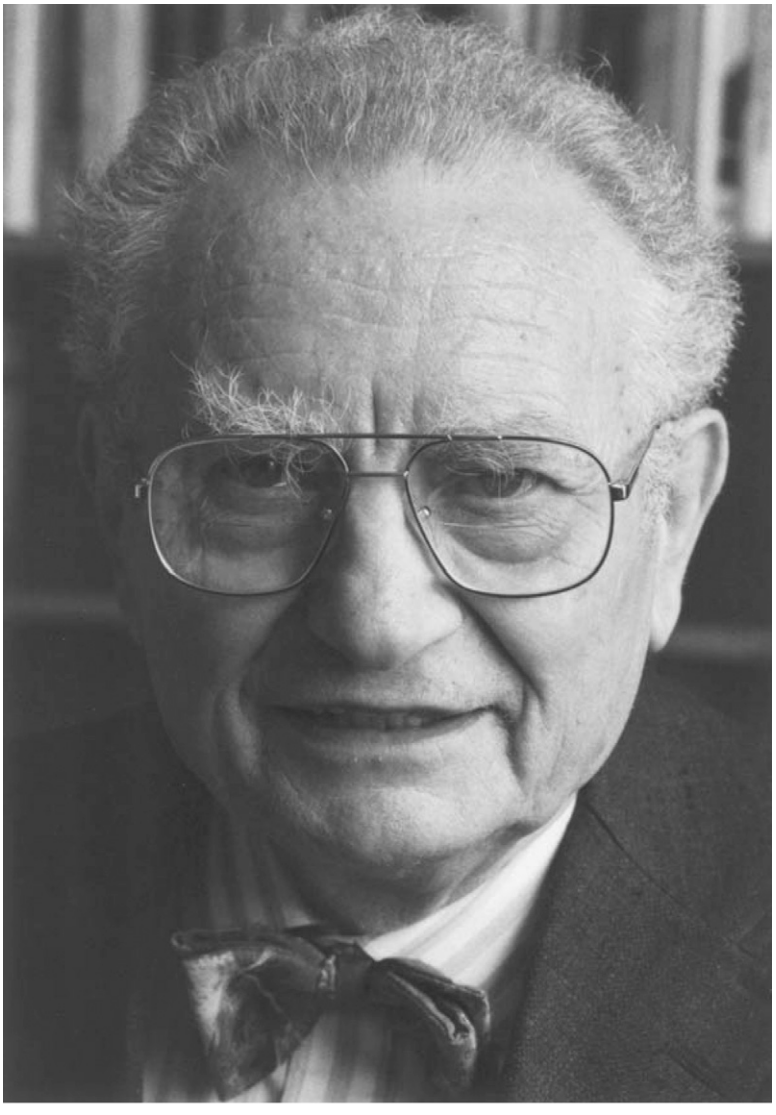

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An Enjoyable Life Puzzling Over Modern Finance Theory

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Abstract

This is a terse account of group creation of modern finance theory; and a sampling of my prosaic autobiographical investing and consulting for nonprofit academies. Eschewing 1900 Bachelier and 1905 Einstein white noise randomness, my martingale version of market *micro* efficiency invoked no violation of economic law. My attempts to establish pricing theory for options fell a bit short of the Black-Scholes-Merton Holy Grail. For life cycle investing, mathematicians' maximum growth Kelly criterion was debunked, as were vulgar notions that necessarily riskiness is averaged downward for long-term investors. Popular Markowitz-Tobin quadratic programming was shown to hold generically only for smallest price variations or for unrealistic risk-aversion functions. Because economic history at best obeys only quasi-stationary probabilities, no sure-thing formulas will ever be definable. Excess returns—excess "alphas"—can result only from early new "insider" knowledge, however acquired—legally or illegally. Boo hoo.

An irrational passion for dispassionate rationality can take the pleasure out of life.

John Maurice Clark, c. 1920

Not for people like me.

Paul A. Samuelson

INTRODUCTION

In a too-kind summing up on my contributions to finance theory, Robert C. Merton counts that most of my best papers in that field were done after I'd reached the age of 50. Since he counts cards well, he must be right. My ego does not object to being linked with such Methuselah masters as Verdi.

But actually my maiden 1937 publication, "Some Aspects of the Pure Theory of Capital" (Samuelson 1937), written just when becoming eligible to vote, was on present-discounted value of future cash receipts. At that time intertemporal capital theory, still the Mount Everest of economic science, was already one of my principal preoccupations.

In more than one keynote lecture I have remarked that modern frontier finance theory dates back to one lifetime. My innuendo was not that merely by my being alive at the creation I was that theory's progenitor. A true history of this subject nicely confirms *père* Robert K. Merton's thesis that scientific advances are almost always a *group* creation. This documents that Newton-Leibniz, Darwin-Wallace, and Joule-Mayer-Helmholz-Kelvin-Clausius-Gibbs *group* discoveries are the rule and that Carlylean unique geniuses are the exception to the rule.

When I once called myself a "Sunday painter" dabbling in stochastic finance, that was not meant to belittle finance theory as a branch of serious economic theory. Such a peculiar view was expressed again and again by the late Milton Friedman, a dizzy view that I still find incomprehensible.

In this meandering account of my travels in the land of finance, I would like to be mostly camera. I was at the right places at the right times and had a front-row spectator seat. However, as happened so often in the seminar or lecture hall, I couldn't stay out of the act. (At Quaker meetings, one talks only when God tells us to.) No Quaker I, but something often provoked me into interruption.

Nabokov-like, I here say to myself: Speak memory. Eschew terse elegance and proof and false bravado. So, readers beware.

THE BEFOREMATH

I was reborn, born as an economist, at 8:00 a.m. on January 2, 1932, in the University of Chicago classroom. But you might say that unusually interesting economic history had earlier already impinged on me prenatally.

