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MIT and the origins of the modern theory of asset pricing

Version 6

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MIT
To understand MIT, visit its campus. Walk across Massachusetts Avenue, up a short flight of steps, through four imposing classical columns and you enter what is known as “the infinite corridor,” shown in Figure 1. As the Institute expanded, many new buildings were added, but the original intention was that all parts would be connected by corridors and walkways to this corridor. It is both imposing and supremely functional, with passages leading off to different parts of the Institute, all connected and identified by number. The Institute’s current website describes it as quirky, inventive, and ‘obsessed by numbers.’ Everything at MIT is represented by numbers: rooms, buildings, courses, and classes. It is said that you have become acculturated when you start referring to the restrooms by their number. The infinite corridor represents MIT’s ethos as a technical institution, an engineer having played an important role in its design (Hapgood, 1993). When the Institute moved to its current location next to the Charles River, the ground was reclaimed, and an engineer was necessary to confirm that tall buildings were possible without sinking into the soft ground. The engineer responsible was not given responsibility for designing the building but many of his ideas about how the Institute should be arranged were used.

Figure 1: MIT’s Infinite Corridor

¹ This paper is an abridged and extensively modified version of material that will appear as a chapter of Founder of Modern Economics: Paul A. Samuelson, Vol. 2: Being Samuelson, 1948-2009, contracted to be published by Oxford University Press.
² https://web.mit.edu/about/.
Engineering, at least as it is understood at MIT, is about using scientific knowledge to solve problems, especially difficult problems that require the use of mathematics—problems that involve modeling (White, 2003). An MIT education is about learning to think. It fosters admiration for people who are very smart, focused entirely on their work, who can think very quickly and who can solve seemingly impossible problems. It is a culture of success and high standards that places students under great pressure, with teachers having to remind students who cannot make it there that there are other engineering schools at which they could be very successful. Additionally, as at some other engineering schools, there is a willingness to recruit and promote young people.

During the 1930s and 1940s, the character of MIT had changed substantially (see Kaiser 2010). Its focus had been practical engineering training. Samuelson once noted how difficult it was to imagine one of his colleagues, the statistician Harold Freeman, an MIT graduate, learning how to pour molten metal into molds. However, under Karl Compton and Vannevar Bush, it changed its focus to the application of pure science—primarily physics and chemistry—to engineering problems. During the 1940s, MIT became one of the main recipients of government and military funding, much of which went into enormous research laboratories, such as the Radiation Laboratory, eventually renamed the Lincoln Laboratory, in which radar was developed. Consistent with this focus on solving problems, it was normal
for MIT’s top engineers such as Vannevar Bush to be involved in outside ventures. Engineering, of whatever type, was seen as needing a foundation in rigorous scientific analysis, but application was equally important.\(^3\)

The argument made in this paper is that the culture of MIT, shaped by engineers and engineering, provided an environment that was highly conducive to the development of the technical, mathematical theories that transformed the theory of finance in the 1960s and 1970s. It was, of course, not the only institution where such ideas were developed—for example, important developments took place in the University of Chicago and in financial institutions outside academia—but MIT played a significant role. The paper starts by introducing Paul Samuelson, the economist who, more than any other, dominated economics at MIT during this period, explaining the main ways in which the MIT environment proved conducive to his work. It then turns to work undertaken at MIT in the 1950s and early 1960s on movements in stock prices and warrant pricing, culminating on two papers published in MIT’s *Industrial Management Review* in 1965. From the late 1960s, the story moves more squarely to option pricing theory and the work of Robert C. Merton, significant parts of which were undertaken in conjunction with Samuelson. The final section draws conclusions about the relationship of these developments to the MIT environment that has just been described.

**Paul Samuelson**

The key figure in the emerging field of finance (as in economics more generally) at MIT was Paul Samuelson, recruited in 1940 at the age of 25 as a specialist in mathematical economics at a time when many universities were reluctant to hire such people.\(^4\) Although he was never an engineer and feigned modesty when comparing himself with MIT’s top natural scientists, the Institute provided an environment that suited him perfectly. Unlike in Harvard’s economics department, there was no one who deprecated the use of mathematics; men of Jewish origin were welcome; the Department of Economics and Social Science had a collegial atmosphere comprising many men in their thirties, and Samuelson was provided with a telephone, secretary and a research grant.\(^5\)

MIT’s location alongside the Charles River meant that it would be impossible for

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\(^3\) In order to focus on the role of the MIT environment, Samuelson’s extensive activities related to finance outside MIT are not discussed here. They will be discussed in Backhouse (forthcoming).

\(^4\) For a broader discussion of MIT in relation to American economics, see Weintraub (ed.) 2014.

