The Past, Present, and Future of Macroeconomic Forecasting

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Abstract: Broadly defined, macroeconomic forecasting is alive and well. Nonstructural
forecasting, which is based largely on reduced-form correlations, has always been well and
continues to improve. Structural forecasting, which aligns itself with economic theory and
hence rises and falls with theory, receded following the decline of Keynesian theory. In
recent years, however, powerful new dynamic stochastic general equilibrium theory has been
developed, and structural macroeconomic forecasting is poised for resurgence.

The reports of the death of large-scale macroeconomic forecasting models are *not* exaggerated. But many observers interpret the failure of the early models as indicative of a bleak future for macroeconomic forecasting more generally. Such is not the case. Although the large-scale macroeconomic forecasting models didn't live up to their original promise, they nevertheless left a useful legacy of lasting contributions from which macroeconomic forecasting will continue to benefit: they spurred the development of powerful identification and estimation theory, computational and simulation techniques, comprehensive machine-readable macroeconomic databases, and much else. Moreover, 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 interplay between measurement and theory, and the corresponding evolution of the nonstructural and structural approaches to forecasting. Nonstructural macroeconomic forecasting methods attempt to exploit the reduced-form correlations in observed macroeconomic time series, with little reliance on economic theory. Structural models, in contrast, view and interpret economic data through the lens of a particular economic theory.

Structural econometric forecasting, because it is based on explicit theory, rises and falls with theory, typically with a lag. Structural Keynesian macroeconomic forecasting, based on postulated systems of decision rules, enjoyed a golden age in the 1950s and 1960s, following the advances in Keynesian theory in the 1930s and 1940s, and the two declined together in the 1970s and 1980s. The evolution of nonstructural economic forecasting, in contrast, is less bound to fashions in economic theory; its origins long predate structural

Keynesian macroeconomic forecasting, and progress continues at a rapid pace.

One is naturally led to a number of important questions. What of the impressive advances in economic theory of the 1980s and 1990s? Should we not expect them to be followed by a new wave of structural macroeconomic forecasting, or has nonstructural forecasting permanently replaced structural forecasting? Related, is it necessary to choose between the structural and nonstructural approaches, or might the two be complements rather than substitutes? If a new structural forecasting *is* likely to emerge, in what ways will it resemble its ancestors? In what ways will it differ? Our answers will take us on a whirlwind tour of the past, present and future of both structural and nonstructural forecasting. We'll begin by tracing the rise and fall of the structural Keynesian system-of-equations paradigm, and then we'll step back to assess the long-running and ongoing progress in the nonstructural tradition. Finally, we'll assess the rise of modern dynamic stochastic general equilibrium macroeconomic theory, its relationship to nonstructural methods, and its implications for a new structural macroeconomic forecasting.

1. The Rise and Fall of Keynesian Macroeconomic Theory and Structural Forecasting

Some important forecasting situations involve *conditional* forecasts; that is, forecasts of one or more variables conditional upon maintained assumptions regarding, for example, the behavior of policy makers. Conditional forecasts require structural models. Structural econometrics, and hence structural macroeconomic forecasting, makes use of macroeconomic theory, which implies that developments in structural forecasting naturally lag developments in theory. The first major wave of twentieth century macroeconomic theory was the Keynesian theory of the 1930s and 1940s, and it was followed by major advances in structural

macroeconomic forecasting.

When Keynes' *General Theory* was published in 1936, theory was distinctly ahead of measurement. Measurement soon caught up, however, in the form of the systems of equations associated with Klein's (1946) *Keynesian Revolution* and Klein and Goldberger's (1955) *Econometric Model of the United States: 1929-1952*. Indeed, 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 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 eloquently to the severe economic problems of the day and seemed to offer a workable solution.

The intellectual marriage of statistics and economic theory was reflected in the 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.[1] 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 central part (although by no means the only part) of the Cowles research program was identification and estimation of systems of stochastic difference equations designed to approximate the posulated decision

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

rules of Keynesian macroeconomic theory.

Just as the 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 macroeconomic system-of-equations research

program appeared impressively successful, and structural econometric forecasting blossomed

in the late 1950s and 1960s, the heyday of the large-scale Keynesian macroeconomic

forecasting models. There was strong consensus regarding the general paradigm, 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

government.