During my first few years of life, the heavy spending by the World War I belligerent nations brought 12-h, seven-days-a-week shifts at U.S. Steel's massive new Gary, Indiana plant. Middle European Slavs, riding their bikes to work, are still part of my Freudian memories. Etched there, too, is the memory of the postwar 1919–1921 recession. And I can dimly recall imported Mexican workers emerging from Gary's many rail stations to do their strike-breaking duties.

Of course, I took for granted my family's relative affluence from the World War I spendings, which after Keynes's (1936) classic *General Theory*, we came to call "multiplier spending and respending." Then at age 10 I learned the downside of boom and bust. My family achieved a small fortune in Florida real estate the hard way: they started out with the proverbial larger fortune. Before reaching my teens, I played cops and robbers on the Coral Gables acres populated by half-built homes destined never to be completed. In the 1929–1939 period, both at Chicago and Harvard, we called such abortions "Hayeks," after Austrian Friedrich von Hayek (1931) who wrote about such stuff.

To hurry on, I skip to early 1929. My young freshman algebra teacher perused the financial pages after class. Too often a teacher's pet, I lingered on with her. Together we selected as winner stocks Atwater Kent radios and Auburn Motors. I shared the choosings; she bore the losses after the October Wall Street Crash.

In the early 1930s, while attending at Chicago the world's most revered conservative economics classes, I could not reconcile what I was being taught with what I observed in the world outside of academia. Four summers, up to my 1935 B.A. graduation, I spent on Lake Michigan's sandy beach. That was not because I was lazy. (I *am* lazy when it comes to doing things I find boring.) Nor was it because my family was filthy rich. A job, any job, would have been most welcome then.

But I learned from observing the experiences of my Chicago classmates. Some of them would apply to more than a hundred places for any kind of a summer job. Zero acceptance was their report to me. So I never felt a twinge of guilt. However, if I had a Boswell, he might conjecture that I would probably become a likely convert to a John Maynard Keynes when and if he pioneered a new disequilibrium economics. Actually, I became a cafeteria Keynesian in the years before Keynes's 1936 *General Theory*.

In summing up, it can be said that I became infatuated and obsessed with economics as a lifetime study in good part because in my first two decades of life macroeconomic factual history was so turbulent and nonoptimal. I presume that many a doctor felt similarly called to medicine because of contemporary plagues that robbed so many folk of their three-score-and-ten longevity.

SOME EARLIEST PERSONAL FINANCIAL EXPLORATIONS (1935–1955)

Curiosity—scientific curiosity—motivated my varied early personal investing. I was not averse to earning a good net risk-corrected total return. But I, and after 1938 wife Marion Crawford and I, were comfortably off on our joint earnings. Thus, essentially the sole reason I started my research into the pricing of put and call options came from my (mistaken!) conjecture that the difference between the market prices of puts and calls would give the macro researcher some exact objective measure of whether investors were becoming expectationally more bullish or more bearish. (Later I developed with a thesis advisee a two-component vector calculus which proved that arbitragers would, under ideal conditions, keep like puts and like calls *approximately equal* (!) in price.)

(Approximate equality of put and call prices for puts and calls needn't rob options of any useful prediction hypotheses. Thus, back in October 1987, when the Dow index fell the fastest ever in one day—22%!—the Vix index of implied volatility earlier soared. That led one economic historian to declare that such a rise is a good forecaster of declining stock prices. In the 1987 case, the spread of the new device of "portfolio insurance"

did contribute much to the decline. However, that same economic historian doing due diligence could have found in the statistical record cases where exogenous increases in *bullishness* [rather than bearishness] could be the true cause of a spike up in the volatility index.)

In 1968 I wrote a critical review of the Thorp-Kassouf (1967) book *Beat the Market* (Samuelson 1968). My main dissent was against their suggestion of easy money from merger arbitrage. Though I had done well with the Packard-Studebaker merger, it had been a harrowing—not an easy—experience. Shorts were hard to execute; short squeezes were a chronic danger. When an acquaintance tried to sell me puts I warned him not to do so, thereby risking a "short squeeze." He regretted not believing me. I do recall a similar story from an earlier time involving Molybdenum Corporation warrants that were destined to expire worthless. But it sure was hard to keep them short, and there were short squeezes and short scares that the intrinsically worthless warrants might have to be purchased for delivery with more shorts outstanding than warrants available to deliver. This fits the Thorp-Kassouf domain. I recall buying puts on the warrants, thereby giving up much of the potential profit, but realizing some of it.

That 1968 item somehow never got included in my *Collected Scientific Papers* (Samuelson 1966a, 1966b, 1972, 1977, 1986). Maybe that was because I regretted my harsh tone. But in retrospect I relish my nihilism that *no perfect hedge is ever possible*. You will ask, what about the Black-Scholes-Merton hedge for log-normal probability distributions? Even that always does depend on an *unknowable* correct volatility parameter. (My 1966 nihilism prepared me for the 1998 Long-Term Capital Management debacle *after* it happened, but not before. Hyper-leveraging, plus off-the-chart exogenous shocks, trumped normal expectations, I think. But we may never know.)

During my 1935–1940 Harvard years I enjoyed a lot of leisure time. That was because, as I learned to my surprise, I was better prepared than the 50-odd other Harvard graduate students for the reason that at Chicago I had taken far more economics courses than my fellow students had.

Then, at the economics headquarters, where Harvard's house journal—the *Quarterly Journal of Economics*—had its office, there was spread out on a big table current exchange magazines from *The Economic Journal*, the *Journal of Political Economy*, *Economica*, *Econometrica*, the *Review of Economic Studies*, etc. Foolishly, I devoured each and all of them. So that gave me a jump start in understanding the early finance publications by Alfred Cowles, III, Holbrook Working, and other statisticians.

