# THE METHODENSTREIT AND THE EMERGENCE OF MATHEMATICAL ECONOMICS

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The "controversy on methodology" — the *Methodenstreit* — surfaced as one of the focal issues of political economy in the German language universe during the last (two) decades of the 19th century. This was of course the very age in which neoclassical economics was emerging, soon making its imprint across the European (and North American) intellectual landscape. Following in the pattern traced out by classical economics, the so-called "marginal revolution" [Blaug 1983, 309-343] accepted (almost) as self-evident its deductive method of analysis and its groping for propositions — "laws" — applicable to the workings of economic agents and systems in general.

The Austrian version of the marginal revolution initiated in 1871 by Menger's *Principles of Economics* — the so called "revolution of subjective value" — was inherently wedded to the deductive method. Its premise that "laws" derived on the basis of a small number of axioms could be used for the study of economic phenomena, was challenged by the Historical School from its very beginning. The critique of the abstract mode of reasoning of the classical economists (Ricardo's comparative cost model and its "free trade" implication was an obvious case in point) was a common theme of the (German) old Historical School from the 1830s onward. Yet the vehemence with which the young Historical School, emerging in the 1860s under Schmoller's leadership, rejected the deductive method of analysis proposed by Menger and his Austrian disciples was mainly due to the direct challenge which they posed to the inductive research methodology and thus in essence to the School's *raison d'être* — its research program.

The positions adopted by *Methodenstreit* antagonists towards mathematics as a tool of economic research were a highly significant, though distinct, subject of the discussion of methodology in that period. The spreading use of mathematical techniques by the two other groupings taking part in implementing the marginal revolution — the English neoclassical economists and the Gallic and Italian members of the Lausanne School — put this issue squarely on the agenda. In view of the very context of the *Methodenstreit*, one would expect the Historical School to be averse to the use of mathematics in economic reasoning. The Austrian School on the other hand, having nailed their flag to the mast of the deductive method, and belonging to the founding fathers of the marginal revolution, would favor the use of mathematics which is deductive *per se*.

Yet this was not the case. The Austrian tradition following Menger rejected the use of mathematical methods for economic research and chose to fight, simultaneously, on two fronts. In the *Methodenstreit* the Austrian School engaged the Historical School

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on the issue of deductive versus inductive economic research. At the same time it argued strenuously with its "collaborators" in the marginal revolution on the relevance and significance of the integration of mathematical techniques as tools of research and presentation. The first section in what follows describes the main contours of the *Methodenstreit* controversy, while the second section focuses on the debate on the relevance of mathematics within the "pro-deductive" method groups. The third section describes and underlines the rather friendly posture towards literary economics adopted by the first generation of mathematical economists with little avail. The final section attempts to trace the impact of the *Methodenstreit* and the rejection of the mathematical method in the German language universe on the development and state of German economics in the early decades of the 20th century and in the 1920s in particular.

## THE HISTORICAL SCHOOL: REJECTION OF THE DEDUCTIVE (METHOD) AND OF MATHEMATICAL TECHNIQUES

The first salvo in what was soon to be labeled the *Methodenstreit* was Menger's *Investigations into the Method of Social Sciences with Special Reference to Political Economy* published in 1883.[1] Schmoller's highly critical review article of this volume drew an immediate response in which Menger proposed to describe "the fallacies of historism" ("Die Irrthümer des Historismus").[2] The publication of Schmoller's review and Menger's pamphlet in 1883 and 1884, respectively, set the battle lines for the coming generation in the German language literature of economics.

More than a decade earlier, Menger had underlined the "formal nature of theoretical economics" which, he said, required a method of research similar to that of the natural sciences [Menger (1871) 1981, preface]. His application of this methodology allowed him to derive a general theory of value applying to factors as well as to goods, reflecting the realities of the marketplace on the basis of a (small) number of "self-evident" premises. The "economizing individual" constrained by scarcity of resources as the linchpin of economic activity and the introspectively derived principle of diminishing marginal utility served as the foundations of his remarkable, coherent conceptual setup.

Schmoller, however, rejected all out the notion and the conceptual framework which Menger had erected in his *magnum opus*. It was not just the postulated premises that he found wanting.[3] First and foremost, he rejected the deductive method of research, which sought to derive general theorems relating to production, cost, prices, and welfare from a small number of "universally accepted" axiomatic premises. In Schmoller's mind such premises which were *ex definitio* abstractions from concrete reality, could only result in "laws" empty of content relevant to real socioeconomic issues, even if these laws were deduced from the premises by full adherence to the rules of logic.

From its inception in the 1840s the Historical School (following List [(1841) 1885]) was critical of the teachings of classical political economy on these very grounds. Its adherents rejected in particular the deductive method applied by Ricardo and his



immediate followers. Schmoller and the young Historical School initially reacted to Menger's attempt to establish the deductive method as a major (though not exclusive) instrument of economic research in the same way. Since they maintained that "there are no natural laws of economics..." the proper method of economic study was to their mind inevitably the "inductive-historical" method.[4] In due course in the wake of an accumulation of studies on the various dimensions of economic history, such studies anchored in fact rather than abstract assumptions might, they maintained, offer the groundwork for the derivation of some general "laws" of economic development.

The Historical School's insistence on the inductive method as the highway of economic research, and its implicit rejection of economic theory as such, excluded of course mathematical techniques as a tool of analysis and as an implement for the demonstration of results. This anti-mathematical attitude was strongly amplified by the young Historical School in the 1870s — the very period in which the application of mathematics to economic analysis began to take off.

It was, of course, not only Schmoller's whim which decreed mathematical methods off limits. The founder of the Historical School, Roscher, had established this rule by the middle of the century. He stuck to this position through all the editions of his *Principles of Political Economics* (*Die Grundlagen der National Ökonomie*), and when the issue flared up again in the 1870s and the 1880s in the wake of the *Methodenstreit* and its English variant. In the first edition of his *Principles*, Roscher says:

> ...many authors endeavored to clothe the laws of political economy in algebraic formulae....but the advantages of the mathematical mode of expression diminish as the facts to which it applied become more complicated...in the portraying of historical life...the algebraic formulae would soon become so complicated as to make all further progress in the operation next to impossible. [1854, 103-104][5]

That attitude — hostility towards the application of mathematical modes of analysis — was adopted also by the English adherents of historical economics. This happened even before the eruption of the "German-Austrian" *Methodenstreit*. Cliff Leslie, an early proponent of the English Historical School had been highly critical of the deductive method of classical political economy as early as the late 1860s.[6] Leslie and his followers inevitably extended their criticism to the contemporary English neoclassical economists, who in those years in the immediate aftermath of the publication of Jevons' *Theory of Political Economy* (1871), were busy promoting and loudly advocating the application of the deductive method of the mathematical genre as the *sine qua non* of economic theory.

