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Pioneers of Financial Economics:
Das Adam Smith Irrelevanzproblem?

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1. Introduction

The steady decline in importance of the history of economic thought is well known (Cardoso 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 examines 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 Adam Smith Irrelevanzproblem* 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* (1759) and the *Wealth of Nations* (1776) (*WN*).[1] *'Das 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 the traditional

approach to the history of economic thought ('traditional thought', hereafter). In contrast, *Das Irrelevanzproblem* is concerned with the bias introduced by static chronicling of the history in terms of individuals and associated texts, rather than by an evolutionary approach to history that reflects the germination and propagation of ideas used in modern economics. In traditional thought, the same specific individuals and texts, such as Adam Smith and the *WN,* constantly play a pivotal role. This static approach to the history resists the evolutionary approach based on chronicling "the genesis of ideas and the evolution of methods" (de Roover 1974, p.366). The perspective of '*Das Irrelevanzproblem'* leads to substantively different questions than those addressed in '*Das Problem'*. In particular, why do Adam Smith and other classical political economists continue to play such a central role in traditional thought when the relevance of these thinkers to modern economics, particularly new additions such as financial economics and econometrics, 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 the methods and ideas used in modern economics?

Despite the reference to Adam Smith in the title of this paper, those seeking a discussion of the financial economics of Adam Smith will be disappointed. Following Poitras (2000), *WN* had little to add and ignored much of what had been contributed to financial economics up to that time. Rather, this paper documents a relevant and useful history of financial economics that commences at least a century prior to *WN* and has a history that is largely independent of the traditional thought which commences with the role of Adam Smith as the leading Enlightenment thinker on issues relevant to political economy.[2] Once *Das Irrelevanzproblem* is identified, fundamental concerns are raised about potential reasons for divergence between histories with comparable time lines. In

particular, tools and concepts used in financial economics, especially the central role played by uncertainty and the pricing of securities with contingent payoffs, are of little concern to the neo-classical economic analysis emphasized in traditional thought. Following Frickel and Gross (2005) and Poitras and Jovanovic (2007), resolving *Das Irrelevanzproblem* leads to a connection between the history of a subject and the networks that control the academic venues and outlets that define a subject. For example, in modern times the Chicago school was systemically connected to the rise of modern Finance, in addition to being involved in a range of other post-WWII changes in economic science.

The search for pioneers of financial economics shares features in common with other recent histories of economic thought. For example, a credible history of econometric thought has been captured by Epstein (1987), Morgan (1990) and LeGall (2007). This exercise in tracing the pioneers of econometrics was facilitated by constructing this history with primary sources appearing largely in the 20th century. Subjects with longer pedigrees, such as microeconomics, have experienced a gradual 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 neo-classicals, such as Walras and Edgeworth, that are central to figures in traditional thought, this is not the case with financial economics. As a consequence, identifying pioneers of financial economics requires the detailing of a history that is does not intersect with traditional thought until well into the 20th century when financial economics emerged on the landscape of modern economics. Many avenues in this history are still unexplored. Following the blueprint of the history of financial economics constructed in Poitras (2000, 2006),[3] seminal contributions by the likes of Edmond Halley and Abraham de Moivre lie well outside the time line of traditional thought. Adam Smith has little

to do with this history, Malthus, Ricardo and Marshall even less.

2. *Das Adam Smith Irrelevanzproblem?*

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 wider 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) and Gronenwegen (2001) as it is in the voluminous research effort still being dedicated to Adam Smith and other classical political economists.[4] 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".

As a rapidly accumulating collection of sources illustrates, slicing vertically into the time line of financial economics reveals substantively different results as to the importance of the individuals involved in traditional thought.[5] Similar to the criticisms of Smith by the German Historical School that engendered *Das Problem*, upon closer inspection the history of financial economics reveals flaws in the traditional interpretation of Smith as "*the* towering figure of Enlightenment political economy" (Schabas 2003). This is an important part of the story underlying Poitras (2000, 2006).

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 primary sources for credible classical political economists that do not commence with a glowing tribute to the contributions of Adam Smith, e.g., Poitras (1998). Such is the accepted interpretation given by those that have written *the* history of classical political economy, i.e., traditional thought. Yet, from the perspective of classical financial economics, there is a much different history that does not include any of the most revered classical political economists.

