[9]

The Methodological Debate Between Carl Menger and the German Historicists

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1

During the past thirty years methodology and methodological issues have become a significant preoccupation of prominent economists in almost every field of the discipline. Most attention seems to focus on the question of the "realism" of economic theory and/or the adequacy of its "foundations" or basic assumptions.

General questions pertaining to "realism" and complaints about the "unreality" of economic theory are not new ones. In the nineteenth century, the historical school accused the classical school of economics and then the Austrians of producing "unrealistic dogma" and "fantasies" divorced from empirical reality. In return, Carl Menger criticized the historicists for their incapacity to transcend that same empirical reality, that is, for their failure to deal with what he termed "economic reality." The resulting bitter and inconclusive dispute between the two factions became known to economists as the *Methodenstreit* ("Conflict of Methods").

One of its major consequences has been to nurture the apparently widespread belief that methodological conflicts are at best sterile and irresolvable and, at worst, positively counterproductive. Such a belief can hardly lead to a sanguine view of current discussions and arguments.

It is the main purpose of this paper to present an analysis of the Methodenstreit that both explains its inconclusiveness and suggests how it and similar disputes may be more productively evaluated.[1] It is hoped that by so doing, the good name of methodology may be restored to some extent and current disputes placed in a more favorable light. The major argument of this paper is that opposed epistemological positions underlay the methodological and morphological issues actually debated by parties to the Methodenstreit. Unfortunately, neither faction clearly and explicitly recognized that the source of their differences in methodological beliefs was epistemological. To this is attributed the inconclusiveness of the debate and the participants' preoccupation with more subsidiary matters. It is contended that the failure to identify the epistemological aspect of the conflict has led historians of economic thought to assessments that are inadequate as explanations of the quarrel's value, sources and basic rationale.

Section 2 summarizes the historical aspects of the Methodenstreit, speculates on its genesis, and notes its historical results. Section 3 classifies and summarizes previous assessments of the conflict. Section 4 offers a new and substantially original thesis concerning the questions really at issue between the Historical and the Austrian schools and the requirements for their solution. Section 5 concludes with a brief evaluation of Menger's part in the debate.

2

The Methodenstreit took place in the form of an exchange of publications between Carl Menger (1840-1921) and Gustav von Schmoller (1838-1917). Menger, the founder and chief spokesman of the Austrian school of economic theorists, directed an attack in 1883 against the German Historical school—of which Schmoller was the leader and primary spokesman at the time. The attack occurred in an essay on the subject of the appropriate goals and methodol-

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[1] For a detailed discussion of the question of the origin of methodological disputes among economists (and, by implication, among all social scientists) and a presentation of what is needed to settle such disputes, see [Bostaph, 1977].

ogy of the social sciences, titled *Untersuchungen über die Methode der Socialwissenschaften und der Politischen Ökonomie insbesondere* [Menger, 1883]. In that essay Menger not only presented his own views on the nature, problems, limits, and methodology appropriate to economics and other social sciences, but he incisively criticized those of the historical school.

The publication of Menger's essay was anything but gratefully received by the historicists. Gustav Schmoller reviewed the book in his journal and expressed strong opposition to key elements of Menger's position [Schmoller, 1883]. The rebuttal to this adverse review took the form of a series of 16 letters to a friend, published under the title *Die Irrthümer des Historismus in der Deutschen Nationalökonomie* [Menger, 1884]. The letters were highly polemical in nature and consisted mainly of a restatement of Menger's position on each question in dispute, laced with a few choice invectives directed against Schmoller.[2]

Not surprisingly, Schmoller took enough offense from Menger's polemic that he closed the debate abruptly by not reviewing the book and returning the review copy of *Die Irrthümer* to Menger with an insulting cover letter that was subsequently printed in Schmoller's journal.

Despite the brevity of the debate itself, the specific publications through which it was conducted were representative of much more than merely a difference of opinion on methodological issues between the spokesmen of two rival schools. They represented a fundamental opposition between two basic methodological tendencies, the historical-empirical and the abstract-theoretical.

Menger's initial attack was directed not at Schmoller, but at the entire school of economists and historians of which Schmoller happened to be the leading contemporary representative. This school—the historical school—originated in Germany in the 1840's with Wilhelm Roscher and existed until well after the turn of the century. On the other hand, Menger's school—the Austrian school—began with his first publications in the 1870's and still exists in the form of fifth and sixth generations of scholars today [Dolan, 1976].

If the historicists had not been so critical of the classical school and so intolerant of any methodologies that they interpreted to exemplify the "excesses" of the deductive method of that school, Menger might have had less reason to open the debate with an attack on the historical school itself. Additionally, animus between members of each school continued to exist after the formal end of the debate in 1884 and surfaced in the form of sporadic statements, book reviews, and so on, for at least two decades thereafter. Rather than just a conflict between two scholars, the Methodenstreit represented a general clash between the entire German Historical school and the Austrian school.