\(^5\) The use of the word “men” is deliberate because, at this point, MIT did not appoint women as professors, even though women were admitted as students.
Harvard, established much earlier and dominating Cambridge, to expand in that direction. This contributed to a great rivalry between the two institutions. One consequence of this rivalry is that whenever MIT turned out to have someone who was clearly better than his or her counterpart at Harvard, that person was celebrated and provided with much encouragement and support. People such as Norbert Wiener and Noam Chomsky never had any reason to doubt the esteem in which they were held. From early on, it was clear that Samuelson fell into this category, and during the 1940s, his promotions came rapidly, with appropriate increases in his salary. Only once was he tempted to leave, and on that occasion, he changed his mind almost immediately.6

As his Harvard PhD thesis, subsequently expanded into *Foundations of Economic Analysis* (1947), shows, Samuelson was already committed to an “operationalist” view of economic knowledge, but it was at MIT during the 1940s, that he was exposed to statistical tools that eventually became crucial to the theory of finance. Graduate students Lawrence Klein and Joseph Ullman had run an advanced statistical seminar, and Samuelson also attended the seminars in which Norbert Wiener had developed the theory of cybernetics (Backhouse 2017). He read extensively in mathematical statistics. He cited Wiener briefly in *Foundations of Economic Analysis* and later paid tribute to him, drafting a brief note shortly after Wiener’s death in 1964 and a more comprehensive appraisal in the 1990s.7 Although there was little personal contact in later years, despite both remaining at MIT, Samuelson observed Wiener in mathematics seminars and described himself as “a sponge for Wiener stories” (Samuelson 1997, p. 38). His contact meant that he was familiar with theories of Brownian motion and Wiener processes that were to become central to his work in finance, and he would later be alert to the relevance of Robert C. Merton’s mathematical skills.

The theory of finance stands at the intersection of economics and business or management studies, the boundaries of which were then fluid. A key development was the establishment, with the support of Alfred Sloan, of the School of Industrial Management, which later became the Sloan School of Management (in the interest of brevity, I refer to it as the Sloan School throughout). Even though it remained part of

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6 Knowing that his position at MIT was secure and that he was held in such high regard might be a factor behind Samuelson’s habits, evident in his work on finance, of being slow to publish some results and of publishing in unrefereed “low status” outlets. Another factor is undoubtedly chronic overcommitment due to the number of activities in which he was involved.

the School of Humanities and Social Studies, the Department of Economics was moved into the Sloan Building, alongside the Sloan School, one of the aims being to foster collaboration between the two units. It was common for professors to teach in both schools and, unlike in some universities, boundary disputes appear to have been avoided. Samuelson remained in the Economics Department, but he was a member of the study committee that assisted in the development of the Sloan School’s graduate program, and he remained close to its activities. He regularly lectured to the Sloan Fellows on the School’s Executive Development Program.8

Taking advantage of its position in MIT and consistent with the MIT ethos, the Sloan School focused on quantitative problems associated with management. This contrasted with the dominant approach to business education in the early 1950s, the Harvard Business School case study method. It was not until the mid-1950s that the Ford Foundation instituted its program to move business education more generally in this direction (see Khurana 2007). Again reflecting on its role as an engineering school, collaboration with industry was not considered a distraction from academic work but was encouraged. In this environment, corporate finance was one of the fields that was developed from the outset, under the leadership of Eli Shapiro, a year younger than Samuelson and the School’s first Associate Dean and Professor of Finance. David Durand (appointed in 1955) and Paul Cootner (MIT PhD in economic history in 1953, appointed in 1959) further developed the field.

**Brownian motion, warrant pricing and efficient markets**

Like many of his colleagues at MIT, Samuelson had long had an interest in investments—in trying to beat the market. He described his motive as being “an economist’s curiosity. What was it like to sell short? What was it like to bet on a Packard-Studebaker merger? [in 1954] What was it like to dabble in puts and calls?” (Samuelson, 2003). Interviewed by a journalist, he elaborated on this, saying “Selling short is different on paper from the sweating feeling you have when you have to cover. Arbitrage looks great but can you execute if on an uptick and at what cost?” (Bender, 1971, 18). He invested in warrants and commodities, sometimes very successfully.9 This was an activity in which other MIT faculty were involved.

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8 This paragraph draws on various issues of the annual MIT Report of the President, and on evidence from Samuelson’s list of publications.

9 His investment activity will be discussed in more detail in Backhouse (forthcoming). Bender (1971) pointed out that his success was well known by the time of his Nobel Prize, though he was reticent in
Samuelson later recalled that at one point in the 1950s, they were investing in soybean futures: “On the third floor of the economics department were the soybean bulls; the soybean bears were on the fourth floor of the Sloan School [in the same building]; possibly all of us succeeded in making losses” (Fox, 2009, 71). On other occasions, however, he did very well in such activities.

