

REMARKS AT THE SEVENTH SAMUELSON AWARD CEREMONIES

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Washington, D.C.

January 3, 2003

2002 was another serendipitous year for prize-worthy contributions to the modern theory of finance. Young John Stuart Mill worried that all possible tunes in music would soon be discovered. In our field of economics we shall never run out of work.

In the corner of economics called finance science, it was not always thus for pioneer researchers. Within one academic lifetime – mine for example – this discipline began and proceeded to develop. No prizes were bestowed on the early giants. Louis Bachelier's path-breaking 1900 Paris thesis on continuous probabilities as applied to speculative price changes was like a tree that sprung up in a forest where no observer took notice. For decades Holbrook Working published Wheat Studies at the Stanford Food Institute, while citations to his findings of quasi-random price vibrations obeyed Poisson's Law of Small Numbers. An amateur multi-millionaire, Alfred Cowles, III had to construct from scratch the earliest S&P 500 index number of the whole stock market; and from his sick bed in the Rockies sanitorium, he had to improvise primitive probability mathematics to test and debunk the predictability performance of popular rule-of-thumb forecasting methods. Only after about 1950 did there come forth with a rush an outpouring of new hypotheses and paradigms, along with databases and computer hardware.

Was this activity universally greeted with praise and support by the established authorities of my time and my teachers' time? Not quite. Two decades of Nobel Prizes studiously ignored finance niceties. And then when the dam cracked, some sages deplored to the press the new abracadabra as neither economics nor even interesting mathematics.

As the television ads blurt out, "We've come a long way baby." One of the many Harvard treasurers I've known – perhaps not the highest in IQ – once told me, "I have only two rules in investing the Harvard Endowment. One, never consult the economics department. Two, never consult the business school." Since 1990 a survey of relative risk-corrected total returns among the 25 largest universities and foundations in comparison with the next 200, will report that much was to be learned from the paradigms of Irving Fisher, John Burr Williams, Markowitz, Tobin, Modigliani, Fama, Sharpe, and Black-Scholes-Merton – along with the wisdoms of Graham-Dodge, Buffett, Bogle, and Neff. Magic formulas? No. But lessons learned the hard way concerning risk, diversification, liquidity, and transaction costs.

Great past inventions – fermentation, cooked meat, the wheel – had discoverers with names lost to history. Also we benefit from social inventions: the variable annuity, from TIAA-CREF Greenough's Harvard Ph.D. thesis; Bogle's first practical Index Mutual Fund at Vanguard; and the money market fund cooked up by one-time TIAA-CREF employees Brown and Bent. Social Security itself was a nineteenth century European invention.

2003 and 2030 no doubt will bring forth new prize-worthy research. Some of it will likely not, in the end, pan out. One keeps in mind the tentative nature of all scholarship. Athletic records are made to be broken. Similarly, textbooks and treatises are made to be later revised. At least one Nobel in Medicine is said to have been awarded for the cure of a disease not now believed ever to have existed.

While we now rightly applaud the worthy prizewinners, one tries to keep in mind the economic verity that the first *excluded* increment is of about the same quality as the last included *increment*. Scholarly advance is the product of the many rather than the eureka of those lucky enough to be hit on the head by the relevant falling apple.