Chapter Two (Book: Positional Option Trading: An Advanced Guide)

The Efficient Market Hypothesis and its Limitations

A lot of trading books propagate the myth that successful trading is based on discipline and persistence. This might be the worst advice possible. A trader without a real edge who persists in trading, executing a bad plan in a disciplined manner, will lose money faster and more consistently than someone who is lazy and inconsistent. A tough but unskilled fighter will just manage to stay in a losing fight longer. All she will achieve is being beaten up more than a weak fighter would.

Another terrible weakness is optimism. Optimism will keep losing traders chasing success that will never happen. Sadly, hope is a psychological mechanism unaffected by external reality.

Emotional control won't make up for lack of edge. But before we can find an edge we need to understand why this is hard and where we should look.

The Efficient Market Hypothesis

The traders' concept of the Efficient Market Hypothesis (EMH) is, "making money is hard". This isn't wrong, but it is worth looking at the theory in more detail. Traders are trying to make money from the exceptions to the EMH, and the different types of inefficiencies should be understood, and hence traded, differently.

The EMH was contemporaneously developed from two distinct directions. Paul Samuelson (Samuelson, 1965) introduced the idea to the economics community under the umbrella of "rational expectations theory". At the same time, Eugene Fama's studies (Fama, 1965 a and b) of the statistics of security returns lead him to the theory of "the random walk".

The idea can be stated in many ways, but a simple, general expression is:

A market is efficient with respect to some information if it is impossible to profitably trade based on that information.

And the "profitable trades" are risk adjusted, after all costs.

So, depending on the information we are considering, there are many different Efficient Market Hypotheses, but three in particular have been extensively studied.

1. The strong Efficient Market Hypothesis where the information is anything that is known by anyone.
2. The semi-strong Efficient Market Hypothesis where the information is any publicly available information, such as past prices, earnings or analysts' studies.
3. The weak Efficient Market Hypothesis where the information is past prices.

The EMH is important as an organizing principle and is a very good approximation to reality. But, it is important to note that no one has ever believed that any form of the EMH is strictly true. Traders are right. Making money is hard, but it isn't impossible. The general idea of the theory and also the fact it isn't perfect is agreed upon by most successful investors and economists.

"I think it is roughly right that the market is efficient, which makes it very hard to beat merely by being an intelligent investor. But I don't think it's totally efficient at all. And the difference between being totally efficient and somewhat efficient leaves an enormous opportunity for people like us to get these unusual records. It's efficient enough, so it's hard to have a great investment record. But it's by no means impossible. "

-Charlie Munger

Even one of the inventors of the theory, Eugene Fama, qualified the idea of efficiency by using the word, "good" instead of "perfect".

"In an efficient market, at any point in time, the actual price of a security will be a good estimate of its intrinsic value."

-Eugene Fama.

There is something of a paradox in the concept of market efficiency. The more efficient a market is, the more random and unpredictable the returns will be. A perfectly efficient market will be completely unpredictable. But the way this comes about is through the trading of all market participants. Investors all try to profit from any informational advantage they have, and by doing this their information is incorporated into the prices. Grossman and Stiglitz (1980) use this idea to argue that perfectly efficient markets are impossible. If markets were efficient, traders wouldn't make the effort to gather information, and so there would be nothing driving markets towards efficiency. So, an equilibrium will form where markets are mostly efficient, but it is still worth collecting and processing information.

(This is a reason fundamental analysis consisting of reading The Wall Street Journal, and technical analysis using well known indicators is likely to be useless. Fischer Black (1986) called these people "noise traders". They are the people who pay the good traders.)

There are other arguments against the EMH. The most persuasive of these are from the field of behavioral finance. It's been shown that people are irrational in many ways. People who do irrational things should provide opportunities to those who don't. As Kipling wrote, "If you can keep your head when all about you are losing theirs,… you will be a man my son."

In his original work on the EMH, Fama mentioned three conditions that were sufficient (although not necessary) for efficiency.

- Absence of transaction costs.
- Perfect information flow.
- Agreement about the price implications of information.

Helpfully for us, these conditions do not usually apply in the options' market. Options, particularly when dynamically hedged, have large transaction costs. Information is not universally available and volatility markets often react slowly to new information. Further, the variance premium cannot be directly traded. Volatility markets are a good place to look for violations of the EMH.

