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Understanding the U.S. Index Futures Stock Market Using Research

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Abstract I begin with five views or camps of beliefs concerning the U.S. stock market. There are efficient markets where prices are correct except for transactions costs, risk premia where excess returns can be made only by bearing additional risk, the idea that efficient markets are hogwash, the belief that great investors exist but one cannot tell who they are in advance, and the study of anomalies and other research. Edges arise from cash flows, institutional practices, and behavioral biases. These include the turn-of-the-year effect, the turn-of-the-month effect, presidential election effects, and mispriced options. I describe the effects and explain why they exist and then discuss their use in trading considering operational risks, the effect of volatility, prediction of stock market crashes, slippage, risk management, and optimal betting sizing. I won the 2015 futures trading contest of the Battle of the Quants in New York and have been able to obtain very good risk-adjusted returns from July 2013 to May 2016 in the Alpha Z Futures Fund.

Keywords U.S. stock market research; calendar anomalies; presidential and Fed effects; trading small and large cap futures indices.

1. Introduction to Seasonal Anomaly Effects

Seasonality of stock markets has a long history despite the academic research being dominated by efficient market theory as surveyed by Fama [19, 20]. Small firm effects were popularized by University of Chicago students Banz [1], Reinganum [64], Blume and Stambaugh [4], Roll [66], Fosback [23], and Ritter [65], among others.

Early surveys are in Lakonishok and Smidt [48], Thaler [76], Gultekin and Gultekin [27], and Ziemba [80]. The last reference considers regularity of various seasonal anomalies in Japan as well as in the United States. Jacobs and Levy [41, 42, 43] have used seasonal and fundamental factor model-derived anomalies to create a multibillion-dollar investment firm. Dimson [12], Keim and Ziemba [47], Zacks [78], and Ziemba [84] present whole books with studies across the world. The *Stock Traders Almanac* discusses some such anomalies in yearly updates; see Hirsch [37] and Hirsch and Hirsch [38]. Zweig [95] presents outstanding trend and Fed following models useful even in 2016, see Swetye and Ziemba [75].

Anomalies of the seasonal variety as discussed here and those based on fundamental and other factors are not fully accepted nor believed by many strong efficient market theorists. Part of this dismissal is that the anomalies are too small to be bothered with, as Ross [69] argues—so too, more or less, does Fama [19, 20, 21]. The great financial empiricist Richard Roll [67] makes the startling statement that even with considerable resources, he has never been able to find a profitable anomaly. The well-known book by Malkiel [58] even states that strong effects such as the January effect do not exist. Rozeff and Kinney [70] discovered that January had high returns. Clark and Ziemba [11] discuss trading the January small firm effect in the index futures markets. Hensel and Ziemba [33] and Ziemba [84] update this. Hensel et al. [35, 36] discuss the turn-of-the-month effect in S&P500 futures. Dzhabavrov

and Ziemba [15, 16] discuss various calendar anomalies. Dzhabavrov and Ziemba [17] show that the stock market from the turn-of-the-month of October until the end of April and in cash from May 1 until the turn-of-the-month of October provides double the returns from large cap S&S500 stocks and triple the return for small cap Russell 2000 stocks from 1994–2015. Marquering et al. [60] argue that the anomalies disappear after they are published, although some reappear; see also Dimson and Marsh [13] and Schwert [72]. Hudson et al. [40] and Lucey and Pardo [56] discuss how anomalies are affected by papers published on them. See also McLean and Pontiff [61], who document the loss in value of many fundamental anomalies after they are published.

There also is the serious issue of data mining. Indeed, many results are in-sample and true tests out-of-sample. Statistical verification of the actual existence of significant seasonal anomaly effects is studied by Sullivan et al. [74], who analyze 9,452 calendar-based trading rules. See also Hansen et al. [29], who study 181 calendar effects, and Lo and MacKinlay [53], who discuss data snooping biases. Also, t values tend not to show statistical significance in many cases where successful trades have been made because of high standard deviations. See also Markowitz and Xu [59] on data mining corrections.

Rather than debate such people, Ziemba and Ziemba [93] simply argue that there are five basic stock market camps. Each has a cut or version of certain sections of the market and makes its point for a certain subset of market participants, instruments, and strategies. There may be other classifications, but these provide a useful framework for discussion. See also the important book by Pedersen [63], which presents a framework for utilizing edges. He, like I, realizes that the anomaly/edge approach requires more effort, but it is well worth it in terms of returns.

The five groups are as follows:[1]

1. Efficient markets (E)
2. Risk premium (RP)
3. Genius (G)
4. Hogwash (H)
5. Markets are beatable (A)

The first group consists of those who believe in *efficient markets*, E. They believe that current prices are fair and correct except possibly for transactions costs. These transaction costs, which include commissions, bid-ask spread, and price pressures, can be very large.[2]

The leader of this school, which had dominated academic journals, jobs, fame, etc., in the 1960s to the 1980s was Eugene Fama of the Booth School of Business at the University of Chicago. A brilliant researcher, Fama is also a tape recorder: you can turn him on or off, you can fast forward or rewind him or change his volume, but you cannot change his views no matter what evidence you provide; he will refute it forcibly. In the aggregate there is much to support his case. One such example is the $300+-million gift from former student David G. Booth to name the Chicago Graduate School of Business earned from fees from his index fund firm Dimension Fund Advisors (DFA), founded with another Chicago student, Rex Sinquefield. Booth never got his Ph.D., but Fama helped him get a job,[3] and later in 1981, DFA was founded with Fama as a key advisor. Some multibillion plus later in fees shows how low fees with simple strategies can add up with huge volume. Booth has coauthored a paper

[1] See also Chapter 6 of Ziemba and Ziemba [93].

[2] A BARRA study by Andy Rudd some years ago showed that these costs averaged 4.6% one-way for a $50,000 institutional investor sale of small cap stock. This is if a naíve market order for the full transaction rather than limit orders or smaller market orders is used. Ed Thorp (in a personal communication with the author) mentioned that he traded about $60 billion in statistical arbitrage from 1992 to 2002 in lot sizes of 20K–100K and found that the mean transaction cost was about 1 cent per share and the market impact was about 4.5 cents per share for shares averaging about $30. So the one-way costs were about 0.18%.

[3] Indeed, Booth told Fama, "I am not sure I am good enough to get a Ph.D. here." Fama replied, "Yes, you are not." But the story ends well, with DFA proving a huge success for Booth, Sinquefield, and advisor Fama.

with Donald Keim on the January effect in the cash market (in the Keim and Ziemba [47] anomalies book).

This group provided many useful concepts such as the capital asset pricing model (CAPM) of Treynor [77], Sharpe [73], Lintner [49], and Mossin [62], which provided a theoretical justification for index funds, which are the efficient market camp's favored investment mode. They still beat about 75% of active managers. Since all the managers make up the market, that is 50% of them beaten by the index. Transactions (commission plus market impact) such as exchange taxes, bid-ask spread, and other costs eliminate another 25%. See Ziemba and Schwartz [91, pp. 44–46] for examples of how few funds beat the index across the world. In a sample of 167 funds, only 48 (28.7%) beat the benchmark.

