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Wesley Mitchell: Institutions and Quantitative Methods

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INTRODUCTION

Wesley Mitchell's methodology has often been seen, and not without justification, as involving an attempt to reduce the role of theoretical preconceptions while undertaking a quantitative analysis of the processes involved in economic phenomena such as business cycles. Many have criticised Mitchell's work as lacking explicit theoretical formulations; none more so than Tjalling Koopmans in his well known article "Measurement without Theory" (Koopmans, 1947). Although Koopmans does not claim that theory is entirely absent, he does argue that the Mitchell-Burns approach is "empirical" in the sense that "the various choices as to what to 'look for,' what economic phenomena to observe, and what measures to define and compute, are made with a minimum of assistance from theoretical conceptions or hypotheses regarding the nature of the economic processes by which the variables studied are generated" (p. 161).

Koopmans is most critical of the lack of any "explicit formal theory" (p. 165) relating business cycles to the "underlying economic behavior of man" (p. 162). While not denying that hypotheses concerning the character of cycles are formulated and examined by Mitchell and Burns, "the decision not to use theories of man's economic behavior, even hypothetically, limits the value to economic science and to the maker of policies, of the results obtained or obtainable by the methods developed" (p. 172).

The usual defense of Mitchell's methodological approach, given by Vining (1949) and supported by Schumpeter (1966, p. 239–59) and more recent interpretative work by Hirsch (1976), is that Mitchell's method should not be seen as an attempt to apply or test a developed theoretical system but rather as an attempt to discover new information and generate new or modified theories or hypotheses[1] through the "detailed sifting of data outside the context of a worked out model" (Hirsch, 1976, p. 206). However, despite the popularity of Mitchell's analytic description of the course of business cycles, he was not conspicuously successful in generating new explanatory theories. Although the possibility that Mitchell's method would have failed in any case is considered (p. 206), Hirsch argues that Mitchell's failure was due to his compromise of the objective of creating new theories by directing his efforts to making empirical generalizations.

This paper undertakes to extend the Vining/Schumpeter/Hirsch view by examining the methodological problems that Mitchell was attempting to overcome. It will be argued that Mitchell's decision to reject the available formal (psychologistic) treatments of man's economic behavior and his use of quantitative methods resulted not from a naive inductivism but from an extensive examination of the problems inherent in orthodox theory and methods. This interpretation is consistent with Klein's (1983) argument that Mitchell's use of quantitative methods can be linked to his institutionalist viewpoint.

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ANTI-PSYCHOLOGISM AND INSTITUTIONS

Mitchell's commitment to the institutionalist perspective is nowhere more apparent than in his essays on the psychological foundations of economics. In common with other institutionalists, Mitchell took the view that human behavior is, in large part, an institutional product, a position which necessarily involves a rejection of psychologism, the program of reducing all social phenomena to a set of psychological givens. To allow institutions a significant role in determining or affecting human behavior is to violate psychologism, as it involves a rejection of the idea that 'human nature,' or, at least, the motives, goals and preferences of individuals are psychologically given (Boland, 1982, pp. 30-37).[2] It should be noted that although a nonpsychologistic position implies that economic theory properly includes the consideration of social influences and pressures on individual behavior, it by no means proscribes taking the institutional system and thus 'human nature' as it is found in a particular institutional context as given for appropriate analytical purposes. What is rejected is the reductive argument that all social theory can be built up from a treatment of the psychological states of individuals taken as exogenous to the social and institutional context.

Unfortunately, Mitchell's criticisms of psychologism do not always clearly distinguish between the problem of psychologism and the argument that the principal difficulty with the 'psychological foundations' of economics is their obsolescence. The latter argument does not necessarily imply a rejection of psychologism, but only the particular versions of it in use at the time. Thus, in the first part of his 1910 article "The Rationality of Economic Activity," Mitchell quotes, with apparent approval, McDougall's view that psychology is the "essential common foundation on which all the social sciences . . . must be built up" (p. 99), and goes on to advocate the substitution of what then were new psychological ideas—such as McDougall's instinct theory—for the orthodox notions of rational self-interested economic man or hedonistic psychology. One of the major points Mitchell wishes to establish is that orthodox economics overstates the rational element in human nature, and McDougall's instinct theory helps him in this respect (pp. 103–111). On the other hand, Mitchell does criticise McDougall for paying insufficient attention to the role of habits and institutions in modifying instincts (p. 103), and concludes that human nature should be 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" (p. 111).

