated toward rational choice and economizing models, and generally anti-interventionist. Again these labels apply more to some than to others, but again they are labels that new institutionalists have willingly applied to themselves, in part in order to clearly distinguish their work from the OIE (Coase 1984; Langlois 1986a).

1.2 Dichotomies and problems

Despite the disparity that exists within both the OIE and the NIE, it appears that the OIE and NIE can be usefully distinguished. This is not to suggest, however, that the old and the new therefore sit on opposite

sides of a series of sharp *dichotomies*, although such a view is easy to slip into. Methodological and theoretical approaches in the social sciences often are dichotomized into formalist *versus* anti-formalist; individualist *versus* holist; rational choice *versus* behaviourist; evolutionary or invisible hand *versus* collectivist; non-interventionist *versus* interventionist. There is a certain ease and comfort in this way of thinking. To dismiss someone's work from further consideration, one merely has to show that it belongs on the other side and therefore can be of no use or interest for one's own program. Each side can pursue its own programs without worrying about the work or the implicit and explicit criticisms made by the other.

On the contrary, virtually all of these standard dichotomies are false and misleading. As this book makes clear, the extreme positions are untenable, any adequate social theory cannot be strictly on one side or the other, and many social theorists do, in fact, adopt more moderate and modest positions. For example, certain criticisms of formalist notions are shared by old and new institutionalists. Also, many individualists do recognize that the social whole deeply influences the individual, while most holists do agree that only individuals, not institutions, can act as agents of change. Similarly, even "behaviourists" like Veblen do not entirely exclude rational choice and economizing behaviour, while among more orthodox economists there is an increasing recognition of the limits to optimizing behaviour and the significance of "rule following." Again, it is impossible for invisible-hand theorists to deny the need for and actual historical importance of deliberative institutional design efforts, and equally hard for collectivists to deny that many conventions and institutions can (and do) grow up spontaneously. Finally, the difficulties that are involved in judging institutional change have created much debate in both traditions, debate that ranges over the welfare criteria to be used and the proper roles for markets and governments. Even the most ardent champions of the benefits of markets and of spontaneous institutional development have had to admit that such processes can sometimes lead societies astray, while interventionists must contend with the many and obvious examples of the failure of deliberate institutional design. To argue in this way is not to claim that there are no areas of serious disagreement, only that the alternatives involved do not resolve into simple either/or choices. This expands rather than contracts the number of possible positions.

The view taken in the rest of this book is that the traditional dichotomies are best thought of as representing methodological or theoretical *problems*. The debates over the merits and demerits of formal versus less formal techniques are reflective of the difficulties involved in the analy-

sis of a complex evolving system. The arguments over holism and individualism concern, at base, the problem of the interrelationship between the individual and society. Society is created out of the actions of individuals, but it can also be argued that the individual, to some significant extent, is the creation of his social situation. Many of the debates over rule following versus rational choice models reflect a further aspect of this same problem. Clearly, most individuals do develop habits and routines and accept social conventions and norms. However, such rule following is not necessarily irrational or unconditional, although some types of norm-guided behaviour do strongly resist explanation in rationalist terms. The debate over whether to take a collectivist or invisible-hand approach to institutions relates to the question of the extent to which individuals, acting in their own interests, unintentionally generate social rules. Considerable disagreement surrounds this question. The issue of interventionism versus non-interventionism relates partly to the problem of whether such spontaneous processes of institutional development and change will necessarily operate in ways consistent with the economic and social advantage. However, particularly in the context of an evolving insituational system, terms such as "economic efficiency" or "social benefit" become uncomfortably difficult to define. These problems are common to both the OIE and the NIE. They, and the various ways they have been dealt with in each tradition, are the subject of the chapters that follow.

Formalism and anti-formalism

Formalism is defined here as the use of an abstract language such as mathematics or symbolic logic in place of natural linguistic or literary methods of presentation. Formal systems were originally developed to investigate the rules of deductive inference, and for this purpose the exact meaning to be attached to the primitive terms employed is unimportant. For economics, however, the interpretation and economic meaning of terms is important, and the use of formal methods usually involves the construction of a "model" that both embodies a formal structure and provides the basis for interpreting the symbolism.

Using the terminology of formal and natural languages may give the impression that it should be possible to translate one into the other without much difficulty. It is true that some arguments can be translated from one language into another with relatively little difficulty or alteration of the content, but this is far from always the case. To formalize a theory is not simply to make it more precise; rather, some aspects of the theory are singled out to produce a highly idealized representation of it. In this, the ambiguity of natural language may be overcome, but at the cost of losing much of its richness and suggestiveness.

Although formalism can be traced far back in the history of economics, it is associated in particular with the development of neoclassical economics. Early neoclassical contributors such as Walras, Jevons, and Edgeworth drew on the example of physics and adopted the mathematics of constrained maximization (Mirowski 1989, 1991). Despite Marshall's more eclectic approach,¹ mathematical formalism has since become one of the hallmarks of neoclassical economics. The chief advantage claimed for this mathematical formalism has always been that it increases the clarity and precision with which theoretical arguments can be stated. Formalism encourages the more explicit statement of assumptions, including initial conditions and behavioural hypotheses, and makes the derivation of implications not only more exact but also much more visible and open to examination. Although less frequently commented upon, another of its advantages is that it promotes the elaboration of sequences of models, with each model representing some development or modification to the assumptions or derivations.