Many traders I have known started out in their idle moments doing thought experiments with successive days' financial pages. When these seemed to work out in their favor, they began to frequent brokerage houses and various exchanges. Of course, often they learned that what seemed to work out on paper, with "virtual" rather than "real" money, could be a bit misleading. When you want to sell short, you have to wait for an uptick in the stock's price. Sometimes you are at the end of a long queue, and either you never can make the trade or you can make it only *after* your discerned profit opportunity had already disappeared.

Early in my marriage to Marion Crawford Samuelson, she came into a moderate legacy. That needed to be cannily invested. 1938 was a pretty good time to begin. We were young. Our elders were burned so badly in the 1929–1932 years as to leave for us some good opportunities: corporate shares were available with seven-to-one price/earnings ratios, along sometimes with an additional 5% or 6% dividend yield.

My knowledge of orthodox and Keynesian macro theory perhaps provided a further small edge (not to be exaggerated). Example: Hotels built in the 1920s could be bought in the mid-1930s at a price far below their reproduction cost. Two MIT grads, Ernest Henderson and Robert Moore, bought the Commander Hotel opposite the Cambridge Common for $5,000 and an assumed mortgage. That's how the Sheraton chain got started. Same with the Hilton hotels. Applying some scientific management and some refurbishing created good profit opportunities. Later, when hotels had to be built new, Marion and I moved on to other investments.

Here is a second example, happy enough in terms of profitability but disillusioning for me as an idealistic do-gooder. My first MIT secretary revealed that she had invested in a saving plan which paid a 2%–3% interest yield, but only if she *never* missed an installment payment. Since new U.S. Series E savings bonds matched and beat that deal, I realized that investing in the management of her kind of company was the way to go.

It would be a better world if the most ethical sectors had the best returns. Instead, when thoughtful people won't deign to invest in cigarette companies, irresponsible investors may be able to do well by being irresponsible.

Warren Buffett, in his later Buffett-Munger phase, has favored "franchise" companies. These, most often, are what economics lecturers call "oligopolies" rather than hard-sod competitors. Alas, monopolies, duopolies and oligopolies are not the ethical utopias that Adam Smith's Invisible Hand would create under most active competition.

If ever I hoped that my Wall Street winners would do best for the good of society, experience dashed such naive hope. A company that sold automatic cancer insurance to Japanese employees, but whose fine print precluded their paying out very many claims, was a profitable investment hard to beat.

EFFICIENT MARKET HYPOTHESES

From writings by Holbrook Working at Stanford and by statistician Maurice Kendall at the LSE, I understood that price changes of corn or pork bellies—spot prices or futures prices—when plotted as time series did look much like a truly random walk. (All this was before a postcard was sent by Jimmie Savage from Yale to a number of us economists asking if any of us knew a small 1914 book by a French mathematician Louis Bachelier, whose title had to do with a theory of speculation. I turned out to be his only respondent.) After I found in MIT's math library Bachelier's (1900) Paris Ph.D. thesis (supervised by no less than Henri Poincaré), I persuaded Paul Cootner (1974) to publish an English translation of this seminal work; Bachelier's work actually dominated Einstein's (1905) analysis of Brownian motion.

Working had reported that when he fabricated random walk data and plotted them along with actual historical Board of Trade price data, the locals at the Chicago Board of Trade he showed them to could not accurately guess which was which. (Art critics, when made to choose between kindergarten daubs, insane asylum artists, and *avant garde* art, did much better than those Chicago locals!)

My admired young friend, the late Dutch-American Hendrik Houthakker, was one listener at Kendall's Royal Statistical Society lecture. He and other economists there took offense to Kendall's innuendo that real-life price movements were chance data *not at all subject to any fundamental economic laws*.

My reaction differed: Work the other side of the street and argue instead that when many buyers and sellers, each with different information, come together in a competitive auction market, then absence of *ex ante* discernable predictive trends was a sign of economic law working at its best. Anything that was almost sure to happen would already have been made to happen. (That bred four of my needed follow-up articles about "efficient markets.") Again, as with Newton and Leibniz or Darwin and Wallace, multiple economists were arriving at similar new insights, independently and cooperatively.

One admired youngster was Eugene Fama at the University of Chicago. At Tufts University near Boston he had been a football player from nearby, concentrating with honors on Romance languages. Only a few years later Fama had become a Chicago full professor and a much-cited world authority. Independent of my writings on the efficient market hypothesis, Fama had provided his own conjectured hypotheses. (In my considered judgment, the Stockholm savants ought to have honored Fama to share with other worthies in the Nobel Prize in economics. The $300 million Booth gift to Chicago's graduate business school owed much to the Fama workshop, which in terms of some metric beats a Stockholm gold medal by more than a hundred to one!)

From the beginning I could not believe that the "efficient market" hypothesis was dependent on a pure Brownian motion white noise or any truly random random walk. Place a minuscule colloidal molecule on a horizontal table that covers unlimited acres. Bombard it from every direction with thousands of minute atoms; and then if you wait long enough that original molecule can have traveled a billion miles in one direction. That's truly a random Bachelier-Einstein walk, but not my notion of economic fluctuations.

Taken literally, a random walk dictates with certainty that in time the price of a luxury Rolls Royce relative to the price of one green pea can reach equality or any ratio you can name. I didn't even then know the name of a statistical martingale. But this was the kind of chance variation which would emerge when many different traders, possessed of different information (true or false), bought and sold stocks or bonds. Bachelier's math of 1900 and Einstein's math of Brownian motion might be hard to tell apart from martingales in short runs, but they were a different species in the finance zoo.

PEOPLE MAGAZINE SNAPSHOTS OF INVESTORS WITH LONG-RUN POSITIVE ALPHAS

Will Rogers once wrote (in my paraphrase), "Buy stocks that are going to go up. When they're going to go down, sell them!" Good work if you can get it. Only a few can.