Let's accept that the EMH is imperfect enough that it is possible to make money. The economists who study these deviations from perfection classify them into two classes: risk premia and inefficiencies. A risk premium is earned as compensation for taking a risk, and if the premium is mis-priced it will be profitable even after accepting the risk. An inefficiency is a trading opportunity caused by the market not noticing something. An example is when people don’t account for the embedded options in a product.

There is a joke (not a funny one) about an economist seeing a $100 bill on the ground. She walks past it. A friend asks: "Didn't you see the money there?" The economist replies: "I thought I saw something, but I must've imagined it. If there had been $100 on the ground, someone would've picked it up." We know that the EMH is not strictly true, but the money could be there for two different reasons. Maybe it is on a busy road and no one wants to run into traffic. This is a risk premium. But maybe it is outside a bar where drunks tend to drop money as they leave. This is an inefficiency. There is also the possibility that the note was there purely by luck.

It is often impossible to know whether a given opportunity is a risk premium or an inefficiency, and a given opportunity will probably be partially both. But it is important to try to differentiate. A risk premium can be expected to persist: the counter-party is paying for insurance against a risk. They may improve their pricing of the insurance, but they will probably continue to pay something.

In contrast, an inefficiency will last only until other people notice it. And failing to differentiate between a real opportunity and a chance event will only lead to losses.

Some traders will profit from inefficiencies. Not all traders will. A lot of traders will use meaningless or widely known information. Many forecasts are easy. I can predict the days the non-farm payroll will be released. I can predict what days fall on weekends. I can predict the stock market closes at 4 pm eastern time. In many cases, making a good prediction is the easy part. The hard part is that the forecast has to be better than the market’s, which the consensus of everyone else’s prediction is. For developed stock indices the correlation between the daily range on one day and the next is roughly between 65% and 70%. So a very good volatility estimator is that it will be what it was the day before (a few more insights like this will lead you to GARCH). It is both hard and profitable to make a one-day forecast that gives a 75% correlation with the previous day. And whether it is because the techniques that are used are published, employees leave and take information with them, or just that several people have a similar idea at the same time, these forecast edges don’t last forever.

Aside: Alpha Decay

The extinction of floor traders is an example of a structural shift in markets destroying a job. Like most people, traders tend to think that their skills are special, and their jobs will always be around. This isn’t true. The floors have gone. Fixed commissions have gone. Investment advisors are being replaced by robo-advisors. There are fewer option market makers, each trading many more stocks than in the past. Offshoring will definitely come to trading, and it is quite possible that a market structure like a once a day auction could replace continuous trading.

But as well as these structural changes, the alpha derived from market inefficiencies (as opposed to the beta of exposure to a mis-priced risk factor) doesn’t last forever. Depending on how easy it is to trade the effect, the half-life of an inefficiency-based strategy seems to be between six months and five years. Mclean and Pontiff (2016) showed that the publication of a new anomaly lessens its returns by up to 58%. And publication isn’t the only thing that erodes alpha. Chordia et al. (2014) showed that increasing liquidity also reduces excess returns by about 50%. Sometimes the anomaly only exists because it isn’t worth the time of large traders to get involved. A similar effect is that the easy access to data will kill strategies. Sometimes the alpha isn't due to a wrinkle in the financial market. It is due to the costs of processing information.

Just as some traders will profit by using a stupid idea like candlestick charting, some traders will succeed for a while with an over-fit model. I'm in no way using this to condone data-mining, but we can learn a valid lesson from this. As Guns and Roses pointed out, "nothing lasts forever". Lucky strategies will never last but even the best, completely valid strategy will have a lifetime. So, when you are making money don't think that being "prudent" is a good idea. The right thing to do is to be as aggressive as possible. Amateurs go broke for a lot of reasons, but professionals often suffer in bad times because they didn't fully capitalize on good times, instead thinking that making steady but small profits was the best thing to do.

