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Economic Methodology: Theory and Practice

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This paper is about the theory *and* practice of economic methodology. It is not about the usual worn-out issue of theory *versus* practice. Identifying the methodology which economists actually practice is more interesting than asking if they practice what they preach. To a certain extent this is an empirical question, and, like all empirical questions, we need a theoretical framework for the examination of the empirical detail. For this purpose I will present the theory of methodology that I have been using for the last thirty years or so. Armed with this theory of methodology I will discuss some of the ways methodology is practiced in economics today.

KNOWLEDGE AND TRUTH STATUS: HISTORICALLY SPEAKING

Traditionally, methodology is considered to be about the identification of 'correct' answers to important questions. Whenever someone claims their answer is correct, the question methodologists might ask is, 'How do you know your answer is correct?' Needless to say, the question has been asked countless times. Many people today view 'science' as the embodiment of 'correct answers', and 'scientific method' as *the* only sure way to demonstrate that one's answer is 'correct'. Of course, methodology has been discussed for centuries. The currently popular belief in Science and Scientific Method is based on a 350-year-old methodology that was refuted 200 years ago.[1]

Since our modern view of methodology has its roots in philosophical problems, a good starting point for the study of methodology is history itself. But 350 years of history is surely filled with an excessive amount of detail. So I will have to simplify the historical detail by presenting a 'theoretical history' concerning the common interest in correct answers and in the methods alleged to yield correct answers. My objective is to explain 'historically' why there has been a concern for a method of knowing the 'correct answers'. From this we may learn why we find popular methodology frozen at a point just one step beyond its refutation in the eighteenth century.

Thinking and 'correct answers'

I suppose I should begin my 'theoretical history' with a disclaimer like one of those found at the beginning of some movies: 'All characters in this story are fictional; any resemblance to real persons is purely *intentional*.'

Students today are too often taught that the primary objective of learning, or even thinking, is finding the correct answers. The basic presumption is that 'knowing is knowing the truth'. It has *not* always been that way. Before the time in which Socrates is supposed to have lived (say, prior to 450 BC) many people considered thinking to be a process of discovering or inventing all of the possible or conceivable answers to any given question. That is, thinking people did not necessarily begin with a burning desire to know the correct answers.

Among the so-called Pre-Socratics were some fellows whom I shall call Sophists. These fellows maintained that there just had to be correct answers. But whenever a Sophist thought he knew the correct answer he could not always prove it to be correct merely by arguing directly in its favor – that is, by simply giving reasons to prove the truth of the answer. Some of these Sophists devised an indirect way to argue in favor of their chosen answer. This Sophist's method, which is still followed today by some members of the so-called Chicago school of economics, proceeds as follows.

First, the Sophist must claim (or presume) that there is a finite number of conceivable answers to any given question. For example, for some questions there are only two possible answers –

'yes' or 'no' (a response such as 'who cares?' is not an answer). The second step is for the Sophist to attempt to refute *all other* answers. If the first step was successful – that is, if all possible answers have actually been listed – then the refutation of all answers other than the one thought to be true would mean that the favored answer is revealed to be the correct one.

The success of this Sophist argument depends primarily on there being a *finite* (and mutually exclusive) set of possible answers. Very often, Sophists argue without always being sure they have identified *all* of the answers. They might not have identified all answers if a complete search takes a long time. In general, the Sophist argues by criticizing competing answers in hopes of convincing everyone that the Sophist's favored answer is the correct one. But the Sophist's argument can work only when *all* of the possible answers have indeed been identified and *all* of the *competing* answers have been refuted.

Knowledge, authority and method

Unfortunately, the legacy of the Sophists is an excessive concern for (quickly) finding *the* correct answer – rather than for (slowly and carefully) identifying *all* the possible answers. For many questions it would be difficult even to list all the answers let alone determine *which* one is correct. But people demand (correct) answers. Politicians and kings demand answers, governmental agencies demand answers, and even corporation directors demand answers. Given these demands, it is easy to understand how the institution of 'authority' might be seen to be able to overcome the insufficiencies of logic – authority gives people answers quickly.

Galileo and the authorities

For hundreds of years the Church was the institutionalization of authority. Its College of Cardinals would decide what we were to consider true knowledge. It is this tradition that faced Galileo (1564–1642). Galileo believed that the truth of one's knowledge could not be decided with a vote by a group of individuals – even a group of cardinals. Rather, the truth of one's knowledge would have to be decided by the real world. Galileo is said to have climbed to the top of the Tower of Pisa to demonstrate the truth of his knowledge of falling bodies. This was particularly challenging to the 'authorities' and thus Galileo was not very popular with them.

As is well known, Galileo ran into difficulty with the Church 'authorities' because he taught his students about a theory of heavenly bodies authored by Copernicus (1473–1543). Galileo's problem was that the authorities had given their approval to the competing theory of Ptolemy (AD 100–170). As the simple story usually goes, the approved Ptolemaic theory was that the earth is the center of the universe and all the planets and stars revolve in circles around the earth. In a more complicated form the Ptolemaic theory allowed for epicycles (the path of a point on a rolling circle) in place of perfect circles.

Galileo chose to discuss the Copernican theory which put the sun rather than the earth at the center of rotation. The Copernican theory was a direct challenge to the authorized Ptolemaic theory. To maintain the authority of the Church, Galileo was told to stop teaching his students about Copernicus. But Galileo responded that *people* cannot dictate which answer is true, nor is the truth of one's knowledge a matter of authoritative opinion. The truth of one's knowledge is a matter of its objective relationship to the 'real world'. If you think you know something about falling bodies, you can climb with Galileo to the top of the tower and test your knowledge.