The genesis of the conflict is by no means as easy to document as its denoument. The publication of Menger's *Principles of Economics* in 1871 received little response outside Austria [Menger, 1871]. (It was not translated into English until seventy-nine years after its publication.) In Germany, the historical school was growing in influence and becoming increasingly critical of any faction which had any affinity to, or smacked of, theoretical analysis in the tradition of "Manchestertum." Despite the obvious differences in general methodology employed by Menger compared with that of the English Classical school, the stigma of an "abstract-deductive" system was enough to preclude his work from consideration by the "more advanced" or "modern" school—as the historicists (and especially Schmoller) viewed themselves.

This situation must have been an intolerable one for Menger, given the originality and ambitiousness of his own work. He had, of course,

[2] Menger justified the relatively low scholarly level of his comments and the frequent use of *ad hominem* against Schmoller by arguing that when scholars, such as himself, are attacked by an "ignoramus" (Schmoller), they should use the opportunity to address his audience and peers—the lay public—on a level that they can understand. See [Menger, 1884, p.2].

intended the *Principles* to be only the first, general part of a comprehensive treatise on economic theory. But as Hayek explained [1934, p.405]:

> "... under these conditions it was only natural that Menger should consider it more important to defend the method he had adopted against the claims of the Historical School to possess the only appropriate instrument of research, than to continue the work on the *Grundsätze*."

Perhaps this can be more readily seen if the reception of Menger's *Principles* by the German journals of the time is reviewed.[3]

In the early 1870's only four professional journals devoted to economics were published in Germany: *Jahrbücher für Nationalökonomie und Statistik; Vierteljahrschrift für Volkswirtschaft und Kulturgeschichte; Zeitschrift für die gesammte Staatswissenschaft;* and *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft,* known as *Schmollers Jahrbuch* and an organ for the historical school. Of these four, only the first three published reviews of the *Principles* [*Jahrbücher,* pp. 342-5; *Vierteljahrschrift,* pp. 194-205; *Zeitschrift,* pp. 183-4]. The review in the *Zeitschrift für die gesammte Staatswissenschaft* missed the central idea of the book while that in the *Vierteljahrschrift für Volkswirtschaft und Kulturgeschichte* did little better, agreeing with Menger's method but finding no innovation in his theory of value. The journal founded in 1863 by the historicist Bruno Hildebrand (and the best of the four), the *Jahrbücher für Nationalökonomie und Statistik,* reviewed the *Principles* by deploring the writing of short textbooks on economics by young men, while *Schmollers Jahrbuch*, the principal historicist organ, did not review the book at all.

Menger must have regarded his work as a positive contribution and an addition to the research previously published in Germany. Had he not dedicated the book to Wilhelm Roscher "with respectful esteem" and concluded his "Preface" with the following tribute? [Menger, 1871, p. 49]:

> "Let this work be regarded, therefore, as a friendly greeting from a collaborator in Austria, and as a faint echo of the scientific suggestions so abundantly lavished on us Austrians by Germany through the many outstanding scholars she has sent us and through her excellent publications."

One can imagine the frustrations of the young author (Menger was only thirty-one when he published the *Principles*) at the reception of his efforts. It would be entirely understandable for him to investigate the question of the sources of the poor reception of such an original work.

Not only that, but given his own strong convictions concerning the methodology proper to the derivation of general economic theory, he had ample incentive to publish his views in the form of an essay. He would have had even more reason to publish such a work when he concluded, as he did, that his *Principles* was poorly received because the historicists granted no legitimacy to his method while failing to recognize the limitations of their own. As Hayek remarks [1934, p. 405], "... he might well have thought that it would be wasted effort to continue [his work on his treatise on economic theory] while the question of principle was not decided." Whatever his reasons, the *Untersuchungen* was published in 1883; and this time *Schmollers Jahrbuch* not only did not fail to print a review as it had failed to do of his *Principles*, but Schmoller himself wrote that review.

The actual issues considered by Schmoller and Menger in the course of the conflict between their schools included: (1) the criteria for designating economic history, economic theory, economic policy, public finance, and statistics as "branches" of economics; (2) the scope and goals of each "branch"; (3) the usefulness of theory in the explanation of empirical events; (4) collectivistic versus individualistic conceptions of economic phenomena; (5) the nature of institutions and their development; (6) the extent to which historical and statistical material—as opposed to the experience of everyday life—are germane to the abstractions of economic theory; (7) the related question of the

[3] The following is based on material found in [Howey].

relevance for economic theory of the complexity of man's psychological nature and the impact of his cultural setting on it; and (8) the "necessity," or causal status, of economic laws and the testing of such laws (and economic theory itself) with empirical data [Bostaph, 1976].

The Methodenstreit was inconclusive at the time and later assessments (to be reviewed in the next section of this paper) by historians of economic thought have been generally negative. Because of his influence, Schmoller was able to exclude all adherents of the Austrian school and the "Austrian method" from academic posts in Germany [Mises, 1969]. Menger was mainly preoccupied for the remainder of his life with the questions raised in the dispute and with other methodological considerations. To this fact has been attributed his failure to complete the writing and publication of his own general economic treatise [Hayek, 1934, pp. 406, 415; 1968a, p. 460; 1968b, pp. 125-6].

