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# Alfred Cowles and Robert Rhea on the Predictability of Stock Prices

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*Brown, Goetzmann, and Kumar (1998) show that Cowles's test of Hamilton's version of the Dow Theory was flawed: Cowles compared rates of return without adjusting for risk. A similar argument against Cowles' test was published by Rhea (1933). Furthermore, Cowles (1944) accepted that there was one stock market forecaster whose prolonged record of success could not be attributed to chance (but whom Cowles did not name). Cowles and Jones (1937), cited by Malkiel (1987) and Harrison (1997) as showing that stock price changes are random and without predictable structure, actually reported the opposite conclusion, but that result was demonstrated by Cowles (1960) and Working (1960) to be in part an artifact of the averaging technique used.*

**Keywords**: Dow Theory, Predictability of Stock Prices, Alfred Cowles

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## I. Introduction

Stephen Brown, William Goetzmann, and Alok Kumar (1998) reconsider one of the landmarks in the development of the efficient markets hypothesis, Alfred Cowles' test of whether stock market forecasters, including William Peter Hamilton's interpretation of the Dow Theory, could forecast the market. The status of Cowles (1933) as a landmark is shown by its inclusion as the first item reprinted in Hooper and Nerlove (1970). Cowles (1933) denied that Hamilton and other forecasters could systematically outperform the market average or random portfolios. Brown et al. point that Cowles failed to adjust for the difference in risk between a portfolio that was always fully invested in stocks and one that, following Hamilton's editorials, would often be held as cash (see also Jovanovic 2009, 60-61). They demonstrate that following the advice in the *Wall Street Journal* editorials from 1903 to 1929 would have yielded positive risk-adjusted returns (like Cowles and Rhea, they erroneously date the start of Hamilton's editorship to the end of 1902, rather than to 1908). Brown et al. (1998) give 1934, rather than 1933, as the date for Cowles' first *Econometrica* article and do not cite an earlier version of Cowles' article circulated in late 1931 as a special supplement to the Investment Supervisory Service's newsletter, so they do not take note of the rebuttal of Cowles by Robert Rhea (1933). Rhea made the point about the difference in risk in another way by pointing out that had Cowles not ended the sample period in December 1929 the fully-invested portfolio would have declined in value by 77 percent. Because they do not examine any later publications by Cowles, Brown et al. (1998) do not discover that Cowles later changed his mind. Cowles and Jones (1937) concluded that stock price changes, rather than being random, had a discoverable structure that could, at least in principle, be

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exploited for forecasting (although Malkiel 1987 and Harrison 1997 cite that article as stating the opposite conclusion). Cowles (1944) acknowledged that one series of forecasts made by three forecasters in succession from 1903 onward had a record of prediction so consistently successful that it could not be attributed to chance. Cowles (1944) did not identify who made that series of forecasts, and it is not certain who Cowles meant. The 1903 start date and the number of successive forecasters match the Dow Theory forecasts of William Peter Hamilton (believed by Cowles to have succeeded as editor of the *Wall Street Journal* immediately after Charles Dow died in December 1902), then Robert Rhea after Hamilton died in December 1929, and then Perry Greiner of Rhea, Greiner & Company after Rhea's death. However, Cowles referred to "the forecasting agency with the best results for the 15 ½ years since 1927," which would put the splicing point for the Hamilton, Rhea, and Greiner series of predictions in the period when Hamilton was writing editorials, not at the point when Hamilton died and Rhea began forecasting. But regardless of whose series of forecasts Cowles had in mind in 1944, Cowles (1944) accepted that one series of stock market forecasters had a record of success that could not be attributed to chance, a dramatic change of position from Cowles (1933). Finally, Cowles (1960) accepted the argument of Holbrook Working (1960) that the structure in monthly stock price changes found by Cowles and Jones (1937) was overstated, resulting from the way that they had averaged their data. The story of Cowles' test of Hamilton's stock forecasting is thus considerably more complicated than Brown, Goetzmann, and Kumar indicate. Cowles' efforts to test stock market forecasting had further consequences (apart from the institutional results of leading him to sponsor the Cowles Commission, Econometric Society, and *Econometrica*, on which see Dimand 2009): Cowles and Associates (1938) created a stock price index from 1871 to 1937 that covered many more stocks than the Dow 30 and, unlike the Dow-Jones index, was weighted by the market value of each stock. In addition, calculating random portfolios for Cowles (1944) led Dickson Leavens (1945) to notice that returns on more diversified portfolios had smaller standard deviations for a given expected return.

