
Comment

Christopher A. Sims, Princeton University and NBER

Data sets that combine information on forecasts and actions by economic decision makers are rare, so this paper's analysis of such a data set is interesting. It supports well its conclusions that expectations of earnings growth are related to investment plans and to actual investment at the firm level, and that these relationships do not wash out when aggregated. This suggests, as the paper argues, that expectations, at least by executives in firms, could be a useful measure of aggregate disturbances in the economy and might be an important part of the transmission mechanism by which disturbances to the economy propagate.

Many empirical rational expectations models assume that expectations are not only rational, they are redundant, in the sense that the observable data used to fit the model can fully explain expectations, so that survey data on expectations contribute nothing to fitting the model. This paper would like to convince us that such models can be seriously deficient. To do so, it offers two kinds of evidence. One kind shows that survey data on expectations have predictive value for macroeconomic aggregates, and that their predictive value does not diminish as other macroeconomic variables are added to a predictive regression. The other kind shows that survey expectations are not rational, in the sense that the survey forecasts make systematic and predictable errors. Let's call these two properties of expectations that the paper would like to disprove "redundancy" and "rationality."

The paper recognizes that expectations can be redundant without being rational, and claims that it is most interested in showing us that expectations are not rational. Nonetheless in tables 6 and 7 we are shown that in the presence of 11 other possible variables, alone or in some combinations that might be used to predict investment, CFO expectations

of earnings growth retain predictive power. But the coefficients on CFO expectations do change, by more than a factor of two, and by statistically significant amounts, as the other variables are introduced into the equation, and the other variables are in many cases highly statistically significant. The simple static baseline regression in these tables, with next year's investment dependent only on expectations of earnings growth and last period's growth in assets, is derived in the appendix from a simple static investment theory. But that theory has no room for the additional variables introduced in these tables, so the tables might be read as rejection of the theory—along with the fragile assumptions about terms that can be ignored in the derivation.

My own view of these two tables is that they are best read as first-pass assessments of the redundancy issue. If we leave aside the theory, we can read the tables as showing that there is predictive power for investment in the survey expectations data, and this predictive power does not go away in the presence of other variables that people have used in empirical investment models. But from that perspective, one would like to see a more ambitious analysis. The variables considered in these tables are all persistent and all clearly related to one another. These regressions are single equations from a one-lag quarterly VAR system that is never estimated, but ought to be the real focus of interest. Furthermore, with these quarterly data, that a one-lag VAR captures all the dynamics is quite implausible.

To address the rationality issue the paper gives us regressions of the difference between actual earnings growth over the next 12 months and the survey forecasts of earnings growth over the next 12 months on various types of information available at the time the forecasts were made. The regressions seem to show a pattern of extrapolative expectations—forecasts tend to be too high when past earnings growth was above normal. Calling these expectations “extrapolative” might give a misleading impression. As the paper points out, and as its appendix figure C1 shows, actual earnings growth is mean reverting, with actual growth higher when lagged growth was lower and vice versa. So the errors being made are not necessarily “high growth will continue” forecasts, but probably “high growth will decline” forecasts that underestimate the amount of the decline.

The evidence presented here is suggestive, but not completely convincing. The nonzero coefficients on lagged earnings in these regressions are indeed large in table 8, panels (A) and (B), and table 9, panel (A), but the coefficient is estimated, with high precision, to be more

than five times smaller in table 9, panel (B). The R^2 of the regression also collapses in table 9, panel (B), to only .003. The t -statistics on the past earnings variable remain about the same in table 9, panel (B), but this is despite the number of observations having risen to on the order of 100,000 from on the order of 100. From table 9, panel (B) alone, one might conclude that there is very strong evidence that the analyst's forecasts at the firm level show negligible tendency to extrapolate. That the table 9, panel (B), results fail to aggregate to the panel (A) results suggests that for analysts, there is very little tendency to extrapolate from a firm's idiosyncratic recent past performance, but some tendency to do so from some measure of aggregate economic conditions not present in the regressions.

The paper discusses in section VI.A the possibility that the CFOs are not forecasting the same object that their forecasts are being compared to. That discussion makes it clear that they are not mostly forecasting month-over-month growth rather than year-over-year growth. But to create bias, it is not necessary that most of the CFOs are misinterpreting the forecast base, or even that all their misinterpretations are the same. There is also an issue about timing. The surveys are handed out with a return deadline early the last month of the quarter. The CFOs therefore cannot possibly know the earnings of the last month of the quarter, and very likely do not have precise current information about the previous month's earnings. If monthly earnings growth were i.i.d., as would be the case if earnings were a random walk with drift, the best forecast of them with no current information would simply be their mean. If CFOs were making their forecasts this way, using only data on the first 10 months of the last year, the best achievable R^2 in explaining their forecast errors (as those are defined in the paper's regressions) would be .167, and the coefficient on lagged earnings would be negative. To be sure, the R^2 in the aggregated CFO table 8, panel (A) are higher than this, and lagged 12-month earnings alone would not achieve the .167 R^2 in this simple example. Nonetheless, the simple example casts doubt on the strength of the evidence in these data for irrationality.