An outstanding expression of the view of the English Historical School of economics on the use of mathematics in the realm of political economy was put across by J. K. Ingram just before the publication of Marshall's *Principles*, which was in more than one sense a watershed on this score. Ingram maintained that "there is no future of this kind of study, and it is only a waste of intellectual power to pursue it." [Ingram 1888, 182].[7] Whether Ingram was aware that Roscher had made a similar judgment

44 years earlier is of minor significance in this context. What counts is the common attitude of the Historical School on the continent and in England, and of the contemporary emerging American institutionalists, with regard to the deductive method in general and inevitably to its mathematical expression.

## KEEPING ALOOF OF MATHEMATICS: THE ATTITUDE OF THE AUSTRIANS AND OF THE LAST COHORT OF CLASSICISTS

The *Methodenstreit* was indeed a clash between the Austrian supporters of the deductive approach to economics and the "inductive case study" approach advocated and applied by the German Historical School. Yet a common sense extension of the subject under dispute would suggest that while the Germans rejected the use of mathematical tools in economic analysis, the Austrians would advocate their application. This presumption is widely off the mark. The Historical School, of course, explicitly rejected the application of the mathematical method. But the Austrians, who had indeed pushed deductive economics to a new frontier, were neither practitioners nor advocates of the mathematical mode of analysis.

The founder of the Austrian variant of neoclassical economics, Menger, did not use even a single mathematical symbol in his analysis and presentation of economic theory. On a small number of occasions algebraic formulae were used by Böhm-Bawerk, who by the mid-1880s as the Menger-Schmoller controversy was making the headlines, had already made a major contribution of his own. Nonetheless, he questioned the relevance of the notions of mutual determination and equilibrium, the essential elements of the mathematical framework of economic theory, as 'satisfactory' tools of economic analysis [Böhm-Bawerk, (1889) 1921, Vol. II, 315]. Von Wieser, the second direct disciple of Menger, similarly eschewed mathematical formulation. A rare application of an algebraic formulation, the exception proving the rule, occurs in his *Natural Value* [(1893) 1989, 88]. Following in the footsteps of the classical economists, and of Ricardo in particular, the Austrians usually relied on numerical examples to buttress their arguments and illustrate their hypotheses. They derived their theorems, however, from basic premises by means of lingual reasoning and strict adherence to the canons of logic.

Indeed, the concept of marginal utility, and the hypothesis of diminishing marginal utility, the *sine qua non* of Menger's general price theory, were spelled out in plain language, supported by several 'self evident' examples, and illustrated by means of numbers arranged in 'ten (declining) scales' for different commodities [Menger, (1871) 1981, 122-28].[8] This tabular form, which in more than one sense represents the synopsis of Menger's theory, can be easily expressed by means of a short mathematical formulation fully consistent with the data and the theory which the Mengerian numerical setup was designed to illuminate.[9] This might suggest that although Menger avoided the use of mathematics, he, like Molière's well-known hero, did not realize that he had actually been using prose. Indeed, the analytical techniques of Menger and of his disciples — von Wieser and Böhm-Bawerk — were conceived by Jevons and later by Fisher as in essence 'semimathematical.'[10]



The avoidance of mathematics in Menger's, von Wieser's and Böhm-Bawerk's books, however, was not just a technical matter. From two letters written to Walras at the very time at which Menger openly challenged the Historical School and initiated the *Methodenstreit*, he offered a clear-cut explanation for his remaining aloof from the application of mathematics to economic analysis.[11] Menger writes:

> I do not belong, of course, to the supporters of the mathematical method in the treatment of our discipline. I believe that the mathematical method could serve mainly as a vehicle for presentation rather than as a tool for research. (..allerdings gehöre ich nicht zu den eigentlichen Anhängern der mathematislhen Methode bei der Behandlung unserer Wissenschaft. Ich bin nämlich der Meinung das die mathematische Methode der Hauptsache nach eine solche der Darstellung der Demonstration und nicht der forschung ist...). [1965, Vol. I 566]

This, of course, does not amount to an all-out rejection of the mathematical mode. Its service as an implement for presentation is accepted, and thus recognized as "an auxiliary discipline of Political Economy" ("eine Hilfswissenschaft der Politisehen. Oekonomie") [ibid., 566]. Yet its contribution to economic studies is accordingly restricted to issues involving quantities — 'Grössenverhältnisse'.

In the second long letter devoted entirely to the problem of method, Menger underlined the main reason for his avoidance of mathematics, which was mentioned only in passing in the first. Underlining his position on this matter, he emphasizes the fact that "We study not only quantitative relationships but also the *substance* (essence) of economic phenomena." ("Wir untersuchen doch nicht nur die Grössenverhältnisse sondern auch das *Wesen* der volkswirtschaftlichen Erscheinungen."). [Jaffé 1965, Vol. V, 3, italics added]. To Menger, the inapplicability of the mathematical method to the analysis of the *substance* (essence) of economic phenomena is the root of that matter. He and his disciples believed that whereas the method applied by him — "the exact method" ("die exacte methode") [ibid., 602], was the term he proposed for it — could offer *causal* explanations of the *nature* of value, the mathematical method could explain no more than the *association* among prices — the relations between good and factor prices.

Epistemological considerations thus suggested to Menger that his was the superior method for economic analysis. This point was underlined in the second letter to Walras in which he stated his position in terms of a query: "How could we attain cognition of value, rent, entrepreneurial profit division of labor bimetallism, etc., by means of the mathematical technique?"[12] Böhm-Bawerk, who describes himself as "not a mathematician" ...("nicht Mathematicker") [(1889) 1921, Vol.I, 426], offered a similar reservation on the usefulness of the mathematical technique of economic analysis and its conceptual setup involving mutual determination and the notion of equilibrium. He maintained that "where causal relationships exist, the mathematical solution can proceed equally well from effects to causes as from causes to effects... Unique determination is neutral in relation to the problem of causation; *it has nothing to do*

*with causation...* and therefore the unique determination of a problem by no means signifies a correct causal solution of the problem in hand, and especially does not signify a guarantee for the freedom from circular explanation. One can reason in a circle even about a mathematically determinate problem..." [1921, Vol. II, 315–16, italics added].[13]

