

The Value of Predictions, or Where'd All This Rain Come From?

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The motivation for trying to guess the direction of stocks or bonds is easy to understand. Observers have for years noted wide price swings, calculated the value of a dollar invested at the bottoms and disinvested at the tops and compared the result against the value of a dollar invested under a "buy-and-hold" strategy. The difference is always temptingly large. But none of the forecaster's attempts to capture the swings has any value unless his or her predictions are right.

I agree with John Kenneth Galbraith, who said, "We have two classes of forecasters: Those who don't know—and those who don't know they don't know." If it were easy to predict the future, it would be easier to attain excellent investment results—then maybe everyone could have above-average performance.

Let's face it: Most of us have roughly the same ability to predict the future. The trouble is, being right as often as the average forecaster won't produce superior results.

Every investor wants above-average results. In the institutional world, relative performance is the Holy Grail. The objective is to be the first to see the future—and take the appropriate route to profit. It obviously doesn't help to be right only as often as others are.

Being "right" doesn't lead to superior performance if the consensus forecast is also right. If the consensus

forecast for real GNP growth is 5%, for example, then stock prices will come to reflect that expectation. If you conclude that GNP will grow at 5% and your expectation of rapid growth motivates you to buy stocks, the prices of the stocks you buy will already reflect that anticipated growth. If actual GNP growth of 5% is subsequently announced, stock prices probably will not jump; their reaction to 5% growth took place when the consensus forecast was arrived at. The best guess is that you will earn the normal risk-adjusted return for equities over your holding period. Bottom line: Correct forecasts do not necessarily translate into superior investment results.

Correctly Forecasting Extreme Events

It was noted at least 25 years ago that stock price movements were highly correlated with changes in earnings. So people concluded that accurate forecasts of earnings were the key to making money in stocks. It has since been realized, however, that it's not earnings changes that cause stock price changes, but earnings changes that come as a *surprise*. Look in the newspaper. Some days, a company announces a doubling of earnings and its stock price jumps. Other doublings don't even cause a ripple—or even prompt a decline.

The key question is not, "What was the change?" but rather, "Was it anticipated?" Was the change accurately predicted by the consensus and thus factored into the stock price? If so, the announcement should cause little reaction. If not, the announcement should cause the stock price to rise if the surprise is pleasant or to fall if it is not.

This raises an important Catch 22. Everyone's forecasts, taken together, form the consensus forecast. If your prediction is consensus too, it won't produce above-average performance even if it's right. Superior performance comes from *accurate nonconsensus* forecasts. Because most forecasters aren't terrible, actual results fall near the consensus most of the time—and nonconsensus forecasts are usually wrong.

Table 1 gives the payoff table in terms of performance. The problem is that extraordinary performance comes only from correct nonconsensus forecasts, but *nonconsensus forecasts are hard to make correctly and hard to act on*.

Table 1. Forecast Payoffs

	Consensus Forecast	Nonconsensus Forecast
Accurate	Average	Above Average
Not Accurate	Average	Below Average

When interest rates stood at 8% in 1978, most people thought they'd stay there. The interest rate bears predicted 9% and the bulls predicted 7%. Most of the time, rates would have been in that range, and no one would have made much money.

The big profits went to those who predicted 15% long bond yields. But where were those people? Extreme predictions are rarely right, but they're the ones that make you big money.

The fact is, most forecasters predict a future quite like the present. One reason is that things generally continue as they have been; major

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changes don't occur very often. Another is that most people don't do "zero-based" forecasting, but start with the current observation or normal range and then add or subtract a bit as they think appropriate. Lastly, real "sea changes" are extremely difficult to foretell.

That's why some of the best-remembered forecasts are ones that extrapolated current conditions or trends but were wrong. *Business Week* may never live down "The Death of Equities" and "The Death of Bonds." At the mid-1990 lows, the press suggested that no one would ever buy a high-yield bond again. In 1989, nobody thought the Cowboys would ever win without Tom Landry, or that the Lakers or 49ers would ever lose. Six years ago, the growth of both coasts' economies was considered assured, and the Rustbelt's suffering was expected to continue forever. Only two years ago, George Bush was a shoo-in.

And that brings me to my subtitle: Where'd All This Rain Come From? The motivation for this memo came as I considered the extraordinary amount of precipitation the West experienced earlier this year—and newspaper articles of late 1992. According to the articles, the rings on old trees suggested that 50-year droughts might be the norm and the five-year drought then going on just the beginning.

No one predicted the drought before it began—when such a forecast might have helped. But just as it may have been about to end, the possibility of its long-term continuation was unveiled. It's at just such times—such inflection points—when accurate forecasts of change would be the most valuable, that they are the hardest to make.

Take high-yield bonds, for instance. In 1989 and 1990 they absorbed a continual beating as a series of negative developments came together. There was the recession, the failure of a number of the leveraged buyouts of the 1980s, enactment of excessively stringent regulation and the collapse of Drexel Burnham, Columbia Savings and Executive Life. All this was tied together—and ac-

centuated—by lots of overly negative publicity.

Each development was another drip of water torture. Each one put an end to some investor's ability to remain optimistic. And so each one eliminated a potential buyer, created a seller, and moved prices lower.

And after all, what is a market bottom? It's that moment when the last holder who will become a seller actually does so—hence the moment when prices hit levels that will prove to be the lows. From that point on, with no one left to turn negative, a few pieces of good news or the arrival of a few buyers with belief in values are enough to turn a market.

