

INSTITUTIONALISM AS "SCIENTIFIC" ECONOMICS

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1. Introduction

It is now a commonplace that the development of economics has been deeply influenced by certain key theoretical and methodological concepts borrowed from the natural sciences, particularly from physics. The ideas of natural laws, of equilibrium, of maximization, and of a 'positive' economics are all examples discussed in the literature (Mirowski 1989; Clark 1992). For the most part this discussion focuses on classical and neoclassical economics, and contains the two central arguments that (i) the invocation of natural science has been a powerful weapon in the neoclassical battle against other types of economics; and (ii) that the natural science model is inappropriate for social science and has led economics astray.

On this latter point, Charles Clark argues that 'the pursuit of an economic theory modeled after the natural sciences . . . eliminated history and society from having any meaningful role' (Clark 1992, p. 176). This argument would seem to imply that institutional economists, interested exactly in the roles of history and society, should not be found pursuing an economics modeled on the natural sciences. The problem here is that the institutionalist literature from Veblen through the inter-war period is full to bursting with

references to science, natural science, modern science, experimental methods, quantitative methods, and so on. What this highlights is the fact that references to natural science by economists have sometimes had the purpose of pointing to aspects of science very different from those emphasized by most classical or neoclassical economists. In the literature to be discussed here, the stress is on the empirical nature of natural science and this is linked to an attempt to cast classical and neoclassical economics as speculative, metaphysical, based on outmoded analogies and discredited psychology, unrealistic, and definitely not of a true scientific character. There can be no doubt at all that the earlier American institutionalists thought of themselves as engaged in the task of turning economics into a genuine science. Some of the difficulties caused by the institutionalists' 'lavish praise of science' have been examined by Mirowski (1987), but there is still much to be said about institutionalist notions of science, how they changed over time, and of the role of the concepts and rhetoric of science in both the rise and decline of the institutionalist movement. As has been argued previously (Yonay 1994; Rutherford 1997), the intense debate between neoclassicals and institutionalists in the 1920s and 30s cannot be cast as between one group who could claim the mantle of science and one who could not. Rather, both claimed to be representing science--the argument was one over the key characteristics of natural science and of their transferability to economics.

2. Veblen's Modern Science

The opening paragraph of Veblen's famous 1898 essay 'Why is Economics not an Evolutionary Science?' contains the following passage:

It may be taken as the consensus of those men who are doing the serious work of modern anthropology, ethnology, and psychology, as well as of those in the biological sciences proper, that economics is helplessly behind the times, and unable to handle its subject-matter in a way to entitle it to standing as a modern science (Veblen 1898, p. 3).

For Veblen, the term 'modern science' indicated a particular point of view, consisting of an evolutionary approach, an unwillingness 'to depart from the test of causal relation or quantitative sequence,' an insistence on theorising in terms of a 'colorless impersonal sequence of cause and effect', or 'cumulative causation', and a 'matter-of-fact habit of mind' (Veblen, 1898, pp. 7-8, 26). Veblen's primary model of modern science was evolutionary or post-Darwinian biology, but it encompassed all sciences, both natural and human, that had substantially adopted the point of view described above. It might also be noted that Veblen saw science as an expression of idle curiosity, a disinterested inquiry, and rejected the notion that science should aim at, or be judged by, practical success or application (Veblen 1906, pp. 530-532).

It is on the basis of this conception of science that Veblen launched his attack on orthodox economics. Veblen's first main criticism relates to the adherence to concepts of natural laws or normal cases. The classical economists formulated their ultimate laws and principles as laws of the normal or natural 'according to a preconception regarding the ends to which, in the nature of things, all things tend' (Veblen 1898, p. 12). With later classical and neoclassical writers this terminology of normality and propensity to an end became

highly attenuated and taken as metaphorical. Nevertheless, 'it is precisely in this use of figurative terms for the formulation of theory that the classical normality still lives its attenuated life in modern economics; and it is this facile recourse to inscrutable figures of speech as the ultimate terms of theory that has saved the economists from being dragooned into the ranks of modern science' (Veblen 1898, p. 13). In this, economics is 'living over again in its turn the experiences which the natural sciences passed through some time back' (Veblen 1898, p. 14).

For Veblen, the use of such a 'normalized scheme' involves a failure to follow out 'an elusive chain of causal sequence' and results in an analysis in which 'the agencies or forces causally at work in the economic life process are neatly avoided' (Veblen 1898, p. 14). Economics consists of laws of the 'normal case', but the 'normal case does not exist in concrete fact' so that the laws of economics are 'hypothetical' and 'apply to concrete facts only as the facts are interpreted and abstracted from, in the light of the underlying postulates' (Veblen 1900, p. 199).

