

Memo To: Oaktree Clients

From: Howard Marks

Re: The Value of Predictions II (or "Give That Man a Cigar")

Date: July 22, 1996

In a February 1993 memo entitled "The Value of Predictions," I expressed my negative opinion of attempts to predict the macro-future. Now, to follow up, I've examined a handful of semi-annual Wall Street Journal economic surveys I've been stashing away. Please note that this was not a scientific study; my sample was limited to the contents of my desk drawer. The conclusions are interesting nevertheless.

First, can accurate forecasts be made? The record shows the predictions of the Journal's average "expert" to have added little value in terms of predicting the future. The table below shows the wide margin by which the consensus missed the mark.

	<u>90-day bill rate</u>	<u>30-year bond rate</u>	<u>Yen/\$</u>
December '93			
12-Month Prediction	3.7%	6.4%	115
December '94 Actual	5.7	7.9	100
December '94			
6-Month Prediction	6.5	7.9	104
June '95 Actual	5.4	6.6	85
December '94			
12-Month Prediction	6.4	7.6	107
December '95 Actual	5.1	5.9	103
June '95			
6-Month Prediction	5.4	6.6	89
December '95 Actual	5.1	5.9	103
June '95			
12-Month Prediction	5.3	6.6	92
June '96 Actual	5.2	6.9	110
December '95			
6-Month Prediction	4.9	6.0	105
June '96 Actual	5.2	6.9	110

On average, these predictions were off by 15%. **In the three sets of half-year data I had available, the average expert forecaster couldn't even get closer than 96 basis points when attempting to predict the level of long rates six months out!** And missing long rates by 96 basis points implies missing the price of the \$1000 long bond by \$120.

Second, are these forecasts of any value? My limited survey shows the average forecast published by the Journal has not been helpful. The key isn't whether the forecasters accurately predicted the **level** of the parameters but, (since you make money by anticipating change), whether they were right about the **likelihood** of significant change and its **direction**. That these forecasts weren't of value can be seen clearly in the following table, which looks at changes rather than levels.

	<u>90-day bill rate</u>	<u>30-year bond rate</u>	<u>Yen/\$</u>
December '93 12-Month			
Predicted Change	+ 60 b.p.	+ 10 b.p.	+3
Actual Change	+260 b.p.	+160 b.p.	-12
December '94 6-Month			
Predicted Change	+ 80 b.p.	--0--	+4
June '95 Actual	- 30 b.p.	-130 b.p.	-15
December '94 12-Month			
Predicted Change	+ 70 b.p.	- 30 b.p.	+7
December '95 Actual	- 60 b.p.	-200 b.p.	+3
June '95 6-Month			
Predicted Change	--0--	--0--	+4
December '95 Actual	- 30 b.p.	- 70 b.p.	+18
June '95 12-Month			
Predicted Change	- 10 b.p.	--0--	+7
June '96 Actual	- 20 b.p.	+ 30 b.p.	+25
December '95 6-Month			
Predicted Change	- 20 b.p.	+ 10 b.p.	+1
June '96 Actual	--0--	+100 b.p.	+7

As the table shows, it's not that the forecasters were always wrong; when there was little change, they were often right. It's just that in times of major changes, (when accurate forecasts would've helped one make money or avoid a loss), the forecasters completely missed them. In the years reviewed, the expert consensus failed to predict **all** of the major developments. Included here are interest rate increases of 1994 and 1996, the rate decline of 1995, and the massive gyrations of the dollar/yen relationship. In summary, there simply hasn't been much correlation between predicted changes and actual changes.

Third, where do these forecasts come from? The answer is simple: If you want to see a high correlation, take a look at the relationship between current levels and predicted future levels. The table below, which does just that, shows a remarkably better "fit."

	<u>90-day bill rate</u>	<u>30-year bond rate</u>	<u>Yen/\$</u>
December '93 Actual	3.1%	6.3%	112
12-Month Prediction	3.7	6.4	115
December '94 Actual	5.7	7.9	100
6-Month Prediction	6.5	7.9	104
December '94 Actual	5.7	7.9	100
12-Month Prediction	6.4	7.6	107
June '95 Actual	5.4	6.6	85
6-Month Prediction	5.4	6.6	89
June '95 Actual	5.4	6.6	85
12-Month Prediction	5.3	6.6	92
December '95 Actual	5.1	5.9	103
6-Month Prediction	4.9	6.0	105

Now that's a correlation! On average, the predictions were within 5% of the levels which prevailed at the time they were made. When rates were low, the experts predicted that they would stay low; after rates rose, they were expected to stay high. High dollar/yen exchange rates brought high dollar/yen forecasts, and vice versa. There's no question about it: each consensus forecast represented a near-extrapolation of then-current levels. Like many forecasters, these economists were driving with their eyes firmly fixed on the rearview mirror.

On the one occasion, in 1994, when the consensus of forecasters was bold enough to venture a prediction for short rates which differed substantially from the then-current levels, they got even the **direction** of the subsequent change wrong. The problem is that, rather than extrapolate the year-end 1994 level, they extrapolated the 1994 trend, which reversed in 1995.

