DYNAMIC MACROECONOMIC ANALYSIS

Dynamic stochastic general equilibrium (DSGE) models have begun to dominate the field of macroeconomic theory and policy-making. These models describe the evolution of macroeconomic activity as a recursive sequence of outcomes based upon the optimal decision rules of rational households, firms and policy-makers. While posing a micro-founded dynamic optimisation problem for agents under uncertainty, such models have been shown to be both analytically tractable and sufficiently rich for meaningful policy analysis in a wide class of macroeconomic problems, for example, monetary and fiscal policy, economic cycles and growth and capital flows. This volume collects specially commissioned papers from leading researchers, which pull together some of the key recent results in diverse areas. This book promotes research using optimising models and will inform researchers, postgraduate students and economists in policy-oriented organisations of some of the key findings and policy implications.

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DYNAMIC MACROECONOMIC ANALYSIS

Theory and Policy in General Equilibrium

EDITED BY

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AND

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Foreword

William A. Brock

In *Dynamic Macroeconomic Analysis: Theory and Policy in General Equilibrium*, Altug, Chadha and Nolan, hereafter 'ACN', have undertaken the extremely difficult task of bringing the reader up to date on the vast literature that has developed in this key area of economics since such seminal books as Cooley's *Frontiers Of Business Cycle Research* (1995).

ACN tackle this formidable assignment by recruiting top scholars to write individual chapters. Each individual chapter is of exceptionally high quality because each scholar is a first-rate expert in the chapter's area.

The approach of the book is 'quantitative theorising', in the sense that each chapter presents not only recent developments in theory but also recent developments in compilation of the facts that confront the theory. Discrepancies between facts and theories are carefully discussed. Many of the chapters offer modifications to the baseline theory that make it do a better job of matching the facts.

I give the reader a quick overview of the contents of this book in a brief introduction to each of the chapters. Three chapters are discussed in more detail than the others in order to keep my foreword within standard space limits. But all of the chapters are equally exciting and have equal command on the reader's attention. I hope this somewhat unusual approach to writing a foreword will entice more readers to add this important book to their libraries. In addition to saying a few words about the chapters, I shall use the discussion of some of them to give some reflections about the field they cover as well as give some speculations and opinions about potentially fruitful future research.

Jim Pemberton (chapter 1) reviews dynamic life cycle consumption/savings models, the facts they are designed to explain, the struggles in attempting to modify the basic core model to fit these facts and other uses to which they are put. Particularly interesting is discussion of how minor modifications can lead to big and rather counterfactual predictions by some of the models. For example, adding a small safety net in one model caused the model's consumer to borrow too large an amount of money early on to be consistent with fact.

Fanny and Michel Demers and Sumru Altug (hereafter, DDA; chapter 2) review investment theory, adjustment cost models, the Q-theory of investment, new developments on the effects of irreversibility and uncertainty upon investment, recent work in the impact of temporary and permanent changes in taxes and other policy instruments on investments and weave a connecting fabric between the facts one needs to explain and the theory available to do it. Not only that, the chapter gives the reader a guided tour through the impacts of increases in uncertainty through various channels upon investment with an especially thorough treatment of the interaction between increases in uncertainty and irreversibilities upon today's investment rate. It also treats simultaneous learning while investing and the influence of this channel upon the investment rate.

Let me offer some speculations about potentially fruitful future directions for investment theory research that are stimulated by the work reviewed in this chapter.

