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Pioneers of Financial Economics: Das Neu Adam Smith Problem?

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1. Introduction

The steady decline in importance of the history of economic thought is well known (Cardosa 1995, Barber 1997, Blaug 2001). The subject has largely disappeared from the graduate economics curriculum. Important history of economic thought journals are excluded from citation indexes commonly used to measure scholarly output, threatening the future well being of scholars working in the area. Job prospects for junior scholars specializing in the history of economic thought are bleak. While the decline is evident, the causes are less clear. For example, Backhouse (2002) attributes the decline to dramatic institutional changes in the modern university while Blaug (2001) identifies "the philosophical overhang of positivism" and the "economics of economics" as reasons. Faced with a falling demand, the subject has experienced dramatic pressures for 'revisioning' from within. This has generated lively and sometimes acrimonious debate over whether the history of economics is part of the much wider 'history of ideas' or 'history of science' (Schabas 1992, Hands 2001). The detractors argue that the history of economic thought is most appropriately aligned with economics. As Blaug (2001) observes: "No history of ideas, please, we're economists". This paper establishes the relevance of the history of financial economics to this debate and suggests a 'histories of economic thought' strategy to improve the future prospects of the subject. In the process, *Das Neu Adam Smith Problem* is identified and discussed.

Das Adam Smith Problem is concerned with the consistency between the two major works of Adam Smith, *The Theory of Moral Sentiments* and the *Wealth of Nations* (*WN)*.[1] The *Problem* epitomizes the central importance of Adam Smith in classical political economy. This importance is amplified by the hallowed role that classical political economy has in contemporary study of the

history of economic thought. In contrast, *Das Neu Adam Smith Problem* is concerned with substantively different questions such as: why do Adam Smith and other classical political economists continue to play such a central role in contemporary history of economic thought when the relevance of these thinkers to modern economics is so limited? In a use-oriented academic world, what is the 'use' in stressing the role of Adam Smith, David Ricardo or J.S. Mill in the history of economic thought when these thinkers have limited relevance to modern economics? Building on Poitras (2000), volume 1 of the *Pioneers of Financial Economics* (*Pioneers)* project illustrates how a relevant, interesting and useful classical history of financial economics can be developed that is largely independent of the traditional approach to the history of economic thought which exaggerates the role of Adam Smith as the leading Enlightenment thinker on issues relevant to political economy.[2]

The *Pioneers* project shares features in common with other recent histories of economic thought. For example, the history of econometric thought has been captured by Epstein (1987) and Morgan (1990). This exercise was facilitated by the time line of this history having almost all primary sources appearing in the 20th century. Subjects with longer pedigrees, such as microeconomics, have experienced a reworking of the classical time line used in identifying contributions, e.g., Ekelund and Hebert (1999). While any plausible history of microeconomic thought has to include important neoclassicals, such as Walras and Edgeworth, that are central to the time line of mainstream history of economic analysis, this is not the case with financial economics. As a consequence, the *Pioneers* project required the detailing of a history that was largely unwritten, with many avenues still unexplored. This paper provides a blueprint of how this history of financial economics was constructed. Identification of seminal contributions to the history by the likes of Edmond Halley and

Abraham de Moivre demonstrate that, even in a history of economic thought for which the time line of the classical political economists is applicable: Adam Smith has little to do with this history, Malthus, Ricardo and Marshall even less.

2. Das Neu Adam Smith Problem?

Blaug (2001) is confounded by the decline in importance of the history of economic thought within the profession, "disparaged as a type of antiquarianism", happening at the same time that there is an increasing popularity of the subject in the scholarly community. Can this discrepancy be attributed to a confusion over the difference between the (relevant) subject that is increasing in popularity and the (antiquarian) subject that is disparaged? The reverence to the contributions of the classical political economists is unmistakable in Blaug (2001) as it is in the voluminous research effort still being dedicated to Adam Smith and other classical political economists.[3] For example, in Blaug (2001) numerous examples are given of how modern economists, such as Lucas (1996), misinterpreted Adam Smith or David Hume. The modern relevance of such 'misinterpretation' is illusive. Even "The Ultimate Justification for Studying the History of Economic Thought" given by Blaug (p.156) involves using Coase's theorem as an example; classical political economy is noticeably absent. Blaug (p.157) concludes: "History of economic thought is not a specialization within economics. It *is* economics – sliced vertically against the horizontal axis of time". From this perspective, *Das Neu Adam Smith Problem* asks the question: where in modern economics does Adam Smith have any relevance? Why is so much emphasis in the history of economic thought put on the progression from the classical political economy of Adam Smith and Ricardo through J.S. Mill up to Alfred Marshall?

As volume 1 of the *Pioneers* project illustrates, slicing vertically into the time line of financial

economics reveals substantively different results as to the importance of the individuals involved in the classical history of economic thought.[4] Similar to the criticisms of Smith by the German Historical School that engendered *Das Adam Smith Problem*, closer inspection of the history of financial economics reveals flaws in the traditional interpretations of Smith's position as "*the* towering figure of Enlightenment political economy". This is an important part of the story underlying the *Pioneers*, volume 1. That Smith was revered within the British upper classes and inspired subsequent writers of the texts and pamphlet literature that comprised classical political economy is undeniable. In the century that followed *WN*, it is difficult to find credible primary sources in classical political economy that do not commence with a glowing tribute to the contributions of Adam Smith. Such is the accepted interpretation given by those that have written *the* history of classical political economy. Yet, from the perspective of classical financial economics, there is a much different history that does not include the classical political economists.[5]

To be relevant, history of economic thought needs to resonate with contemporary economists, speak to their interests. Concepts basic to classical political economy, such as the labor theory of value, are far removed from the landscape of modern financial economics where more immediate problems, such as determining the price of a financial security, are a central concern. Confronted with the difficulties of pricing the complicated contingent financial claims that are traded in modern securities markets, a history of the methods employed to solve such problems in the past is useful and relevant to modern financial economists.[6] Raymond de Roover (1904-72) who made an immense contribution to the history of early financial economics (see Kirshner 1974) was close to the mark in saying the history of economic thought was about "the genesis of ideas and the evolution of methods." Too often, the contemporary history of economic thought is consumed with notions,

such as ontology and rational reconstruction, that hold little interest for modern economists. To paraphase Blaug (2001): "Less moral philosophy and epistemology, please, we're economists"[7]