Over time, the hard efficient market line has softened into a *risk premium* (RP) camp. They feel that markets are basically efficient, but one can realize extra return by bearing additional risk. They strongly argue that if returns are above average, the risk must be there somewhere; you simply cannot get higher returns without bearing additional risk. For example, beating the market index S&P500 is possible but not risk adjusted by the CAPM. They measure risk by Beta, which must be greater than 1 to receive higher than market returns. That is, the portfolio risk is higher than the market risk. But they allow other risk factors such as small cap and low book-to-price. But they do not believe in full-blown 20–30 factor models such as those used by Jacobs and Levy [41, 42, 43] for the United States and Schwartz and Ziemba [71] for Japan. Rather, they prefer to use just a few factors, and small cap and price-to-book value are favorites. I recall Barr Rosenberg focusing on small cap and low price-to-book as the key factors in 1967; see Rosenberg et al. [68]. Later, Fama and French [22] took the credit for these ideas with a more complete study. Fama and his many disciples moved to this camp in the 1990s. This camp now dominates the top U.S. academic journals and the jobs in academic finance departments at the most famous business schools in the United States and Europe.

The third camp is called *genius*, G. These are superior investors who are brilliant or geniuses, but you cannot determine in advance who they are. The late Massachusetts Institute of Technology (MIT) economist Paul Samuelson championed this argument. Samuelson felt that these superior investors do exist but that it is useless to try to find them, as in the search for them you will find 19 duds for every star. Surprisingly, Samuelson was an early investor in the very successful futures trading operation Commodity Corporation run, partly, by one of his MIT students. This view is very close to the Merton–Samuelson criticism of the Kelly criterion: that is, even with an advantage, it is possible to lose a lot of your wealth. See MacLean et al. [57] for simulations discussing this point and Ziemba's [84] response to private letters he received from Samuelson. The evidence, though, is that you can determine some superior investors ex ante, and to some extent, they have persistent superior performance; see Jagannathan et al. [44], Ziemba [81], and Gergaud and Ziemba [24]. Billionaire investor George Soros did this with futures, with superior picking of commodities and currencies to bet on; this is the "traders are made, not born" philosophy. This camp will isolate members of other camps (e.g., A or H).

The fourth camp is as strict in its views as camps E and RP. This group feels that efficient markets that originated in and are perpetuated by the academic world is *hogwash*, H. In fact, the leading proponent of this view—and one with whom it is hard to argue, as he tops the lists of richest people and greatest investors—is Warren Buffett, who wants to give university chairs in efficient markets to further improve his own very successful trading. An early member of this group, the great economist John Maynard Keynes was an academic. We see also that although they may never have heard of the Kelly criterion, members of this camp do seem to use it implicitly with large bets on favorable investments. See MacLean et al. [57]. Ziemba [86] present a table of actual asset positions in a Soros fund where about half the portfolio is invested in one position, and equity weights near 10% for several

positions are in Buffett's Berkshire Hathaway. They do not care about monthly losses, and have many losses, but they focus instead on high long-term wealth growth attainable most but not all of the time. So they resemble Kelly bettors.

This group feels that by evaluating companies and buying them when their value is greater than their price, you can easily beat the market by taking a long-term view. They find these stocks and hold them forever. They find a few such stocks that they understand well and get involved in managing them, or they simply buy them and make them subsidiaries with the previous owners running the business. They forget about diversification because they try to buy only winners. They also bet on insurance when the odds are greatly in their favor. They well understand tail risk, which they only take at huge advantages to themselves when the bet is small relative to their wealth levels. Indeed, Buffett's long-term, approximately 15- to 18-year bet shorting S&P500 over-the-counter puts, for which he received much incorrect criticism, sure looks good now with the S&P500 closing 2011 at 1,257.60 and over 2,000 in October 2016. The odds favor him not to have to pay back the $4.6 billion premium he is now using for trades such as the 10% loans to General Electric and Goldman Sachs. Calculations indicate that Buffett collected about twice the fair value of these American put options that cannot be exercised until expiry many years in the future.

The last group is made up of those who think that *markets are beatable,* A, through behavioral biases, security market anomalies, and other research using computerized superior betting techniques. They construct risk arbitrage situations with positive expectation. They research the strategy well and follow it for long periods of time, repeating the advantage many times. They feel that factor models are useful most but not all of the time and show that beta is not one of the most important variables to predict stock prices. They use very focused, disciplined, well-researched strategies with superior execution and risk control. Many of them use Kelly or fractional Kelly strategies. All of them extensively use computers. They focus on not losing, and they rarely have blowouts. Members of A include Ed Thorp (Princeton Newport and later funds), Bill Benter (the Hong Kong racing guru), John Henry (the Red Sox owner), Blair Hull, Harry McPike, Jim Simons (Renaissance hedge fund), Jeff Yass (Susquehanna Group), and David Swensen (Yale Endowment). Most of these I have had dealings with or consulted for. This group has made many billions trading. Blowouts occur more in hedge funds that do not focus on not losing and true diversification and over-bet; when a bad scenario hits them, they get wiped out (as was the case for Long-Term Capital Management, Niederhofer, and Amaranth Advisors, for example); see Chapters 11–13 of Ziemba and Ziemba [93].

So we will proceed assuming we are in category A and look at the data, possible explanations, and some trading results. Statistical significance is frequently an issue even in long sequences of successful trades because of high standard deviations.

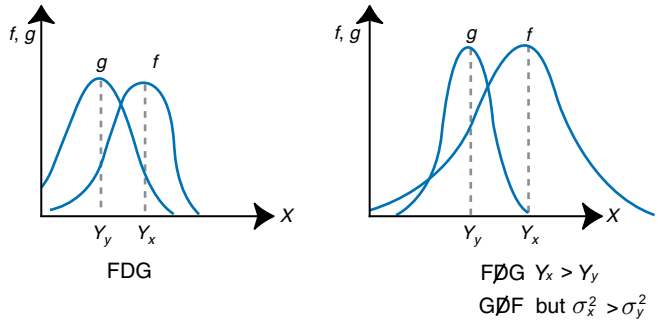
2. What Can We Learn from Static Portfolio Theory? The Importance of the Mean

If two cumulative return distributions cross only once, then we have the powerful result that the one with the higher mean has higher expected utility for all concave utility functions.

Theorem 1. *Theorem (Hanoch and Levy [28]). If $X \sim F(\cdot)$ with the higher mean and $Y \sim G(\cdot)$ have cumulative distribution functions that cross only once but are otherwise arbitrary, then F dominates G for all concave u. The mean of F must be at least as large as the mean of G to have dominance. Variance and other moments are unimportant. Only the means count.*

With normal distributions X and Y will cross only once iff the variance of X does not exceed that of Y. That is the basic equivalence of mean-variance analysis and expected utility

FIGURE 1. The main theorem of mean-variance analysis.



analysis via second-order (concave, nondecreasing) stochastic dominance. See Figure 1 with densities shown (the cumulative distribution functions cross the same way).