But the Church authorities replied, so my story goes, that Galileo simply had no authority to challenge the authorities or even authoritarianism. Furthermore, the Church *did* have the authority and the overwhelming power to prevent Galileo from challenging it. With a simple show of their immense power, Galileo was forced to give in. He was banished to southern Italy and no longer taught his students about the Copernican view.

The humanists' challenge and their social contract

Another reaction to the authoritative Ptolemaic view that 'earth is the center' was the claim that by accepting this view we are actually led to further considerations which might also contradict the authority of the Church. Specifically, it was argued by some of those who witnessed the Church's victory over Galileo that if the earth is the center of the rotation of heavenly bodies then potentially Man or humanity is the center of rotation. I shall call this interpretation of the Ptolemaic view *humanism*. Although there were many different aspects to this extension of the Ptolemaic view (e.g. the rise of Protestantism), I will be concerned only with what it means for our modern view of knowledge. The humanist's argument was, in effect, that if Man can be the center of everything, then all knowledge can reside in the minds of humans.

My concern here will not be with whether the humanist's view of the possibility of human knowledge is a logically sound view or even an acceptable view on its own. Rather, I will be concerned only with how it challenges the authority of the Church in all matters and in particular in matters of knowledge. Since the Church accepted the responsibility of determining what is (or is not) correct knowledge, there would seem to be little room left for independent human knowledge. No individual person was allowed to claim that his or her knowledge was true without the authoritative approval of the Church. But the humanists claimed that one's knowledge could be true regardless of the opinion of Church authorities.

The Church authorities were unable to fight back as effectively as they did in Galileo's case. For one thing, all overwhelming or excessively powerful victories have a common problem – the victors tend to be discredited in the eyes of the spectators and critics. Such was the case with the victory over Galileo. Thus, the Church authorities had to be more careful with the humanists. The tactic adopted by the Church was to offer the humanist challengers a 'deal' – namely, a specific social contract.

Now, my story of an explicit confrontation between the Church authorities and the humanists may very well be entirely fictional – I was not there. I can only propose the following heuristic viewpoint. While the Church authorities wanted to defeat the challenge of the humanists, the humanists wanted to establish that humans could possess correct or true knowledge. The authorities offered the following contract: Any individual can claim to have knowledge *only if* he or she can prove or 'justify' its truth.

The humanists eagerly accepted and signed the offered contract. I shall henceforth call this hypothetical contract the *Social Contract of Justification*. Although the humanists did not realize it, by signing they had agreed to play a 'no-win' game with the authorities – which of course is exactly why the authorities wanted to play (from the authorities' standpoint, it was a 'no-loss' game). But before I explain this, let me first consider why the humanists were so eager to agree to play.

The authority of justification

The reason why the humanists were willing to sign the Social Contract of Justification was simply that they thought there would never be a problem proving one's knowledge to be true whenever it is true. Today it is difficult to see why they could have thought that it would be so easy. If we try not to be wise in retrospect, we can see that the reasons were easy. Far from the direct power of the Church in southern Europe, there was one thinker – Francis Bacon (1561–1626) – who was arguing that if one was 'scientific', one could always provide rational arguments for the truth of one's knowledge. Thus, Bacon was the humanists' 'secret weapon'. Bacon's inductive Science would be their alternative to the Church's authority.

Before examining the nature of Bacon's Scientific Method of proving the truth of one's knowledge, we should ask why this Scientific Method might be of interest to the humanists or anyone else. I think the reason is simple. By justifying one's knowledge using the Scientific

Method, the Method itself replaces the authority of the Church. The Scientific Method is not a challenge to authoritarian*ism*. Rather, it is merely a challenge to those who play the social role of authorities.

The Scientific Method

The Scientific Method of Bacon promised that whenever your knowledge is true, you could always prove the truth of your knowledge by following his method. The promise of the Scientific Method is founded on the following doctrines: (1) *Truth is Manifest* in Nature (i.e. the truth of anyone's knowledge of the real world is manifest and thus discoverable in the real world); and (2) *To Err is Sin* (thus, error must be avoided). An appreciation of these two doctrines is essential for a clear understanding of Bacon's Scientific Method. So let us examine his doctrines.

These two doctrines are not independent. If 'truth is manifest', truth is there to be seen. Only people who blind themselves to manifest truth would ever make false claims – that is, make claims that their (false) knowledge is true. But would anyone ever be so blind? Bacon argued that blindness to the truth is a symptom of prejudice and impatience for success and fame, and both are consequences of greedy self-interest. Since greedy self-interest is often considered a Sin, it is a Sin to make a false claim about the truth of one's knowledge. To avoid Sin, one must not make any claim *until* one has gathered the facts to prove it true. Only a greedy, impatient, self-interested person would commit the error of jumping to a conclusion without first collecting all the facts.

The warning 'do not jump to conclusions' is both the key to Bacon's Scientific Method and its primary legacy. When following his Method, one must always be careful, patient, unprejudiced, open-minded, diligent, etc., and if one works hard and long enough (i.e. collects enough facts) then one *cannot* commit an error. Bacon's Scientific Method then is a recipe. Every scientific investigation begins with an unbiased collection of data, followed immediately by a logical demonstration (i.e. 'proof') of any knowledge derived from the collected data. Thus, Bacon's Scientific Method is both a method of assuring that the collected facts are beyond question since the *collector* was scientific (i.e. unbiased, unprejudiced, etc.) and a method of justifying claims to true knowledge.

