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JOSEPH A. SCHUMPETER,

Historian of Economics

Perspectives on the History of Economic Thought

Edited by Laurence S. Moss



JOSEPH A. SCHUMPETER, HISTORIAN OF ECONOMICS

Joseph A. Schumpeter was one of the greatest economists of the twentieth century. His *History of Economic Analysis* is perhaps the greatest contribution to the history of economics, providing a magisterial account of the development of the subject from Ancient Greece to the mid-twentieth century.

Schumpeter's views on his predecessors have proved to be a constant source of controversy. Here individual chapters examine such disparate questions as Schumpeter's apparent disregard for the American Institutionalists, his grudging respect for Adam Smith, the perspicacity of his views on Quesnay, and his preference for Walras over Pareto. Four chapters are devoted to the early medieval schools, neglected in all of his writings. Schumpeter's magnum opus is related to the rest of his economic output, especially his views on money and on methodology.

With contributions by leading historians of economics from six countries, this volume analyses Schumpeter's contribution to the history of economics, considers its lasting significance, and uses it as a benchmark to assess the current state of the field.

Laurence S. Moss is Professor of Economics at Babson College, Massachusetts.

JOSEPH A. SCHUMPETER, HISTORIAN OF ECONOMICS

Perspectives on the history
of economic thought

Selected papers from the History of Economics Society
Conference, 1994

Edited by Laurence S. Moss



London and New York

First published 1996
by Routledge
2 Park Square, Milton Park, Abingdon, Oxon, OX14 4RN

Simultaneously published in the USA and Canada
by Routledge
270 Madison Ave, New York NY 10016

Transferred to Digital Printing 2006

© 1996 History of Economics Society

Typeset in Garamond by Keystroke, Jacaranda Lodge, Wolverhampton

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British Library Cataloguing in Publication Data

A catalogue record for this book is available from the British Library

Library of Congress Cataloguing in Publication Data

Joseph A. Schumpeter, historian of economics / edited
by Laurence S. Moss.

p. cm. — (Perspectives on the history of economic thought)

Includes bibliographical references and index.

1. Schumpeter, Joseph Alois, 1883–1950.
2. Economists—United States.
3. Evolutionary economics—History. I. Moss, Laurence S., 1944– . II. Schumpeter, Joseph Alois, 1883–1950. History of economic analysis. III. Series.

HB119.S3J673 1996

330'.90—dc20 95-40590

CIP

ISBN 0-415-13353-X

Publisher's Note

The publisher has gone to great lengths to ensure the quality of this reprint but points out that some imperfections in the original may be apparent

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CONTRIBUTORS

Richard Arena (Chapter 11) is Professor of Economics at the University of Latapses in Nice-Sophia Antipolis, France.

Roger E. Backhouse (Chapter 2) is a Reader in the History of Economic Thought at the University of Birmingham, England.

Louis Baeck (Chapter 7) is a Professor at the Catholic University of Leuven, Belgium.

William Barber (Chapter 3) is Andrews Professor of Economics Emeritus at Wesleyan University, Middletown, Connecticut.

Peter J. Boettke (Chapter 16) is a Professor of Economics in the Department of Economics at New York University.

Philippe Broda (Chapter 15) is a Professor at the Centre d'Histoire de la Pensée Economique, University of Paris I Pantheon/Sorbonne.

Antonio Callari (Chapter 17) is Professor of Economics at Franklin and Marshall College, Lancaster, Pennsylvania.

Annie L. Cot (Chapter 5) is Professor of Economics at University of Lille I and University of Paris I.

Ghislain Deleplace (Chapter 10) is a Professor of Economics at University of Paris VIII.

Agnès Festré (Chapter 11) is preparing her dissertation at the University of Latapses in Nice-Sophia Antipolis, France.

Hamid Hosseini (Chapter 6) is Professor of Economics at King's College, Wilkes-Barre, Pennsylvania, and is a Visiting Scholar at Harvard University.

Jérôme Lallement (Chapter 5) is Professor of Economics at the University of Bourgogne.

Nelson P. Lande (Chapter 8) is Lecturer in the Department of Philosophy at the University of Massachusetts-Boston.

CONTRIBUTORS

Laurence S. Moss (editor) is Professor of Economics at Babson College, Babson Park, Massachusetts.

Spencer J. Pack (Chapter 12) is Professor of Economics at Connecticut College, New London, Connecticut.

Mark Perlman (Chapter 1) is Professor Emeritus at the University of Pittsburgh.

Bette Polkinghorn (Chapter 4) is Professor of Economics at California State University at Sacramento.

Steven Pressman (Chapter 14) is Professor of Economics and Finance at Monmouth University, West Long Branch, New Jersey.

David L. Prychitko (Chapter 16) is a Professor of Economics in the Department of Economics at SUNY-Oswego.

Yuichi Shionoya (Chapter 18) is Professor Emeritus at Hitotsubashi University, Tokyo, Japan.

Nathalie Sigot (Chapter 13) is at University of Paris I Pantheon/Sorbonne and at University of Picardie.

Mark Tomass (Chapter 9) is a Visiting Professor at Masaryk University, Czech Republic.

ACKNOWLEDGMENTS

I wish to acknowledge the kind assistance of Professors Rick Holt and Ingrid Rima, who assisted with the editorial review work that needed to be done by serving along with myself on the editorial committee. Mr. Alan Jarvis, Ms. Olivia Eccleshall, and the rest of the staff at Routledge were especially efficient in completing the many chores associated with publishing this manuscript in a timely and tidy manner. Babson College was especially gracious in providing a beautiful campus site for our June 1994 Conference and providing me with several course releases. Finally I wish to mention the Babson College Board of Research, which funded some of the typing and editorial work on the project. The following individuals deserve a special word of thanks for the good work they did helping me organize the June 1994 History of Economics Society Conference, where the papers now included in this volume were first presented: Mr. Carl Citron, Mrs. Anita Clymas, Mr. Craig Decker, Ms. Kathleen Forrest, Ms. Widdy S. Ho, Ms. Desiree Koutouan, Mr. Michael McBrien, Mr. Joshua Louis Moss, Ms. Diep Phan, Ms. Cecily Sanchez, Ms. Wendy Silverman, and Dr. Mark Tomass.