Veblen frequently characterized modern science as 'matter-of-fact', but he did not intend to imply by this that modern science was not theoretical or that the methodology to be followed should be purely inductive. He criticised the German historical school for failing to 'offer a theory of anything or to elaborate their results into a consistent body of knowledge' (Veblen 1898, p. 5). He also denied the possibility of ridding science of metaphysical preconceptions, and regarded the preconceptions of modern science as also metaphysical (Veblen 1900, p. 197; Veblen 1906).¹ Veblen's references to the 'matter-of-fact' approach of modern science relate to the refusal to 'go back of the colorless sequence of

phenomena and seek higher ground for their ultimate syntheses' (Veblen 1898, p. 8), in other words to the notion of the cause and effect sequence as impersonal, materialistic, and objective, and not personal, animistic, teleological, or dramatic. The terms 'impersonal' and 'matter-of-fact' often appear together as in: 'the formulations of science have made another move in the direction of impersonal matter-of-fact' (Veblen 1906, p. 529). At the same time there is an empirical element in this, as Veblen clearly did think that the preconceptions of modern science, and the rejection of theorizing in terms of normal cases, would allow for the concrete facts of economic life, such as technological change and the growth and development of 'business usages and expedients' (Veblen 1909, p. 160-161), to be brought within the compass of economic theorizing instead of being ignored or explained away.

Veblen's second main criticism of orthodox economics concerns its reliance on hedonistic psychology. This psychology provides a view of human nature as passive, substantially inert, and 'immutably given' (Veblen 1898, p. 19). Not only is this out of date-- 'the psychological and anthropological preconceptions of the economists have been those which were accepted by the psychological and social sciences some generations ago' (Veblen 1898, p. 19)--but it also fails to provoke an analysis running in the evolutionary terms of a cumulative causal sequence. Modern psychology and anthropology present the individual as a 'coherent structure of propensities and habits which seeks realization and expression in an unfolding activity' (Veblen 1898, p. 20). An individual's desires may seem ultimate and definitive to the individual who is acting upon them, but 'in the view of science they are elements of the existing frame of mind of the agent, and are the outcome of his

antecedents and his life up to the point at which he stands' (Veblen 1898, p. 20). As economic evolution is largely a matter of the evolution of institutions and widely held habits of thought, an evolutionary economics has to deal, in causal terms, with the cumulative change in such habits of thought and cannot treat the objectives of human action as constant and unchanging. Thus, the psychological and anthropological preconceptions of orthodox economics are not consistent with an economics constructed along modern scientific lines.

Veblen's critique of hedonism also connects to his concern about economic theory missing out all the crucial issues by confining itself to the normal case. For example, from a hedonistic point of view money is simply a convenient medium of exchange, so that under hedonistic assumptions the 'whole 'money economy' with all the machinery of credit and the rest, disappears in a tissue of metaphors to reappear theoretically expurgated, sterilized, and simplified into a 'refined system of barter,' culminating in a net aggregate of pleasurable sensations of consumption' (Veblen 1909, p. 174). From Veblen's point of view money is a vital factor in a complex set of pecuniary practices and phenomena that cannot be reduced to the smooth interaction of consumption preferences and production technology.

Veblen's arguments, then, were that the preconceptions of the orthodox type of economics were based on, or borrowed from, out of date conceptions of both the natural sciences and human sciences such as psychology and anthropology. These preconceptions were incompatible with an economics that could properly incorporate institutions and institutional change. In contrast, the preconceptions common to 'modern science' were consistent with--and indeed were tending to promote--the attempt to build an evolutionary

institutional economics. To borrow terminology from Yonay (1994), Veblen was attempting to marshal various 'allies' and strengthen his cause by linking institutional economics with what he saw as the latest trends in both the natural sciences, particularly biology, and related social sciences.

3. Institutionalist Criticism of Veblen's Science

Veblen's views on the nature of modern science influenced later institutionalists to varying degrees. Notably, John R. Commons entirely rejected Veblen's emphasis on efficient cause and questioned the appropriateness of analogies to the natural sciences--including 'the more recent biological sciences'--on the grounds of the role of the human will in shaping institutional evolution (Commons 1934, pp. 96, 651-655). More usual, however, was the view of Veblen as opening up new possibilities and pointing the way to a scientific economics, but falling short in his actual practice. In the case of virtually all of those who were influenced by Veblen one finds a much greater stress on the conduct of empirical investigation. For example, R. F. Hoxie, writing in 1901, accepts that the key analogy is with the biological sciences and to studies of 'life processes and genetic accounts of living organisms' (Hoxie 1901, p. 7). Hoxie freely uses the Veblenian terminology of cumulative change, causal process, life process, and argues that 'economics as a modern science' should consist 'first, of a scientific reconstruction of the economic organization as it at present exists, and second, of a study of the genesis of the economic organization' (Hoxie 1901, p. 11). Hoxie's own approach to this endeavour can be found in his investigations of the trade union movement.