The scientific facts are accordingly the primary basis for any rational argument in favor of one's knowledge – one's *human knowledge*, that is. Thus, we see why the humanists saw Bacon's Scientific Method as their secret weapon. The humanists saw no risk in putting their signatures on the Social Contract of Justification since Bacon's Scientific Method assured them that there existed a way to prove one's knowledge true whenever it is true. And most important, the proof, the rational argument consisting only of the unbiased scientific facts, would never require the authority of the Church.

The success of the Scientific Method

It was often claimed that there were many examples of successful applications of Bacon's method. The most famous is Newton's physics. Isaac Newton (1642–1727) claimed to have arrived at his 'Laws of Physics' by using the Scientific Method. With Bacon's Scientific Method, a proposition about the nature of the real world can be called a 'Law' only after it has been proven beyond a shadow of a doubt. Can one ever argue with someone who claims that his knowledge has been arrived at by the Scientific Method?

The promises of the Scientific Method even go so far as to suggest that all knowledge of the world can be shown to be based on real-world experience – that is, on empirical data. It promises that it is possible to show that our knowledge is based only on facts since the logical demonstration of the truth of one's human knowledge will be based only on the scientific collection of empirical facts – gathered, so to speak, by experience.

Knowledge versus psychologism

The problematic 'no-win' contract: the Social Contract of Justification

For a long time Bacon's Scientific Method reigned as the solution to the problem of providing the rational basis for human knowledge. In short, all human (i.e. all subjective) knowledge could be shown to follow *logically* from objective facts or experience. In this light, there are only two elements that constitute human knowledge: (i) facts or experience, and (ii) logical proofs. But this also means that the humanists, by relying on Bacon's Scientific Method, signed a contract which had a built-in contradiction. Let me explain.

Specifically, if human knowledge must be justified by logical proofs using only empirical facts, where is the *humanity* in human knowledge? Clearly, if facts must be found in the objective real world, they are not human. This leaves only the logic of the argument in favor of one's knowledge. If there is humanity in human knowledge, as the humanists hoped, it must reside in the logic of argument.

Now, it should be easy for anyone living today to see that this is a problem. Consider the use of computers and consider that there are satellites circling the earth and others traveling by Jupiter and Saturn. These are merely logical machines and some of them just collect facts, without the hand of any human. It is not difficult for us to see that today there is no humanity in being logical. Logical decisions can be represented by a machine without any human having to make real-time decisions. In fact, the entire essence of logical proofs is their universality – *anyone* can understand them. The inventor of the proof does not have to be present to explain the proof.

Whenever the humanist *is* successful in justifying the truth of his or her knowledge with a logical proof using only empirical (objective) facts, he or she has produced something which is necessarily *not human*! Thus, *there is no humanity in (justified) human knowledge*. This means that the Church has defeated the challenge of the humanists on at least one count. The legacy of this apparent defeat is simply the common view that rationality or logic is itself the humanity in human knowledge. After all, as it has been often argued: How do we distinguish humans from mere animals? – Well, of course, animals cannot reason!

The problem of the infinite regress

There were more serious problems for the humanists. The adequacy of logical proofs was always suspect. For a logical proof to be a justification, it must be possible to demonstrate the proof for all to see. Failure to do so is evidence of an error. An example of a failure to demonstrate is the so-called 'infinite regress'. If we give reasons for *why* some particular statement is true, we might be asked to show why we think our reasons are true. Following the Social Contract of Justification, we must step backward and provide another set of reasons to prove the truth of the first set of 'reasons'. But if that is possible, then any subsequent reasons can also be questioned. This requires still another backward step and another set of reasons. There is no limit to the number of required sets. Hence, we have an infinite regress. Such a possibility means that one could never provide a complete (and thus finite) proof of one's knowledge.

This is precisely the challenge of David Hume (1711–76). He argued that there did not exist any objective logic that could do the job of providing a logical proof of one's knowledge based *only* on experience. This is a serious indictment of Bacon's Scientific Method. It means that one cannot even get started. For example, whenever one claims to have collected the facts to prove one's knowledge is true, someone else can ask for an additional proof showing that one's facts are true as well as logically sufficient. In face of these difficulties, nineteenth-century romantics would have us consider relaxing the doctrine that to err is sinful. So, today most people would instead accept Goethe's claim that to err is human.

The real source of the problem for the believers in the Scientific Method is that the Method depends on the existence of an inductive logic – a logic which can proceed from the truth of

particulars (of experience) to the truth of general statements such as those which comprise anyone's knowledge. Although Hume may have recognized that such an inductive logic does not objectively exist, he argued that people still claim that, on the basis of *their* experience, they know that particular statements are true and those 'knowers' are often correct. Hume concluded that they therefore must have a workable inductive logic in their heads. Thus, we see how the study of knowing becomes the study of the *mind of the knower* – that is, of the psychology of *knowing*. If there is no objective rational proof of one's knowledge, then there can only be subjective proofs of one's knowledge. In this case, every rational proof of knowledge reduces to a study of the psychology of the knower.

