The Cowles Commission and Foundation on the Functioning of Financial Markets from Irving Fisher and Alfred Cowles to Harry Markowitz and James Tobin

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The Cowles Commission and Foundation on the Functioning of Financial Markets from Irving Fisher and Alfred Cowles to Harry Markowitz and James Tobin

Robert W. DIMAND

Abstract
This paper explores the development of financial economics at the Cowles Commission and its successor the Cowles Foundation, emphasizing that Alfred Cowles III was an important financial economist (not just a source of financial support for the research of others), and examining the linkages within a continuing tradition of financial research.


Résumé : La Commission Cowles et la Fondation Cowles au sujet du fonctionnement des marchés financiers d’Irving Fisher et Alfred Cowles à Harry Markowitz et James Tobin
Ce document étudie le développement de l’économie financière à la Commission Cowles et à la Fondation Cowles, l’institution qui lui succéda, soulignant que Alfred Cowles III était un économiste financier important (et pas seulement un soutien financier pour la recherche d’autres économistes), et examinant les liens à l’intérieur d’une tradition ininterrompue de recherche en finance.


1 An earlier version was presented to the History of Economics Society session marking the 50th anniversary of the move of the Cowles Commission from Chicago to Yale and of James Tobin’s succession to Tjalling Koopmans as director of Cowles, at the Allied Social Science Associations meetings, Philadelphia, January 8, 2005.
Introduction

The role of the Cowles Commission for Research in Economics, and its successor the Cowles Foundation, in the institutionalisation of econometric modelling and mathematical economic theory is well known, but it was also a crucial institutional setting for the emergence of modern financial economics. James Tobin declined to leave Yale University to succeed Tjalling Koopmans as director of the Cowles Commission for Research in Economics, located since 1939 at the University of Chicago but subject to increasing friction with the « Chicago School » dominating the Economics Department. As Jacob Marschak (Koopmans’s predecessor as director of Cowles) remarked, if Mohammed would not come to the mountain, the mountain would come to Mohammed. In 1955, the Cowles Commission and its research associates moved to Yale, with the Commission becoming a Foundation as the Cowles family provided Yale with an endowment in place of the previous subsidies. One result of this move was that Harry Markowitz spent the 1955-1956 academic year at Yale, transforming his Chicago doctoral dissertation into his Cowles Monograph on Portfolio Selection and providing what James Tobin termed « a fruitful stimulus » to the research on portfolio balance and financial markets published as Tobin. This conjunction of Cowles, Yale, and financial theory was doubly appropriate. The Cowles Commission was founded in the wake of the Wall Street crash of 1929 because Alfred Cowles, disillusioned with his own inability and that of other stock market forecasters to do better than random guesses, wanted to encourage the application of formal theory and statistics to the better understanding of financial markets. Irving Fisher of Yale was Cowles’s close associate in the Econometric Society and in the creation of the Cowles Commission and Econometrica. Notorious for a spectacularly mistaken prediction about stock prices, Fisher had long sponsored pioneering financial research such as Norton and Fisher et al., and the Tobin separation theorem of 1958 was in the spirit of the Fisher separation theorem of 1907.

Alfred Cowles and the Predictability of Stock Prices

When I was a graduate student, a popular T-shirt at the Cowles Foundation displayed a photograph of Irving Fisher with the caption « This is not Alfred Cowles ». That T-shirt appropriately recognized Fisher’s eminence in general equilibrium analysis, econometrics, index numbers, and monetary economics, and his role in economics at Yale and in the founding of the Cowles Commission. However,
with the notable exceptions of Peter Bernstein 11 and, in French, Christian Walter 12, the literature on the innovative and influential scholar and philanthropist who actually was Alfred Cowles is skimpy and dismissive. Thus, Harrison 13 devotes a single sentence to the existence of Cowles 14 and Cowles and Jones 15, cited only as evidence of «a trickle of empirical research» on financial economics and as offering «evidence that stock market forecasters were worse than random forecasts» 16. Brown, Goetzman, and Kumar 17 acknowledge Cowles’ critique of the Dow Theory as «a landmark in the development of the empirical evidence about the informational efficiency of the market» and re-examine his evidence, reaching a contrary conclusion. However, they cite only the first of his publications (and consistently misstate its year of publication, which was 1933, not 1934), and consequently miss Cowles’s 1944 reversal of his earlier verdict on William Peter Hamilton’s version of the Dow Theory 18.

Alfred Cowles 3rd (b. 1891, d. 1985), the son and grandson of leading shareholders and executives of the Chicago Tribune Company, graduated in 1913, where his father and uncle had studied at the same time as Irving Fisher. Stricken with tuberculosis (as Fisher had been), Cowles moved to Colorado Springs, Colorado, for the climate. In a striking parallel to his response to misfortune in the stock market, Cowles became treasurer and a director of the Colorado Foundation for Research in Tuberculosis, as well as a patient, and published in the Journal of the American Statistical Association a correlation and regression analysis indicating that climate did significantly affect the distribution of death rates from pulmonary tuberculosis across American states 19. Carl Christ 20, recalling a 1952 conversation with Cowles, states that Cowles was an investment counsellor in Colorado Springs, who became disillusioned with his profession after the October 1929 stock crash, and stopped publishing his market advisory letter in 1931. However, Bernstein 21 clarifies that Cowles was helping to invest his family’s money, and that he was a disillusioned consumer of market forecasts. Such loss of confidence was, of course, widespread: the confident Appraisal of American Business Forecasts published during the boom by a future Dean of Business at Chicago 22 became rather more restrained in the 1930 edition. What was unusual was that Cowles undertook a serious statistical investigation of whether stock market forecasters could forecast, and that, in addition to a broad survey of stock price predictions by twenty fire insurance companies, sixteen

12 WALTER, 1999.
13 HARRISON, 1997, 175.
14 COWLES, 1933.
15 COWLES, JONES 1937.
16 Which is true of COWLES (1933), but not of COWLES, JONES (1937).
18 In contrast, NIEDERHOFFER (1997, 103-104) cites COWLES and JONES (1937) but not COWLES (1933, 1944 and 1960a), and presents Cowles as someone «who, while studying at Yale, decided to apply mathematical methods to predicting stock prices» without noting that COWLES (1933) denied that forecasters succeeded in predicting stock prices or that COWLES (1960b) explained why the statistical results of COWLES, JONES (1937) were spurious.
19 COWLES, CHAPMAN, 1936.
22 COX, 1928.