The objection to the application of mathematical methods to the study of the *modus operandi* of economic systems did not weaken as the second generation of the Austrian school took over. An outstanding case in point is Von Mises, von Wieser's student, whose writings came on line after the turn of the century. As a witness to the rapidly-rising pattern of mathematical economics and of econometrics in the first half of the 20th century, his rejection of that methodology is, if anything, much more extreme than that of the "founding fathers." In his magnum opus *Human Action*, written in the United States where he had been living since the early 1940s, he condemns mathematical economics outrightly in its various manifestations. Mises writes: "If this antagonism between the logical and the mathematical economists were merely a disagreement concerning the most adequate procedure to be applied in the study of economics, it would be superfluous to pay attention to it. ....However, this is not a dispute about heuristic questions, but a controversy concerning the foundation of economics. The mathematical method must be rejected not only on account of its barrenness. It is an entirely *vicious method* starting from false assumptions and leading to fallacious inferences. Its syllogisms are not only sterile, they divert the mind from the study of the real problems and distort the relations between the various phenomena...." [1949, 347, italics added]. Thus, while it might be said that the attitude of the "founding fathers" of the Austrian School did not amount to an all-out rejection of the mathematical method, though they eschewed its use, Mises and his followers — the neo-Austrian school which surfaced in the 1950s and 1960s in the United States — rejected its application to economic issues on epistemological grounds, root and branch.[14]

Significantly, the attitudes of the members of the last generation of classical economists still practicing in the 1870s, who were of course firmly committed to its deductive method, were quite similar. John Stuart Mill was still around when Jevons' clear-cut challenge to some components of classical economics, but not to its method, appeared in the first edition of his *Theory of Political Economy* [1871].[15] It was, however, left to Cairnes, the "crowned" disciple of Mill to make the response to Jevons' claim, made in the preface of the first edition of this book, that political economy "must be a mathematical science in matter if not in language...as it deals throughout with quantities..." [ibid., VI]. In the preface of the second edition of his *Character and Logical Method of Political Economy*, Cairnes faced squarely Jevons' proposal to use mathematics as a major vehicle of economic research. He wrote, "so far as I can see, economic truths are not discernable through the instrumentability of mathematics....What I venture to deny is the doctrine which Professor Jevons and others have advanced....that mathematics can be applied to the development of economic truth...." [Cairnes, (1875) 1975, IV-V]. To make his point Cairnes challenged Jevons to point to an "economic truth not before known" which was discovered by applying the mathematical method.



An immediate response to Cairnes' challenge could have been easily made by reference to, say, Cournot's *Researches into the Mathematical Principles of the Theory of Wealth* [(1838) 1960]. This small volume offered a set of examples of "economic truth not before known." Jevons could have cited several theories discovered by Cournot's application of mathematical methods: the theory of monopoly, the theory of duopoly, and the identification of the optimal behavior of firms in a perfectly competitive market as a generalization of the rules applying to the two former market structures in the case in which the number of firms is infinite. These offered obviously more than one example of an economic truth not "discovered" by the classical economists. And this does not mention yet Cournot's clear-cut clarification of the meaning of such fundamental notions as demand and supply which were vaguely specified in the classical literature and by Cairnes himself.

What counts most in our context is evidently the attitude of a leading economist representing the tradition of classical political economy. Though, like Menger, Cairnes was an all-out protagonist of the deductive method, he was quite reluctant to let mathematics into the edifice. The similarity of the views on the role of mathematics in economics of the founding fathers of the Austrian school, the last generation of classical economists, and the German and English Historical Schools (though the former two groups were at loggerheads with the latter on the methodology of economic research) may raise some eyebrows. The practitioners of historical economics were undoubtedly on firmer ground in their objection to the use of mathematics. Their rejection of the deductive method as a vehicle of research and analysis applied evidently at one remove to mathematics the deductive technique of research *per se*. The attitudes of the two groups of adherents to the deductive method to the application of mathematics is evidently more difficult to fathom. They were indeed less extreme in their rejection of that instrument and were ready, grudgingly, to grant mathematics an entry permit as a device for the presentation of economic theorems discovered by the traditional lingual method of reasoning from first principles. Seemingly, they were not convinced by the case in favor of the mathematical method put forward as early as 1885 by Launhardt in which he attempted to offer "peace" to the opponents of that novel branch of economics by claiming that "[m]athematics is nothing but a language..." [(1885) 1982].

## THE DEFENSIVE POSTURE OF MATHEMATICAL ECONOMISTS

From the very beginning of what turned out to be a three-sided debate on methodology-deductive versus inductive method (the *Methodenstreit*) and literary versus mathematical economics — the mathematically "inclined" economists had been offering an olive branch to their opponents. They never claimed exclusive possession of the field and suggested their technique to be not a substitute but, for many purposes, a more efficient complementary tool for the discovery of "economic truth" and its presentation. This moderate stance was already clearly visible at an early stage of the debate — in the middle 1870s in England where the last generation of classical economists (favoring, of course, the deductive method) exchanged blows with the emerging group of "mathematically inclined" neoclassical economists. In his review of Cairnes'

*The Character and Logical Method of Political Economy* [(1875) 1975], Newcomb attempted to uphold the (then) novel technique, saying: "[M]athematical analysis is simply the application to logical deduction of a language more unambiguous, more precise and for this particular purpose, more powerful than ordinary language..." [Fisher, (1892) 1926, 113]. Almost the same conciliatory line on that issue was adopted a decade later by Launhardt, though his statement was made in the context of the German *Methodenstreit* which had been raging by that time. In the preface to his *Mathematical Theory of Prices* (*Mathematische Theorie der Preisbestimmung*) he says, "[m]athematics is nothing else but a language. It could specify unique relationships between measurable entities, which is either impossible, or could be only vaguely expressed in ordinary language." ("es ist ja die Mathematik nichts anderes als eine Sprache, welche in strenger Folgerichtigkeit die Beziehungen messbarer Dinge zu einander darstellt, was durch die gewöhnliche Sprache entweder gar nicht oder doch nur in weitschweittiger ungenauer Weise erreicht werden kann") [Launhardt, (1885) 1982, VIII].