Romanticism and neo-romanticism

The consequence of Hume's argument that knowledge exists in the minds of people, rather than in objective proofs which might please the Church, is that the minds of humans matter more than 'the facts' since the facts themselves must exist in the minds of humans. For many people today things have not progressed beyond Hume's observations. Most of the romantic literature of the early nineteenth century is merely examining the ultimate in truth – everything is centered in the human mind rather than in objective rationality. Even the existentialists (or neo-romantics) of the early twentieth century adopted the view that everything may be a product of the mind – hence everything may be arbitrary. In either case, the justification of human knowledge is supposed to be based on the rationality of the human mind and thus justified knowledge is a product of Human Nature.

An ultimate reliance on Human Nature as the foundation of explanations is precisely what some philosophers today call psychologism. It is this type of explanation which was rejected in my 1982 book and, of course, in Karl Popper's writings. But, as can be seen from my heuristic history of human knowledge, psychologism is only a symptom of a more serious problem – namely, the signing of the 'no-win' Social Contract of Justification by the eager and optimistic humanists.

Anti-justificationism

There is no reason why anyone today should consider themselves bound to abide by a contract they did not sign. Thus, everyone is quite free to make any claims they wish. That anyone *thinks* his or her theory is true does not guarantee the truth of that theory. Conversely, not knowing the truth of one's theory does not guarantee that the theory is not true. Likewise, the truth of one's theory or knowledge cannot be decided by a vote – simply because, even when the vote is unanimous, the voters could be unanimously wrong!

EPISTEMOLOGY VS METHODOLOGY: THE THEORETICAL PERSPECTIVE

The primary object of my heuristic story was to identify three elementary notions: (1) the doctrine of Manifest Truth; (2) the doctrine that To Err is Sin and thus error must be avoided; and (3) what I called the Social Contract of Justification. I turn now to examine one particular theoretical legacy of that contract – namely, the historic fusion of questions of epistemology with questions of methodology. The distinction between epistemology and methodology can be simply stated. Epistemology is concerned with the *nature* of knowledge (i.e. with *what* is knowledge) and methodology is concerned with *how* knowledge is acquired. In other words, epistemology is like a restaurant's menu whereas methodology is more like a street map showing how to get to the restaurant.

Sensationalism, methodology and epistemology

In addition to these three elements of theories of knowledge and methods of knowing, I wish to make explicit the common-sense notion about learning that says all knowledge comes by way of the senses. This view, called sensationalism, is the foundation of virtually all views of methodology and epistemology and is responsible for the fusion between epistemology and methodology. Here, I want to focus on the two major views which are based on sensationalism – inductivism and conventionalism – because, as I have been saying, they are found at the root of all methodological controversies and prescriptions in economics today.

One way to understand any theory is to understand the intellectual problem at issue. One can always take a retrospective view of any theory by conjecturing what problem is solved (intentionally or not) by that theory. This will be my program here for the study of methodology. Specifically, I will conjecture a problem situation in order to explain the existing views of methodology.

Throughout its long history, methodology has served to solve both epistemological and sociological problems. That is, methodologies have existed to deal with knowledge itself and with society's view of knowledge. Before discussing the specific matter of methodology in economics, I will attempt to formulate a general theory of methodology by discussing some of the philosophical and social problems that methodology has been, at times, thought to solve.

The primary philosophical problem that methodology has been said to solve arises out of various theories of knowledge which are based on the aforementioned Manifest Truth doctrine – namely, the doctrine that truth is there to be seen or discovered. The problem is: 'How do we mere humans *uncover* the truth without making errors if "to err is human"?' The 'how' will depend on the details of one's theory of knowledge, that is, on one's epistemology.

From the standpoint of sensationalism, the epistemological question ('What is knowledge?') is answered when one answers the methodological question ('How do I know?'). According to sensationalism, the answer to the second question is: '*I* know only by having either "observable facts" or "demonstrable truths"'; hence, 'knowledge is essentially factual or demonstrable'. This latter conclusion precludes the existence of theoretical knowledge, that is, of knowledge which is not based on sense observations or demonstrable 'truths' alone. The next question is: '*How* does one *have* the "facts" or "demonstrable truths"?' Is this methodological question separate from epistemological questions ('What are "facts"?' and 'What are "demonstrable truths"?')? The question of how one knows is not separable from specifying what the facts are or what is provable. The result is that methodology traditionally deals with the epistemological questions 'What are facts?' and 'What are demonstrable truths?'. If one followed Hume, the question of *how* I know would be considered a psychological phenomenon.

Inductivism

One variant of sensationalism which has been attributed to Bacon is what I have been calling inductivism. Inductivism needs to be further examined because it has been institutionalized. Its institutionalization has overcome its weak foundation, namely, the belief in the existence of an inductive logic. Inductivism attempts to answer simultaneously the methodological question 'How do I know?' and the epistemological question 'What is knowledge?'. It does this by attempting to objectify knowledge – that is, by making the logical basis of knowing non-psychological.

Bacon's inductivism objectifies knowledge by eliminating subjective influences in the process of establishing the 'facts'. Once the 'facts' are established the mental process becomes irrelevant since it can be replaced by a non-subjective inductive logic. To do this the *existence* of an inductive logic is simply assumed. Truth then will be manifest in the 'facts' if the facts and the logic are independent of human influence. For inductivist-sensationalism, methodology is thus a procedure which eliminates human influences and thereby minimizes error.