3

Assessments of the general historical and methodological value of the Methodenstreit published by historians of economic thought and others may be usefully divided into three categories: (1) those that argue that it was largely a waste of valuable time by the parties involved [Wicksell, 1958; Gide and Rist, 1948; Schumpeter, 1954; Hutchison, 1953, 1973; Seligman, 1962; Lekachman, 1959; Newman, 1952; Landreth, 1976; Ekeland and Hebert, 1975]; (2) those that take no position on the value of the controversy itself, but merely attempt to outline it and comment on the issues and implications [Ingram, 1967; Haney, 1949; Rima, 1978; Roll, 1974; Oser and Blanchfield, 1975]; and (3) those that indicate the controversy was an important and valuable one [Böhm-Bawerk, 1890; Seager, 1893; Hayek, 1968b].

Reasons given for viewing the dispute as a "fruitless" waste of time are varied. Knut Wicksell sees the choice of method to be a pragmatic question not worth such a "literary feud" [Wicksell, p. 193]. Charles Rist makes clear his own belief in a place for the historical method coequal with the abstract approach favored by the classical school (which he also identifies as Menger's method) [Gide and Rist, p. 400].

This status of the two methods is supported by Ben Seligman [Seligman, p. 274], "... since there ought to be enough room for both approaches in a field that pretends to deal with human society in motion," by Robert Lekachman [Lekachman, p. 249] because "... some problems yield to one technique and other problems to its alternative," and by Harry Landreth [Landreth, p. 275] because "... a healthily developing discipline requires a variety of methodological approaches."[4] Joseph A. Schumpeter regards the clash as one that [Schumpeter, p.814] "... was about precedence and relative importance and might have been settled by allowing every type of work to find the place to which its weight entitled it."[5]

In a 1973 article, T.W. Hutchison identified the differences in method advocated by the two schools as determined by the differences in the field of studies of interest to each. He argued that Menger's methodology is suited to the study of microeconomics, while that of the historicists is suited to the study of macroeconomics. Thus, the Methodenstreit was "... a clash of interests regarding what was the most important and in-

[4] It is difficult to be sure to what "method" Landreth refers. He cites a differentiation of theory from history, deduction from induction, abstract model building from statistical data gathering without rigorously linking each to a faction in the Methodenstreit, or clearly indicating how each constitutes a methodology opposed to the other. This may account for his apparent failure to recognize the differences in Menger's methodology compared to that of Jevons' and Walras', with whom Menger is uncritically lumped.

[5] Schumpeter is only referring, insofar as the historicists are concerned, to the Younger Historical school. He does not consider the Older Historical school to actually be a "school" in the sense of "a definite sociological phenomenon." Cf. [Schumpeter, 1954, pp. 808-9] in this regard.

teresting subject to study . . . ."[6] He concluded that the two methods must be combined in order to find answers to the important economic questions in all fields. A more naive version of this conclusion is that of Philip Charles Newman [Newman, p. 195] that ". . . the inductive method is an indispensable complement to the deductive."

Those writers who take no explicit position on either the historical or methodological value of the Methodenstreit generally present the same or similar assessments of its nature as do those who denigrate it. John Kells Ingram [Ingram, p. 235] concludes that the differences that existed between the two factions ". . . [were] mainly differences of emphasis [of how important theory is and what are the practical economic sciences] due to radical temperamental differences." He argues that the dispute served to reveal similarities in the views of Schmoller and Menger in that each method had its place and each was essential to the development of economics. Lewis Haney argues that each method has its place and prescribes a need for both inductive and deductive methods, as does Ingrid Rima [Haney, p. 550; Rima, p. 177].

Characterizing the initiation of the Methodenstreit as "a means by which the new theory [marginal utility] sought to clear its own mind," Eric Roll argues that there were actually no substantial points of disagreement between the two factions and that they both eventually realized this, which led to the decline of the controversy. Specifically, Roll explained [Roll, 1974, pp. 307-10]:

> ". . . the two methods which were contrasted were not mutually exclusive and had indeed been used together by the greatest of the classics. There is clearly room for serious disagreement about the choice of premisses; but it is generally admitted that premisses which stand at the beginning of the deductive process are themselves empirical in origin. Induction and deduction are interdependent."

The same general conclusion characterizes the views of Jacob Oser and William Blanchfield [Oser and Blanchfield, pp. 204-11].

Three writers take the position that the Methodenstreit was worth the time and effort expended on it. H.R. Seager was a student at both Berlin and Vienna during the early 1890's, and studied with both Schmoller and Menger. Although he takes no position on one side of the dispute or the other, he argues that it is certainly time (1893) to be deciding what methods are appropriate to conduct what studies. The conflict was thus "of a decided scientific value" because it cleared away many misapprehensions [Seager, p. 237]. Eugen von Böhm-Bawerk argued for the parity of the two methods–assigning each to its proper sphere of concern [Böhm-Bawerk, p. 256], although he reserved the category of theoretical problems for the Austrian approach [Böhm-Bawerk, p. 258].