2.1. Errors in Means, Variances, and Covariances: Empirical

Replace the true mean μ_i by the observed mean $\mu_i(1+kZ_i)$, where Z_i is distributed $\mathrm{N} \sim (0,1)$ with scale factor $k = 0.05$ to 0.20 being the size of the error. Similarly, replace the true variances and covariances by the observed variances $\sigma_i^2(1+kZ_i)$ and covariances $\sigma_{ij}(1+kZ_i)$. We use monthly data from 1980 to 1989 on 10 Dow Jones Industrial Average (DJIA) securities that include Alcoa, Boeing, Coke, DuPont, and Sears. See Chopra and Ziemba [10], which updates and extends Kallberg and Ziemba [46].

The certainty equivalent, CE, of a portfolio with utility function u equals u^{-1} (expected utility of a risky portfolio). This comes from the following equation:

$$u(CE) = E_\xi u(\xi' x) \quad \Rightarrow \quad CE = u^{-1}[E_\xi u(\xi x)].$$

Assuming exponential utility and normal distributions yields exact formulas to calculate all quantities in the certainty equivalent loss:

$$(CEL) = \left\{ \frac{CE_{\text{opt}} - CE_{\text{approx}}}{CE_{\text{opt}}} \right\} 100$$

.

Observe that the mean-variance problem is

$$\text{mean} \left(\frac{\text{risk aversion}}{2} \right) \text{variance}.$$

Calculations show that the errors in mean are about 20 times the errors in covariances in terms of CEL value, and the variances are twice as important as the covariances. So there is a roughly 20:2:1 ratio in the importance of these errors. Also, this is risk aversion dependent with $T_R = (R_A/2)100$ being the risk tolerance. So for high risk tolerance—that is, low risk aversion—the errors in the means are even greater. Hence for utility functions such as the log of Kelly with essentially zero risk aversion, the errors in the mean can be 100 times as important as the errors in the other parameters. So Kelly bettors should never overbet.

Errors in means, variances, and covariances (Kallberg and Ziemba [46], Chopra and Ziemba [10])

Observed mean

Replace true mean μ_i by $\mu_i(1+kZ_i)$ $\quad Z_i \sim N(0,1)$

size of error → scale factor k = 0.05 to 0.20

Replace true variances σ_i^2 by $\sigma_i^2(1+kZ_i)$

covariances σ_{ij}^2 by $\sigma_{ik}^2(1+kZ_i)$

10 DJIA securities, 1980–1989: Alcoa, Boeing, Coke, DuPont, Sears, etc.

Monthly data

$u(CE) = E_\xi u(\xi' x) \Rightarrow CE = u^{-1}[E_\xi u(\xi' x)]$

Certainty equivalent $CE = u^{-1}$ (expected utility of risky portfolio)

Measure CE loss u = exponential, normal distributions → mean-variance

exact formulas to compute $\mu - \frac{1}{t}\sigma^2$

Certainty equivalent loss $\Rightarrow CEL = \left(\frac{CE_{\text{opt}} - CE_{\text{approx}}}{CE_{\text{opt}}}\right)100$

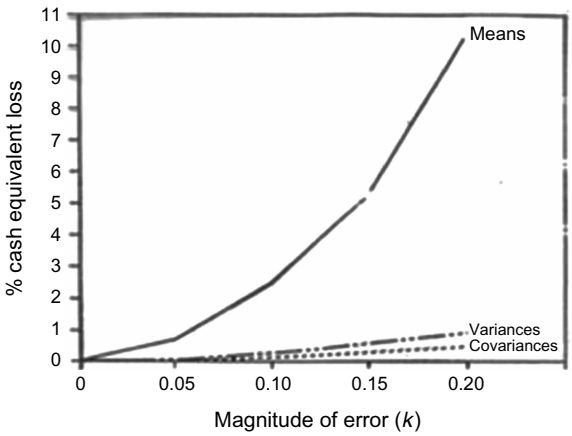
Mean percentage cash equivalent loss resulting from errors in inputs

Average ratio of CEL for errors in means, variances, and covariances

t risk tolerance	Errors in means vs. covariances	Errors in means vs. variances	Errors in variances vs. covariances
25	5.38	3.22	1.67
50	22.50	10.98	2.05
75	56.84	21.42	2.68
	↓	↓	↓
	20	10	2

The error depends on the risk tolerance but roughly

Error mean	Error var	Error covar
20	2	1



Risk tolerance is the reciprocal of risk aversion.

When R_A is very low such as with log u, then the errors in means become 100 times as important.

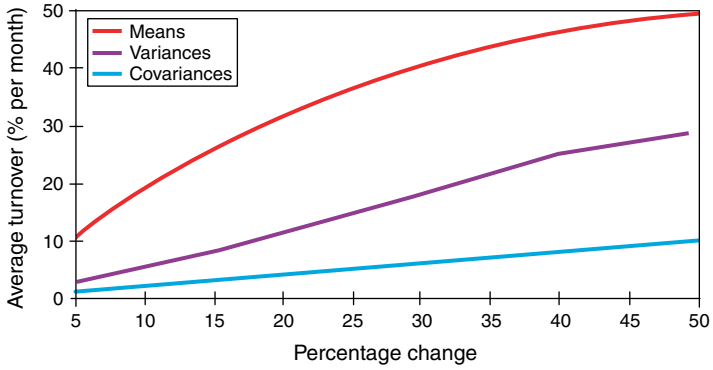
Conclusion: Spend your money getting good mean estimates and use historical variances and covariances.

In conclusion, you should spend your money to get good mean estimates and use historical variances and covariances. This can be done with good estimation or using some clever way of avoiding the problem of poor mean estimates.

Chopra [9] shows that the same relationship holds regarding turnover, but it is less dramatic than for the cash equivalents; see Figure 2.

This does not mean that you need to estimate the mean accurately, but rather, you must proceed with some method to avoid poor mean estimates. An example I use in the Alpha Z

FIGURE 2. Average turnover: Percentage of portfolio sold (or bought) relative to preceding allocation.



Source. Chopra [9].

Futures Fund is an A–B portfolio, where B, a short hedge, is fairly valued and A is a set of long positions with large edges through option sales of overpriced options. Then, as time progresses, A drops day by day, so in the end, the portfolio A–B has good gains most of the time. I do not need to forecast the future but just find the components of A and deal with the risk management.

The results here apply to essentially all models. You must get the means right to have good results.

If the mean return for U.S. stocks is assumed to equal the long-run mean of 12% as estimated by Dimson et al. [14], the model yields an optimal weight for equities of 100%. A mean return for U.S. stocks of 9% implies less than 30% optimal weight for equities. This is in a five-period, 10-year stochastic programming model. See Figure 3.

FIGURE 3. Optimal asset weights at stage 1 for varying levels of U.S. equity means in a multiperiod stochastic programming pension fund model for Siemens Austria (see Geyer and Ziemba [25]).

