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"Macroeconomic Forecasting is Alive and Well"

by

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The reports of the death of large-scale macroeconomic forecasting models are *not* exaggerated (e.g., Peter Passell, "The Model Was Too Rough: Why Economic Forecasting Became a Sideshow," *The New York Times*, February 1, 1996). But many outside observers, including Passell, incorrectly interpret the failure of the large-scale macro-econometric models as indicative of a bleak future for macroeconomic forecasting more generally.

Passell's premise is incorrect and his conclusion is unwarranted in any event. First, the large-scale macroeconomic forecasting models were not failures in all respects: although they didn't live up to their original promise, they nevertheless made lasting contributions -- they spurred the development of identification and estimation theory, powerful computational and simulation techniques, comprehensive machine-readable macroeconomic databases, and much else. Second, past failures do not necessarily imply a bleak future: we learn from our mistakes. Just as macroeconomics has benefitted from rethinking since the 1970s, so too will macroeconomic forecasting.

Understanding the future of macroeconomic forecasting requires understanding the evolution of the nonstructural and structural approaches to forecasting, and the corresponding interplay between measurement and theory.[1] I advance a two-part thesis:

(1) Nonstructural econometric forecasting has progressed steadily over the last century and continues to do so.

(2) Structural econometric forecasting, which by necessity lags innovations in

[1] "Nonstructural" macro-econometric models capture the correlations in observed macroeconomic time series without relying on any particular economic theory. Structural models, in contrast, explain the correlations in observed series as the outcome of purposeful economic behavior.

economic theory, receded during the times of rapid theoretical advance in the 1980s and early 1990s but will likely reemerge as the new theory matures. The new structural macroeconomic forecasting models, however, will bear little resemblance to their ancestors.

In short, macroeconomic forecasting, broadly defined, is alive and well. Nonstructural forecasting has always been well and continues to improve, while structural forecasting has been dormant for some time but is poised for resurgence.

To make my case, I first chronicle the evolution and progress of nonstructural forecasting in section 1. Then, in section 2, I first trace the advance and retreat of Keynesian theory and structural forecasting, and then the emergence and growth of modern dynamic stochastic general equilibrium theory. I argue that a related new breed of structural macroeconomic forecasting is similarly emerging and will likely continue to grow. I conclude in section 3.

1. Nonstructural Forecasting[2]

Nonstructural econometric forecasting has progressed steadily over the last century and continues to do so. In this section, I chronicle that progress, which serves several purposes. First, nonstructural forecasting is of tremendous interest in its own right and is one of the great successes of modern econometrics and statistics. Second, it provides background and contrast relevant for our subsequent discussion of the evolution of structural forecasting. Finally, it provides a partial guide to the future of structural forecasting, which will

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[2] For a more detailed introduction to modern nonstructural time series forecasting, see Diebold (1997).

incorporate many of the elements of modern nonstructural forecasting. I break the chronology, which by necessity is highly selective, into two rough parts: pre- and post-1970.

Pre-1970

The 1920s were a period of fertile intellectual development in nonstructural modeling and forecasting. Many key ideas were hatched and nurtured, which even if not particularly sophisticated from today's vantage point, laid the groundwork for the impressive technical advances of the ensuing decades. In particular, Slutsky (1927) and Yule (1927) argued that simple linear stochastic difference equations provide a convenient and powerful framework for modeling and forecasting a variety of time series, including those that arise in economics, finance, and business. They proposed and studied autoregressive and moving average processes, which remain great workhorses of applied forecasting. Frisch (1933) put the Slutsky-Yule framework to work in formulating the idea of impulse and propagation mechanisms in economic dynamics.

As the 1930s and 1940s unfolded, macroeconomists and econometricians busied themselves largely with Keynesian theory and Keynesian structural econometrics. Nevertheless, a pathbreaking nonstructural contribution was made by the economist H. Wold, after which nonstructural techniques progressed impressively in the hands of the mathematicians N. Weiner and A. Kolmogorov, and the engineer R. Kalman.