Launhardt's felicitous and forthcoming formulation suggesting that mathematical techniques do not differ inherently from run-of-the-mill logic conveyed by means of ordinary language did not carry the day with the literary economists, even those with a theoretical bent. Even the increasing professionalism of economics, which amongst others involved the growing dominance of academics working in the field, did not settle the issue for many decades to come. For example, almost 80 years later, Paul Samuelson had to restate the case for the application of mathematical techniques in economics, by repeating effectively Launhardt's statement — "Mathematics is a language."[16] Its placement as a motto on the title page of his *Foundations of Economic Analysis*, clearly suggests that even by the middle of the 20th century, the issue which surfaced when neoclassical economics was still in its infancy in the 1870s and tangentially involved in the *Methodenstreit* exploding in the 1880s, had not yet been finally resolved.[17]

Indeed, judging from repeated utterances on the subject by the small group of practitioners, mathematical economics was clearly on the defensive in the ongoing engagement on method during the closing decades of the 19th century and the first decades of the 20th century. The issue was, of course, put squarely on the agenda by the neoclassical economists. The initial claim, on behalf of mathematics, was made by Jevons, in the preface to the first edition of the *Theory of Political Economy* [1871]. It was followed by Walras' *Elements* (1874), and reiterated in Jevons' second edition (1879). Yet, it was the noise of the battle related to the *Methodenstreit* which soon forced statements for the defense such as Launhardt's statement quoted above, made in 1885 at the height of the German's *Methodenstreit*. It amounted clearly to an attempt to defend the novel technique which by that time included not only several of his own contributions, but the major treatises of Jevons and Walras, Marshall's *Economics of Industry*, and Edgeworth's *Mathematical Psychics* [1881]. The title of Edgeworth's presidential address to Section F of the British Association for the Advancement of Science — "On the Application of Mathematics to Political Economy" [(1889) 1925] speaks for itself on this score.



Similarly, Irving Fisher felt it necessary to add an appendix (Appendix III) to his path-breaking volume, *Mathematical Investigations in the Theory of Value and Prices* [(1892) 1926], in which he offered a vigorous and detailed defense on the mathematical mode of analysis. Marshall in *Principles of Economics* [1890-1961] hid indeed the mathematical skeleton of his conceptual setup behind his enormous and elaborate text, relegating figures to footnotes and algebra and calculus to a mathematical appendix. In the well-known section of the preface to the volume in which he explained the benefits of that technique to economic research [Marshall, 1890-1961, X–XI], he avoided a verbal confrontation with the German and English proponents of the Historical School.[18] Similarly, Wicksell (in 1893) and Pareto and Fisher (around the turn of the century) mounted the defenses to plea for the legitimacy of the method.[19] In a felicitous understatement, in the Introduction to the *Lectures on Political Economy 1901-1906* [1934] a decade later, Wicksell offered explicitly the olive branch to the Historical School. He wrote: "[A]s regards the controversy concerning the so called historical treatment of economics (of which the latter must of necessity be more or less mathematical) this is a matter, which can in my opinion, be settled *only* by a division of labor." [Wicksell, 1341, XXI].

The very fact that these leading members of the neoclassical school were compelled to defend the legitimacy of the research method they adopted suggested that they were not dominating the scene. Fisher admitted that this was indeed the case, conceding "that probably in the entire world of economic students the opponents of the (mathematical, H.B.) method still outnumber its friends..." [(1898) 1926, 37]. Yet he underlined the momentum of the novel approach to economics which used mathematics to support and reinforce the deductive method of political economy inherited from the "founding fathers."

Using Jevons' original *Bibliography of Mathematical Economics*, which Fisher adapted and brought up to date (through 1897), Fisher offered quantitative support of his reading of the trend by referring to the acceleration in the number of publications that used mathematical formulations.[20] He noted that between 1838 — the date of publication of Cournot's *Researches,* and 1871 — the date of publication of Jevons' *Theory*, about 30 'mathematical economic' writings appeared — an annual rate of only *one*. This rate increased to six each year in the two decades between the publication of Jevons' and Menger's books and the publication of Marshall's *Principles* in 1890. And though Marshall used understatement in his explicit reference to the applicability of mathematics to the discipline, and the *Principles* was clothed in literary garb, its appearance stimulated the application of the new technique of research and presentation. The rate of publication of the mathematical genre of writings accelerated according to Fisher's count to 18 per year in the closing decade of the century. This indicated that, though still struggling for recognition, the new method was here to stay [Fisher, 1898, 136-37].[21]

This means, of course, that neither the prediction of the English historical economists that the mathematical method in economics "will in fact never be anything more than an academic plaything...", nor the onslaught of Schmoller and his followers, nor the more subtle opposition shown by the reluctance of the French economic

establishment to admit Walras into the mainstream of the French profession had succeeded in stopping the momentum of the train.[22] Economics as a distinct discipline, using various branches of mathematics as a vehicle for research and for presentation and instruction, had finally come of age in the early decades of the century. Correspondingly, the economic profession acquired influence and stature although the main breakthrough occurred only after the 1930s and particularly after World War II.

## THE DIFFERENTIAL IMPACT OF THE (TWO RELATED) METHODOLOGICAL CONTROVERSIES ON THE DEVELOPMENT OF ECONOMICS

The confrontation on methodology in the decades around the turn of the century had a dual aspect: On the one hand, it ranked the 'neoclassical' economists in their different groupings — the Austrian School, the (French-Italian) Lausanne School, and the English neoclassical economists — against the German Historical School, and its English and French supporters. The debate between these camps on the applicability of the deductive vs. the inductive methodology in political economy involved, simultaneously, a controversy on a contiguous methodological subject — the applicability of mathematical techniques for the study of economic phenomena. This argument involved a different composition of the combatants: It more or less ranked the German-speaking universe *vis à vis* the Lausanne group and the neoclassicists in the English-speaking universe. One might, therefore, ask whether these two tangential but still distinct controversies had a longer-run differential impact on the development of economics in these differing lingual and cultural spheres.

Retrospectively, Fisher's judgment, rendered in the mid-twenties, that neoclassical economics underpinned by its mathematical support and formal conceptual framework had won the day, was clearly on the mark. It could also be argued that the inherently quantitative character of the subject would have sooner rather than later pushed the evolving discipline from the use of numerical examples toward the generalized concept of interdependence — the function. And inevitably, in view of the idea of a search for an optimum inherent in the economic discussion from the 18th century onwards and the very nature of the two concepts — diminishing marginal product and marginal utility (both of which were undoubtedly "discovered" by literary economists) the application of calculus to economics was merely a matter of time. Similarly, the notion of the profit rate as an equilibrating mechanism (in the longer run) which was part and parcel of the *Weltanschauung* of classical political economy would have ultimately forced the use of algebra to establish the (general) equilibrium conditions of the system. Thus, the onslaught of the Historical School could not, ultimately, have made a difference in any case. Although the Historical School undoubtedly attempted to do so, it could not stop the emergence of mathematical economics and of economics with a theoretical bent as a self-contained discipline. But this does not mean that it could not and did not have an impact in specific cultural environments.