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financial services, and twenty-five financial publications, he concentrated his attack on the strongest target, not the weakest. It would have been easy to criticize Irving Fisher and his remark about stock prices having reached a permanently high plateau. Cowles examined the record of the pundit who had been right in October 1929: William Peter Hamilton, editor of the Wall Street Journal from January 1908 until his death in December 1929, and author of an October 21, 1929 editorial « The Turn in the Tide », predicting a stock market break. Hamilton’s role as leading proponent of the Dow Theory was taken up by Cowles’s Colorado Springs neighbour Robert Rhea, who both published a book on the theory and, in mid-1932, began a market letter, Dow Theory Comment. Remarkably, Cowles remained friends with Rhea despite debunking market letters and the Dow Theory, just as Cowles’s relations with Fisher were not marred by disagreement over the possibility of predicting stock prices. Rhea even agreed to act as one of the five readers who would classify each of Hamilton’s editorials for Cowles as bullish, bearish, or neutral.

Cowles concluded that from January 1, 1928, to July 1, 1932, an investor who had followed the 7,500 common stock recommendations made by sixteen financial services would have averaged a return 1.43 per cent per annum worse than the market average. Six of the sixteen fared better than the stock market as a whole, but « Statistical tests of the best individual records failed to demonstrate that they exhibited skill, and indicated that they more probably were results of chance » . The twenty fire insurance companies managed an average return over 1928-1931 that was 1.20 per cent per annum less than the market. Twenty-four financial publications lagged behind an average of twenty-four random stock forecasters by 4 per cent per annum from January 1, 1928, to June 1, 1932, with some evidence that « the least successful records are worse than what could reasonably be attributed to chance ». The twenty-fifth financial publication, Hamilton’s editorials from December 1903 to December 1929, announced changes in the market outlook on ninety occasions, and was right forty-five times, wrong forty-five times with respect to the industrial average (right forty-one times, wrong forty-nine with respect to the railroad average). Allowing for brokerage charges, cash dividends, and interest (when out of the market), and assuming that when Hamilton was bullish he would have bought equal dollar amounts of stocks in the Dow-Jones industrial average and Dow-Jones railroad average, Cowles calculated that Hamilton would have earned 12 per cent a year on industrial stocks and 5.7 per cent per year on railroad stocks. On industrial stocks, such results far exceeded « a supposedly normal investment return of about 5 per cent » but over the same period the Dow-Jones industrial average rose 15.5 per cent a year (including reinvestment of dividends) and the railway average 5.7 per cent a year. Cowles did not report the success rate of his own investments.

Cowles returned to the forecasting records of eleven of the forecasters he had studied in 1933 (four financial periodicals and seven financial services) over longer

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23 Bernstein (1992, 29) notes that Hamilton had also predicted such a break in stock prices in January 1927, June 1928, and July 1928. A sufficiently persistent run of such predictions will eventually be right.


25 Rhea, 1932.

26 Cf. Rhea (ibid.) quoted by Bernstein, 1992, 34.

27 Cowles 1933.

28 Ibid., 323.

29 Cowles, 1944.
sample periods: from January 1928 to July 1943 for seven of them, from January 1928 until the end of available data in 1938 or 1939 for the other four. Six of the eleven performed better than a random forecasting group constructed by Cowles, the other five worse, with the eleven forecasters taken together outperforming the random forecasts by 0.2 per cent per year. Individual results ranged from 6.02 per cent per year better than random forecasts to 5.62 per cent per year worse than the random forecasting record. Cowles concluded that these performances «fail to disclose evidence of ability to predict successfully the future course of the stock market. Of the 6,904 forecasts recorded during the 15.5-year period, more than four times as many were bullish or bearish, although more than half of the period was occupied by bear markets, and stocks at the end were at only about two-thirds of their level at the beginning» 30. However, Cowles 31 conceded that the strong record of the most successful forecaster in 1927-1943 (a record that held up even when extended back to 1903) might not be random, given that «statistical tests» 32 indicated that there was some structure in stock price movements.

While the critique of the financial services, fire insurance companies, and most financial publications in Cowles 33 still stands, Brown, Goetzmann, and Kumar 34 use sophisticated techniques such as neural net modeling to make an important point in Hamilton’s defence that, unknown to them, Robert Rhea had made in a supplement to his market letter on January 18, 1933, without recourse to formal statistics. The market return of 15 per cent a year is for a buy-and-hold portfolio continuously fully invested in Dow 30 industrial stocks, while the 12 per cent a year return on the putative Hamilton portfolio is for a portfolio held much of the time out of the stock market in Treasury bills, therefore a less risky portfolio. The comparison in Cowles 35 does not demonstrate that the Hamilton portfolio would have had a lower risk-adjusted rate of return. Brown, Goetzmann, and Kumar 36 express this as the finding that Hamilton’s timing strategies yield Sharpe ratios and positive alphas for the period 1902 to 1929 (presumably meaning from December 1903, the starting point for the editorials used by Cowles). Robert Rhea made the point more simply and dramatically: extending the study beyond Hamilton’s death in December 1929, a buy-and-hold investor would have lost more than 80 per cent of his or her portfolio’s value by the lowest point in stock prices in 1932, while an investor who heeded Hamilton’s October 21, 1929, editorial would have been safely in Treasury bills 37. Remarkably, Rhea had two forecasting successes comparable to Hamilton’s October 1929 prediction before his death in 1939: «He called the bottom of the great bear market on the exact day it hit its low, on July 8, 1932, and then predicted the top of the market in 1937» 38. Christian Walter 39, by far the most thorough study of Cowles’s articles, emphasizes Cowles’s innovative construction of twenty-four