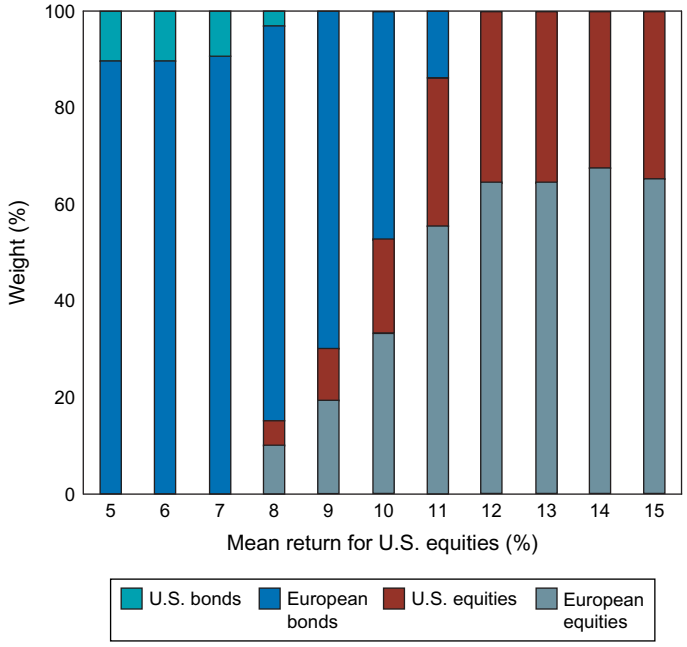
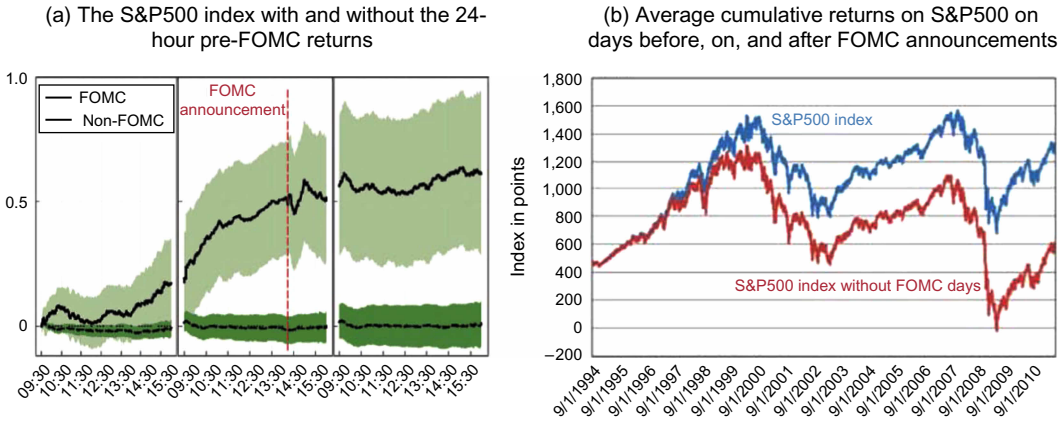


FIGURE 4. Impact of FOMC on S&P500 returns.



Sources. Panel (a): Thomson Reuters Tick History (available at Tickdata.com); panel (b) Thomson Reuters Tick History.
Note. In both panels, the sample period is 1994 to 2011.

The Effect of the Fed Meetings[4]

Former Federal chairman Alan Greenspan wrote in his 2007 memoir (Greenspan [26]),

> People would stop me on the street and thank me for their 401(k); I'd be cordial in response, though I admit I occasionally felt tempted to say, "Madam, I had nothing to do with your 401(k)." It's a very uncomfortable feeling to be complimented for something you didn't do.
> (quoted in Housel [39])

This is naíve—his policies created a stock market bubble that burst and then created a bubble in real estate, which also crashed.

A study by Lucca and Moench [54, 55] of the Fed shows the influence of the Federal Open Market Committee (FOMC) meetings since they were publicized and announced in 1994 through mid-2011. Since then, the S&P500 has risen from 450 to 1,300 (see Housel [39]). More than 80% of the equity premium on U.S. stocks was earned over the 24 hours preceding scheduled FOMC meetings—and virtually all in the three-day window around these meetings, as shown in Figure 4.

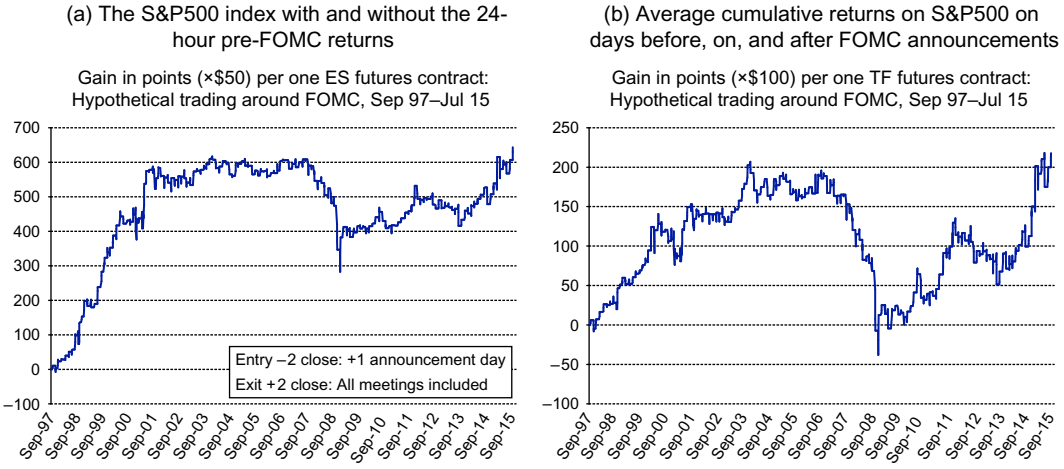
The returns are uncertain with a positive bias but considerable variation. So the Fed effect does seem to work, but there is risk in it in a particular meeting. Given the confidence intervals, one sees that the returns are not straight up. Also, some tests Constantine Dzhabarov ran for me indicate that the effect is less pronounced in recent data ending September 30, 2012.

As reported in Housel [39], Matt Koppenheffer, managing director of *Motley Fool,* used market data going back to 1994. He randomly removed 136 days (eight per year for 17 years, or one for every FOMC meeting). He ran the simulation several hundred times, removing different sets of random days. The difference was trivial. Nothing came within a third of the skew caused by removing the days shortly before FOMC meetings. Over the long haul, stocks are driven by fundamentals; in the short term, they are impacted by headlines including waiting for the FOMC announcements that add to volatility.

To study this further, we updated this research to July 2015 using the S&P500 mini futures as well as the Russell2000 mini futures, plus Constantine and I investigated the five-day period adding one day before and one day after the three-day Fed meeting. The results Dzhabarov and Ziemba [18] are best shown in Figure 5.

[4] Portions of this section were adapted from Housel [39].

FIGURE 5. Graphs summarizing the impact of the five-day period around the Fed meetings.