To be relevant, history of economic thought needs to resonate with contemporary economists, speak to their interests. This requires updating and rewriting history to capture the evolution of modern economics. Concepts basic to classical political economy, such as the labor theory of value, are far removed from the landscape of modern economics, in general, and financial economics, in particular, where the more immediate problem of determining the price of a financial security is 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 economics.[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 traditional thought, the beginning of classical political economy, is invariably identified with Adam Smith (1723-1790). The *WN* has been justly recognized as the first book to develop a comprehensive and cohesive body of economic analysis 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 Lewin (2003), 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. Discrediting some of this knowledge, such as the early mercantilist writings on foreign exchange markets, was a central concern for Smith. Other areas, such as the pricing of aleatory contracts, are not examined by Smith. Despite the claims by Gronenwegen (2001), e.g., "Richard Price's work ... had been an important authority used in writing the *Wealth of Nations",* important figures in the early history of financial economics receive no mention in traditional thought. Historical developments involving elements central to financial economics – such as valuation of joint stocks, the emergence of life insurance companies and methods of pricing life contingent government debt – receive only superficial coverage in classical political economy, in general, and the *WN*, in particular.[8]

Das Irrelevanzproblem is concerned with the lack of relevance to modern economics of a particular interpretation of the history of economic thought. At issue are the definition of the subject of economics and the relationships between the sometimes disparate fields and sub-fields of the subject. The associated 'histories of economics' solution to *Das Irrelevanzproblem* appears upon examining the history of important fields within modern economics. In the case of financial economics, the roots

of this field stretch back to antiquity, involving the valuation of financial transactions, such as determining payment on a loan or distributing profits from a partnership. Poitras (2000) uses the late fifteenth century as a starting point for the early or pre-classical history of financial economics, more than three centuries prior to the publication of the *WN*. As early as Fibonnaci (1170?-1250?), elements of financial economics were being disseminated among the merchant classes in the commercial arithmetics that, by the fifteenth century, formed the core of the reckoning school curriculum, e.g., Swetz (1987). A fundamental historical demarcation point appears with Christian Huygens (1629-1695) seminal introduction of the modern theory of expectations.

From this point, until the appearance of the *WN*, the founding work of classical political economy, financial economics underwent a dramatic transformation. By the time the *Theory of Moral Sentiments* appeared, 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 were being applied to the establishment of actuarially sound life insurance plans and pension funds. Hald (1990), Poitras (2006), Lewin (2003) and Rubinstein (2003) among others identify the earliest pioneers of modern financial economics, the beginning of classical financial economics, from the contributors that developed these pricing methods. As such, there is a close connection between the classical histories of financial economics, statistics and actuarial science. *Das Irrelevanzproblem* appears because these relevant and useful histories are ignored in the treatment of classical political economy that uses Adam Smith to demarcate the historical time line and to define the analytical issues of relevance.

In contrast to Poitras (2000) where pre-classical figures such as Fra Luca Pacioli, Nicholas Chuquet, Simon Stevin, Thomas Gresham and Gerard de Malynes are included in the early history,

Poitras (2006) begins the classical history of financial economics during the second half of the17th century, an appropriate starting point due to the emergence of notions from probability theory being used in valuation methods; such notions now form the basis of modern contingent claims analysis, e.g., Rubinstein (2003). Following on the development of the concept of expected value by Huygens, in 1671 the Dutchman Jan de Witt (1625-1672) provided the first actuarially sound solution for the price of a life annuity. In addition to intellectual contributions, the 17th century also featured significant institutional developments such as the emergence of exchange trading in joint stocks, commencing in 1602 with trade in shares of the Dutch East India Company in Amsterdam and encompassing the 1694 share issue for the Bank of England. As reflected in Joseph de la Vega's *Confusion de confusiones* (1688; Fridson 1996), securities markets had achieved a remarkable level of development by the end of the 17th century (Kellenbenz 1957; Cardoso 2002,2006).

Somehow, it is difficult to give up the vision that Adam Smith is the intellectual father of modern economics. It is as if this vision was formed in a vacuum, ignoring the sometimes startling scientific and social developments that took place during the Enlightenment. Of the numerous threads in the relevant histories of that time, Adam Smith is seen in isolation as an intellectual giant fostering a line of analytical development – including Malthus, Ricardo, and J.S. Mill – that evolved into the neoclassical economics of Jevons, Walras, Marshall, Hicks and Samuelson. Other possible historical threads that produced economic methods and ideas of central relevance to specific fields within modern economics are ignored. In effect, traditional thought is concerned with issues and individuals that were perceived as important at the time, as opposed to studying the *actual* 'genesis of ideas and the evolution of methods' used in modern economics. Solving *Das Irrelevanzproblem* involves recognizing and explaining this bias with the objective of restoring the relevance of the history of

economic thought to modern economics.