The crescendo of negativism, the lowest prices and the greatest difficulty in predicting a rise all occur simultaneously. No wonder it's hard to profit from forecasting.

Let's say the average investor was approached in October 1990 by someone who had enough imagination and courage (because that's what was needed) to make a positive case for high-yield bonds. Would the investor have believed and bought? Probably not.

Potentially profitable, nonconsensus forecasts are very hard to believe in and act on for the simple reason that they are so far from conventional wisdom. If a forecast were totally logical and easily accepted, then it would be the consensus forecast (and its profit potential would be much less).

If someone told you that U.S. auto makers' share of the domestic market was going back to 100% in five years, that would be a forecast with enormous implications for profit. But could you possibly believe it? Could you act on it?

The more a prediction of the future differs from the present, (1) the more likely it is to diverge from the consensus forecast, (2) the greater the profit will be if it's right, and (3) the harder it will be to believe in and act on it.

Not only must a profitable forecast have the event or direction right, it must be correct as to timing as well.

Let's say you accepted the forecast that the Big Three would again come

to own 100% of the U.S. market, and you bought the stocks in response. What if, a year later, their share was lower (and their stocks too)? Could you continue to hold out for the long term, or would your resolve weaken? What if their shares (and stocks) were unchanged five years later? Wouldn't you give up? And wouldn't that be just in time to see the prediction come true?

In poker, "scared money never wins." In investing, it's hard to hold fast to an improbable, nonconsensus forecast and do the right thing . . . especially if the clock is telling you the forecast is off base. As I was told years ago, "being too far ahead of your time is indistinguishable from being wrong."

Forecasts Can Cost You Money

Going into and out of the markets based on forecasts of broad market movements will work only if the forecasts are right (and right more often than the consensus is right). I would argue that, because forecasting is uncertain, it's safer *not* to try.

For example, people hold equities because they find prospective long-term equity returns attractive. The average annual return on equities from 1926 to 1987 was 9.44%. But if you had gone to cash and missed the best 50 of those 744 months, you would have missed *all* the return. This tells me that attempts at market timing are a source of risk, not protection.

It would be nice to be able to vary the amount invested in anticipation of subsequent performance, but I think it's just too risky to try. The best thing might just be to settle for average long-term performance in markets that are hard to predict.

Efficient marketeers think stock market forecasts are about as good as coin tosses. If you're right half the time without bias, your forecasts won't help or hurt versus a buy-and-hold policy. But forecasts are implemented through transactions, which cost money. If you're right half the time and spend money to try, your performance will fall further below buy-and-hold results the more trading you do.

We always read, "I think the stock market's going to go up." We never read, "I think the stock market's going to go up (and eight out of my last 30 predictions were right)" or "I think the stock market's going to go up (and by the way, I said the same thing last year and was wrong)." Can you imagine deciding which baseball players to hire without knowing their batting averages? When did you ever see a market forecaster's track record?

Most Forecasts Don't Allow for Alternative Outcomes

I imagine that, for most money managers, the process goes like this: "I predict the economy will do A. If A happens, interest rates should do B. With interest rates of B, the stock market should do C. Under that environment, the best-performing sector should be D, and stock E should rise the most." The portfolio expected to do best under that scenario is then assembled.

But how likely is E anyway? Remember that E is conditioned on A, B, C and D. Being right two-thirds of the time would be a great accomplishment in the world of forecasting. But if each of the five predictions has a 67% chance of being right, then there is a 13% probability that all will be correct and the portfolio will perform as expected.

And what if some other scenario unfolds? How will the portfolio do? How do forecasters/investors make allowances in their portfolios for the likelihood that their predictions will prove incorrect?

"Why Me?"

If someone has made a potentially valuable forecast with a high probability of being right, why is it being shared with you? Think how profitable a correct market forecast could be. With very little capital, a good forecaster could make many times more in the futures market than in salary from an employer. Okay, let's say he likes to work for other people—then why does his employer give his forecasts away rather than sell them? Maybe the thing to ask yourself is whether you would write out a check to buy the forecast you're considering acting on.

Groucho Marx said, "I wouldn't join any club that would have me as a member." Another formulation may be, "I would never act on any forecast that someone would share with me." I'm not saying that no one has above-average forecasting ability. Rather, as one University of Chicago professor wrote in a paper years ago, such forecasters are more likely to be sunning themselves in St. Tropez than going around entreating people to borrow their forecasts.

In sum, I feel that

- most forecasters have average ability,
- consensus forecasts aren't helpful,
- correct nonconsensus forecasts are potentially very profitable but are also hard to make consistently and hard to act on, and
- forecasts cost money to implement and can be a source of risk rather than return.

The implications are clear: I, at least, will continue to eschew portfolio management based on forecasts of market trends, about which I think neither I nor anyone else knows much.

Instead, I will continue to try to "know the knowable"—that is, to work in markets that are subject to biases, in which noneconomic motivations hold sway, and in which it is possible to obtain an advantage through hard work and superior insight. I will work to know everything I can about a small number of things, rather than a little bit about everything.

Convertible securities, high-yield bonds and distressed-company debt are all markets in which market inefficiencies give rise to unusual opportunities in terms of return and risk. These opportunities can be exploited in a manner that is risk-averse and not reliant on macro-forecasts.