This idea that is implicit here--that the positive program of a modern scientific economics had to involve a greater amount of concrete empirical investigation than Veblen himself had conducted--was in later years stated much more explicitly by leading institutionalists, and often in terms that involved direct criticism of Veblen. The key point made was that it was a lot more difficult to establish causal relationships than Veblen suggested. J. M. Clark can be found arguing that Veblen's 'chief scientific importance' lay in his basic conceptions of the 'problems and methods of economic science' including his emphasis on matter-of-fact, impersonal, cause and effect. But, Clark goes on, Veblen failed to perceive that 'historical observation is too limited to prove relations of cause and effect with scientific completeness'. Veblen's conclusions as to cause and effect are 'based on judgment or insight, rather than on scientific demonstration'. Nevertheless:

. . . even though Veblen neglects scientific procedure in proving his detailed conclusions, he is still a great apostle of scientific method. . . . In Veblen's case, his contribution is in a conception of the problem, the range of data to be envisaged and the kind of results to be looked for. . . . His work of orientation has fulfilled itself in the studies of other men more adapted to plodding induction and detailed verification (Clark 1927, pp. 248-249).

Somewhat similar things were said by other major institutionalists of the inter-war period. Walton Hamilton (1958, pp. 21-22) talks of Veblen as an 'emancipator', as someone who inspired but could not have actually done the more detailed empirical work completed

by institutionalists such as Hoxie, Walter Stewart, or Wesley Mitchell. Rexford Tugwell (1937, p. 239) presents Veblen's contribution as a primarily a critical one. While this was a vital contribution, 'all the constructive work remained to be done; and most of the tools for it were yet to be invented'. Wesley Mitchell, too, was highly critical of aspects of Veblen's method. As Mitchell clearly gave a substantial amount of thought to this issue and was an important influence among inter-war institutionalists his discussion is worth looking at in detail.

At a relatively early stage in his career Mitchell had been strongly enough influenced by Veblen to attempt to complete a project on the evolution and present functioning of the money economy. The project appears to have been conceived as an application of Veblen's notions of modern science and an analysis in terms of a cumulative causal sequence. Part of this project involved him in a study of the rise of markets and pecuniary institutions more generally from their feudal origins (Mitchell 1996). This project was eventually abandoned by Mitchell in 1910, and his experience seems to have helped to shape his methodological views:

. . . problems of cumulative change and "life history" are exceedingly difficult to treat by any method of measurement. Each change is by hypothesis a unique event, begotten by an indefinite number of causes. To disentangle the tangled skein is impossible. Without the aid of elaborate technique it is hard to do more with such problems than what Darwin and Veblen have done--that is, to study the evidence and select for particular attention what seem to be the

salient factors. . . . It is only when he comes to recent changes that an investigator has tolerably accurate data. These materials Veblen did not reject; but he made no great effort to exploit them. In this respect, at least, his practice resembled that of most orthodox economists (Mitchell 1936, pp. xxxi).

Mitchell had expressed similar misgivings before. In a letter to J. M. Clark written in 1928 he argued that 'if anything were needed to convince me that the standard procedure of orthodox economics could meet no scientific tests, it was that Veblen got nothing more certain by his dazzling performances with another set of premises':

His working conceptions of human nature might be a vast improvement; he might have uncanny insights; but he could do no more than make certain conclusions plausible--like the rest. How important were the factors he dealt with and the factors he scamped was never established (Mitchell 1928, p. 412).

For Mitchell, Veblen's great contribution was replacing hedonism with the 'instinct-habit psychology'. This opened up new perspectives, but 'like other intrepid explorers of new lands, Veblen made hasty traverses' and as a result his 'sketch maps are enormously suggestive, but not accurate in detail' (Mitchell 1929, p. 29).

4. Institutionalism and Science in the Interwar Period

The criticisms of Veblen's method outlined above work came from Mitchell, Clark, Hamilton, and Tugwell who were among the prime movers of the institutionalist movement in its immediate post-Veblenian phase. The arguments involved were part of a shift in the institutionalist view of the lessons that were to be drawn from natural science. This shift in viewpoint was, however, reflective of more than just direct experience with the attempt to apply Veblen's particular program.² The appeal of evolutionary analogies seems to have been on the wane more generally, and the institutionalist movement as it formed in the period immediately after the First World War was inspired by a crusade for greater realism (in the sense of realisticness) and relevance to the solution of social problems. In this, institutionalists were reflecting the intellectual trends affecting philosophy and the other social sciences in America (Ross 1991).