The gain in points per S&P500 mini futures contract from September 1997 to July 2015 peaked at slightly above 700 (that is $35,000 per contract, a huge gain) in September 2007 and was lower, about $425, at the end of the Lucca and Moench [54, 55] study. The decline coincided with the S&P500 drop from a peak of 1522.50 on July 14, 2007 (see Lleo and Ziemba [50] for the June 14, 2007 signal exiting the S&P500 based on the bond-stock earnings yield differential (BSEYD) model being in the danger zone). A history of the BSEYD predictions is in Lleo and Ziemba [51]. Lleo and Ziemba [52] compares the BSEYD model with Shiller's high PE models. Both models add value with BSEYD the better one. Bertocci et al. [2] show that periods with high returns start with low PEs and end with high PEs. The March 9, 2009 low was 676.53. Since then, there have been gains back to the peak level. The Russell2000 small cap index had a similar pattern but with a much lower low and a much lower total gain, which peaked at 215 in June 2015, versus a local high of 205 in September 2003. According to Hensel and Ziemba [32, 33] and Ziemba [83], small stocks are supposed to outperform large caps with Democratic presidents and handily beat a 60–40 stock-bond mix. In fact, during Obama's two terms, small caps did outperform but not during these Fed meeting periods. Small cap is still beating the S&P500, which is still beating the 60–40 mix during Obama's two terms; see Figure 6.

Finally, there are the five-day stock movements surrounding Fed meetings; see Figure 7.

FIGURE 6. Small cap beating S&P500 beating 60–40 during Obama's two terms.

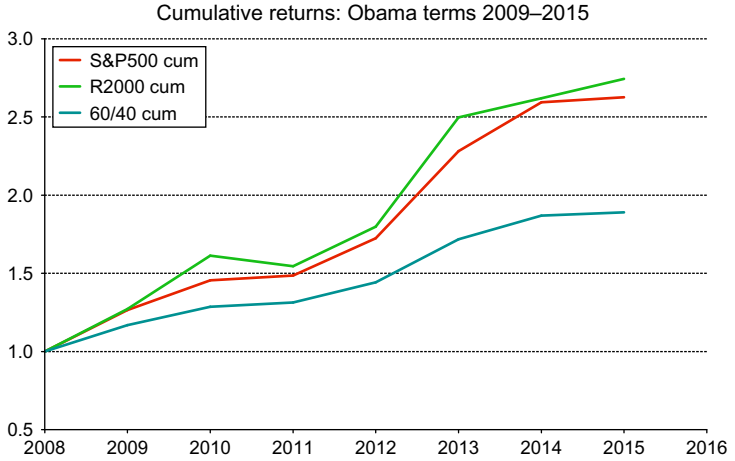
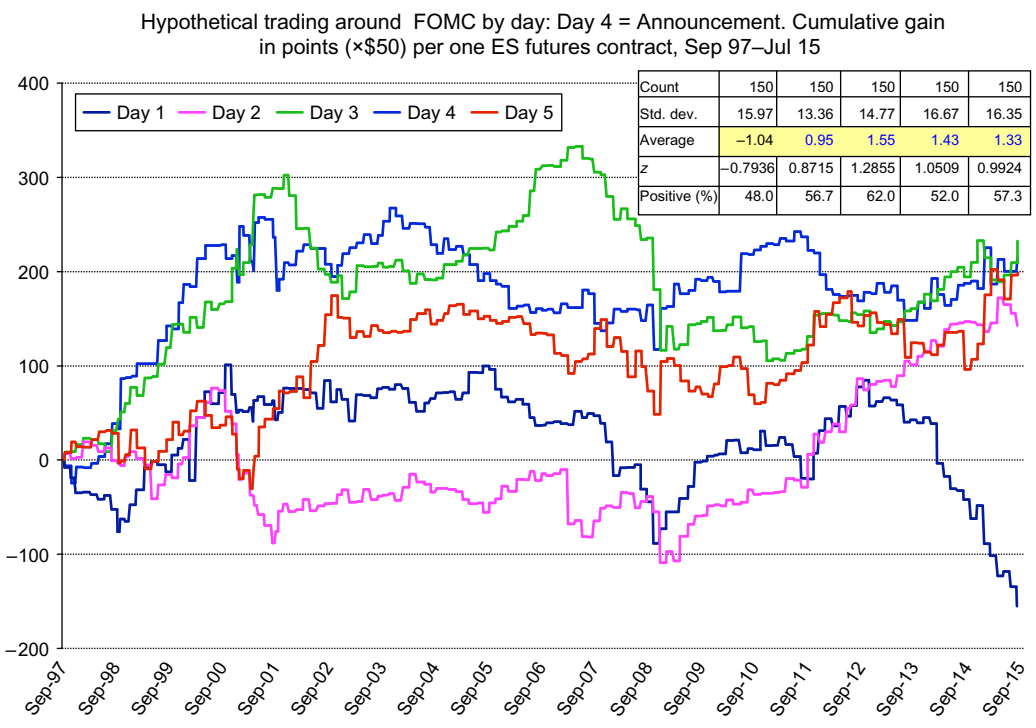


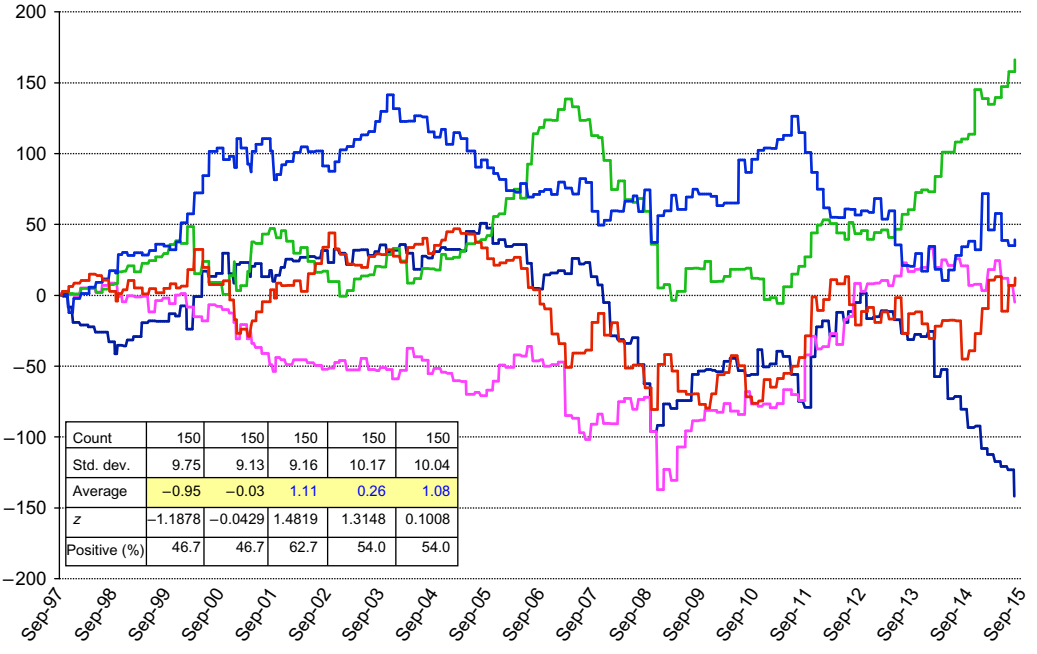
FIGURE 7. Graphs summarizing the day-by-day impact of the five-day period around the Fed meetings.