## II. Rhea on Cowles

Cowles chose Hamilton's predictions for particular study, because Hamilton was renowned for having been right about the Wall Street crash with an editorial entitled "The Turn of the Tide" on October 21, 1929. As Peter Bernstein (1992, p. 29) reports, "Hamilton had made similar predictions of impending disaster in January 1927, June 1928, and July 1928. So 'The Turn of the Tide' was a lucky call." Irving Fisher, who had been so quotably confident in the other direction, would have been too easy a target for Cowles and a tactless one after Cowles' October 1931 letter to Fisher offering financial support for the Econometric Society and for a research institute, which became the Cowles Commission (Dimand and Veloce 2007, Dimand 2009). According to Rhea (1933), Cowles was provided by another Colorado Springs resident, Robert Rhea, with a duplicate set of the *Wall Street Journal* and *Barron's* editorials from 1903 to 1929 that Rhea assembled for use in *Barron's* articles and in Rhea's 1932 book on the Dow Theory. Rhea's 1933 comment confirms the belief of Brown et al. (1998, p. 1314) that "Although it is not cited in his references, this source is likely the one used by Alfred Cowles in his analysis of the Dow Theory." Following Rhea, Cowles (1933, p. 314), Bernstein (1992, p. 28), and Brown, Goetzmann, and Kumar (1998, p. 1312) thought that Hamilton became editor of the *Wall Street Journal* following Charles H. Dow's death in December 1902. However, Thomas F. Woodlock was Dow's immediate successor and was succeeded in 1905 by Sereno Stansbury Pratt. William Peter

Hamilton was editor from January 1908 until his death on December 2, 1929. Of the 260 editorials attributed to Hamilton in the appendix to Rhea (1932), 18 appeared during the editor-ships of Woodlock and Pratt (Bishop 1960, pp. 172-73, 184).

From 1932 until his death in 1939, Robert Rhea sent out two to four mailings each month of his *Dow Theory Comment*, to which he had six thousand subscribers (Bishop 1960, p. 212). At the end of Mailing No. 9, dated January 18, 1933, Rhea included a "Random Comment" responding to Cowles and to a pamphlet based on Cowles (1931). Peter Bernstein (1992, pp. 34-35) devotes three paragraphs to discussion of Rhea's comment, which is otherwise little known in the literature. Rhea's comment appeared before the July 1933 publication of Cowles's *Econometrica* article and not quite three weeks after Cowles presented that paper to a joint session of the Econometric Society and the American Statistical Association on December 31, 1932, because it responded to an earlier version of Cowles' paper issued as a special supplement to the newsletter of Cowles' Investment Supervisory Service. As Rhea remarked, Cowles had discontinued his investment advisory letter and "now pitches for the other team."

Rhea (1933) pointed out that Cowles compared the returns on the portfolio of a hypothetical investor who was always fully invested in the market with the returns received by an investor who followed the advice of Hamilton's editorials even though the latter investor would not always have been in the market. According to Cowles (1933), Hamilton's portfolio would have been long in stocks 55 percent of the time, short 16 percent, and in treasury bills 29 percent (Brown et al., rereading Hamilton's editorials, calculate long 46 percent, short 16 percent, and treasury bills 38 percent of the time). Using techniques not available at the time to Cowles or Rhea (starting with an approximation of beta and progressing to non-parametric tests, Sharpe ratios, and neural network modeling), Brown, Goetzmann, and Kumar (1998, p. 1315) calculate that, while Hamilton's version of the Dow Theory would have yielded a return of 12 per cent per annum from 1903 to 1929, the risk-adjusted rate of return was 29 per cent per annum, easily dominating the return of 15.5 percent per annum from always being fully invested in stocks. Lacking such techniques, Rhea (1933) expressed the risk of the fully invested portfolio in another way: if Cowles had continued his sample period to the date when Rhea wrote his comment, instead of closing it with Hamilton's death on December 2, 1929, the fully invested portfolio would have lost 77 percent of the value it held when Hamilton died, reversing Cowles' comparison of rates of return. Rhea's comment is reprinted here.