Samuelson was familiar with the work of Alfred Cowles and Holbrook Working, implying that changes in stock prices were unpredictable. He attached sufficient importance to this that, in October 1953, together with his colleagues Ralph Freeman and Eli Shapiro, he applied to the Sloan School for funds to pay for a preliminary investigation into the behavior of publicly listed stock prices. He then learned, from Hedrik Houthakker, about a paper by the eminent British statistician Maurice Kendall, which found that the prices of shares and of a limited range of commodities were “wandering” in the sense that the change in price in one week was independent of what had happened in the previous week. It was, he wrote, “almost as if once a week the Demon of Chance drew a random number from a symmetrical population of fixed dispersion and added it to the current price to determine the next week’s price.” It was, Kendall and his coauthor claimed, “a kind of economic Brownian motion.” Samuelson, with his experience of trading in such markets and with his knowledge of Brownian motion through attending Norbert Wiener’s seminars, could see the significance of what Kendall was doing.

After encountering Kendall’s work, Samuelson suggested that a PhD student, Richard Kruizenga, work on put and call options. The resulting thesis, submitted in May 1956, addressed some of the questions he had raised with Houthakker, in particular questions about the expected values of options and the profits that might be earned on them. Kruizenga’s key assumption was that there was uncertainty about the future price of a security that could be described by a probability distribution. He calculated the expected values of put and call options and the effect

10 Freeman, R.E., Samuelson, P.A. and Shapiro, E., Letter to E. P. Brooks, 26 October 1953, PASP, Box 64 (S 1939-56, 1 of 2).
12 Ibid., p. 13.
13 Ibid., p. 18.
14 MacKenzie (2006, p. 63-4) reports Samuelson having told him that he also used such work while working on fire control for several months in the wartime Radiation Laboratory.
15 Kruizenga, 1956, Chapters 7-8.
on their value of being able to exercise them on different dates rather than on a single date. He was able to show, for example, that the difference between the price of a put and a call option could be used to measure the stock price expected by options traders.\textsuperscript{16}

On March 20, 1956, the statistician Jimmie Savage wrote a circular letter to several colleagues, a copy of which was sent to Samuelson, saying that he had discovered “a remarkable mathematical economic theory” developed by the French mathematician Louis Bachelier, in approximately 1910.\textsuperscript{17} Bachelier’s work on diffusion processes was well known, but what was less well known was that his main example was not any physical process, such as the diffusion of molecules throughout a liquid, but rather commodity markets. Savage explained that Bachelier modeled prices as Martingales—stochastic processes where the change in price from one period to the next is a random variable with a mean of zero, implying that the expected value of tomorrow’s price is today’s price. He also reported that Bachelier had explored many problems, “including the equitable prices of various sorts of options, including I think puts, calls, and straddles, though I must confess I don’t really know what those things mean even in English.” He asked whether anyone knew of Bachelier’s \textit{Théorie de la Spéculation}, a book he had been unable to trace. Samuelson had heard of Bachelier from Stanislaw Ulam, a mathematician in the Society of Fellows, in the late 1930s but did not know of this work. He could not find the book Savage mentioned, even in Harvard’s Widener Library, but he did find Bachelier’s doctoral thesis dating from 1900. This turned out to contain some of the results that Kruizenga, presumably with Samuelson’s assistance, had derived in his nearly finished dissertation.

Where Bachelier went beyond Kruizenga’s previous analysis was in assuming that price expectations followed a normal distribution with a mean of zero and a variance depending on time, with prices following a random walk (a specific type of Martingale). Using simple numerical examples (for example, that in each period, price is equally likely to rise or fall by one unit), Kruizenga worked out the probability distributions of future prices from which it was straightforward to calculate the value of different options and the profits to be held from holding them. A very specific result was that the price of an option should vary according to the

\textsuperscript{16} Ibid., p. 178.

\textsuperscript{17} L.J. Savage, Bachelier’s theory of commercial speculation, 1956-03-20. PASP, Box 67(Savage).
square root of its duration.\textsuperscript{18}

At some point in the next few years, Samuelson took up Kendall’s notion of “economic Brownian motion”.\textsuperscript{19} When reading Bachelier and supervising Kruizenga’s thesis, he had been concerned about two implications of seeing stock prices as Brownian motion. As the period over which the option could be exercised approached infinity, so too would the value of the option, which seemed wrong. This could be related to the problem that although the price of a stock can rise to any level, it cannot fall below zero due to limited liability. Both problems could be mitigated, if not completely solved, Samuelson argued, by modifying Brownian motion so that it described proportional changes in prices. For example, instead of there being an equal chance that a price would rise or fall by one dollar, there was an equal chance that it would rise or fall by one percent. Such a series can never turn negative.