(a) S&P500



Count	150	150	150	150	150
Std. dev.	15.97	13.36	14.77	16.67	16.35
Average	−1.04	0.95	1.55	1.43	1.33
z	−0.7936	0.8715	1.2855	1.0509	0.9924
Positive (%)	48.0	56.7	62.0	52.0	57.3

(b) Russell2000



Count	150	150	150	150	150
Std. dev.	9.75	9.13	9.16	10.17	10.04
Average	−0.95	−0.03	1.11	0.26	1.08
z	−1.1878	−0.0429	1.4819	1.3148	0.1008
Positive (%)	46.7	46.7	62.7	54.0	54.0

The Fed announcement and statement is at 1 P.M. Eastern time on day 4. Days 3 and 4 prior to the announcement had high gains, but neither closed in July 2015 at the peak. Days 2, 3, 4, and 5 all had gains that were similar in total up to July 2015. Day 1 had losses that were accelerated since September 2012. The Fed anomaly is a powerful one that I watch carefully. The large cap minus small cap advantage in this period is another trading opportunity. We see that the effect during the Fed meetings is mainly in the large cap stocks with the small cap stocks hardly moving. This Fed effect did not play a major role in Battle of the Quants contest discussed below.

3. Anomalies Research

In 1988 I was invited to be the first Yamaichi Visiting Professor of Finance at the University of Tsukuba in Japan. While economics was well developed in Japanese universities, finance was not taught, despite Japan having four of the six largest brokerage firms in the world at that time. Yamaichi's idea was to have a five-year program to bring finance into Japanese universities—specifically, Tsukuba. While others who followed me focused on their prior research, I studied Japanese markets. I had two study groups on stock market crashes and calendar and fundamental anomalies. There were about 10 students/young researchers from the Yamaichi Research Institute participating in the research and taking my classes at the university. The plan was for me to present the U.S. research, and together, we would research Japan. Well-known effects such as the holiday, end of month, full turn of the month, and turn of the year, which had advantages in the U.S. markets, appeared in Japan with differing dates. At that time there were no futures markets in Japan, but there were in Singapore as well as the United States. For example, the Singapore SIMEX anticipated the Japanese turn of the month three days prior (-8 to -6 trading days) with the Japanese cash effect on days -5 to $+2$, compared with -1 to $+4$ in the United States. Here, -1 is the last trading day of the month, etc. The year was very productive, and I spent considerable time with Mr. Okada (a Yamaichi executive) discussing the markets and testing them. I explained the U.S. results, and he, through a translator, responded on the Japanese situation.

The output consisted of three jointly written books, a number of research papers plus presentations at the Berkeley program of finance and other outlets, and a course at the University of Chicago Booth Business School in 1996. I ended my stay in Japan by making a factor model based on 30 fundamental anomalies to rank the approximately 1,000 Tokyo first-section stocks. It was a model where the data are updated monthly and the model reestimated yearly. The model was useful for long, long-short, and other purposes where you wanted to rank the stocks from best to worst. I wrote up the model in Ziemba and Schwartz [91], and the London hedge fund Buchanan Partners saw it in the book, which led to a consulting job with the firm in London. Buchanan were long cheap warrants and short overpriced stocks. The model was estimated in a market that was increasing in value but did apply to the period 1992 onward, when the market was declining using minus signs.

Once I returned to Vancouver, I got a call from David Cariño, who knew me from Stanford. Frank Russell, the U.S. top financial consulting company, had hired a few operations research–type Ph.D.s to develop asset liability planning models. They had a contract with Yasuda Kasai, a major Japanese insurance company, to build a model that required a new approach rather than static mean-variance models. Among other things, they wanted "losing money" to replace variance as a risk measure. They were stuck after nine months and came to me. I suggested what became the Russell-Yasuda Kasai multiple-period stochastic linear programming planning model. The model was a big success, and the team won second prize in the 1993 Franz Edelman Practice of Management Science Prize competition, and the papers on it are in *Interfaces* (Cariño et al. [8]) and *Operations Research* (Cariño and Ziemba [6], Cariño et al. [7]). After the first model's great success, Russell took my idea of a new division involved with Russell business engineering and arranged for me to switch

to anomalies and Asian financial market research. I continued asset-liability management (ALM) research at the University of British Columbia as well as anomaly research at Russell. In 1995, I organized two weeks of the Isaac Newton Institute's six-month program in financial mathematics, which led to two Cambridge University Press books: one on ALM with John Mulvey of Princeton (Ziemba and Mulvey [90]) and the other on anomalies with Donald Keim of the University of Pennsylvania (Keim and Ziemba [47]). The ALM research continued in a monograph for the Association for Investment Management and Research (AIMR) in 2003 and the two-volume handbook of ALM with Stavros Zenios in 2006–2007. Bertocchi et al. [2] presents pension and retirement models.

Winning the Battle of the Quants Trading Competition, May 2015. My own trading has gone well, and I was pleased to win first prize in the Battle of the Quants trading competition for futures funds, beating the equity winner in May 2015 (see Ziemba [85]). My daughter Rachel presented my acceptance speech and accepted the prize for me in New York as I was lecturing Ph.D. students at the University of Bergamo on portfolio theory and asset liability management at the time of the awards ceremony.

Acceptance speech

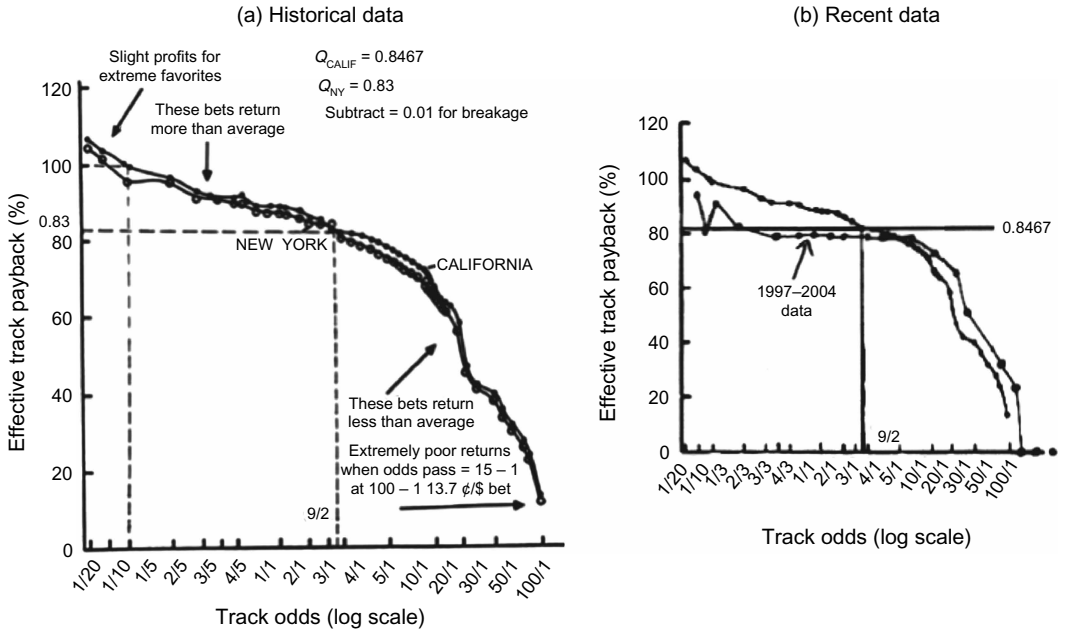
First, many thanks for the organizers for having my futures fund Alpha Z Futures Fund LLC, in their Battle of the Quants trading competition. Thanks also to my daughter Rachel Ziemba, Managing Director of Emerging Markets, Roubini Global Economics here in New York, for accepting the award for me while I am in Europe.