Laurence S. Moss
Babson College
July 1995

INTRODUCTION

Laurence S. Moss

The twenty-first annual meeting of the History of Economics Society was held at Babson College, 10–13 June 1994. Since 1994 marked the fortieth anniversary of the publication of Joseph A. Schumpeter's *History of Economic Analysis* (*HEA*), a request went out to those presenting papers at the conference to appraise what Schumpeter said about their subjects (if, indeed, he said anything at all) and to assess how well Schumpeter's treatment stacks up to contemporary thinking on their areas of expertise. The point of this exercise was not only to pay homage to perhaps the greatest work by one of the greatest economists of the twentieth century but also to use the *HEA* as a benchmark against which to measure progress in our small but thriving subdiscipline within economics. I was delighted that many of the 130 presentations harked back to Schumpeter's majestic *HEA*, either to document an alternative treatment of a current topic of interest or else to commend the esteemed Harvard economist for his pioneering investigations.¹

At the end of the Babson conference a referee committee was set up and charged with the practical responsibility of deciding which of the papers submitted for publication would be accepted for inclusion in this volume. Eighteen of the thirty-six submissions were selected to become chapters. Together these chapters offer a rich tapestry of commentary and reflection on economic literature spanning several centuries. What they have in common is their connection to Schumpeter's magisterial volume or (with several exceptions) to important themes that Schumpeter raised in that volume.

Schumpeter had not completed his work on the *HEA* manuscript at his death in 1950. It remained for his third wife, the scholar Elizabeth Boody Schumpeter, to cull fragments, incomplete chapters, and nearly completed sections of the early drafts. She assembled a coherent and monumental study that ranged from Ancient Greece to Keynes and, in the words of one eminent reviewer, aimed "to account for every writer who made a significant contribution to the development of economic theory" (Viner 1991: 327). Of course, no book could do all that, especially for a discipline like economics in which the outer frontiers were changing as its scope enlarged and its methods of investigation developed. Despite its many blemishes, Viner got it right when he described Schumpeter's

History of Economic Analysis as “the most constructive, the most original, the most learned, and the most brilliant contribution to the history of analytical phases of our discipline which has ever been made” (1991: 328). When asked to prepare a suitable introduction to the 1994 reprinting of the *HEA*, Mark Perlman concurred with Viner’s assessment while acknowledging that in certain areas the work remained unfinished: “Flawed by its incompleteness (due to the author’s sudden death), there is, nonetheless, nothing else like it in the English language; and even when one turns to other cultures, nothing has appeared which has its appeal, if not its scope” (Perlman 1994: xxi). The task of remedying this incompleteness is what motivated several of the authors who have prepared chapters for this current volume, as I shall explain below.

The following chapters were selected as high-quality contributions to economics and its historical development but they were chosen with a broader goal in mind. The chapters taken together highlight some of the most interesting facets of Schumpeter’s book: its treatment (or mistreatment) of certain thinkers and topics and most remarkably its relationship to the whole of Schumpeter’s scientific contribution made during the first half of the twentieth century.

In the remainder of this Introduction I shall highlight the interconnections between the chapters below and the light they throw on both Schumpeter’s historical treatise and other aspects of his scientific contribution. I make no claim whatsoever that the interpretive opinions of the nearly two dozen economists presented here add up to an embroidered whole; admittedly, we have a patchwork quilt of one sort or another. The linkages that I record here would probably be no surprise to the individual authors, but no vote has been taken on them. This volume provides an interesting and, I hope, not too controversial companion to the *HEA*. At the same time it represents the twelfth in the History of Economics Society’s “Perspectives on the History of Economic Thought” series. In its topics and in the distribution of its authors, this volume of the series continues the Society’s history of bringing together distinguished economists from around the globe and promoting inquiry into the history of economics and related areas of intellectual history.²

The best place to start is with Schumpeter’s personal understanding of what it was that he was trying to do in *HEA*. Schumpeter announced that his book was to be a history of economic analysis: “theorems and not persons [were to be] the heroes of [his] story.” In point of fact, the great book never stuck to this or any single historical approach. The reviewers of the book acknowledged as much in 1954, when the book first appeared. According to Viner, a legitimate history of economic analysis can be written from a variety of perspectives. First, one can concentrate on ideas and concepts, the “intellectual ingredients of theories.” Second, one can write about the history of theories as ingredients in large systems of thought. There is always the third alternative, of writing about the economists or philosophers themselves and their respective schools of thought. Finally, one can concentrate on the history and use of particular analytic tools. Viner concluded that Schumpeter provided his readers with a

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little bit of every conceivable approach and “move[d] from one to another freely as he [went] along” (Viner 1991: 328). The reader senses a hypnotic quality to the work as Schumpeter declares what is or is not important about the immense literature that he somehow managed to survey in a thousand or so pages of small text. Still, despite the unanimous praise, it is part of a scholar’s business to scratch beneath Schumpeter’s rhetorical swirls and erudite opinions to ask what it is that Schumpeter *thought* he had accomplished and, equally as important, what it is that he *actually* accomplished.

Mark Perlman’s “Assessing the Reprinting of Schumpeter’s *History of Economic Analysis*” (Chapter 1) provides a stimulating opening for this volume. It is well known that Schumpeter praised Léon Walras for his vision and technical accomplishments in formulating the idea of general equilibrium. What is so puzzling to scholars like Perlman, then, is why Schumpeter did not heap the same quantity of praise on Vilfredo Pareto, whose broader sociological concerns line up so closely with what Schumpeter considered to be important to economic sociology and economic history. With its emphasis on the nonrational aspects of human action, Pareto’s sociology should have had strong appeal for Schumpeter “the man” as well as Schumpeter the “social scientist.” Yet it is Walras and not Pareto who emerges as the hero in the work that was, most ironically, supposed not to have any heroes at all. Mark Perlman asks why Schumpeter generally overlooked the importance of Pareto’s sociology. Schumpeter’s obituary of Vilfredo Pareto was prepared a year or so before Schumpeter himself died, and it is tempting to speculate on how Schumpeter might have revised his treatment of Pareto had he been blessed with more time to finish the *HEA* (Schumpeter 1949). It is clear that both Schumpeter and Pareto emphasized the “nonrational” side of human action, although Schumpeter considered it mostly in terms of preanalytic visions that subsequently motivate the development of analysis (which then somehow escapes the limitations of the preanalytic vision). Pareto, on the other hand, stressed that nonrational elements are manifest in all human action. Other contributors – Roger E. Backhouse (Chapter 2), Antonio Callari (Chapter 17), and Yuichi Shionoya (Chapter 18) – also touch on the problem of the presuppositions of economics and how they shape analysis.