There are two important and well-known variants of inductivist-sensationalism. One is the verificationism associated with the twentieth-century 'logical positivists' and the other is classical empiricism. Both are well known to economists. Both have to do with the status of theories in the nature of knowledge. All that inductivism says is that if theories exist they must have followed *inductively* from the existing facts (hence cannot go *beyond* the facts). Verificationism allows for hypothetical leaps beyond the available facts so long as one goes back later and verifies the hypotheses with facts. It is in this spirit that we are urged to say something is 'hypothetical' if not known to be true. For classical empiricism all theories must always be directly related to *existing* facts. That is, no theory can go beyond experience – theories only *represent* our experience.

Most details of any inductivist methodology are concerned specifically with the question 'What are "facts"?' (e.g. distinguishing between positive and normative statements). This question needs to be answered in order to answer the primary methodological question 'How do *I* know?'. The question 'What are "facts"?' is dealt with by explaining *how* one should collect them. The quality of the facts is supposed to be related to the personal competence of fact collectors (e.g. collectors must be unbiased, unprejudiced, clear-thinking, etc.). From this perspective methodology is seen to be concerned with the personal mode of behavior of the 'fact collector'. In particular, can just any ordinary individual's observation report be accepted as a 'fact' worth noting or using? Obviously not.

Despite all its philosophical problems and controversial aspects, inductivist methodology lives on as ritual. Textbooks are written to satisfy inductivist principles, curricula are organized according to inductivist learning principles (viz learning from examples, no speculation before data collection, practical questions before theoretical ones, etc.).

The *combination* of the doctrine of Manifest Truth and the doctrine of sensationalism fails without something like an inductive logic. Although the combination has been institutionalized in academic economics through curriculum and textbook rituals, it is striking that it is no longer *openly* adhered to among economic methodologists. How does one abandon this combination of doctrines? There are three options available – abandon sensationalism, abandon Manifest Truth, or abandon both.

The view which results when denying sensationalism while still maintaining Manifest Truth is merely the well-known and oft-despised 'apriorism'. If we were instead to drop the doctrine of Manifest Truth but retain sensationalism we would construct the foundations of the philosophy I have been calling conventionalism. If we drop both doctrines we obtain the basis of Popper's views of methodology.

With apriorism all methodological matters reduce to matters of deductive logic (i.e. ordinary logic), hence reference to the real world is unnecessary. We need not discuss this further since there are so very few apriorists today. By denying Manifest Truth, conventionalism suggests that our senses need help – that is, that the facts we collect are always 'theory-laden' since factual reports contain theoretical elements which cannot be separated out. Conventionalism is the methodology which McCloskey [1983] calls 'Modernism'. Conventionalism needs to be clearly understood because it is both the methodology advocated today and the basis of most methodological arguments in economics.

Conventionalism

Given the hypothesized Social Contract of Justification, should *all* facts be theory-laden, the basis of knowledge would still need to be objectively justified yet this would in turn lead to an infinite regress. The combination of the failure to provide an inductive logic to make inductivism work with the failure to justify (rationally) any knowledge within the doctrine of sensationalism has always been the basis for many bitter disputes within the sciences and between scientists and non-scientists. How congenial the world would be if an inductive logic could be found. Almost all disputes could be rationally resolved since everyone could appreciate the logic. Another way to

avoid disputes over whose theories are supported by facts, and thereby shown to be true, would be to relinquish the idea that theories can be either true or false.

Giving up truth and falsity does not avoid a primary sensationalist problem – that is, the avoidance of controversies and disputes over whose senses have produced knowledge. Many think that what is still needed is an objective authority – something to substitute for the previous combination of inductive logic and Manifest Truth. It might be said that without an objective authority we would have mere 'existentialism'. The solution to the implied problem is rather easy, it would seem. We can still rely on rationality itself (i.e. deductive logic and mathematics) to be the needed objective authority. This is just the program of a conventionalist alternative to inductivism, namely to rely on universal rationality without giving up sensationalism.

Conventionalist methodology is concerned also with the question 'What are demonstrable truths?'. Like inductivism, this question needs to be answered in order to answer the primary, but now modified, question 'How do *we* know?'. Without Manifest Truth, conventionalist methodology consists of a set of (social) conventions or decision rules for accepting a given theory or for choosing one theory from a set of competing theories. The need for a (rational) choice exists because (the retained) sensationalism denies the existence of informative theories (i.e. information beyond the facts or known truths). The appearance of informative theoretical knowledge must be explained away if sensationalism is to be retained. By using non-theoretical criteria, possibly involving independent observations, we can choose to accept a theory. The standard means of making a choice is to view all theories as catalogues of 'facts', classification systems or even languages and then apply some criteria such as simplicity, generality, or minimization of statistical error with respect to observations. In other words, choose the 'best' approximation where the definition of 'best' is based on *explicit rational* criteria.

The 'explicit rational' criteria simply do the job that the doctrine of Manifest Truth was supposed to do when applying inductive logic. Their use avoids pure subjectivism in the process or state of knowing. Thus to complete the conventionalist version of sensationalist methodology, we need one more assumption which will ensure objectivity. That assumption is about the existence of universal rationality, namely, the view that if *everyone* begins with the same mutually consistent premises (or criteria) everyone will necessarily reach the same conclusions. Here it is the common acceptance of the criteria by rational (hence 'objective') people that is the basis of all knowledge. Facts are demonstrable truths. Facts, by being logically derivable from accepted theories, are thus defined by those theories used to demonstrate the truth of the 'facts'. By defining facts, theories have no epistemological status. It is the logically derived (i.e. 'valid') facts (hence demonstrable truths) which are the sought-after goal (viz knowledge). With conventionalism it is said that *we* 'know' when *we* accept particular theories. The only possible errors one could make within this conventionalist view of knowledge (which combines sensationalism with the *denial* of Manifest Truth) are those which result from being irrational; hence if one is rational then errors will be avoided. In short, conventionalist methodology, by choosing the 'best theory' to define the 'facts', solves the problem of establishing a factual basis for rational (social) agreement over what is knowledge.