Lastly, Friedrich A. von Hayek [Hayek, 1934, p. 406] identifies Menger's emphasis on the "atomistic" method of analysis and his ". . . extraordinary insight into the nature of social phenomena . . ." as valuable results of the dispute.[7]

In general, it can be seen that whatever their differences regarding the fruitfulness of the Methodenstreit, most of the major historians of economic thought who have examined the dispute are united in the belief that the methods advocated by the two schools are, if not complementary goods, at least coequal in their usefulness for research. According to this view, some economic problems are best investigated through the use of the "historical method," while others require the "abstract-deductive

[6] [Hutchison, 1973, p. 34]. Unfortunately, Hutchison interprets a comment made by Menger in 1894–to the effect that the difference between the two schools was founded on a different view of the objectives of research–to denote confirmation of his (Hutchison's) own conclusion. Menger probably meant that the historical method was useful for some purposes (history and empirical studies–given that they were conducted on the foundation of adequate general theory) while the "isolating" method was the means of constructing such theory. If Menger had meant that the historical method was at all useful for constructing general theory (macro or any other) he would have been repudiating a decade of his own stated views.

[7] Cf. [Hayek, 1973, pp. 8-9].

method." Some may even benefit from the use of both methods together. In cruder (and more simplistic) terms, both "induction" and "deduction" have their place. Acceptance of this position could (and, in some cases, did) then quite understandably lead to a negative value assessment of a controversy between factions who apparently failed to realize this simple truth, and who each fought unrelentingly for the exclusive adoption of that faction's own method in economic research.

In addition, because neither side in the dispute was converted to the other point of view and a great deal of lasting antagonism was generated between the two schools, the debate has been generally regarded as counter-productive on these grounds alone. Add to this a belief that the issues in dispute were minor or irrelevant and/or the positions of the adversaries highly similar, but obscured by rhetoric, and it is not difficult to accept the view, as many have, that the controversy was wasted energy—a pointless quarrel.

But, why would one of the most brilliant theoretical minds in the history of economics (Menger) waste his energies in a pointless quarrel? It is the purpose of the next section of this paper to demonstrate that such views result from a superficial evaluation of the conflict and an inadequate understanding of its sources and basic rationale.

4

Ludwig von Mises recently identified the Methodenstreit as an epistemological struggle generated by Menger's rejection of the epistemological foundations of historicist methodology; however, he concluded that the controversy [Mises, p. 27] ". . . contributed but little to the clarification of the problems involved." Schumpeter had started in the same direction many years previously in his *Economic Doctrine and Method* when he noted that [Schumpeter, 1924, p. 169] ". . . epistemological differences, which in themselves have nothing to do with economic method, were dragged into the discussion; nevertheless it brought about without a doubt a clarification of views."

Unfortunately, Mises did not state his reasons for his conclusion and Schumpeter grew to accept the viewpoint that the debate was about precedence and relative importance. This section will summarize a recent analysis by this author of the Methodenstreit that clearly identifies its nature as an epistemological struggle at root and infers the main epistemological issues and their positions on those issues from the specific writings of the disputants.[8] The key to the recognition of the epistemological nature of the quarrel is found in arguments concerning the last three of the eight major issues debated (summarized at the end of Section 2 above).

In arguing issues (6) and (7), the historicists and Menger were actually arguing over the theory of concepts. The question that underlay issue (6) was: Are concepts merely labels attached to universal summarizations that are subject to alteration depending on how extensive the data is from which they are derived (Schmoller), or are they abstract generalizations from only a few instances that have universal applicability (Menger)? That is, for issue (7), does the concept of a complex entity, such as man, have to refer to an enumeration of all his empirical characteristics and recognize all these when it is used in the construction of theory (Schmoller)? Or is there some central characteristic, such as "self-interest," which can be emphasized without robbing the derivation and use of the concept of all legitimacy (Menger)?

With respect to issue (8), another issue underlay the ostensive one: the issue of the nature of

[8] [Bostaph, 1976.] The multitude of sources used and their correct identification precludes specific reference to them here. Interested readers should refer to the dissertation itself for detailed specific references and justification for the interpretation of them presented in this current paper. Other aspects of the Methodenstreit not mentioned here due to space limitations are also treated in that work.

the law of causality and its application to economic laws and reasoning. Does an "empirical" law make the same statement of necessity as an "exact," or deductive law (Schmoller), or is there a difference in kind between them (Menger)? What, then, is the nature of causality?

The position that each faction took on these epistemological issues led them to their methodological positions and prevented methodological agreement between them. I shall argue that those epistemological differences underlay the methodological criticisms and dissatisfactions expressed by each party to the dispute; that is to say, conflict on the epistemological level surfaced as conflict on the methodological one.[9] Some supporting and conflicting material from other sources will be indicated where appropriate.

The German Historical school is conventionally divided into the "Older" and "Younger" Historical schools–Wilhelm Roscher, Bruno Hildebrand and Karl Knies being identified as the Older school, and Gustav von Schmoller being the leader and spokesman for the Younger school. In spite of the fact that the Older and Younger Historical schools differed in many respects (the most significant being the absence of any belief in either organicism or "absolute" laws of economic development in Schmoller's writings), they were united in their primarily empiricist orientation. As empiricists, the similarity between them rests most firmly in their argument for the application of a descriptive "historical method" to the data of history in order to derive economic laws–whatever they may have argued concerning the scope or necessity of those laws. This does not mean that they made such an application, it means only that they argued in favor of such an approach.