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30 Ibid., 214.
31 Ibid.
32 COWLES, 1936b; COWLES, JONES, 1937.
33 COWLES, 1933.
34 BROWN, GOETZMANN, KUMAR, 1998.
35 COWLES, 1933.
37 BERNSTEIN, 1992, 34-35.
38 Ibid., 31.
random portfolios to contrast with twenty-four forecasters, but misses Rhea’s point that the lower rate of return on the Hamilton portfolio was on a less risky portfolio. Neither Brown, Goetzmann, and Kumar 40 nor Walter 41 cite Bernstein 42 which could have alerted them to Rhea’s contribution, perhaps because Bernstein’s book was written for a broad, non-academic audience 43. Brown, Goetzmann, and Kumar would not have independently discovered their anticipation in Rhea’s 1933 riposte to Cowles because of their belief that Cowles’s article appeared in 1934.

Brown, Goetzmann, and Kumar 44, noting that Cowles’s critique of Hamilton was “a watershed study that led to the random walk hypothesis, and thus played a key role in the development of the efficient market theory”, conclude that their contradiction of Cowles’s verdict on Hamilton, once rates of return are adjusted for risk, «suggests that the empirical foundations of the efficient market theory may not be as firm as long believed». Such a reversal of conclusion from re-examination of a classic study using subsequent advances in technique has a parallel in the replication by David Hendry and Mary Morgan 45 of Irving Fisher’s 1925 critique of «the so-called business cycle». Hendry and Morgan found that even fitting a twelfth-order autoregressive error could not completely whiten the residuals, contrary to Fisher’s insistence that his data did not show cyclical tendencies. These revised conclusions are to be expected, since the point of advances in analytical technique is that they sometimes lead to new and different conclusions. Whether or not Cowles’s negative verdict on Hamilton and chartist efforts at stock prediction or Fisher’s denial of business cycles still stands, Cowles’s 1933 construction of twenty-four random stock forecasters is, like Fisher’s 1925 use of distributed lags, a technical innovation of lasting value.

With respect to Cowles’s critique of the Dow Theory, one may note that Cowles 46 acknowledged that one of the eleven unnamed forecasters examined there had a record that, taken together with the findings by Cowles 47 and Cowles and Jones 48 of structure in stock prices, «indicates a likelihood that whatever success may be claimed for the very consistent 40-year record is not entirely accidental» 49. That forecaster’s recommendations on market timing produced a return of 3.3 per cent per year over 1903-1943 than continuous investment in the Dow-Jones industrial average. «While three individuals were for different periods responsible for the forecasts throughout those 40 years, the general principles followed by them all were similar and the succeeding forecasters were avowed disciples of their predecessors» 50. Cowles found it possible to extend the published record of that forecaster’s predictions back to 1903 – that is, back to when Cowles thought William Peter

41 WALTER, 1999.
42 BERNSTEIN, 1992.
43 WALTER (1996) cites both Cowles and Bernstein, but in separate contexts.
46 COWLES, 1944.
47 COWLES, 1936b.
48 COWLES, JONES, 1937.
49 COWLES, 1944, 214.
50 Ibid., 210.
Hamilton assumed of the editorship of the *Wall Street Journal* 51. Cowles 52 was mistaken in attributing to Hamilton all 260 editorials from 1903 to 1929 listed in the appendix to Rhea 53. Hamilton became editor in January 1908, so that the earlier editorials appeared during the editorships of Thomas F. Woodcock and Sereno S. Pratt, editors after Dow’s 1902 death and Clarence Barron’s purchase of the newspaper. Given Cowles’s 1933 spotlight on Hamilton’s editorials as the outstanding apparent success story in stock forecasting, it is hard to imagine who else the anonymous most successful forecaster could be. If this identification of the most successful forecaster since 1903 is correct, then, without naming names, and without his readers noticing, Cowles 54 conceded that the forecasting success of the Hamilton-Rhea version of the Dow Theory was due to more than just chance, with Brown, Goetzmann, and Kumar 55 confirming his concession and putting a name to the prophet of profits.

The results of Cowles 56 « naturally suggested the question : Is stock price action random in nature or, if not, to what extent is it possible to define the nature of its structure ? » 57. Cowles 58 and Cowles and Jones 59 found evidence of inertia or momentum in stock price movements. These results contradicted the evidence for a random walk in prices presented by Cowles 60 for stock prices (although Burton Malkiel’s *New Palgrave* article on the efficient market hypothesis erroneously lists Cowles and Jones 61 as corroborating a random walk, and finding no serial correlation between successive price changes). Holbrook Working 62 primarily for commodity prices, and, unknown to Cowles or Working, by Bachelier 63. Working, a professor at Stanford’s Food Research Institute who took part in Cowles Commission summer conferences (e.g. in 1940) and a leading figure in random walk theory 64, eventually found the source of the Cowles and Jones results. Working 65 pointed out, and Cowles 66 accepted, that the analysis of Cowles and Jones 67 « was actually an investigation of the first-order serial correlation in the first differences of the stock price series, and Professor Holbrook Working of Stanford University has pointed out that taking monthly averages of daily or weekly prices will produce a positive correlation in such a series even where the original series is a random chain » 68.