Anti-sensationalism as a social theory of knowledge

I have been arguing that traditional philosophy has dealt historically with the question 'What is knowledge?' within the confines of the Social Contract of Justification and thus that knowledge can never be explained *without* explaining 'knowing'.

Although the origins of psychology may be found in the history of the problems of fulfilling the Social Contract, the everyday, commonplace solutions are more sociological. In simple terms, *knowledge is whatever a knower knows*. The only social problem then would seem to be about how to determine who the 'knowers' are. There are two extant solutions, which I will call the 'role theory of knowledge' and the 'status theory of knowledge'.

The *role theory* says that a knower is anyone who plays the role of a knower in society – the most obvious example is the 'expert witness'. In general, the role theory implies that 'it is not what you say, but how you say it' – but of course, how you say it may depend on what you want to say. Role playing with regard to knowledge is rather vague and uncertain. The *status theory* is much less ambiguous – it implies that 'it is not what you say, but who you are'. There are many obvious examples. Knowers usually hold university degrees or professional licenses.

Although role or status gives the appearance of solving the problem of determining who is a knower and hence what is knowledge, few philosophers or methodologists would ever be impressed. What can be noted, however, is that both theories are non-sensationalist. But of course, philosophers are generally more impressed by sensationalist theories of knowledge or method. The (sensationalist) view that knowledge is obtained through our senses can clearly be seen as a way of fulfilling the Social Contract of Justification. Inductivist-sensationalism is an attempted explanation of subjective knowledge (*I* know…) of the objective world. Conventionalist-sensationalism is an attempted explanation of group-subjective knowledge (*we* know…) of the objective world. At this stage let us consider a new question: 'Is it possible to explain knowledge *without* explaining the *process* of knowing?' An affirmative answer to this question is a denial of the Social Contract. Such a denial also makes it possible to reject sensationalism and instead adopt the view that all knowledge contains essential theoretical elements.

THE PRACTICE OF ECONOMIC METHODOLOGY

Conventionalist methodology in economics

When discussing their philosophy of science, most economists advocate inductivism in the long run and conventionalism in the short run. Of course, if one had an infinity of time, then one could always make induction work *in the long run*. Most economists who advocate conventionalism will readily admit that there is a problem *with* induction in the short run. These economists will be concerned with a different problem – namely, the conventionalist choice problem: 'How can we choose the "best" theory when there is no inductive logic?' This would seem to be a simple matter of economic analysis where the only question concerns our objective function – that is, our choice criterion.

The conventionalist's choice problem

Most methodological debates in economics are about the criterion to be used to choose between competing theories. I will list a few of the most commonly discussed criteria. Concerning the choice of one theory over another, conventionalism admonishes us to choose the theory which is one of the following: (i) more simple, (ii) more general, (iii) more verifiable, (iv) more falsifiable, (v) more confirmed or (vi) less disconfirmed. For the followers of Friedman's instrumentalism, that is, the economists interested only in solving practical problems, the confirmation criterion, (v), should probably be more important, but usually instrumentalism would have us just try each theory until one is found which *works* regardless of these criteria.

Criticizing conventionalist methodology in economics

While I never wish to prescribe methodology to anyone, I do think economists who wish to propound their versions of conventionalism ought to consider two elementary criticisms of conventionalism.

The first concerns the irrelevance of the conventionalist choice problem. Once one drops the Social Contract of Justification, choosing a 'best' theory would no longer seem to be essential. Of course, there may be sociological needs for choosing one theory. For example, textbooks are

easier to write when there is only one theory to be described. Also, a certified 'best' theory provides a shibboleth which can be used to determine who are the 'good guys' and who are the 'bad guys'. The choice of one theory among competitors might be appropriate for practical or policy concerns – since only one can be applied at a time – but the choice cannot solve any intellectual problems. Without the Social Contract of Justification, the onus is on anyone practicing conventionalism to show why we should even have to choose *one* theory.

The second criticism is quite simple. It concerns the circularity of conventionalist criteria. Although economic methodologists who practice conventionalism usually deny that a theory is true or false (a theory is either 'better' or 'worse'), they presume their criteria can be true. Each of the criteria listed above presumes something about *the* true theory of the real world. For example, saying the 'best' theory is one which is most simple presumes that the real world is essentially simple. In other words, whenever economic methodologists propose any particular criterion for choosing the 'best' theory, we can always ask, 'How do they know that is the "best" criterion?'. Of course, such a question can lead to an infinite regress. If instead economic methodologists argue that their proposed criterion is 'best' because by using it one can show that the chosen theory is 'best', then conventionalism is reduced to circularity.