Consider, for example, the role that Jacob Bernoulli (1654-1705) played in the early history of financial economics. Sylla (2006, p.11) 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 could also serve as a useful historical demarcation point between the intellectual tradition constrained by pre-Smithian scholastic and humanist considerations and the significantly more modern insights generated during the Enlightenment. Sylla (2003, 2006) connects the roots of probability theory contained in Bernoulli's posthumously published *Ars Conjectandi* (1713) with the foundations of Christian Huygens' approach to mathematical expectation. Both are grounded in Renaissance commercial arithmetic and issues of fairness and justice in exchange. Though Bernoulli was aware of Jan de Witt's work 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 with Huygens, Jacob Bernoulli is also a more than fitting individual to characterize the end of the pre-classical period in the history of financial economics than contemporaries relevant to pre-classical political economy such as, say, Locke or Petty.

Various sources identify the origination of valuation models for pricing life annuities as the appropriate starting point for the classical history of financial economics, , e.g., Poitras (2000, 2006), Rubinstein (2003). The 1671 contribution of Jan de Witt and the solution for the single life annuity value given by Edmond Halley (1656-1742) in 1693 each deserves some recognition for making a seminal contribution in using discounted expected value to price a contingent claim. While de Witt made the first discoveries, his results were developed in a public policy context, in this case a life annuity issued by the Dutch government, and were 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.[9] The most impressive classical statement of the analytical solution for pricing various types of joint life annuities is provided by Abraham de Moivre (1667-1754).[10] An important pioneer of classical financial economics, de Moivre was the first to substantively develop mathematical techniques, such as series solutions, to obtain remarkable closed form solutions for the price of fixed income securities with contingent outcomes.

There is considerably more to solving *Das Irrelevanzproblem* than just recognizing the lack of attention given in traditional thought to historical figures that pioneered the methods and ideas used in modern financial economics. *Das Irrelevanzproblem* speaks to the process by which intellectual histories are created and how subjects are defined. To see this, consider the treatment of a contemporary of Adam Smith: the Rev. Richard Price (1723-1791). Those unfamiliar with the life and contributions of Richard Price will find Pearson (1978, p.370-421) and Ogborn (1962) to have helpful overviews. Other useful sources include the memoirs by Morgan (1815) and the modern biography by the philosopher Thomas(1977). By any reasonable standard, it is an understatement to say that Richard Price (1723-1791) was a remarkable individual; a caricature for English intellectual achievement in the 18th century. So much of what Price contributed still resonates in modern economics, providing insights into subjects ranging from social security reform to the proper management of insurance companies to the political implications of government debt issuance. Yet, compared to the voluminous efforts expended on detailing the contributions of contemporaries in political economy such as Adam Smith or David Hume, Price has received scant attention. Most of the attention Price has received in modern times focuses on his subsequent contributions to the

subjects of actuarial science, philosophy and political theory.

Why does Richard Price receive no attention in traditional thought? The transmission of Bayes theorem, *The Observations on Reversionary Payments* (1771; 6th ed. 1803) and the first feasible proposal for old age pensions would seem to be sufficient to warrant at least passing mention. The absence of such discussion is evidence for the role that networks play in *Das Irrelevanzproblem*. Despite the relevance of his methods and ideas to modern economics, it seems that Price has been ignored because of his situation relative to the networks that dominated academic discourse in political economy at the time his writings appeared. Being a dissenting minister and strident supporter of the American position in the War of Independence, Price did not endear himself to the upper classes of later 18th century England, a constituency well suited to the ideas of Adam Smith. Similar to de Moivre, though for different reasons, Price did not hold a university position, though both were fellows of the Royal Society (in Price's case due to transmission of Bayes theorem). It is within the scientific networks associated the Royal Society, not those of traditional thought, that both Price and de Moivre have survived.

