Forecasting Session

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I expect Michael Darby invited me to participate knowing my views of forecasting, expressed eleven years ago in my Presidential address to this association. I will summarize them in two sentences -

**FIRST (1)** - Economics is not the science that provides accurate quarterly or annual forecasts of GDP, inflation or other macro variables

**SECOND (2)** - There is no such science.

One of the main developments in economic science in the past generation is the random walk theory. Most short-term movements are often about as well-described by R.W.s as by other forecasts. This statement is **NOT** entirely true. At times like the present, when output grows at a variable rate from year to year, without recessions, forecasters can do better than the R.W. for some variables.

When I considered forecasts in 1986, one rule of thumb
was that forecasters errors, on average, predicting real GDP growth were about 2/3 to 100% of the average rate of change. In recent years, forecasts have improved. Errors by the best forecasters are only about 1/3 of the average rate of growth for the period 1985 to date. This improvement says more about the long expansion -- 15 years with one mild recession -- than about improvements in forecasting.

How good is a standard deviation of one-third the average growth rate? Average growth for the period is about 2.6% so a two root mean square error band for the best forecaster is roughly 2.6 ± 1.6 or from 1.0% to 4.2% -- i.e. From sluggish to exuberant growth. Better than before when forecasters could not distinguish booms from recessions -- but not very helpful.

That may explain why the National Association of Business Economists -- many of whose members would have been forecasters a decade ago -- has changed its name to reflect the change in mission of many of its members. Forecasting has not gone out of style, but it is no longer a growth industry. The reason, I suspect, is that the value added is much smaller than many economists once believed.

A recent issue of the Economic Review published by the San Francisco Fed has a paper showing forecast errors for
forecasts of quarterly GDP using monthly data. In my earlier study, I found that forecasters using data for the first two months of the quarter, could not reduce the errors very much, if at all.

The authors, Ingenito and Trehan, found that the best they could do was a SEE equal to about 1/2 the average growth rate for the period 1985 to 1995 -- about the same period used for annual forecasts. Annual forecasts are subject to smaller errors in this period. It appears better to look over a longer time period. This is consistent with my 1987 conclusion about forecasts of real GDP.

The lesson to be drawn for economic policymakers is not the usual "forecast often". The size of errors raises a question about why policymakers respond to forecasts at all. Similarly it is hard to understand why policymakers pay attention to noisy, high frequency data when deciding on policy actions.

A second issue about forecasts concerns both high and low frequency data. Suppose a student asks you? "Professor, what is the trend growth rate of the U.S. economy?" You can answer that question by looking in the tables, but we all know that such an answer tells us about what we measure, not what has happened. The correct answer, I believe, is: "We
don't know, but it is probably between 2 and 3-1/2%”. This is a relatively wide range, and even that wide range should be accompanied by a statement that if the growth rate is near the top of the range, we are not sure how long it will stay there. The main reasons are: (1) we don't have much verified understanding about why it increased, if it has; and (2) we don't know whether the increase is a change in level, distributed overtime, or a permanent change in growth rate. Anyone who has followed the literature and trend vs. difference stationarity knows this is a hard issue.

I submit that the answer we give to that issue is far more important for public and private decision making than any judgment made using quarterly or annual forecasts. Take as an example the long-term problem of what is to be done about promises for health and public pensions. If the economy is growing at 3.5% instead of 2.5% and continues to do so, -- the increase in growth is permanent. There will be approximately $1 \times 10^{12}$ of additional real GDP at 1997 prices in 10 years rising to $4 \times 10^{12}$ in 25 years. The public health and retirement problem are very different in the two cases.

At the individual level, planning my portfolio to decide how to save for retirement, along with choice of mate and career, is one of the three most important decisions an
individual makes. If asset prices are close to random walks, as they appear to be, short-term forecasts cannot help much. Better information about low frequency movements would be much more useful. The same is true for corporate investment decisions.

To conclude this section: I have had the opportunity to both participate in, and observe, individual, corporate, and government decisions at levels up to the highest. I cannot think of a single time when the decision was improved by forecasts of quarterly or annual events. Unfortunately, I can think of the opposite -- major mistakes that were made by growing attention to high frequency data.

The example I have chosen is the use of quarterly inflation forecasts in the 1970s and 1980s. This is a good example, an important policy error, although I could choose other examples of major mistakes based on misinterpreting high frequency changes or basing policy on short-term forecasts.

From 1976 to 1982, the Federal Reserve relied on a Phillips curve equation to forecast inflation. Each quarterly error, without exception, was negative. The equation always underpredicted inflation in this period. On average inflation rose. The first overprediction did not come until 1982, when
inflation declined.