Conventionalism and the sociology of economics

My many criticisms of conventionalism are sometimes acknowledged by economic methodologists but seldom heeded since economic methodologists regularly claim that they long ago rejected conventionalism. They often claim to have rejected the explicit criteria listed above since these criteria no longer seem to hold promise – even Popper's criterion of falsifiability. Some methodologists claim to have gone beyond conventionalism and even beyond Popper. But if a methodologist walks like a duck and quacks like a duck, then he or she is a duck. In a fundamental way it does not matter what methodologists claim they are doing. Of more concern is what economists do that depends on accepted methodology. In the remainder of this paper I shall dig deeper to show how conventionalist methodology permeates the economics profession and its practiced methodology.

By rejecting the Manifest Truth doctrine but accepting the romantic's doctrine that 'to err is human', practitioners of conventionalism would have us think that the fundamental social problem concerning knowledge is: 'How does our society, now and in the future, avoid mistakes with respect to understanding the world around us?' One of society's many social institutions is the economics profession itself. As such it produces economic knowledge which *represents* acceptable knowledge based on a rational minimization of error. The standard ways of making this representation concrete are the particular institutions of textbooks, professional meetings and, above all, academic departments and curricula. To understand more clearly how conventionalism permeates economics, I will now attempt to analyze each of these 'concrete' institutions to show that conventionalism is the methodology practiced among economists.

Textbooks

Standard textbooks are deliberate attempts to represent the consensus concerning accepted facts (and theories) in a given area of study. The logic of the textbook business is that a book can only become one of the standard textbooks if it does in fact represent the consensus in terms of both content and form. What the standard textbook contains is the latest accepted work on what are the accepted theories in a given area of study. Any would-be textbook whose contents deviate from this will fail as a textbook since it will not be generally used. The form in which textbooks are written is as important as their contents. Any attempt to deviate here may also be doomed. For example, in the area of elementary economics, where the consensus is very strong, one finds that virtually all textbooks about 'principles' contain only minor variations in their tables of contents

from that of the leading textbook; for years it was the one written by Paul Samuelson, today it is more likely Richard Lipsey's. Such mimicry is often true in more advanced areas such as microeconomic theory; for years, all accepted textbooks were variants of one of the older leading textbooks (perhaps one written by Richard Leftwich, C.E. Ferguson or George Stigler). Furthermore, most of the standard textbooks that do have an introductory chapter on methodology provide nothing more than a statement of some variant of conventionalism – even though they still give references to Friedman's 1953 instrumentalist essay, of course. The philosophical aspects of economic theories are confined entirely to that chapter – otherwise one might be suggesting that there could be some controversy over a particular theory.

The problem that is solved by such an institutionalized consensus (concerning the proper form and content of any textbook) is not clear. It might only be that it permits teachers to estimate what any rational student or colleague expects of them when teaching courses in a given area of study. Or it might help to assure that students are getting their money's worth. Most likely, it minimizes the obvious mistakes one might make in thinking about the given area of study.

Professional meetings

Specialized professional meetings are organized much like standard textbooks. Opening addresses (typically like after-dinner speeches) are usually the depository of all philosophical matters while the meetings themselves (i.e. lectures, symposia, etc.), that follow contain the non-philosophical matters. (Of course, meetings among methodologists can easily be exceptions.)

Ideally, the lectures, symposia, etc. would contain the latest attempts at solving new problems or the latest findings concerning some old problems, thereby solving the social problem of keeping the profession aware of new developments. Unfortunately such meetings are very difficult to organize. In reality the meetings are characterized either by 'cronyism' or by 'anti-cronyism' – either one invites papers only from friends or one does not invite papers from any friends. Cronyism is most prevalent today.

To organize a large professional meeting, a select group, supposedly representing the consensus concerning the proper areas of interest, delegates the job of organizing sessions in chosen areas. Usually, the criteria applied to choosing papers for presentation would be irrelevant for an ideal meeting. In particular, large meetings today usually serve the purposes both of a social gathering and a market for recruiting and employment. Although the purpose of ideal meetings can be used to explain why smaller professional meetings continue to be held, if the social aspects were recognized as the real purpose, the universities or companies that pay the expenses of holding the meetings would be unwilling to finance the attendance of an ordinary member. But they are quite willing to finance the intended consequences because these promote the progress of science through timely communication of the latest developments, findings, etc.

Departments and curricula

Despite what some economists might think, the administration of academic economics is quite similar to that of other academic disciplines. By far the most interesting social phenomena of the scientific community are the academic institutions of departments and curricula. Let us consider some problems that might be solved by having separate departments of Economics, Physics, Sociology, Philosophy, etc. Since the conventionalist view is that scientists do not get involved in arguments over truth, one way to make sure that this view is correct is to separate those 'schools of thought' administratively such that there is little contact, hence overcoming the social problem of having scientists 'fighting it out'. In other words, separating departments within a university or partitioning a department into such sub-disciplines as microeconomics, macroeconomics, international trade, industrial organization, managerial economics, finance, accounting, etc., makes the practice of conventionalism possible. By grouping together those scientists who speak the

same ‘language’, it makes agreement more possible since if they speak the same language they will be able to concentrate on the logic of the discussion. Similarly, since all rational people will ultimately agree if they start from the same premises, if we group together scientists who use the same premises we minimize the possible disagreement. Moreover, since those in one group (by definition) will accept the same theories, they will agree on what are to be the accepted facts in their area. This makes it possible to write textbooks, hold meetings, etc. Above all, agreement on facts makes it possible to agree on what students must learn.