Roscher asserted that he sought absolute laws of economic development in inter-temporal and inter-spatial comparisons between societies, social processes, and social institutions. Rather than the study of individual economic behavior, Roscher advocated a "holistic" approach, an examination through historical comparative studies of *national* economic behavior. Hildebrand and Knies differed with Roscher concerning the "absolute" nature of any laws of development so obtained, but they did not reject his empiricist and holistic method. The laws of development that Roscher hoped to derive through historical comparative studies were to be different in nature from the economic laws that would characterize a given "stage" of a particular society. Such "short-run" laws would be relative in space and time and could not claim universality. For short-run purposes, the "physiological" processes of specific economies were to be described and "relative" economic laws formulated. This utilization of an empirically descriptive approach to derive laws differing in their degree of necessity implied epistemological inconsistency–an inconsistency that Schmoller later eschewed.[10]

In his own version of the "historical method," Schmoller denied entirely the existence of non-empirical law in economics, that is, the existence of any "absolute" laws of development embodying a degree of necessity not found in "relative" laws; however, Schmoller's writings contain the most significant clues to the historicist theory of concepts and the most explicit statements of their view of causal relations. A coherent, reasonably consistent epistemological framework can be constructed for the entire historical school on the basis of Schmoller's writings that explains and, in a sense, brings consistency to the writings of his forbears to a degree that they failed to achieve. It also preserves their identification as empiricists.

The historicist position has been aptly labeled "methodological collectivism" by Schumpeter because of its concentration on social institu-

[9] A discussion of the general relation between epistemology and methodology and the use of the knowledge of this relation in the examination of methodological conflict will be found in [Bostaph, 1977].

[10] Detailed references and justification for the foregoing interpretation of the Older Historical school will be found in [Bostaph, 1976, Chapter II].

tions and processes that are collectives of individuals or of their relations. Roscher and the Older historical school were more interested in the study of the entire social "organism" and its evolution, while Schmoller studied institutions and their interrelation, and social processes within the national economy. More specifically, Schmoller argued for the observation, description, classification, and formation of concepts of social institutions, their relations, and the relation of the state to the economy. He viewed his descriptive work as the necessary preparation for describing the "general essence" of economic phenomena, or general theory.

Schmoller, in searching for the "essence" of specific phenomena though, desired a description of all characteristics to the extent possible. The more complete the description, the more accurate and representative he believed the concept of a phenomenon to be. "Essence" was to be obtained by a summarization over entities of all their characteristics, rather than an apprehension or perception of a central and defining characteristic. Schmoller can thus be most usefully identified as a "nominalist" in his theory of universals, although a less consistent and more investigative nominalist can scarcely be imagined.[11] Schmoller's "essences" are virtually encyclopedic, while most nominalists would find such completeness unnecessary.

Schmoller's notion of causality, like his notion of concepts, is a descriptive one; it is one of empirically observed and verified uniformity in sequence. To Schmoller, the essence of a concept is subject to modification as the number of existents to which it applies increases. Because a theory of concepts is an implicit assumption in any theory of causality, it follows that causal relations among existents must also appear contingent on the experiential context in which they occur to someone believing concepts are contingent–if he is consistent. By the examination of experience using the comparative method, Roscher had hoped to distill generalizations of uniformities of sequence in phenomena in the form of laws of development. But Roscher had expected these to be "absolute" laws, whereas the laws for which Schmoller searched were not viewed in that manner. Instead, he confined his attention to the discovery of "short-run" empirical economic law.

The notion of causality that best explains the "short-run" relative laws sought by the Older historical school and the empirical laws sought by Schmoller is that of David Hume. It was Hume who originally offered the explanation of the causal relation as merely uniformity in succession. In this theory events are perceived either together or in succession. All that is meant by causality is that events have been perceived in succession, not that any intrinsic or necessary connection uniting these events has been perceived. Human thought processes interpret this sequence into the relation of cause and effect. Thus, necessity is something that exists only in the mind, not in objects. It was such an "inner necessity" of thought to which Schmoller must have been referring when he argued that empirical laws obtained by the "historical method" embody the same degree of necessity as

[11] Nominalism holds that concepts are names applied to existential phenomena after men arbitrarily pick certain characteristics of those phenomena to designate them in the future. Only the name is universal, not the phenomena. Designating Schmoller a nominalist reconciles his position on the theory of universals with that on the nature of causality–which seems thoroughly nominalist. For an argument that historicism actually embodies the "methodological essentialism" of Aristotle see [Popper]. Popper may be describing as "essentialist" the Hegelian elements in the approach of the Older Historicists (Roscher, in particular) rather than characterizing the historical school as a whole. Unfortunately, he identifies no members of the historical school specifically.