They also spend too much in good times, forgetting that they won't last. I've literally had a floor trader tell me about his new Ferrari about an hour before laughing about the stupid spending habits of NFL and NBA players (the last I heard he was selling houses). Many times, traders have short careers because a valid strategy dies. Amateurs blow up, but professionals don't allow for alpha-decay. For example, many floor traders didn't survive the death of the open outcry pits. Their edge disappeared, and their previous spending habits left them with little (In this case "trickle down" economics was correct, as profits from market making trickled down to prostitutes, strippers and cocaine dealers. At least it wasn't wasted.).

Behavioral Finance

"Think about how stupid the average person is, then realize half of them are stupider than that."

-George Carlin.

The history of markets is nowhere near as big as we often assume. For example, equity options have only been traded in liquid, transparent markets since the CBOE opened in 1973. S&P 500 futures and options have only been traded since 1982. The VIX didn't exist until 1990 and wasn't tradable until 2004. And the average lifetime of an S&P 500 company is only about 20 years. In the long-term, values are related to macro variables such as inflation, monetary policy, commodity prices, interest rates and earnings. And these change on the order of months and years. Even worse, they are all co-dependent.

So, what might seem like a decent length of history that we can study and look for patterns, quite possibly isn't (this does not apply to HFT or market-making where a huge number of data points can be collected in what is essentially a stationary environment). When it comes to volatility markets, I think that while there appear to be many thousands of data points, there might only be dozens. A better way to think of market data might be that we are seeing a small number of data points, and that they occur a lot of times.

I think this makes quantitative analysis of historical data much less useful than is commonly thought.

But there is something that has been constant: human nature.

Humans have been essentially psychologically unchanged for 300,000 years when Homo Sapiens (us) first appeared. This means that any effect that can conclusively be attributed to psychology will effectively have 300,000 years of evidence behind it. This seems to be potentially a much better situation.

The problem with psychological explanations (for anything) is that they are incredibly easy to postulate. As the baseball writer Bill James said, ‘‘Twentieth-century man uses psychology exactly like his ancestors used witchcraft; anything you don’t understand, it’s psychology.’’ The finance media is always using this kind of pop psychology to justify what happened that day. “Traders are exuberant” when the market goes up a lot; “Traders are cautiously optimistic” when it goes up a little etc. I try not to do this, but I’m as guilty as anyone else. I think psychology could be incredibly helpful, but we have to be very careful in applying it. Ideally, we want several psychological biases pointing to one tradeable anomaly and we want them to have been tested on a very similar situation to the one we intend to trade.

Further, traders aren’t psychologists and reading behavioral finance at any level from pop psychology to real scientific journals is probably just going to lead to hunches and guesses. To be fair, traders currently make the same mistakes from reading articles about geo-politics or economics. One week, traders will be experts on the effects of tariffs on soybeans and the next week they will be talking about Turkish interest rates. It is far easier to sound knowledgeable than to actually be so. It isn’t obvious that badly applied behavioral psychology is any more useful than badly applied macroeconomics. And it is obvious that traders can’t do better than misapply either.

After I explained this nihilistic view to an ex-employer he said, “Well I have to do something”. And what we do is exactly what I’ve said isn’t very good: we apply statistics and behavioral finance. These are far from perfect tools, but they are the best we have. The edges they give will be small, but some edges can be found. We will always know only a small part of what can be known. Making money is hard.

Proponents of behavioral finance contend that various psychological biases cause investors to systematically make mistakes that lead to market inefficiencies. Behavioral psychology was first applied to finance in the 1980s but for decades before that psychologists were studying the ways people actually made decisions under uncertainty.

The German philosopher Georg Hegel is famous (as much as any philosopher can be famous) for his triad of thesis, antithesis and synthesis. A thesis is proposed. An antithesis is the negation of that idea. Eventually, synthesis occurs, and the best part of thesis and antithesis are combined to form a new paradigm. Ignoring the fact that Hegel never spoke about this idea, the concept is quite useful for describing the progress of theories. A theory is proposed. Evidence is found that supports the theory. Eventually it becomes established orthodoxy. But after a period, either for theoretical reasons or because new evidence emerges, a new theory is proposed which is strongly opposed to the first one. Arguments ensue. Many people become more dogmatic and hold on tightly to their side of the divide, but eventually aspects of both thesis and antithesis are used to construct a new orthodoxy.