But cracks in the foundation, which began as intellectual dissatisfaction with the

underpinnings of Keynesian macroeconomic systems of equations, 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. A new and still-ongoing research program

began, which sought microfoundations for Keynesian macroeconomic theory, particularly for

the central tenets of sticky wages and prices. 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 ad

hoc treatment of expectations. Building on 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 an important and starkly simple early paper.

Third, and most generally, economists became dissatisfied not only with certain parts of the Keynesian macro-econometric program, such as the assumptions about price behavior and expectations formation, but rather with the overall modeling approach embodied in the program. The 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 ad hoc postulated decision rules ("consumption functions," "investment functions," etc.) as opposed to more fundamental parameters of tastes and technology. Newly-emerging macroeconomic work in the late 1960s and early 1970s, in contrast, was firmly grounded in tastes and technology; Lucas and Prescott (1971) and Lucas (1972) remain classic examples. Work in the tastes-and-technology tradition accelerated rapidly following Lucas' (1976) formal critique of the system-of-equations approach, based on the insight that analysis based on decision rules is a fundamentally defective paradigm for producing conditional forecasts, 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 naturally led economists to question the alleged inflation/unemployment tradeoff embedded in the Keynesian systems of equations. In addition, a series of studies published in the early 1970s

revealed that simple statistical extrapolations, making no assumptions at all about economic structure, often forecasted macroeconomic activity just as well as large-scale Keynesian macroeconomic models; Nelson (1972) remains a classic. Keynesian macroeconomics soon declined, and Keynesian structural econometric forecasting followed suit.

2. Nonstructural Forecasting

By the late 1970s, it was clear that Keynesian structural macroeconomic forecasting, at least as traditionally implemented, was receding. One response was to augment the traditional system-of-equations econometrics in attempts to remedy its defects. Important work along those lines was undertaken by R. Fair and J. Taylor (see, e.g., Fair, 1984, 1994 and Taylor, 1993), who developed methods for incorporating rational-expectations into econometric models, as well as methods for rigorous assessment of model fit and forecasting performance. Models in the Fair-Taylor spirit are now in use at a number of leading policy organizations, including the Federal Reserve Board and the International Monetary Fund, as described for example in Brayton et al. (1997). They are an important step forward, even if the theory on which they are built remains largely in the system-of-equations tradition.

Another response, involving a more radical change of direction, was to explore alternative, nonstructural forecasting methods. Many forecasting chores involve *unconditional*, rather than conditional, forecasts -- that is, interest often centers on the likely future path of the economy when policy remains unchanged, so that the Lucas critique is not relevant -- and unconditional forecasting does not require a structural model. That insight, together with the emerging discontent with Keynesian macroeconomic theory and the lack of a well-developed alternative, produced tremendous interest in nonstructural econometric

forecasting in the 1970s. The 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 spirit of the times.

Interestingly, the impressive intellectual development of nonstructural forecasting spans many decades; it predates the Keynesian episode and continues to the present. Macroeconomists and econometricians didn't pay much attention at first, in spite of the fact that key early contributions were made by economists; they were too busy with Keynesian theory and Keynesian structural econometrics. Nevertheless, rapid development took place in the hands of some of the most talented mathematicians, statisticians and engineers of the twentieth century.

Let us begin our account in the 1920s, which were a period of fertile intellectual development in nonstructural modeling and forecasting. Many ideas were hatched and nurtured, and the groundwork was laid for the impressive technical advances of the ensuing decades. In particular, Slutsky (1927) and Yule (1927) argued that simple linear difference equations, driven by purely random stochastic shocks, provide a convenient and powerful framework for modeling and forecasting a variety of economic and financial time series. Such stochastic difference equations are called autoregressive processes, or autoregressions. They amount to regression models in which the current value of a variable is expressed as a weighted average of its own past values, plus a random shock. Autoregressive processes are closely related to moving average processes, also studied by Slutsky and Yule, in which a the current value of a variable is expressed as a weighted average of current and lagged random shocks alone. In fact, under reasonable conditions, one can convert an autoregressive process

to a moving average process, and conversely. Either way, the key insight is that system dynamics can convert random inputs into serially correlated outputs, a phenomenon often called the Slutsky-Yule effect. Frisch (1933) put the Slutsky-Yule insight to work in formulating the idea of "impulse" and "propagation" mechanisms in economic dynamics.