3. The Search for Pioneers of Financial Economics

The starting point for the history of economic analysis, the beginning of classical political economy, is invariably identified with Adam Smith (1723-1790). The *Wealth of Nations* has been justly recognized as the first book to develop a comprehensive and cohesive body of theory that was substantively distinct from other related subjects. Contributors prior to Smith are typically categorized as 'precursors of Adam Smith' or 'pre-Smithian economists' or 'pre-classical economists'. Yet, as demonstrated in (Poitras (2000), Hald (1990) and other sources, in key areas of relevance to financial economics, there was an impressive and substantive body of knowledge that had been developed well prior to Smith. Some of this knowledge, such as the early mercantilist writings on foreign exchange markets, was discredited by Smith. Other areas, such as the pricing of aleatory contracts, are not examined by Smith. Still other elements of interest to financial economics, such as joint stocks, sinking funds and methods of government financing, receive only cursory coverage in the *Wealth of Nations*.

The roots of financial economics stretch back to antiquity. The valuation of financial transactions, such as determining payment on a loan or distributing profits from a partnership, is so ancient that any search for pioneers would be fruitless. A less ambitious beginning is required. Poitras (2000) uses the late fifteenth century as a starting point for the early history. By this time, elements of financial economics were being widely disseminated among the merchant classes in the commercial arithmetic that formed the core of the reckoning school curriculum. From this point, until the appearance of the *Wealth of Nations*, the founding work of classical political economy, financial

economics underwent a dramatic transformation. By the latter half of the 18th century, sophisticated methods for pricing contingent claims, such as the life annuities sold by various individuals, municipalities and national governments in western Europe, had been developed and applied to the establishment of life insurance and pension funds. Absent a watershed historical figure sufficient to provide a well-defined beginning for financial economics, this book selects the earliest pioneers from the contributors that developed these pricing methods.

The search for 'pioneers of financial economics' places the origins of financial economics well outside the conventional boundaries of the history of economic thought. These differences extend to the philosophical foundations underlying the various subjects that have contributed to financial economics. While there are roots that can be traced as far back as the commercial arithmetic taught in the Renaissance reckoning schools, the identification of specific pioneers requires more substantive contributions.[8] Prior to the Enlightenment, many essential problems in financial economics posed ethical and theological, as well as analytical, difficulties. For example, determining the fair price of a sequence of future cash flows involved concepts of interest, that were affected by usury considerations. Other examples involved setting a price for risk or determining fair compensation for a contingent claim, both of which involved the ethics of gambling. All this complicates the task of constructing an early history of financial economics and identifying the pioneers.

Of the possible starting dates, this volume selects the second half of the17th century. While this starting date could be moved back some centuries to facilitate the inclusion of figures such as Fra Luca Pacioli, Nicholas Chuquet, Simon Stevin, Thomas Gresham and Gerard de Malynes (Poitras 2000), the latter half of the 17th century marks the the emergence of notions of probability that form

the basis of modern contingent claims analysis. Following on the development of the concept of expected value by Christian Huygens (1620-1699), in 1671 the Dutchman Jan de Witt (1625-1672) provided the first actuarially sound solution for the value of a life annuity. In addition to intellectual contributions, the 17th century also featured significant institutional developments. In particular, there is the emergence of trading in joint stocks, commencing with trading of the shares in the Dutch East India Company in 1602 in Amsterdam. As reflected in Joseph de la Vega's *Confusion de confusiones* (1688), securities markets had achieved a remarkable level of development by the end of the 17th century.

4. Contents of Volume 1

Volume 1 of the *Pioneers* (*Contributions prior to Irving Fisher*) has eleven chapters collected into four parts. The three chapters contained in Part I deal with the historical and philosophical background for the early history of financial economics. Three 17th century historical figures are featured: Jacob Bernoulli (1654-1705), Isaac Le Maire (1559-1624) and Joseph de la Vega (1650?-1692). In chapter 1, Sylla observes about Jacob Bernoulli: "Like Moses, Bernoulli had seen the promised land – financial economics, in this case – but he did not live to enter it." Bernoulli's contributions to mathematics and probability theory serve as a useful demarcation point between the intellectual tradition constrained by scholastic and humanist considerations, and the modern insights generated during the Enlightenment. Sylla admirably examines the roots of probability theory contained in Bernoulli's posthumously published *Ars Conjectandi* (1713) stretching the time line back to Aristotelean ethics. The foundations for Christian Huygens' approach to mathematical expectation are grounded in Renaissance commercial arithmetic and issues of fairness and justice. While Bernoulli was writing around the turn of the 18th century, significant earlier contributions to

financial economics were unfamiliar to him. In particular, though he was aware of the work of Jan de Witt on life annuity valuation, he was not able to obtain copies of de Witt's contribution. Halley's work on life annuities was unknown to him. As such, Jacob Bernoulli is a more than fitting individual to lay the intellectual foundation for the contributions of the early pioneers of financial economics.

Chapter 2 examines the institutional origin of trading in common stocks. This trade can be traced to the transactions in shares of the Dutch East India Company on the Amsterdam Exchange that began with the founding of the company in 1602. There is an important secondary source for this early trading: van Dillen (1930), which is written in Dutch and also includes numerous primary documents, such as the text of legal presentations concerning the valuation of VOC shares; van Dillen (1935), in French, summarizes much of the material in van Dillen (1930). While providing considerable detail about early stock trading, the discussion in these two sources revolves around the activities of Isaac Le Maire, the first of a long line of stock operators that stretches into modern times. After an introductory overview section, Chapter 2 provides an English translation of van Dillen (1935). In addition to providing details of Le Maire's role in the first stock market manipulation, the chapter also contains a wealth of detail about the workings of early securities markets and the characters that populated those markets.