In the four years between Kruizenga’s thesis and Samuelson’s presentation of his paper to the American Philosophical Society, others, including Harry Roberts, a Chicago statistician, and M. Osborne, an astrophysicist, reported similar results.\textsuperscript{20} When Osborne was challenged, his response was to provide a long list of work that had come to his attention since his paper was written and that anticipated both the idea that stock prices followed a random walk and that a logarithmic formulation should be used.\textsuperscript{21} Another economist working on the problem, who would have discussed his work with Samuelson, was Sidney Alexander in the Sloan School. In one paper, he addressed the issues of whether price changes should be measured weekly or monthly and what averaging should be used.\textsuperscript{22} Using weekly or monthly changes supported the idea of a random walk. However, if continuous upward or downward movements were considered, filtering out very small movements, it was possible to make a profit, showing that prices were not completely random. In short,

\textsuperscript{18} In other words, a four-period option should be twice as valuable as a one-period option and a nine-period option three times as valuable.

\textsuperscript{19} P.A. Samuelson, The economic Brownian motion: Paper presented to American Philosophical Society, Philadelphia, 1960-11-11. PASP, Box 95. The date when this paper was first written is not clear. It was certainly written after May 1956, when Kruizenga submitted his thesis, and it was eventually read to the American Philosophical Society on November 11, 1960. In this paper, he describes his work on this theory in the past tense, as although it was something he had completed long before

\textsuperscript{20} Roberts, 1959; Osborne, 1959a.

\textsuperscript{21} Osborne, 1959b.

\textsuperscript{22} Alexander, 1961.
if the stock market moves up $x$ percent, it is likely to move up by another $x$ percent before it moves down $x$ percent.\textsuperscript{23} The main reason for this was that there was, overall, an upward trend in stock prices. In a second paper, he further explored such filtering methods.\textsuperscript{24}

Such papers were the tip of the iceberg, with many economists exploring the implications of randomness for stock prices. Symbolic of the changed attitude was the publication of \textit{The Random Character of Stock Market Prices}, edited by a former graduate of the Sloan School, Paul Cootner.\textsuperscript{25} It reprinted the papers discussed here by Bachelier, Kendall, Roberts, Osborne and Alexander, along with chapters from Kruizenga’s thesis, a translation of Bachelier’s doctoral thesis and Kendall’s paper. There were also recent papers by Working, Cowles and a range of younger economists working on these same problems. It covered the formulation and testing of the random walk hypothesis and statistical analysis of option pricing. The book’s message was that a new field had been established based on rigorous scientific analysis with an established and rapidly growing set of results. Moreover, a high proportion of those involved, including Cootner himself, now a Professor at the Sloan School, had connections with MIT. Samuelson, although he had just one paper in the volume, was, thus, at the core of this new field.\textsuperscript{26}

By this point, although Samuelson had been thinking seriously about finance for at least a decade, it was a field in which his publications were not commensurate with his reputation. He presented his paper on “Economic Brownian motion” at several universities, including MIT, Carnegie Tech and Yale.\textsuperscript{27} At Yale, it inspired a graduate student, Case Sprenkle, to work on option pricing using the model of proportional price changes that Samuelson advocated, but it remained unpublished.\textsuperscript{28} Aside from the chapters in his textbook, his only publication on the subject was an article in a German journal, \textit{Weltwirtschaftliches Archiv}, “Intertemporal price equilibrium: a prologue to the theory of speculation.”\textsuperscript{29} A long survey of more than 40 pages, this article began, like his textbook, with the relation between prices

\textsuperscript{23} Ibid., p. 26.
\textsuperscript{24} Alexander, 1964.
\textsuperscript{25} Cootner, 1964.
\textsuperscript{26} Samuelson’s paper is discussed below.
\textsuperscript{27} Samuelson, 1965b, p. 13.
\textsuperscript{28} MacKenzie, 2006, p. 120. Sprenkle, 1961, reprinted in Cootner’s volume, was a chapter from his 1960 PhD dissertation.
\textsuperscript{29} Samuelson, 1957. I have been unable to ascertain why it was published in that journal.
in different places before drawing the analogy with the behavior of prices over time. A significant part of the paper was a more rigorous, although still verbal, account of material covered in *Economics*. He explained that there were many futures prices, and, making simplifying assumptions, showed how prices could be analyzed using supply-and-demand diagrams. It was, however, only a “prologue” to a theory of speculation in that it confined attention to the case of certainty. Even without introducing uncertainty, there was much about intertemporal price relations that needed to be understood.

This was the background for two papers published in 1965 in the Sloan School’s *Industrial Management Review*. The first, which had been included in Cootner’s volume the previous year, was “A rational theory of warrant pricing.”\(^{30}\) Samuelson began by describing his paper as “a compact report on desultory researches stretching over more than a decade,” claiming that he had been lecturing on the results since 1953.\(^{31}\) After summarizing the theory, he explained that, despite the modifications he and others had made, it was still unsatisfactory. The main problem was that the theory implied that warrants would never be converted into common stock and that the price of a perpetual warrant should be exactly the same as the value of the underlying stock. It was easy to cite examples where both of these results were not true. His method was to start by assuming that the current price of a stock was known, along with all past values, and that future values were uncertain, described by a probability distribution.