Mitchell's position becomes much less ambiguous in the second part of this article (1910B, pp. 197–216). In a notable passage he argues that the economist is guilty of serious error "when, in accounting for the current situation, he treats the concepts which modern men have gradually learned to use as if they were a matter of course, an integral part of man's native endowment, something generally human" (p. 204), instead of treating them as institutional products. Mitchell even applies this argument to the concept of rationality itself: Rationality is "an acquired aptitude—not a solid foundation upon which elaborate theoretical constructions may be erected without more ado" (p. 201). In accounting for rationality Mitchell gives the use of money—or, more exactly, the rise of pecuniary business institutions—a central role. Pecuniary concepts train men in the use of reason, to rationalize economic life itself, so that the use of money lays the foundation for a rational theory of that life.[3] "Money may not be the root of *all* evil, but it is the root of economic science" (Mitchell, 1950, p. 171; see also Mitchell, 1944B). This observation is uncompromisingly anti-psychologistic, but the real question is whether treating rationality as a psychological given rather than as an institutional product does any great damage to most economic analysis.

It might be argued that orthodox theory can be interpreted as a theory of economic behavior under pecuniary institutional conditions. Indeed, Mitchell himself interprets orthodox economics as a result and expression of institutionalized pecuniary concepts (1910B, pp. 212–214). Nevertheless, he makes the argument that the orthodox tendency to argue in psychologistic terms and to give the role of institutions no explicit recognition results in a number of shortcomings. He claims that treating rationality as psychologically given greatly exaggerates the degree and scope of man's rationality. Some areas of life are more dominated by pecuniary institutions than others, and Mitchell often argues that consumption decisions are not usually analysable on the assumption of rational maximization. Household consumption decisions, for Mitchell, are deeply affected by the constraints imposed by the institutions and customs of family life and organization, by a lack of relevant information, and by the absence of a clearly defined or precise relationship between particular purchase decisions and family welfare (1910B, pp. 200, 209–216; 1950, pp. 3–19).

On the other hand, business decisions can be more properly analysed on the assumption of rationality due to the clearly defined end of business enterprise (money making), the reinforcement given to the rational pursuit of that objective by the institutional system, and the greater availability of information (1910B, p. 298; 1950, pp. 6–14). However, even here Mitchell feels that orthodox theory exaggerates the role of rational calculation, in that it assumes all decision makers embody the "mental operations of an ideally perfect money maker" (1910B, pp. 206, 209, 212–215).

More significantly, Mitchell demonstrates a concern with the reductive aspects of the psychologistic version of individualism often found in orthodox economics. Mitchell's position does not imply that individualism should be abandoned in the sense of abandoning explanations which run in terms of individual decision makers. His point is that individuals take over many of their basic goals and concepts from the social system which surrounds them. Furthermore, institutions often have a *prescriptive* authority over the actions of individuals. The social system, therefore, gives rise to a "standardization of thinking and acting," and molds individual action into common patterns (1910B, pp. 202–203, 208).

The importance of this, besides the stress on institutions, is that it led Mitchell to emphasize that the patterns of behavior thus generated may have unintended and unforeseen consequences (1910B, p. 203). The internal development of the 'logic' of the system, which takes place through individuals pursuing and developing certain lines of thought and action and through the conflicts which arise out of the behavior promoted by different elements in the system, can have far reaching and unforeseen effects (1910B, p. 204).[4] For example, Mitchell claims that it was the behavior promoted by the institutional norm of pecuniary success which led to the development of an economic system containing such things as the pecuniary organization of business, monetary and banking systems, the financial policies of governments, the interadjustments of prices, security markets, and so on; a system "no man willed" and which is beyond the control of any individual and possibly "of even society as a whole" (1910B, pp. 208–209). Thus, to deal with man outside of the developing institutional system he inhabits is to do two things: To turn him into a "thin and formal character in comparison with the heir of all the ages," and to construct an economic analysis which is likewise artificial, superficial and incomplete (1910B, pp. 209–210, 216). In Mitchell's view it was because of this that orthodox theory had tended to leave out such Veblenian issues as the conflict between making money and making goods, the "precarious" nature of the price adjustments upon which industrial production depends, and the significance of financial and banking practices for the "welfare of millions" (1910B, p. 210).