Wold showed that, given sufficient stability of the underlying probabilistic mechanism generating a time series, its stochastic part can be represented as a model of the form studied by Slutsky and Yule.[3] Thus, the Slutsky-Yule models are not only convenient and powerful,

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[3] In the formal jargon, Wold required the series to be "covariance stationary."

they are absolutely central -- they're the only game in town. Weiner and Kolmogorov worked out the mathematics of optimal linear forecasting for models satisfying Wold's conditions; that is, they produced the formula for the linear projection of a future value of a series onto the infinite sequence of its current and past values. Kalman completed the theory by working with a general and flexible representation called state space form. Kalman's formulae, which have a convenient recursive form amenable to real-time forecasting, are called the Kalman filter. Prediction using the Kalman filter allows for relaxation of the covariance stationarity assumption and also allows for conditioning on only a finite past.

The Wold-Weiner-Kolmogorov-Kalman theory, which effectively represents the pinnacle of the Slutsky-Yule research program, is beautifully exposited in Whittle (1963, second edition 1983). Appropriately enough, a leading economist, T. Sargent, wrote the second edition's introduction, which catalogs the tremendous impact of the prediction theory on modern dynamic economics.

Post-1970

Early on, the nonstructural forecasting explosion consisted in large part of econometricians simply discovering and appreciating the powerful advances made by the likes of Wold, Weiner, Kolmogorov and Kalman. But a key development in the linear tradition occurred with the publication of the landmark book by Box and Jenkins (1970; third edition Box, Jenkins and Reinsel, 1994).

First, Box and Jenkins promoted and popularized the use of autoregressive moving average (ARMA) models, which are very closely related to the autoregressive and moving average models of Slutsky and Yule, but which have the potential to approximate dynamics more accurately and with fewer parameters than purely autoregressive or moving average models.[4] Moreover, Box and Jenkins integrated ARMA models into a workable framework for applied forecasting, consisting of iterative cycles of model formulation, estimation, diagnostic checking, and forecasting.[5]

Second, Box and Jenkins focused professional attention on the idea that, under certain conditions, trending economic time series may be made stationary by differencing. Series that are appropriately detrended by differencing are said to contain stochastic trend, or a unit root.[6] The pioneering, if controversial, work of Dickey and Fuller (e.g., Fuller, 1976) on unit root testing grew from attempts to understand whether and when differencing is appropriate.

Third, Box and Jenkins heightened professional awareness of the fact that series with stochastic trend behave in certain respects very differently from series with a fixed deterministic trend. In particular, shocks to integrated series have permanent effects, so that long-run forecasts fail to revert to any fixed trend; effectively, the underlying trend location is redefined each period.[7] That message was subsequently amplified and elaborated in the

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[4] In the Box-Jenkins parlance, which has since become standard, ARMA models are said to be *parsimonious*.

[5] See also Nerlove, Grether and Carvalho (1979) and Harvey (1989), who study unobserved components models, in which observed series are treated as additively composed of underlying latent trend, seasonal, cyclical and noise components, and show their intimate relationship to ARMA models.

[6] Stock and Watson (1988a) provide a good introduction to stochastic trends and unit roots. Unit root processes are also called integrated processes, and the Box-Jenkins strategy of fitting ARMA models to differenced data is called ARIMA (autoregressive *integrated* moving average) modeling.

[7] See Diebold and Senhadji (1996) for an illustration in the context of U.S. aggregate output forecasting.

empirical macroeconomics literature associated with such important contributions as Nelson and Plosser (1982) and Campbell and Mankiw (1987).

Following the seminal work of Box and Jenkins, an ongoing flood of work has sought to complete their research program. Because macroeconomics is crucially concerned with multivariate dynamics, many extensions of the Box-Jenkins program involve multivariate modeling and forecasting. Early examples include Granger (1969) and Sims (1972), who develop methods for exploring causal patterns in multivariate systems. The Granger-Sims causality notion is predictive, not philosophical; we say that x Granger-Sims causes y if the history of x is useful for forecasting y, over and above the history of y.