First, there is a substantial discussion in the chapter on the impact that learning has upon investment. Learning in DDA is Bayesian. The impact on investment can be dramatic. For example DDA show that 'Adopting a uniform prior . . . the impact of uncertainty about the permanent component of the price of capital is to induce a dramatic decline in investment expenditures, and a much greater frequency with which firms prefer to delay current investment . . . Even with an informative prior, the decline in total investment is still very substantial . . . the results . . . suggest that policies that are aimed at reducing uncertainty about the unknown costs of investing will have a greater impact in increasing investment than policies that are aimed at reducing the cycical variation in the price of capital or other components of the costs of investing.' I quote DDA at length here because replacing 'uncertainty' in their treatment by 'ambiguity' and extending recent work on optimization under 'ambiguity' (e.g. Epstein and Wang 1994; Hansen and Sargent 2001) may strengthen this effect found by DDA. Furthermore 'ambiguity' seems particularly appropriate to their treatment of 'political risk' since, here, it seems particularly difficult to attach probabilities in a Bayesian manner. Indeed, one might even view the elimination of 'political risk' in monetary policy as a major impetus behind the recent spurt of research on rule-based approaches to monetary policy (e.g. discussion of Taylor rules, in Taylor and Woodford 1999). After all a transparent rule-based approach to monetary policy should allow economic agents to do a much better job of attaching precise probabilities to how the monetary authorities will act conditional on each state of the economy. However, 'political risk' seems to lie more in the domain of imprecise probabilities which are modelled by sets of probability measures with updating modelled by sets of likelihoods (e.g. Epstein and Schneider 2002 and references to workers in the area of imprecise probabilities such as Peter Whalley). Imprecise probabilities are one way of modelling 'Knightian Uncertainty' in the original spirit of Frank Knight. I expand on the theme of modelling 'ambiguity' via tools from the area of imprecise probabilities below. The end result of extending DDA's work in this direction is likely to be a general strengthening of their findings on the dampening effects upon investment induced by uncertainty increases including increases in 'Knightian Uncertainty.'

The recent burst of interest in robust control (e.g. Hansen and Sargent 2001) suggests an extension of received investment theory to a setting where the information that firms must act upon is not precise enough to assign probabilities (Epstein 1999). This theory is motivated by phenomena such as 'ambiguity' avoidance (e.g. Epstein and Wang 1994, and their discussion of Ellsberg's covered urn). If investment theory were generalised to include investing under ambiguity, one might be able to locate a set of sufficient conditions for an increase in ambiguity to depress investment and/or employment. If so, this would lead to an interesting empirical problem: How does one use data to distinguish between 'ambiguity' aversion, risk aversion and aversion to irreversible investments? While Epstein (1999) provides some results on using data to investigate a market counterpart to the Ellsberg paradox, it will be more difficult to design a test that separates increases in ambiguity from increases in risk or, perhaps, increases in amount of irreversibility.

An adaptation of Epstein and Schneider (2002) seems promising. Consider, for example, their differentiation between responses to signals of a firm making investment decisions who models the future using a Bayesian model with known precision in contrast to a firm who models the future using an ambiguity model. The ambiguity modeller takes bad news especially seriously, so one would conjecture that such a firm would be slow to make an investment commitment in contrast to the Bayesian. In the case of asset markets studied by Epstein and Schneider (2002), they show that patterns of 'over reaction' to seemingly 'small' news about fundamentals that are implausible in a Bayesian context emerge easily in an ambiguous context. It seems promising to investigate whether ambiguity models might help explain 'anomalies' in investment theory. This should be a fruitful area of future research.

This brings us to the issue of modelling the act of learning. Much robust control literature uses a stationary infinite horizon setup with a 'shroud' (i.e. a neighborhood of misspecifications around a baseline model) of constant size which one wishes to 'robustify' against. One looks at the maximin and solves for it using methods from zero-sum two-player infinite horizon games with the planner maximising against a malevolent player who minimises against her (e.g. Hansen and Sargent 2001). The malevolent player is constrained in his action set. This setup would probably need modification to include non-stationary settings before application to investment theory problems, many of which take place in a non-stationary setting where 'learning' takes the form of chasing a moving structure.

Much ambiguity modelling uses a family of priors which could be updated into a family of posteriors, unlike usual Bayesian learning where one prior is updated into one posterior. Under stationary conditions, one can locate sufficient conditions for convergence (i.e. 'merging') of beliefs that are quite general. The same conditions could be applied to a family of priors (Epstein and Schneider 2002). So this suggests that continued injection of 'new' risk and/or new 'ambiguity' is needed to keep a neighbourhood of constant size logically plausible in a robust control setting applied to economics. Epstein and Schneider (2002) inject new ambiguity with a family of likelihoods against each prior in a family of priors and give a nice motivation for this way of doing it. They also discuss generalizations of Laws of Large Numbers to this setting which should be relevant for study of which of these micro-level phenomena survive aggregation (a type of cross-sectional Law of Large Numbers) to matter at the macro level. I have discussed this type of modelling in detail because it seems relevant for extension of investment theory to economic settings where the economic environment is constantly changing and changing in ways where assignment of probabilities as a Bayesian would do does not seem natural and where full learning and merging of opinions does not seem natural either.