Stripped of Veblenian terminology, Mitchell's concern was that the full complexity of the system of institutions and the way in which it bears on individual behavior should be taken into account; and that attention should be directed to the unintended consequences, particularly the unintended aggregate or social consequences, of the behavior patterns induced by the institutional system taken as a whole.

Mitchell's position, as outlined above, remained virtually unchanged throughout his career. The only modifications or additions to these views were occasioned by the gradual decline in instinct theory and the development of ordinal utility theory, which claimed to avoid psychological conceptions altogether. The first of these two developments, however, merely strengthened Mitchell's emphasis on the institutional factor[5] and his insistence that the problem of the treatment of human behavior in economics could not be solved simply by substituting some 'modern' psychology for older ideas. Thus, "psychology gives no finished answer to the question about human motives that the economist can borrow." The economist cannot "expect the psychologist to solve the riddles of economic behavior," and "must himself contribute toward the understanding of human nature" (1950, pp. 407–408).

Developments in economic theory gave Mitchell little additional difficulty. His criticisms of 'pure' economics relate to the unreal assumption that preference scales exist prior to any trading and maintain that treating tastes and preferences as given severely narrows the range of questions that can be addressed. For example, questions about business advertising, salesmanship and, in general, the whole of the *social* processes involved in valuation are excluded. In addition, the arguments outlined above concerning the exaggeration of the rational element in human life are repeated (1950, pp. 161–163; 1944A, pp. 212–219). For these reasons Mitchell felt the effort to avoid the problem by excluding explicit psychological conceptions to be unhelpful.[6] The solution lay not in simply excluding references to psychology—in the general sense of ideas concerning the possible determinants of human behavior—but in excluding *psychologism,* and thereby including within the range of inquiry those institutions and conventions which affect economic behavior.[7] This, of course, raises the question of how to go about such a reconstruction of economics. In Mitchell's view quantitative methods were particularly appropriate for this task. Two factors seem to have shaped this opinion: (i) his thinking on the nature of proper scientific method in general; and (ii) his arguments relating the use of quantitative methods to the importance of institutions in economic life in particular.

QUANTITATIVE METHODS

The influence of J. Laurence Laughlin on Mitchell's views on scientific method, and Mitchell's general skepticism concerning speculative and untestable or untested theoretical systems are well known (Hirsch, 1976, 1967A and 1967B). Even Veblen did not escape Mitchell's criticism: Veblen's work was "not accurate in detail," he "paid too little attention to checking his conclusions by patient observation," and failed to establish the relative importance of "the factors he dealt with and the factors he scamped" (Mitchell, 1929, p. 29; Mitchell, 1928, p. 412). Mitchell thus rejected any approach to the development of an institutional economics which simply involved deducing the consequences of certain assumed institutional incentives and constraints, while confining the role of observation to providing illustrations or 'verifications' of conclusions.

Mitchell's criticisms of the method of deduction followed by empirical verification have led to the conclusion that he adopted the opposite, *a posteriori,* sequence of observation followed by theorizing as the correct one. This interpretation, however, pictures Mitchell as methodologically naive, which was not, in fact, the case. Mitchell's critique of the standard methodology successfully isolates a number of its major weaknesses. Firstly, the empirical work may simply be neglected and the theory never tested (Burns and Mitchell, 1946, p. 8). Secondly, 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, pp. 8–9). Thirdly, 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, p. 9). This problem is compounded to the extent that the investigator is "prone to adduce only the evidence and arguments which seem to prove his explanation" (Mitchell, 1927, p. 181). Lastly, Mitchell argues that the behaviour of economic agents is often more complex than it appears in arguments that proceed by deducing behavior from simplistic assumptions such as the importance of profit for the businessman. That business is conducted for profit "is not a simple matter" which 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" (Mitchell, 1928, p. 415).

Mitchell's conclusion was that to overcome these problems observations should play a greater role, but he neither denied that some conceptual apparatus and working hypotheses were necessary to guide enquiry, nor did he accept the usual empiricist notion of an objective empirical base (Mitchell, 1927, p. 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*" (Mitchell, 1928, pp. 413–415). Mitchell's method does not dispense with theorising, but, as he put it, the place for it is "*inside* the investigation" (Mitchell, 1928, p. 413). His aim was to bring factual research and theorising into the closest possible contact.