Sargent and Sims (1977) and Geweke (1977) develop dynamic factor models, in which some economic shocks are common across sectors, whereas others are idiosyncratic. The common shocks produce realistic comovements among variables and facilitate parsimonious modeling and forecasting of large numbers of variables. Dynamic factor models are particularly useful with the emergence of macroeconomic panel datasets, including cross-country, cross-region, and cross-state data. Hence the recent resurgence of interest in the idea, as in Quah and Sargent (1993) and Forni and Reichlin (1997).

Granger (1981) and Engle and Granger (1987) develop the idea of cointegration, which is closely related to factor structure. Two series are cointegrated if each is individually integrated, yet there exists a linear combination that is covariance stationary and hence mean reverting. Thus, for example, each of two series x and y may be integrated, but the spread x-y may be mean reverting. A key insight is that in an N-variable cointegrated system, although each variable has a univariate stochastic trend, the system is driven by fewer than N underlying stochastic trends, because some are shared. This is the essence of the Stock-Watson (1988b) "common trends" representation of cointegrated systems and is precisely the same idea as with the dynamic factor model.

Cointegration is potentially important for forecasting, because if present it lets us allow for serially-correlated deviations from equilibrium in short run forecasts, while nevertheless incorporating the idea of long-run reversion to equilibrium.[8] This is the idea of error-correction, pioneered by Sargan (1964) and long a cornerstone of "LSE econometrics."[9] Indeed, there is a formal equivalence between cointegration and error correction, as established in Engle and Granger (1987).

Most of the multivariate elaborations of the Box-Jenkins tradition are conveniently implemented in the central model of modern multivariate linear forecasting, the vector autoregression, or VAR.[10] The VAR was advocated in econometrics by Sims (1980) as a less restrictive alternative to traditional econometric simultaneous equations models, in which variables were rather arbitrarily labeled "endogenous" or "exogenous."

In a univariate autoregression, we approximate dynamics by regressing a variable on lags of its own past. In a vector autoregression, by way of logical extension, we regress each variable in the system on lags of itself and lags of every other variable in the system. Least squares estimation of VARs is not only statistically efficient, but also simple and numerically

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[8] Engle and Yoo (1987), for example, establish the important result that long-run forecasts from cointegrated systems satisfy the cointegrating relationships exactly.

[9] For a good discussion, see Hendry (1995).

[10] Witness, for example, recent books such as Lütkepohl (1991) and Johansen (1995).

stable, in contrast to the tedious numerical optimization required for estimation of multivariate ARMA models. Early on, moreover, it was recognized that VARs estimated using Bayesian shrinkage techniques produced forecasts superior to those from unrestricted VARs. The "Minnesota prior," a simple vector random walk, soon became dominant.[11] The Bayesian VAR tradition continues to progress, as for example with the work of Sims and Zha (1997), who develop methods applicable to large systems, and Ingram and Whiteman (1994), who shrink in directions suggested by economic theory.

The Future

The future of nonstructural economic forecasting will be more of the same -- steady progress -- fueled by cheap, fast computing, massive storage, and increased sophistication of numerical and simulation techniques. Such techniques are rapidly allowing us to estimate complicated models not amenable to treatment with standard methods, and to dispense with the unrealistic assumptions often invoked in attempts to quantify forecast uncertainty, such as normally distributed shocks and no parameter estimation uncertainty.[12]

Nonlinear forecasting methods have also received increasing attention in recent years, as the Slutsky-Yule theory of linear modeling and forecasting has matured, and that trend will likely continue.[13] I have, however, intentionally avoided discussion of nonlinear methods

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[11] For an extensive discussion, see Doan, Litterman, and Sims (1984).

[12] Efron and Tibshirani (1993) and Gourieroux and Monfort (1996) provide good overviews of recent developments.