For institutionalists of this period Veblen's work played a key role in illuminating certain 'modern' trends in other disciplines and in undermining the authority and legitimacy of orthodox economics and existing economic institutions. Veblen, both in his criticisms of orthodox economics and in his ideal of the matter-of-fact habit of mind, could be seen as pointing the way to a more scientific economics, but, as argued by Tugwell, the constructive work remained to be done. Here, institutionalists were much less interested in understanding the evolutionary processes that had brought about the existing state of affairs, than in comprehending the existing economic system in the way it functioned (and failed to function) in actual practice, and in contributing to the resolution of significant economic problems. While these interests connect to Veblen's concept of modern science in that they

involve the rejection of theory running in terms of immutable natural laws or normal equilibrium states, and the rejection of hedonistic psychology, they also contain a very much greater stress on empirical and quantitative investigation, and the Deweyian idea of science as arising out of problem solving activity and performing as an instrument for social reform. These things did not play a major part in Veblen's concept of science.

An important result of this was that the key institutionalist conception of science moved away from the model of evolutionary biology and cumulative causation towards a model of natural science that more frequently mentioned physics and chemistry and that was generally conceived as involving a central concern with empirical investigation, statistical methods, testing, experiment, verification, and instrumental application to pressing social problems.³ A shift, it should be noted, from an central evolutionary metaphor, used by Veblen to provide both methodological and theoretical organization, to a more purely methodological reference that, in itself, provided no such core theoretical analogy.⁴

The idea of science contained within the literature of interwar institutionalism can be illuminated in more detail by considering the writing on this subject by J. M. Clark, Lionel Edie, by many of the contributors to Rexford Tugwell's 1924 volume *The Trend of Economics* (including, George Soule, Tugwell, A. B. Wolfe, and F. C. Mills), and by Wesley Mitchell.

J. M. Clark's notion of what constitutes science and a scientific economics is of particular interest as his own work was far from purely descriptive and he made a number of important theoretical and conceptual contributions--for example the accelerator, inappropriables (external costs and benefits), and workable competition. One of Clark's

earliest statements on the proper nature of economics indicated 'two main ideals to be achieved'. The first of these was an economics 'actively relevant to the issues of its time', and the second was an economics 'based on a foundation of terms, conceptions, standards of measurement, and assumptions which is sufficiently realistic, comprehensive, and unbiased' to provide a basis for the analysis and discussion of practical issues (Clark 1919, p. 280). Relevance to practical issues, accuracy of data, and comprehensiveness, in the sense of not excluding any evidence relevant to the problem at hand, were the characteristics of a scientific approach to economics that Clark most frequently stressed (Clark 1924, p. 74). Clark recognized that the demand for comprehensiveness might conflict with the requirement to use only accurate data, and could also give rise to difficulties in formulating economic theory, at least as traditionally conceived. Clark certainly thought of theory as playing a key role, but he saw the aim of theorizing as that of forming hypotheses 'grounded in experience' for further study and inductive verification, rather than the production of a highly abstract system of laws. Hypotheses must therefore be formulated in terms that allow for empirical verification or refutation (Clark 1924, p. 76).

Economics must come into closer touch with facts and embrace broader ranges of data than "orthodox" economics has hitherto done. It must establish touch with these data, either by becoming more inductive, or by much verification of results, or by taking over the accredited results of specialists in other fields, notably psychology, anthropology, jurisprudence and history. Thus the whole modern movement may be interpreted as a demand for

procedure which appears more adequately scientific . . . (Clark 1927, p. 221).

As for the relationship between institutionalism and science, Clark argues that the term institutional economics is a term 'used by a group of the younger American economists to define a point of view--one might almost make it coextensive with the scientific point of view--in economic study'. This point of view 'sets up the ideal of studying the interrelations of business and other social institutions as they are and not through the medium of any simplified abstractions such as are employed by classical, static, and marginal economics' (Clark 1927, p. 271).

Lionel Edie can be found saying not dissimilar things. He describes institutional economics as 'an extension of scientific method in economics', with a special emphasis on the use of recent work in sociology and social psychology to replace the assumption of 'independent individual rationality', on the role of empirical investigations of various kinds to verify or disprove theories, modify theories, or suggest new theories 'pertinent to the problems confronting us' (Edie 1927, pp. 407-410). In his slightly earlier survey of institutionalist research entitled *Economics, Principles and Problems* (Edie 1926), Edie outlines the main characteristics of the 'new approach' as including the influence of newer historical and anthropological research and the use of psychological presuppositions in line with modern psychology, the rejection of the notion of immutable natural laws and a substitution of a view of economic conduct as governed by institutions, the use of quantitative methods to supplement qualitative, a view of economic generalizations as tentative and of the nature of hypotheses 'to be tested by experimental and statistical science',

and a 'frank ethical concern for welfare and well-being' (Edie 1926, p. viii).