Taking account of the possibilities for arbitrage, Samuelson then worked out some limits on the price of a warrant—a contract that gave the right to purchase a unit of stock at a specified price over some period. When the warrant was about to expire, it would be worth nothing unless the stock price was higher than the exercise price; if it were higher, then its value was the difference between the stock price and the exercise price. At the other extreme, a warrant with no expiration date (of infinite duration) could never be worth more than the corresponding stock because owning a share gives one the option to sell it to oneself for nothing. In between, the value of a warrant should never fall when the duration of the warrant increases because the holder can exercise it earlier if he or she chooses to do so. This was all that theoretical arguments about arbitrage could establish. Going beyond this required making

\(^{30}\) Samuelson, 1965b.  
\(^{31}\) Ibid., p. 13. His papers contain one unpublished paper on the topic, unfortunately undated.
assumptions about probabilities. Here, he made the assumption that share prices were described by a Markov process in which future prices depended on just the current price and that past prices could be ignored, and he took into account the constraint that stock prices could never be negative. To move from the stock price to the price of the warrant, he introduced what he called the “axiom of mean expectation.” This statement stated that if the value of a stock were expected to grow at rate $\alpha$, then the value of the corresponding warrant had to grow at least at rate $\alpha$; otherwise, it would not be worthwhile to hold it. If the stock paid dividends, then, as warrants paid no dividends, the rise in the price of the warrant, $\beta$, had to be higher than $\alpha$ to compensate. Given these three pieces of information—$\alpha$, $\beta$ and the probability distribution of the stock price—it was possible to derive the price that a rational investor would pay for warrants of different durations.

For the rest of the paper, Samuelson focused on working out what he described as “intuitive” explanations of what was going on. In proving the results, he drew on results that an MIT mathematician, Harry McKean, proved in a paper published as an appendix to Samuelson’s. Its title, “A free boundary problem for the heat equation arising from a problem of mathematical economics,” reminded readers that solving such problems required mathematics to be more familiar to physicists than to economists.

Samuelson’s second paper carried the self-explanatory title, “A proof that properly anticipated prices fluctuate randomly.” He summarized the argument in his opening paragraph:

“In competitive markets, there is a buyer for every seller. If one could be sure that a price will rise, it would have already risen.” Arguments like this are used to deduce that competitive prices must display price changes over time, $X_{t+1} - X_t$ that perform a random walk with no predictable bias.

He asked whether this was a correct fact about well-organized commodity markets or stock exchange prices and whether this was a refutable hypothesis about actual markets or something that, like Pythagoras’s theorem, must be true. Doubts arose because share prices had risen at approximately 5 percent a year for many decades, and there were predictable annual patterns in wheat prices between one harvest and the next. It was only after spending a page and a half motivating the paper that he

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32 Samuelson, 1965a.
33 Ibid., p. 41.
turned to a mathematical model, claiming that, although he could deduce a fairly strong theorem, empirical analysis was necessary to determine whether his assumptions described reality. “I shall be content,” he argued, “if I can, for once, find definite and unambiguous content to the arguments of the opening paragraph [quoted above]—arguments which have long haunted economists’ discussions of competitive markets.”\textsuperscript{34}

The key assumption in Samuelson’s mathematical model of prices was that the probability distribution of a price one period ahead did not change, that is, given the current price, the same probability distribution could be used to determine what the next period’s price was likely to be. The price two periods ahead was found by applying that distribution twice, giving a distribution that was necessarily more spread out. Three periods ahead involved applying it three times. He then assumed that the futures price was determined by competitive bidding so that no one expected to make either a profit or a loss. Because he abstracted from risk-aversion and interest payments, this implied that the futures price must equal the price expected to prevail when the contract matured. Using these two assumptions, Samuelson showed that there was no way to make an expected profit by extrapolating past price changes into the future: “The market quotation [the futures price] already contains in itself all that can be known about the future and in that sense has discounted future contingencies as much as is humanly possible (or inhumanly possible within the axiom of the model).”\textsuperscript{35}

The theory begs some important questions. Who held the expectations assumed by the theory? Were they the expectations of some “representative individual”? Or were they the result of a “necessitous compromise” between people with divergent expectations? Given that people hold different expectations—that must be the case if trade in financial assets is to take place—this means that the application of the theory is not clear. The difficulty in relating the theory’s assumptions to reality also meant that the normative implications of his analysis were strictly limited. Samuelson wrote,

\begin{quote}
One should not read too much into the established theorem. It does not prove that actual competitive markets work well. It does not say that speculation is a good thing or that randomness of price changes would be a good thing. It
\end{quote}

\textsuperscript{34} Ibid., p. 42.  
\textsuperscript{35} Ibid., p. 44.
does not prove that anyone who makes money in speculation is *ipso facto* deserving of the gain or even that he has accomplished something good for society or for anyone but himself. All or none of these may be true, but that would require a different investigation.\textsuperscript{36}

Given that Samuelson’s earlier work had referred to an “economic Brownian motion,” it is important to note that his theorem did not show that financial markets would necessarily exhibit such behavior. It rested on a weaker assumption: that the expected change in the futures price might always be zero, but the degree of uncertainty might vary with the length of the contract.\textsuperscript{37} Unlike true Brownian motion, economic series would not wander indefinitely far from their starting point.