Care is required, however, in interpreting Mitchell's concepts of working hypotheses and testing. For Mitchell, the working hypotheses included definitions, measurements, and low level empirical hypotheses, as well as explanatory hypotheses concerning behavioral or causal relationships. In parts of Mitchell's work the first three types predominate, but this is not because Mitchell 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 generate a more meaningful testing procedure. On the other hand, 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. In other words, a verification, to be meaningful, should involve a process of empirical investigation which is at least open to the possibility of finding contrary evidence, or 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 more clearly define, in terms of what had to be explained, the task remaining to the theorist (Mitchell, 1927, p. 58; Mitchell, 1950, p. 409; Burns and Mitchell, 1946, pp. 9–10). Mitchell had considerable optimism concerning the potential of his method to ultimately generate fresh insights and new or modified hypotheses. Nevertheless, Mitchell's caution, particularly with respect to the appraisal of explanatory hypotheses, is at times almost overwhelming. In *Measuring Business Cycles* he argues that the researcher should "not think himself equipped to judge what contribution any hypothesis makes to the understanding of business cycles until he has attained as clear a view as he can of the whole congeries of interrelated movements," and that if "this attitude of suspending judgment taxes patience at times, the investigator can comfort himself with the belief that, so far as he succeeds in showing what cyclical behavior is characteristic of economic activities, he will put others as well as himself in a better position to evaluate hypotheses" (Burns and Mitchell, 1946, p. 10).

The shortcomings of this method are not difficult to locate. Although Mitchell is *not* guilty of naive empiricism, he viewed theories primarily as attempts to systematize the known facts (Mitchell, 1950, p. 409). This position militates against the making of bold theoretical conjectures or predictions of 'novel facts,' and easily leads to *ad hoc* theorising as new information is found, or, alternatively, to the continual delaying of the tasks of appraising and re-formulating theories on the grounds that the search for new information should first be allowed to proceed further.[8]

INSTITUTIONS AND QUANTITATIVE METHODS

Although Mitchell's use of quantitative methods pre-dated his institutionalism, his methodological views developed a close relationship with his opinion of the importance of institutions in economic life (Klein, 1983). He came to believe that the methodology he proposed was particularly suitable for the development of institutional theories. Economic observations are observations of the outcomes produced by the standardizing effect of the institutional system on the behavior of masses of individuals. Patterns and regularities are to be similarly understood (Mitchell 1950, pp. 25–37).

Mitchell's many references to institutions standardizing behavior must be understood as referring to mass behavior. There is no presumption that institutions make the behavior of each and every individual entirely conditioned or predictable, only that the operation of institutions creates predictable mass behavior. Here Mitchell is again rejecting the reductive aspects of psychologistic individualism, as becomes obvious when he argues that instead of attempting "to lay a foundation in the behavior of individuals on which could be built an explanation of mass phenomena" the quantitative worker can "start with the mass phenomena which the qualitative analysts approached indirectly through their hypothetical individuals" (Mitchell, 1950, pp. 25–26). Quantitative research, then tends to theorising "about the relationships among the variables that measure objective processes"; a tendency that Mitchell felt to be quite at odds with orthodox theorising concerning "imaginary individuals coming to imaginary markets with ready made scales of bid and offer prices" (Mitchell, 1950, p. 26). Moreover, as orthodox theory missed the institutional factor and delved instead into psychological propensities and motives, significant parts of it remained metaphysical, untestable and of little use to applied economists, economic historians and policy makers, none of whom could ignore the importance of institutions. Mitchell argued that his quantitative/institutional approach could overcome these problems. If institutions are made the focus of investigation theories could be made more testable—Mitchell even avocated experimental methods—and the gulf between pure theory, history, and policy application could be bridged as "all studies of special institutions become organic parts of a single whole" (Mitchell, 1950, pp. 28, 31–32, 372).

Mitchell argued that quantitative workers, unlike orthodox theorists, would be unable to evade the Veblenian distinction between making money and making goods because statistical series are expressed either in physical or in monetary units. Thus, "out of this technical characteristic of the statistical data we may expect to come a close scrutiny of the relations between our pecuniary institutions and our efficiency in producing and distributing goods, and, further, that investigations of this type will broaden out into a constructive criticism of that dominant complex of institutions known as the money economy" (Mitchell, 1950, p. 30). In this way Mitchell quite explicitly joined his quantitative methodology to the Veblenian dichotomy between pecuniary institutions and technological means. While Mitchell did not seek a Veblenian solution to the problems he attributed to pecuniary institutions, he argued that the problem of "how to make production for profit turn out a larger supply of useful goods under conditions more conducive to welfare" was a problem that was particularly amenable to attack through quantitative methods (Mitchell, 1950, pp. 137–148).