From the early 1960s until the late 1980s the EMH was the dominant paradigm amongst finance theorists. These economists modeled behavior in terms of rational individual decision-makers who made optimal use of all available information. This was the “thesis”.

In the 1980s an alternative view developed, driven by evidence that the rationality assumption is unrealistic. Further, the mistakes of individuals may not disappear in the aggregate. People are irrational and this causes markets to be inefficient. Behavioral finance was the antithesis.

Synthesis hasn't yet arrived, but behavioral finance is now seen as neither an all-encompassing principle nor as a fringe movement. It augments, not replaces, traditional economics.

What have we learned from behavioral finance?

First, behavioral finance has added to our understanding of market dynamics. Even in the presence of rational traders and arbitrageurs, irrational "noise" traders will prevent efficiency. And although it is possible to justify the existence of bubbles and crashes within a rational expectations framework (for example Diba and Grossman, 1988), a behavioral approach gives more reasonable explanations (for example, Abreu and Brunnermeier, 2003 and De Grauwe and Grimaldi, 2004).

Second, we are now aware of a number of biases, systematic misjudgments that investors make. Examples include:

Overconfidence: an unreasonable belief in one's abilities. This leads traders to assign too narrow a range of possibilities to the outcome of an event, to underestimate the chances of being wrong, to trade too large and to be too slow to adapt.

Over-optimism: Overconfidence compresses the range of predictions. Over-optimism biases the range, so traders consistently predict more, and better opportunities than really exist.

Availability heuristic: We base our decisions on the most memorable data even if it is atypical. This is one reason teeny options are over-priced. It is easy to remember the dramatic events that caused them to pay off, but hard to remember the times when nothing happened, and they expired worthless.

Short-term thinking: the irrational preference for short-term gains at the expense of long-term Performance.

Loss aversion: Investors dislike losses more than they like gains. This means they hold losing positions, hoping for a rebound even when their forecast has been proven wrong.

Conservatism: being too slow to update forecasts to reflect new information.

Self-attribution bias: attributing success to skill and failure to luck. This makes Bayesian updating of knowledge impossible.

Anchoring: relying too much on an initial piece of information (the "anchor") when making a forecast. This leads traders to update price forecasts too slowly because the current price is the anchor and seem more "correct" than it should.

And at least 50 others.

It is these types of biases that traders have tried to use to find trades with edge. Results have been mixed. There are so many biases that practically anything can be explained by one of them. And sometimes there are biases that are in direct conflict. For example, investors under-react, but they also over-react. Between these two biases you should be able to explain almost any market phenomena. The psychologist and finance theorists working in the field are not stupid. They are aware of these types of difficulties and are working to disentangle the various effects. The field is a relatively new one and it is unfair and unrealistic to expect there to be no unresolved issues. The problem is not really with the field or the serious academic papers. The problem is with pop psychology interpretations and investors doing “bias mining” to justify ideas.

It is common in science for a new idea to be overly hyped, particularly those that are interesting to lay people (traditional finance is not interesting). In the 1970’s there were popular books about catastrophe theory, a branch of physics which was meant to explain all abrupt state changes and phase transitions. It didn’t. In the 1990’s, chaos theory was meant to explain practically everything, including market dynamics. It didn’t. Behavioral finance is being over exposed because it is interesting. It provides plenty of counter-intuitive stories and also a large amount of schadenfreude. We can either feel superior to others making stupid mistakes, or at least feel glad that we aren’t the only ones who make these errors.

And people love intuitive explanations. We have a great need to understand things, and behavioral finance gives far neater answers than statistics of classical finance theory. Even though behavioral finance doesn’t yet have a coherent theory of markets, the individual stories give some insight. They help to demystify. This is reassuring. It gives us a sense of control over our investments.

A science becoming interesting to the general public doesn’t necessarily mean it is flawed. For example, there have been hundreds of popular books on quantum mechanics. However, behavioral finance does have some fairly serious problems to address.