George Soule contrasts the confidence that is given to scientific knowledge in the realms of physics and chemistry with the lack of authoritative 'tested knowledge' in the area of economics. Classical economics moved too quickly from induction to general conclusion, they 'improvised their psychology' and subsequently lost touch with the growth of 'scientific psychology' based on 'experimental method and quantitative testing', and were without the benefit of 'modern anthropology'. All of this led to the 'building up of bodies of economic doctrine which more resembled closed systems of metaphysics than an account of the real world' (Soule 1924, pp. 359-361). However, Soule detects a 'rapid growth toward maturity' to be found in the critiques of institutional economists and others aware of the deficiencies of classical theory and its psychological foundations, and in the pressure to 'make the science practically useful' accompanied by greater availability of data and the use of quantitative research and statistical methods (Soule 1924, p. 364).

Tugwell argues for an 'experimental economics', claiming that the 'assurance of rightness in science' is to be found in the replication of experimental results. In this vein he discusses Newton and Galileo. In Tugwell's words 'it is sometimes more, sometimes less, difficult to isolate and to demonstrate by experiment the bits of truth that scientists discover; but nothing is accepted as truth unless it can be so demonstrated' (Tugwell 1924, p. 386). Social scientists, according to Tugwell, see themselves in 'direct line of descent' from natural scientists, the only difference being that the conclusions of social science having to 'meet the test of application in a complex going system immediately'. Social science must try to 'isolate its problems and to devise and use special tools for dispassionate verification'

(Tugwell 1924, p. 387). These tools include the efforts of specialised research organizations and the use of quantitative and statistical methods.

Tugwell also argues that 'the truth must be useful; and if science does not help to solve a problem it cannot reach out toward truth' (Tugwell 1924, p. 387). This idea allows Tugwell to argue that natural laws in the physical sciences have a different status from the so-called laws of classical economics. Tugwell seems to regard natural laws, even the natural laws of physics, as simply useful generalisations or hypotheses and not as ultimates. However, in the physical sciences natural laws such as the law of gravitation have proven themselves in 'innumerable experiments' and in problem solving, while the laws of classical and neoclassical economics have not. Supposed economic laws are often little more than an embodiment of ideology or an expression of dialectical dilettantism (Tugwell 1924, 393).

In Tugwell's words:

Natural law has lost its force in analogous application because so many times events have disproved its premises. There has been a drift toward the substitution of consequences for premises in the search for truth in all fields. . . . Nothing can be taken as ultimate any more. And there are no ultimates, therefore, to form premises for economics either within the economic field or outside of it. . . . The whole conception of science, then--and the modern world has gone over to science--*is* experimentalism. Scientists have learned to distrust premises and to depend upon consequences. And in social science this is bound to involve social facts as they are to be observed in a going

society. These facts are the consequences. Theory must have reference to them if it is to be useful (Tugwell 1924, pp. 394-395).

A. B. Wolfe remarks on the differences of viewpoint between various of the 'younger men' but goes on to state that they all 'hold that economics ought to be scientific'. Wolfe ascribes this growing 'demand for a realistic, inductively analytical, non-metaphysical, scientific economics' to the matter-of-fact spirit of the times and a 'growing conviction that the older economic theory, whether classical, neo-classical, or marginalistic, is deficient in scientific quality' (Wolfe 1924, p. 447). According to Wolfe, the main features of the scientific method are (1) unbiased selection of factual data, without 'undue limitation of range', and freedom from personal or class interest; (2) hypotheses seen as devices in a trial and error method; (3) all generalizations regarded as tentative; and (4) deductive inferences to be tested by 'repeated appeal to experience', and long chains of deductive reasoning to be avoided (Wolfe 1924, p. 451). Clearly, Wolfe regarded orthodox economics as seriously deficient in all of these respects. Wolfe, however, did not see science as solely a quest for knowledge for its own sake, but also motivated by ethical ideals and normative standards, and rejected the notion that 'all science is a matter of measurement' as an unwarranted importation from the natural sciences (Wolfe 1924, p. 463). On the other hand he holds out the prospect of a 'scientific ethics' based on behaviouristic psychology and social psychology. A scientific understanding of human nature should 'point the way to a fundamental, objectively scientific, ethical norm or ultimate end of life' (Wolfe 1924, p. 478).