In general, we can say with certainty that these forecasters were much better at telling us where things stood than where they were going. This bears out the old adage that **"it's difficult to make accurate predictions, especially with regard to the future."** The corollary is also true: predicting the past is a snap.

And using the prevailing levels to predict the future would have been just about as effective as the average forecast. The prevailing levels differed from the future levels by 16% on average, while the consensus prediction erred by 15%.

Fourth, can't anyone get it right? It is absolutely not true that nobody makes accurate forecasts. Every six months, when the Journal reports on a new survey of forecasts, it takes the opportunity to cite the forecaster in the previous survey who came closest to accurately predicting the three financial indicators shown above plus the change in GNP and CPI. It prints the winner's picture and lauds the unique insights which led to the accurate forecasts.

And the truth is that the winner's accuracy is often startling, as shown in the following table with regard to what we consider the most important of the indicators, the interest rate on the 30-year Treasury bond. Each time, the winner's forecast was quite close to the actual and much more accurate than the consensus.

	Susan Sterne Economic Anal. Assoc. <u>December 1994</u>	James Smith Univ. of No. Caro. <u>June 1995</u>	Michael Cosgrove The Econoclast <u>December 1995</u>
Winner's Prediction	6.80%	6.05%	6.90%
Subsequent Actual	6.62	5.94	6.89
Consensus Prediction	7.92	6.60	6.00

Looking at the winning forecasters' results shown above, one might even be tempted to conclude that accurate predictions are in fact achievable.

Fifth, then why do I remain so negative on forecasters' ability? The important thing isn't getting it right once. It's doing so consistently.

The table below shows two things that might make you think twice about heeding the winners' forecasts. First, they generally failed to make accurate predictions in surveys other than the one they won (shown in bold). And second, in the surveys they didn't win, their forecasts were much more wrong than even the inaccurate consensus half the time.

	<u>December 1994</u>	<u>June 1995</u>	<u>December 1995</u>
Susan Sterne	6.80%	6.00%	5.00%
James Smith	7.40	6.05	5.55
Michael Cosgrove	7.50	7.70	6.90
Consensus Prediction	7.92	6.60	6.00
Subsequent Actual	6.62	5.94	6.89

As the Journal itself pointed out in reviewing the results of the December 1995 survey:

. . . by giving up the comfort of the consensus, those on the fringes of the economic prediction game often end up on the winning or losing end. James Smith of the University of North Carolina and Susan Sterne of Economic Analysis Associates, the winners six months and one year ago, respectively, **didn't even get the direction of interest rates right this time.** The same happened last year to

Wayne Angell of Bear, Stearns Securities Corp., the winner before them
(Emphasis added)

An interesting pattern emerges from the data shown above. In all three surveys, Ms. Sterne's prediction was the lowest of the three experts and Mr. Cosgrove's was the highest. One way to get to be right is to always be bullish or always be bearish -- if you hold a fixed view long enough, you may be right sooner or later. And if you're always an outlier, you're likely to eventually be applauded for an extremely unconventional forecast that correctly foresaw what no one else did. But that doesn't mean your forecasts are regularly of any value.

A lot of adages fit this data. I've heard it said that "even a blind squirrel occasionally finds an acorn," "a stopped clock is right twice every day" and "if you put enough monkeys in a room with typewriters, eventually one of them will write the Bible."

I feel the sum of this data shows that it's possible to be right about the macro-future once in a while, but not on a regular basis. It doesn't do any good to possess a survey of 64 forecasts that includes a few which are accurate; you have to know which ones they are. And if the accurate forecasts each six months are made by different economists, it's hard to believe there's much value in the collective forecasts.

By the way, there's an important analogy to be drawn here: Efficient market advocates don't say it's impossible to beat the market; lots of people do it every year. (Remember, half the observations in any sample are above the median.) They only assert that no one can consistently do so in risk-adjusted terms.

Finally, can macro-forecasts be used to gain an advantage? I pointed out in my 1993 memo that most of the time, you can't get superior results with inaccurate forecasts **or** with accurate forecasts that reflect the consensus. (This is because the consensus view of the future is already embedded in the price of an asset at the time you buy it). To bring above average profits, a forecast generally must be different from the consensus **and** accurate.

But, as I described in 1993, it's difficult with regard to a non-consensus view of the future (1) to believe in it, (2) to act on it, (3) to stand by it if the early going suggests it's wrong, and (4) to be right. Those who invest based on fringe predictions are often wrong to an embarrassing and costly extent.

At Oaktree, we don't spend our time attempting to guess at the future direction of economies, rates and markets, things about which no one seems to know more than anyone else. Rather, we devote ourselves to specialized research in market niches which others find uninteresting, unseemly, overly complicated, beyond their competence or not worth the effort and risk. These are the inefficient markets in which it is possible to gain a "knowledge advantage" through the expenditure of time and effort. They also happen to be markets in which micro factors relating to companies, assets and securities matter the most. This is where it's possible to find bargains, and only bargain purchases can be counted on to dependably lead to returns which are above-average relative to the risk entailed. We say "**we try to know the knowable**" -- **and that doesn't include the macro-future.**

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