[13] Models of volatility dynamics, which permit volatility forecasting, are an important example. The literature began with Engle's (1982) seminal contribution; recent surveys include Bollerslev, Chou and Kroner (1992) and Bollerslev, Engle, and Nelson (1994).

because although many of the nonlinear methods are clearly of value in areas such as finance, they are less useful in macroeconomics, for two reasons. First, many of the nonlinear methods require huge amounts of high-quality data for successful application, whereas in macroeconomics we typically have short samples of data contaminated by substantial measurement error. Second, many of the nonlinearites relevant in fields such as finance simply don't appear to be important in macroeconomics, perhaps because macroeconomic data are highly aggregated over both space and time.[14]

One strand of the nonlinear literature, however, is potentially highly relevant for macroeconomic forecasting -- the idea that business cycle expansions and contractions might be usefully viewed as different regimes, which focuses attention on tracking the cycle, charting the timing of turning points, and constructing business cycle chronologies and associated indexes of leading, lagging and coincident indicators.[15] Burns and Mitchell (1946) is a classic distillation of early work in the nonlinear tradition, much of which was done in the first four decades of the twentieth century, and which was extended in breadth and depth by G. Moore, V. Zarnowitz and their colleagues at the National Bureau of Economic Research in the ensuing decades.

Regime-switching models are the modern embodiment of the Burns-Mitchell nonlinear forecasting tradition. The idea of regime switching is implemented through threshold models,

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[14] Early on, for example, ARCH models were fit to macroeconomic data, such as aggregate inflation, but those ventures were abandoned as it rapidly became clear that the real action was in high-frequency financial data.

[15] See Diebold and Rudebusch (1996, 1998) for extensive discussion of these issues, as well as discussion of the interplay between linear and nonlinear methods in macroeconomic forecasting.

in which an indicator variable determines the current regime (say, expansion or contraction). In the observed indicator models of Tong (1990) and Granger and Teräsvirta (1993), the indicator variable is some aspect of the history of an observable variable.[16] In contrast, Hamilton (1989) argues that models with *unobserved* regime indicators may be more appropriate in many business, economic and financial contexts. In Hamilton's widely-applied model, sometimes called a "Markov-switching" or "hidden-Markov" model, the regime is governed by an unobserved two-state first-order Markov process.[17]

2. The Rise and Fall (and Rise) of Structural Macroeconomic Forecasting[18]

Nonstructural models, linear and nonlinear, are unrestricted reduced-form models. As such they are useful for producing *unconditional* forecasts -- best guesses about the future, not conditioned on assumptions regarding the future stance of policy or other "exogenous" effects. Nonstructural models are widely used in a variety of environments ranging from firm-level business forecasting to economy-wide macroeconomic forecasting; one of their virtues is their wide applicability -- one can use them without assuming much about the inner workings of the system being forecast.

Particularly in macroeconomic environments, however, we often want to analyze various scenarios, such as the effects of a change in a policy rule or a tax rate. Such

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[16] For example, the current regime may be determined by the sign of last period's growth rate; if time t-1 growth was positive then the time-t regime is expansion, and conversely.

[17] See Hamilton (1994), Chapter 22.

[18] The title of this section was inspired by Pagan's (1996) entertaining paper on the evolution of the business cycle.

conditional forecasts require *structural* models. Structural econometrics, and hence structural macroeconomic forecasting, makes use of macroeconomic theory; thus developments in structural forecasting naturally lag developments in theory. There have been two major theoretical advances in twentieth century macroeconomic theory, the Keynesian theory of the 1930s and the dynamic stochastic general equilibrium theory of the 1970s and 1980s. The first was followed by a major advance in structural macroeconomic forecasting, and the second, I argue, will be as well.

The Rise and Fall of Keynesian Macroeconomic Theory and Structural Forecasting

With the publication of Keynes' *General Theory* in 1936, macroeconomic theory distinctly led structural macro-econometrics, which effectively began in the wake of the *General Theory*. Structural econometrics soon caught up, however, with the classical work associated with the "Keynesian revolution" of Klein (1946) and Klein and Goldberger (1955). The period following the publication of the *General Theory* was one of unprecedented and furious intellectual activity directed toward the construction, estimation and analysis of Keynesian structural econometric models. The statistics side of the structural econometrics research was fueled by the pathbreaking advances of Fisher, Neyman, Pearson, and many others earlier in the century. The economics side, of course, was driven by Keynes' landmark contribution, which spoke so eloquently to the severe economic problems of the day.