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52 Cowles, 1933.
53 Rhea, 1932.
54 Cowles, 1944.
55 Brown, Goetzmann, Kumar, 1998.
56 Cowles, 1933.
57 Cowles, Jones, 1937, 280.
58 Cowles, 1936b.
59 Cowles, Jones, 1937.
60 Cowles, 1933.
61 Cowles, Jones, 1937.
62 Working, 1934.
63 Bachelier, 1900.
64 Cf. his selected papers in Working, 1977.
66 Cowles, 1960b.
67 Cowles, Jones, 1937.
68 Cowles, 1960b, 909.
Ironically, the classic source of this result, Eugen Slutsky ⁶⁹, originally published in Russian (with an English summary) in 1927, appeared in English translation in the April 1937 issue of *Econometrica*, the issue immediately preceding the July issue containing Cowles and Jones ⁷⁰ (and one of Slutsky’s former students, Jacob Marschak, later became research director of the Cowles Commission).

Alfred Cowles’s final published contribution on randomness and structure in stock prices was a letter of March 1965, published in Niederhoffer and Osborne ⁷¹: « If professionals actually do habitually profit from a knowledge of these patterns, that might explain a phenomenon which for many years has intrigued me. As a result of repeated analyses of large numbers of purchases and sales made through various brokers for investors’ accounts, I have noted repeatedly that the average price at which series of 100 or more orders have been executed consistently averaged at prices slightly less favourable to the investors than the average of high and low for the day for each stock purchased or sold ». That difference would be the compensation to the specialist for providing stabilizing services to investors.

Cowles made another major contribution to stock market research, the construction of a common stock price index extending back to 1871, and much broader in coverage than the Dow-Jones averages ⁷². The Cowles Commission Composite Monthly Common Stock Price Index is now the Standard & Poor 500. This monumental private venture in creating and updating a price index was in keeping with the example of Irving Fisher, whose Index Number Institute, operating out of his home at 460 Prospect Street, New Haven, produced and published a weekly commodity price index.

Beyond his empirical results (which also included Cowles ⁷³ on the influence of building activity on stock prices), Cowles, looking back in later life, summed up the basic theoretical argument against market forecasting services: « Market advice for a fee is a paradox. Anybody who really knew just wouldn’t share his knowledge. Why should he? In five years, he could be the richest man in the world. Why pass the word on? ». However, « Even if I did my negative surveys every five years, or others continued them when I’m gone, it wouldn’t matter. People are still going to subscribe to these services. They want to believe that somebody really knows. A world in which nobody really knows can be frightening » ⁷⁴.

Prominent among those Cowles failed to persuade was Irving Fisher. In his *Econometrica* obituary of Fisher, Max Sasuly ⁷⁵ referred to Fisher’s « Formulary for Anticipating Short-Time Changes in Market » and insisted that « Difficult as it may be some to believe, this formulary actually worked out with a definitely favourable statistical margin. (…) Made known – for public use – during the life of the National Recovery Administration, these principles were eagerly received by some of the statisticians come from Wall Street to "help" in the Recovery effort. It appears that the procedure was later used with success in stock-market trading ». Despite

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⁶⁹ Slutsky, 1937.
⁷⁰ Cowles, Jones, 1937.
⁷¹ Niederhoffer, Osborne, 1966, 908.
⁷² Cowles, et al., 1939.
⁷³ Cowles, 1936a.
⁷⁴ Quoted by Bernstein, 1992, 35-36 and 38.
⁷⁵ Sasuly, 1947, 272.
October 1929 and despite his close association with Alfred Cowles, even Irving Fisher went to the grave believing he could predict stock prices. In his presidential address to the American Statistical Association (at the same conference at which Cowles 1933 was presented to a joint ASA/Econometric Society session), Fisher praised Alfred Cowles for having «stepped forward to finance the Econometric Society in the hope that out of it might grow scientific prediction». However, that was Fisher’s hope, not shared by Cowles, who argued that stock market forecasting was inherently an attempt to predict the unpredictable. No-one with true knowledge of what stock prices would do would ever be satisfied with a mere fee for the information. The empirical success of the efficient market hypothesis that asset prices fluctuate randomly and incorporate all relevant information remains intensely controversial. Regardless of how or whether that debate is settled, Alfred Cowles ranks with Louis Bachelier and Holbrook Working as an innovative and fruitful pioneer investigating the predictability or randomness of price changes. Alfred Cowles was a significant researcher in his own right, not just a benefactor of research.

Alfred Cowles, the Cowles Commission, and the Econometric Society

In August 1931, Cowles wrote to Irving Fisher, then president-elect of the American Statistical Association and already President of the newly-created Econometric Society, offering to fund a research institute on the society’s interests, mathematical economics and econometrics. The mathematician Harold T. Davis, whom Cowles met in 1931, put Cowles in touch with the Econometric Society. Cowles’s father and uncle had been friends of Fisher when the three were Yale undergraduates. Malinvaud notes that when Charles Roos, the founding Secretary of the Econometric Society and first research director of the Cowles Commission, left both posts in 1937, Alfred Cowles succeeded Roos as Secretary. Malinvaud’s account of Cowles as an officer of the Econometric Society is correct but incomplete and possibly misleading. Alfred Cowles was not only Secretary of the Econometric Society from 1937 to 1948 and circulation manager of Econometrica. He was also the Treasurer from February 1932, serving until 1954. The Econometrics Society was founded on December 29, 1930 with Fisher as president, nearly three years after a discussion between Fisher, Roos, and Ragnar Frisch at Fisher’s New Haven home in early 1928 (Frisch returned to Norway in February 1928), about the possibility of creating a society devoted to quantitative research in economics, something Fisher (and J. Pease Norton) had attempted in 1912 and that looked doubtful even in 1928. Until Cowles wrote to Fisher on 27 August 1931, offering to finance a journal and research institute, the Econometric Society’s activities were largely limited to sponsoring sessions at scholarly meeting. Meeting with Fisher and Roos at Fisher’s

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76 FISHER, 1933, 9.
77 FAMA, 1970.
81 DIVISIA, 1953 ; COWLES, 1960a ; BJERKHOLT, 1995.
house in New Haven on 18 October 1931, Cowles put in writing his commitment to « make up any deficit in the proposed journal, "Econometrica", including all the expenses of editing, printing... » 82.