**Merton, Black-Scholes and option pricing**

In 1967, Robert C. Merton, the son of Samuelson’s friend Robert K. Merton, the sociologist of science, enrolled in his graduate class in mathematical economics at MIT. Merton (junior) had studied engineering at Columbia before going to Caltech for graduate work in applied mathematics, with a taste for mathematics that was relevant to dynamic engineering problems: partial differential equations and the calculus of variations. Since childhood, he had been fascinated by investments, and at Caltech, he regularly went to a brokerage house at 6:30 a.m., watching stocks and trading, before starting classes at 9:30. He traded stocks, convertible bonds, warrants and over-the-counter options. This drew him into economics, not just to understand finance but because of a belief that economics could make a difference in people’s lives. However, Caltech did not offer a PhD option in economics, so he applied elsewhere, and MIT accepted him because Harold Freeman thought it worth taking the chance that Merton’s knowledge of mathematics would prove useful. Learning economics from Samuelson’s *Foundations* and drawing on the dynamic optimization techniques he had studied at Caltech, he wrote a term paper on optimal growth theory with endogenous population changes that was published two years later.\textsuperscript{38}

\textsuperscript{36} Ibid., p. 48.

\textsuperscript{37} Brownian motion would require that successive changes in the futures price were statistically independent; his theory merely required that the correlation between them, given knowledge of the first price, be zero. Ibid., p. 44. The difference between these two conditions is that relationships may be nonlinear (see Merton, 2006, p. 15).

\textsuperscript{38} Merton.
Given the close alignment of their interests, it is not surprising that Samuelson’s collaboration with Merton extended beyond supervising his thesis. In the Spring of 1968, Samuelson hired Merton as his research assistant, and over the summer, they extended the theory of warrant pricing Samuelson had developed in 1965. Samuelson had had in mind developing his earlier ideas on warrant (option) pricing, probably because of a recently published article by Sheen Kassouf that, in the course of testing the theory, asked why the additional return needed to compensate investors for the additional risk involved in holding warrants rather than the underlying stock should be constant through the lifetime of the warrant. More fundamentally, Samuelson ignored the fact that returns on shares and warrants depended on the behavior of all market participants. To take this into account, it was necessary to construct a theory of how prices were determined, which meant a theory of supply and demand, period by period. This was the point at which Merton’s knowledge of mathematics would be invaluable.

The result was “A complete model of warrant pricing that maximizes utility.” On one side of the market, there were firms selling stock to raise funds to invest in risky production processes. On the other side of the market, demand for stock was the result of investors’ decisions about holding stock (risky because the returns on investment were uncertain) and cash (or any completely safe asset). Decisions about how much stock to hold were modeled by assuming utility maximization over an infinite time horizon. As Samuelson had done in his earlier papers, they assumed that stock prices followed a geometric Brownian motion, rising or falling at a given percentage rate at each moment in time and that arbitrage arguments were used to determine the price of warrants (options to purchase stock at a given price at any point in a given period of time). Having derived a formula for stock prices over time, Samuelson and Merton could then derive prices for warrants. The return on equities needed to be higher than the return on the safe asset (cash) to compensate for the risk, and the return on warrants—perfectly correlated with the return on equities, but riskier—had to be even higher.

Unlike in Samuelson’s earlier paper, the higher return from holding warrants in relation to equities was derived from the behavior of risk-averse, utility-maximizing

41 The utility function was assumed concave, implying that investors were risk averse.
individuals rather than being assumed. It was a complicated intertemporal optimization problem that could be solved using the techniques Merton had learned at Caltech and after. Although they did not do this, it would have been possible to use their formula to compute tables to price warrants for different assumptions about the volatility of stock prices and their trends.

The paper was given an unusual level of publicity because, that October, Samuelson was scheduled to give the first paper for the newly established Harvard-MIT Joint Seminar in Mathematical Economics.

It was to be a big event, held in a special room in the Holyoke Center at Harvard, and Samuelson’s name was the featured draw on all the publicity notices. All the big names would be there—Kenneth Arrow, Wassily Leontief, Zvi Grilliches, Robert Dorfman, Hedrik Houthakker—and important faculty would be visiting from other area universities as well. As a further mark of the event’s special status, no students would be allowed to attend—no students, that is, except for Robert Merton, who Samuelson arranged to give the talk in his place, but without telling any of the organizers.

When the moment came, after the requisite ceremony and introduction, Samuelson stood up at one end of the long conference table and spoke. “This is a joint paper, and my coauthor will present it. I’d like to introduce him as a professor, but he is not a professor. I’d like to introduce him as Doctor, but he has no PhD. So I’ll just introduce him as Mr. Robert Merton.”