As it turns out, a number of factors combined to deprive Price of a dedicated community of scholars to carry on and explore the aspects of his work related to economics. While Price's legacy survived in the history of a number of other subjects, it had little impact on the political economists of the late 18th and early 19th centuries. Given that much of traditional thought is a reflection of the methods and ideas common to a particular period, Price gets no mention in this history even though there is considerable relevance of his methods and ideas to modern economics. As the contemporary subject of economics has evolved, the static history of traditional thought has become increasingly disconnected with the ideas of methods in current use leading to *Das Irrelevanzproblem*. The

'community of scholars' play an essential role in understanding the development and evolution of what constitutes modern economics. In addition to the Chicago school, recent examples of the emergence of a well defined community of economic scholars united by a coherent body of results includes: the evolution of economics as a mathematical science (Weintraub 2002); the establishment of institutional economics in the inter-war period (Yonay 1994); and, the post-WWII emergence of Post Keynesian economics (Davidson 2007).

4. Developments During the 19th Century

The original *Das Problem* begins and ends with the books of Adam Smith. In contrast, *Das Irrelevanzproblem* looks beyond Smith to demonstrate the bias in traditional thought associated with exhaustive study of other figures in traditional thought, such Ricardo, Malthus, Mill and Marshall. Closer inspection reveals histories of economic thought more relevant to specific fields of modern economics that go unrecognized in traditional thought. Only recently has it been established that notions central to 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. Alex Preda (2004, 2006) details the social and economic developments that laid the foundation for this early progress towards the modern theory of efficient markets. The needed cognitive and cultural background required transforming financial investing into a science. This required altering public perception to see financial securities as investments rather than gambling.

Jovanovic (2006a,b) and Jovanovic and Le Gall (2001) examine the seminal contributions to the 19th century 'science of financial investments' made by Henri Lefèvre and Jules Regnault. In the

process, a quandary arises concerning the remarkable amount of attention given to the 1900 thesis of Louis Bachelier (1870-1946) by the community of scholars that founded modern 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. The accolades of various modern writers have attributed this to Bachelier being too far ahead of his time, e.g., Sullivan and Wethers (1991). Various studies, including Jovanovic (2006a, 2006b), Zimmermann and Hafner (2006, 2007), Hafner and Zimmermann (2009) and Dimand and Ben-El-Mechaiekh (2006) now 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 from the history of financial economics that was far ahead of its time, Bachelier's thesis appears to be better viewed as seminal contribution to the theory of stochastic processes and Brownian motion.

It is now recognized that the revival and subsequent reverence accorded to Louis Bachelier is misplaced. The somewhat haphazard 'rediscovery' of Bachelier was initiated by Leonard Savage and Paul Samuelson in the mid-1950's (Bernstein 1992). While marvelous theorists, neither specialized in the creation of intellectual history and traditional thought provided them no relevant guidance. Absent voices other than the seemingly authoritative Savage and Samuelson, Bachelier rose to prominence in the wider community of modern financial economists primarily from connections to option pricing theory and the efficient markets hypothesis. This prominence was reflected in the inclusion of a translation of Bachelier (1900) in Cootner (1964). Following Dimand

and Ben-El-Mechaiekh (2006), the role of Bachelier in the history of financial economics has to recognize not just the contributions to options pricing and speculation but the views of Poincaré, Lévy, Kolmogorov and Keynes, among others. The history of financial economics has now progressed to where there can be useful debate about the extent of Bachelier's pioneering role in financial economics. That Bachelier was a pioneer in the realm of probability theory and stochastic processes is apparent. Given the evolution of the methods and ideas used in modern economics, that contribution is now arguably sufficient to bring Bachelier within the historical boundaries of economic science.

Since Jovanovic and Le Gall (2001) rehabilitated the work of Regnault, the 'originality' of Bachelier's contribution to the efficient markets hypothesis has been brought into question. In detailing the insightful work on option pricing by Bronzin (1908), Zimmermann and Hafner (2006, 2007) and Hafner and Zimmermann (2009) raise questions about the claim to primacy for Bachelier in the area of option pricing. 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, 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. While the modern reverence accorded Bachelier for his seminal role in financial economics needs some reconsideration, it is also apparent that by the end of the 19th century the time line for the history of financial economics was still far from the contributions to traditional thought by historical contemporaries of Bachelier such as Jevons, Walras and Marshall.

5. The Emergence of Modern Financial Economics

There are sound reasons underpinning the observation, "No history of ideas, please, we're economists". What separates history of economic thought from the history of ideas is 'economics', a subject with fluid boundaries that change over time, e.g., Hausman (1992). Precisely when the boundaries of economics expanded to include financial economics is open to interpretation. While the history of financial economics at the end of the 19th century had little overlap with the traditional thought, from that point to the present 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.