Schumpeter’s notion of a preanalytic “vision” must have seemed an odd idea to those economists who in the second part of this century campaigned to make economics a rigorous science more like physics than like psychology. By the 1960s, however, Schumpeter’s pattern of thought had become popular among historians of science through the related Kuhnian construct of the “paradigm,” which came to dominate historiographical writing. Roger E. Backhouse (in Chapter 2) persuades us that Schumpeter’s notion of vision actually anticipated the one made famous by Thomas Kuhn. Indeed, the *HEA* recognizes at one place after another that economics, like all sciences, emerges from the activities of collections of thinkers who have some conscious understanding of the involvement of the others. These discussions add up to an implicit contribution of the now fashionable field of “sociology of science.” Backhouse’s observations

are firmly supported by Shionoya (Chapter 18), and both credit Schumpeter for his contributions to the fledgling field of sociology of science.

Although Schumpeter had a remarkable tendency to uncover clusters of intellectuals and construct from them veritable “schools of thought,” there are places in his *HEA* where he seems to turn a blind eye to important bodies of economic literature. In some cases, these omissions are not the sorts of matters that Schumpeter might have corrected in a final, polished version of his *HEA*. Many of the chapters that follow focus on errors and omissions in the story Schumpeter tells about the history of our discipline. William Barber, who has done much to call attention to the American economic contribution, suggested that Schumpeter’s selective vision may have seriously biased his handling of American economic thought in the *HEA*. Schumpeter was distinctively unkind to the Wisconsin school of thought – a school that included John R. Commons. The Wisconsin school’s considerable impact on the New Deal policy during the 1930s merited at least a mention. (Could Schumpeter’s opposition to the New Deal have biased his historical judgment?) The great institutionalist John R. Commons, who pioneered an intelligent appreciation of the role legal institutions play in giving shape to the market process, does not receive as much as a nod in the *HEA* (indeed, his name does not even appear in the *HEA* if we can rely on the author index). Another example is the great Darwinian-style economist, Thorstein Veblen, who is mentioned several times but is not fully appreciated. (In Chapter 15, Professor Broda offers an interesting comparison of the methodological approaches of Commons and Veblen, repairing this obvious gap.)

Of course, there are several American economists that do receive praise for their analytic achievement, but Barber suggests that an ideological bias on Schumpeter’s part may have clouded his judgment. Barber finds it hard to avoid this verdict, especially given the praise Schumpeter heaped on Henry C. Carey, for whom the bulk of the American economics profession had only contempt. Similarly, Bette Polkinghorn (in Chapter 4) expresses puzzlement as to why Harriet Martineau and Millicent Fawcett, both of them English writers with publications enjoying major market successes and not without some analytic acumen, receive no attention from Schumpeter.

Part I concludes with a stimulating chapter by Annie L. Cot and Jérôme Lallement. In the grand tradition established by Schumpeter himself, Cot and Lallement try to define the historical moment when the concept of “the economy” arose. The emergence of this distinct mental entity separate from other longstanding notions – “community,” “empire,” “borough,” “church,” and so on – required the juncture of new categories of thought. Cot and Lallement identify three “ruptures” that occurred over two centuries and that are represented in important writings by Bernard Mandeville, John Locke, and Jeremy Bentham. The junction of these ruptures with the past establishes the frontiers of a new discipline which we now recognize to be “economics.” Economics studies the novel idea of “the economy.” Cot and Lallement suggest that one cannot

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suppose that ideas about prices, commodities, interest rates, and self-seeking human behavior were always there to be studied as “economic facts,” as Schumpeter contended. Instead, it may be impossible to write a history of analysis that does not situate economics in a cultural nexus of understandings and special meanings. Cot and Lallement can be interpreted as proclaiming that Schumpeter’s research program in *HEA* was more than marred by several errors and omissions; indeed, the project was flawed from top to bottom.

According to Cot and Lallement, who draw inspiration from Michel Foucault’s pioneering work on the archaeology of the human sciences, what Schumpeter tried to accomplish was virtually impossible (Foucault 1994). How can anyone write a history of economic analysis going back to ancient times when the concept of an economic system is of such recent vintage? Most of the authors in this volume, however, would not go so far as Cot and Lallement and rule out the search for the historical roots of ideas about the economy that Schumpeter pioneered so fruitfully. What unites most of the other contributors, in fact, is an apparent agreement that Schumpeter’s goal was a legitimate one but that he made several errors and many omissions.

Of all Schumpeter’s alleged errors in the *HEA*, the one that seems to have stirred up the most debate among historians is Schumpeter’s remark about an alleged “great gap” in the flow of analytic economic discussion between the ninth and fourteenth centuries (Schumpeter 1954: 73). That is why this volume devotes a considerable amount of space to the subject. Part II contains a most interesting and diverse collection of chapters relevant to this most notorious assertion of Schumpeter’s. I suspect that it is best to review Schumpeter’s apparent error first and then move on to a short summary of the arguments presented in the papers.

At the start of the second part of the *HEA*, in a remarkably polished chapter entitled “The Scholastic Doctors and the Philosophers of Natural Law,” Schumpeter explained to his readers why he was jumping 500 years ahead to St. Thomas and the thirteenth-century renaissance from early Christian thought (St. Augustine and the Roman Church fathers), thereby ignoring the enormous literature of the Eastern Byzantine Empire and by implication the literature of Egypt, Persia, Spain, India, China, Africa, and other places as well. Schumpeter acknowledged legal rights, monetary matters, including fiscal tax policy, and commercial and agricultural policy were of course subjects of an immense literature. This is undoubtedly true, but Schumpeter went on to assert that among the thousands of manuscripts and fragments that have survived between, say, AD 800 and AD1300 there is precious little that amounts to a philosophical or speculative outlook about money, taxes, and law. For that reason, “no piece of reasoning that would have to be mentioned here has been preserved” (73). Is that true? Is there a great gap in the literature of analytic economics?