This is clearly indicated by the differential pattern of publication. The rapidly rising trend of the mathematical mode of analysis in political economy, reflected in



the Fisher rate of publication count of the last three decades of the 19th century, holds indeed for the global picture. But the increase was not consistent across the politico-cultural regions in which the economics of the late 19th and early 20th century was being forged and had been evolving. A comparison of the rates of publication of mathematical economic writings in the German-speaking universe with those in the English-speaking world shows that this was undoubtedly the case.

Applying Fisher's quantitative criterion, we note that out of the 44 publications of mathematical economic writings in the first early phase, from the publication of Cournot's *Researches* [(1838) 1960] to that of Jevons' *Theory* [1888] appearing in the Jevons-Fisher list, 9 were by German authors. Indeed, due to an error in the list — the omission of the second part of Von Thünens' *Isolierter Staat*, published in 1850 — the number of German publications entered should be 10 out of 45, almost 25 percent of the total. Furthermore, among the German entries we have the path-breaking contributions of Von Thünen, Gossen and of Mangoldt.[23] Moreover, it was Gossen who explicitly formulated the message adopted two decades later by the standard bearers of mathematical economics: "Therefore, it is impossible to describe all the manifestations of the real national economy without the aid of mathematics, as it would be impossible to describe astronomy, mechanics, etc., without it." ("Darum ist es denn unmöglich, die wahre Nationalökonomie ohne Hilfe der Mathematik vorzutragen, wie dieses bei der wahren Astronomie, der wahren, Mechanik u.s.w der Fall ist.") [(1854) 1983, VII]. Though hardly anyone knew about the existence of this book until the late 1870s, when Jevons and Walras discovered Gossen as a clear forerunner, Gossen's statement, supported of course by the substance of his contribution and those of Von Thünen and Mangoldt, puts Germany at the pioneering frontier of this emerging novel method of economic inquiry during the three to four decades preceding the marginal revolution."[24]

Things changed dramatically, however, in the two decades in which neoclassical economics really took off. Out of 115 publications in Fisher's list for the 1871–1890 interval, only 19 are German. And 15 are the works of two men — Wilhelm Launhardt and Julius Lehr. The emerging pattern during these two decades show not only a significant comparative decline of German output — to 17 percent of the total, but also its almost exclusive dependence on the output of two researchers. The German performance did not improve in the 1890s — if anything, it was even worse.

In the English-speaking universe the trend ran in the opposite direction. The Fisher list which describes the publications for the post-Marshallian phase and (owing to the date of its publication) refers to only eight years through 1897, shows a major acceleration of work in the field globally. Yet Germany's comparative performance was down again — to 15 percent. And if Wicksell's four (major) contributions are excluded, as they should be for this purpose, the German contribution amounted only to about 12 percent. Again, Launhardt's and Lehr's contributions are a significant though a lower fraction of the German publications than in the previous phase; German research evidently was also lower in comparative quality. It could not boast contributions similar to those of Von Thünen, Gossen, and Mangoldt. While Wicksell wrote and published in German, this neither reflects a specific German background,

nor even more significantly, acceptance into the realm of the German *Volkswirtschaftslehre.*

This poor performance undoubtedly reflects what Launhardt complained of at the very beginning of his preface to his *Mathematische Begründung*: "The repeated attempts to present a mathematical statement of political economy have hitherto received scant recognition. The rejectionist attitude of authors in the field to mathematical research in political economy can be mainly attributed to external factors." ("Die wiederholt hervorgetretenen Versuche einer mathematischen Begründung der Volkswirthschaftslehre haben bis jezt wenig Anerkennung gefunden. Die ablehnende Haltung der volkswirthschaftlichen Schriftsteller gegen mathematische Untersuchungen ist wohl meistens auf äussere Umstände zurick zu führen...") [Launhardt, (1885) 1982, VIII). Though an understatement, the last sentence clearly refers to the *Methodenstreit,* which at the time of Launhardt's writing was exploding all around, and to the all-out rejection of the deductive method, of which mathematical economics was conceived as the extreme variant, by Schmoller and his disciples.

The relatively poor showing of German theoretical and mathematical economics during an almost 50-year period through World War I and beyond was undoubtedly due to the dominance of Schmoller and the Historical School in the German liberal arts and law faculties. This had an immediate impact on academic appointments and goes a long way to explain the minor rank through 1920 — "extraordinary" (i.e. assistant) professor — of Bortkiewicz, who "upheld the flag of economic theory...at an epoch and a country in which hardly any one would hear of it." Though making major contributions early in the century he was made full professor at the University of Berlin in 1920 after 20 years of service in that institution.[25]

The relevance of this policy of exclusion is highlighted by the fact that Launhardt, the mainstay of German mathematical economics from the 1860s through the turn of the century, came into economics from civil engineering, and his academic appointments were in an Engineering and Technology Institute, the Hanover Polytechnic School in which he served as a professor of roads, railways, and bridge building. Most of Launhardt's articles, which were studies of optimal tariffs and railroad rates, were published in transportation journals. Indeed, the "outsider" status of all economists working on theoretical issues was clearly shown by the type of journals in which those writing in this genre were published. A case in point is one of the papers on Fisher's list, which was published in the *Zeitschrift für Mathematik und Physik.*[26]

But perhaps the best example describing the atmosphere which excluded economic theory and the use of mathematical tools from the realm of the German *Volkswirtschaftslehre* is revealed in the attitude to Wicksell. Between 1893 and the turn of the century, Wicksell made three major contributions, all of which were written in German and published as monographs in Germany. Yet though Wicksell, among others, directly challenged the message of German's historical economics on some issues, Schmoller's *Grundriss* [Vol. I, 1900; Vol. II, 1904], his *magnum opus* designed to survey the whole field in which he "attempted to refer to all *relevant* (italics are mine, H.B.) sources through 1902," as he put it, ignores Wicksell altogether.[27] Neither Wicksell's analytical framework, nor his argument and results in *Value, Capital, and*



*Rent* [(1893) 1954] and in *Interest and Prices* [(1898) 1936] were considered important enough for Schmoller even to mention them.