By the end of the 17th century, stock trading on the Amsterdam exchange had reached a strikingly modern state of development. Much of what is known about these activities is gleaned from Joseph de la Vega's descriptive insights about the Amsterdam bourse in the 1680s contained in the *Confusion de confusiones* (1688). Chapter 3 provides an insightful examination of this source by José Luis Cardoso. Only Thomas Mortimer in the 18th century is close to the standard of descriptive

analysis of the early securities markets set by de la Vega in *Confusion*. While a careful examination of the primary source fairly indicates that Vega did not fully understand the details of certain transactions that were being conducted, such as arbitrage transactions involving derivative securities, Vega does demonstrate an active appreciation of the trading process, as well as excellent intuition about the impact of specific fundamentals on the value of joint stocks. Cardoso performs the invaluable task of examining the original of this rare primary source that is only currently available in English in an abridged form.[9]

Part II of volume 1 details the contributions of a select group of important 18th century pioneers of financial economics. While a number of noteworthy 18th century pioneers are not included – such as Richard Price (1723-1791) and Thomas Mortimer (1730-1810) – the pioneers that are included definitely deserve the recognition. Chapter 4 details the contributions of those pioneers that developed the valuation models for pricing life annuities. After reviewing the 1671 contribution of Jan de Witt and assessing the connection to the results of Nicholas Bernoulli (1695-1726), the solution for the single life annuity value given by Edmond Halley (1656-1742) is examined. Following Hald (1990) and Poitras (2000, ch.6) de Witt deserves recognition for making the seminal contribution of using discounted expected value to price a contingent claim, in this case a life annuity issued by the Dutch government. However, this result was developed in a public policy context and was not widely distributed. Halley provides a more rigorous theoretical solution to the valuation problem, as well as solving for annuity prices using an empirically determined life table. Halley also provides an insightful geometric solution for the value of a joint life annuity for three lives. This chapter also considers the most complete statement of the analytical solution for pricing various types of joint life annuities by Abraham de Moivre (1667-1754) and Thomas Simpson (1710-1761). As

a pioneer of financial economics, de Moivre was the first to substantively develop important mathematical techniques, such as series solutions, to the pricing of complicated fixed income securities.

Certain pioneers in the early history are important, not so much for a contribution to the theory of financial economics, but, rather, for a contribution to the history of financial markets. One such figure is John Law (1671-1729), the subject of Chapter 5. This chapter is contributed by Antoin Murphy, who is not only the leading authority on John Law and the Mississippi scheme but also on Richard Cantillon ((16??-1734) another important figure of the period. Murphy (1986) on Cantillon and Murphy (1997) on John Law are outstanding books that are required reading for all those interested in the history of financial economics. Chapter 5 provides a brief overview of ideas contained in Murphy (1997). While the conventional history of economic thought recognizes John Law for his contributions to monetary economics, Law's importance as a pioneer of financial economics stems from his prime mover status in the Mississippi scheme. Whatever the misguided theories that underpinned the scheme, this event qualifies as one of the most remarkable in recorded financial history. A Scotsman, a fugitive from British justice, was able to assume control over the financial policies of the French government, perpetrating one of the most fantastic 'schemes' of all time. The scheme itself produced a number of novel financial innovations, including the use of options to facilitate debt management objectives.

Not all the pioneers selected in volume 1 have the historical stature of Edmond Halley or John Law. Intellectual history is replete with examples of individuals making contributions that were seminal in content but, by and large, did not receive the corresponding recognition and attention because there was no scientific community at the time to accept and develop the idea. Financial

economics is no exception. Based on a translation of Biondi (2003, in French), Chapter 6 examines such an individual, Emmanuel-Etienne Duvillard (1755-1832). Historically, Duvillard's *Recherches sur les Rentes* (1787) was concerned with the construction of a sinking fund for the large outstanding amount of life annuities issued by the French government. While the contributions of Duvillard have received some modern attention, this chapter illustrates that modern sources such as Baumol and Goldfeld (1968) contain considerable confusion about the substance of Duvillard's contribution. As Biondi demonstrates, Duvillard made two pioneering contributions to the development of financial economics: the use of calculus to solve for an optimal lending period; and, identification of the modified internal rate of return where variations in the reinvestment rate over time are incorporated into the analysis.

Compared with studies on the contributions to financial economics made during the 18th century, relatively little has been written about the 19th century. Only recently has it been established that notions central to modern financial economics – such as the random walk hypothesis and the associated 'science of the stock market' – can be traced to the latter half of the 19th century when French writers such as Jules Regnault (1834-1894) and Henri Lefèvre (1827-1885) extended the positivist program of Auguste Comte (1798-1857) to financial markets. Part III of volume 1 details these contributions. In chapter 7, Alex Preda examines the historical and intellectual developments that laid the foundation for the modern theory of efficient markets. Written from the perspective of a sociologist of science, Preda provides an insightful examination of the cognitive and cultural background represented by popular efforts to transform financial investing into a science. This involved altering the public perception that financial securities were investments rather than gambling . The distinction between vernacular and academic economics that is used to motivate the

discussion is as applicable to modern financial economics as to developments in the 19th century, presenting a fascinating area for future research.

In chapters 8 and 9, Franck Jovanovic provides two path-breaking studies that recognize the seminal contributions to the 19th century 'science of financial investments' made by Henri Lefèvre and Jules Regnault. The remarkable amount of attention given to the 1900 thesis of Louis Bachelier (1870-1946) by modern writers has presented a quandary to historians of financial economics. While Bachelier's thesis has been hailed as a path breaking contribution by modern pundits, the thesis was not as well received at the time it was initially presented. Modern writers have attributed this to Bachelier being too far ahead of his time. These two studies by Jovanovic demonstrate that this judgment is somewhat misplaced. Not only did Bachelier borrow the option expiration diagrams used in the thesis from Lefèvre without attribution, the notion of applying stochastic process theory to price differences to solve for option prices was a central element in Regnault's *Calcul des chances et philosophie de la bourse* (1863). In this light, instead of being a masterpiece of intellectual history that was far ahead of its time, Bachelier's thesis appears to be better viewed as part of a continuum of academic contributions.