A high proportion of the institutionalist discussions concerning a scientific economics

in the inter-war period contained at least some mention of the importance of quantitative work. In this, the work of Wesley Mitchell, and of those who followed his lead, such as F. C. Mills, was central. Mitchell reacted particularly strongly against the speculative, normatively biased, untested, and often untestable nature of existing economic theory. He argued that the social sciences were held in low repute and, given the lack of exactness and certainty of their conclusions, deservedly so. The solution was to imitate the natural sciences in their careful and painstaking work of observation and experiment, systematic analysis, and desire to eliminate normative biases and achieve objective results (Ginzberg 1997; Biddle 1998). Mitchell put the matter as follows:

There seemed to be one way of making real progress, slow, very slow, but tolerably sure. That was the way of natural science. . . . Not the Darwinian type of speculation which was then so much in the ascendant--that was another piece of theology. But chemistry and physics. They had been built up not in grand systems like soap bubbles; but by the patient processes of observation and testing--always critical testing--of the relations between the working hypotheses and the processes observed. There was plenty of need for rigorous thinking, indeed of thinking more precise than Ricardo achieved; but the place for it was inside the investigation, so to speak--the place that mathematics occupied in physics as an indispensable tool. The problems one could really do something with in economics were problems in which speculation could be controlled (Mitchell 1928, p. 413).

Quantitative and statistical work combined with careful policy experiments Mitchell saw as the closest approach to the methods of the natural sciences possible in economics. Quantitative work in economics required all the trappings of the natural sciences--a 'statistical laboratory', research assistants and fieldworkers (Mitchell 1925). This, of course, was the ideal of scientific research that Mitchell embodied in the NBER. Mitchell also talked of experimentation, at least in the form of experiments on group behaviour. He recognised some of the difficulties of attempting to apply experimental methods to economics, but argued that they could be mitigated by more reliance on 'statistical considerations and precautions' (Mitchell 1925, p. 31).

For Mitchell, quantitative work would lead to a significant recasting of economics along institutionalist lines. He rejected the idea that quantitative work would simply complement orthodox theory, as that theory was not stated in terms amenable to statistical attack. Hedonistic theory was based on unobservable subjective states, not objective measurable phenomena, and orthodox theory was in general stated in terms of mechanical invariant laws and not statistical relations. Mitchell, quoting Clerk Maxwell, argued for the adoption of the statistical conception--or view of nature--based on notions of 'variety, of probability, of approximations' (Mitchell 1925, p. 35). Due to the greater degree of variation and uncertainty in economic observations, Mitchell claimed that this shift in viewpoint would lead to more far reaching changes in economics than had occurred in physics.

Mitchell also explicitly linked quantitative methods to an institutionalist and behaviouristic perspective. Quantitative work would lead to a focus on the patterns of

mass behaviour that, for Mitchell, were clearly of institutional origin. He frequently argued that economics was one of the sciences of human behaviour and has close links to psychology, particularly with the development of psychology toward 'objective observation' (Mitchell 1910, p. 100). In addition, Mitchell's concern with scientific objectivity did not prevent him from strongly advocating that social scientific research be used to inform the policy debate, or supporting a kind of economic planning to solve economic problems. However, more than many institutionalists, he attempted to maintain a strict division between scientific research as such and the debate over policy objectives (Biddle 1998).

A similar emphasis on quantitative and statistical approaches can be found in F. C. Mills's essay 'On Measurement in Economics' (Mills 1924). Mills quotes Lord Kelvin to the effect that without measurement and numerical expression there cannot be a *science*, and quotes both Clerk Maxwell and Pearson on the statistical view of nature. For Mills all social relationships do not hold universally or with absolute certainty. In the statistical approach 'we forego the searching for sole causes and, instead, seek to measure the degree of association found in experience' (Mills 1924, pp. 43-44). Furthermore, such relationships are not seen as 'final formulations of truth' but in a process of development towards higher degrees of probability. Interestingly, Mills finds the statistical conception 'in complete agreement with the views of philosophers of the pragmatic school' such as Dewey. Mills quotes with approval Dewey's remarks that generalizations are 'not fixed rules . . . but instrumentalities for . . . investigation' and are 'hypotheses to be tested and revised by their further working' (Mills 1924, pp. 45-46). Mills also links a quantitative and statistical economics to the more effective solution of economic problems. Practical problems will be

more readily solved 'by quantitative study of specific conditions than by the attempt to apply vague generalizations of doubtful validity' (Mills 1924, p. 70).