Just as in conventional finance theory, behavioral finance studies individual decision making despite the fact that people do not make investing decisions independently of the rest of society. Everyone is influenced by outside factors. Most people choose investments based on the recommendations of friends (Katona, 1975). And professionals are also influenced by social forces (Beunza and Stark, 2012). Over the last 30 years the sociology of markets has been an active research field (for example Katona, 1975, Fligstein and Dauter, 2007, and references therein), but this work hasn’t yet been integrated into behavioral finance. Because behavioral finance largely ignores the social aspects of trading and investing, we don’t have any idea of how the individual biases aggregate and their net effect on market dynamics. This is necessary because, even though we don’t understand how aggregate behavior emerges, it is very clear that markets cater to irrational behavior rather than eradicate it. For example, the services of financial advisors, stock brokers and other financial intermediaries made up 9% of the US GDP (Philippon, 2012) despite the fact that they are almost all outperformed by much cheaper index funds and ETFs.

Next, behavioral finance has largely limited itself to the study of cognitive errors. There are many other types of non-rational behavioral inputs into decision making, including emotion, testosterone levels, substance abuse and the quest for status.

And behavioral finance gives no coherent alternative theory to the EMH. A catalog of biases and heuristics, the mistakes people make, is not a theory. A list of facts does not make a theory. Of course, sometimes observations are necessary before a theory can be formulated. Mendeleev drew the periodic table well before the atomic structure of matter was understood. We knew species existed well before we understood the process of speciation by natural selection. Still, to be scientific, behavioral finance eventually needs to lead to a unifying theory which gives explanations of the current observations and makes testable predictions.

Behavioral finance can still help. Whenever we find something that looks like a good trading idea we need to ask, “Why is this trade available to me?” Sometimes the answer is obvious. Market makers get a first look in exchange for providing liquidity. Latency arbitrage is available to those who make the necessary investments in technology. ETF arbitrage is available to those with the capital and legal status to become authorized participants. But often a trade with positive edge is available to anyone who is interested. Remembering the joke about the economists, “why is this money sitting on the ground?” Risk premia can often be identified by looking at historical data, but behavioral finance can help to identify real inefficiencies. For example, post earnings announcement drift can be explained in terms of investor under-reaction. Together with historical data, this gives me enough confidence to believe that the edge is real. The data suggests the trade, but the psychological reason gives a theoretical justification.

High Level Approaches: Technical Analysis and Fundamental Analysis

Technical Analysis

Technical analysis is the study of price and volume to predict returns. Aronson (2007) categorized technical analysis as either subjective or objective. It is a useful distinction.

Subjective technical analysis incorporates the trader’s discretion and interpretation of the data. For example, “If the price is over the EWMA I might get long. It depends on a lot of other things. “These methods aren’t wrong. They aren’t even methods. Subjectivity isn’t *necessarily* a problem in science. A researcher subjectively chooses what to study, and then subjectively chooses the methods that make sense. But if subjectivity is applied as part of the trading approach, rather than the research, then there is no way to test what works and what doesn’t. Do some traders succeed with subjective methods? Obviously. But until we also know how many fail, we can’t tell if the approach works. Further, the decisions different traders who use ostensibly the same method, make won’t be the same or even based on the same inputs. There is literally no way to test subjective analysis.

Some things that are intrinsically subjective are Japanese candlesticks, Elliot waves, Gann angles, trend lines and patterns (flags, pennant, head and shoulders etc.). These aren’t methods. In the most charitable interpretation, they are a framework for (literally) looking at the market. It is *possible* that using these methods can help the trader implicitly learn to predict the market. But more realistically, subjective technical analysis is almost certainly garbage. I can’t prove the ideas don’t work. No-one can. They are unfalsifiable because they aren’t clearly defined. But plenty of circumstantial evidence exists that this analysis is worthless. None of the large trading firms or banks have desks devoted to this stuff. They have operations based on stat arb, risk arb, market making, spreading, yield curve trading and volatility. No reputable, large firm has a Japanese candlestick group.

As an ex-boss of mine once said, “That isn’t analysis. That is guessing.”

Any method can be applied subjectively, but only some can be applied objectively. Aronson defines objective technical analysis as “well defined repeatable procedures that issue unambiguous signals”. These signals can then be tested against historical data and have their efficacy measured. This is essentially quantitative analysis.

It seems likely that some of these approaches can be used to make money in stocks and futures. But each individual signal will be very weak and to make any consistent money various signals will need to be combined. This is the basis of statistical arbitrage. This is not within the scope of this book.