Part IV of volume 1 continues with the themes in Part III, moving the time frame into the early 20th century. Given the remarkable reverence accorded to Louis Bachelier by modern pundits, a volume on pioneers of financial economics prior to Irving Fisher would be woefully incomplete without a detailed and scholarly examination of the work of this pioneer. In chapter 10, this task is admirably accomplished by the team of Robert Dimand and Hichem Ben-El-Mechaiekh. The argument advanced in chapters 8 and 9 is that, in areas of relevance to financial economics, the modern view of Bachelier as a path breaking pioneer needs adjustment. Bachelier was building on the foundation

of scientific financial economics constructed by the likes of Regnault and Lefèvre. While chapter 10 does lay out the evidence for the modern position supporting the importance of Bachelier, the presentation is even-handed and well reasoned. Numerous modern accolades about Bachelier are provided recognizing, not only contributions to option pricing theory and speculation, but to the theory of diffusion processes and, in particular, Brownian motion. It is demonstrated that the history of Bachelier's academic career has to recognize the views of, among others, Poincaré, Lévy, Kolmogorov and Keynes. Even if there is debate about the extent of Bachelier's pioneering role in financial economics, chapter 10 demonstrates that Bachelier definitely was a pioneer in the realm of probability theory and stochastic processes.

Bachelier rose to current prominence in modern financial economics primarily from the connection to both option pricing theory and the efficient markets hypothesis. Chapter 9 on Regnault brings the 'originality' of Bachelier's contribution to the efficient markets hypothesis into question. In a seminal contribution detailing the fascinating and insightful work on option pricing by Vincenz Bronzin that appeared in 1908, Heinz Zimmerman and Wolfgang Hafner raise serious questions about the claim to primacy for Bachelier in the area of option pricing. As observed in chapter 10, the 'rediscovery' of Bachelier by Leonard Savage and Paul Samuelson in the mid-1950's was somewhat haphazard. Chapter 11 reveals that a more systematic examination of the historical record has revealed Bachelier 'was not the only one who was working successfully on option pricing at the beginning of the 20th century'. Working in German, it is reasonably certain that Bronzin developed his results on option pricing independently of Bachelier. In addition, based on the analysis in chapter 11, it appears that Bronzin produced a more rigorous treatment of the subject. While Bachelier produced his doctoral thesis in 1900, a more developed treatment of the subject by a professor and

actuary carries considerably more academic weight. Based on the research in chapters 9 and 11, it seems fair to conclude that the modern reverence accorded Bachelier for his seminal role in financial economics needs some reconsideration.

5. The Development of Modern Financial Economics

The history associated with voluem 2 of the *Pioneers* (*Twentieth Century Contributions*) posed a decidedly different set of difficulties than for volume 1. Without sufficient separation of the past from the present, it is difficult to make an accurate assessment of the historical importance of a particular scientific movement. In the spirit of Hands (1997), a sociology of knowledge perspective was adopted to assess the common threads that contributed to the success of modern financial economics as a scientific movement. Following Fickel and Gross (2005, p.209), these threads include "high-status intellectual actors harboring complaints against what they understand to be the central intellectual tendencies of the day". In the case of modern financial economics, this thread can be illustrated by the Modigliani-Miller contributions which launched an assault on essential components of "the traditional approach to Finance" (Weston 1967, p.539). As Merton (1987) observes:

> The Modigliani-Miller work stands as the watershed between 'old finance', an essentially loose connection of beliefs based on accounting practices, rules of thumb and anecdotes, and modern financial economics, with its rigorous mathematical theories and carefully documented empirical studies.

Employing theoretical arbitrage arguments, the Modigliani-Miller 'irrelevance theorems' (MM) were a direct challenge to the relevance of key elements of the 'old finance' – firm capital structure (Modigliani and Miller 1958, 1963) and dividend policy (Miller and Modigliani 1961).

The attack by MM did not go unchallenged by leading figures of old finance, such as David Durand

(e.g., Durand 1959). However, it did not matter that the criticisms being launched were crude and that old finance did have a tradition of 'rigorous mathematical theories', as evidenced by Durand (1957), and carefully documented empirical studies, as evidenced by Macaulay (1938). The opposition was too difficult to handle. In addition to Franco Modigliani, the 1985 Nobel laureate in Economics, the list of high status individuals making early contributions to modern financial economics included Paul Samuelson (1970 Nobel laureate), James Lorie (founder of the Center for Research in Security Prices in 1960), Harry V. Roberts and Paul Cootner. It did not help that the bulk of adherents to the old finance were technically incapable of understanding the arguments that the "mathematical-theoretical analysts" (Sauvain 1967, p.541) were advancing. Instead of aiming to advance and absorb the "New Finance", adherents of the old finance approach fought rear-guard actions that engendered heated debate at important professional venues, such as the American Finance Association (AFA) meetings in 1966.

Another thread in the emergence of a scientific movement involves "the movement's capacity to frame its intellectual message in a way that resonates with potential recruits". This resonance was provided by the attractiveness of the intellectual agenda of modern financial economics. Writing on the fiftieth anniversary of the publication of Markowitz (1952), Rubinstein (2002, p.1041) makes the metaphorical claim:

> This year marks the fiftieth anniversary of the publication of Harry Markowitz's landmark paper, "Portfolio Selection" ... With the hindsight of many years, we can see that this was the moment of the birth of modern financial economics. Although the baby had a healthy delivery, it had to grow into its teenage years before a hint of its full promise became apparent.

With some qualifications, the claim that Markowitz (1952) marks the beginning of the scientific movement associated with modern financial economics is generally accepted within that community

of scholars (e.g., Haugen 1999, p.11; Markowitz 1999; Varian 1993). However, as Bernstein (1992) observes, the uptake of the Markowitz portfolio optimization model was slow, with little recognition of the contribution until after the storm of controversy that accompanied the appearance of the Modigliani and Miller irrelevance theorems.