5. Institutionalism as Science: Some Concluding Remarks

A number of points emerge from the above. First, that the identification of institutionalism with the model of biology is not accurate. It is frequently said that 'institutional economics was modeled after the evolutionary models of biology' (Ganley 1995, p. 403), but while it is true that institutionalists have always talked in general terms about the importance of 'process' and attacked the static nature of orthodox theory, it is difficult to find any institutionalist other than Veblen who has made deep or extensive theoretical or methodological use of the analogy to biological science. References to the biological model occur in some of Hoxie's earlier work, and Morris Copeland (1931) continued to refer to biology into the 1930s, but for the most part institutionalists shifted their view away from biology specifically toward a scientific model that drew on the instrumentalist philosophy of science and the methods of the laboratory sciences more generally. These scientific ideals had to be adapted for the social sciences, but the central point was that to be scientific meant to be committed to the development of an empirical and instrumentally useful economics.

Second, it is obvious the institutionalists of the inter-war period saw themselves as part of an exciting movement of 'younger men' engaged in the task of making economics genuinely scientific. Being scientific meant devoting time and resources to the production of much more, and more reliable, economic observations and quantitative data; taking a view of

theories as tentative; stating theories and hypotheses in a form that permitted them to be subject to examination and test on the basis of empirical observations, including statistical analysis and experimental tests; being dispassionate and, as far as possible, free from ideological bias; using behavioral or motivational premises consistent with the state of scientific knowledge in other related fields, particularly psychology; and creating knowledge useful for solving concrete problems. This definition of what it meant to be scientific was contrasted with the highly abstract and hypothetical nature of orthodox economics, the difficulty of testing orthodox theories given their abstract nature, the out of date psychological premises that exaggerated the rational element in human behaviour, the tendency to regard the laws of economics as absolute truths, the admixing of ideological, pro-market, passions, and the lack of useful guidance provided for dealing with even the most pressing of economic problems.

Third, it is also obvious that for these writers there was nothing incongruous about linking institutionalism with methods and ideas borrowed from the natural sciences, indeed *quite the opposite*. The methods of natural science were seen as entirely consistent with the recognition that institutions and not natural laws governed much of human behaviour. It was the metaphysical structure of orthodox theory, its foundations in a discredited psychology and old fashioned conceptions of deterministic and invariant natural laws, that was seen as endangered by the critical, experimental, objective, statistical, and empirical spirit of modern natural science. What this demonstrates is that neoclassical economists had no monopoly on appeals to natural science. The lessons institutionalists wished to take from natural science differed from those emphasized by neoclassical economists, so it is wrong to think of there

being *a* natural science model. References to particular organizing principles or methodologies to be found in the natural sciences have not infrequently become part of both sides in the conflicts between schools of thought in economics. As stated by Mirowski (1987) science may be realist or idealist, rationalist or empiricist, monistic or dualistic, naturalist or operationalist or instrumentalist: 'Nothing is substantially illuminated by the mere invocation of science by economists, although it has in the past proved useful in cowering certain critics' (Mirowski 1978, p. 1002).

Fourth, taking for granted the prestige of natural science and the rhetorical power of an appeal to natural science, the pertinent question is not who can make an appeal to science, but who can make the most persuasive appeal, or who can cow whom. In this, the history of the relative fortunes of the institutionalist and the neoclassical appeals to science is a fascinating one. Veblen's appeal to modern science in the guise of evolutionary biology seems not to have been particularly effective, at least in the sense that there does not seem to have been a widespread or serious discussion about the merits of adopting a biological model in economics. However, the combination of some of Veblen's criticisms--particularly his criticism of hedonism--with the empirical and instrumental view of science that inspired the institutionalists of the interwar period does appear to have been quite effective in challenging the scientific standing of orthodox economics and in putting its proponents on the defensive. The view of science contained in the institutionalist literature of this time probably reflected a common view of scientific method and was explicitly linked with the influential instrumentalist philosophy of science of John Dewey. If anything, it was the institutionalists who were cowering the neoclassicals with their rhetoric of modern science as observation,

measurement, and verification. To be sure Raymond Bye (1924), Frank Knight (1924), Jacob Viner (1928), and Allyn Young (1929) all mounted defenses of orthodox theory and methods, but their arguments all contain qualifications in the attempt to link neoclassicism to natural science. They argue that orthodox theory is like natural science in the sense of *theoretical* or *mathematical* physics, but they cannot deny that most natural science is a good deal more empirical than orthodox economics. As a result they seek to limit the more general transfer of natural science methods, usually on the grounds of the greater complexity of social phenomena and the inability to conduct controlled experiments. Viner, for example argues, in response to Mitchell and Mills, that 'methodological analogies from physics should not be applied to economics as a whole without the most serious qualifications and reservations' (Viner 1928, p. 31). This demonstrates the justice of Porter's remark that 'while neoclassical economists may have derived much of their mathematical theory using analogies with physics, they were very far from accepting the prevailing standards of physics as a practice' (Porter 1994, p. 157). Porter's example of an economist who *did* take the problem of measurement seriously is Wesley Mitchell.