[12] [Schmoller, 1883, p. 280 (978).] The transmission of Hume's epistemology to the historical school occurred, more than likely, through the medium of John Stuart Mill. Schumpeter argues [1954, p. 540] that ". . . Roscher . . . went out of his way to express agreement with J.S. Mill's methodology." Cf. [Roscher, pp. 105-6]. Hume's influence on Mill is specifically mentioned in [Windelband, p. 635; Jones, p. 164]. Schmoller, also, speaks approvingly of Mill [Schmoller, 1883, p. 281 (979)]. Nevertheless, the identification in this paper of historicist epistemology as Humian rests on similarities in expressed views rather than on any historical "detective" work.

laws obtained by Carl Menger's "exact" or abstract deductive method.[12]

Just as the "essences" of Schmoller's concepts of things are subject to alteration as additional characteristics of the subject phenomena are observed through time or across cultures (and so his concepts are dependent on the context for their content), so perceived causal relations (that is, economic laws) are subject to change as further investigations of empirical phenomena reveal apparent influences other than those first identified. Because every empirical context differs in some respect(s), no concept or conceptual relation is ever truly universal. All are relative to the context from which they are derived.[13] Schmoller's view of causation is thus a strictly empirical one and is fully consistent with his theory of concepts.[14]

Unlike the historicists, who spoke of only a single method appropriate to economics, Carl Menger maintained that several methods are useful in economic research. The methods of economic history differ from those of economic theory, which differ from those of economic policy. This is so because the formal nature of the knowledge in each subdivision is different, as are the goals sought.

He argued that the goal of economic history is the description of the individual nature and individual connection of economic phenomena; however, in order to summarize and generalize over time, history must consider economic phenomena *collectively* (as opposed to the *singular* consideration of an individual). Economic theory, on the other hand, seeks to discover the *general* nature and *general* connections of economic phenomena. *Collective* and *general*, then, have entirely different meanings, and considerations of collective phenomena and general phenomena are each appropriate to different branches of economics.

He believed that in seeking the general nature and general connections of economic phenomena, economic theory may employ two different approaches—the "exact" and the "realistic-empirical." Both result in economic theory, but differ in the degree of "absoluteness" to be assigned to their results.

Regularities in the coexistence and succession of phenomena discovered by the "exact" approach admit no exceptions because of the process of cognition by which they are recognized. In order to derive "exact" laws, it is first necessary to establish what constitute typical phenomena. Thus, Menger's initial concern was the theory of concepts, or universal ideas. To Menger, the identification of an empirical form or type was the identification of an essential *defining quality* or "essence" *in* individual phenomena *that made possible their recognition* as representatives of that type. Menger's view of essence was thus different from Schmoller's.

In his solution of the problem of universals, Menger can usefully be identified as a "moderate realist" or "Aristotelian."[15] Menger sought the "simplest" elements of everything real, the essences, the nature (*das Wesen*) of the real. In his exact approach, he used a process of abstraction from the individual phenomena of the empirical world to discover their essences, to isolate them, and then to utilize the "simple elements" so obtained to deduce ". . . how more compli-

[13] Schmoller's methodological views on the use of statistics substantiate these conclusions about his notion of causality. To Schmoller, the science of statistics is the proper tool to examine and identify the *actual* relations that are present in every experiential context. It yields causal explanations and makes possible the measurement of the degree of "influence" of "essential and contributing causes." Cf. [Schmoller, 1893, p. 37].

[14] Detailed references and justification for the foregoing interpretation of Schmoller's position will be found in [Bostaph, 1976, Chapters IV and V].

[15] Moderate realism, or Aristotelianism, holds that concepts are formed by the mental intuition of the pure essences of existential phenomena from the phenomena themselves, in which the essences are somehow manifest. After concluding my own original research, it was with a great deal of pleasure that I found an identification of Menger's epistemology as Aristotelian in works by Emil Kauder. See [Kauder, 1958, pp. 413-25; 1965, pp. 97-100]. Kauder identifies not only Menger, but also Böhm-Bawerk as Aristotelian and is quite specific in showing how this influence entered and influenced the development of early Austrian economics.

cated phenomena develop from the simplest, in part even unempirical elements of the real world."[16] This approach has been designated as "methodological individualism" by Schumpeter.

The objects of Menger's epistemological considerations–the objects of his thought–were the essential characteristics of the individuals and how those characteristics in the form of "simple elements" could be used to explain how more complicated phenomena arise from individual phenomena. It cannot, in justice, be called an *a priori* method because it begins with "simplest elements" *that are derived from empirical reality* by the mental process of concept-formation and are not simply assumed *a priori.* Menger sought not only the general knowledge exemplified in *types*, but also that exemplified in *typical relationships.* These typical relationships, or general connections between economic phenomena, could be discovered in an exact sense as exact laws. An exact, or causal law was an absolute statement of necessity to which, Menger pointed out, exceptions were inconceivable due to the "laws of thinking."

In the Aristotelian epistemology, all thinking takes place in accordance with these laws, which are known respectively as the "law of identity," the "law of contradiction," and the "law of excluded middle." But, these are not just laws of thinking, they are laws of things, they are statements that are attributed to the real because they are apprehended in the real. They are the apprehension of a necessity in the being of things and thus are metaphysical or ontological. In addition, the law of causality is derived from the law of identity. The causal connection exists and is to be detected between determinate things in existence that have a determinate nature. To apprehend a causal relation is to apprehend this connection *by means of* the determinate things in the connection whose action produces it. To act at all, determinate things must act in accordance with their nature and must produce effects necessarily in accordance with that nature.

In identifying the essence or "simple elements" of economic phenomena, Menger was identifying the nature according to which he believed those phenomena must act. He was making possible the identification of the causal laws that would connect some phenomena to others or would show how more complicated economic phenomena develop from the simplest. Menger's references to "abstract economic reality" as the domain of the "exact" approach were not a reference to some other dimension of reality, but to the necessity revealed in the connections of things and inherent in their nature.