The decade of the 1960's marks the emergence and ascendancy of modern financial economics. The contributions were numerous and substantive. Though detailed empirical observations about the random character of security prices stretch back to the 19th century, Cootner (1964), Samuelson (1965) and Fama (1965) crystallized these notions into the basis of the 'efficient markets hypothesis'(EMH). Being based on "carefully documented empirical studies" the EMH was a direct and devastating attack on key practical elements of the old finance: security analysis and technical analysis. While earlier studies that appeared in the Cootner (1964) volume were primarily concerned with the time series behavior of security prices, the Fama (1970) review article illustrates that, by the end of the decade, substantial progress had been made in the scope and depth of the EMH. The connection between 'security prices fully reflecting available information' and martingale behavior for security prices had been developed, laying the foundation for a future connection between the equivalent martingale measure and absence of arbitrage in security prices. Fama, Fisher, Jensen and Roll (1969) had proposed a statistical methodology that was applicable to testing of the 'semi-strong form' version of the EMH, solidifying the empirical case against the strongest pillar of the old finance – security analysis.

The EMH was a crucial building block for modern financial economics. If markets are efficient then techniques for selecting individual securities will not generate abnormal returns. In such a world, the best strategy for a rational, expected utility maximizing individual is to optimally

diversify. Achieving the highest level of expected return for a given level of risk involves eliminating firm specific risk by combining securities into optimal portfolios. Building on Markowitz (1952, 1959), Sharpe (1963, 1964) and Lintner (1965a,b) made key theoretical contributions on the capital asset pricing model and the single factor model. A new definition of risk is provided. It is not the total variance of a security return that determines the expected return. Rather, only the systematic risk – that portion of total variance that cannot be diversified away – will be rewarded with expected return. An *ex ante* measure of systematic risk – the beta of a security – is proposed and the single factor model used to motivate empirical estimation of this parameter. Contributions on the inherent difficulties in determining empirical estimates and important techniques aimed at providing such estimates by leading figures of modern financial economics such as M. Miller, M. Scholes and F. Black are contained in the edited collection of Jensen (1972)

The combination of these three essential elements – the EMH, the Markowitz mean-variance portfolio optimization model and the CAPM – constitute the core elements of intellectual progress on modern portfolio theory (MPT) during the 1960's. Taking contributions to Jensen (1972) as the beginning of the improvement and refinement stage of MPT, this intellectual exercise would have been more than sufficient to propel modern financial economics into the intellectual mainstream of Economics. What is quite remarkable is that, just as a decade of improvement and refinement of MPT was about to commence, another kernel of insight contained in Cootner (1964) came to fruition with the appearance of Black and Scholes (1973). Though the influential Samuelson (1965) was missing from the edited volume, Cootner (1964) did provide, along with other studies of option pricing, a English translation of Bachelier's 1900 thesis and a chapter by Sprenkle (1964). The Sprenkle chapter points back to Sprenkle (1961) where the partial differential equation based solution

procedure employed by Black and Scholes was initially presented. While the development of an empirical pricing formula for options was quite remarkable, as Jarrow (1999) observes, this was "just the proverbial tip of the iceberg". Black and Scholes (1973) marks the beginning of another scientific movement -- concerned with contingent claims pricing -- that was to be larger in practical impact and substantially deeper in analytical complexity. Because this contingent claims pricing movement involved various individuals that had achieved high academic status from contributions made to MPT, it was natural for this new scientific movement to come under the academic umbrella of modern financial economics.

Compared to the readily discernable history of the contingent claims pricing movement detailed in volume 1, the intellectual roots of MPT are less certain. Though anecdotal references to the use of covariance to control risk in Shakespeare's *Merchant of Venice* (1600) are appealing (Rubinstein 2002; Markowitz 1999; Poitras 2000) option pricing can also claim anecdotal roots stretching back to Aristotle's discussion about Thales in the *Politics.* Such brief and undeveloped anecdotes cannot be considered as more than historical curiosities. Daniel Bernoulli's introduction of log utility to resolve the St. Petersburg paradox is a possible beginning for the intellectual history of MPT, but this only captures the expected utility slice of the theory. In the 20th century, elements of MPT, such as the tradeoff between risk and return and the "caution coefficient" were advanced by Irving Fisher (Poitras 2005, sec.2.4). However, as Markowitz (1999) observes:

> What was lacking prior to 1952 was an adequate *theory* of investment that covered the effects of diversification where risks are correlated, distinguished between efficient and inefficient portfolios, and analyzed risk-return trade-offs on the portfolio as a whole.

As such, the reliance of MPT on "rigorous mathematical theories and carefully documented empirical studies" places this part of modern financial economics within the intellectual transformation that

took over the whole field of economics in the post WWII period.

6. Contents of volume 2

With this background, a number of essential features of financial economics can be discerned. The subject is currently dominated by a scientific movement – modern financial economics – that rose to ascendancy in the post-WWII period by supplanting the traditional finance school that was closely associated with the institutionalist school in economics. The core values of this movement – mathematical rigor and empirical verification – were shared with similar developments taking place in other parts of economics. Modern financial economics has two central components. One component is the MPT which is associated with Markowitz, Sharpe, Miller and Fama. The MPT shares many features in common with mainstream economic theory and has generally failed to achieve widespread success with financial market practitioners. The other component is the contingent claims stream associated with Black, Scholes and Merton.[10] The classical history of this stream has as close an affinity with actuarial science as with economics. Contributions from this stream have been highly successful in the practitioner realm, being largely responsible for spawning the financial engineering industry.