Among economists John Maynard Keynes (1883–1946) did well over a lifetime. He had a high I.Q. and must have been a better-than-average bridge player. Apparently some of his triumphs in currency trading stemmed from his micro- and macroeconomic hunches. However, after scoring well on bets that Germany's postwar inflation would cause the mark to depreciate, he did go virtually bankrupt when for a few months the mark reversed from its down trend. A kindly City friend enabled him to avoid bankruptcy, a fate worse than death in the post-Edwardian Age. (Again in 1929 a number of people incurred losses when a Keynes-Robertson speculative fund did badly. I learned this from Lionel Robbins. However, in autumn 1932, German Professor Hans Neisser heard Keynes give a lecture at Cambridge, in which he said, "Right now is your lifetime opportunity. Borrow and beg to invest in diversified common stocks that are going to recover now that the pound has ceased to be over-valued." Not a shabby call.)

However, with my degree of risk intolerance, I would hesitate to put my entire nest egg in Keynes's hands only. Successes tempted him to leverage up. That means you have to be rarely wrong, a higher hurdle than to be mostly right.

I skip rapidly to Yale's Irving Fisher. This truly great American scholar, who married into a fortune, was the son of a poor Protestant minister. Around 1919 he earned for himself a bundle by inventing a visible Rolodex-like filing system. Then, as a 1920s Wall Street investor, he scored well until the October Crash brought margin calls to him that he could not meet. He became a virtual bankrupt.

Around 1931, Fisher's statistical measures convinced him that the Dow Jones Index had hit its bottom. He then advised the public to newly invest at what he believed had become bargain prices. Alas, that was a second bad guess. His wife's nest egg and his own were lost. His sister-in-law, who was president of Wellesley College, as an amateur Bayesian avoided letting Irving lose her fortune too.

Traders successful over a long time period, in my experience, generally do have a wholesome respect for risk. When things first go against them, their chosen model would say: "Expected profits will be higher still." However, for canny traders *the moment comes when they have to doubt their own model*. At that point they close positions and go fishing. (In 1998, the Long-Term Capital Management group apparently never did try to go fishing.)

I intrude into this section a brief account of David Ricardo's legendary advice to his friend Robert Malthus just before the 1815 Battle of Waterloo: "Invest now in British bonds, which will go up when Napoleon meets defeat at Waterloo." Though tempted, Malthus didn't care to do so risky a thing. (See Sraffa, 1951, volume 7.) Too bad for him. But why was Ricardo so confident? After the event, Britain's commander, the Duke of Wellington, said: The Waterloo victory was a close-run thing. Had the Prussian allies of Britain not arrived at the battlefield late in the day, things might have turned out differently.

Facts gathered by Piero Sraffa, Ricardo's biographer, seemed to be these. Ricardo did have an observer near the battlefield. He by fast horse brought the news to the nearest harbor where a packet ship was on the wait. So very early Ricardo in London did know the outcome, and did personally convey the news to the English government.

It is interesting how Ricardo reacted to the news. On his customary chair at the Exchange, he *sold* (!) British Treasury stuff again and again. The other traders saw this, and suspecting that he would know the true story, they joined in the selling. Then, suddenly, Ricardo reversed course and bought and bought. It was his biggest coup ever, and enabled him to retire from active trading and become a passive rentier investor for the rest of his life.

I have two reactions. If not illegal, an ethical purist would have to fault Ricardo for in effect profiting from his own spreading of false rumors. In this millennium that might be something to criticize or even to litigate about. Second, it could be the case that Ricardo was not depending solely on a Napoleon defeat. Instead, he could have made a shrewd bet that *volatility* would in any case spike up. And then he, on the exchange floor, could hope to nimbly benefit his own purse and Malthus's too.

Note that I have written naught about the legendary Rothschilds' role in pre- and post-Waterloo speculations. Actually, Ricardo put them in the shade. And truth to tell, as money lenders to the crowns of Europe, the Rothschilds might have privately prayed for a continuation of French and English wars.

EXISTENCE OF A FEW HARD-TO-FIND, EXPENSIVE-TO-HIRE MAGICIANS

Investors such as Warren Buffett are strong and rare exceptions to the efficient market dogma. My take on that topic boils down to something like the following.

There are no easy pickings in Wall Street. Still, some tenors are better than other tenors; some billiard sharks will win most of the time. Why not then expect that a scarce subset of speculators can enjoy a "positive alpha" during most of their active lives, meaning by those words risk-corrected extra returns as compared to 99% of the trading mob. Such talents are hard to find. And they don't provide their services on the cheap. Lastly, even those guys' "hot" hands often do turn cold.

I never considered myself to be an A+ trader. What I did become was a useful monitor of traders. I skip here my long years as activist charter investor and Board of Directors member for the Commodities Corporation in Princeton. Space does not allow me to go into that intricate story. I learned to carefully abstain from influencing successful traders by offering them my macroeconomic views. Most of such views were registered already in yesterday's and today's markets. Star traders—such as Bruce Kovner's Caxton and Paul Tudor Jones—somehow had the knack to go beyond what was already in today's financial pages. To maintain their positive alphas required a concentration that for me had to be devoted to *avant garde* scientific economic discoveries.

MICRO EFFICIENT MARKETS THAT ARE MACRO INEFFICIENT

Experience quickly teaches that, if it is common knowledge that General Motors will pay a $100 dividend per share to GM shareholders on August 15, the following caper will not work: 1. Shortly before August 15 buy GM shares; 2. collect their dividends on August 15; then 3. on August 16 sell those shares you recently bought; finally 4. presto you are a sure-thing winner. Of course not. Instead, in reality, past the date of receiving the dividend, each GM share will drop in price by the amount of that dividend. In words, markets do tend to be *micro* efficient. *Only when you know new correct news that others don't yet know can you capture easy returns in micro-efficient markets.*

Does that mean that every rise and fall in the indexes of most stock prices are rational reactions to knowable correct news? Not at all. The big cumulative swings in mean prices that economic historians document—as in 1929–1934 or 2007–2008—are well-known features of historic business cycles. Only those who are naive think it easy to be a successful "timer"—defined as one who sells out stocks just before the S&P 500 index will turn down; then buys back in only when stocks are at their nadir.