In both of these respects, neoclassical economics has had considerable success. In particular, formalization has enabled theorists to build upon and rapidly develop the work of their predecessors. Of course, formalization tends to bring with it an increased degree of abstraction from the particularities of a historical or institutional type. This high degree of abstraction has, however, traditionally been seen as more of a benefit than a cost, allowing the theorist to focus attention on those factors thought to be most important or of the most *general* relevance. Thus, abstracting from many of the complexities of the real world has frequently been seen as a necessary means of gaining insight into its functioning.

Those who have criticized formalism have usually done so on the grounds that the available formalisms are inappropriate or insufficient for the explanation of certain classes of social phenomena. It is often claimed that formal methods direct inquiry to those topics, or to the use of those particular assumptions, that are mathematically tractable. Equilibrium analysis based on the assumption of maximizing behaviour is a great deal more tractable than the analysis of sequential processes of change based on adaptation, and it is not surprising that those traditions that have stressed the importance of process and raised questions concerning the use of maximizing assumptions have also been sceptical of the use of formal methods. Moreover, the same high degree of abstraction that creates generality may cause indeterminacy, indicating that the outcome reached in any particular case may depend on the specifics of the situation omitted from the model. For reasons such as these, those who reject formal methods tend to utilize more literary forms of theorizing, often (but not always) combined with studies of a historical, institutional, or case study nature.

The debate between formalists and anti-formalists has been a staple of methodological dispute in economics, often over the realism and relevance of economic theory. It is closely related to, but not identical with, the long-running debate over induction versus deduction. Formalism is identified with the study of deductive inference, and those classed as inductivists have generally been anti-formalists. Nevertheless, as the Austrian tradition demonstrates, a commitment to deductive methods need not imply the use of highly formal methods of inference. Although the induction/deduction distinction is one that is often found in the literature on the OIE, many of the issues involved have had much more to do with the appropriate degree of formalism than with induction or deduction as such. When the distinction is seen in these terms, it becomes apparent that members of the OIE and NIE have more concerns in common than is usually thought.

2.1 Formalism and anti-formalism in the OIE

According to Langlois (1986a: 5), one of the central difficulties with the work of "early institutionalists" is that "they wanted an economics with institutions but without theory." A similar argument, but in harsher terms, is made by Coase (1984: 230): "American institutionalists were not theoretical but anti-theoretical. . . . Without a theory they had nothing to pass on except a mass of descriptive material waiting for a theory, or a fire." This view of the OIE as primarily descriptive and non- or even anti-theoretical in nature is by no means uncommon. It takes support particularly from Wesley Mitchell's statistical work on business cycles and John R. Commons's vast documentation of the history of the labour movement, but the impression of the OIE as anti-theoretical or purely descriptive is quite misleading. Even the work of Mitchell and of Commons contains theoretical underpinnings and theoretical purposes (Rutherford 1983, 1987). What is true is that old institutionalists reject the more orthodox neoclassical forms of theory and model building as overly formal, abstract, and narrow. The methodological dispute here is less over theory versus description than over the appropriate degree of abstraction to be used in the analysis of a complex evolving system. Since Allan Gruchy's (1947) first major book, old institutionalists have frequently characterized the issue in terms of the less formal "cultural" or "holistic" perspective of the OIE versus the formalism of economic orthodoxy (Wilber and Harrison 1978; Gruchy 1987). Although not explicitly expressed in these terms, the criticisms of orthodox theorizing made by earlier contributors to American institutionalism are not dissimilar.

2.1.1 From Veblen to Clark

Veblen is perhaps best known for his stinging criticism of the psychological preconceptions of neoclassical and Austrian economics.² In Veblen's view, both adopted the hedonistic view of man as a "lightning calculator of pleasures and pains," a "homogeneous globule of desire of happiness" (Veblen 1898: 73). This comment of Veblen's raises a number of issues, some of which will be examined in more detail in later chapters. Within the present context, what is most significant is Veblen's argument that the assumption of the rational "hedonic calculus" combined with an assumption of a *given* institutional situation (including the "natural right of ownership") leads to an economic theory that is nothing more than the detailed and rigorous deduction of the "rational response to the exigencies of the [given] situation in which man is placed" (Veblen [1909] 1961: 234–236). Such an economics lends itself

to formal methods of presentation. The theory is a theory of the "normal" case, a highly refined and developed theory of equilibrium states, but one that simply excludes the issues of evolutionary institutional change ([1900] 1961: 164–165). Veblen even characterizes the so-called dynamic neoclassical theory not as a treatment of the underlying "phenomena of change," but at most as a discussion of the "rational adjustment to change which may be supposed to have supervened" ([1909] 1961: 232). As a result, whenever any institutional phenomenon "is involved in the facts with which the theory is occupied, such institutional facts are taken for granted, denied, or explained away" ([1909] 1961: 233).