The intellectual marriage of statistics and economic theory was reflected in the formation and growth of the Econometric Society and its journal, *Econometrica*, and beautifully distilled in the work of the Cowles Commission for Research in Economics, at the

University of Chicago in the 1940s and early 1950s.[19] The intellectual focus and depth of talent assembled there were unprecedented in the history of economics: Cowles researchers included T.W. Anderson, K. Arrow, G. Debreu, T. Haavelmo, L. Hurwicz, L.R. Klein, T. Koopmans, H. Markowitz, J. Marshak, F. Modigliani, H. Simon, A. Wald, and many others. A primary focus of the Cowles researchers was understanding identification and estimation of systems of stochastic difference equations designed to approximate the Keynesian macroeconomic theory. The Cowles approach was dubbed the "system-of-equations" approach by Prescott (1986), in reference to the fact that the it concentrated on the estimation of parameters of equation systems representing decision rules (the "consumption function," the "investment function," *etc.*) as opposed to the more fundamental parameters of tastes and technology.

Just as the marvelous blending of mathematical statistics and economics associated with the Cowles commission was historically unprecedented, so too was the optimism for solving pressing macroeconomic problems. Early on, the Cowles Foundation research program appeared impressively successful, and structural econometric forecasting blossomed in the late 1950s, 1960s, and early 1970s, the heyday of the large-scale macroeconomic forecasting models, which sometimes consisted of systems of thousands of equations. There was strong consensus regarding the validity of the system-of-equations approach, even if there was disagreement on details such as the relative slopes of IS and LM curves, and the models were routinely used for forecasting and policy analysis in both academia and

[19] For a concise history of the Chicago days of the Cowles Commission, see Chapter 1 of Hildreth (1986).

government.

But cracks in the foundation, which began as intellectual dissatisfaction with the underpinnings of Keynesian macroeconomics and system-of-equations econometrics, began to appear in the late 1960s and early 1970s. First, economists became dissatisfied with the lack of foundations for the disequilibrium nature of the Keynesian model. Attempts to understand and explain sticky prices began a new research program in the microfoundations of macroeconomic theory that continues to the present. Many key early contributions appear in the classic Phelps *et al.* (1970) volume, and more recent contributions are collected in Mankiw and Romer (1991).

Second, just as macroeconomists became increasingly disenchanted with the *ad hoc* treatment of sticky prices in traditional models, they became similarly disenchanted with the *ad hoc* treatment of expectations in those models. Building on key early work by Muth (1960, 1961), who introduced the idea of rational expectations and showed that schemes such as adaptive expectations were rational only in unlikely circumstances, the "rational expectations revolution" quickly took hold; Sargent and Wallace (1975) is a key and starkly simple early paper.

Third, and most generally, economists became dissatisfied not only with certain parts of the Keynes-Cowles system-of-equations program, such as the assumptions about price behavior and expectations formation, but rather with the overall modeling approach embodied in the program. The system-of-equations tradition, in particular, is based on the estimation of decision rules rather than the more fundamental economic parameters of tastes and technology. Early work in the tastes-and-technology tradition includes Lucas and Prescott

(1971), and work accelerated rapidly following Lucas' (1976) famous formal critique of the system-of-equations approach to structural econometrics, in which he argues that analysis based on decision rules is a fundamentally defective paradigm for analyzing the effects of alternative economic policies, because the parameters of decision rules will generally change when policies change.

Finally, if the cracks in the foundation of Keynesian structural forecasting began as intellectual dissatisfaction, they were widened by the economic facts of the 1970s, in particular the simultaneous presence of high inflation and unemployment, which did not accord with the predictions of the Keynesian model. Moreover, econometricians began to absorb some of the impressive advances that had been made in the nonstructural tradition and reached the then-startling conclusion that simple Box-Jenkins ARIMA forecasting models often outperformed the large structural models; Nelson (1972) remains a classic. Keynesian macroeconomics soon declined, and Keynesian structural econometric forecasting followed suit.

In the theoretical vacuum of the late 1970s, economic forecasters continued to absorb and contribute to the nonstructural tradition. The memorable title of an important paper by Sargent and Sims (1977), "Business Cycle Modeling Without Pretending to Have too Much *a Priori* Theory," nicely summarizes the econometric spirit of the times. As economists, however, we still have grand econometric aspirations, which is to say structural econometric aspirations, but progress had to await the theoretical advances of the 1980s in dynamic stochastic general equilibrium (DSGE) modeling.