Hamid Hosseini does not think so, and in Chapter 6 he does his best to set the record straight by mining the Persian sources. Hosseini calls attention to many medieval Muslim writers, inspired by the Islamic ethos and influenced by

Greek and Iranian thought, who explained the economic realities of their societies during the centuries of Schumpeter's great gap. Louis Baeck follows in Chapter 7, with an interesting treatment of Ibn Khaldun, a cleric of the fourteenth century (at the tail end of Schumpeter's blackout period) who, Baeck argues, is the greatest social scientist of medieval Islam. Schumpeter was aware of Ibn Khaldun's seminal contributions as a historian and pioneer sociologist, but he perhaps understated Ibn Khaldun's contributions to the theory of economic development (Schumpeter 1993).

In Chapter 8, Nelson P. Lande provides a clear analysis of the linkages the great twelfth-century Jewish philosopher Maimonides detected between wealth and charity. Apparently, charity is justified not in terms of the salutary consequences it will have on the recipients but in terms of the personal impact it has on the state of mind of the donor. Maimonides ranked the different categorical ways in which charity may be given from most preferred manner of giving charity to the least preferred method. Lande does not make any attempt to distinguish religious or theological discussion from the rudiments of an emerging social science. Still, Lande's accurate and authoritative presentation of Maimonides's views will pave the way for a greater appreciation of this philosopher by historians of social science. The ground has been cleared for others to debate whether the work of Maimonides should be considered an exception to the "great gap." Interestingly, Maimonides is mentioned at least once in Schumpeter's *HEA* as the great Jewish theologian who, like St. Thomas, tried to reconcile religious doctrine with Aristotelian analysis.

Finally, in Chapter 9, Mark Tomass offers us a readable and extremely useful translation of the significant parts of the Egyptian Al-Maqrīzi's pioneering essay on money, which was written at the start of the fifteenth century. Technically, this work, like Ibn Khaldun's, lies outside Schumpeter's blackout period, but it suggests that even in the literature Schumpeter did cover there is more work to be done. Ordinarily the manuscript would be classified as "late Renaissance" except that it was written in a tumultuous period: at the time, the Islamic community was nearing the end of its glorious age of accomplishment and prosperity. Al-Maqrīzi's goal was to explain the depression-like crisis to the masses. His method was to review significant events in history with an eye toward making policy recommendations for ending the current crisis. In order to lay the foundation for policy formulation, Al-Maqrīzi went beyond Maimonides in his writings. Al-Maqrīzi claimed that it is not the quality of one's acts that is of prime importance but the consequences of the acts. He moralized about the evils of corruption, currency debasement, high rents, oppressive taxation, and so on, but his moral outrage is remarkably relegated to the strictly scientific claim that corrupt activities cause living standards in the realm to fall. Although Tomass is much more guarded in his praise of the analytic quality of this discussion than, say, Hosseini, his chapter suggests that Al-Maqrīzi's special book may indeed be the beginning of modern social science. Tomass also argues that future histories of economic thought can ill afford to do what Schumpeter did and omit

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references to fourteenth-century Islamic writers Ibn Khaldun and Al-Maqrīzi, or for that matter the rich harvest of Persian and Jewish writers also.

Thanks to the research presented in this part and to a variety of important other writings and papers, some of which appeared in previous History of Economics Society conference volumes, Schumpeter's "great gap" has begun to be filled (Essid 1987; Ghazanfar 1991; Soofi 1995). As this century ends, historians can celebrate some success in responding to Schumpeter's implicit challenge. (The textbook literature in the history of economics has yet to take adequate notice of this literature, however.)

Part III contains two stimulating chapters responding to significant controversies sparked or at least fanned by the *HEA*. It is a well-known fact that Schumpeter intended to publish a major work on money and banking that would elaborate and in some ways supplant an earlier monograph that had been favorably received by economists (Schumpeter 1952). Schumpeter's major work existed in the form of a manuscript written in German that he had prepared in Europe and carried with him to Harvard, where he planned to make major revisions. That manuscript was finally published in 1970. It is clear that Schumpeter considered his theoretical manuscript on money, begun in the 1930s, to be inadequate and, according to Robert Loring Allen, he "had been fussing with it ever since." It is clear from Schumpeter's personal correspondence that as late as 1949 he was planning a revised English version (Allen 1991, 2: 228–9). His unsettled views on money and credit and their relationship to the formal features of a capitalist society as expressed by the general equilibrium model must have shaped his understanding of the history of monetary economics. There are clues hidden in the *HEA*.

Two papers at the conference were addressed to Schumpeter's monetary views, and both have become chapters in this part. In Chapter 10, Ghislain Deleplace reviews the complicated discussion that Schumpeter offered in Part II, Chapter 6 of the *HEA* about the differences between real analysis and nominalist monetary analysis and how Schumpeter's personal views on the subject were in a confusing state of development. It is clear from Schumpeter's discussion that he drew heavily on the quantity theory of money to make his antimetallist position consistent with his real approach to production and exchange. According to Deleplace, Schumpeter, despite all of this, could not deal with the general equilibrium problem that confronts the general equilibrium school; namely, that in equilibrium any commodity can serve as the numeraire in which to measure prices and that money seems to have no place in such schema at all. Deleplace speculates about what Schumpeter's views on money would have been like had he paid more attention to the 1577 French debate about the separation between the "unit of account function" of money and the "medium of exchange function" of money. Had Schumpeter stepped beyond Bodin and Malestroit, he might have broken new ground and seen a way to integrate money into general equilibrium analysis. This distinction between money as a "medium of account" and money as a "medium of exchange" does seem to be coming into its own in

monetary economics, and there is some evidence that Schumpeter may indeed be named a pioneer of this development (Cowen and Kroszner 1994; Shah and Yeager 1991).

In Chapter 11, Richard Arena and Agnès Festré remind us of Schumpeter's extensive search in his many theoretical books and articles for an alternative to mainstream economics, which posits that the financial sector plays a fundamental role in the development of a capitalist economy. Inventions do not instantly become innovations and thereby raise living standards. It is a complicated business to wrest resources free from other steady-state applications. In *Business Cycles* Schumpeter even went so far as to define capitalism as "that form of private property economy in which innovations are carried out by means of borrowed money, which in general, though not by logical necessity, implies credit creation" (Schumpeter 1939: 1, 223). Arena and Festré amass an impressive list of references where Schumpeter tries to explain why the financial sector must have real effects, and they suggest that Schumpeter's *HEA* ought to be informed by this insight as well. It remains for others to take this next important step and demonstrate the connections between financial intermediation and real economic development.