Cowles contributed more than his time and money to the Econometric Society. Mary Morgan 83 suggests that Cowles’s stock market research may have attracted the businessmen and government officials who comprised a substantial fraction of the Society’s membership in the 1930s, members to whom more narrowly academic and theoretical studies in econometrics and mathematical economics might not appeal.

Alfred Cowles funded the Cowles Commission and the Econometric Society in what may have been the last period in which a single private benefactor could influence a major intellectual discipline. Another Yale graduate, the Wall Street financier and amateur physicist Alfred Loomis (likely the only physicist in the National Academy of Sciences whose postgraduate education was at the Harvard Law School), provides a striking parallel 84. Loomis maintained his own physics laboratory at Tuxedo Park, New York. Bohr, the Compton brothers, Einstein, Fermi, Heisenberg, Kistiakowsky, Lawrence, and Szilard visited or worked in the Loomis Laboratory, roster at least equal to the Cowles Commission’s phenomenal collection of future Nobel laureates in Chicago in the 1940s. As the trusted emissary of his cousin Secretary of War Henry Stimson, Loomis brought leading physicists from his laboratory into war work, becoming a key figure in radar research and in the Manhattan Project. Although Paul Samuelson worked at Loomis’s MIT Radiation Laboratory during the Second World War and although Loomis was a founding trustee of the RAND Corporation after the war, Loomis’s interests were primarily in physics, not economics and finance. Loomis was able to pursue such interests because in early 1929 he had turned his stock portfolio into cash and Treasury bonds, partly on the advice of his cousin Henry Stimson, Irving Fisher’s long-time friend and Yale classmate (and thus a fellow student with Cowles’s father and uncle). Stimson, then Governor-General of the Philippines and about to become Herbert Hoover’s Secretary of State, wrote to Loomis warning that the stock market boom did not seem to him to be justified by general business conditions 85. Loomis also later attributed his decision to liquidate his portfolio to « the mathematical charts he devised to follow the market » 86. Brought in by Stimson as an informal adviser in March and April 1933, during the transition between the Hoover and Roosevelt Administrations, Loomis (like Irving Fisher) supported Roosevelt’s advisers in preferring reflation and a managed currency to maintaining a fixed dollar price of gold 87, but thereafter he stayed aloof from both economic policy and Wall Street.

The example of Waddill Catchings (like Loomis, a Harvard Law graduate turned Wall Street financier) shows that money and energy were not sufficient for a benefactor to shape economics. Catchings, a senior partner at Goldman Sachs, funded the Pollak Foundation for Economic Research, directed by William T. Foster, his friend and associate in underconsumptionist monetary heresy. The Pollak Foundation

82 BJERKHOLT, 1995, 757.
83 MORGAN, 1990, 158 sqq.
85 CONANT, 2002, 73.
86 Ibid., 77.
87 Ibid., 83-86.
published Irving Fisher’s *The Making of Index Numbers*, a long, highly technical work that no mainstream publisher would have accepted without a subsidy. However, Catchings was Fisher’s equal as a New Economy enthusiast during the stock boom of the 1920s, and so came to financial grief. Later, after Foster’s death, Catchings joined former Cowles research director Charles Roos in a polemical attack on the intrusive socialist designs of the Federal Reserve System, but his influence on the mainstream of the economics profession was long gone.

Unlike Foster and Catchings, Cowles’s sponsorship of research was in keeping with a widespread international movement towards formal use of mathematical methods in economics and statistics. The publication of *Econometrica* from 1933 had a parallel in the publication from 1935 of the *Archiv fuer Mathematische Wirtschafts- und Sozialforschung*, which did not survive the war. Stephen Stigler’s « The History of Statistics in 1933 » dates the emergence of mathematical statistics as a distinct, recognizable discipline to 1933, the year of Jerzy Neyman and Egon Pearson on hypothesis testing, of A.N. Kolmogorov’s axiomatization of the foundations of probability, of P.C. Mahalanobis’s founding of *Sankhya*, and above all of the separation of the *Annals of Mathematical Statistics* from the American Statistical Association. Cowles played a crucial role in the founding of the Cowles Commission and *Econometrica*, and in the growth of the Econometric Society, but these steps towards more formal use of mathematics in economics and statistics were in step with other developments of the day.