Important intermediate steps, no doubt, were taken by Fair (e.g., 1984, 1994) and

Taylor (e.g., 1993), who built important bridges from traditional system-of-equation structural econometrics to modern DSGE structural econometrics. Their work, in particular, shows increased concern with microeconomic foundations, expectations formation and cross-country interactions, sophisticated use of nonstructural time series methods, and rigorous assessment of model fit and forecasting performance.[20] But the theory on which such structural econometric models are based remains largely in the system-of-equations tradition. DSGE theory, to which we now turn, proceeds very differently.

The Rise of Modern Dynamic Stochastic General Equilibrium Theory

As we have seen, an early wave of structural econometrics followed the development of Keynesian theory in the 1930s and was associated with the Cowles commission and the subsequent large-scale systems of equations that blossomed in the 1940s through the 1960s. But the Keynesian theory was largely based on decision rules, rather than the true economic primitives of taste and technology; the Cowles system-of-equations approach to structural econometric forecasting inherited that defect and hence wasn't really structural. Ultimately the system-of-equations approach to both theory and forecasting declined in the 1970s.

A new wave of powerful theory soon followed. The new theory has its roots in the dissatisfaction with the system-of-equations approach that percolated in the late 1960s and 1970s, and it congealed with the seminal paper of Kydland and Prescott (1982). Early on, the new theory was dubbed "real business cycle theory," because it emphasized the idea that real shocks to technology (that is, supply shocks) in simple representative-agent flexible-price

[20] Models in the Fair-Taylor spirit are now in use at a number of leading policy organizations, including the Federal Reserve Board and the IMF.

competitive equilibrium models could explain a surprisingly large share of business-cycle fluctuations. The essence of the new approach, however, is ultimately not about whether the shocks that drive the cycle are real or monetary, whether prices are flexible or sticky, or whether competition is perfect or imperfect.[21] Rather, the essence of the new approach is methodological and reflects a view of how macroeconomics should be done: it should be based on fully-articulated model economies (preferences, technologies, and rules of the game), and it should be fundamentally dynamic, stochastic, and aggregative. Hence the newer, more descriptively accurate, name: dynamic stochastic general equilibrium modeling.

At its core, the approach boils down to stochastic dynamic programming, a well-known recent economic treatise on which is Stokey, Lucas and Prescott (1989). The key innovation is that DSGE models are built on a foundation of fully-specified stochastic dynamic optimization, as opposed to reduced-form decision rules, and are therefore not subject to the Lucas critique. But ultimately the "new" theory is neither new nor radical; rather, it is very much in the best tradition of neoclassical economics.

<u>The Future: A New Structural Econometric Forecasting?</u>

Nonstructural forecasting has evolved steadily and with little controversy, in contrast to structural forecasting, which aligns itself with economic theories and hence rises and falls with those theories. Waves of theory are followed by waves of structural econometrics. As the DSGE theory reaches maturity, a new wave of structural econometric forecasting -- radically different from the earlier variety -- is emerging. Measurement and theory are

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[21] The more recent literature is filled with numerous variations on the general theme, including models that allow for imperfect competition and sticky prices, and hence real effects of monetary shocks. See Cooley (1994) for a good overview.

beginning to be united in fundamental ways rarely seen before, a phenomenon driven by our deepening understanding of the links between structural economic models and their "reduced forms," or implied systems of decision rules. The result is a marriage of the best of structural economic theorizing and nonstructural economic time series analysis.

Hansen and Sargent (in press), for example, stress the convenience and power of simple "linear-quadratic" economic models, with linear technology and quadratic preferences, for which the implied system of decision rules is the great workhorse nonstructural model -- a vector autoregression, subject to restrictions arising from theory.[22] Linear-quadratic models are surprisingly more flexible than a superficial assessment might indicate; they nest a variety of popular and useful preference and technology structures. Linear-quadratic models are also convenient. A huge literature provides powerful methods for solving and analyzing such models, and as Hansen and Sargent stress, state-space representations in conjunction with the Kalman filter are readily used for maximum-likelihood estimation and for forecasting.