In place of this orthodox type of theory, Veblen wanted to substitute an inquiry into the evolution of institutions and their impact on human conduct. In this, he made repeated references to Darwin and to evolutionary biology as a model of "modern science." By "modern science" he meant a focus on sequential, continuous, change explained in causal (as opposed to intentionalist) terms, and without any presumption of a final term or consummation (Veblen 1898). This institutional economics was not to be purely descriptive. He criticized the German historical school for having produced nothing more than a "narrative survey of phenomena" (1898: 72). What Veblen wished to produce was a "genetic account of an unfolding process,"³ a treatment of institutional evolution as a process of "cumulative causation." For Veblen, this cumulative process was based less on rationalistic calculation than on habituation to material conditions and constraints:

The growth of culture is a cumulative sequence of habituation, and the ways and means of it are the habitual response of human nature to exigencies that vary incontinently, cumulatively, but with something of a consistent sequence in the cumulative variations that so go forward, – incontinently, because each new move creates a new situation which induces a further new variation in the habitual manner of response; cumulatively, because each new situation is a variation of what has gone before it and embodies as causal factors all that has been effected by what went before; consistently, because the underlying traits of human nature . . . by force of which the response takes place, and on the ground of which the habituation takes effect, remain substantially unchanged. ([1909] 1961: 241–242)

The issue of habituation versus rational choice will be discussed in more detail later but Veblen's notion of cumulative causation raises two other important points. The first is the claim that a proper treatment of institutional evolution should consist of more than a treatment of adjustment to a series of exogenously given shocks. Much of the NIE fails this test, dealing not with the internal dynamic of the system but only with the rational (or intendedly rational) responses to exogenously given changes in population, technology, trading opportunities, or

ideology. The second, and related, point is that Veblen's discussion of cumulative causation involves a clear idea of path dependency. History matters in the sense that what happens next depends critically on the details of the existing state of affairs, which in turn is the outcome of the pre-existing situation. Small differences in initial conditions can make for widely differing outcomes. There is, in this, the idea of the openness of the evolution of a complex system. From similar starting points different cultures will develop in different ways; Veblen ([1908a] 1961: 230) talks of the "exuberant uncertainties of cumulative change." Of course, the process is not unconstrained and one can develop theories about the overall evolutionary processes at work and provide an analysis of institutional history. However, no particular historical or institutional development can be explained without referring to the details of the actual historical circumstances involved. As in evolutionary biology, the theory need not be able to predict the next step in evolution in order to claim explanatory value. The explanations of particular events must combine the theory with a great deal of concrete information. This is what Wesley Mitchell (1927: 54) called "analytic description." Theory is by no means excluded. Even highly formal theory can play a role in such a program, but any attempt to understand the actual course of institutional history cannot be limited to highly abstract formal models.

Veblen's own work is almost always a blend of theory with a discussion of the related historical sequence of events. Veblen does not test his theories against the factual and historical information he presents, but weaves the two into an analytic description of the process of cumulative causation. Veblen's main concern in his methodological essays, however, is to emphasize the evolutionary approach based on the idea of cumulative causation. He provides little by way of more detailed methodological guidelines and many parts of his work are open to empirical criticism. Veblen's tendency to ignore the job of critically examining his conclusions is one remarked upon by Wesley Mitchell (1929: 29).

Wesley Mitchell's own discussion of methodology also centers on the issue of the complexity of many economic phenomena.⁴ In his study of business cycles, Mitchell remarks on the vast number of competing theories, each of which focuses on a small number of causal factors and many of which seem to have at least some empirical support. For Mitchell, this "multiplicity of explanations" arose from the "complexity of the problem itself." Business activity "depends upon the smoothly coordinated functioning of many processes" and "any of the factors or processes can be made to yield a plausible theory of business cycles, provided some investigator can show that it is an independent source of recurrent fluctuations in the activity of trade" (Mitchell 1927: 48).

Mitchell's aim was to produce a "thoroughly unified explanation of business cycles," but to achieve this end he rejected the standard method of abstract model building as one that made it too easy to neglect "phenomena which do not fit neatly into preconceived schemes" (1927: 49). Mitchell's critique of the standard methodology successfully isolates a number of its major weaknesses. First, the empirical work may simply be neglected and the theory never tested (Burns and Mitchell 1946: 8). Second, the need for theoretical simplicity may result in simplifying assumptions which give a theory only a "problematical relation" to the actual world and may render it, in practical terms, untestable (Burns and Mitchell 1946: 8-9). Third, even if the theory is testable, "the worker who tries to verify it must examine the processes on which it centres attention." The test is therefore "superficial" as it is quite possible that many competing theories could all be similarly verified (Burns and Mitchell 1946: 9). This problem is compounded to the extent that the investigator is "prone to adduce only the evidence and arguments that seem to prove his explanation" (Mitchell 1927: 181). Fourth, Mitchell argues that the behaviour of economic agents is often more complex than it appears in arguments that proceed by deducing behaviour from a few assumptions such as the importance of profit for the businessman. That business is conducted for profit "is not a simple matter" that enables the theorist to deduce results with any degree of certainty. Describing his own view, Mitchell put the point as follows: "There is much in the working of business technique which I should never think of if I were not always turning back to observation. And I should not trust even my reasoning about what businessmen will do if I could not check it up" ([1928] 1936: 415).