### Marschak and Leavens

Next to the predictability or randomness of asset prices, the other leading issue in the development of financial economics has been how investors should allocate their portfolios in a risky world. This question was raised by J.R. Hicks in his now-famous « Suggestion for Simplifying the Theory of Money » and in an Econometric Society paper on « Application of Mathematical Methods to the Theory of Risk » and by his London School of Economics colleague S.P. Chambers, with Chambers drawing indifference curves over the mean and variances of returns on investment. The question was discussed further by Jacob Marschak, then director of the Oxford Institute of Statistics, in articles on his own and with his colleague Helen Makower, work which cited both Hicks papers and Fisher, but not

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88 Fisher, 1922.
89 Endlich, 2000, 43-49.
90 Catchings, Roos, 1958.
92 Cf., e.g., Tinbergen, 1937.
93 Chapter 8 of Stigler, 1999.
94 Hicks, 1935.
95 Hicks, 1934.
96 Chambers, 1934.
98 Marschak, 1938.
99 Makower, Marschak, 1938.
100 Fisher, 1906.
Chambers. Marschak \(^{101}\) was presented to the Econometric Society in 1935 and to the 1937 Cowles summer conference in Colorado Springs, with an abstract published in the Cowles conference proceedings. Marschak, a refugee from both the Russian Revolution and Nazi Germany, came to the Cowles Commission as research director in 1943, by way of the New School’s University in Exile. His earlier work on money and assets in a risky world, presented to the Cowles Commission summer conference, formed the background to his sponsorship of Markowitz’s dissertation.

Dickson H. Leavens was one of three research associates at the Cowles Commission in Colorado Springs and Chicago from 1936 to 1947. Unlike his fellow research associate, Harold T. Davis, a professor of mathematics at Indiana University, Leavens was a full-time salaried employee of the Cowles Commission, involved in administration as well as research. Cowles \(^{102}\) listed Leavens first, out of alphabetical order, among those tabulating the stock price forecasts and computing the investment performances that would have been achieved by relying on those forecasts. Leavens’s main scholarly interest, the subject of his Cowles monograph \(^{103}\), of his talk to the 1936 Cowles summer conference, and of six journal articles from 1931 to 1946, was the role of silver as money, and particularly how the two major silver-standard economies, China and India, fared in the Great Depression. He does not appear to have worked on financial economics before taking part in constructing the common stock price index, extending retrospectively to 1871, published as Alfred Cowles and Associates \(^{104}\) and became the S & P 500. Noticing a striking pattern while calculating and plotting distributions of returns for Cowles \(^{105}\), Leavens \(^{106}\) made an extraordinary, but isolated, contribution to portfolio theory. In a five-page paper addressed to practitioners rather than academics, published in a journal called *Trusts and Estates* read by estate-planners rather than economics or statistics professors, Leavens invoked probability theory to advocate portfolio diversification. Leavens presented diagrams of the distribution of returns showing that for a given mean (average return), diversification could greatly reduce the spread of the distribution. This analysis of how to reduce the riskiness of a portfolio was an important step beyond Cowles’s 1933 critique of Hamilton, which suffered from comparing average rates of return without allowing for differences in portfolio risk.

From Leavens \(^{107}\), demonstrating that diversification could reduce the dispersion (variance) of the distribution of returns for a given mean, it would be a natural progression to Markowitz \(^{108}\), computing the diversification that would minimize the variance for a given mean. It does not appear, however, that this path was taken. Leavens \(^{109}\) was not included in the series of reprinted Cowles Commission Papers, and was published in a journal that few if any economists read. Markowitz does not seem to have known of Leavens \(^{110}\) until long after completing his dissertation. In

\(^{101}\) Marschak, 1938.

\(^{102}\) Cowles, 1944.

\(^{103}\) Leavens, 1939.

\(^{104}\) Cowles, et al., 1939.

\(^{105}\) Cowles, 1944.

\(^{106}\) Leavens, 1945

\(^{107}\) Ibid.

\(^{108}\) Markowitz, 1952 et 1959.

\(^{109}\) Leavens, 1945.

\(^{110}\) Ibid.
Bernstein 111, Leavens, Fisher, and John Hicks share a single sentence as scholars who had even mentioned in passing that the theory of investing includes risk as well as return. Arrow 112 includes two sentences on Leavens, noting that it was Markowitz who drew Arrow’s attention to Leavens. However, Markowitz 113 acknowledges no precursors, and Markowitz 114 has only a single paragraph on the history of portfolio theory.

Portfolio diversification, as advocated by Leavens 115, Markowitz 116, and A.D. Roy 117, contradicted the investment policy implied by what was then the leading scholarly work in the field, John Burr Williams’ *Theory of Investment Value* 118, accepted as a Harvard PhD dissertation in 1937 (apparently without the difficulties that Bachelier and Markowitz experienced in having financial topics approved for their theses), at the start of a long and initially influential career in investment analysis 119. Williams’s Discounted Dividend Model followed Irving Fisher 120 in valuing an asset as the present discounted value of the expected stream of income from owning it, so that the value of a stock would be the discounted present value of the expected stream of dividends, reduced by some factor to compensate for uncertainty. Like Graham and Dodd 121, Williams 122 proposed that investors look for and purchase stocks undervalued by the market (in contrast to the efficient market theory that the market will correctly value the stocks). Williams 123 advocated investment in the stock of the company that had the highest expected rate of return (Fisher’s « rate of return over costs »), calculated by equating the current market price of the stock to the present discount value of the investor’s expectations of the company’s future stream of earnings (adjusted by some risk factor). The implied portfolio would consist of just one asset, the stock with the highest risk-adjusted expected rate of return. Diversification was also opposed in the 1930s by John Maynard Keynes and best-selling Wall Street pundit Gerald Loeb 124.