For example one might believe that during 'normal times' when markets are operating well, economic agents would have less trouble assigning precise probabilities than in 'abnormal times'. If I am allowed to speculate, 'normal times' could be modelled by separating shocks to the system into a category of small and frequent shocks and 'abnormal times' modelled by a category of rare but large shocks. The level of imprecision of probabilities could be modelled by the size of the support over which the minimizing measure is taken in the Epstein and Wang (1994) framework.

Stephen J. Turnovsky (chapter 3) shows how to compute equilibria in continuous time recursive intertemporal general equilibrium stochastic macroeconomic models, especially those with externalities common in new growth theory. The chapter reviews applications of continuous time stochastic methods, then sets out a basic model which extends AK growth models, investigates fiscal policy effects on the equilibrium growth rate and studies optimal fiscal policy. The chapter makes extensions of the basic model by adding money, adjustment costs, elastic labour supply, productive government spending, puts forth a theory of the optimal size of government in this context and studies risk and international tax policy. This chapter also extends the model to recursive preferences and shows how this makes a difference.

Matt Canzoneri, Bob Cumby and Behzad Diba (chapter 4) review a subset of papers in the 'New Neo-classical Synthesis' (NNS) literature that is particularly relevant to a recent debate on the question 'Is price stability a good strategy for macroeconomic stabilisation?' Much of the current debate over stabilisation policy is concerned about asymmetries in what might be called 'welfare-damaging inertias' and what monetary policy might do to counteract these welfare-damaging inertias. Examples include inertias in price and wage setting, inertias in response abilities of individual economic sectors to outside shocks and the like. Inertias in response to outside shocks might be due, for example, to endogenous constraints on borrowing (due to incentive compatibility constraints, perhaps). Much of this chapter focuses on wedges due to monopolistically competitive price and wage setting with nominal inertia.

Jagjit Chadha and Charles Nolan (chapter 5) review the large literature on joint interaction of monetary and fiscal policy including such controversial subjects as the Fiscal Theory of the Price Level (FTPL). It gives a precise formulation of FTPL and then uses this precise formulation to discuss the controversies unleashed by this approach. Not only that, it reviews recent work on the design of jointly optimal 'simple rules' (e.g. Taylor-type rules) when monetary and fiscal policy are jointly determined.

It is interesting to contrast this chapter with much of the recent literature on 'Taylor' rules. Much if not most of the recent literature on Taylor-type rules abstracts away from fiscal policy and concentrates on the 'tradeoff' between inflation and unemployment. It emphasizes the 'Taylor Principle' that rules with coefficients on inflation bigger than unity with relatively smaller coefficients on unemployment have good stabilisation properties. The usual abstraction away from fiscal policy in the 'rules literature' may arise from the specialised concerns of Central Banks with managing price stability. Chadha and Nolan discuss joint design of simple rules. I will take the liberty here of using the discussion of jointly optimal rules in this chapter to make some speculative remarks about potentially fruitful future research in this area.

Since monetary policy is typically managed by a professionalized Central Bank staff which is highly trained in thinking about tradeoffs in the overall public interest, this management group contrasts with the 'management group' that manages fiscal policy. Fiscal policy is typically determined by a political–authoritative allocation of value process whose properties not only draw the negative attention of scholarly commentators, but also the derision of late-night show comedians. The descriptions of the efficiency and predictability of this process are not always favourable. I am leading up to the following issue. As I said above, there has been much recent activity in applying methods from robust control and ambiguity modelling to economics. This is especially so in recent works in the 'Taylor rules' literature (e.g. Onatski and Stock 2000; Onatski and Williams 2002; Orphanides and Williams 2002). The main posture of these papers is that there is a neighbourhood N of possible departure models from a 'baseline' model that one wishes to robustify against. So one first allows 'Nature' to minimise the policy-maker's objective over N, then the policy-maker maximises over Nature's worst within N. This is a 'local' approach in the sense that there is one basic baseline model with a neighbourhood N (which could be quite large) around it.