In summary, because his view of reality was Aristotelian, he believed that entities in reality act according to their nature in "typical" relationships. Thus, a concept of an entity, if it embodies the essence of that entity as an instance of a type, will embody its nature. Reasoning which uses those conceptual "simple elements" will be reasoning that proceeds according to the premise that entities act according to their natures, and will construct (deduce) conceptual systems of causality corresponding to the causality of the empirical world. Thus, conceptual or "theoretical" causal laws are laws of

[16] [Menger, 1883, p. 61.] This applies only to Menger's "exact" approach. How this is related to the "realistic-empirical" approach by Menger is not explicitly treated but may be inferred. The "realistic-empirical" approach involves the observation of types as they exist in their "full empirical reality," says Menger [p. 56]. "Realistic-empirical" description of types implies abstraction of some sort because selection is implicit in any description and selection is an abstracting process. But the search for essences, for "simplest elements," is not necessitated by that method–only a cataloging of characteristics and relations. On the other hand, a knowledge of the essences is needed to identify the entities to be included in "realistic-empirical" description and the principle of the selection process. The characteristics of an empirical form of money cannot usefully be described without an identification of its "essence" or defining characteristic. Thus the concept "money," obtained by identifying the "essence" of its empirical form, gives a point of view that allows the organization of any description into a meaningful whole–whether the description is infinite in detail or brief. Many forms of money actually occur and have occurred, but all forms embody the essence "money" in this view.

the real. Menger concluded that the exact method derived, ". . . laws of phenomena which are not only absolute, but according to our laws of thinking simply cannot be thought of in any other way but as absolute."[17]

Typical relationships could also be discovered in a "realistic-empirical" sense as empirical laws, argued Menger. An empirical law is a summary of observed regularities in the coexistence and succession of actual phenomena. Exceptions to it are both conceivable and probable due to its nature *qua* empirical. Depending solely upon observation, only the actual regularities belonging to observed empirical forms can be identified. There is no assurance that these regularities are "absolute" or (to use the usual phrase implying causality) that they are "laws of nature," admitting no exception. They are merely what is observed. No question of intrinsic relations, and thus causality, is involved in the knowledge of the external regularities in the coexistence and succession of economic phenomena that such laws provide.

Empirical laws are truly "historical knowledge" of the real. They embody and summarize all the influences present in real economic phenomena, that is, they include the "totality and whole complexity" of empirically observed phenomena, not just their general nature or "essence." It follows that they are modified by temporal change and vary from culture to culture. To the extent that the historical school confined their research efforts to the use of some collectivistic form of the "realistic-empirical" method–as Menger believed their "historical method" to be–they would tend to find some confirmation of their expectations regarding the relativity of economic laws.

Menger entirely rejected any attempt to verify laws obtained using one orientation of research by the other orientation, viz., the attempt to modify "exact" laws by "realistic-empirical" research or to place "realistic-empirical" research (the results of the use of a "historical method") above exact research. He argued that this was similar to trying to test the principles of geometry by measuring real objects. For example, no amount of time-series data relating price and quantity could disprove the "exact" law of demand. Also, Menger believed that any attempt to derive the exact, or general theory of economics from a study of the history of economies was either the result of a failure to recognize the fundamental differences between the discipline "economic theory" and the discipline "economic history" or, alternatively, between the exact method and the "realistic-empirical" method. In his own view, it was exact theory that *explained* history and economic development while the "realistic-empirical" approach passively summarized it. "Realistic-empirical" laws were not "laws" in the same sense that the general, or exact laws of economics were "laws."[18]

It is not difficult to understand the historicist rejection of the deductive universalistic theory implied by Menger's exact approach, given their strong empiricistic orientation and fundamental rejection of any more abstraction from the whole empirical complexity of economic phenomena (as they occur in specific social contexts) than pragmatically necessary. Menger's use of the "simple element" of self-interest would be "unrealistic" from the viewpoint of someone who saw economic behavioral motives to be multitudinous in any empirical context and who saw all theory to be rooted in an empirical context. Likewise, Menger's mono-causal approach would be considered wrong-headed to anyone who saw empirical phenomena to be the product of multitudinous influences in any empirical context and who saw all theory, all causal relations, to be rooted in an empirical context. The historicists rejected universality in economic the-

[17] [Menger, 1883, p. 61.] Menger's idea of causality is also one of "mono-causality." Given an initial set of conditions, only one thing can occur in this view.

[18] Detailed references and justification for the foregoing interpretation of Menger's position will be found in [Bostaph, 1976, Chapters III, IV, V].

ory because they believed that theory should be empirically descriptive of a given social context. As the social context changed, the theory must necessarily change.