Rachel with Battle of Quants award

This is a victory for the anomaly approach to investing based on extensive research by others and myself. Anomalies always draw interest when I speak about them, but they are poorly understood and not used much except by a few great traders. Most people seem to believe that they either do not exist or go away quickly or have limited possibilities for gain. A prime example is the January turn-of-the-year effect where small cap stocks outperform large cap stocks. Most finance books say the effect does not exist anymore. But the reality is that it often does in the latter half of December in the futures markets. I have now traded this 20 times with positive results. Fourteen were from the 1982–1983 start through 1995–1996. I stopped because of low volume on the value line contract, plus I taught these ideas Peter Muller's group at Morgan Stanley. Years later, after a consulting job for options trader Blair Hull to study anomalies in the United States of the calendar variety as opposed to the fundamental type, I started trading again using the Russell2000 versus the S&P500 futures contract. The next six years were successful. But 2015–2016 failed. That followed the first Fed rate hike in seven years in December 2015, with the first five days of January having the worse return in U.S. stock market history for the first five days. The rest of the year has recovered and gone close to the all-time high.

My background from my Ph.D. at Berkeley with a lot of Stanford involvement was in stochastic optimization. This was a great help in portfolio theory and practice, and I became a leading theorist in the 1970s and 80s. In 1988, I went to Japan for a year as the first Yamaichi Visiting Professor at the University of Tsukuba and consultant to the Yamaichi Research Institute in Tokyo on anomalies and crash prediction models. The books by Ziemba and Schwartz [91, 92] record this period in history. I had been working on racetrack anomalies and my book *Beat the Racetrack*, with Donald Hausch (Ziemba and Hausch [87, 89]), changed the way people looked at racing. We viewed it as a financial market so then we could study biases and profit from them. That book and our later book (Hausch et al. [35, 36]), which became the bible for racetrack syndicates (especially in Hong Kong), prepared me for U.S. and Japanese calendar and fundamental anomalies through factor models.

FIGURE 8. Favorite-longshot bias.



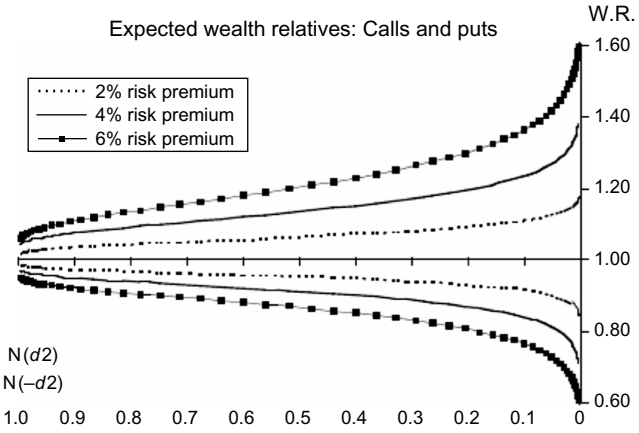
After Japan, I helped the Frank Russell company make asset liability management models and they allowed me to study practical implications of anomalies. While there I wrote papers with Chris Hensel and Doug Stone. We discovered (see Hensel and Ziemba [32, 34]) that the small caps, when held with Democrat presidents and bonds, when held during Republican administrations, gave 20 times more wealth at the end of the 1940–1995 period than the 60/40 stock bond mix that Russell was then recommending. Updating to account for three more presidents administrations—Clinton, Bush, and Obama—in 2012 gave the same 20-times result. So in my trading in the Alpha Z Fund, this is a useful strategy in the futures markets. One other strategy employed is mean reversion in the S&P500—see my *Calendar Anomalies and Arbitrage* book (Ziemba [84]) for examples in NFL playoff and Super Bowl betting, which is also potentially effective. Mispriced options can be traded with a similar bias as the racetrack favorite-longshot bias. See Figure 8.

That is similar to Kahneman and Tversky's [45] observation that in real situations, low-probability events are observed to be overestimated and high-probability events are observed to be underestimated. This leads to multiple versions of an options selling strategy with different deltas and different levels of hedging depending on projected market conditions. Other anomaly strategies that are used are turn-of-the-month and options expiry effects.

This collection of strategies, along with risk management models and the use of prediction models to monitor for potential market corrections, is the core of the Alpha Z Fund portfolios performance. For details, see Ziemba [84]. I always have an exit strategy for each new position and corrective action to modify a trade that is not working so that there are gains in the end. Again, the idea here is similar to NFL football on Betfair in London, where one might bet on team A to beat team B, but because of various mean reversion moves, one may win overall even though team B ultimately wins the game. The option bias strategies are based on the way people buy and sell options. A study of all puts and calls from 1985 to 2012 is in Ziegler and Ziemba [79]; it gives the return from buying and selling put and call options hedged or unhedged in overall markets as well as bull and bear markets. See Ziemba and Ziemba [94] for examples of the NFL playoffs and the Super Bowl. The basic idea comes, I think, from the racetrack favorite-longshot bias studies; see Figure 8.

The graph Figure 8(a) shows the net expected return (track payback less breakage for various odds levels for more than 300,000 races in various years at various racetracks). The

FIGURE 9. Theoretical put call returns based on Black–Scholes options pricing theory vs. moneyness.



Source. Ziemba [82].

difference between California and New York is the difference in track take (Ziemba and Hausch [88]). The new graph, Figure 8(b), has a lower expected return for the short odds horses and is similar for the longer odds horses. This is for data from 1997 to 2004 (Ziemba and Hausch 2008).

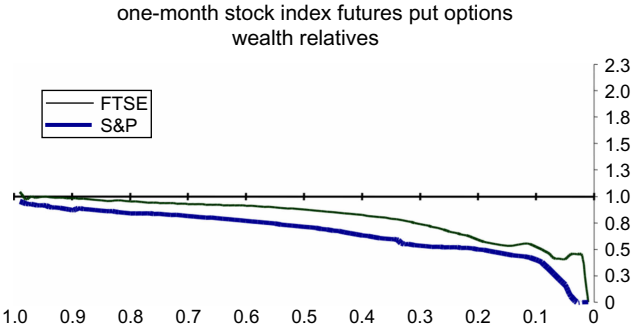
Then compare the expected value versus moneyness of puts and calls from a theoretical Black–Scholes option pricing model in Figure 9 (Black and Scholes [3]). One sees that puts are overpriced depending on equity risk premia. Then compare this to actual option price returns in Figure 10. Since insurance companies, pension funds, and others must buy protective puts regardless of their price, these are frequently overpriced with expected values 0.10 to 0.90, as shown here. So there is a big advantage in shorting these puts, but you must do it carefully—either hedged or with lots of cash (VIX dependent) behind each position.