A main difficulty with the existing treatments of robustification is modelling the formation of N itself. The managers of fiscal policy would seem to be a good source of 'ambiguity production' in the sense that one needs continuous injections of 'new ambiguities' to prevent plausible learning mechanisms from collapsing the 'width' of the ambiguity, i.e. collapsing the size of N down towards zero. (See Epstein and Schneider 2002, where they model 'ambiguity production' as a family of likelihoods at each date t, conditional on data received at date t–1. They argue that this mechanism can prevent the usual 'merging-of-opinions' results from collapsing the 'width' of the ambiguity as time passes on.) In the USA the mere change of one Senator from the Republican Party to an Independent dramatically changed the direction and momentum of policy – i.e. fiscal policy dynamics would appear to be a good place for the devotees of imprecise probabilities to find sources of phenomena that are 'ambiguous' enough that one can not convincingly assign probabilities to them. Furthermore the structure and balances of the House and the Senate are constantly changing with each election in the USA. The structure of pressures upon these congresspeople are also constantly changing due to outside shocks as well as internal pressures.

Hence a Central Bank which is attempting to design jointly optimal monetary and fiscal rules is likely to face a lot of 'technical ambiguity' on the fiscal side because it is difficult for it to formulate an agreed-upon 'baseline model' of the fiscal policy implementation and effect process. It is difficult enough to formulate a baseline model of the monetary policy implementation and effect process, much less the fiscal policy counterpart.

In a different but related growth policy-making context, Durlauf and myself (Brock and Durlauf 2001) took the posture that we needed more than one baseline model that we need to robustify against. We have a similar project underway for the design of 'Taylor-type' rules. Extension of this chapter's work on the joint determination of monetary and fiscal policy in a 'Taylor-type' rule framework to a setting where the degree of ambiguities differs between the fiscal part and the monetary part would seem to be a very promising area for future research.

Philip Lane and Giovanni Ganelli (chapter 6) review the recent literature on 'new open economy macroeconomics' (NOEM). Since these models have explicitly stochastic frameworks with explicit micro-foundations and precisely defined wage- and price-setting rules they can directly highlight the role of uncertainty embodied in wages and prices. Several contentious issues must be dealt with in constructing useful abstractions in this area.

A major issue that must be dealt with is where (and in which currency) to put the sticky prices and sticky wages. Sticky wages are not as controversial as sticky prices because economists are used to differentiating labour markets with all their institutional detail and the 'commodities' being traded (e.g. human time accompanied by all those feelings of 'fairness', 'self-esteem', etc. that are not present when trading bushels of soybeans) and commodity markets where 'things without feelings' are traded. (See Bewley 1999 for a strongly argued case for the behaviour of labour markets.)

Even in the commodity markets, controversies lurk concerning on which commodity markets to put the sticky prices. When shocks hit the system, the markets that are 'temporarily' free end up momentarily adjusting to absorb the whole shock before the other markets 'loosen up.' This effect of a shock hitting the momentarily small sliver of 'free-price' markets can cause embarrassing conflicts with data if these models are not set up carefully. No one believes soybean prices are 'sticky' but what about house prices, automobile prices, and the like? But even here high intertemporal substitutability between a car delivered at date t and the same car delivered at date $t+1$ can generate an illusion of a fixed price when they may actually be flexible.

Another major issue is the impact of openness on the conduct of monetary policy. For example, what is the proper measure of inflation in 'Taylor rule'-type exercises? What is the role of the current account and net foreign assets in adjustment processes? For example, net external liabilities can loom large in open economy settings. As pointed out by Cristina Arellano and Enrique Mendoza's chapter 7, external liabilities can interact with credit constraints in such a way as to expose a country to risk of a 'Sudden Stop' disaster. So we are in a difficult area here. This chapter handles this touchy area by walking the reader through the different predictions generated by different models as well as empirical evidence for or against those predictions.