## *RANDOM COMMENT: THE WYCKOFF EXPLOSION*

In a little pamphlet edited by Mr. Richard D. Wyckoff there recently appeared an article title, *The Dow Theory Exploded*. My name was mentioned; therefore, it seems proper to discuss the manner in which a scholarly analysis was distorted by omission of facts.

Mr. Wyckoff tried to sell me a $500 course on how to do this and that to the stock market. He offered me a commission on business derived from friends and customers whose names and addresses he suggested I send him. Beyond a refusal in each case, I have had no dealings with him.

In pamphlets edited by him in June and September 1932, appeared *Charts and the Dow Theory* and *Higher Development of the Dow Theory*. Anyone reading these articles would infer that the $500 instruction course was largely built around that theory, but the writer of these articles admitted having improved and refined Dow's Theory to a point where those who paid the price could learn how to pick the turns of the market to the hour and minute. Editorials attributed to Dow were quoted to corroborate the claims made. Considering these changes, one

may wonder why the improved Dow Theory exploded and whether the witness to the catastrophe was damaged. Any novice could have told him that Dow's Theory simply will not jell when mixed with hours, minutes, and the last eighths. The writer of *The Dow Theory Exploded* complained that the theory had become, to him, as clear as mud. If unwarranted liberties were taken with another man's theory, why then blame the original theory if a disaster of some kind resulted? Why call it an "ice-wagon"?

Now let's test the dynamite with which the Wyckoff magazine exploded Dow's Theory. The attack is based upon an article called, *The Stock Market Barometer* (not Hamilton's book), which appeared more than a year ago in a special supplement of the Investment Supervisory Service, published by Cowles & Company, Inc. of this city. Mr. Alfred Cowles, III, long a friend of mine, heads that company which, besides conducting the service mentioned, directs the operation of an investment trust. He is a Dow Theory skeptic and for years has devoted his time to the task of attempting to assemble business statistics of past years into a forecasting device, an effort which will bear no fruit if the Dow Theory is sound. Mr. Cowles and I have had many friendly debates on the subject. The service above mentioned has been discontinued, and he now pitches for the other team. He is doing a commendable job in proving up on the defects of advisory services generally, and one of his recent speeches on the subject was widely quoted in the press.

When assembling the editorials written by Hamilton on the price movement, preparatory to writing a book on the Dow Theory, I provided a duplicate set for Mr. Cowles, whose organization proposed to make a study of the effectiveness of Hamilton's methods. I was one of five students of the averages who assisted in scoring each editorial as either "bullish," "bearish," "conditional," or "no forecast." I was unaware that Mr. Cowles intended to use this material as a basis of a published study and only saw the report when it appeared in print. Many *assumptions* upon which this report was founded, some of which *unfavorably* distort the ultimate results, were probably not well considered. Otherwise, the report is a clear-cut, concise, and masterly treatise; and I want to say here that I know Mr. Cowles intended it to represent an impartial and scientific investigation of the theory.

Some of the debatable assumptions around which the report was built warrant discussion here. It was assumed that Hamilton was an investment counselor, and his editorials were written for the purpose of guiding traders in speculation. Nothing could have been further from the truth. The articles were educational as anyone can see by reading a dozen of them in *The Dow Theory*. Also he was trading from the time of his first editorial to his death, a 26-year period. His transactions were arbitrarily closed whenever his editorials expressed a doubt as to trend, and it was assumed that he forecasted regularly – in other words, that forecasting was his occupation. I here submit that he would never have incurred brokerage charges by closing his account whenever he was in doubt as to the trend. Has any successful trader ever survived by so doing? No allowance was made in the computation for the fact that during long periods Hamilton declined to discuss the market or to the fact that he was frequently abroad, possibly at times when critical situations occurred, which would have merited his comment had he been in America. Mr. Cowles elected to terminate Hamilton's short position at the date of his death, December 9, 1929, when the Industrial average stood at about 260. This was several weeks after he had written an editorial proclaiming the beginning of a great bear market. Surely no Dow Theory student saw anything resembling the termination of a bear market until June and July 1932, and it is inconceivable that Hamilton would have turned bullish before then. Remember that Mr. Cowles' article was based upon a theoretical comparison of Hamilton's record as against that of an outright investor who was assumed to have bought common stocks in 1903,