A number of essential features of modern financial economics can be discerned (Jovanovic 2007, 2008). This scientific movement rose to ascendancy in the post-WWII period by supplanting the 'old finance' school (Haugen 1999; Poitras 2005) that was loosely 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, e.g., Weintraub (2002). Modern financial economics has two central components. One component is the

'modern portfolio theory' (MPT) which is associated with Markowitz, Sharpe, Miller and Fama. The MPT shares many features in common with mainstream economic theory but 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.[11] The classical history of this stream has closer affinity with actuarial science than with political economy. Contributions from this stream have been highly successful in the practitioner realm, being largely responsible for spawning the financial engineering and financial risk management industries.

Though modern financial economics as a scientific movement only obtained the critical mass of a community of scholars during the 1960's (Jovanovic 2008, 2007), the historical threads associated with the methods and ideas of this movement go back to the inter-war period. Overlap with important figures in traditional thought can be clearly discerned in contributions of 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. Given that the modern financial economics movement originated in the US, Fisher deserves recognition for a seminal role in the history 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 for the inclusion of financial economics as a field within modern economics with Irving Fisher.[12] Students of Fisher went on to play key roles in future development of financial economics (Dimand 2007, 2008). Unfortunately, Fisher is often remembered for his gross miscalculation about the collapse of stock prices at the end of the 1920's, e.g., Poitras (2005, p.98-105).

From a sociology of knowledge perspective, e.g., Hands (1997), there are common threads that

contributed to the success of modern financial economics as a scientific movement. Following Frickel 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 the network that developed and sustained 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, to a lesser extent, 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 *ex post* 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 the modern financial economics network 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 analytical 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 exercise would have been more than sufficient to propel modern financial economics into the mainstream of economic science. 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 (1967) was missing from the edited volume, Cootner (1964) did provide, along with other studies of option pricing, an 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 (Mackenzie 2007, 2003). 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 network.

It is the readily discernable history of the contingent claims pricing movement that is the central concern of classical financial economics that starts with de Witt and Halley. The 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 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 mathematical transformation that took over the whole subject of economics in the post WWII period.

6. Alternative Perspectives and *Das Irrelevanzproblem*

Despite perceptions to the contrary, e.g., Rubinstein (2002), there is much more to the history of financial economics than the time line of the modern financial economics movement. Alternative perspectives include: the interwar impact of the institutionalists; and, the contributions of J.M. Keynes and the Post Keynesians. Following Rutherford (1994), Morgan and Rutherford (1998) and Yonay (1994, 1998), the inter-war period in American economics was characterized by the

"pluralism" of the institutional school, centered around W.C. Mitchell and the National Bureau of Economic Research (NBER). Coming closely on the heels of the manifesto of institutional economics – Hamilton (1919) – the establishment of the NBER in 1920 was an important milestone in the emergence of institutionalism as, arguably, the dominant school in American economics during the inter-war period. While institutionalism as an intellectual force was not able to recover from the post-WW II 'measurement without theory' criticism leveled by Koopmans (1947) and others, this school of economic thought made contributions to the conduct of economic policy and government practice that survive to present.

Following Rutherford (2001), the institutionalist agenda emerged in the immediate aftermath of WWI and was propelled by a desire to support an enhanced role for government in the economy to achieve much needed social and economic reform. This created a demand for improved economic data and policy analysis that were the touchstones of institutionalism. Proposing a "modern" and "scientific" empirical approach analogous to that used in the natural sciences, institutionalism aimed to replace the theoretically driven neo-classical approach to economics that dominated economics prior to WWI, e.g., Yonay (1994). While Thorstein Veblen (1857-1929) is often recognized as the "intellectual inspiration for institutionalism" (Rutherford 2001, p.174) and John R. Commons (1862-1945) is credited with playing a key role after 1924, it is Wesley C. Mitchell that served as a founding father of the movement, as a guiding light during its development and as the originator of the most significant intellectual contribution of the movement, the empirical measurement of business cycles.[13] Mitchell produced *Business Cycles* (Mitchell 1913) a book which Arthur Burns (1952,p.22) describes as "one of the masterpieces in the world's economic literature". Together with *Business Cycles: The Problem and Its Setting* (1927) and *Measuring Business Cycles* (1946, with

Arthur Burns), these three books are Mitchell's definitive work on the subject that still epitomizes his career, e.g., Klein (1983).