Things were not significantly different at the other end of the axis of the *Methodenstreit.* Excluding the outstanding contribution to economic theory of Auspitz and Von Lieben [1889], the number of Austrian contributions in the realm of mathematical economics between 1870 and 1914 can be counted on the fingers of a single hand. And Auspitz and Von Lieben were outsiders *par excellence.* Their education and training was in mathematics and natural sciences, and their careers were in business (and politics) and banking. Furthermore, though their partial equilibrium theory was inherently pegged onto the Mengerian notion of diminishing marginal utility, Menger's review of their book, and later Böhm-Bawerk's critique, were hardly forthcoming.[28]

This undoubtedly reflected Menger's presumption, described above, that the mathematical mode of analysis is not a proper tool of economic analysis. Menger's adverse reaction to Auspitz's and Von Lieben's book could not of course reflect a response to their attempt at "a pure theoretical investigation of a narrowly defined field — price theory" ("...rein theoretischen Untersuchungen auf ein eng umschriebenes Feld....die Preistheorie...,") as they put it in the preface to the volume [1889, IX]. In this — an investigation of price theory — they, after all, followed along the path which Menger himself cut out of the rock in his *Principles.* What might have made him angry may have not been only the mathematical technique which was employed to derive their results, but also the *reason* they gave for their revealed preference: the application of mathematics as a tool of economic research. They wrote: "We have used the analytical method, specifically graphic presentation...mainly because it allows precision which excludes misunderstandings due to ambiguous verbal definitions..." ("...Wir haben uns bei unseren Untersuchungen der analytischen Methode und namentlich der graphischen Darstellung bedient ....hauptsächlich auch darum, weil sie eine Präzision mit sich bringt welche alle aus vieldeutigen Wortdefinitionen entspringenden Missverständnisse ausschliess...") [ibid., XIII–XIV]. This statement linked with the suggestion made in the very first section of the preface, attributing many, if not most, of the misunderstandings and disputes among economists to semantics — due to the "private" terminology adopted by each writer. This formulation might have been perceived by Menger as a latent criticism and slight of his lingual method of reasoning (see above and footnotes 11 and 12).

In the preface they present a series of most brilliant early formulations of the scope and propositions of neoclassical economics. One of the most felicitous is presumably Auspitz and von Lieben's specification of price theory as the theoretical universe embracing product and factor pricing leading to their identification of Ricardian rent theory as the forerunner of the "modern" general theory of value. The reference to Ricardo's seminal contribution anticipates in print Marshall's well-known view on the significance of Ricardo's teachings and message on this score which Marshall said that "though obscurely expressed...anticipated...the modern doctrine of relations between cost, utility and value..." [1890-1961, 6, XXXCIC]. The date of the publication of the *Principles* indicates, of course, that Marshall's view of Ricardo's contributions

to the subject was developed independently — it undoubtedly was already in print when Auspitz-Lieben's *Investigations into the Theory of Price* [1889] was published.

The hostile attitude to their method of both the leading members of the German Historical School and of their Austrian rivals could not, of course, affect researchers like Auspitz and Von Lieben, who were following in the footsteps of Jevons and Walras yet were not interested in academic careers. But it could not have had a neutral effect on students and graduates aspiring to academic careers in Austria and Germany. After all they had to write their Ph.D. theses and later their habilitation studies — the condition for a tenured position in German and Austrian universities — for professors who were, at best, ignorant of the new methodology even if they did not reject it outrightly. This proposition — that the attitude of the leading academic members of the profession toward the application of mathematics explains the lag in the development of mathematical economics in the German cultural milieu — can be supported by resorting to a counter example: the comparatively rapidly rising trend of this branch of economics in the English-speaking academic environment in the last decades of the 19th century and early decades of the 20th century.

Note first that the rapid expansion of the mathematical genre in economic theory, applied economics, and statistics at that time — already showing up in the last phase specified by the Jevons-Fisher list (ending in 1897) — was first and foremost an Anglo-American phenomenon, although Italy and Lausanne were at that time also important centers of mathematical economics. On this score, the relevant fact is that Marshall was elected to the chair of political economy in Cambridge in 1885, and in the early 1890s Edgeworth was elected to the Drummond Professorship of Political Economy in Oxford. They, of course, did encounter opposition and not only in the literature. It took Marshall almost 18 years despite his increasing prestige until he convinced the academic establishment in Cambridge to set up the Economic Tripos in the University which finally established economics as a distinct academic undergraduate degree. This, indeed, settled the issue in the English-speaking intellectual milieu. In the first decade of 20th century economics was finally recognized as a distinct discipline.[29] With two men of the stature of Edgeworth and Marshall firmly established as the leading English economists, the use of mathematical techniques and deductive reasoning — extensively qualified, by sociological and historical evidence in Marshall's case, though less so in Edgeworth's — was a foregone conclusion. Their students (Pigou, Bowley, and Keynes amongst others) spread the message, which soon filtered into the red-brick establishments — the universities in London and the provinces. Though the Historical School led by Ashley at the turn of the century continued to snipe from the sidelines, economic theory supported by a mathematical tool set in which two-dimensional geometry was paramount, came to be the undisputed technique of economic discourse in the English-speaking universe by the turn of the century.[30] With Irving Fisher established at Yale from the early 1890s, and Moore, Clark and Taussig at Columbia and Harvard, a similar pattern developed throughout the other Ivy League institutions and soon spread all over the continent.

The major difference in the pattern of development of economic theory, and of mathematical economics, between the German-speaking cultural milieu — Germany



in particular — and the Anglo-Saxon and Lausanne French-Italian environment in the fifty years through the 1920s, is a matter of fact. It is inexplicable without reference to the altogether different impact of the *Methodenstreit* and the simultaneous debate within the group fostering the marginal revolution on the applicability of mathematical methods in economic research on the structure and the manning of academic institutions.

## NOTES

This article was first published in its German translation as "Der Methodenstreit und das Aufkommmen der Mathematischen "Öekonomie" in B. Schefold ed. *Vademeccum Zu einem klassiker Der Preistheorie*, the companion volume to the reprint of R. Auspitz and R. Lieben, *Untersuchungen über die Theorie des Preises, Klassiker der Nationalökonomie* (Verlag Wirtshaft und Finanzen, Düsseldorf, 1993). This revised English original is published by permission of Verlagsgruppe Handelsblatt.

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1. The full title of this volume in German is *Untersuchungen über die Methode der Socialwissenschaften und der Politischen Öekonomie insbesondere*.
2. The references are to Gustav Schmoller: A Contribution to the Methodology of the State and Social Sciences (*Zur Methodologie der Staat und Social-Wissenschaften)* [1883, 974-94]; and C. Menger [(1884) 1971]. From its very beginning the debate turned acrimonious. Schmoller concluded his review of Mengers' *Investigations* with the sentence, "We have written off that book." ("Wir sind mit dem Buche fertig.") Menger returned in kind saying, "The future, I hope not the very far future, will decide whether Schmoller succeeded to write off my methodological enquiries, or I have succeeded to write off Schmoller, the methodologist." ("Die Zukünft, und zvar wie ich hoffe, eine nicht allezu ferne Zukunft, wird darüber entscheiden, ob-Schmoller mit meinen methodologischen Untersuchungen, oder ich mit dem Methodiker Schmoller fertig geworden.") [(1884) 1971, 86].