holding them for the long-pull investment without ever changing his position. He terminated the test at the date of Hamilton's death in December 1929 with the average standing at 260; thus the outright investor got credit for his sale at a tremendously high price; whereas, Hamilton's record did not receive the advantage of the fact that he was short of stocks on that day, and presumably would have been short for several years thereafter. Consequently, had the calculations been carried on to date, Mr. Hamilton's funds would have increased some 77 percent; whereas, those of the outright investor would have suffered a similar shrinkage.

The Wyckoff magazine quotes such portions of the Cowles' report best suited to discrediting the Dow theory, ignoring the "meat" of the report which, among many other interesting phrases, includes the following: "*Were Hamilton's short position continued from December 1929 to date, his performance would, of course, show heavy superiority.*" – "*Hamilton undoubtedly rescued himself hurriedly from unfavorable situations, while he tended to let his profitable ones ride.*" – "*Our analysis of the Dow method as interpreted by Hamilton, reveals a distinctly satisfactory method.*" – "*In declining markets he conserves his principal intact, while the long-term investor's funds undergo a steady and dismal attrition.*" – "*The barometer will hold its own in horizontal markets and gain heavily in a long-term declining trend.*"

Mr. Cowles' calculations, although predicated upon assumptions most unfavorable to Hamilton's record, nevertheless concede, "*The total profit which would have been made in the 26 years, applying Hamilton's forecasts to the stocks composing the Dow-Jones Industrial average, was 1804.95 percent.*" This appears to be 69.42 percent per year average gain on original capital. I, for one, would not complain at such a gain. I wonder if Mr. Wyckoff understands the meaning of "12 percent per annum compounded" conceded to Hamilton's record? The sum of $100 compounded at 12 percent per annum for 26 years increases to $2,003.48.

If the figures of Mr. Cowles' report are accurate (I am sure they are, and I only questioned the assumptions upon which the study was made), and assuming all other factors discussed above, then had the test been carried forward to date, Hamilton's $100 would have increased to more than $3,500.

To this writer, Dow's Theory remains undamaged by the Wyckoff explosion. Probably the criticism was due to the magazine writer's ignorance of that classical theory. It is hard to imagine his being able to understand only a portion of the Cowles' report.

### III. Cowles Changes His Mind about the Possibility of Stock Prediction

Eleven years later, Cowles published another *Econometrica* article about stock market forecasting. In 1933 he had compared 24 financial publications with 24 random portfolios for the period January 1928 to June 1932 (apart from Hamilton) and found no evidence that the financial publications outperformed the random portfolios. In 1944, he examined 11 survivors from those twenty-four publications. For seven of them, he had records from January 1928 to July 1943 for the other four he had records from January 1928 to 1938 or 1939. Of these 11 forecasters, 10 did no better than chance, 5 of them earning more than the market average and 5 less. As to the eleventh: "It was found possible to extend back to 1903 the published record of the forecasting agency with the most successful record for the period from 1928 to 1943. 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" (Cowles 1944, p. 210)[1]. The three successive forecasters fits with the Dow Theory predictions made by William Peter Hamilton from 1903 until his death in December 1929 (leaving aside the question of which editorials he wrote from 1903 to 1908 during the editorships of Woodlock and Pratt), then Robert Rhea until his death in November 1939, and then Perry P. Greiner of Rhea, Greiner & Company, who continued the mailings of *Dow Theory Comment*; but, as a referee has noted, the 1928 splicing point for the series of predictions does not fit. But regardless of whether the successful forecaster discussed by Cowles in 1944 was the Dow Theory succession of Hamilton, Rhea, and Greiner, or the Babson Statistical Organization, or some other series of forecasters, Cowles (1944) acknowledged that one source of forecasts of stock price movements had a record of success that Cowles did not feel could be attributed to chance. Cowles (1944, p. 212) calculated that following the 11th forecaster would have yielded a rate of return of 14.2 percent a year from 1903 to 1943: "In the same period a continuous investment in the stocks composing the Dow-Jones industrial average would have shown a return, including dividends, of 10.9 percent a year. Following the forecasts, therefore, would have resulted in a gain of 3.3 percent over the result secured by a continuous investment in the common stocks composing the Dow-Jones industrial average." He then showed that these high returns were consistent across sub-periods; and that, in the only two sub-periods in which the forecasts would have resulted in losses, the market average lost even more. In his concluding paragraph, Cowles (1944, p. 214) reported that, "statistical tests disclose positive evidence of structure in stock prices, which indicates a likelihood that whatever success may be claimed for the very consistent 40-year record is not entirely accidental." The negative conclusion reached by Alfred Cowles (1933) about whether any stock market forecaster could consistently do better than chance was thus overturned 11 years later by Cowles himself in the same journal. (Christian Walter 1999, pp. 184-186, discusses Cowles 1944 as an "ambiguous confirmation" of Cowles 1933 but does not indicate that Cowles singled out one particular forecaster as having been consistently successful or that he traced back the record of any forecaster earlier than 1928.)