Volume 2 of the *Pioneers* has thirteen chapters divided into three parts. Each part is substantively different from another. Part I continues to explore the historical time line set out in volume 1. Unlike the chapters in Parts II and III, the first two chapters in this part could easily have appeared in volume 1. The lead chapter, "A Brief History of Yield Approximation" by Gabriel Hawawini and Ashok Vora, provides an insightful connection between the historical time lines of volume 1 and volume 2. A central claim of the promoters of modern financial economics was that 'old finance' was "an essentially loose connection of beliefs based on accounting practices, rules of thumb and

anecdotes". This chapter demonstrates that the 'rules of thumb' used in the old finance for calculating the yield to maturity were mathematically rigorous and have a long history in finance. The search for a method of solving the yield in compound interest problems stimulated work by Tartaglia in the 16th century on the solution of algebraic equations. Important figures from mathematics, actuarial science and statistics, such as Augustus de Morgan (1806-1871), made substantive contributions to the solution of the annuity version of this problem. While the widespread availability of calculators and desktop computers has eliminated the need to consider the analytical solution of yield to maturity problems in a classroom setting, this chapter is a delightful reminder of the importance of this topic to the intellectual history of finance.

One of the oddities of modern financial economics has been the success of this movement in securing the academic high ground in the Finance curriculum of business schools despite being relatively sterile in practical applications. As professional schools, issues relevant to 'real-world' financial managers are of central concern in the classroom. This relevance was, at least partly, achieved by absorbing and adapting core material from old finance relevant for teaching financial management. Yet, in claiming a seminal role for figures from the modern financial economics movement, such as Markowitz and Sharpe, the intellectual history of the financial management stream was displaced and ignored. In chapter 2, Morgen Witzel performs the masterful task of resurrecting contributions to financial management from the period from 1880 to 1917 by the likes of Jeremiah Jenks, Edward Meade and William Ripley. During this period American financial management experienced "far-reaching and revolutionary changes". Contributions to financial management from this time period shaped many of the processes through which business is conducted and the ways in which management is conceptualized. Chapter 2 demonstrates that

modern financial management in the United States took on much of its present form during this period.

Much as volume 1 would have been incomplete without a detailed examination of the contribution of Louis Bachelier, volume 2 would be incomplete without a contribution dealing with Irving Fisher. The importance of Fisher to the intellectual history of financial economics is reflected in the subtitle to volume 1 of the *Pioneers*: *Contributions Prior to Irving Fisher.* In so many aspects of economics during the 20th century – from macroeconomics to capital theory to index numbers to monetary economics to the theory of interest – Fisher looms as a seminal contributor. As chapter 3 demonstrates, the same recognition is required in the realm of financial economics. If the beginning of modern financial economics is associated with the introduction of "rigorous mathematical theories and carefully documented empirical studies" then a strong case can be made for starting the time line with Irving Fisher. As in volume 1, the task of dealing with the contribution of Irving Fisher to financial economics – a task that is essential to the success of volume 2 – is admirably executed by Robert Dimand, arguably the leading modern authority on Fisher. While Fisher is often remembered for his gross miscalculation about the collapse of stock prices at the end of the 1920's, chapter 3 demonstrates there is much more to this story.

The final chapter of Part I deals with two seminal figures in fixed income analysis: Frederick Macaulay and Frank Redington. Macaulay is recognized for introduction of the duration concept and Redington for 'discovering' classical immunization theory. Even though fixed income analysis is an essential component of financial economics, and the duration and immunization concepts were path-breaking contributions to that subject, neither individual received recognition until decades after the contribution. Macaulay had died before the eponym 'Macaulay duration' was introduced and,

though revered within actuarial science, Redington is still a relative unknown within financial economics. In addition to detailing the development of the notions of duration and immunization, chapter 4 also explores the issues surrounding why, even though these contributions satisfy the requirements of 'mathematical rigor' and 'careful empirical study', neither received timely and adequate recognition within modern financial economics. The reasons advanced in chapter 4 for this lack of recognition provide strong evidence for the sociological interpretation of scientific movements and knowledge.

Intellectual prestige plays a key role in the success of scientific movements. As Fickel and Gross (2005, p.215) observe, scientific movements "that offer participants ways to secure additional prestige above and beyond that which they currently possess, to maintain prestige when it is threatened, or to regain lost prestige have a greater likelihood of success than those that do not". Prestige has played a key role in modern financial economics becoming and continuing to be a successful scientific movement. A key objective of Part II is to explore the process of prestige creation and reinforcement in modern financial economics. From a sociological perspective, this process involves 'mobilizing structures'. As organizations that "embody and and perpetuate disciplines" (Kohler 1982, p.8), university departments are "central to the reproduction of expert labor and as institutional carriers of disciplinary authority, identity and culture" (Fickel and Gross 2005, p.218). In addition to university departments, the mobilizing structures of scientific movements include scholarly organizations, communications networks – especially scholarly journals – and institutional networks. Mobilizing structures play a central role in the success of a scientific movement and prestige is the 'glue-that-binds' these structures together.

Scholars located in lower-status positions within mobilizing structures are essential for the success

of a scientific movement. These scholars carry out supportive and confirmatory 'scientific' investigations, conferring status and prestige on the intellectual leaders of the movement. These lower-status scholars function as organizational leaders: serving as editors of journals; organizing conferences and colloquia; serving on departmental committees that recruit new members for the scientific movement and so on. Within a successful movement such as modern financial economics, there is a widespread hierarchy of such lower-status individuals. Mobilizing structures can be and are used to determine status and prestige within the movement. The ranking processes are well known. There is the perceived status of the university and the department, the 'quality' and 'impact' of scholarly journals, service on the editorial boards of those journals and the boards of the scientific associations affiliated with the journals, funding provided by granting agencies, citations by other scholars in the field, and awards and distinctions granted by scholarly bodies. The pinnacle of the prestige hierarchy within modern financial economics, and economics in general, is being selected for a Nobel memorial prize in economic sciences or, more formally: The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel.

To date, five individuals – Harry M. Markowitz, William F. Sharpe, Merton H. Miller, Robert C. Merton and Myron S. Scholes – have received the 'Nobel prize' distinction for contributions solely in the realm of modern financial economics. Markowitz, Sharpe and Miller were joint winners in 1990 with Merton and Scholes receiving the award jointly in 1997. The contributions of Fischer Black (1938-1995) were explicitly recognized in making the award to Merton and Scholes but, as the prize cannot be awarded posthumously, Black was not a named recipient. Four others Nobel prize winners – Paul A. Samuelson (1970), John R. Hicks (1972), Franco Modigliani (1985) and Daniel Kahneman (2002) – made significant contributions to financial economics but were awarded

the prize for an overall impact that covers a wider range of the economic sciences. The award provides an opportunity for high status individuals within the modern financial economics movement to reinforce the prestige of the award by writing congratulatory articles in highly ranked scholarly journals summarizing the important scientific contributions of the award recipient. Recognizing that the number of such articles is numerous, in chapters 5 to 8 of Part II a representative sample of four such articles is provided.