   Gustav Schmoller (1838–1917) who graduated in 1860 from the University of Tübingen was appointed Professor of Political Science and Political Economy at the Universities of Halle, Strassburg and finally (1882–1913) Berlin. He was one of the founders of the Union for Social Policy (Verein für Sozialpolitik) which was in favor of evolutionary social reform, and was averse to laissez-faire notions of the 19th century liberalism. He was the prominent leader of the Younger German Historical School of economics [Blaug, 1986, 13–214].
3. The focus on the individual pursuing his "selfish" interest, the linchpin of Menger's value theory was of course alien to Schmoller's "Weltanschauung." In contrast, his conception involved individuals as members of a group — members of a nation who were motivated not only by considerations of their own welfare, but also the welfare of the nation as a whole.
4. This statement on the laws of economics attributed to Schmoller was made in a exchange with V. Pareto at a Conference in Bern [Spiegel, 1971, 427].
5. The passage, reprinted in all the subsequent editions, occurs in a chapter entitled "Methods of the National Economy" ("Methoden der Nationaloekonomie"). The two closing sections of this chapter were devoted to a discussion on the "advantages" and the "practical character of the historical method." The use of mathematics in political economy is discussed in a section that in the English edition (translated from the German 13th edition and published in 1878) bears the prophetic heading — "Former Methods." The German first edition (1854), however, does not carry a heading for this particular section, which is the first section of the chapter.
6. In an essay written in the early 1870s, Cliff Leslie, the doyen of English historical economics, proposed "the deletion of the deductive method of Ricardo...(since)...we view long trains of deduction with suspicion." [(1888) 1969, 72; Hutchison, 1953, 20].

7. This statement appeared in a volume devoted to the history of political economy published in 1888 at the height of the *Methodenstreit* in Germany. In England, though, the latter had but a faint resonance. Though undoubtedly influenced by the message of the German Historical School, Cliff Leslie, Thorold Rogers, J. K. Ingram, W. Cunningham and later W. J. Ashley represented a "native" English school of historical economics, highly critical of the methods and message of classical and, by extension of neoclassical, economics. Leslie's strictures, amongst others, anticipated the *Methodenstreit* by more than a decade.
8. The term "grenznutzen" (marginal utility) though was coined by von Wieser in his first contribution — *Über der Uhrsprung und die Hauptgesetze de sirthscaftliches Werthes* [1884].
9. Menger and his disciples were presumably not aware that this numerical set could be easily expressed in terms of the well-known expression for an arithmetical progression. Thus writing $U_i$ for the marginal utility of a given quantity of the $i$th commodity, $U_0$ for the marginal utility of the initial quantity of that commodity, $n$ for the relevant quantity of commodity in the sequence, and d for the common difference (the identical decrease of marginal utility in response to the single digit increase in quantity) the Menger tabular presentation can be formally spelled out as $U_i = U_0 - d(n - 1)$. The negative sign of the derivative with respect to $n$ is an expression of the law of diminishing marginal utility, the pivot of Menger's price theory. Using Menger's entries for $U_0$ in each of his 'ten scales' and following his illustration putting $d$ — the "common difference" — equal to unity, his whole tabular setup [Menger, (1871) 1981, 27] can be reconstituted.

   The linear form of Menger's implicit diminishing marginal utility function suggested by his numerical example implies a quadratic utility function. Note, further, that the arithmetical progression formulation for the diminishing utility function adopted above is fully consistent with the more general Austrian formulation of the utility function which does not assume, as the traditional mathematical specification of that function does, a second derivative at each point. This explicit assumption about continuity and the differentiability of the utility function at each point is "clearly not anchored in economical fact...while the Austrian formulation of decreasing marginal utility....is valid even if there are places where the function does not admit a second derivative." This point had been made by Karl Menger, Carl Menger's son and a mathematician by profession [Menger, 1973, 40].
10. This consideration led Jevons, who compiled a bibliography of "Mathematico-Economic Writings," published as an appendix in the second edition of *The Theory of Political Economy* [1888] and Fisher who republished and extended it [(1892) 1926, 120-21; Cournot, (1838) 1960, 173–209] to include Menger's *Principles* and also von Wieser's and Böhm-Bawerk's writings in the list as one of "the writings...whose mathematical character is not explicitly expressed in symbols or diagrams".... [Fisher, (1892) 1966 120-21].
11. The letters were dated 28 June 1883, and February 1884. The first was written just after the publication of the *Untersuchungen* and the second when he completed the *Irrtümer*. He enclosed the latter pamphlet with the second letter.

    These two letters, the second letter in particular, offer a clear explanation of Menger's preferred method of economic research, and the feasible contribution, strictly limited to presentation, which mathematics could, to his mind, offer to economic analysis. Hayek, in his well-known introduction to the London School of Economics reprint of Menger's *Principles* says that "so far as I am aware, he has nowhere commented on the value of mathematics as a tool of economic analysis." [(1871) 1981, 14–15]. At the time of writing (in the 1930s) he was probably unaware of these two letters, published in the Jaffé edition of Walras' letters in the 1860s.
12. The German original of this statement puts the causal-cognitive issue in the following terms: "Wie vermöchten wir nur aber zu den *Erkenntniss*....des wertes, der Grundrente, des Unternehmer - Gewinnes, der Arbeitstailung, des Bimetallismus u.s.f. auf mathematischen Wege zu gelangen?" [Jaffé, 1965, 602, italics added]. Böhm Bawerk offers a similar reservation on the usefulness of the mathematical technique of economic analysis. Its failure to offer "a correct causal solution of the problem" [Böhm Bawerk, 1921, 315] is according to him its inherent weakness.
13. The English rendition of the German original is Stigler's [Stigler, 1946, 181–82].
14. An earlier formulation of Mises' view on the methodology of economics appeared in his *Grundprobleme der Nationaloekomie* [1933] — written whilst he lived still in the geographical and intellectual environment of Austria. Leading members of the neo-Austrian school in the United States include M. Rothbard, I. Kirzner and L. Lachman. Mises' methodology is discussed in Blaug [1980] and Herbener [1993]. See in particular, R.W. Garrison's article in Herbener's volume.