In the 1930s, the mathematician-turned-economist H. Wold made a stunning contribution, paving the way for later work by the mathematicians N. Wiener 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. Thus, the Slutsky-Yule models are not only convenient and powerful, they are absolutely central -- they're the only game in town. Wiener and Kolmogorov worked out the mathematical formulae for optimal forecasts from models of the type studied by Slutsky, Yule, and Wold. Kalman extended the theory in the late 1950s and early 1960s by relaxing some of the conditions that Wiener and Kolmogorov required; his forecasting formulae, known as the Kalman filter, are designed to work with a powerful model representation known as state-space form and have a convenient recursive form amenable to real-time forecasting.[2] The Wold-Wiener-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.

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[2] See Harvey (1989) for extensive discussion of state space representations and the Kalman filter in relation to forecasting.

In part, the nonstructural econometric forecasting explosion of the 1970s was driven by econometricians absorbing the powerful earlier advances made by the likes of Wold, Wiener, Kolmogorov and Kalman. But there was a major additional push: in 1970, just as discontent with Keynesian structural econometric forecasting was beginning to emerge, Box and Jenkins (1970; third edition Box, Jenkins and Reinsel, 1994) published a landmark book on nonstructural time series analysis and forecasting.

Many of the Box-Jenkins insights started literatures that grew explosively. For example, prior to Box and Jenkins, trend was typically modeled as a simple linear deterministic function of time, whereas Box and Jenkins allowed trend to be driven by the cumulative effects of random shocks, resulting in "stochastic trend."[3] Stock and Watson (1988a) provide an insightful discussion of stochastic trend and its wide-ranging implications. Shocks to series with stochastic trend have permanent effects, an idea amplified in the empirical macroeconomics literature associated with Nelson and Plosser (1982) and Campbell and Mankiw (1987), among others. The direct implication for forecasting is that long-run forecasts fail to revert to any fixed trend; effectively, the underlying trend location is redefined each period, as emphasized for example in Diebold and Senhadji (1996).

The most important contribution of Box and Jenkins, however, is their sweeping

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[3] Processes with stochastic trend are also called integrated processes, or unit-root processes. The pioneering work of Dickey and Fuller (e.g., Fuller, 1976) on unit root testing grew from a desire, motivated by Box and Jenkins, to determine whether various series displayed stochastic trend. The similarly pioneering work of Granger and Joyeux (1980) on "long memory," or "fractionally-integrated," processes grew from attempts to generalize the idea of integration on which Box and Jenkins relied so heavily; see Diebold and Rudebusch (1989) for a macroeconomic application of long memory models and Baillie (1996) for an insightful recent survey.

vision, articulation, and illustration of an operational framework for applied nonstructural

forecasting, consisting of iterative cycles of model formulation, estimation, diagnostic

checking, and forecasting. Autoregressive moving average (ARMA) models are the

centerpiece of the Box-Jenkins framework. ARMA models are simple combinations of the

autoregressive and moving average models of Slutsky and Yule, and they have the potential

to approximate dynamics more parsimoniously than purely autoregressive or moving average

models.

An ongoing flood of work followed Box and Jenkins. Macroeconomics, in particular,

is crucially concerned with cross-variable relationships, whereas the basic approach of Box

and Jenkins uses only the past of a given economic variable to forecast its future. In other

words, much of macroeconomics is concerned with multivariate relationships, whereas the

basic Box-Jenkins models are univariate. Thus, many extensions of the Box-Jenkins program

involve multivariate modeling and forecasting, and vector autoregressive models have

emerged as the central multivariate model. Vector autoregressions were forcefully advocated

in econometrics by Sims (1980) as a less restrictive alternative to traditional econometric

system-of-equations models, in which variables were arbitrarily labeled "endogenous" or

"exogenous."[4] In vector autoregressions, in contrast, *all* variables are endogenous.

The mechanics of vector autoregressions are simple. Recall that we approximate

dynamics with a univariate autoregression by regressing a variable on its own past values. In

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[4] Ben McCallum notes in private communication that in an important sense, the situation was even worse: the endogenous-exogenous labeling was arguably not arbitrary, but rather *systematic*, with policy variables labeled as "exogenous" on the grounds that they *could have been* managed exogenously by policymakers if they had been unorthodox enough to do so.

a vector autoregression, by way of logical extension, we regress each of a set of variables on past values of itself and past values of every other variable in the system. Cross-variable linkages are automatically incorporated because we include lags of all variables in each equation, and because we allow for correlations among the disturbances of the various equations. It turns out that one-equation-at-a-time least squares estimation of vector autoregressions is statistically efficient in spite of the potential correlation of disturbances. Moreover, it is simple and numerically stable, in contrast to the tedious numerical optimization required for estimation of multivariate ARMA models.