Arellano and Mendoza start out by listing the empirical regularities associated with the 'Sudden Stop' phenomenon of emerging market crises. Empirical regularities of 'Sudden Stop' include sudden loss of access to world capital markets, large reversal of the current account deficit, collapse of domestic production and demand, big 'corrections' in asset prices and sharp increase in prices of tradeables relative to non-tradeables. The authors review recent work on recursive infinite horizon stochastic small open economy models with constraints on borrowing from world capital markets. The borrowing constraints are formulated using recent work on incentive compatibility constraints; there must be willingness to pay in most or all states of the world, and ability to pay. Extensive discussion is given of the ability of these types of advances in theory-building to mimic the empirical regularities of 'Sudden Stops' as well as the ability of such theories to enhance our understanding of the forces that deepen welfare losses caused by 'Sudden Stops'.

Paul Söderlind in chapter 8 reviews the large literature on consumption-based asset pricing models and shows how these models miss not only well-known facts such as the equity premium puzzle and the risk-free rate puzzle, but also cross-sectional facts. The chapter also discusses other recent models of the stochastic discount factor that attempt to fix the data mismatches of earlier models.

Let me use this chapter to launch a few speculations about promising future directions of research on asset pricing. There are two main directions that one might go to improve the match between fact and theory: (i) relatively minor modifications of existing theory, (ii) relatively drastic modifications of existing theory.

In the first category, Akdeniz and Dechert (2002) have shown that they can produce equity premia closer to fact with much smaller risk aversion using a rather conventional production-based asset pricing model with heterogeneous firms and depreciating capital. They have developed a very fast computational algorithm that solves the model with heterogeneous firms where capital that is invested in a sector must remain fixed there for one period before it can be moved. The interaction between (*A*) diminishing returns in the individual sectoral technologies, (*B*) depreciation of the capital used, and (*C*) tastes of the consumers appears to be key. Economic forces *A* and *B* are not available in usual endowment-based models of asset pricing. For example Basu and Samanta (2001) are able to produce the well-known 'Constant Elasticity of Variance' (CEV) relationship between volatility and stock prices by exploiting the relationship between marginal product of capital value of the firm's claims on the income stream from that capital and decreasing returns to that capital. I believe one reason that production-based asset pricing models have not been used as much as endowment-based asset pricing models is because of the lack of rapid computational algorithms and of the lack of very fast computers make them difficult to solve.

Another reason for lack of use of production-based models may be the many puzzles and the grave difficulties that received models have in matching facts that are surveyed in Campbell's chapter in Taylor and Woodford (1999). For example, Campbell suggests the simplest versions of production-based models will generate very stable movements in asset prices because the real interest rate equals the marginal product of capital, which in turn is perturbed by technology shocks. Capital is also costlessly transformable into consumption in the basic model. Hence, Campbell states that modifications such as adjustment costs will be needed to generate realistically volatile asset returns.

Of course other modifications will be needed to the simple Real Business Cycle (RBC)-type model like that used by Akdeniz and Dechert (2002) to study the equity premium in order to avoid conflict with other facts. Adding ingredients such as heterogeneity, more realistic treatments of labour markets and incomplete markets is sure to be a major route to improvement (Calvet, Grandmont and Lemaire 2002; Browning, Hansen and Heckman's chapter 8 in Taylor and Woodford 1999; King and Rebelo's chapter 14 in Taylor and Woodford 1999). Browning, Hansen, and Heckman study both infinite horizon models and overlapping generations models with agent heterogeneity. They give a strong argument that macroeconomic model-building must treat heterogeneity much more seriously than the current literature in order to do a better job in matching and explaining facts.

I believe that as computational methods like Akdeniz and Dechert's become more popular we will see more movement in the asset pricing literature away from the general equilibrium endowment-based models towards general equilibrium production-based models with more heterogeneity, not only on the firm side, but also on the consumer side, as argued by Brown, Hansen, and Heckman. The investment theory work surveyed in this chapter will be a major input.