Mitchell's methodology, then, was closely connected with his views on the importance of institutions. Because of this he could place his quantitative research within a conceptual and theoretical context, although admittedly a very broad one. Mitchell's approach involved the close examination of data and working hypothesis in the light of an overall framework of investigation which emphasized the role of institutions, profit seeking, and the distinction between making money and making goods. Mitchell's work on business cycles exemplifies these ideas (Mitchell, 1927, pp. 47–82; Burns and Mitchell, 1946, pp. 3–8, 21–22; Klein 1983). Nevertheless, the linkages that Mitchell made between his quantitative methods and his institutionalism were not without their weaknesses, particularly given Mitchell's own emphasis on the complexity of the way in which institutions affect behavior. Given this complexity, it is not obvious that concentrating on measuring the overall results in price and quantity terms would necessarily provide much additional insight into the various possible institutional forces and interrelationships involved.

MITCHELL'S METHOD IN RETROSPECT

Mitchell's approach to economics can be seen as a response to two methodological problems: (i) the problem created by psychologism for an adequate treatment of institutions; and (ii) the problem of generating a more meaningful testing procedure than that provided by uncritical verification. The solutions to these two problems were, in Mitchell's mind, closely linked: To abandon psychologism was to recognize the role of institutions in standardizing human behavior; to provide more exacting tests was to require more extensive use of quantitative research. These were cojoined through the natural focus of quantitative research on precisely the patterns of mass behavior generated by the institutional system. His concern with the weakness of standard testing procedures has been echoed in numerous modern critiques of the current state of economics (see particularly Leamer 1983; Mayer, 1980; Hutchison, 1981, pp. 266–307). Also, the anti-psychologistic position which legitimizes inquiry into the relationship between social forces and pressures and individual behavior, has found recent expression in a variety of forms: In models which include socialization (Akerlof, 1983; Jones, 1984); in the rejection of reductivism in macrotheory by, among others, those whom Coddington (1983, pp. 92–114) has called "fundamentalist Keynesians"; and in the methodological criticisms of neoclassical treatments of institutions provided by Boland (1979; 1982, pp. 27–43).

Although the problems Mitchell identified are still with us, his methodological views contain weaknesses that clearly hindered achievement of his goals. Perhaps the most troublesome aspect of Mitchell's methodology is the linkage he draws between the role of institutions in standardizing behavior and quantitative methods. To the extent that this is simply a demand for empirical theories and more testing it is innocuous, but Mitchell often implies much more and with much less justification. Even if one accepts that empirically discovered patterns and regularities may reflect the standardizing influence of institutions, this does not necessarily imply that the examination of quantitative data will somehow be revealing or suggestive of the underlying institutional forces at work, so that quantitative work will provide a fruitful source of new insights, or that quantitative workers will tend to be drawn towards the development of explicitly institutional theories. In addition to this, as Mitchell was concerned not to take his theorising too far beyond the facts, his theoretical work often appears tentative and lacking in rigor. The various explanatory hypotheses to be found in Mitchell's treatment of cycles, for example, are often underdeveloped and logically flawed (Hansen, 1951, p. 404; Harberler, 1946, pp. 107–110).[10] Moreover, because his empirical work was directed primarily at defining or describing what was to be explained within only a broad conceptual framework, it was not always even capable of providing a genuinely critical test of the hypotheses he did put forward. These problems are not accidental or simply matters of practice, but relate to Mitchell's views on the nature of theories and the purpose of empirical research.[11]

Thus, while criticisms of the lack of "effective conceptualization" (Schumpeter, 1966, p. 254) in Mitchell's work have justice, Koopmans' argument that Mitchell's work could have been improved by the use of formal models of man's economic behavior, by which Koopmans meant formal models of the established variety (Koopmans, 1947, p. 166), is simply to miss the point of his theoretical and empirical efforts. Mitchell correctly identified the psychologistic nature of orthodox theory as a barrier to achieving a coherent theory of institutions and their effect on economic behavior and outcomes, and this realization played a vital role in his methodological position. For Mitchell, quantitative research was an approach to a better economics—that is to say, to an empirically justified institutional economics. That Mitchell himself failed to successfully elaborate such an institutional economics was the result not of his rejection of psychologism or his desire to bring theorising into closer contact with empirical investigation, but because he failed to resolve the very real methodological problems he identified in a way compatible with rapid theoretical advance. His entirely understandable suspicion of much orthodox (and institutionalist) theorising and the obvious weakness of such empirical testing as was undertaken, led him to search for an alternative method, but his rejection of standard methods took him to an approach which, in certain important respects, conflicted with his clear understanding of the complexity of the phenomena he was studying, the need for new theoretical explanations, and the necessity of increasing the critical component in empirical work. It is this that creates the tensions and paradoxes in Mitchell's approach: The search for new theories and a more critical method, but within constraints which militated against just such results being achieved.