Chapter 5 on the contributions of the 1990 Nobel winners is by al Varian, a chaired professor of economics at UC Berkeley, Chapter 7 by Robert Jarrow, a chaired professor of finance at Cornell University, is about the contributions of the 1997 Nobel winners. Both articles appeared in the generalist economics journal, *The Journal of Economic Perspectives.* As such, the discussion is aimed at non-finance specialists. Chapter 6 by Rene Stulz, former editor of the *Journal of Finance* and a chaired professor at Ohio State University, appeared in the specialist finance journal, *Financial Management*, and is concerned solely with the contributions of Merton Miller. Being a PhD student at Miller's MIT in the late 1970's, Stulz is well suited to the task of describing the contributions of this "founding father" of modern financial economics. Each of these chapters is an excellent example of the slanted interpretations of the intellectual history and inflated claims of scientific significance common in modern financial economics, in particular, and in emerging scientific movements, in general. In reviewing these chapters, the observation of another Nobel prize winner – George Stigler (1982) – comes to mind: "we commonly exaggerate the merits of originality in economics ... we are unjust in conferring immortality upon the authors of absurd theories while we forget the fine, if not particularly original, work of others" (Stigler 1965, p.15).

While chapter 8 is also a reprinted contribution, this chapter is decidedly different than the previous

three. The chapter is written by two Nobel recipients, Merton and Scholes, and is concerned with Fischer Black, who had died from throat cancer by the time the prize was awarded for the contributions that all three had shared in developing. The original article appeared in the *Journal of Finance*, generally recognized as the leading journal in modern financial economics. Being a significant player in both the practitioner and academic realms, Black was a distinctly different figure compared to the other five Nobel recipients. This is, perhaps, best illustrated by the affiliation to Long Term Capital Management (LTCM) listed by Merton and Scholes.[11] They did not know at the time the "Fischer Black" article appeared in 1995 that their role as principals in LTCM would be evidence of the 'failure of genius' rather than evidence of how academic stature could be translated into financial market success, e.g., Lowenstein (2000). Unlike the five Nobel recipients in from the modern financial economics movement, Black spent a significant portion of his working life as a more than successful finance practitioner. This chapter details the impressive list of both scholarly and practitioner-oriented contributions made by Black. It is difficult to deny his status at the forefront of the pioneers of financial economics in the 20th century.

In addition to the four reprinted chapters, chapter 9 of Part II also contains an original contribution on the efficient markets hypothesis (EMH) by Kian-Guan Lim. As this chapter masterfully demonstrates, the EMH is a far reaching, multifaceted concept with deep historical roots. Being a precursor to the modern financial economics movement, the EMH was, in turn, shaped by developments in that scientific movement. This chapter demonstrates that the EMH cannot be credited to any one individual, though Eugene Fama did make important contributions at a critical time when the EMH was being merged into modern financial economics. The EMH has evolved over time and, at present, has been severely challenged by the empirical identification of anomalies,

such as the January effect, the small firm effect, the success of momentum strategies and so on. In effect, the EMH is still a work in progress. What is clear from this chapter is that the EMH has provided a framework to gain a "new perspective on old ideas" and has "changed the beliefs and interests of economists and thus changed economics" (Stigler 1965, p.4). As such, the EMH deserves the recognition and attention awarded in this chapter.

A volume that seeks a sociological interpretation of a recent intellectual history would be incomplete without a number of chapters exploring this approach. This is the task of Part III. Each of the four chapters explores the history of modern financial economics from an alternative perspective. Chapter 10 by Franck Jovanovic uses the sociology of scientific movements to analyze the emergence of modern financial economics during the 1960's. As chapter 4 on Macaulay demonstrates, there is much more to the process of knowledge creation than the substance of ideas. This chapter demonstrates that the success of a scientific movement and the 'knowledge' created by that movement depends on the creation of a community of scholars. Many of the arguments underpinning volume 2 concerning the application of general concepts from the sociology of scientific movements to the specific case of the modern financial economics movement are carefully laid out in this chapter. Though this approach has been applied to others topics in the history of economic thought, e.g., Yonay (1994), Chapter 10 is the first application of such notions to the history of financial economics.

Chapter 11 contributed by the renowned sociologist Donald Mackenzie, stands in stark contrast to chapters 7 and 8 that were written by renowned financial economists. Though the historical and intellectual ground being covered is more or less the same, the interpretations and conclusions are dramatically different. The contrast is well illustrated by a quote at the end of chapter 10: "When

in 1968 David Durand, a leading figure from the older form of the academic study of finance, inspected the mathematical models that were beginning to transform his field he commented that 'The new finance men ... have lost virtually all contact with terra firma'". In this chapter, the self-congratulatory claims of insiders of modern financial economics are subjected to the scrutiny of an informed outsider. The result is fascinating reading. Without wanting to steal the thunder for Mackenzie's analysis, in the end the option pricing component of modern financial economics comes out somewhat scuffed but still in healthy condition. Important connections to later analytical developments such as the equivalent martingale measure are correctly identified and the case is made for a number of pioneers to be 'discovered' by historians of economic thought at a later time.

Chapter 12 turns the focus from sociology to philosophy. Together with George Frankfurter, the author of this chapter – Elton 'Skip' McGoun – has brought the epistemological shortcomings of modern financial economics into the 'intellectual attention space', e.g., Frankfurter and McGoun (1996). In this chapter, McGoun explores the epistemological foundations of the risk measurement techniques used in modern financial economics. The basic issue is captured in chapter 5 where Varian states: "Risk and return are such fundamental concepts of finance courses that it is hard to realize that these concepts were once a novelty." The rush to mathematical rigor and empirical verification that is at the core of modern financial economics has accepted, without question, the frequentist notions of probability that underpin, among other facets, the risk measurement techniques employed. This chapter demonstrates that the 'novelty' in these methods was not due as much to ignorance of the techniques as distrust. The dismal empirical performance of key features of the MPT, and the subsequent disconnect with the real world, is evidence for the skepticism that prevailed prior to the ascendancy of the modern financial economics movement.