Mitchell's conclusion was that to overcome these problems and to capture the full extent of the complexity of economic phenomena, observations should play a greater role, but he neither denied that some conceptual apparatus and working hypotheses were necessary to guide enquiry nor accepted the usual empiricist notion of induction from an objective empirical base ([1927] 1936: 59, n. 2). He considered the scientific method to consist of "the patient processes of observation and testing - always critical testing - of the relations between the working hypotheses and the processes observed," as contrasted with the method of orthodox economics of "trying to think out a deductive scheme and then ... verifying that" ([1928] 1936: 413-415). Mitchell's method does not dispense with theorizing, but, as he put it, the place for it is "inside the investigation" ([1928] 1936: 413). His aim was to bring factual research and theorizing into the closest possible contact.

Care is required, however, in interpreting Mitchell's concepts of working hypotheses and testing. For Mitchell, working hypotheses included definitions, measurements, and low-level empirical hypotheses, as well as explanatory hypotheses concerning behavioural or causal relationships. In parts of Mitchell's work, the first three types predominate, but not because he was only interested in arriving at empirical generalizations, but rather because of his views on the weakness of standard empirical verifications and his desire to arrive at a more meaningful testing procedure. At the same time, Mitchell's "critical testing" did not go so far as to imply a Popperian search for falsifiers or any abandonment of the basic verificationist idea that defines a successful test in terms of corroboration. What Mitchell rejected was the extremely uncritical process of searching for, and usually finding, only verifications, not verificationism itself. To be meaningful, a verification should take the form of empirical investigation that is at least open to the possibility of finding contrary evidence, or, what is often more important, evidence that the theory is incomplete.

Mitchell thus came to argue that empirical work should be directed at discovering new information by examining in detail the process or phenomenon to be explained within an overall conceptual framework and utilizing existing theories as working hypotheses to help guide inquiry in a close interplay between the working hypotheses and empirical findings. In this fashion Mitchell hoped to discover both the strengths and weaknesses of existing hypotheses and to define more clearly, in terms of what had to be explained, the task remaining to the theorist (Mitchell 1927: 58, [1931] 1950: 409; Burns and Mitchell 1946: 9-10). Only in this way did Mitchell think that the investigator could arrive at a full understanding of the complex interactions of the many causal elements that both produced business cycles and gave rise to their differences over time.

Commons, too, was deeply concerned with the issue of how to analyze the operation of a complex evolving whole. Commons did not want to abandon entirely the insights of orthodox theory, but he did want to build them into a more complete institutional political economy, one that would give collective action, both in the sense of collective organizations such as firms and unions and in the sense of the collectively enforced rules of custom and law, its proper place in economics. In Commons's ([1934] 1961: 5-6, 161-162, 439-440) view, orthodox theory had failed to do this for a number of reasons. First, it assumed harmony of interests instead of conflict of interests, and thus failed to bring out the need for institutionalized rules to constrain individual

behaviour. Second, orthodox theory tended to substitute given psychological propensities for what were actually customary (and evolving) modes of behaviour. And third, orthodox theory confused physical materials with the institutional aspects of property rights and thus led to the conflation of materials and ownership.

To overcome these problems and to include all relevant factors, Commons realized he would have to find a way to analyze the enormous complexity of the evolving legal and economic system. To help organize his efforts, Commons ([1934] 1961: 94) utilized what he called "principles" or "similarities of action," and "formulas" or "relations between the parts and the whole."⁵ In terms of the legal/economic system as a whole, the major principles utilized by Commons are "Efficiency," "Scarcity," "Custom," "Sovereignty," and "Futurity" (or forward lookingness). These principles interrelate according to such formulas as that of "Limiting and Complementary Factors" ([1934] 1961: 627, 737-738). When dealing with politics as a whole, the principles become "Personality," "Political Principle," "Organization," "Jurisdiction," "Rationing," "Stabilization," and "Justification" ([1934]: 753). It is this approach that gives Commons's work its flavour of being little more than a loose and often confusing conceptual framework. Commons's work does contain general ideas concerning the nature of the evolutionary processes at work (which will be discussed later), but much of his intellectual effort was directed at the testing, modifying, and policy application of his hypothesized principles and formulas. For Commons, a "theory" was "a complex activity of analysis, genesis and insight, actively constructed by the mind in order to understand, predict and control the future" ([1934] 1961: 102):

The method of analysis consists in breaking up the complexity into all the supposed similarities of behavior, and then giving to each similarity a name which designates it as a proposed scientific principle to be tested by investigation. The method of genesis consists in the discovery of changes which have occurred in the past as explanations of why the present situation exists as it is. The method of insight consists in understanding the ways of leadership and fellowship. ([1934] 1961: 753)

In this activity, Commons made extensive use of the collection of relevant documents and pioneered the use of interviews, particularly with key negotiators and decision makers. His "prime method of investigation" was the "constructive method of interviewing" ([1934] 1961: 106). Commons's case studies and his involvement in institutional reform display this methodology in action. Through his own case studies, and those conducted by his students, Commons continually modified and refined his conceptual apparatus of principles, developed

his explanations of the various factors that shaped the present situation, and sought to uncover the "negotiation psychology" he could apply in each case in order to bring about change or resolve a dispute (Biddle 1990b).