Many multivariate extensions of the Box-Jenkins paradigm are conveniently implemented in the vector autoregressive framework. Here we introduce a few; most will feature in the sequel, and all will help convey a feel for the breadth of modern time-series econometrics and forecasting. The discussion is necessarily brief; for a more detailed introduction modern time series forecasting, see Diebold (1998).

Granger (1969) and Sims (1972) made important early multivariate contributions, providing tools for exploring causal patterns in multivariate systems. The Granger-Sims causality notion is predictive; 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. We commonly use Granger-Sims causality tests to help identify and understand the patterns of cross-linkages and feedback in vector autoregressions.

The dynamic factor model of Sargent and Sims (1977) and Geweke (1977) is another important early multivariate contribution. The essential idea of dynamic factor models is that some economic shocks are common across sectors and others are idiosyncratic, so that large

sets of variables may depend heavily on just a few common underlying sources of variation, a common feature of economic models and evidently also of economic data. The common shocks, or "factors," produce comovements and facilitate parsimonious modeling and forecasting of large numbers of variables. Dynamic factor models have proved particularly useful with the emergence of macroeconomic panel datasets, including cross-country, cross-region, and cross-state data. Important recent contributions include Stock and Watson (1989), Quah and Sargent (1993), Forni and Reichlin (1997), and Stock and Watson (1997).

Granger (1981) and Engle and Granger (1987) develop the related idea of cointegration. We say that two series are cointegrated if each contains a stochastic trend, yet there exists a linear combination that does not. Thus, for example, each of two series x and y may contain stochastic trend, but the spread x-y may not. It is apparent that in such situations stochastic trends are shared, which makes the series move together. This is the essence of the Stock-Watson (1988b) "common trends" representation of cointegrated systems and is precisely the same idea as with the intimately-related dynamic factor model: comovements may be due to dependence on common factors. Cointegration is also intimately connected to the idea of error-correction, pioneered by Sargan (1964) and long a cornerstone of "LSE econometrics," in which the current deviation of a system from equilibrium conveys information regarding its likely future course and is therefore useful for forecasting.[5] Indeed, there is a formal equivalence between cointegration and error correction, as established by Engle and Granger (1987).

All of the discussion thus far has been based on *linear* models. Nonlinear forecasting

[5] For a good exposition of econometrics in the LSE tradition, see Hendry (1995).

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. Models of volatility dynamics, which permit volatility forecasting, are an important example; the literature began with Engle's (1982) seminal contribution, and recent surveys include Bollerslev, Chou and Kroner (1992) and Bollerslev, Engle, and Nelson (1994). We will, however, avoid discussion of nonlinear methods for the most part, because although they are clearly of value in areas such as finance, they are less useful in macroeconomics. There are two reasons. First, many of the nonlinear methods require large 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 nonlinearities 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. Early on, for example, ARCH models were fit to macroeconomic data, such as aggregate inflation, but those ventures were quickly abandoned as it became clear that volatility dynamics were much more important in high-frequency financial data.

One strand of nonlinear literature, however, is potentially 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 (Diebold and Rudebusch, 1996, 1998). 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.[6]

Regime-switching models are a modern embodiment of certain aspects of the Burns-

Mitchell nonlinear forecasting tradition. The idea of regime switching is implemented

through threshold models, 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. For example, the current regime may be determined by the sign of last period's

growth rate. 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 indicator.

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. Efron

and Tibshirani (1993) and Gourieroux and Monfort (1996), for example, provide good

examples of recent developments. The future of nonstructural macroeconomic forecasting

will likely also involve combining aspects of the linear and nonlinear traditions, as for

example with vector autoregressive models that allow for factor structure and regime

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[6] For overviews, see for example Moore (1983) and Zarnowitz (1992).

switching (Diebold and Rudebusch, 1996; Kim and Nelson, 1998a, 1998b).

3. A New Wave of Macroeconomic Theory -- and Structural Forecasting

Nonstructural models are unrestricted reduced-form models. As such they are useful for producing unconditional forecasts in a variety of environments ranging from firm-level business forecasting to economy-wide macroeconomic forecasting. Again, however, in macroeconomics we often want to analyze scenarios that differ from the conditions presently prevailing, such as the effects of a change in a policy rule or a tax rate. Such conditional forecasts require structural models.