"In view of this moderately but consistently successful result over such a long period," wrote Cowles (1944, p. 212), "it may be of interest to consider the forecasting method used and some statistical evidence as to the soundness of the principles involved." (If 3.3 percent a year, compounded annually for 40 years, is a modest additional return, what would be an immodest return?) "The theory of these forecasters was that there was a prevalence of sequences over reversals in the movements of stock prices; and it was, therefore, desirable to swim with the tide." This is consistent with the identification of the unnamed forecaster with the 40-year record as the Dow Theory but would also be consistent with other forecasting approaches. To support the existence of structure in stock prices making sequences prevalent over reversals (that is, serial correlation of stock price changes), Cowles referred readers to Cowles and Jones (1937).

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[1] As an anonymous referee points out, that Hamilton's editorials were not issued by a forecasting agency. Rhea, Greiner & Company would fit Cowles's term "forecasting agency," and Cowles (1944) only states it was possible to extend the forecasts of the most successful forecasting agency back to 1903 by including those of an acknowledged predecessor, not that the predecessor was a professional forecasting agency. But, as the referee notes, the splicing point for Rhea's forecasts to succeed Hamilton's does not fit what Cowles (1944) said about the forecasting agency with the best record from 1928 onwards. The referee also notes that the Babson Statistical Organization had a continuous record of predictions going back to 1903. However, Cowles (1944) referred to three successive forecasters acting on the same general principles, not to one continuous record of predictions, so any identification of the successful forecaster discussed by Cowles (1944) remains conjectural and uncertain.

## IV. Cowles and Jones (1937) on Structure in Stock Prices

The existence of Cowles' 1937 article with Herbert E. Jones, a statistician on the staff of the Cowles Commission, has not been entirely forgotten; but its message has. In his *New Palgrave* article on the efficient market hypothesis, Burton Malkiel (1987) cited Cowles and Jones (1937) as one of the early studies that "generally found that the serial correlation between successive price changes was essentially zero." Paul Harrison (1997, p. 175) reports that, "Alfred Cowles (1933) and Cowles and H. Jones (1937) presented evidence that stock market forecasters were worse than random forecasts," which is correct about Cowles (1933). However, Cowles and Jones (1937) did not compare the records of stock market forecasters with those of random forecasters, and their conclusion from studying patterns of sequences and reversals was that the serial correlation between successive price changes was *not* zero (see also Cowles 1936a, 1936b, Jones 1937). They were cautious not to promise that trading according to the prevalence of sequences over reversals would always be profitable or that the profits would necessarily be large, but they did conclude that a definite pattern was discernable: "This type of forecasting could not be employed by speculators with any assurance of consistent or large profits. On the other hand, the significant excess of sequences over reversals for all units from 20 minutes up to 6 months, with the exception of 2 weeks and 3 weeks mentioned previously represents conclusive evidence of structure in stock prices" (Cowles and Jones 1937, p. 294).