Study after study reports how dismal are the records of a thousand would-be timers. Maybe it is easier to sell near the top than to know when the bottom is near. When nonfalse recoveries occur, they tend to advance fast. Again, what makes *macro* efficiency impossible is the hard fact that economic history is at best *quasi-stationary* time series. That quasi kills all certainties.

DECISION-MAKING UNDER UNCERTAINTIES

Economics appealed to me because I enjoyed looking for theoretical understandings of empirical reality. The longer I studied and taught economics, the more dissatisfied I became with mere theoretical cleverness or high IQ's.

Even when I began to write more popular journalism, I tried never to leave my mathematical insights back at home. The good journalist, by definition of the word "good," has to be a good *contemporary* economic historian. He has to mobilize what is knowable about already past history into conjectures about what *future* economic history is likely to be.

Bob Solow has for more than half a century occupied an office adjacent to mine. When young he would say, if you don't regard probability theory as the most interesting subject in the world, then I feel sorry for you. I always agreed with that. However, probability with zero applications to the real world I found to be dull measure theory.

Much of the satisfaction felt by the researcher comes from making probability math the servant of policy choice. *Evidence-based* medicine is the jargon for what I am talking about. Early on I would confer with my internist, using words like the following. Deciding to take a cholesterol-reducing statin pill should be done by me in much the same way that I come to decide to buy General Motors stock and sell General Electric stock. (After my eightieth birthday, when we got what was my first dog, I used the same methodology when consulting his vet.)

One could ask, Why for the last several decades have you subscribed for and read the prestigious weekly *New England Journal of Medicine*? Did you regret not becoming an M.D.? There is no iota of such a regret. It is because modern medicine tries to examine—"with dispassionate rationality"—what alternative treatments may minimize risk and maximize cure that I selfishly indulge in such extracurricular reading. Many professors relax by playing chess or bridge. That stuff is too much like my own economic research to tempt me.

Honesty compels me to admit that "curiosity" often trumps utilitarian motivation. Thus, I've written quite a lot on basic similarities between the paradigms of competitive microeconomics and the paradigms of classical thermodynamics. These papers appeal to a small jury of curious scholars. And again, after my 1970 sojourn to Stockholm somewhat liberated me from the grindstone of economics, I wrote half a dozen biological papers on Mendelian dynamics. Always unsolved problems motivate trying to solve them. The reward is not dollar royalties; nor even oodles of citations. My thousandfold newspaper columns have nuances unappreciated by most of their million readers.

MODUS OPERANDI

I caution myself from enjoying any rush from trading triumphantly. When my math recommended wide diversification with low, low turnover, I ate my own cooking. Especially after John Bogle created the customer-oriented Vanguard Group, with its lean, lean management costs and low turnover, my time and energies got freed up to concentrate on week days on revealed preference and comparative advantage foreign trade theory.

You can't make bricks without sand and straw. *Value Line* was what I used as a convenient data source. Its evaluations of bargains and duds I deemed to be less useful than its database.

Also, despite my chronic risk aversion, my self-confidence (or over-self-confidence) led me, while eschewing margin loans, to lean toward the deliberate high volatility of leveraged firms. In effect, decades before the legendary Modigliani-Miller (1961) theorem was published, I was disbelieving in it because I knew that I could not borrow personally at interest rates available to successful corporations.

DIGRESSION ON THE MODIGLIANI-MILLER MISUNDERSTOOD THEOREM

Top MIT graduate students would too often write exam passages like the following: Modigliani-Miller have proved that any and every corporation can *indifferently* employ much or little positive leverage and much or little negative leverage. *Ergo*, societies of rabid risk-relishers will end up with the same degree of risk taking as societies of paranoid risk averters.

This is quite wrong. What Modigliani-Miller asserted boils down to is only a weak tautology: Under idealized conditions where everyone can borrow or lend at the same interest rate, whenever I am interested in a particular corporate activity, no matter what degree of (algebraic) leveraging it has chosen, I can, by my own borrowing or lending, *offset exactly* whatever I don't like about *its* choices.

Here is briefest proof of my take on what will be a society's optimal degree of risk taking. Crusoes on two different islands can harvest the corn they live on by each choosing between two different known technologies. Technology A grows *with certainty*, rain or shine, 1^{A} bushels of corn per unit of land; Technology B harvests 4^{B} or $\frac{1^{B}}{4}$ of corn, at even 50-50 odds depending on whether the season is rainy or dry, respectively.

"Dan" Crusoe on his island will (rightly) choose to put half his land acres into B and half into A because Dan agrees with Daniel Bernoulli (1738), whose Laplacian utility function was *logarithmic*: $\text{U(Corn Crop)}^{\text{Dan}} = \log(\text{Corn Crop})$. However, "Gabriel" (Kramer) Crusoe on his island is *less* risk averse. His Laplacian utility function is $\text{U(Corn Crop)}^{\text{Gabe}} = \sqrt{\text{corn crop}}$. Baby calculus shows that Gabe Crusoe will put *all* his land into the riskier B technology. (Readers can confirm by use of freshman calculus those two clashing degrees of diversification.)

Only by misunderstanding what Modigliani-Miller concocted could readers doubt that it matters much to Dan on his island, and much to Gabe on his, for each to get only *his* desired best degree of optimal diversification. The same holds for divergent societies made up of many Dans only or of many Gabes only (QED).

In an article dedicated to the great Harry Markowitz on an occasion for his eightieth birthday (Samuelson 2009), I show that 500 Dans will end up with equilibrium contracts with 500 Gabes in Arrow-Debreu complete markets. Gabes will give Dans specified amounts of corn in the bad dry seasons in return for receiving from Dans specified amounts of corn in good wet seasons. This Arrow-Debreu Pareto-optimal result raises society's use of risky production and thereby (nonparadoxically) improves everybody's certainty-equivalent levels of well beings.