As we have seen, an early wave of structural econometrics followed the development of Keynesian theory. But the Keynesian theory was largely based on postulated decision rules, rather than the economic primitives of taste and technology; the 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.   Progress toward a new and truly structural macroeconomic forecasting had to await a new wave of powerful theory developed in the 1970s and 1980s. The new theory has its roots in the dissatisfaction, percolating in the late 1960s and early 1970s, with the system-of-equations approach. In many respects, the essence of the new approach is methodological and reflects a view of how macroeconomics should be done. Lucas (1972), in particular, paved the way for a new macroeconomics based on dynamic stochastic model economies with fully-articulated preferences, technologies, and rules of the game. Hence the descriptively accurate name: dynamic stochastic general equilibrium (DSGE) modeling. 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.

The new research program has sought from the outset to make clear that DSGE models can address practical, empirical questions. Early on, for example, Kydland and Prescott (1982) used DSGE models to argue that a neoclassical model driven purely by real technology shocks could explain a large fraction of U.S. business cycle fluctuations. Hence the early name "real business cycle" models. Later work, however, broadened the approach to allow for rich demographic structures, imperfect competition and sticky prices (and hence real effects of monetary shocks), and much else; the papers collected in Cooley (1995) offer a good overview. Ultimately, again, the essence of the new approach is 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, but rather about the way macroeconomic questions should be approached.

The earliest and still rapidly developing strand of the DSGE literature makes use of simple "linear-quadratic" models, in which agents with quadratic preferences make optimizing decisions in environments with linear production technologies. 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, because a large literature provides powerful methods for solving, analyzing and forecasting with them. Moreover, it turns out that optimizing behavior within linear-quadratic economic models implies decision rules, such as those that govern

consumption or investment behavior, that are stochastic *linear* functions of other variables. In particular, the decision rules conform to the great workhorse nonstructural model, the vector autoregression, subject to restrictions arising from theory. The result is a marvelous union of modern macroeconomic theory and nonstructural time-series econometrics, paving then way for a new structural econometrics

Maximum likelihood methods are central to linear-quadratic DSGE modeling and trace to the important early work of Hansen and Sargent (1980); the modern approach is to construct and maximize the likelihood function using a state space representation in conjunction with the Kalman filter. Initially, maximum likelihood estimation was challenging in all but the simplest cases, but recent improvements in numerical algorithms and computing power have begun to make estimation and forecasting with linear-quadratic DSGE models workable for routine analysis and forecasting. Hansen and Sargent (1998) provide a powerful overview, synthesis, and extension of linear-quadratic DSGE modeling; 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.

Kydland and Prescott (1982) started a distinct, but intimately-related and equally important, strand of the DSGE literature. Two key features differentiate their product. First, Kydland and Prescott do not require that preferences be quadratic and technology be linear; instead, they use non-linear-quadratic models that are (arguably) more natural. Non-linear-quadratic models are challenging to solve, and the Kydland-Prescott program spurred a great

deal of important research on numerical and computational aspects of model solution.[7] One

interesting outcome of that research is that although non-linear-quadratic models don't have

tidy vector-autoregressive systems of decision rules, they nevertheless often have decision

rules that can be accurately *approximated* by vector autoregressions.

Second, Kydland and Prescott acknowledge from the outset that their models, like all

models, are false, and they recognize that traditional econometric estimation procedures such

as Gaussian maximum likelihood may loose some of their appeal in such situations.[8] Partly

for that reason, and partly because of the sheer difficulty, non-linear-quadratic DSGE

modelers often eschew formal estimation in favor of less structured "calibration" methods, as

described in Kydland and Prescott (1996).[9] Calibration means different things to different

people, but the central idea is learning about the properties of a complicated DSGE model,

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[7] See, for example, Rust (1996) and Judd (1998), who describe and contribute to the impressive advances being made for solving non-linear-quadratic stochastic dynamic programming problems.