Since the entire fabric of the academic scientific community is organized to prevent (embarrassing) disagreement from breaking out and thereby organized to make the ordinary economists’ conventionalist methodology work or seem to be true, we cannot risk allowing students to be a source of disagreement. Thus, students must be socialized as soon as possible. The primary technique of socializing them is to have a set pattern of prerequisite courses that they must take *before* we allow them to think on their own about any particular area. If such an organization is successful, again one can show that conventionalism today is true *by construction*. (Such a proof would be very popular among mathematicians and other advocates of conventionalism.)

The methodology of mathematical economics

In my 1982 book I explicitly examined the two ways in which the economic researcher practices conventionalism. One of my chapters presented the view that all of analytical economics is ‘defeatist conventionalism’. Analytical economics retreats to dealing with analytical truths that are not dependent on empirical statements about the real world rather than dealing with the difficult problems of determining the truth of statements about the real world. Another chapter presented the view that all of positive economics is ‘optimistic conventionalism’. Specifically, I argued that positive economics is nothing but repeated attempts to prove inductively that neoclassical economics is true. I said it does this by showing that neoclassical economics can be successfully used to explain ordinary behavior.

For many years, the existence of these two competing views of the appropriate methodology for economics (which is really a family dispute) fostered considerable tension in many Economics departments. In recent years, things seem much less tense. There are two possible reasons for the reduced tension. First, many optimistic proponents of conventionalism have retreated to departments of Applied Economics that are located within business schools. And second, those optimistic economists who have remained have found ways to co-exist without surrendering to extreme hard-core mathematical economists, that is, to those who are interested in formalism-for-formalism’s-sake. The result is an economics discipline that can appeal to most positive economists and to most theorists interested in mathematics-based model-building techniques. Today, nobody would ever feel that they have to choose between positivism and mathematical economics. non-hard-core mathematical economics journals). Even journal editors and referees will accept both types of papers. However, to be accepted, a paper must obviously either involve a logically rigorous model or provide empirical evidence about a model. This limited compromise has allowed positive economics to acquire a dominant methodological position in the economics profession, as is evident in almost any generalist journal (i.e. non-hard-core mathematical economics journals). By all means, having achieved a successful détente, it would be unwise to allow anyone to rock the boat. To see this, one need only look at the last fifty years of the *American Economic Review* to see how the use of mathematics has developed. It is astounding to find that in the entire 1950 issue of the *Proceedings* there was only one equation. Opening almost any recent issue of the *American Economic Review* [June 1991], one will find about fourteen major articles. About half will show some concern for formal ‘theorems’, ‘lemmas’, ‘proofs’, etc. Five others will be exercises which apply econometrics to formal mathematical models. Of the remaining two, one may be a report on survey results and the other is an exercise in pure mathematical economics. Things have really come a long way in fifty years.

Despite the monumental growth of mathematics-based economics, there seems to be no public discussion of the use of mathematics in economics. Ten years ago, my colleague Herbert Grubel and I surveyed opinions concerning the economics of mathematical economics [see Grubel and Boland 1986]. That is, we asked prominent economists whether they think there are any net benefits to encouraging more mathematical economics at the expense of more modest literary and applied economics. The idea of even asking about net benefits caused much wailing and abuse from those of our colleagues who spend most of their time manipulating mathematical models. But just what *are* the net benefits?

Let us look at the commonly stated benefits. The most common claims are that mathematics ensures a high degree of 'rigor' and promotes 'economy of thought'. This latter is related to mathematics being a 'common language'. Without arguing whether mathematical economics is rigorous, or whether also non-mathematical economics is incapable of rigor, it is interesting to note that whenever we make our theories and models more dependent on mathematical analysis neither of these supposed attributes of mathematical economics ensures that we will thereby be able to make better predictions or that our models will be true or better able to explain economic phenomena. The ostentatious use of mathematics-based models is only a matter of 'proper scientific form' rather than substance. The emphasis on form rather than substance is a characteristic of conventionalism. Since conventionalism denies that theories can be true or false, what can be of concern other than form?

The question to ask believers in mathematics-based positive economics is, just what has been accomplished in the last fifty years? While the believers will be quickly getting their list ready, a better question is, what has been accomplished with mathematical model building that could not have been accomplished without mathematical model building? The honest answer to the second question is that nothing has been accomplished that could not be done without sophisticated mathematics. And whatever is listed for the first question will be seen to be an accomplishment only by believers.

I think I have said enough to indicate that the methodology practiced in economics is what I have been calling conventionalism. The firmly established acceptance of mathematical economics even among those interested in positive economics is the most convincing evidence. Form is more important than substance, and logical validity by itself is considered more important than difficult questions of empirical relevance. Today if you wish to show you are a 'knower', you had better express your thoughts using mathematics-based model buiding – even if you are interested in so-called positive economics. Make sure you have used only acceptable techniques of analysis. In the 1990s, some form of game theory seems to be the most promising strategy. And, if you want tenure or promotion, you would be wise to try to publish your papers in journals with status, that is, in those that give prominence to mathematics-based economics. But most important, never be caught worrying about the truth of your analytical models or how you might learn whether your model is actually true or false.

NOTE

1 As is the tradition in discussions of economic methodology since the publication of McCloskey's first paper on economic rhetoric [McCloskey 1983], everyone is careful to distinguish between big-M Methodology and small-m methodology. The former involves the philosophers' Big Questions and the latter concerns only the everyday business of practicing economists. In this spirit I am distinguishing here between big-S and small-s science (and scientific method). The big letter Science is built upon beliefs and promises that go beyond what is possible. The small letter science is about the unassuming business of everyday science.

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