BACK TO EARLY PAS INVESTING

Value Line did not purport to help me separate winners from losers in the put-and-call option markets. There, for data, I relied on a Fried letter that cost a couple of hundred dollars per year. It would say things like: Buying the calls on the RKO Corporation warrant will yield a 1000% profit, etc. Looking into the future, Fried might tell me: If stock A rises by 10%, a call on A will rise by 22%; but if A drops by 20%, the call will drop by only 10%. If you can believe that sort of thing, I have a good bridge to sell you at a bargain price.

One of my MIT students would borrow my Fried letter periodically. Once he said to me, "Why do you waste your money? That dope sheet is rarely right." Unfazed, I replied,

"Actually, if I get one or two good ideas, they're worth more than a few hundred bucks." Later I regretted my overly quick response. The next day I corrected the previous day's reply: "If I get one or two good ideas *net*, that's money in the bank."

Most hot tips in *Barron's*, *The Wall Street Journal*, or *Forbes* lead to fool's gold, not the real stuff. Bombarding yourself with lots of plausible stories is guaranteed to accelerate the turnover of your portfolio and help pay for your broker's yacht. As true today, in the twenty-first century, as it was in the mid-twentieth century.

NO SURE THINGS?

From 1938 to 1941 Pearl Harbor, one could smell an upcoming World War II. Surely Boeing Aircraft stocks would be both safe and extra profitable? But no. For whatever reason, in that period of run up to U.S. participation in World War II, one could, and did, register losses from Boeing stocks. Go figure.

This brings back to mind the crucial already mentioned truth that "economic history obeys no *stationary* probabilities." *No* means none at all. At best, the keenest trader is faced only with quasi-stationary approximations.

The good traders I've known were not necessarily adept at advanced mathematical probability. But still they did have a wariness against risk. A trader could have a hot hand for a lengthy period of time. However, when changing reality deviated from the hunches that had been utilizing so well, the hot hand could turn cold—sometimes permanently cold.

Experience confirms that successful traders cannot explain in Wharton School seminars how you or I can be good traders. Example: Amos Hostetter, Sr., commodity broker and trader, could score positively in say all of 40 years. This encouraged Princeton's Commodity Corporation to assign to him a Boswell, who would write out the Hostetter wisdoms in a privately printed book. Alas, that Boswell remained the mediocre trader he had previously been.

I suggest a second example. Warren Buffett has been no slouch as an investor. His explanations are simple. You look for and find a good company—say *The Washington Post*. If your chemistry and theirs are compatible, you quickly shake hands. And, as it says in *Brothers Grimm* or *Mother Goose*, all of you then live happily and profitably forever after.

When I hear this, I pinch myself and remind myself that Joseph Schumpeter at Harvard brainwashed me into believing that there are no permanently "good franchise" companies. (In fairness to Buffett, he translates "permanently hold" into "hold for a long time.")

STINT AS BOARD DIRECTOR AT ADDISON-WESLEY PUBLISHERS

MIT before World War II was the biggest technology university, and arguably MIT and Cal Tech were world leaders in engineering and sciences. However, after and during World War II, MIT got co-opted into a physical and biological sciences powerhouse.

That's why it was natural for me to buy some shares in the new Addison-Wesley publishers, which started in Kendall Square, Cambridge, a stone's throw from my office. It prospered from its best seller, George Thomas's *Calculus* textbook. Same story for Sears' physics texts. Because Addison-Wesley employed a German typesetting expert on math printing, it was a canny bet that the new small firm would grow.

On account of what financial engineers call access to public idiosyncratic knowledge, I bought 50 shares again and again. I had previously always steadfastly refused to serve on

corporation boards as a director. I reluctantly agreed to serve temporarily on the Addison-Wesley board to represent its Class B nonvoting shareholders. At first this meant only an occasional quarter-mile walk from my MIT office. A minor secondary motive was curiosity: How will a mathematical economist judge from the inside the rationality and irrationality of small business sociology and anthropology?

Both purposes were served well. What I observed seemed to me to disagree with much of the current reports by Oxford and Harvard Business School interviewers who poohpoohed the importance of interest rates to real-life firms. Only when credit became more plentifully available did Addison-Wesley move to impressive new headquarters on the legendary Massachusetts Route 128. That sweetened life for Addison-Wesley's earliest staff and possibly helped enlist new textbook writers. But maybe moderate expansion of the Kendall Square spartan space could have been a better deal for both Class A and Class B share owners.

Once a year, Addison-Wesley did have to be free of debt to its lead banker, the First National Bank of Boston. A promising plan to start early a West Coast branch near to Silicon Valley was definitely delayed by liquidity restraints.

Also, one could observe that early employees of successful firms received compensation benefits beyond what was being paid for janitors, secretaries and salespersons in adjacent neighborhood firms. All know that in Seattle a thousand millionaires were spawned by Microsoft. That same qualitative effect will be found in small businesses, too.

This is how and why I bowed out from board directorships. The original Addison-Wesley owner was the print salesman who served a number of MIT departments. He thereby owned in perpetuity more than 50% of Addison-Wesley's voting shares. His right-hand go-getter—I'll call him "Bailey"—was energetic and experienced. Mostly, it was he who most authors, and would-be authors, dealt with when Addison-Wesley shifted to book publishing.

One day the activist go-getter came to me to say that he was losing sleep at night realizing that he could never achieve 50%+ ownership of the voting power. I had never signed up to be an ombudsman; I certainly didn't want to know which insider subgroups were against other subgroups. Therefore, I advised Bailey to go to wherever he could get some sleep. (He did. And for a while prospered.)