However, we do need to be aware of a bad classic mistake when doing quantitative analysis of price or return data: data mining

This is where we sift through data using many methods, parameters and time scales. This is almost certain to lead to some strategy that has in-sample profitability. When this issue is confined to choosing the parameters of a single, given strategy it is usually called "over fitting". If you add enough variables, you can get a polynomial to fit data arbitrarily well. Even if you choose a function or strategy in advance, by "optimizing" the variables you will the get the best in sample fit. It is unlikely to be the best out of sample. As the mathematician and economist John Neuman said, "With four parameters I can fit an elephant, and with five I can make him wiggle his trunk."

This mistake isn't only made by traders. Academics also fall into the trap. The first published report of this was Ioannidis (2005) and Harvey et. al (2016), and Hou et. al (2017) discuss the impact of data mining on the study of financial anomalies.

There are a few ways to avoid this trap.

- The best performer out of a sample of back tested rules will be positively biased. Even if the underlying premise is correct, the future performance of the rule will be worse than the in-sample results.
- The size of this bias decreases with larger in-sample data sets.
- The larger the number of rules (including parameters), the higher the bias.
- Test the best rule on out of sample data. This gives a better idea of its true performance.
- The ideal situation is where there is a large data set and few tested rules.

Even after applying these rules, it is prudent to apply a bias correcting method.

The simplest is Bonferroni's correction. This scales any statistical significance number by dividing by the number of rules tested. So, if your test for significance at the 95% confidence level (5% rejection) shows the best rule is significant, but the rule is the best performer of 100 rules, the adjusted rejection level would be 5%/100 or 0.005%. So, in this case, a t-score of 2 for the best rule doesn't indicate a 95% confidence level. We would need a t-score of 2.916, corresponding to a 99.5% level for the single rule. This test is simple but not powerful. It will be overly conservative and skeptical of good rules. When used for developing trading strategies this is a strength.

A more advanced test is White's reality check. This is a bootstrapping method that produces the appropriate sampling distribution foe testing the significance of the best strategy. The test has been patented and commercial software packages that implement the test can be bought. However, the basic algorithm can be illustrated with a simple example.

We have two strategies, A and B, which produce daily returns of 2% and 1% respectively. Each was developed by looking at 100 historical returns. We can use WRC to determine if the apparent outperformance of strategy A is due to data-mining.

- Using sampling with replacement, generate a series of 100 returns from the historical data.

- Apply the strategies (A and B) to this ahistorical data, to get the pseudo strategies A' and B'.
- Subtract the mean return of A from A' and B from B'.
- Calculate the average return of the return-adjusted strategies, A" and B".
- The larger of the returns of A" and B" is the first data point of our sample distribution.
- Repeat the process N times to generate a complete distribution. This is the sampling distribution of the statistic, maximum average return of the 2 rules with expected return of zero.
- The p-value (probability of our best rule being truly the better of the two) is the proportion of the sampling distribution whose values exceed the returns of A, i.e. 2%.

A realistic situation would involve comparing many rules. It is probably worth paying for the software.

There is also a totally different and complementary way to avoid over-fitting. Forget about the time-series of the data and study the underlying phenomenon. A hunter doesn't much care about the biochemistry of a duck, but she will know a lot about their actual behavior. In this regard a trader is a hunter, rather than a scientist. Forget about whether volatility follows a GARCH(1,1) or a T-GARCH(1,2) process, the important observation is that it clusters in the short-term and mean-reverts in the long-term. If the phenomenon is strong enough to trade, it shouldn't be crucial what exact model is used. Some will always be better in sample, but that is no guarantee that they will work best out of sample.

As an example, this is the correct way to find a trading strategy.

There is overwhelming evidence that stocks have momentum. Stocks that have outperformed tend to continue outperforming. This has been observed for as long as we have data (for example see Geczy and Samnov(2016), Lempérière (2014) and Chabot et al. (2009)) and in many countries (for example, Fama and French, 2010). The observation is robust with respect to how momentum is defined and the time-scales over which it is measured. In the trading world, the evidence for stock momentum is overwhelming. Starting from this fact, design a simple model to measure momentum (e.g. 6-month return). Then sort stocks by this metric and buy the ones that score well.