These erroneous forecasts helped to convince Federal Reserve policymakers to underestimate inflation and act too hesitantly to prevent it.

My reason for choosing this relation is that a revised version of the same equation -- a new Phillips curve -- is currently used at the Fed and in the forecasting community generally to predict inflation. The predictions depend on estimates of the natural rate, or its companion NAIRU. No economist should believe that NAIRU is a constant. There are good reasons to believe that the NAIRU or the natural rate changes with tax rates, deregulation, demographics, devaluation, labor laws and other factors. As discussed earlier, we don't know with much precision what the current growth rate is so we can't infer what the current equilibrium value of output or NAIRU is.

It is not surprising, therefore, that forecasts of inflation based on revised Phillips curves cannot predict current or near-term inflation with useful accuracy and cannot say much about why inflation has remained low.

Consider what happened this winter. Forecasters were misled by a mild winter. A few additional houses in Maine, a few sunbonnets in Vermont and seasonally adjusted
measured real GDP soared. Suddenly the inflation forecasters were excited. They predicted continued increases in the Federal funds rate throughout the year, a tightening of Fed policy to head off the boom.

It appears now that they mistook the seasonal for the trend. Instead of warning markets that economy had been on a 2 to 3% average growth path (as measured), many extrapolated part of the first quarter increase, possibly adding to the noise and variability in financial markets.

This raises a serious issue. One of the major problems of forecasting is to separate permanent or persistent from transitory or temporary changes. There are reliable ways of making that separation, but they cannot draw any conclusion from a single month or quarter. Often they cannot tell much for a year or longer.

Let me now make a forecast that illustrates part of this problem. At the end of this month, Commerce will release revisions of the GDP accounts. I predict, and I am not alone, that the revisions will raise the level of GDP, thus raising the measured growth rate. Suddenly, recent productivity growth will be much higher than reported. This revision will have the interesting side effect of making those who forecast close to correctly wrong. The errors by the “best forecasters” that I
Some may hear these remarks as a criticism of forecasting in general and the Phillips curve in particular. That would not be entirely right. I find the Phillips curve useful but not as a forecasting equation for inflation.

When teaching macro-economics, I introduce the Phillips curve as a short-run supply equation. I regard it as a useful construct to explain why changes in aggregate demand are distributed between changes in output and prices or inflation. But as an explanation of inflation, it is but one blade of the Marshallian scissors. Predicting inflation from the position of output on the supply curve is somewhat similar to predicting the average sales price of a particular model of automobiles from the sticker price on the window. There is no information about demand. Demand changes are an important part, probably the more variable part of the inflation, unemployment story.

Consider the following. As we all know, the Phillips curve is vertical in this long-run. Long-run output is independent of inflation. The short-run Phillips curve is negatively sloped; lower unemployment is associated with higher inflation, ceteris paribus. And conversely, lower inflation is associated with higher unemployment during the transition.
Observations for recent decades show exactly the opposite. Unemployment and inflation rose together on average in the 1970s and have generally fallen together in the 1990s.

There is no paradox. The difference is in demand, as the chart suggests. The chart shows differences in Fed. Policy -- measured by the acceleration of the monetary base -- in three long expansions of the past forty years. The 1960s, 1980s, and 1990s.

Insert Chart

Let me turn, finally, to two questions for Roger or for anyone else? First, why do we as a profession devote so many resources to short-term forecasting and so few to improving the data?

Of course, one part of the answer is that there is a market for forecasts; they have a positive price. This perhaps explains DRI and Roger, but it doesn't explain CBO, the Federal Reserve -- both the banks and the Board -- the Treasury, the IMF, and other governments and agencies. Nor does it explain, the efforts within universities.

Long ago, our colleagues in finance gave up forecasting stock prices and turned to other tasks. They explained that asset market prices are close to random walks -- hence not
subject to forecasts that are much better than chance. Second question: why don’t we do the same, at least for quarterly or annual forecast?

For the past few years, there has been a growing recognition by officials of the credibility problem and, consequently, growing interest in the development of consistent, predictable policies -- systematic, rule-like procedures. Many of these procedures, like Taylor’s rule, do not depend on forecasts. Taylor’s rule, and other rules that have been used, are adaptive monetary rules.

Elsewhere, in Britain, New Zealand, Sweden and elsewhere, central banks have developed nominal GDP or price rules. Again, some of these procedures do not rely on forecasts.

This makes the first question more pressing: Why do we as a profession over-invest in forecasting?