Mitchell was instrumental in organizing the NBER, where he served as Director of Research until he resigned in 1945. From the founding of the NBER, "the National Bureau was the focus of his intellectual interest, the emotional center of his own work, and the work responsibility that lay closest to his inner life" (Burns 1952, p.102). The NBER was established with grants totaling $24,000 with which Mitchell was able to hire a small research staff to undertake the first major study on the size, growth, fluctuation and distribution of national income. The initial research staff for the national income study had three members: Willford King, Oswald Knauth and Frederick Macaulay. Though results of this study were published within three years, there were a number of follow-on business cycle projects generated by this initial effort. Among these special studies that got underway in the early 1920's was one on the cyclical fluctuations in interest rates undertaken by Macaulay (Fabricant 1984; Poitras 2007). A seminal figure in modern fixed income analysis – Frederick Macaulay – completed a PhD under Mitchell at Columbia. In the history of financial economics, Macaulay is recognized for the NBER study: *The Movements of Interest Rates. Bond Yields and Stock Prices in the United States since 1856* (1938); a contribution that contains the initial treatment of the 'Macaulay duration' concept. Even though fixed income analysis is conceptually essential to financial economics, and the duration concept is a significant contribution to that subject, Macaulay had died before the eponym 'Macaulay duration' was introduced. Even though these contributions satisfy the requirements of 'mathematical rigor' and 'careful empirical study', why did Macaulay not receive timely and adequate recognition within modern financial economics? The answer to this question lies in the role of intellectual networks in generating *Das Irrelevanzproblem*.

Through his connection with the NBER, Macaulay came in contact with David Durand, also a Columbia PhD and member of the NBER staff after Macaulay's departure. After leaving the NBER in 1938, Macaulay took up the position of research director with the Twentieth Century Fund for a study commissioned by the New York Stock Exchange on short selling. The final results of this study, Macaulay and Durand (1951), is Macaulay's last published research contribution. Paul Samuelson claims that, while at the NBER, Durand "pioneered the empirical study of how long-term bonds usually require a higher yield than short. Everyone understands that today, but he was the first to document it". Durand assumed the position of Associate Professor at MIT in 1955 and professor in 1958, a position he held until his retirement in 1973. Given his strong connections to the institutionalist network, Durand used his prestigious academic situation to question the rise of modern financial economics, e.g., Durand (1959). Mackenzie (2007) observes: "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'".

When Stigler (1965, p.4) claims progress in science is measured by the opening of "eyes to new ideas or to new perspectives on old ideas", this creates the milieu for *Das Irrelevanzproblem* to appear. Stigler observes that, in most cases, recognition of originality requires the techniques of persuasion (p.5): "The techniques of persuasion ... repetition, inflated claims, and disproportionate emphases ... have preceded and accompanied the adoption on a large scale of almost every new idea in economic theory". This requires "the techniques of the huckster". As such, *Das Irrelevanzproblem* arises because traditional thought recognizes only the ideas and methods that were most prominent along a given historical time line and within a particular community of scholars. By

ignoring the ideas and methods that were unpopular, too seemingly outlandish or irrelevant at the time, useful connections with the methods and ideas used in modern economics are lost. It is not history of economic thought that is antiquarian but, rather, traditional thought that elevates the history of classical political economy to the exclusion of histories containing methods and ideas of more relevance to modern economics.

As evidenced by the lack academic attention given to Macaulay (1938) and the various contributions of Durand, not to mention the "vernacular" old finance camp – from Graham and Dodd to Peter Bernstein – the costs in loss of academic reputation and prestige are considerable. The battle for the high ground in financial economics between the institutionalist network -- emcompassing the 'old finance' -- and the neoclassical network -- represented by the Chicago school and members of the Cowles foundation -- played out quite late relative to other fields in economics but the final results were particularly vicious, even by academic standards. The opposition, it seems, was completely flattened and forgotten. Any helpful ideas were rolled into the scientific movement express train that was modern financial economics. As it played out, the wide gap between the *ex ante* claims advanced by the modern financial economics movement and the actual *ex post* performance of the theories in the market place brings to mind another observation of Stigler (1965, p.15): "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".