**Markowitz and Tobin**

Markowitz 125 recalls that the basic principles of his portfolio theory occurred to him one day while, as a University of Chicago graduate student and Cowles Commission research associate, he was reading Williams 126, and realized that

111 BERNSTEIN, 1992, 55.
115 LEAVENS, 1945.
117 ROY, 1952.
118 WILLIAMS, 1938.
120 FISHER, 1906 and 1907.
121 GRAHAM, DODD, 1934.
122 WILLIAMS, 1938.
123 Ibid.
124 Cf. excerpts from KEYNES and LOEB, in ELLIS, VERTIN, 1989.
126 WILLIAMS, 1938.
Williams's analysis implied that an investor (although concerned enough about risk to adjust expected streams of earnings by a risk factor) would maximize the expected value of his or her wealth by owning only the security with the highest risk-adjusted expected rate of return. According to Bernstein, Markowitz went to see Cowles director Jacob Marschak about possible dissertation topics, and happened to chat with a stock broker who was also waiting outside Marschak’s office and who suggested Markowitz write his thesis about the stock market. Marschak welcomed the suggestion, noting that Alfred Cowles had done research on the stock market. Not knowing the literature on the stock market, Marschak sent Markowitz to the Dean of Business, who recommended Williams.

Markowitz had taken courses with the Bayesian statistician Leonard J. Savage (who, like the Cowles Commission, was to move from the University of Chicago to Yale in the 1950s), as well as Tjalling Koopmans’s linear programming course. Although the probability distribution of future returns is unknown, Markowitz followed Savage in positing that investors have subjective beliefs about the distribution of those returns. Given their subjective probability distribution of the returns on each assets, investors should diversify their portfolios in such a way as to minimize the risk (the variance of the distribution of expected returns on the portfolio as a whole) for each given expected return (the mean of the distribution of returns on the portfolio as a whole). Anything else would be inefficient for a risk-averse investor, taking on more risk for the same expected return, as Leavens had noted. The benefit of diversification, as both Leavens and Markowitz realized, was that the variance of returns on a properly diversified portfolio would be less than the variance of returns on any single asset in the portfolio because of risk-pooling. Unlike Leavens, Markowitz was able to use techniques of linear programming (recently developed by, among others, Tjalling Koopmans of Cowles) to actually solve for the efficient frontier of risk/return combinations that minimized risk (variance) for each level of expected return (mean), providing the menu for choice by a risk-averse investor.

Markowitz published this result in the *Journal of Finance* (co-edited by Marshall Ketchum, the Dean of Business who had suggested reading Williams) three months before A.D. Roy published his analysis of diversifying a portfolio to minimize the probability that the value of the portfolio will fall below some disaster level. Although Markowitz received fewer than twenty citations by 1960, that was fame compared to the reception of Leavens and Roy. Milton Friedman was sceptical about accepting Markowitz’s dissertation, on the grounds that portfolio

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127 BERNSTEIN, 1992, 46.
128 WILLIAMS, 1938.
129 LEAVENS, 1945.
130 MARKOWITZ, 1952.
131 ROY, 1952.
132 Related work by Bruno DE FINETTI (1940) was known only to Italian actuaries until 2006. *Cf.* MARKOWITZ, 2006 ; BERNSTEIN, 2007.
133 MARKOWITZ, 1952.
134 BERNSTEIN, 1992, 41.
135 LEAVENS, 1945.
136 ROY, 1952.
theory was not (then) part of economics, a reaction similar to Henri Poincaré’s unease about accepting Louis Bachelier’s thesis on financial speculation as a proper topic in mathematics. Friedman was more generally a sharp critic of the Cowles Commission and its research.

Tobin explained that while « Markowitz’s main interest is prescription of rules of rational behavior for investors; the main concern of this paper is the implications for economic theory, mainly comparative statics, that can be derived from assuming that investors do in fact follow such rules ». The presence of Markowitz at the Cowles Foundation at Yale in 1955-1956, revising his doctoral dissertation as a Cowles Monograph, provided a vital stimulus to Tobin’s writing of « Liquidity Preference as Behavior Towards Risk » and « The Theory of Portfolio Selection » (published in 1965 in the proceedings of a conference held in 1961). Tobin was dissatisfied with the liquidity preference (money demand) theory of Keynes, which implied that each wealth-owner would hold his or her portfolio entirely in bonds or entirely in money, depending on whether the prevailing interest rate was above or below a critical value, the wealth-owner’s expectation of the normal level to which the interest rate would revert, with the inverse relation between money demand and the interest rate resulting from the variation in the expectations held by different wealth-owners. Tobin took from Markowitz the concept of an efficient portfolio of risky assets (the portfolio that minimized risk for each level of expected return), and introduced money as a riskless asset. Nominal returns on money had a standard deviation of zero and a mean that was low (if the riskless asset is thought of Treasury bills or interest-bearing bank deposits) or zero (in real returns, money is a risky asset with a varying purchasing power, but still with a different risk/return combination from other assets).

Tobin drew indifference curves over the mean and variance of returns, representing a special case of maximization of expected utility that holds true if investors have a quadratic utility function (so they only care about the first two moments of the distribution of returns) or if returns are distributed so that the distribution is completely described by its first two moments. Tobin accepted Martin Feldstein’s comment that the normal distribution is the only two-parameter distribution appropriate to Tobin’s analysis. The attainable combinations of risk and return (the least risk for each level of return) are given by a line from the point on the vertical (expected return) axis representing the riskless asset and tangent to the Markowitz efficient risk/return frontier of the portfolio of risky assets. The investor’s degree of risk aversion would determine where an indifference curve would be tangent to this risk/return line, determining the division of the overall portfolio between risky and riskless assets. This is the Tobin separation theorem, dividing the optimal combination of risky assets (which is the same for everyone, and follows Markowitz’s procedures) from the decision about what fraction of the portfolio to

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138 COURTAULT, KABANOV, 2002.
139 Cf. CHRIST, 1994, 35, for Friedman’s attitude as put into verse by Chicago graduate students circa 1949, to the tune of Gilbert and Sullivan’s « When I Was a Lad ».
140 TOBIN, 1958, 66 sqq.
141 Ibid.
142 KEYNES, 1936.
143 TOBIN, 1958.
144 TOBIN, 1969.
invest in risky assets (which depends on how risk averse the individual investor is). If constant relative risk aversion is assumed, the fraction of the portfolio invested in risky assets is independent of the size of the portfolio. This striking simplification is in the spirit of the Fisher separation theorem \(^\text{145}\) between the time-pattern of expected income and the time-pattern of planned consumption, which is determined by consumption-smoothing (implied by the same properties of the utility function as risk version) given the present discount value of expected lifetime income and the terms of which consumption in one period can be substituted for consumption in another period.