Of the earlier writers in the OIE tradition, J. M. Clark represents an interesting case. Clark was certainly more theoretically inclined than most other old institutionalists, and he made significant contributions, such as the accelerator (Clark 1917), to economics in general. Despite his greater theoretical orientation and willingness to use formal methods, Clark was especially concerned with extending economics into the realm of the dynamic analysis of economic phenomena and institutions. Clark considered this shift to dynamic analysis to be of fundamental importance:

The key to statics, as we have seen, is a problem: that of levels of equilibrium. . . . The key to dynamics is a different problem: that of processes which do not visibly tend to any complete and definable static equilibrium. The importance of this shift from the search for levels to the study of processes can hardly be overemphasized; it is not less significant than the change from static to dynamic conditions. (Clark [1927] 1967: 203)

Clark made many references to Veblen's work and it is clear that he regarded "dynamics" as having to do with process and cumulative change. He also argued for more "inductive" work designed to develop appropriate premises and an understanding of the phenomena to be explained. Clark's (1923, 1961) own work on overhead costs and the dynamics of competition are good examples of his blending of theoretical and empirical investigation into an informal dynamic analysis.

While Clark was of the view that "the complexities of these dynamic realities could not be compressed into conventional geometric or algebraic models" (Markham 1968), and argued for the development of a "non-Euclidian" economics (Clark 1921), he did not reject static or more formal models entirely. Indeed, Clark was significantly kinder to more orthodox theory than other old institutionalists. Even "in the pursuit of dynamic analysis" Clark ([1927] 1967: 226) thought that "certain aspects of static analysis will find a place." A complete dynamic analysis may not always be possible, and a more simplified static approach may be necessary to penetrate the complexities involved. In addition, static analysis may provide a point of departure for dynamic analysis, or a benchmark for the appraisal of the impact of dynamic forces ([1927] 1967: 726-728).

Despite their variety, none of the methods discussed above can be regarded as having been entirely successful. Veblen's approach resulted in broad cultural histories and interpretations of sometimes highly

doubtful validity.⁶ In contrast, Mitchell and Commons, in their different ways, often allowed their emphasis on concrete detail to obscure their more general theoretical ideas and purposes. Clark, perhaps, had most theoretical success, but he is best known for work that is closest in nature to orthodox analysis, and even within the OIE his "dynamics" has had relatively little impact. At the same time, they were all struggling with the problem of how to analyze a system that is not simple, stable, and recurrent but complex, highly interrelated, and evolving over time. This problem still faces writers in both the old and the new traditions of institutionalism, and it is a problem with no obvious or easy solution.

2.1.2 Pattern models and participant-observation

Within the OIE, the more recent literature has closely associated the "cultural" or "holistic" outlook of old institutionalists with the idea of a "pattern model" borrowed from Abraham Kaplan (1964) and Paul Diesing (1971). This is most obvious in the work of Wilber and Harrison (1978), Wilber and Jameson (1983), Ramstad (1986), and Gruchy (1987).

Kaplan (1964: 332) describes the "pattern model of explanation" as the idea that "we know the reason for something when we can fit it into a known pattern." Thus, "according to the pattern model . . . something is explained when it is so related to a set of other elements that together they constitute a unified system" (1964: 333). Kaplan's work, however, does not place the pattern model entirely in opposition to deductive models, his argument being that "both may serve a useful purpose in methodology" (1964: 333). Diesing's (1971: 137) position is quite similar, but he does see the pattern model and its related methods as particularly suitable for "studying a whole human system in its natural setting." As this seems to have a close correspondence with the objectives of the OIE, Diesing's position deserves closer analysis.

Diesing's argument begins from the proposition that "human systems tend to develop a characteristic wholeness" or unity that "manifests itself in nearly every part" (1971: 137). This is then taken to imply that the characteristics of any part and its functioning are "largely determined by the whole to which it belongs and by its particular location in the whole system." Further, the techniques of investigation and concept development *should* be such as to "somehow capture and express this holistic quality" (1971: 138). These techniques and concepts must not "distort" the subject matter by abstracting too much. The concepts used must be "relatively concrete and particularized, close to the real system being described" (1971: 139-140). More specifically, he outlines a method of

investigation that uses data collection, interviews, and, most important, the technique of the "participant-observer" (1971: 144-155).