Part IV of this book moves on to the classical school of economics and fleshes out some details of Schumpeter's thinking about this important development in economics. In Chapter 14, Steven Pressman gives Schumpeter high marks for a correct understanding of the Physiocratic notion of the *Tableau Economique* – the famous zigzag diagram. Schumpeter's remarks in 1954, especially those to the effect that the *Tableau Economique* was neither a general equilibrium model of the economy nor an input–output schema, are remarkably coherent and have held up well. Pressman explains that the *Tableau* was not a general equilibrium model because it gave pride of place to money as the means by which economic exchanges allowed the surplus production in agriculture to nourish all classes. It was not an input–output model either, Pressman argues, because of the importance Dr. Quesnay attached to the surplus idea. Pressman praises Schumpeter for his perceptive understanding of the Physiocratic school.

Two characteristics of Schumpeter's treatment of classical economics have been much commented on by reviewers. The first is Schumpeter's apparent dislike for Adam Smith as an original theorist and his consequent dismissal of the *Wealth of Nations* for containing little that was original for its time (Viner 1991: 338). The second is Schumpeter's strong distaste for vulgar utilitarianism, which, according to Schumpeter, owes its popularity to Jeremy Bentham's towering influence but which ironically had very little to do with the theoretical content of Bentham's economics (Viner 1991: 334). Both conclusions may have been hasty and overdrawn, as Chapters 12 and 13 demonstrate.

In Chapter 12, Spencer Pack argues that had Schumpeter a more complete knowledge of the entire body of Adam Smith's writings – especially in the 1762–3 report of Smith's lectures, which was rediscovered only in 1958 (Smith 1978) – he might have had more genuine praise for Smith's analytic contribution.

INTRODUCTION

Pack explains that the idea of the “invisible hand” is not a description of laissez-faire policy but instead a metaphor that Smith employed to communicate the subtle insight that customs, habits, and institutions often have unintended consequences, which may be undesirable as well as desirable. In the end, a careful study of the entire body of Smithian literature suggests that Smith was an epistemological skeptic and not at all unlike David Hume.

In Chapter 13, Nathalie Sigot examines the impact that Bentham’s utilitarian approach to legislation had on the specific description and analysis Bentham offered of the segmented labor market. This presentation is enough to cast doubt on Schumpeter’s claim in the *HEA* that the analytic stature of Bentham’s economics is totally independent of his utilitarian theory of legislation. Sigot examines how the utilitarian approach influenced what Bentham had to say about wage rate determination.

The three chapters of Part V cover ground that is not directly related to Schumpeter’s *HEA*. As we are reminded in Chapter 3, Schumpeter was maddeningly silent about the American Institutionalists as theorists. In Chapter 15, Professor Broda contrasts the use of evolutionary ideas in the writings of Thorstein Veblen and John R. Commons. This essay fills an important lacuna in the *HEA* in a manner that would have appealed to Schumpeter, who had a rich interest in related streams of thought in science.

In Chapter 16, Peter J. Boettke and David L. Prychitko review the considerable number of contributions to subjectivist economics that we find in the economics of Kenneth Boulding. Their discussion points up the irreconcilable contrast between a subjectivist economics that argues that the social world is not much more than a social construction of reality carried out under conditions of radical uncertainty and a general equilibrium framework that assumes that information is costlessly available to all and objective and measurable in character. It is clear that Schumpeter favored a thoroughgoing objectivism in economics, at times denying that “economic facts” were anything more than sense sensations. Boulding and the modern Austrian school represent an approach to economic literature that is from this point of view “anti-Schumpeterian” in both detail and spirit and yet deserving of a place in any comprehensive history of *analysis*.

In Chapter 17, Antonio Callari indicts classical political economy for its attachment to patriarchal modes of discourse that themselves help promote the subordination of women in our culture. I do not think that Schumpeter would have ever suspected that conceptual thought could have such an unintended effect on both social and political organization. Callari is convinced that a discourse that does not distinguish the gender of agents perpetuates, and even justifies, the lack of discussion about subordination. This is the great failing of modern social science. So long as economic discourse is patriarchal discourse, feminist economists are not likely to find much interest in either classical or neoclassical economics. Callari is concerned that those who have studied the methods of historical scholarship in our discipline – and Callari’s list probably includes Schumpeter – are ignorant of the way patriarchal modes of discourse

divert attention from the reality of the situation in which women find themselves. With Callari's chapter we have passed far beyond the style and content of the history of economics that characterized Schumpeter's *HEA* to the politics of discourse itself.

Finally, in Chapter 18, we return to the broad problem of historical scholarship in Schumpeter's great book and how that effort interrelated with Schumpeter's contribution to economics and methodology. Professor Yuichi Shionoya, Japan's most distinguished Schumpeter expert and past president (1990) of the International Schumpeter Society, has provided us with an erudite and perhaps definitive account of Schumpeter's ideas about the sociology of science. According to Shionoya, "the sociology of science draws attention to the actual activity of science that is carried out in social surroundings and tries to clarify empirical and dynamic phenomena such as growth and decline, acceptance and rejection of specific sciences." Schumpeter's interest in sociology of science was related to his quest for a "universal social science."

Shionoya explores the tension in Schumpeter's writings between the pre-analytic vision and scientific explanation on the one hand and the clustering of economists into schools of thought and related sociological groupings on the other. These explorations lead Shionoya to the perennial problem of Schumpeterian research – that is, why Schumpeter praised Walras and Marx at the same time. Shionoya reviews the relevant literature and offers his own original take on the relationship between statics and dynamics in Schumpeter's theoretical system.

Shionoya ends his chapter with suggestive remarks about the important differences between John Maynard Keynes and Schumpeter. It is generally agreed among scholars that the public perception was that (by 1950) Keynes had scooped Schumpeter in popular attention as the world's most celebrated economist. Keynes's scientific contribution has only in recent years lost some of its luster. Shionoya points out that Schumpeter was concerned with economic development and not the Keynesian problem of short-term stability of the economy. Schumpeter considered capitalism an essentially stable system that does not require activist fiscal and monetary policies. Furthermore, by upholding the importance of saving and the entrepreneurial process, Schumpeter provided the "severest critique" that Keynesian economics ever faced.