Critics such as Karl Borch and Martin Feldstein objected to assuming either a quadratic utility function or normally distributed returns rather than the full generality of expected utility maximization \(^\text{146}\). Responding to Borch and Feldstein, Tobin \(^\text{147}\) viewed this criticism as merely a reminder of the difficulty of deriving propositions that are both interesting and general. Until recently, Tobin noted, the basic model of portfolio choice had only a single parameter, expected return adjusted by an arbitrary, constant risk premium \(^\text{148}\). He insisted that « the most endeavour of doubling the number of parameter of investors’ probability estimates involved in economists’ analyses of asset choice » met a practical need that would be « satisfied neither by the elegant but nearly empty existence theorems of state preference theory nor by normative prescriptions to the individual that he should consult his utility and his subjective probabilities and then maximize » \(^\text{149}\). In addition, Levy and Markowitz \(^\text{150}\) showed that there are a variety of utility functions and probability distribution such that a function of the mean and variance will be a good approximation to expected utility.

Going beyond the single-period portfolio choice diagram of Tobin \(^\text{151}\), Tobin \(^\text{152}\) held that, provided the probability distributions of single-period asset returns are independent over time and stationary, the same portfolio should be chosen in each period. However, Guy Stevens \(^\text{153}\) found a counterexample, for just two assets (one safe, the other the efficient portfolio of risky assets) and two periods, in which Tobin’s stationary portfolio is only one possible optimum \(^\text{154}\).

**Conclusion : The Cowles Commission and Foundation and Finance**

Just as Tobin’s 1958 separation theorem brought to mind Fisher’s 1907 separation theorem, Tobin’s mean-variance analysis built upon Markowitz \(^\text{155}\) and, although he did not know it, followed in the spirit of Leavens \(^\text{156}\), which was a by-product of

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\(^{145}\) Shown in the two-period optimal consumption diagram of Fisher, 1907, 409.

\(^{146}\) As in Arrow, 1974.

\(^{147}\) Tobin, 1969.

\(^{148}\) He cited no examples, but could have mentioned Williams, 1938.

\(^{149}\) Tobin, 1969, 14.

\(^{150}\) Levy, Markowitz, 1979.

\(^{151}\) Tobin, 1958.

\(^{152}\) Tobin, 1965, 43.

\(^{153}\) Stevens, 1972.

\(^{154}\) Tobin, Golub, 1998, 95.

\(^{155}\) Markowitz, 1952 and 1959.

\(^{156}\) Leavens, 1945.
Leavens’s construction of distributions of returns for Cowles.\textsuperscript{157} It continued a Yale tradition of quantitative analysis of financial markets going back to Fisher’s sponsorship of Norton.\textsuperscript{158} Outlining the mean-variance approach to fundamental valuations, Tobin\textsuperscript{159} insisted that he was «the last person to assert that financial markets in fact generate fundamental valuations […] actual volatility of market prices appears to be wildly disproportionate to the information content of current data. This is a puzzle for CAPM as well as for the model of this paper». He often invoked the research of his younger Cowles Foundation colleague Robert Shiller\textsuperscript{160} on the excess volatility of asset prices. Alfred Cowles\textsuperscript{161} argued that stock market forecasters could not forecast (except perhaps for William Peter Hamilton) because prices in efficient markets follow a random walk. Later at the Cowles Foundation, James Tobin\textsuperscript{162} and Robert Shiller followed Keynes\textsuperscript{163} in questioning the efficiency of financial markets as being subject to speculative bubbles, fads, and excessive volatility rather than fundamental valuations incorporating all relevant available information into current asset prices.\textsuperscript{164} Alfred Cowles was motivated to fund the establishment of the Cowles Commission and the Econometric Society because of his conviction that stock market forecasters could not forecast because the stock market efficiently followed a random walk, but Cowles\textsuperscript{165} and Cowles and Jones\textsuperscript{166} demonstrate his willingness to follow evidence and theory on the matter wherever they might lead. It is fitting that, with Markowitz, Tobin, and Shiller, the Cowles Foundation continued to produce financial economics that would have fascinated Alfred Cowles, and also fitting that since 1955 the Cowles Foundation has been located at Alfred Cowles’s alma mater, Yale, where Fisher\textsuperscript{167} and Norton\textsuperscript{168} made their contributions to financial economics. It would also be appropriate for the literature to pay serious attention to Alfred Cowles 3\textsuperscript{rd} as one of the important, pioneering financial economists associated with the Cowles Commission and Foundation.

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\textsuperscript{157} COWLES, 1944.
\textsuperscript{158} NORTON, 1902.
\textsuperscript{159} TOBIN, 1984b, 26 and 32.
\textsuperscript{160} SHILLER, 1989 and 2000.
\textsuperscript{161} COWLES, 1933, 1944 and 1960a.
\textsuperscript{162} TOBIN, 1983.
\textsuperscript{163} KEYNES, 1936, 156-160.
\textsuperscript{165} COWLES, 1936b.
\textsuperscript{166} COWLES, JONES, 1937.
\textsuperscript{167} FISHER, 1906 and 1907.
\textsuperscript{168} NORTON, 1902.
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