The notion of the participant-observer lies at the heart of Diesing's method. The method begins with a case study in which the investigator is to "become part of the community or group he is studying." He must "allow himself to be socialized and accept the point of view and ideology of his hosts" (1971: 144). Through this process of socialization into the group, the investigator hopes to "be impressed by recurrent themes that reappear in various contexts" (1971: 145). These themes are then developed, interpreted, and tested by "contextual validation," that is, by the use of several sources and kinds of evidence in a process of "cross checking" (1971: 146-149). Out of many themes and concepts developed in this way the researcher gradually builds a "pattern model," the end result being "a model or account of the whole system being studied" (1971: 157). Out of many such case studies, and by use of the comparative method, a more general theory or model can gradually be constructed (1971: 182-196).

Diesing's discussion of the participant-observer method is clearly based on the methods of field research in anthropology and sociology, and on the kinds of models found predominantly in functionalist anthropology and sociology. Diesing's case study is the method of the anthropologist studying some isolated society by joining the group and becoming a part of it, or of the industrial sociologist studying the sociology of the factory floor by joining a work group and gradually becoming accepted as a member of it. Two related questions now arise: is Diesing's method suitable for the study of large-scale systems such as the economy of a modern industrialized nation, and is Diesing's method the method of the OIE?

On the first issue, Diesing's emphasis on the participant-observer *becoming* a member of the group in fact restricts this method to small groups or relatively simple social systems. Diesing (1971: 7) himself is quite clear on this point, stating that the most successful examples of his technique involve "studies of simple non-literate societies or small formal organizations." Studies of larger complex systems, he argues, require the use of a variety of methods: "Attempts to study the U.S. or world economy have necessarily involved great reliance on statistics and thus have moved toward the survey research method, which is much better suited to a large subject matter" (1971: 7). He further points out that the old institutional economists' objective of studying "the total set of institutions in which a particular economy functions, seen in historical perspective," involves great difficulty owing to the "size and complexity of its subject matter" and goes on to express the view that while there

is not currently any single institutionalist methodology, "if a unified institutionalist method is ever fully developed it will probably be some amalgam of clinical-historical, survey research, and even formal methods" (1971: 7).

In contrast to Diesing, Wilber and Harrison argue that the participant-observer method is also *the* institutionalist method: "Clearly institutionalists have been participant-observers in the sense used by Diesing" (Wilber and Harrison 1978: 75). Ramstad is more cautious, doubting that there is *an* institutionalist methodology, but claiming that "many or most" institutionalists have followed the method outlined by Wilber and Harrison, and that Commons, in particular, is an "exemplar" of that approach (Ramstad 1986: 1068–1069). The opinions expressed by Wilber and Harrison and Ramstad, however, suggest some confusion between participant-observation as Diesing means it, where the investigator *becomes* a member of the group being studied, and the broad participation of American institutionalists in economic policy making and in all kinds of agencies, commissions, and associations. This is obviously *not* the kind of participation Diesing is thinking about. The claim that Diesing's method applies in a general way to old institutionalists is simply false. Veblen, Mitchell, Clark, and Ayres do not fit at all,⁷ and Commons, who probably comes closest to being a participant-observer in Diesing's sense, and who utilized interview techniques and completed vast numbers of case studies, qualifies only in part. More recent work in the OIE provides no evidence that participant-observation is in common use, or indeed, that old institutionalist writings are distinguished by the use of *any* techniques "not 'allowed' in mainstream economics"; what does distinguish the old institutionalists is that "they do *not* use certain techniques such as mathematical modelling and 'advanced' econometrics" (Lind 1993: 8).

None of this should be taken as denying that there are some aspects of Kaplan's and Diesing's discussion that apply to the OIE. Leaving out the specific method of participant-observation, the overall idea of the pattern model does seem to capture some key elements of the approach found in the OIE. For example, in the OIE business behaviour is not formally deduced from a set of axioms, but rather related, less formally, to a general conceptual apparatus and an understanding of the surrounding institutional context. Observation is not focused so narrowly on the testing of the particular predictions of a formal model, but has a larger, although less well defined, role. It is the broad range of business behaviour that is to be explained by demonstrating its consistency with the pattern of established business goals and existing institutional constraints and incentives. Inconsistencies might raise questions about the

underlying concepts or, more commonly, the understanding of the institutional context contained in the pattern model, or about the observational evidence and its interpretation. Much of the work of Veblen, Commons, Galbraith, and other old institutionalists can be seen in this way. Of course, there is often much in pattern models that could be expressed more formally, but greater formalization is not a goal that most old institutionalists have adopted. Although Radzicki (1988: 636) has argued that "a method for adding precision, rigor, and structure to pattern modeling must be devised," and Wisman and Rozansky (1991) claim that some old institutionalists (notably Gruchy and Samuels) might have sympathy with the view of pattern modeling as a first step to the development of a more sophisticated body of theory, it is clear that the majority of old institutionalists find pattern models entirely appropriate to the subject matter of economics.