Finally, how and why did this situation change? One obvious factor is that orthodox economic methodology itself changed over time. The institutionalist adoption of a purely methodological vision of science, and the lack of a central organizing theoretical conception, metaphor or analogy of its own, left institutionalism highly vulnerable to the development of empirical methods that could be connected to orthodox theory. Here, the development of econometrics and the efforts of the Cowles Commission were crucial, as was the development of Keynesian macroeconomics that offered both policy relevance and

opportunities for empirical modelling. J. M. Clark himself explained the appeal of Keynesian economics in terms of it addressing the 'major sickness' of Western economic society, and doing so 'with an analysis that commands standing as objectively scientific, centring in a formula of the way in which the economic mechanism operates, the analysis being translatable into statistically observable quantities' (Clark 1957, p. 14). In other words, institutionalists could no longer claim that only they represented science, even according to their own standards. Indeed, in terms of practical policy effectiveness and empirical testability, Keynesian economics was clearly superior to anything they themselves could offer and a number of institutionalists joined the Keynesian ranks. Other factors contributed, including the decline of the influence of Dewey's instrumentalism and the influx of European economists during the Second World War, many of whom were impatient with what they saw as the institutionalists' somewhat naive empiricism.

Nevertheless, what is important to understand here is that many of the key elements of the institutionalist view of science were not ultimately rejected by more orthodox economists, but taken over and brought into the 'official', 'modernist' rhetoric that combined an emphasis on observation, testing, and objectivity along with a greater stress on the role of the 'hypothetico-deductive' method and a modified instrumentalism--courtesy of Milton Friedman--that provided a way of avoiding uncomfortable questions about the realism of assumptions. The list of modernist propositions provided by McCloskey (1983), contains many positions commonly taken by institutionalists well before they became part of the standard rhetoric. These include 'Kelvin's Dictum' concerning science and measurement that, despite its inscription on the University of Chicago Social Science Research Building, was

the view held by Mills and Mitchell and not by their Chicago critics.⁵ At the same time references to quantitative measurement, statistics, and statistical testing procedures completely dropped out of institutionalist discussions of method. Although still containing much of Dewey's instrumentalist ideas, the more recent institutionalist literature on methodology has moved sharply away from its history of references to natural science and quantitative methods and towards discussions of holism and pattern models--on grounds of the complex interdependencies found in social systems. In other words, the orthodox mainstream managed to take over much of the modernist and scientific rhetoric of institutionalists in a suitably modified--that is to say positivistic--form, and successfully turn the tables. This despite the fact that the 'official' modernist methodology has never been fully reflected in the actual practice of most neoclassicals. Institutionalists, no longer able to hold to the ground of natural science, turned to criticism of the applicability of the natural science analogy and positivist methods to economics. The exact manner in which this remarkable role reversal was accomplished requires further and more detailed research, but neoclassical adoption of econometrics and savage attacks on institutionalism as measurement without theory certainly provided leverage.

NOTES

1. Veblen's philosophy of science and some of its difficulties is discussed more fully in Mirowski (1987) and Samuels (1990).
2. The experiences of Hoxie and Mitchell in attempting to apply Veblen's theory of evolutionary change are discussed at more length in Rutherford (1998).
3. The work of Morris Copeland is a bit of an exception here. Copeland (1931) discusses natural science method in terms of an analogy with biology. However, he shares with other institutionalists a stress on the importance of formulating hypotheses "relevant to the subject matter of science, which are capable of empirical test" (1931, 68).
4. Clarence Ayres later attempted to reconstruct institutionalism along more general theoretical lines utilizing the instrumental/ceremonial dichotomy as his key construct. This represents a combination of Deweyian instrumentalism with Veblen's distinction between institutions and technology. Although Ayres occasionally talks about Darwinism, he does not refer to evolutionary biology as a model or make use of biological analogy in the way of Veblen.
5. This is made clear in the discussion of quantitative methods that involved Viner

(1928) and in Knight (1924). Mitchell and Burns were, of course, attacked by Koopmans (1947) and Mills' book *The Behaviour of Prices* (1927), written for the NBER, was the subject of criticism by Raymond Bye (1940). Bye's attack was done as a part of the examination of the methodology of the social sciences conducted by the Social Science Research Council. Publication of Bye's report was followed by a discussion between both supporters and detractors of Mills' work. Marschak (1941) provides one view of the debate.

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