[8] The reasoning is simple. Loosely speaking, under correct specification, Gaussian maximum likelihood estimates converge to the true parameter values as the sample size grows; hence the estimated model converges to the true model, which is the best model to use for any purpose. Under misspecification, however, the parameters can't converge to the "true" values, because an incorrect model has been fitted. Instead, the parameters converge to values that make the fitted model the best *approximation* to the data, where the measure of goodness of approximation is induced by the estimation procedure. The key insight is that, under misspecification, the best approximation for one purpose may differ from the best approximation for another purpose. The measure of goodness of approximation associated with Gaussian maximum likelihood estimation is 1-step-ahead mean squared forecast error. Thus, if the model is to be used for 1-step-ahead forecasting, and if mean squared error is the relevant loss function, Gaussian maximum likelihood estimation is a logical choice. If, on the other hand, the model is to be used for another purpose, such as 4-step-ahead forecasting, Gaussian maximum likelihood estimation is less appealing.

[9] Important exceptions exist, however, such as McGrattan, Rogerson, and Wright (1997), who estimate non-linear-quadratic DSGE models by maximum likelihood methods.

and attempting to assess its agreement with the data, based on simulations of 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.

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

therefore fails to deliver the goods necessary for forecasting with DSGE models.

Econometric discontent based on recognition of that fact has been simmering for some time

and is expressed forcefully by Sims (1996) in the Winter 1996 symposium in this journal on

calibration and econometrics.[10] 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

desirable for at least two reasons. First, forecasting is intimately concerned with the

quantification of the uncertainties that produce forecast errors. Accurate assessment of such

uncertainties is key, for example, for producing credible forecast confidence intervals.

Calibration methods, unlike probabilistic econometric methods, are ill-suited to the task.

Second, simply using *a priori* "reasonable" parameter values, although useful as a

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

preliminary exercise to gauge 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 adopt a simple first-order autoregressive scheme and set the serial correlation

coefficient to an arbitrary but "reasonable" value, such as .95. But the first-order

autoregressive process might be an oversimplification of reality, and even if adequate, the

serial correlation coefficient that maximizes forecast accuracy might be, say, .73, not .95.

Although such details might make little difference to a qualitative analysis of the model's

properties, they are likely to make a major difference for forecast accuracy. In short, *accurate*

forecasting demands quantitative precision.

The upshot is that for forecasting we need to take seriously the "fit" of DSGE models

and search for best-fitting parameters. Moreover, we need estimation methods that are

tractable yet capable of delivering probabilistic inference, and we need to take

misspecification seriously. Calibration and maximum likelihood estimation meet some, but

not all, of those goals. Calibration is tractable and takes misspecification seriously, but it is

not probabilistic. Maximum likelihood is probabilistic, but it is often challenging to

implement and may not take misspecification seriously enough.[11]

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[11] Nevertheless, if calibration and Gaussian maximum likelihood estimation were the *only* strategies available for parameterizing a serious DSGE forecasting model, the choice would probably not be difficult: maximum likelihood estimation appears preferable, because (a) it enables probabilistic inference, (b) recent improvements in computing and algorithms are making implementation less tedious, especially in the linear-quadratic case, and (c) although the measure of goodness of approximation associated with Gaussian maximum likelihood estimation is 1-step-ahead mean squared forecast error, which may not be appropriate in all situations such as when interest centers on longer-horizon forecasts, short-horizon forecasts often *are* of interest.

The choice set, however, now includes a number of procedures other than calibration and maximum likelihood; in particular, new estimation procedures are being developed that attempt to find a middle ground. The basic idea is to explore goodness-of-fit measures other than the 1-step-ahead mean squared prediction error measure implicit in Gaussian maximum likelihood; there are a variety of ways to proceed. Sims and his co-authors, including Leeper and Sims (1994), Leeper, Sims and Zha (1996), and Sims and Zha (1996), use a strategy based on examining the entire likelihood function, rather than just its maximum. Christiano and Eichenbaum (1992) match selected moments of real macroeconomic data and data simulated from a DSGE model. In similar fashion, Canova, Finn and Pagan (1994) match vector autoregressions, Rotemberg and Woodford (1997) match impulse-response functions, and Diebold, Ohanian and Berkowitz (1997) match spectra. Finally, Rotemberg and Woodford (1996) and Diebold and Kilian (1997) develop procedures that enable us to assess agreement between model and data predictability at various horizons of interest.