Unfortunately, the idea of pattern model explanation does have its difficulties. It is very open to uncritical "validation," particularly as a pattern containing broadly defined elements that work in opposite directions, such as the Veblenian dichotomy of institutions vs. technology, may be made compatible with almost any set of observations. Pattern modeling also encourages one to resort to functionalism or other forms of argument that run along holistic lines. However, many of the most serious problems to be found in the methodology of the OIE lie not so much in the pattern model in and of itself, but in the almost total rejection of any and all formal techniques, even where these could play an illuminating role (as in the case of game theory),⁸ and in the overly empiricist view of the way in which pattern models should be developed and constructed. This is evident in the attempt to adopt Diesing's participant-observer method, despite the fact that Diesing explicitly applies his method only to simple systems and small groups.

Another way of expressing this problem is to ask what is to be understood by statements such as Diesing's that the "holistic quality" of a system should be captured or Wilber and Harrison's that "social reality must be studied as a whole" (1978: 79). If they mean simply that the part should always be analyzed within its broader context and that those factors that create the organization, structure, and evolution of the whole should be objects of study, then the claim is not objectionable, but also not necessarily inconsistent with the use of abstract or formal techniques. Alternatively, the statements can be interpreted as the argument that the social scientist should be trying to capture the whole in its *total* complexity. Strictly speaking, it is not possible to study a whole in the sense of a *totality* (Popper 1961: 76–78), but the idea that one should try to limit the amount of abstraction and formalism in theory

and stay as close to the complex concrete reality as possible is one that has influenced many institutionalists, including Mitchell and Commons. The appropriateness of the approach, *especially* when applied to complex systems with many interdependencies, is rarely explicitly discussed within the OIE. The shortcomings of the available formal techniques are emphasized, but the opposite problems created by less formal approaches are simply glossed over. It is difficult not to agree with Diesing that the subject matter of institutionalism is such as to require a mix of techniques, not excluding (but equally not limited to) more formal ones, or to endorse Lind's (1993: 14) argument that institutionalists should apply a pluralist methodology in which "interviews, surveys, and participatory observation are put to systematic and sophisticated use together with the methods of mainstream economics."

2.2 Formalism and anti-formalism in the NIE

The various streams of thought that make up the NIE have a common emphasis on the need for more explicit theoretical content than is usually found in the OIE. However, the exact nature of the theoretical content, and the desirable degree of formalism, are seen in very different ways by different groups of writers.

Perhaps the most formal part of the NIE is to be found in certain agency models of organization. Not all agency theory is so formalist,⁹ but as agency theory represents the "neoclassical response" to certain questions concerning organizational behaviour (Levinthal 1988), it is not surprising that the formalism of most neoclassical theorizing has been brought into this area. Much of the theory of agency rests upon standard neoclassical assumptions concerning self-interested rationality. Agency theory is designed to deal with "the problems posed by limited information and goal conflict within organizations," but it has done so by retaining a narrow view of rational self-interest and postulating very high degrees of "sophistication and cognitive capabilities" on the part of all individuals (Levinthal 1988: 154, 181). These assumptions certainly help in retaining the ability to utilize formal mathematical methods of presentation¹⁰ but have left the theory open to criticism for its artificiality and largely ahistorical nature. Indeed, in many areas of the NIE there has been a significant movement away from models based solely on maximizing behaviour and toward a more evolutionary point of view.

The other highly formalized area of the NIE is to be found in the mathematical institutional economics of game theorists (Shubik 1975; Schotter 1983). Both Shubik (1975: 546) and Schotter (1983: 675) criticize neoclassical general equilibrium theory on the grounds that it

is "static," "tightly coupled," and "error free" and lacks any "behavioral or strategic complexity or interest." Game theory can be used to model interaction within given rules of the game interpreted as institutions and conventions. It is, however, important to realize that institutional detail is often required in order to obtain a deterministic solution to a game. The more generally a game is represented, the more the possible strategies, and thus the more the possible solutions. Game theory can also be used to model "invisible-hand" processes of institutional development through the device of the repeated game or supergame (Schotter 1981), but several limitations exist. Game theorists sometimes claim that they can show how institutions can emerge *purely* out of the self-interested behaviour of individuals. As will be seen below, this is a highly questionable claim. Moreover, it has been pointed out that game theory cannot deal adequately with institutional *change* as it requires "three assumptions of constancy": the constancy of the players, the constancy of the basic rules, and the stability of the objectives and the environment. These assumptions are required for the mathematical formalism and the solution concepts employed. Thus, "contrary to the claims often made in the literature on supergames, those models cannot encompass historical change" (Mirowski 1986: 252-255; see also Mirowski 1981; Field 1979, 1984).

Among other contributors to the NIE, there are varying degrees of attachment to highly formal techniques. In a recent paper De Alessi (1990: 11) argues that the NIE can be divided into a formalist and a "literary" branch and claims to find a "growing bifurcation" between the two. This division within the NIE reflects some of the same issues that led to the separation of the OIE from the neoclassical mainstream. On the one hand, the desire to deal with more of the complexities of institutional history leads to less formal approaches, but they bring conflict with the widely accepted emphasis on rigour. On the other hand, concentrating on rigorous formal modelling means confining attention to more simple and idealized models that obviously fail to capture important elements of institutional history. De Alessi himself comments on the benefits of formalism in terms of greater rigour, but also notes "the drift of neoclassical economic theory into irrelevance and an emphasis on formalism for its own sake" (1990: 12).