The paper was then published in MIT’s student-run *Industrial Management Review*. This was a complicated paper and, somewhat unusually, after alerting them to the complexity of the analysis, the editor wished readers “Good luck” as they set out to read it. The justification for publishing such a mathematically complex paper was that theory could be applied not only to warrants but also to other types of securities.

When Merton graduated, Samuelson nominated him to be a Junior Fellow at Harvard, the position he had himself used so productively over thirty years earlier. He wrote to Wassily Leontief,

I know a dozen economists who could write a paper as good as my own discrete-time paper that appeared in the same issue as Merton’s. However, I

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42 Mehrling, 2005, p. 123.
don’t know two—I’m not sure I know one—who has the analytical ability to do the Wiener Brownian motion continuous-time case, which Merton has successfully solved. The results are beautiful and beautifully simple.⁴⁴

Lest Leontief thought he was exaggerating, Samuelson continued that he reproached himself for having been insufficiently enthusiastic about another student, Joseph Stiglitz, a few years earlier, saying that Stiglitz had turned out to be “about the hottest thing going.” Merton’s application for a Junior Fellowship was, however, unsuccessful, and in 1970, he took a teaching position in the Sloan School, where his collaboration with Samuelson continued even more easily. In the same year, he completed his PhD dissertation, supervised by Samuelson, Analytical Optimal Control Theory as Applied to Stochastic and Non-Stochastic Economics.⁴⁵ As its title suggested, the five essays that made up the thesis were linked by concern with intertemporal choice, covering consumption and portfolio choice, optimal economic growth and warrant pricing, all topics in which Samuelson had a close interest.

Although such work was still not widely accepted as central to economics, there was an active interest in it from both academics and finance practitioners, including two young economists, Fischer Black and Myron Scholes. Black had a PhD in applied mathematics from Harvard, after which he ended up working for the consulting firm Arthur D. Little based in an office adjacent to the MIT campus. There, he developed an interest in finance. In September 1968, he met Myron Scholes, who had just obtained a PhD in finance, supervised by Merton Miller at Chicago. Scholes had just been appointed to the Sloan School, and on the advice of a fellow graduate student, he contacted Black as soon as he arrived. They found a common interest in finance but had complementary skills, Black being focused on theory and Scholes on data analysis. Through a consulting company Black established in 1969, they became involved in a series of consulting projects related to portfolio management and the Capital Asset Pricing Model (CAPM).⁴⁶ At some point, they became interested in warrant pricing, perhaps through hearing about the Merton-Samuelson paper, and by June 1969, Black had an equation that, had he been able to solve it, would have yielded a formula for pricing warrants.⁴⁷ That autumn,

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⁴⁷ Black (1989) mentions the Merton-Samuelson paper but not the point at which
he discussed the problem with Scholes and, relying on intuition as much as advanced mathematics, they managed to work out a solution. The key was seeing that if an appropriate mix of warrants and underlying stocks were held, it would be possible to eliminate any risk associated with changes in the market, meaning that the expected return should be the riskless rate of interest. They presented their paper at a conference at MIT, sponsored by Wells Fargo, in July 1970, at which Merton learned about their formula. Given his background, he naturally approached the problem not through CAPM but as an intertemporal optimization model. Because he worked with continuous time, portfolio adjustments could be made continuously, meaning that perfect hedging was possible, leading him to the Black-Scholes formula.

Earlier in the summer, working as a consultant for a Southern California bank, Merton had come very close to solving the problem; had he considered the continuous-time version of a model he was working with, he would have derived the Black-Scholes formula but, not seeing the crucial arbitrage condition, and not realizing the significance of the problem, he had not found it. He later sent Samuelson a copy of the report on warrant pricing he had written for the bank, dated September 1969, saying:

"PAUL, I found this in the same folder ... perhaps my first serious consulting job ... the formula on Page 4 (for the clearly ad hoc “equal yield” model) implies the Black-Scholes formula [in the limit of continuous time]. ... Fun for a graduate student. Bob" 48

However, when he heard about the formula derived by Black and Scholes, he did not believe it was sufficiently general, but at some point in August, he told them they were correct. In a version of their paper dated August 1970, Black and Scholes concluded, “Robert C. Merton has developed the same formula as ours, starting from somewhat different assumptions. The knowledge that his formula agrees with ours gives us greater confidence that we haven’t made any substantive errors along the way.” 49

What happened after the formula had been worked out by Black and Scholes and he read it. Mehrling (2005, p. 127) mentions it as a possible source of Black’s interest in the subject.