FOOTNOTES

1. In terms of his work on cycles, it might be argued that Mitchell was responding to the plethora of existing theories and was less interested in generating entirely new ideas than in making sense of the vast range of ideas already available. However, Mitchell tended to regard the multitude of different theories concerning cycles as indicative of the complexity of the phenomenon, of the inadequacy of each individual theory as a complete explanation, and of the need for "a fresh attack upon the problem" (Mitchell, 1927, pp. 47–49).
2. Although 'methodological individualism' is often defined in such a way as to involve reductive psychologism, a commitment to individualism does not necessarily imply a reductive or psychologistic program. Neither Popper's "methodological individualism" nor Agassi's "institutional individualism" are psychologistic or reductive in nature. See Agassi (1960 and 1975).
3. To many economists it may seem perverse to argue that economic rationality is an institutional product but ideas quite similar to Mitchell's can be found in Hayek (1973 and 1979). Hayek argues that 'mind' has "developed in constant interaction with the institutions which determine the structure of society" (1973, p. 17), and that "culture and reason develop concurrently" (1979, p. 155).
4. Mitchell's outline of the processes involved in institutional change is based on Veblen's. See Rutherford (1984).
5. This development can be seen beginning in Mitchell (1914). A few years later Mitchell was deeply critical of instinct theories which did not give institutions a role in greatly modifying behavior (Mitchell, 1918).
6. Mitchell also expressed views on the "psychological school" as represented by Fetter, and Marshall's neo-classical economics. It should be noted that Marshall avoided psychologism and clearly regarded both tastes and preferences and human nature itself as changing over time (in the very long run) as part of a process of social evolution and development. For an illuminating discussion of the debates over psychology and economics see Coats (1976).
7. Mitchell's reasons for rejecting psychologism are, in all essentials, identical to those put forward by Karl Popper. Popper argues that "psychology cannot be the basis of social science. First, because it is itself just one of the social sciences: 'Human nature' varies considerably with the social institutions, and its study therefore presupposes an understanding of these institutions. Secondly, because the social sciences are largely concerned with unintended consequences, or repercussions, of human actions. And, 'unintended' in this context does not perhaps mean 'not *consciously* intended'; rather it characterizes repercussions which may violate *all* interests of the social agent whether conscious or unconscious" (Popper, 1979, p. 158).
8. The latter point is also made by Robert S. Lynd (1967, pp. 120–121). Mitchell's response to Lynd's criticisms can be found in Lucy Sprague Mitchell (1953, pp. 553–568).
9. Mitchell's approach did have much more success in establishing new measurements and indicators. Klein (1983) is particularly relevant here. I owe to Mary S. Morgan the observation that Mitchell and Burns 1946 work on specific and reference cycles represented a novel measurement of the business cycle.
10. Mitchell, as is well known, did plan a final theoretical volume on Business Cycles. However, the fact that this work remained unfinished is perhaps not unconnected with the priorities contained in Mitchell's methodological position.
11. In a recent article Hirsch and de Marchi (1984) suggest a methodological link between Mitchell and Milton Friedman. They argue "there are important instrumental components in both Mitchell's and Friedman's approach to economics although both deviate from instrumentalism in very different ways." The Deweyian pragmatic instrumentalism to which these authors refer certainly influenced Mitchell, but the case with respect to Friedman is much less clear. Boland's description of Friedman as an instrumentalist is based on a different, Popperian, definition of the term. To this author it does not appear that Friedman has any interest in the methodological and theoretical problems created by institutions, which so concerned Mitchell, and neither does it appear that Friedman's response to the problems of the orthodox weak verificationist method has much similarity with Mitchell's. Friedman's response is to concentrate on predictive success, in contrast with Mitchell's great concern with the realism of underlying assumptions, his view of theories as (properly) systematizations of factual material, and his continued attachment to verificationism—although in a modified form. See also Boland (1984) and Friedman (1952).

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