Figure 11 simulates strategy A, which is short a ridge of puts, long a ridge of calls, and delta neutral in regard to the puts with short S&P500 futures. One can also short some deep out calls with low expected value. The graph has all quarters plus one that eliminates some quarters that are dangerous according to a predictive crash model.

Finally, Table 1 discusses possible returns from strategy A; a strategy B that is A plus additional short S&P500 futures that neutralize the long calls; and strategy C, where you sell naked puts with lots of margin money behind each position.

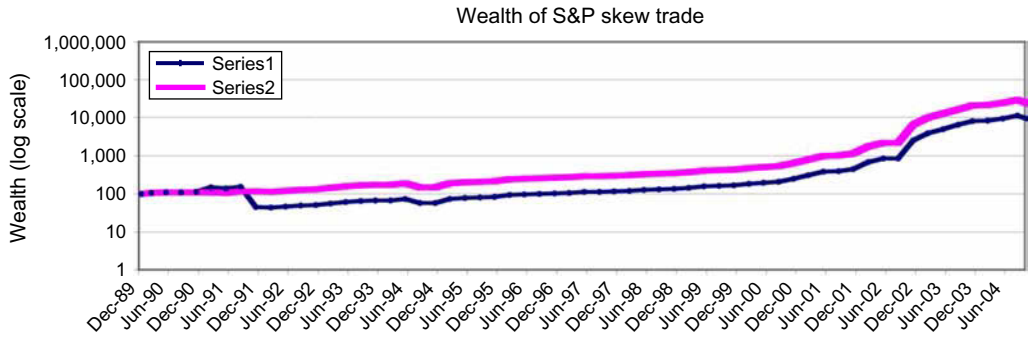
Alpha Z Advisors LLC, the entity that manages futures fund and individual accounts, is registered in the United States, and our fund is audited. I can be reached at wtzimi@mac.com, and my books and papers, etc., are on the website http://www.williamtziemba.com as well

FIGURE 10. One-month stock index futures put option wealth relatives by moneyness for S&P500 and FTSE100, 1985–2004.



Source. Ziemba [82].

FIGURE 11. Wealth of skew trade A.



Source. Ziemba [82].

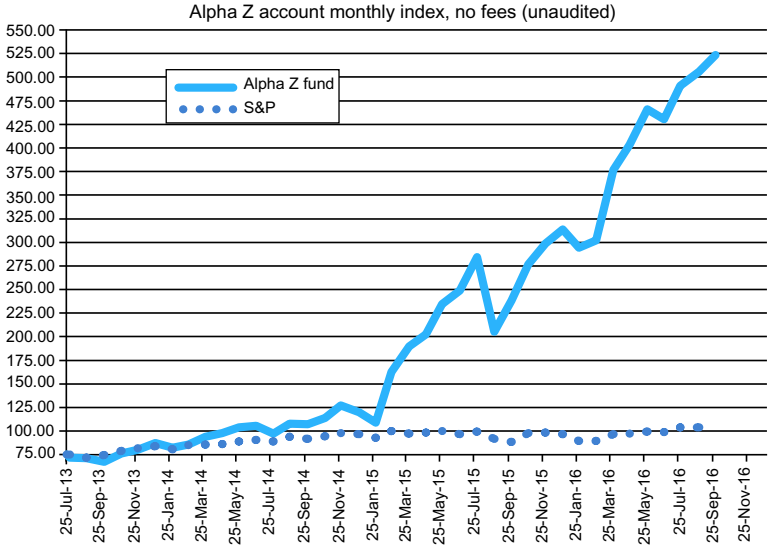
TABLE 1. Possible returns for strategies A, B, and C.

	A: Hedge puts	B: Hedge puts and calls	C: Just sell deep out puts with lots of margin
Market goes up $15\% \to 25\%$	Result large win	Small gain	Win all on deep out short puts
Market goes up $0\% \to 15\%$	Win on short puts and deep out short calls Futures lose difference between futures and spot being less than gain in market But the calls gain what the futures lose Result: large gain	Have same as A but lose a second set of short futures Could lose a little	Win all on deep out short puts
Market stays even	Win on short puts and short deep out calls Futures even Lose or long calls Win T plus difference between futures value and spot start	Win on short puts and short deep out calls Futures even Lose on long calls Win T plus difference between futures and spot at start on two sets of futures Win is substantial	Win all on deep out short puts
Market falls $0\% \to 5\%$	Large win	Large win	Win all on deep out short puts
Market falls $6\% \to 10\%$	Large win	Large win	Small win if decline is sharp and quick Large win is decline orderly
Market falls $11\% \to 15\%$	Small win	Should have large win	If decline is gradual, still can win if margin is sufficient If decline is fast, will get stopped out of some positions, so some losses
Market falls $>16\%$	Small losses, but if hedge ratio is adjusted well, losses minimal or could have a small gain	Still should win	Losses but moving down positions minimizes them

Source. Ziemba [82].

FIGURE 12. The record of the Alpha Z Futures Fund.

Date	Alpha Z fund	S&P
25-Jul-13	100.00	100.00
31-Jul-13	96.62	100.06
30-Aug-13	96.14	96.87
30-Sep-13	92.34	99.42
31-Oct-13	101.23	103.98
29-Nov-13	105.26	107.13
31-Dec-13	112.14	109.32
31-Jan-14	107.19	105.49
28-Feb-14	110.99	110.30
31-Mar-14	119.19	110.72
30-Apr-14	122.94	111.52
30-May-14	128.76	114.10
30-Jun-14	130.46	115.94
31-Jul-14	122.08	114.30
29-Aug-14	132.95	118.85
30-Sep-14	132.15	116.72
31-Oct-14	138.80	119.45
28-Nov-14	152.36	122.70
31-Dec-14	145.17	121.88
30-Jan-15	133.87	118.08
27-Feb-15	187.62	124.87
31-Mar-15	214.78	122.37
30-Apr-15	227.25	123.46
29-May-15	259.57	125.06
30-Jun-15	273.96	122.00
31-Jul-15	309.42	124.61
31-Aug-15	230.44	116.94
30-Sep-15	263.28	113.35
30-Oct-15	301.40	123.14
30-Nov-15	323.65	123.50
31-Dec-15	338.84	121.81
29-Jan-16	319.46	114.61
29-Feb-16	327.17	114.58
31-Mar-16	401.85	121.82
29-Apr-16	428.46	122.27
31-May-16	465.93	124.41
30-Jun-16	455.43	124.12
29-Jul-16	490.96	128.76
31-Aug-16	505.75	128.83
30-Sep-16	523.61	128.30



as on http://www.amazon.com. The track record of this fund (from July 2013 through May 2016) is displayed in Figure 12.

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