The failings of the risk management methodologies promoted by modern financial economics is one aspect of the *ex ante* versus *ex post* problems that have plagued the movement almost since inception. Frankfurter and McGoun (1996) and McGoun (2007) explore the epistemological

foundations of the risk measurement techniques used in modern financial economics. The basic issue is captured in Varian (1993): "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. The 'novelty' in these methods that was claimed was greeted with considerable skepticism at the time, not due as much to ignorance of the techniques as distrust of the predicted outcomes. The dismal empirical performance of key features of the MPT, and the subsequent disconnect with the real world, is evidence there needs to be additional effort given to the historical voices of skepticism that cried out prior to the ascendancy of the modern financial economics movement, especially those with grave concerns about the notions of uncertainty that underpin 'modern Finance'.

The emergence of modern financial economics coincided with the historical transformation of economic science that took place following WWII. This drive to mathematical rigor that consumed economic science in the second half of the 20th century did not go unquestioned. In addition to the institutionalists, critics and skeptics of the interwar period include, above all, J.M. Keynes. While interest in Keynes has thrived within certain fields of economics, the silence on Keynes in modern financial economics is deafening; *Das Irrelevanzproblem*, again. That Keynes was a professional speculator making the bulk of his wealth from securities and commodities trading would seem to be grounds enough for in depth study of the connections with, say, the famous chapter 12 of the *General Theory* (1936). Arguably, accurate understanding of chapter 12 requires recognition that Keynes was providing a viewpoint that was formed by a practitioner 'in the trenches'. That Keynes understood

something about resolving the *ex post* to *ex ante* dilemma facing theories in financial economics gives good reason to have substantive explorations of this thread in the history of financial economics. Yet, despite the presence of archives with detailed information about Keynes trading activities, there are only a few scattered studies providing, at best, only general information.

The subsequent development of the Post Keynesian perspective on financial economics e.g., Davidson (2007), seems to be a natural progression on the historical thread that captures the contributions of J.M. Keynes. However, these contributions are too close to the present to fall within the scope of a history of economic thought . Davidson speaks with scholarly authority on: 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; and, the key role played by liquidity and speculation in financial markets. The Post Keynesian insights are deep and numerous. In particular, Poitras (2005, ch.1; 2010) explores the implications of the ergodicity assumption for the *ex post* to *ex ante* dilemma facing modern financial economics. The questions raised are fundamental, e.g., 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 to accurately capture security market uncertainty? Is it sensible to apply models developed for the immutable laws of the natural sciences to predict the behavior of humans acting in groups with free will? While alternative approaches to solving such questions used in the past would be helpful in addressing such questions in the present, *Das Irrelevanzproblem* has stifled such notions from being effectively explored within a histories of economic thought context.

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NOTES

1. *Das Problem* has generated debate since *WN* was published and 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 as early as 1777, discussed in Montes (2003). However, the problem has survived partly 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. Rubinstein (2003) is the first of a sequence of contributions by Rubinstein that provide an interpretation of the history of financial economics sympathetic to modern financial economics.

4. 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 in Montes (2003) and Schabas (2002) contain many of the more recent contributions.

5. In the present context, the 'classical' time period roughly corresponds to 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.

6. The remarkable popular success of Bernstein (1992, 1996) speaks to the value of a history of economic thought focusing on issues of contemporary relevance.

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. This point eludes Gronenwegen (2001) where *Das Irrelevanzproblem* produces an exaggerated concern with the relationship of Poitras (2000) to the traditional time line of classical political economy.

9. In private correspondence, de Witt and Jan Hudde developed an ingenious analytical solution to the joint life annuity value based on application of the binomial formula (Poitras 2006a).

10. This leaves open the question as to the extent that Thomas Simpson (1710-1761) contributed to the development of useful joint life annuity pricing formulas, see Hald (1990).

11. 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.

12. Rubinstein (2006) demonstrates that Bruno de Finetti anticipated the results of Markowitz during the interwar period.

13. Mitchell received his college education and, in 1899, a doctorate from the University of Chicago. During this time he studied with and was deeply influenced by Veblen, J. Laurence Laughlin (1850-1933), the monetary economist, and John Dewey (1859-1952), the influential philosopher, psychologist and educational reformer. After a brief term at the Bureau of the Census and two years teaching at Chicago, in 1903 Mitchell followed one of his former teachers, Adolph Miller, to the University of California at Berkeley where, apart from a few brief excursions, he stayed until 1912. Mitchell joined the faculty of Columbia University in 1913. Except for a brief period of government service at the end of WWI and three years as a lecturer at the New School for Social Research (1919-1921), Mitchell was a member of the faculty at Columbia until his retirement in 1944.