Perhaps Schumpeter died before he had time to include his own contributions to analysis in the *HEA*. If he had been able to do so, we would have a better understanding of what constituted genuine analytic progress for Schumpeter and what constituted retrogression or decline. We shall not be able to settle these issues completely, and the debates over the meaning and significance of Schumpeter's economics will continue well into the twenty-first century. The essays in this volume are evidence that Schumpeter's great historical account of economic analysis remains a benchmark from which historians of economics of all stripes and interests, from finance and fiscal policy to feminism and philosophy of science, can still draw inspiration.

INTRODUCTION

NOTES

- 1 This book is part of a longstanding series published under the title *Perspectives on the History of Economic Thought*. The first eleven volumes in this series were published between 1987 and 1995 by Edward Elgar Publishing Limited, Gower House, Croft Road, Aldershot, Hants GU11 3HR, England, and are still in print. In 1995 Routledge agreed to take over the series; this book (volume 12) is the first to appear under the new publication arrangement.
- 2 Compare the Constitution of the History of Economics Society (rev. 19 June 1988), which was originally adopted at the first annual business meeting of the Society at Chapel Hill, North Carolina, on 29 May 1974. Requests for information concerning membership should be directed to the current secretary-treasurer, Professor John J. Bethune at Bellarmine College, 2001 Newburg Road, Louisville, KY 40205-0671.

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Part I

METHODS OF
SCHOLARSHIP

ASSESSING THE REPRINTING OF SCHUMPETER'S *HISTORY OF ECONOMIC ANALYSIS*

Mark Perlman

This is the fortieth anniversary of the original publication of Schumpeter's *History of Economic Analysis*.¹ Routledge, which holds the distribution rights ex-North America, has brought out a new printing and asked me to prepare an introduction for it.² To dispose of my introduction quickly, let me say that it has five parts. The first two summarize Schumpeter's place in the history of economic thought, and the place of the history of economic thought in Schumpeter's own work. The third summarizes in moderate detail the contents of the book. The fourth surveys the critical reviews given the book, principally the opinions of George Stigler, Frank H. Knight, I.M.D. Little, Lionel Robbins, Mark Blaug, Ronald Meek, and Jacob Viner. And in the fifth section, I give my own assessment, an assessment which takes account of what others have thought, but which goes somewhat beyond their reactions.

In that fifth section I speculate on two points. Virtually all of the other reviewers comment on the breadth of the vision Schumpeter wanted the book to contain. I address myself to that point and raise some questions about that vision. And that is the first point I wish to discuss here.

My second point goes to some matters of judgment. As almost everyone in the field of the history of thought knows well, Schumpeter reserved his greatest praise for Léon Walras. Robbins thought this judgment a major error, since Robbins thought that it was David Ricardo who deserved the laurel wreath. In my printed essay I have tried to explain that difference in evaluation in terms of Schumpeter rejecting the Utilitarian Creed which underlay (and underlies) most of Anglo-American economics. But, I went on to suggest that it was probably Vilfredo Pareto, rather than Walras, who truly deserved the laurel which Schumpeter intentionally awarded to Walras. It is my judgment, one flying somewhat in the face of the explicit assessment Schumpeter made in one of his last essays, the one on Pareto, which I shall try to explain in the second part of this chapter. As my two views, one relating to the vision and the other to the place of Pareto, are related, let us turn first to the easier, the one about the vision.

THE FLAW IN SCHUMPETER'S VISION

When I wrote that I thought that there was a flaw in Schumpeter's *History of Economic Analysis* vision, I certainly made it plain that the flaw was not fatal: the book remains as the outstanding achievement in the history of our field. Rather, Schumpeter, as Mark Blaug pointed out (Blaug 1962: 51), was unable to deliver the goods that he had promised. What set Schumpeter's dream above the others was the multiplicity and complexity of its parts. But, assuming that Schumpeter sought to offer a vision, how can his vision be judged? Hayek, in some senses a product of the same Viennese *Gymnasium*-mold which produced Schumpeter, offers the beginnings of an interesting comparison and ultimate criticism. Hayek came to embrace the complex paradigm of individualism–utilitarianism. Accordingly, had he written of Schumpeter's vision, he probably would have said (no doubt politely) that Schumpeter had it wrong.

But Hayek's enthusiasm for the individualism–utilitarianism paradigm, emphasizing in his economics the centrality of Bernard Mandeville, Adam Smith, J.S. Mill and personal liberty, brings to my mind the question of various possible alternative paradigms (Hayek 1978: 249–66). I mention but three: the centrality of scarcity, the centrality of uncertainty, and the centrality of essential (i.e. stable) moral imperatives (i.e. values).

As we have seen, Schumpeter rejected the paradigm of individualism–utilitarianism (and personal liberty). He did not seriously consider the paradigm of uncertainty. But, in the absence of any other specification, it seems to me he was groping for some paradigm of fundamental social morality. He was easily sidetracked, and spent too much effort decrying ideology (although he never decried theology).

Meek noted in his Marxian interpretation that prior to the classical tradition, economics dealt with social (by which I suspect he might have meant stable imperatives) issues like the relationship between workers and their lords (Meek 1962: 1ff). He went on to say that during the classical period that paradigmatic interest shifted away from an historically appropriate discussion about classes, people, and social organization to an historically inappropriate nexus between producers and goods. My suggestion is that the vision that Schumpeter really sought was one involving something akin to a theological paradigm – integrating fundamental, non-changing, ethical and social values and the dynamic workings of an evolutionary economy.

By fundamental human and social values I mean an absolute, true system which was exogenous to time and place. It was for this reason that so much of Schumpeter's interest focused on medieval writers and Natural Law, but his own remarriage after his divorce alienated him from the religion of his ancestors. Loran Allen asserts that while Schumpeter seemed to believe that *conventional* religious beliefs were for mortals lesser than he, he became increasingly mystical as he grew older – to the point of writing to and talking with his dead mother and his dead second wife (Allen 1991, 2: 199–200). My own assessment differs from

Allen's, who like many modern scientists offer their discussions and judgments of concepts of religion and religiosity on narrow, somewhat formalistic and institutionalized planes.

When he was a younger man he had thought that science could furnish answers covering all topics. By the time he had gotten to this work, he had less faith in science (note his bow to Hayek's crusade against Scientism), replacing it with an interest in historical sociology (Hayek 1964). My point is simply that his sense of vision, great by comparative standards, was nonetheless admittedly incomplete. On the one hand, there was from his religiosity a sense of timeless all-encompassing truth, which included but transcended science, for science was the name given to marvelous sets of analytical tools, when perfected timeless in nature, but certainly never as grand as the basic vision itself. On the other, there was historical sociology which gave some limited system to the bodies of material, including methods of exposition, relating to ever-changing societies.