If structural modeling and forecasting have come a long way, they still have a long way to go; in closing this section, it is tempting to comment on a few aspects of their likely future development. DSGE theory will continue to improve and will begin to take certain aspects of reality, such as heterogeneity, more seriously. The stochastic dynamics of driving variables, such as technology shocks, will be similarly enriched to reflect recent developments in nonstructural modeling, such as the possibility of regime switching, and to allow for multiple sources of uncertainty, including measurement error. The resulting models will have approximate representations as VARs with factor structure, possibly involving cointegration, as in King, Plosser, Stock and Watson (1991), and possibly with regime switching, as in

Diebold and Rudebusch (1996) and Kim and Nelson (1998a, 1998b). Formal econometric procedures will be used to diagnose possible model inadequacies, as in Chow and Kwan (1997).

One might expect the scale of DSGE forecasting 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, eight or ten variables in equilibrium.[12] 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 principle.[13] 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 estimated, which limits the complexity of the models that can be entertained.

Last and not least, *shrinkage* will likely emerge as a key component of estimation techniques for DSGE forecasting models. Shrinkage refers to the idea of coaxing, or "shrinking," parameter estimates in certain directions. Shrinkage can be implemented using Bayesian methods to coax parameter estimates away from the likelihood maximum and toward the prior mean. It seems obvious that shrinkage in a "correct" direction will likely improve forecast performance. Less obvious, but equally true, is the insight that even shrinkage in "incorrect" directions can improve forecast performance, by drastically reducing

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

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

forecast error variance at the potentially low price of a small increase in bias.

Shrinkage has a long history of productive use in nonstructural modeling and forecasting. For example, it has long been known that vector autoregressions estimated using Bayesian shrinkage techniques produce forecasts drastically superior to those from unrestricted vector autoregressions. The "Minnesota prior," a simple vector random walk, remains widely used.[14] Shrinkage has an equally bright future in the new structural modeling and forecasting. Shrinkage is a potentially tailor-made device for estimating potentially misspecified DSGE forecasting models, because, as we have seen, DSGE theory essentially amounts to restrictions on vector autoregressions. At one extreme, we can ignore the theory and forecast with an estimated unrestricted vector autoregression (no shrinkage, loosely corresponding to a Bayesian analysis with a diffuse prior). At the other extreme, we can directly impose the theory and forecast with a restricted vector autoregression (complete shrinkage, loosely corresponding to a Bayesian analysis with a "spiked" prior). Intermediate cases, corresponding to forecasting with vector autoregressions estimated with various informative, but not spiked, priors are potentially more interesting. First, we may use statistically-oriented priors, such as the familiar Minnesota prior, which shrinks toward a vector random walk. Second, we may use statistically-oriented, but theory-inspired, priors, such as one corresponding to factor structure. Third, we may use DSGE theory-based priors, as in Ingram and Whiteman (1994), in order to coax the estimates in directions implied by an explicit economic theory, without *forcing* the theory on the data.

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[14] For an extensive discussion, see Doan, Litterman, and Sims (1984). The Bayesian vector autoregressive tradition continues to progress, as for example with the work of Sims and Zha (1997), who develop methods applicable to large systems.

4. Concluding Remarks

In a recent *New York Times* article entitled "The Model Was Too Rough: Why Economic Forecasting Became a Sideshow," economics writer Peter Passell noted that "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, however, 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 structural and nonstructural 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. Only time will tell whether linear-quadratic or non-linear-quadratic approximations to the macroeconomy are the best approach for practical macroeconomic forecasting, but regardless, the seeds have been sown for a new structural econometrics and structural econometric forecasting, a modern and thorough implementation of the Cowles vision. The new structural econometrics is emerging more slowly than did 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 some time to get on with macroeconometric work, but progress is evident.

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 techniques that will help macroeconomists to solve, estimate, simulate, and yes, *forecast* with rich models. Moreover, related developments will occur in a variety of fields well beyond macroeconomics. It's already happening, and in some cases progress has been underway for years, as evidenced for example by the recent literatures in industrial organization (e.g., Ericson and Pakes, 1995), labor economics (e.g., Eckstein and Wolpin, 1989; Stock and Wise, 1990; Rust, 1994), public economics (e.g., Rios-Rull, 1995), agricultural economics (e.g., Rosen, Murphy and Scheinkman, 1994), health economics (e.g., Gilleskie, 1997), development economics (e.g., Rosenzweig and Wolpin, 1993), environmental economics (e.g., Rothwell and Rust, 1995), and international economics (e.g., Backus, Kehoe and Kydland, 1994).

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