Although linear-quadratic models are surprisingly flexible, their quadratic preferences and linear technologies are nevertheless sometimes restrictive. Thus many theorists, particularly those less inclined toward empirical economics and forecasting, prefer models that are not linear-quadratic. Such models don't have tidy VAR equilibria (linear decision rules), but they have equilibria that can often be accurately *approximated* by VARs. Only time will tell whether full-blown econometric analysis of non-linear-quadratic models, which are challenging to solve and analyze but preferred by theorists, or econometric analysis of

[22] Interestingly, chapter drafts circulated for a decade before the authors finally let go, as the furious pace of advancement necessitated continuous reworking and extending of the manuscript.

linear-quadratic approximations, which are simpler and evidently favored by empirically-oriented economists, represents the best compromise for practical forecasting. Either way, the seeds have been sown for a radically new structural econometrics and structural econometric forecasting.

The new structural econometrics is emerging more slowly than was the case with the earlier wave following Keynes, because the baby was almost thrown out with the 1970s bathwater: the flawed econometrics that Lucas criticized was taken in some circles as an indictment of *all* econometrics. It has taken take us some time to get on with our econometric work, but now progress is evident.

One important tool that has found wide use in the analysis of DSGE models is Calibration.[23] Calibration means many things to many people, but central to calibration is the idea of learning about the properties of a complicated dynamic model by simulating the model economy. The parameters underlying the simulated model economy are typically set informally, sometimes by statistical considerations such as generating realistic amounts of volatility in observed variables, sometimes by economic considerations such as producing "reasonable" steady state behavior, and sometimes by appealing to previous empirical studies. Selected features of the simulated model economy are then compared to the same features of the actual economy in an attempt to assess the adequacy of the fitted model.

Calibration is the natural response of economic theory to the computer age; hence the commonly-used synonym "quantitative economic theory." Calibration, however, fails to provide a complete and probabilistic assessment of agreement between model and data and

[23] For a recent exposition, see Kydland and Prescott (1996).

therefore fails to deliver the goods necessary for estimation and forecasting with DSGE models. Econometric discontent based on recognition of that fact has been simmering for some time and is expressed forcefully and eloquently by Sims (1996) in a recent *Journal of Economic Perspectives* symposium on calibration and econometrics.[24] The growing list of such symposia includes a special issue of *Journal of Applied Econometrics* (see the introduction by Pagan, 1994) and an *Economic Journal* "Controversy" section (see the introduction by Quah, 1995).

If DSGE models are to be used for forecasting, formal econometric analysis is necessary for at least two reasons. First, simply using *a priori* "reasonable" parameter values, although useful as a preliminary exercise to assess agreement between model and data, is not likely to produce accurate forecasts. For example, it might be commonly agreed that a technology shock is likely to be serially correlated, and for purposes of a preliminary calibration exercise we might set the serial correlation coefficient to an arbitrary but "reasonable" value, such as .95. The serial correlation coefficient that produces the closest agreement between model and data, however, might turn out to be .73. Although the choice of .95 *vs.* .73 is likely to make little difference in a qualitative analysis of the model's properties, it can make a big difference for forecasting. Accurate forecasting demands quantitative precision.

Second, forecasting is intimately concerned with the quantification of various uncertainties, including innovation uncertainty, parameter uncertainty, and model uncertainty,

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[24] See also Hansen and Heckman (1996), in the same symposium, the lead paper in which is Kydland and Prescott (1996).

which are jointly responsible for our forecast errors. Accurate assessment of such uncertainty is key, for example, for producing credible and accurate interval forecasts.