I think that there was genius in Schumpeter's linkage of science and greater truth, but he knew of a flaw as well. He was aware that scientific advance in one area not only could be translated to work in other areas, but that in the process more was occasionally transferred than merely scientific method. The original area had its own *Gestalt*, and the transference often brought along pieces of that original *Gestalt*, which could be essentially alien to the new area. Isaac Newton, one of the inventors of the calculus, was a physicist interested in mechanics and therefore concerned with equilibrium. Economists, appreciating the potential of the calculus, often were unaware that they were applying a physics-derived technique to a sociobiological type of discipline, where the one important truth was not movement towards an equilibrium but constant mutation.

Thus, I conclude that Schumpeter wanted a vision which embraced and bound together the permanent and exogenous with the sociological-transitory and indigenous, and he failed to find it. Had he chosen to build on the American Institutionalists writers such as John R. Commons and Wesley C. Mitchell as exemplars of the sociological-transitory with their inability to find the timeless truth, he could have shown the dilemma from the non-"theoretical" side. Unlike many of the theorists of his time, Schumpeter expressed some, if limited, respect for what they were trying to do; but he did not go on to say what should have been said:

- (a) that they did not see beyond the Hobbes-Locke individualist-utilitarian paradigm; and
- (b) that their ignorance of Pareto's work on nonrational systems made their work far more barren theoretically than it should have been.

WHY DID SCHUMPETER OVERLOOK PARETO'S SOCIOLOGY?

Why he overlooked these Institutionalists is one thing, but why did he generally overlook the relevance of Pareto's sociology in the discussion of his own vision?

Here is another dilemma. What explains this neglect – particularly since Schumpeter was part of the Harvard seminar in the 1930s which focused somewhat productively on the English translation, *Mind and Society*, of the *Tratatto de Sociologia Generale*?³ Possibly, this problem haunted him, and, in one of the last and best of his essays, printed in 1949, he set out to evaluate Pareto, the man, his economics, and his theory, for what were clearly defensive, nonetheless competent purposes (Schumpeter 1965: 110–42).

Let me only note here that while Schumpeter chose to emphasize Walras and Pareto mostly as mathematical economists, each was also deeply steeped in historical, institutional, and even empirical detail. It is a tragedy that our students think of them as only abstractionists, but it is a tragedy easily prevented if they are told to look at the actual texts.

About Pareto the man, Schumpeter noted the obvious; how brilliant was Pareto's mind as well as how difficult was his personality. More than that, Schumpeter took great care to show how much Pareto suffered from being misunderstood by those who should have known better, and how poorly understood he was by those who while claiming him as their guru distorted what he meant to say. It comes across clearly that in Schumpeter's mind Pareto was a scientist *sans pareil*: unlike Walras, Pareto created a following, and unlike most leaders Pareto was ever conscious of his debt to Walras, whom apparently he disliked thoroughly.

The other point to make about Pareto, the man, was his passionate identification with Italy, a country whose political corruption then (like today) exasperated many who admired its creativity and cultural sophistication. Nonetheless, it is significant that Schumpeter, truly no more than a self-made quasi-aristocrat (some even allege something of a pseudo-aristocrat), judged Pareto to be a real aristocrat but one tarnished with a bourgeois reformist outlook. I wonder why Schumpeter included such *obiter dicta*.

As for Pareto, the economist, Schumpeter's assessment is mixed. On the one side Schumpeter lists many but far from all of Pareto's innumerable "firsts." Pareto's Law of Income Distribution was one of the first empirically discovered regularities known to economics, and although the interpretation of the Law varies, its existence involving a fascinating stability is clearly a Paretian first. In Schumpeter's words, "Pareto's 'Law' is pathbreaking in the literal sense even though in the end nothing whatever is left of its particular form" (Schumpeter 1965: 121).

And, Pareto's ideas about pricing in a socialist economy presaged Barone's famous paper. Schumpeter further identifies Pareto as the one who first drew, albeit awkwardly, the distinction between a "dynamics that studies successive equilibria and seems to me to denote comparative statics; and another dynamics that studies the *mouvement du phénomène économique* and seems to merge genuine dynamics with the problems of evolution" (Schumpeter 1965: 125).

Yet, for example, Schumpeter neglects Pareto's concept of *le sentier* ("the path"), a construct more sophisticated than *tâtonnement*. *Le sentier* is a

construct involving path-dependent solutions – solutions relating to explicit terminal prices as well as relative power within both economic markets and social bargains.

Schumpeter credits Pareto with being the architect of the now-accepted non-cardinal utility theory of value. Pareto's contribution was "to replace utility postulates by postulates about observable behavior and thus to base economic theory on what seemed to Pareto to be more secure foundations" (Schumpeter 1965: 129). But as an architect, Pareto was, in Schumpeter's ideas, no more than the sketcher of the design, not the draftsman. Nonetheless, it is these sets of designs which gave rise to the "New Welfare Economics."⁴

Schumpeter allows how Pareto's theory of production was another of his monumental firsts. It contained a comprehensive structure *lignes du plus grand profit, lignes de transformations, completes et incompletes* (Schumpeter 1965: 132). And although Pareto's system was presented with fixed coefficients of production, Pareto was aware of the desirability of introducing variations. But, according to Schumpeter, by that time Pareto was elderly and ill, and he indicated that others could handle those refinements.

I, myself, have found in Pareto's work on production many other insights, including as an example his empirical observation – what today General Motors Corporation may call "Lopes's Law" – that a small number of input-type widgets account for most costs, and if costs are to be meaningfully reduced these are the prime candidates for concentrated attention.

On the other hand, Schumpeter felt that Pareto's monetary theory was faulty, even more so than Walras's. I would have thought that given Schumpeter's own frustrations in this area this would have been more a point of personal identification than the point of criticism it seems to be.

It was with Pareto, the sociologist, that Schumpeter had his troubles. For one thing, Pareto introduced a set of constructs which Schumpeter found both alien and arcane. For another, Pareto himself believed that his economics led to his sociology and although Schumpeter may have taken a tour in that direction, he did not like the sights.