On the other hand, Menger refused to grant full theoretical status to any theory generated by a historical method because he held theories of concepts and causality that differed from those of the historical school. The essences of economic phenomena with which he was concerned were "atomistic" or "individualistic" rather than "collectivistic"; they were what made an individual concrete a member of a type and did not depend upon the social context for their content. In his opinion, the collective concepts of the historicists were not *types*, they were *assemblages* of individuals. They were not *general*. As a result, he had no use for a "holistic" and contextual approach. Menger refused to grant causal status to historicist empirical laws because he believed that it transcended experience to state that an empirically observed phenomenon followed another empirically observed phenomenon "absolutely." Only the exact approach with its "intrinsic" causality could transcend experience in this manner. He thus implicitly rejected the "non-intrinsic" theory of causality of the historical school.

The Methodenstreit can, in the light of the above, now be seen to have been rooted in opposed epistemological positions that by no stretch of the imagination could be termed complementary goods. Consequently, it is not surprising that Schmoller and Menger should fail to grant value to each other's views. Unfortunately, neither of them clearly realized the epistemological nature of the issues that separated them, or clearly identified the position of the other faction as determined by epistemological considerations. To this may be attributed the inconclusiveness of the debate and the preoccupation with the more subsidiary matters of economic methodology and morphology that has misled historians of economic thought.[19]

A clear recognition of their epistemological differences might have resulted in less wasted effort through a discussion of the issues underlying the apparent points in dispute. This is not to say that there is any surety that those issues would have been resolved, but at least the debate would have been confined to a relatively limited sphere of subjects rather than roaming the whole field of the social and natural sciences, as it unfortunately tended to do. Ultimately, such very basic epistemological differences are so incompatible that resolution of arguments concerning them by disputants not trained specifically in philosophy may be entirely too much to expect and may most usefully be left to philosophers. The greatest service to the disputants is then merely to identify the source of their dispute as epistemological and hope for the best. There is no reason to suppose that this service would be any less useful to disputants today than it would have been to the parties to the Methodenstreit.[20]

5

Previous assessments of the Methodenstreit can now be seen to contain a number of errors as a result of the failure to identify the argument as, at heart, epistemological. The argument that the two methods contrasted were not "mutually exclusive," that induction and deduction are interdependent, misses the point of the debate. The question of "induction versus deduction"

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[19] Cf. [Schmoller, 1883, p. 286 (982), p. 280 (978); Menger, 1883, p. 108, p. 31]. Of course, Menger stated in the "Preface" to his *Untersuchungen* that methodology proper was not the subject of his work because the question of the nature of economics itself and its proper research goals had to be agreed upon first, before methodology could be argued. Then he proceeded to treat methodological questions as well as questions of both goals and the morphology of economics in his text. It does not seem unreasonable to have expected a more explicit treatment of the historicists' method from him in this regard–especially, since he was so outspoken about the "erroneousness" of that method.

[20] A beginning in this direction will be found in [Bostaph, 1976, Chapter VI].

was never really at issue between the parties involved. It may be admitted that in the usual construction of general theory, "inductive" and "deductive" procedures are used together; that is, premises are induced from empirical reality and then deductive arguments constructed from them. Whatever their views on this might have been, there was a significant difference in how each faction in the Methodenstreit viewed the *conception* of premises and the *derivation* of economic causal laws. Only Menger's "exact" method was uncompromising in its view of economic law as deductive in origin from "induced" premises. Historicism saw economic concepts and law as empirical summarization.

The view that both methods are "equally necessary" to the economics profession as a whole fails to state clearly for what they are necessary. Menger was certainly not so ambiguous. He argued that a "historical method" was not the way to construct economic theory if it was desired that theory be general. On the other hand, the exact method would not be applicable to the writing of history (although the results of it, in the form of general theory, would) in his view. To call the two methods coequal and to fail to state a basis of coequality is to leave the matter ambiguous. A similar criticism can be made of the assertion that some problems yield to one technique and some to another.

The conclusion that the differences between the position of the historicists and that of Menger were minor compared to the similarities seems wholly unsupported. The differences (in epistemological beliefs) were so great that the debate raged and, more than likely, was not resolved *because* the fundamental sources of the disagreement lay unidentified and (substantially) untreated by both factions. The epistemological points at issue are matters of crucial importance to anyone who attempts to be self-conscious about his own methodological choices. Expenditures of time on such considerations can hardly be viewed as wasteful because an inappropriate choice can (potentially) lead to a lifetime of wasted effort. In that sense, the debate was important and valuable. On the other hand, the failure to debate the "right" issues did lead to a relative waste of valuable time in an inconclusive and agonized exercise in mutual frustration. The degree of that waste is difficult to evaluate, given the importance of the topic.

With respect only to Menger's part in the debate, there is no difficulty whatsoever in recognizing that his substantial contributions as a methodologist exhibited the same power of mind and originality of thought as his contributions to economic theory proper. In addition to his other virtues, Carl Menger: (1) stated his epistemological assumptions and methodological prescriptions more precisely and at greater length than virtually any economic theorist either before or since his time (Mises was certainly a most welcome exception to this generalization); (2) intentionally sought to develop a methodologically self-conscious economic theory; and (3) began the tradition of methodological individualism that an increasing number of economists are coming to believe is *the* epistemologically legitimate and potentially most productive general methodological approach for economic theory.

For these reasons alone, there is ample cause to be glad that Menger was drawn into a Methodenstreit and did publish his methodological and epistemological views. It is only to be regretted that his research work on these topics in later life has not been published.

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