The upshot is that for forecasting we need to take seriously the "fit" of DSGE models and search for best-fitting parameters. There are a variety of ways to do so. The theory and practice of generalized method of moments estimation and inference, for example, has advanced rapidly, particularly in conjunction with recent advances in simulation.[25] The mechanics of classical procedures such as maximum likelihood have been greatly advanced by the development of linear and nonlinear filtering theory, and the mechanics of Bayesian procedures have been similarly advanced by Markov chain Monte Carlo techniques.[26] New estimation procedures, which include both calibration and maximum likelihood estimation as special cases and attempt to exploit the middle ground, have also been developed.[27]

Formal econometric analyses of DSGE models are becoming more common, and the outlook is encouraging. Prominent examples, which have taken us closest toward workable DSGE models for forecasting and policy analysis, include:

a. Christiano and Eichenbaum (1992), who provide an early example of formal generalized method of moments estimation of a DSGE model with government spending shocks

b. Hansen and Sargent (1997) and McGrattan, Rogerson, and Wright (1997), who

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[25] See Hamilton (1994), Chapter 14.

[26] See, for example, Harvey (1981, second edition 1993, Chapter 5), Gelman *et al.* (1995), and Geweke (1994).

[27] See Diebold, Ohanian and Berkowitz (1995).

estimate DSGE models, many of which have an explicit role for policy, by maximizing a Gaussian likelihood

c. Leeper and Sims (1994), Leeper, Sims and Zha (1996), and Sims and Zha (1996), who estimate rich classes of DSGE models using a strategy based on interpreting data from the vantage point of the likelihood principle.[28]

Thus the new wave of structural macroeconomic forecasting *has already begun*. Many of the tools of nonstructural econometrics, moreover, figure prominently in the new structural work. The ideas behind ARMA and VAR models, for example, are central to specifying and analyzing both DSGE model inputs and outputs, or in business cycle parlance, the classic duo of impulse and propagation mechanisms. Other nonstructural developments, such as nonlinear models of regime switching, have not yet been thoroughly explored in the context of empirical DSGE models.

One might expect the scale of empirical DSGE models to grow over time. That is likely to happen, and current models that determine, for example, three or four variables in equilibrium, are likely to evolve into richer models that determine, say, ten or twelve variables in equilibrium.[29] But the expansion in scale is likely to stop there, for two reasons. First, the demise of the large-scale models heightened professional awareness of the fact that bigger models are not necessary better, an idea memorably enshrined in Zellner's (1992) KISS

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[28] That is, they examine the entire likelihood function, in contrast to the classical prescription of simply computing its maximum and examining an ϵ-neighborhood of the maximum.

[29] The work of Sims and his coauthors has already reached that point.

principle.[30] Second, in contrast to models in the system-of-equations tradition, which are typically estimated equation-by-equation and then assembled in modular fashion, the nature of DSGE models requires that their parameters be jointly numerically estimated, which limits the complexity of the models that can be entertained.[31] That is a virtue, not a limitation of the DSGE models.

3. Concluding Remarks

As Passell notes, "...Americans held unrealistic expectations for forecasting in the 1960's -- as they did for so many other things in that optimistic age, from space exploration to big government ..." Our expectations for forecasting were quite appropriately revised downward in the 1970s and 1980s, and the ensuing era of humility has been good for all. The new humility, moreover, is not symptomatic of failure, just as the bravado of the 1960s was not symptomatic of success.

As the 1990s draw to a close, we find ourselves at a critical and newly-optimistic juncture, with the futures of nonstructural and structural forecasting very much intertwined. The ongoing development of nonstructural forecasting, together with the recent developments in dynamic stochastic general equilibrium theory and associated structural estimation methods, bode well for the future of macroeconomic forecasting. The hallmark of macroeconomic forecasting over the next twenty years will be marriage of the best of the nonstructural and structural approaches, facilitated by advances in numerical and simulation

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[30] ***K**eep **I**t "**S**ophisticatedly **S**imple."*

[31] Sims and his coauthors, however, are partially able to overcome this "curse of dimensionality" -- some would say at a cost -- by incorporating prior information in a Bayesian framework.

techniques that will help us to approximate, solve, estimate, simulate, and forecast with rich dynamic stochastic general equilibrium (or disequilibrium) models.[32] Development will occur in a variety of fields well beyond macroeconomics, including public economics, industrial organization, labor economics, international economics, and agricultural economics.

It's already happening.

[32] Rust (1996) and Judd (1998), for example, catalog the impressive advances being made for solving stochastic dynamic programming problems.

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