In the second category one might include theories that depend upon non-stationarities such as Rational Belief Equilibrium (RBE) theory to replace Rational Expectations Equilibrium theories (REE) as in Kurz (1997). Kurz (1997) argues that many asset pricing puzzles including the equity premium puzzle vanish in RBE theory. Another line of theory is 'ambiguity aversion theory' also called 'Knightian Uncertainty' (Epstein and Wang 1994). Epstein and Schneider (2002) argue that this type of theory can explain intertemporal patterns of asset price behaviour like that immediately following the 9–11 attacks more easily than conventional theories. Situations where it is difficult to argue that one can attach probabilities (or satisfy axioms for existence of probabilities) would seem natural for applications of Knightian Uncertainty in finance. But in pure finance theory only 'undiversifiable' risk and 'undiversifiable' Knightian Uncertainty should be priced at any point in time as in the derivations of the Capital Asset Pricing Model (CAPM) and the APT. In intertemporal settings, Epstein and Schneider (2002) discuss Laws of Large Numbers (LLNs) in ambiguity settings as well as mechanisms that prevent the ambiguity from collapsing asymptotically under learning mechanisms. Future research in this area would seem promising for finance.

One might use this theory to argue that the US government itself is currently (26 September 2002) injecting extra Knightian Uncertainty into the environment, that this contribution is at the 'macro' level (it cannot be 'diversified away'), and hence, the stock market seems unable to rebound even though interest rates are at an extremely low level and the fundamentals do not look that bad. Of course a serious study of this possible effect would require a whole paper, not just a speculative comment.

Carl Walsh in chapter 9 reviews empirical facts such as the large and persistent hump-shaped real responses of real output to monetary shocks and the difficulties that models based upon nominal rigidities have in matching these facts. The chapter reviews models based upon a unification of aggregate matching functions in labour economics with optimising models of price rigidities and of monopolistic competition. The chapter treats some of the most difficult facts for DSGE models (even after they are augmented with sticky prices and other bells and whistles). As in Talmain's chapter 11, the problem again is the fact of real persistence. Walsh goes after this challenging problem by combining 'a Mortensen–Pissarides aggregate matching function with an optimising model of price rigidity'. Since relationships between firms and employers are a type of real rigidity this route to producing persistence seems promising. The chapter mentions other routes to persistence such as the Cooley–Quadrini model where money is introduced into a DSGE model, a matching model of the labour market is present, but prices are flexible while nominal portfolio adjustment is sticky. This device gets proper propagation of monetary shocks. Walsh uses a model with wholesale and retail sectors where wholesale production requires matching with retail firms having sticky prices with a fraction optimally adjusting each period. He linearises this model, calibrates it and simulates it to produce many interesting results which he compares to other models.

Stephen Turnovsky in chapter 10 studies the impact of distortions such as taxes in continuous time intertemporal general equilibrium macroeconomic models of Romer type where there are externalities. Many comparative dynamics results are derived. Both chapters by Turnovsky give the reader a nice guide to methods for computing equilibria and doing comparative dynamics in these kinds of models.

Gabriel Talmain in chapter 11 reviews recent literature on RBC models and their mismatches with facts. There is also a short discussion of endogenous business cycle models. There is useful material on computing approximate solutions for a class of RBC models; the chapter then discusses how a more systematic treatment of aggregation across different sectors in multi-sectoral models can lead to models that are more consistent with the facts, e.g., persistence.

Indeed, the chapter argues that once that part of measured persistence due to aggregation of the individual component time series that make up macroaggregates is partialled out of total measured persistence, many 'puzzles' in macro and macrofinance vanish. After a discussion of sources of measured persistence from aggregation that might confuse the unsuspecting researcher, Talmain uses an example (apparently originating from Griliches) of a time series regression of a 'dependent variable' y_t upon its lag, and another 'independent variable' x_t that is AR(1). With an AR(1) error in the y-equation and shows how bad the bias is in the estimates of the constant term and the slope term in the y-equation. He then uses this regression as an expository vehicle to contend, for example, that 'paradoxes such as the well-known uncovered interest parity (UIP) puzzle are purely an artifact of persistence'.

Regardless of one's position on Talmain's arguments, careful investigation of the relationship between properties of component series making up macroaggregates used in empirical work in macro and in macrofinance and the use of these macroaggregate series in attempting to resolve empirical puzzles and anomalies seems a promising area for future research. For example, writers in empirical finance put a lot of stress on how properties of individual firm return series which, in turn, are caused by institutional details in the market settings in which those individual securities are traded, can cause behaviour in index returns series that is difficult to understand without breaking out the individual components of the index. It is surely in the profession's interest to follow a similar research programme in macro, especially when apparent 'puzzles' emerge.