I suggest three reasons why Schumpeter, who obviously chose to give Pareto the highest marks in virtually all of his "papers", nonetheless never gave him the "Exhibition for His Career." First, Schumpeter had worked through all of the Walras available to him, including a great many commentaries; and, while Schumpeter was a member of the famous Harvard Seminar on Pareto, the quality of their criticisms, although high, did not match what was available on Walras. In short, in his own division of labor Schumpeter came on Pareto's sociologist critics too late. Second, as I have indicated earlier, Schumpeter was something of a believer, that is a Deist, and whatever can be said of Pareto and his sociology there was no place for that sort of thing at all.

And, finally, to have yielded to Pareto's domination went beyond the point where he was prepared to go. It is not for nothing that the most important part of *History of Economic Analysis* is the first, in which he pleads for a general social

science approach to the economics discipline. But, he was not prepared to make historical sociology the Queen.

There remain simply two points. Schumpeter's limited endorsement of Pareto was not, in my considered opinion, based on the uses that the Italian Fascists put his name to. Schumpeter's own personal history at Harvard in the late 1930s and particularly during World War II indicate that he was not afraid of damaging his own political reputation; that had already been accomplished (Allen 1991, 2:136–55).

Second, the real point of this essay is to suggest that the need is for seminars to do for Pareto's work what they had done for Walras's. It is a good thing to admit monumental achievement (and that we do with regard to the *History of Economic Analysis*), but it is a better thing to treat it as the cornerstone of a school. And that we haven't done. We should pick up the unfinished task.

NOTES

- 1 The original edition was published in 1954 under the title *History of Economic Analysis* and was based on the numerous pieces and sections of the book that Schumpeter's widow, Elizabeth Boody Schumpeter, was able to locate after Schumpeter's sudden death. It was published by Oxford University Press in New York and went through numerous reprintings.
- 2 Oxford University Press, which holds the North American distribution rights, currently plans to include the essay when its present stock is exhausted and a new reprint is made.
- 3 Other members of that Harvard Seminar included J. L. Henderson, Pitirim Alexandrovich Sorokin, and Talcott Parsons.
- 4 I should add that Schumpeter would have liked to give that architectural gold medal to his old friend Irving Fisher, but still did not do so. In two of his essays Schumpeter expresses amazement that Pareto actually voiced to him an admiration for Fisher.

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VISION AND PROGRESS IN ECONOMIC THOUGHT

Schumpeter after Kuhn

Roger E. Backhouse

SCHUMPETER'S PERSPECTIVE

Schumpeter's *History of Economic Analysis* (1954) is written from a distinctive perspective, outlined in Book I.¹ There is great emphasis on economics being a science, where science involves going beyond everyday explanations of economic phenomena. Many of the phrases Schumpeter uses to describe science reflect the influence of logical positivism, then developing into the dominant approach to the philosophy of science. Thus Schumpeter writes that the rules of "modern" or 'empirical' or 'positive' science . . . reduce the facts we are invited to accept on *scientific grounds* to the narrower category of 'facts verifiable by observation or experiment'; and they reduce the range of admissible methods to 'logical inference from verifiable facts'" (8). Such philosophy of science now seems somewhat dated. So too does Schumpeter's historiography. According to Schumpeter:

Economic analysis has not been shaped at any time by the philosophical opinions that economists happened to have . . . even those economists who held very definite philosophical views, such as Locke, Hume, Quesnay, and above all Marx, were *as a matter of fact* not influenced by them when doing their work of analysis.

(31–2)

It thus becomes possible for him to focus on the filiation of ideas. This perspective is given added significance when combined with Schumpeter's view that interdependence, seen in Walrasian terms, is the central economic problem.

[T]his all-pervading interdependence is the fundamental fact, the analysis of which is the chief source of the additions that the specifically scientific attitude has to make to the practical man's knowledge of economic phenomena; and that the most fundamental of all specifically scientific questions is the question whether analysis of that interdependence will yield relations sufficient to determine . . . all the prices and quantities of products

and productive services that constitute the economic “system”. . . . The discovery [of this fundamental problem] was not fully made until Walras, whose system of equations, defining (static) equilibrium in a system of interdependent quantities, is the Magna Carta of economic theory. . . . The history of economic analysis or, at any rate, of its ‘pure’ kernel, from Child to Walras might be written in terms of this conception’s gradual emergence into the light of consciousness.

(242)

Today such a perspective seems naive, both as philosophy of science and as historiography: we have learned much in the past forty years.

The main reason why Schumpeter’s perspective seems dated today is the influence of Kuhn’s *Structure of Scientific Revolutions* (1962). In the 1960s economists began to interpret the history of economic thought in terms of Kuhn’s categories of paradigms, normal science and scientific revolutions.² Progress was defined only within paradigms, not across them, which meant that unless one argued that the whole of economic thought from Child to Walras constituted a single paradigm, Schumpeter’s account must be flawed. Science could, after Kuhn, be understood only with reference to its sociology and its history. Though it is arguable exactly how far Kuhn himself went in this direction, the “rules” of scientific procedure were to be found not in philosophy but in scientists’ practices. Also important has been the influence of the Popperian school, notably Popper, Feyerabend, and Lakatos. Popper’s falsificationism makes it impossible to see scientific method as “logical inference from verifiable facts.”³ Feyerabend’s methodological anarchism has made fun of the notion that there are absolute standards in science, dispelling the air of confidence that pervades histories such as Schumpeter’s. Lakatos’s methodology of scientific research programs provided a framework, seemingly more rigorous than Kuhn’s, through which the history of economics could be interpreted, whilst his methodology of historical research programs, involving the idea that philosophy and history could inform each other through the method of rational reconstructions, provided a way to write philosophically informed history.

But how much have we learned? There is now considerable skepticism about the relevance of falsificationism to economics. It is hard to identify the components of Lakatosian research programs, and his method of rational reconstructions is seen by many as distorting history.⁴ Though it may nonetheless be important in altering our perspective (see Hausman 1994), Kuhn’s framework of paradigms and normal science does not take us very far in analyzing history.⁵ Insofar as it is possible to speak of a trend in methodological and historiographical thinking in the 1990s, it is probably an emphasis on what has been called “recovering practice” – away from some of the questions that dominated the subject in the 1970s and 1980s, the answers to which made Schumpeter’s position unacceptable.⁶

Given this trend, Schumpeter’s *History of Economic Analysis* needs to be