The worst thing to do is take a pre-defined model and see if it works. Has a 30-day, 200-day moving average crossover been predictive of VIX futures? What if we change the first period to 50 days? I don't know or care…

Fundamental Analysis

Fundamental analysis aims to predict returns by looking at financial, economic and political variables. For example, a fundamental stock analyst might look at earnings, yield, sales and leverage. A global macro trader might consider GDP, currency levels, trade deficit and political stability.

Fundamental analysis, particularly Global Macro, is particularly susceptible to subjectivity. It also tempts otherwise intelligent people to make investment decisions based on what they read in the Wall Street Journal or The Economist. It is exceedingly unlikely that someone can consistently profit from these public analyses, no matter how well the story is sourced or how smart the reader is.

Consider these statements from "experts".

"Financial storm definitely passed"

-Bernard Baruch: economic advisor to Presidents Woodrow Wilson and Franklin Roosevelt in a cable to Winston Churchill, November 1929.

Stocks dropped for the next 3 years, with the Dow losing 33% in 1930, 52% in 1931 and 23% in 1932.

“The message of October 1987, should not be taken lightly. The great bull market is over.”

-Robert Prechter: prominent Elliot wave theorist and pundit, in November 1987. The Dow rallied for 11 of the next 12 years, giving a return (excluding dividends) of over 490%.

“A bear market is likely…It could go down 30 or 40%”.

-Barton Biggs: chief strategist for Morgan Stanley, October 27th, 1997.

The Dow had its largest one day gain on October 28th and continued to rally hard for the next six months.

In most situations it is just mean to make fun of people’s mistakes. We all make mistakes. But the people I have quoted have proclaimed themselves experts in a field where real expertise is very, very rare.

And evidence of this is more than anecdotal.

The poor prediction skill of experts is a general phenomenon. Gray (2014) summarizes the results of many studies that show that simple, systematic models out-perform experts in fields as diverse as military tactics, felon recidivism and disease diagnosis. Expertise is needed to build the models, but experts should not make case-by-case decisions.

Koijen et al. (2015) show that surveys of economic experts (working for corporations, think tanks, chambers of commerce and NGOs) has a negative correlation to future stock returns. They were also contra-indicative for the returns of currencies and bonds. This effect applies across 13 equity markets, 19 currencies and 10 fixed income markets. A simple “fade the experts” strategy would have given a Sharpe ratio of 0.78 from 1989 to 2012.

Financial advisors are equally bad. Jenkinson et al. (2015) look at the performance of advisors in picking mutual funds. They conclude with,” we find no evidence that these recommendations add value, suggesting that the search for winners, encouraged and guided by investment consultants, is fruitless”. And fund managers themselves can’t consistently beat the averages. Due to costs, most managers underperform and there is no correlation between performances from one year to the next. So, managers can’t pick stocks and it is pointless to try to pick good managers.

It is also likely that much of the “alpha” generated by fundamental analysis is smart beta, compensation for exposure to a certain risk factor. There is absolutely nothing wrong with this. Trading profits are profits, no matter whether they are due to smart beta or alpha. But before we ascribe a trader’s results to skill, we should know what is causing the profits. Beta should cost a lot less than alpha.

Conclusion

It is difficult to make money in financial markets. The Efficient Market Hypothesis isn’t completely true but it is closer to being correct than to being wrong. If a trader can’t accept this she will see edges in noise and consequently overtrade. Behavioral finance, technical analysis and fundamental analysis can all be used as high level organizing principles for finding profitable trades but each of these needs to be believed only tentatively, and the most robust approach is to look for phenomena that are independently clear. For example, momentum can be discovered through technical analysis but also understood as a behavioral anomaly. The observable phenomenon must come before any particular method.

Summary

- Exceptions to the EMH exist but they are rare.
- Exceptions will either be inefficiencies; temporary phenomena that last only until enough people notice them, or poorly priced risk premia.
- Risk premia will persist and can form the core of a trader's operations but the profits due to inefficiencies will decay quickly and need to be aggressively exploited as soon as they are found.
- A promising trading strategy is one whose basis is independent of the specific methods used to measure it. Start with observation, then move to quantification and justification.

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