I resigned quietly as a director, pleading that my economics research mandated that. One problem remained. I wanted to sell all of my shares, even at the beaten-down price that they would bring if dumped suddenly on the market. So cannily, I began to feed gradually to the pink-sheet brokerage marketplace a few hundred shares at a time. My good sleep would trump a few thousand dollars of lost capital gains. Why *lost* capital gains? Because every one of the thousands of readers of Samuelson's *ECONOMICS* (Samuelson 1948, 1980; Samuelson & Nordhaus 1985, 2010) knew that "increase in supply will *lower* price."

Instead, in this instance the more shares I fed the pink-sheet market, the higher went Addison-Wesley's share price! I came to realize why. When few shares were being bought and sold, few brokers knew or cared about Addison-Wesley. Added shares announced for sale motivated brokers and investors to study Addison-Wesley's salubrious profitability.

Moral: To understand economics you need to know not only fundamentals but also its *nuances*. Darwin is in the nuances. When someone preaches "Economics in one lesson," I advise: Go back for the second lesson.

USEFUL PERSONAL CODE

Early on I chose not to serve as a paid economic expert in court litigation cases. I could afford to do that. I did not criticize those who did. Our whole system of jurisprudence is based squarely on adversary procedures. When you accept to testify for one interested party in a litigation, you have given up your freedom to bring to court any truth prejudicial to your client. Once you accept the Queen's shilling, you have lost the freedom to seek out truth.

A different set of considerations applied when Chairman John Bogle sounded me out for board membership of the Vanguard Group. I refused in accordance with my usual resolve. But, as I explained, that was not at all because I disagreed or disapproved of Vanguard's functioning. Mentally, I awarded A++ grades to Vanguard and Bogle, as almost the only mutual fund group dedicated solely to its client's interests. My reason for refusal, I explained, was this: In my many finance writings, I remain *freer to enthuse* about Vanguard than I would have been as a board director.

TIAA-CREF TRUSTEE: 1974–1985

Bill C. Greenough was a roommate of mine in the Harvard Graduate School. His Ph.D. thesis on "variable annuities" first persuaded TIAA (and later others) to set up retirement funds partly invested in equities. So shortly after age 35 he was President of the TIAA and later Chairman of TIAA-CREF. He long wanted me to become a trustee. However, for whatever reasons MIT did not use TIAA-CREF for its retirement funds, thereby thwarting Greenough's wish. When in 1973 MIT did adopt certain catch-up supplementary retirement options, I could and did put some of my money into TIAA-CREF venues. That empowered Greenough to get me elected as a trustee.

I took $20–$80 billion seriously. Appointed to the Finance Committee of (equity) CREF, I pursued my usual activistic ways. (I am a resolute polite troublemaker who asks questions about any and all current procedures.) I strongly urged for more foreign stocks in the CREF funds. You didn't have to be a genius to advocate that.

Always CREF finance committees consisted of both academics and Wall Street money managers. Before and after my time, most academics favored broadly diversified "indexing" for so large a fund as ours. Opposed to this were many of the nonacademics who believed in opportunistic picking out of best stocks. Peace prevailed when a compromise was agreed upon that put most of the portfolios in low-turnover, no-load, broad indexes ("Vanguard" type tactics), and the lesser residual in judgmental stock portfolios. This latter group did not do badly in my time (although earlier it had done less well).

However, a couple of the other academics and I debated against certain legacy views. First, since TIAA-CREF had a captive *growing* clientele, it allegedly did not have to worry about enjoying liquidity at all times. That, I argued, did not provide us with a positive alpha edge. Second, prevailing dogma, inside TIAA-CREF and outside it, *opined that investing for the long run over a time horizon of many years causes riskiness to erode downward*. Several of my published works raised doubts about that.

Reported to me was that Milton Friedman, a trustee before my time, had disagreed with my later heresies. At Columbia, Friedman had learned some Laws of Large Numbers that allegedly negated my views. I said stubbornly: Who's afraid of Virginia Woolf? Who's afraid of Milton Friedman and *his* odd version of the Law of Large Numbers?

NON-PROFIT FINANCE COMMITTEES

I will not dwell upon the few occasions when the MIT endowment and treasury offices benefitted from consultations with Professors Solow, Modigliani, Merton, Fischer Black, and me. (In the 1970s stagflation, bond yields soared high above equity dividend yields. Most of the deans and at least one of the MIT presidents wanted to bail out of stocks so as to be able to have "more income to spend out of." We professional nerds did MIT's future a good turn by lending support to the treasurer's office specialist demurrals.)

I've known personally almost a dozen Harvard treasurers. One of them, perhaps not one with the highest I.Q., told me his "two rules" in running the Harvard endowment: 1. Never consult the economics department. 2. Never consult the business school.

The story about the Harvard treasurer who was skeptical over new-fangled finance theory reminds me to comment on the stunning successes in the 1985–2005 period of a new type of investor for top universities and foundations—Yale, Harvard, Princeton, and Stanford, for example. Wanting to be scrupulously honest about the reality of our markets being a microefficient pricing regimen, when I started to write up this topic I did have to ask myself: Aren't Treasurer David Swenson at Yale and his like elsewhere in the Ivy League clear refutations of the claim that nobody and no system scores long-run positive risk-corrected alphas?

However, I must revise my admission that the Yale-Harvard kind of trading does rebut the efficient market hypothesis. I was wrong to declare that these biggest universities and foundations had learned how to squeeze extraordinary risk-corrected long-run total returns on their portfolios. Yale-Harvard type *alternative* investing collapsed in 2007–2008. Instead of there being "much gain with no pain," the post-2007 meltdown of new financial engineering instruments did dry up the private financing that Yale-Harvard type investing crucially depended upon.

So, as yet, there is no magic formula for sure-thing "alpha" gains (in the long run on a risk-corrected basis)! So far only those with earlier knowledge of *changed* news—illegally or legally arrived at—can be top winners in today's financial markets.