48 R.C. Merton, Letter to Paul A. Samuelson, 1998. PASP, Box 52 (Merton, RC); eclipses and brackets are in original.

49 Quoted in Mehrling, 2005, p. 132.
by Merton illustrates how the theory of finance was still considered marginal to economics. Much of the literature was published in unrefereed journals and conference volumes rather than in journals read by most economists. Black and Scholes submitted their paper first to Chicago’s *Journal of Political Economy*, only for it to be rejected immediately as too specialized. It was then sent to Harvard’s *Review of Economics and Statistics*, with the same outcome. Neither journal considered it worth sending to referees. It was accepted by the *Journal of Political Economy* only after Merton Miller and Eugene Fama, two central figures in the emerging finance literature, persuaded the editor to take the paper more seriously. Both papers were eventually published in 1973, Merton’s in the *Bell Journal*.50

Samuelson’s work, in 1965, was the starting point for much of this research, and Merton later described it as a “near miss,” having brought the problem very close to a solution, not that having a “near miss” counted for much for Samuelson.51 In October 1971, Samuelson gave the John von Neumann lecture at the Society for Industrial and Applied Mathematics, held at the University of Wisconsin. For his audience of mathematicians, his lecture surveyed the many mathematical techniques used to determine prices. The title of his lecture, “Mathematics of speculative price”, was defended with the rhetorical question, “Is there any other kind of price than ‘speculative’ price?”52 The published version was dedicated to Jimmie Savage, who had died in November, and included an appendix by Merton covering continuous-time processes. In another paper published the same year, Samuelson followed up his classic paper showing that properly anticipated stock prices should fluctuate randomly, with a paper showing that “properly discounted” prices should “vibrate randomly.”53 This was a theoretical argument, but it had strong practical implications concerning the investment strategies that people should pursue. It was widely held that when people were young, they should be willing to accept more risk than when they were older, the reason being that they had more time to recoup any losses. Given that equities are generally riskier than bonds, this means that younger people should hold more of their portfolios in equities.54 Samuelson’s

50 Merton, 1973, and Black and Scholes, 1973. The role of the formula in the rise of options trading has been extensively discussed. See, for example, Mehrling, 2005, and MacKenzie, 2006.
51 Siegel, 2019, p. 309.
54 This ignores the problem of “junk bonds” paying high interest because the probability of default is high.
theoretical results show that this argument was fallacious. Intuitively, the reason is that although a long time horizon does mean that the proportion of years when equities beat bonds is likely to rise, a long horizon also opens up the possibility of a huge underperformance; however long the time horizon, equities can never be guaranteed to beat bonds.\textsuperscript{55}

\section*{Concluding remarks}
MIT, and in particular the Sloan School, became one of the main centers of the emerging field of finance. There was a core of economists in both the Sloan School and the Economics Department who enjoyed proving that they were bright enough to beat the market. Samuelson himself was very successful in such activities. Such activity was an embodiment of the MIT ethos. Not only was it part of the MIT philosophy to foster close relationships with outside companies (there were, for example, many representatives of engineering companies on its governing body), but beating the market was also their equivalent of solving seemingly impossible engineering problems. As Samuelson repeatedly stated, the theory of efficient markets did not mean that it was impossible to beat the market. What it meant was that, although profits could be made, there were no \textit{easy} pickings to be had. This view was entirely consistent with smart people at MIT (including himself) being able to beat the market, just as MIT engineers could solve problems that defeated people who were less smart and did not work as hard. It was also consistent with the position widely taken by MIT economists that, although markets were important, they were often imperfect.

Perhaps more important, the approach to finance of Samuelson and his colleagues was consistent with the approach to engineering that had come to dominate MIT with its transition, in the 1930s, from a school giving training in practical techniques to a place where scientific knowledge was systematically brought to bear on practical problems. This explains why, although MIT economists were concerned with concrete problems in finance, and using their knowledge to beat the market, they devoted much of their effort to developing “pure” theory. Problems were to be solved by bringing scientific economics to bear on them, in the same way as mechanical engineers drew on physics, or chemical engineers on theoretical chemistry. Of course, MIT was not unique in having such an attitude, but

\textsuperscript{55} These remarks draw heavily on Siegel, 2019, Sections 9-11.
its commitment to such an approach explains why it could become one of the main institutions in which the theory of finance was developed.

More than this, the mathematics needed to understand markets—theories of Brownian motion and stochastic processes such as Martingales—had a clear MIT provenance through Wiener. Having become familiar with Wiener’s work soon after arriving at MIT, Samuelson immediately saw its relevance when he read the work of Kendall. Whether he could have developed suitable theories on his own, Samuelson’s task was made much easier by being able to work with a series of MIT students, including Kruizenga, Cootner and Merton, who shared his interests and commitments to mathematical analysis. He could also draw on the expertise of colleagues in MIT’s mathematics and natural science departments.

MIT was certainly not the only place where modern theories of asset pricing were developed—the example of Black-Scholes alone is enough to establish that. Neither was efficient-market theory unique to Samuelson, having also been developed at Chicago by Eugene Fama. However, what can be established is that the culture of MIT, very much that of an engineering school, contributed to creating an environment that proved extremely conducive to such work.

References


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