It is natural to launch some speculations prompted by this chapter. First recent work on aggregation of heterogeneous beliefs by Calvet, Grandmont and Lemaire (2002) suggests that puzzles such as the equity premium puzzle may be explained under plausible assumptions on heterogeneity, e.g. 'It is shown further that an upward adjustment of the market portfolio due to the heterogeneity of beliefs, may contribute to explaining such challenges as the so-called "equity premium puzzle" whenever aggregate relative risk aversion is decreasing with aggregate income' (Calvet, Grandmont, and Lemaire 2002: 2).

Second, aggregation theory is closely related to cross-sectional Laws of Large Numbers. Methods from statistical mechanics have been applied to deliver tractable models where cross-sectional LLNs break down (cf. papers in Arthur, Durlauf and Lane 1997). The interaction between cross-sectional LLNs and temporal properties of macroaggregates seems to still need further research.

This ends my short description of each chapter and my musings about several specific chapters. Much of the book is centred around extensions of DSGE models and how they must be modified in order to fit the facts. A fundamental problem that must be faced in macroeconomics (and many other branches of economics) is this. Think of matching facts with a core baseline model with modifications of that baseline model as a mapping M from a domain subspace of Model Space into a subset of Fact Space. Even if we can get macroeconomists to agree on the facts that need to be explained, the set of competing 'core baseline' models equally consistent with these commonly agreed upon facts is likely to be large. This set of equally consistent competing models can be further narrowed by comparing impulse response behaviour with observations and sensibility as in, for example, Christiano, Eichenbaum and Evans's chapter in Taylor and Woodford (1999). But there still will be competing models left after this screening process. This raises the issue of which baseline model serves as the best one on which to erect a research programme. The analogy with multi-armed bandit theory suggests that it may be socially optimal to encourage several competing core baseline models to erect modifications upon.

Even more difficult is the task of getting structural models like DSGE models to do a better job of predicting out of sample than naive and simple models with little economic structure. This problem is well known in the exchange rate area. See Obstfeld and Rogoff's discussion (1996, chapter 9) of the early work of Meese and Rogoff on the inability of 'structural' models to predict out of sample better than naive models such as random walk models and the work that followed. They discuss later work that is consistent with statistically significant superior performance of structural models at very long horizons. They point out that the problem of getting superior performance over naive models on out of sample prediction with structural models is 'shared with virtually any other field that attempts to explain asset price data'. Of course out of sample prediction is not the only criterion. Reasonable Impulse Response Functions (IRFs) is another. Whatever the case, however, this performance problem is likely to lead to enduring controversies since the class of models that do equally well on several reasonable performance criteria is likely to be large and quite varied in their normative implications.

Let me now cut to the chase. This book provides an extremely important service to the economics community. It gets readers to the frontiers of the subject, giving them central theories and central facts that the theories must match and explain. It shows the reader where the baseline theories fail and gives good advice on what must be done to fix them. One can not ask more of a book than that. This book belongs on every economist's bookshelf.

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More information

Preface

The aim of this volume is simple: to demonstrate how quantitative general equilibrium theory can be fruitfully applied to a variety of specific macroeconomic and monetary issues. There is, by now, no shortage of high-quality advanced macroeconomic and monetary economics texts available – indeed two of the contributors to the present volume (Stephen Turnovsky and Carl Walsh) have recently written first-rate graduate texts in just these areas. However, there is often rarely space in a text book to develop models much past their basic setup, and there is similarly little scope for a detailed discussion of a model's policy implications. This volume, then, aims to bridge some of that gap.

To that end, we asked leading researchers in various areas to explain what they were up to, and where they thought the literature was headed. The result, we think, bears testimony to the richness of aggregate economic modelling that has grown out of the real business cycle (RBC) approach to growth and business cycle fluctuations. We treat this book as both a mark of the tremendous progress in this field and a staging post to even further progress subsequently.

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