I would be remiss if I did not mention that outfits like the Medallion Fund from the stables of the Renaissance Group on Long Island and under the direction of James Simon (math star formerly Chairman of the Stonybrook Math Department) rely on super high-speed computer buy-and-sell orders. They, along with a Goldman Sachs high-speed computer group, seem almost to coin gains endlessly. They beat other traders to the new punches. Then, later, they earn a premium when they sell or buy with ordinary traders and with the last-in-line smaller traders. Medallion insiders must laugh at the notion of an efficient market dogma! Whether future regulation can plug this loophole, I don't know.

THE GLOBAL MELTDOWN EXPORTED FROM U.S. WALL STREET

While writing the rest of this memoir, I had not planned to add this last section on how a down bubble in real estate following an up bubble in real estate *recreated* global panics, bankruptcies and economic chaos reminiscent of the 1929–1939 Great Depression. To tell the candid truth, I had never expected in my long lifetime to experience once again so much of what I have vividly remembered from the early 1930s when my serious study of political economy had begun in January 1932. This has been *déjà vu* with a vengeance.

To my surprise, the best and the brightest—at MIT, Harvard, or Stanford—when they wrote about the Great Depression in the 1970s and 1980s were almost as much off base about that 1930–1939 disequilibrium economy as Milton Friedman & Anna Schwartz (1963) had been in their book on money, spelled with a capital M. Their cardinal error was to overplay the role of central banks like the Federal Reserve and the Bank of England, and to underplay the role of sustained large deficit budgetary spending in bringing U.S. and German employment virtually back to full employment levels by 1939.

Put another way, oddly around 1950, Milton Friedman reverted back to the MV = PQ tautology of Irving Fisher (1911) and Alfred Marshall (1923). (What is so odd in this is that for the period 1950–2008, innovation of computers balkanized M beyond M_0 and M_2 into M_0 to $M_{\text{seventy-eleven}}$.) Even with only M_2 in the relevant picture, Friedman's conjectured constancy for its Velocity (short term, long term, during up booms and down booms) was whimsically off the chart. (Canada, with few banks to fail, maintained its M's in the Depression in contrast to the U.S.; yet both places had similar collapses in real output, employment, and price levels.)

Irving Fisher in America and Knut Wicksell in Sweden, both early believers in such a 1911 Quantity Theory of Money, came to know better after World War I. And thereby they died better macroeconomists than they had been. Dr. Friedman lacked ability to feel regret or to change. He continued to misconstrue equivalence between "Money matters" and "*only* Money matters."

All scholars are toast after a few decades in a dynamic science. However, my sole reason for mentioning what a poor macro paradigm monetarism proved itself to be in the last third of the twentieth century, is that those 1980s Ph.D. thesis writers on the Great Depression were led down the garden path to a powerful influence of unorthodox fiscal policy in the 1933–1939 recovery period. Macro experts trained in the 1970s and 1980s were ill-prepared for the post-2007 meltdown.

I suspect that future rocky post-2008 behavior on Main Street—globally and in the United States—will need to be stabilized by some heavy deficit spending. On the Chicago Midway that I knew as an undergraduate, Henry Simons, Jacob Viner, and Charles Hardy created a cogent oral tradition that justified large budgetary deficits and utilization of new agencies like the Reconstruction Finance Corporation that would knowingly make some investments that would not pay off. In those days there were *liquidity traps* and *paradoxes of thrift*, whereby extra savings served only to *reduce* (!) investment. In such time what would be imprudent in "normal" times, became both prudent and necessary. Evidence: After the 1931 devaluation of the U.K. pound, the places loathe to devalue and deficit spend (like France) suffered badly in comparison with the places (like Belgium) that devalued its franc.

Alas, Alan Greenspan at the pre-2006 U.S. Federal Reserve, Governor Mervyn King at the Bank of England, and Greenspan's successor Governor Ben Bernanke were slow off the mark to move away from mere inflation targeting. Bad economic news should have taught them that the time to preach about moral hazard should have been *before* Rome is burning and not while it is burning.

I lack space to indict the many whose deregulations and conflicts of interest created the tinder for the recent burning of Rome. Suffice it to mention that this may have been the first time in history that what used to be a plain-vanilla down bubble in real estate impinged on the new fiendish, frenzied Frankenstein creations in financial engineering. These brainchildren of MIT, Chicago, and Wharton could have rationalized investing and sharing

of risks—and thereby reduced unnecessary riskiness. But this time actually they blinded transparency and caused CEOs to hyper-leverage without knowing that they were doing so. Securitization of mortgage was only one part of the new terrible iceberg. In fairness, as pointed out to me by MIT Professor Andrew Lo, as early as 1970 the federal government and private Wall Street promoted securitizing mortgages. Maybe what has been needed is not fewer financial engineers, but rather more and better financial engineers enforcing prudent social versions of corporate governance and regulation.

Moral: free markets do not stabilize themselves. Zero regulating is vastly suboptimal to rational regulating. Libertarianism is its own worst enemy!

WINDUP

Markets are not perfect, which is true even for rationally regulated markets. Nevertheless, over the last thousand years every attempt to organize sizeable societies without important dependence on markets has generated its own failures, à la Marx, Lenin, Stalin, Castro, Mao, and many more.

Limited *centrist* societies with regulated markets and a rule of law are those "serfdoms" that von Hayek (1944) [and Friedman (1962) too] used to warn us about. That tells us something about them rather than something about Genghis Khan or Franklin Roosevelt. It is paranoid to warn against inevitable slippery slopes downward to hell once individual *commercial freedoms* are in anyway infringed upon. Paranoidal guesses are as prone to misfire as are utopian delusions about the self-correcting mechanisms of a pure *laissez-faire* market economy, or an ideal *centrist* economy. Willy nilly I am compelled to be a dull *centrist*. I applaud the Ben Bernanke who flexibly moved beyond orthodox inflation targeting by the Fed.

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