15. Mill passed away in 1873 and never reacted in print to Jevons' challenge to the classical value theory and to his statement on the role of mathematics in economics. His last published writing on economics, in which he made the famous recantation of the "wage fund theory," was published in 1869. Jevons, indeed, raised the issue as early as 1862 in a paper presented to Section F of the British Association for the Advancement of Science. Yet for a decade, until the publication of the *Theory*, no one reacted to the methodological issues raised in this paper nor to its substance — the application of the theory of utility to the analysis of value (and of prices).
16. Samuelson makes the case for the "[t]he strict equivalence of mathematical symbols and literary words." [1952, 56]. He attributed the statement though, to Willard Gibbs of Yale — probably being unaware of Launhardt's precedence. Laundhardt's statement, though, is reproduced verbatim, in German in Appendix III of Fisher's *Mathematical Investigations into the Theory of Value and Prices* [(1892) 1926, 113). In a private communication Professor Samuelson convincingly argued that Gibbs did not get his aphorism from Launhardt.
17. The recent paper on "The Overuse of Mathematics in Economics" [Quddus and Rashid, 1994] published in this *Journal* and Paul Samuelson's comments on it [Samuelson, 1994] in which he refers to "the once and forever Battle...a methodenstreit between shirt-sleeve hard fact economists and fancy Dan theoretical economists...." [ibid., 268] indicates that the debate is still alive and kicking fifty years later. The title of the Quddus and Rashid article might suggest that in the last decade of the century the debate had changed form: the argument is by now on dosage and not on the legitimacy of the use of mathematics. This reading of events puts, however unwarrantedly, the recent neo-Austrian challenge out of bounds. Thus, "nothing is new under the sun"... (Ecclesiastes).

    I have no reservations to Professor Samuelson's statement made in the first note of his paper that a "dichotomy between fact gatherers and theory spinners" [Samuelson, 1994, 272] in economics would be an improper classification of members of the profession. I regret if my article conveys that oversimplified impression — it was definitely not my intention to put it that way. Indeed, I believe that the "crude logical positivists" like Professor Samuelson (according to his description in a private communication) following the trail blazed out by Fisher, Wicksell and Marshall have warrantedly cornered the market in the second half of the 20th century.
18. Though Marshall does not say so, and was obviously unaware of the exchange between Menger and Walras on the subject (see above) in which the former conceded the use of mathematics for presentation yet insisted on its inapplicability to economic research, Marshall puts it exactly the other way around. "The chief use of pure mathematics in economic questions seems to be in helping persons to write down quickly, shortly and exactly, some of his thoughts for his own use; and to make sure that he has enough and only enough premises for conclusions (i.e., that his equations are neither more nor less in number than the unknowns)..." [1890-1961, X]. This statement, though, might have been a latent response to Cairnes who also conceded the use of diagrams for purpose of presentation (above). Marshall was, of course, aware of Cairnes' statement in the Preface to the Second Edition of his *Method* [(1875) 1975]. See also, Samuelson [1952, 60].
19. Wicksell refers to the "usefulness of....mathematical treatment...." of economic issues in *Value Capital and Rent* [(1893) 1954, 20]. This was soon followed by Pareto's *Journal of Political Economy* paper [1897, 485–507] designed as a comprehensive defense of the deductive method in general and the application of mathematics in economic research in particular. Several sections in Fisher's 1898 paper, written on the occasion of the publication of a translation of Cournot's *Researches*, are devoted to a rebuttal of the arguments against the application of mathematics.
20. Fisher, like Jevons, was quite liberal in the definition of mathematical economics for his adapted list. Amongst others Marx's *Das Kapital* is included. In the second version of this list he excluded some entries in Jevon's original list among which was Menger's *Principles*, since "they did not make use of symbols." [Fisher in Cournot, (1838) 1897, XI].
21. In the Foreword to the 1927 reprint of the English translation of Cournot's *Researches* Fisher declared victory: "In the twenty years (actually almost 30 years, H.B.) since the book appeared, the mathematical method has become so general in economic and statistical studies (thus)....there is today little need, as there was (then), to emphasize the value of that method, and it is *now* seldom, if ever challenged..." [Fisher in Cournot, (1838) 1960, VII].
22. The statement is Ingram's, in the 9th edition of *Encyclopedia Britannica*, Vol. XIX, page 399, quoted by Fisher [(1892) 1926, 117]. On Walras' struggle for acceptance from the early 1870s, through the

first decade of the century see Howey [1972, 292–96]. The fact that Walras never had a chair in Paris is an obvious case in point.

23. In the second part Von Thünen presents his wage theory in which he anticipates the marginal productivity theory of factor pricing. This was implicit already in his theory of location presented in the first part of the book which was published 24 years before the second [Von Thünen, (1826-1850) 1966, XXIX, XL]. In his *Volkswirthschaftslehre* [1863] Mangoldt followed Von Thünen on this score, and made independent contributions at least in terms of presentation.
24. The "unavailability" of Gossen's book is underlined by the fact that Launhardt in 1885 did not succeed in tracing a copy. [Launhardt, (1885) 1982, XI].
25. The statement is by Schumpeter, who rated Bortkiewicz's mathematical analysis of Marx's value theory, and Rodbertus and Marx's rent theories as "masterpieces" [Schumpeter, 1951, 303; Blaug, 1986, 29–30].
26. The paper is Georg S. Helm's [1893]. The reference to Launhardt's academic career is attributable to J. Niehans' article in *The New Palgrave Dictionary*. Vol. II, [XXXX, 140–42], and to Blaug [1986, 122–24].
27. The statement that he made an effort to refer to all the *relevant* sources published through 1902, was made in Schmoller's preface to Volume II of the *Grundriss*.
28. On this see Jürg Niehans' paper in Schefold [1993, 13, 53–54].
29. On the struggle of Marshall to establish the Economic Tripos in Cambridge, finally set up in 1903, see Kadish [1991, 290-92]. (The Tripos is the set of examinations which a student has to take in Cambridge as a condition for an undergraduate degree). The relevance of the formal 'recognition' of economics as a discipline in its own right is underlined by the fact that in the early 1890s Wicksell who had a second degree in mathematics and had already made major contributions to economics, had to study and take a degree in law as a condition for a professorial appointment in Lund. Of course Sweden at that time followed strictly the German (and continental) academic conventions.
30. An interesting case in point is the well-known, rather late in the season, exchange between Clapham and Robertson in the *Economic Journal* of 1925. The former tried to ridicule the 'empty economic boxes' of the economists, while the latter responded in kind.

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