The final chapter of volume 2 comes full circle to place modern financial economics within the overall transformation of economic science that took place following WWII. The author of this chapter – Paul Davidson – needs no introduction. Editor of the *Journal of Post Keynesian Economics*, one of the 'founding fathers' of Post Keynesian economics, Davidson can speak with scholarly authority on the drive to mathematical rigor that consumed economic science in the second half of the 20th century. The chapter contains threads from a number of essential contributions that Davidson has made over a long and distinguished career: the questionable empirical relevance of the axiomatic approach to economic theorizing; the implications of assuming ergodicity; the empirical orientation and simplicity of the Keynesian approach; the connection to market efficiency; the key role played by liquidity and speculation in financial markets. The insights are deep and numerous. In particular, Poitras (2005, ch.1) demonstrates the implications of the failure of the ergodicity assumption in modern financial economics, for example: what practical validity does mean-variance optimization have when the conditions required for an *ex post* optimal portfolio to have similar *ex ante* performance do not apply due to failure of the ergodicity assumption? Is it sensible to apply models developed for the natural sciences to predict human behavior? This chapter makes a fitting, if not transparent, conclusion to task of compiling a record of the pioneers in the history of financial economics.

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NOTES

1. The problem has generated debate since *WN* was published. The *Problem* still concerns historians of economics, e.g., Montes (2003). The use of German to identify the problem refers to the origins of the debate in German sources, e.g., Feder (1777) discussed in Montes (2003).

However, the importance of the problem has survived due to the intellectual conflict between the tradition of British laissez-faire represented by Smith and central European traditions of political economy. The German Historical School played a key role in identifying and formulating the problem.

2. The modern reverence to Smith is illustrated by the first sentence in Schabas (2003): "Adam Smith (1723-90) was *the* towering figure of Enlightenment political economy, a stature he attained in his own lifetime, much as Isaac Newton had in his." An example of the extent to which much modern Smith scholarship lies in the realm of philosophy and history, rather than economics, is captured in Montes (2003, p.80) discussing *Das Adam Smith Problem*: "... the *Problem* has been tackled at a hermeneutic level, leading to some novel approaches ... Dupuy exploits the reflective nature of sympathy in the relationship between spectator and actor and attempts to show that for Smith, self-love is simply a reflection of sympathy. He follows Thomas Reid and Lange-Stephen in considering that 'self-love is in reality a reflexive modality of sympathy'." Concepts such as hermeneutics, ontology and deconstruction, commonly encountered in the modern history of economic thought, are the preserve of philosophers and not immediately relevant to the modern mainstream economics curriculum.

3. The modern literature on Adam Smith is voluminous. Wright (2002) is a survey of the recent literature that provides a fascinating analysis of the resurgence of research interest in Adam Smith. Together with Wright (2002), the bibliographies to Montes (2003) and Schabas (2002) contain many of most recent contributions.

4. In the present context, the 'classical' time period roughly corresponds the latter part of the 17th century until the latter half of the 18th century. The 'neoclassical' time period corresponds to the latter half of the 19th century until the beginning of World War II. The 'modern' time period covers post-WW II until the present.

5. Circa 1780, it is not the *WN* but, rather, the *Observations on Reversionary Payments* (1772) by Richard Price that has the claim to be the most important book in classical financial economics (Poitras 2000). Almost an unknown in the modern literature on the history of economic thought, Price made a number intellectual contributions that argue for a respected place in the history of economic thought, including the first feasible plan for old age pensions. Perhaps being a dissenting, non-Anglican minister and a vocal supporter of the American side in the War of Independence did not endear Price to the British upper class and the writers that produced the classical political economy in which Smith has a hallowed position?

6. The remarkable success of Bernstein (1996) speaks to the popularity and relevance of a history of economic thought focusing on methods of handling risk.

7. Modern economics has come a long way from the time when Frank Knight could claim: "One who aspires to explain or understand human behavior must be, not finally but first of all, an epistemologist." This is not a statement of the correctness of Frank Knight's viewpoint. Rather, it is an empirical observation that modern economics has adopted a decidedly more 'scientific'

epistemology driven by theoretical modeling and empirical estimation.

8. As was common in this era, a number of different variations of 'Jacob' Bernoulli were used. Jacob is also called Jakob (in German), Jacques (in French) and James (in English). Jacob is the name used on the title page of *Ars Conjectandi*. Jacob was the older brother of John (Johann) Bernoulli (1667-1748), also a prominent mathematician. Three sons of John Bernoulli also achieved prominence, Nicholas (1695-1726), Daniel (1700-1782) and John (the younger) (1710-1790). John the younger had two sons that also were prominent academics. The Bernoullis – perhaps more correctly, called, the Bernouillis – were originally of Dutch origin, driven from Holland by the Spanish persecutions, that finally settled at Bâle in Switzerland.

9. There is a typo in the Introduction to volume 2 where it is reported that only 40 of 391 pages are reproduced. The point that available English editions of *Confusiones* are abridged is correct but the actual number of pages omitted is difficult to determine, if only because the editing out of material occurs on many pages of the text. A closer number would be 240 of 391 pages are reproduced.

10. As demonstrated in Black and Scholes (1973) under appropriate assumptions it is possible to derive the Black-Scholes formula from the CAPM. However, the CAPM is concerned with the relationship between expected returns while the absence-of-arbitrage consistent Black-Scholes formula is based on the relationship between current prices.

11. In the reprinted version given in chapter 8 of the *Pioneers*, the production editor omitted the salutation that appears at the end of the published *Journal of Finance* article where both Merton and Scholes list their names followed by explicit acknowledgment of their affiliation with LTCM.