Cowles and Jones (1937) influenced Victor Niederhoffer's 1964 Harvard undergraduate thesis, "Non-Randomness in Stock Prices: A New Model of Price Movements" (summarized in Niederhoffer 1965). Contrary to how Malkiel and Harrison read Cowles and Jones, Niederhoffer (1997, p. 104) reports, "The weight of scholarly study in this field, now called Rational Expectations and Efficient Markets Theory, has oscillated like a pendulum between the structural school, advanced by Cowles, and the Random Walk Theory, advanced by Cootner, Fama, Malkiel, and others who believe that markets are completely unpredictable." Niederhoffer had an advantage in interpreting Cowles; he (and M. F. M. Osborne, the nuclear physicist who in 1959 identified the randomness in stock price changes with Brownian motion in physics[2]) corresponded with Alfred Cowles. Niederhoffer and Osborne (1966, p. 908) quote a March 1965 letter from Cowles to them, "If professionals actually do habitually profit from a knowledge of these patterns [clustering of limit orders on the order books of specialists], 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 favorable to the investors than the average of high and low for the day for each stock purchased or sold." After 1933 Cowles' attitude to the predictability or unpredictable randomness of stock price changes were more complicated and nuanced than has been generally supposed.

However, Cowles (1960, p. 909) had to acknowledge that "Our analysis [in Cowles and Jones 1937] 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. He has suggested that this effect of

[2] Osborne was unaware in 1959 of Louis Bachelier's 1900 representation of asset price randomness as a stochastic process which was mathematically equivalent to Einstein's 1905 formalization of Brownian motion. Unlike Osborne, Bachelier did not refer to the physical phenomenon of Brownian motion of particles (see Cootner 1964, Dimand and Ben-El-Mechaiekh 2006, and Davis and Etheridge 2006).

averaging may explain the particularly high apparent predictability of monthly changes despite virtual absence of predictability in changes over three-week intervals, which was noted in our 1937 paper as a puzzling phenomenon" (see Cowles 1960 and Working 1960, both reprinted in Cootner 1964, and see also Jovanovic 2009, 64).

The conclusion of Cowles (1960, pp. 914-915) may be taken as representing his final position: "Consideration of this effect of averaging invalidates conclusions based on the high ratio of sequences to reversals for the monthly series reported in the Cowles-Jones 1937 *Econometrica* paper. The results there reported for the daily, weekly, and annual series, however, have been confirmed by computations recently made in which the effect of averaging was avoided. A positive first-order serial correlation in the first differences has been disclosed for every stock price series analyzed in which the intervals between successive observations are less than four years. When allowance is made for brokerage costs, however, there is nothing in this situation to indicate that the stock exchange is not functioning as a free-competitive market in which theoretically any such tendency toward correlation would be eliminated. Professor Tjalling C. Koopmans has suggested that if the persistence in stock price movements were sufficient to provide capital gains appreciably in excess of brokerage costs, professional traders would presumably be aware of this situation; and through their market operations would inadvertently wipe out the persistence in price movements from which they were attempting to profit. Whether or not this has actually occurred, the fact remains that, although our various analyses have disclosed a tendency toward persistence in stock price movements, in no case is this sufficient to provide more than negligible profits after payment of brokerage costs."

Cowles' involvement with testing stock market predictability extended long after 1933. It led him to develop a retrospective index, weighted by market value, extending back to 1871 (Cowles and Associates 1938); and computing returns on random portfolios for Cowles (1944) led Dickson Leavens (1945) to observe that more diversified portfolios are less risky for given average returns. As early as January 1933, Robert Rhea objected that, by closing the sample period in December 1929, Cowles' original test of Hamilton's version of the Dow Theory understated the risk associated with a fully invested portfolio. It is striking to note that 11 years after his original study, although he concluded that all other forecasters fared no better than chance, Cowles (1944) accepted that one (unidentified) forecaster consistently outperformed the market average over four decades. Cowles (1944) reconsidered Cowles' 1933 negative verdict on the predictability of stock prices long before Brown, Goetzmann, and Kumar (1998) did.

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