De Alessi places writers such as Alchian, Coase, Demsetz, and Williamson in the literary group. One might add others such as Douglass North. Of course, the literary group do not dispense with standard economic analysis, but they present it non-mathematically and closely bound to a discussion of particular institutions or institutional history. They also tend to be more willing to relax the strict adherence to the

assumption of universal optimization than is normally the case in neoclassical economics.

Examples of the above can be found in the work of Coase, North, and Williamson. Coase's (1984: 230) theorizing uses virtually no mathematical methods of presentation. He argues that what is distinctive about the NIE is that it does use "standard economic theory to analyse the working of . . . institutions and to discover the part they play in the operation of the economy," but he goes on to modify his support of standard neoclassicism by claiming that the assumption that "man is a rational utility maximizer" is both "unnecessary and misleading." He concludes that "modern institutional economics should study man as he is, acting within the constraints imposed by real institutions" (1984: 231). This, of course, sounds very much like the views of the old institutional economists, and is certainly one that would seem to militate against formalism, as least to the extent that is found in most neoclassical economics or in game theory. At times Coase has even sounded a little like Wesley Mitchell. Commenting on Williamson's studies, he argues for more empiricism: "An inspired theoretician might do as well without such empirical work, but my own feeling is that the inspiration is most likely to come through the stimulus provided by the patterns, puzzles, and anomalies revealed by the systematic gathering of data, particularly when the prime need is to break our existing habits of thought" (1983: 71). Coase still looks forward to formalization, but he sees the first stage as involving more empirical investigation: "once we begin to uncover the real factors affecting the performance of the economic system, the complicated interrelations between them will clearly necessitate a mathematical treatment . . . and economists like myself, who write in prose, will take their bow" (1992: 719).

A similar emphasis can be found in the work of transaction cost theorists such as North and Williamson. North (1990) has been a consistent advocate of the use of neoclassical theory but has also admitted limitations. He argues that ideology and changes in ideology play a vital role in secular change and that most secular change cannot be explained simply in terms of "the strictly neoclassical constraint of individualistic, rational purposive activity" (1981: 58). Although some aspects of North's work can be formalized (Eggertsson 1990: 318-326), his own work is not formal, but an analytic discussion of institutional history, and one that recognizes the many interdependencies involved (North 1990: 7-9). Williamson (1975, 1985) also modifies standard neoclassical approaches by adopting the assumption of bounded rationality together with a more evolutionary perspective. Williamson admits that most transaction cost economics is "crude" and its models "primi-

tive." He points to greater formalization as a goal, but expresses the view that formalism can result in losses and "is not wanted at any cost." He argues that his own research enterprise is broadly consistent with Morishima's view that economists should move toward the "institutionalization of economics, in the sense of slowing the speed of all development toward mathematization and developing economic theory in accordance with knowledge of economic organizations, industrial structure and economic history" (1985: 386, 390-391). It is noteworthy that Williamson has been criticized for his lack of formalism by more neoclassically oriented economists, although some, such as Baumol (1986), modify their complaint by recognizing that a large part of the difficulty lies "in the nature of the subject matter chosen for analysis" (Baumol 1986: 285).

Other interesting cases involve the work of Nelson and Winter and those belonging to, or influenced by, the Austrian tradition. Nelson and Winter explicitly link the high mathematical formalism of neoclassical economics with the adoption of a maximizing and equilibrium framework. These they reject in favour of notions of decision rules and evolutionary change. Given the nature of their thinking, they use simulation methods rather than the more usual modelling techniques. They argue that while "critics of orthodox theory can be accused of not appreciating the importance of a coherent theoretical structure and of underestimating the resiliency and absorptive capacity of prevailing orthodox theory, the defenders of orthodoxy can be accused of trying to deny the importance of phenomena with which orthodox theory deals inadequately and at the same time overestimating the potential ability of models within the orthodox framework somehow to encompass these phenomena" (Nelson and Winter 1982: 48).

Many Austrians and neo-Austrians also reject mathematical formalism. Ever since Menger, Austrians have pursued a non-formal approach, based on their subjectivist view of knowledge, the importance they attach to process, and their stress on the complexity of systems of social order. Despite this rejection of formalism, they have consistently emphasized general theoretical conceptions and have rejected historical and empirical approaches. This non-formal but deductive approach can be found in the work of Mises (1949), Hayek (1945, 1978: 23-34), and others influenced by Austrian ideas such as James Buchanan. Buchanan emphasizes subjectivism in particular, and he criticizes the formalism of orthodox economics as leading to the idea that an optimum or efficient solution is objectively definable and a matter of computation only. Thus:

It is indeed hard for almost anyone trained in economics almost anywhere in this part of our century to exorcise the false constructions and presuppositions

