

On the Role of Small Models in Macrodynamics*

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Abstract

This paper focuses on the role of small models in macrodynamics. It discusses the insights that I believe these models offer and the extent to which they can address some of the complex issues, such as heterogeneity and interactions among agents, that are receiving increasing attention in the literature. I comment on what I view to be the most productive role for numerical simulations, and finally offer some brief comments in defense of the state of modern macroeconomic theory in light of the criticism it has received as a result of the recent financial crisis.

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1. Introduction: Some background

In this paper I will discuss the role that small models have played in the development of macrodynamics over the last 70 years or so. I will consider the insights that I believe these models offer and the extent to which they can address some of the complex issues, such as heterogeneity and interactions among agents that are receiving increasing attention in the literature. I will comment on what I view to be the most productive role for numerical simulations, and finally offer some brief comments in defense of the state of modern macroeconomic theory in light of the criticism it has received as a result of the recent financial crisis.

I was first introduced to macrodynamics back in 1963 when, as a Junior Lecturer in the Department of Mathematics at Victoria University of Wellington in New Zealand, I presented a departmental seminar based on material in RGD Allen's (1956) seminal book. The topics I discussed included the Samuelson (1939)-Hicks (1950) model of the business cycle (trade cycle), the nonlinear cycle models developed by Kalecki (1935) and Goodwin (1951), and the application of the stabilization rules pioneered by Phillips (1954, 1957). This was all very appealing to me, since it seemed like an intriguing use of the methods and techniques that I had learned as an undergraduate, majoring in mathematics. Applied mathematics in those days, at least in New Zealand, usually referred to some aspect of theoretical physics or hydrodynamics, and so I was unusual in showing an interest in applications to economics.

The training I received taught me early on how to construct small dynamic models and to examine their stability and dynamic characteristics. This background was consolidated when, in my second year of the graduate program at Harvard University, I took a course in Economic Growth, taught by Rodney Dobell which included much of this material. I also acquired much of my technical background for dynamic optimization methods at Harvard, where I had the opportunity to attend a course in control methods taught by Arthur Bryson, who is sometimes referred to as the "founder of modern optimal control theory". I was introduced to the methods of continuous-time stochastic optimization by another distinguished control theorist, Murray Wonham, whom I had the good fortune to meet fairly regularly while I was at the University of Toronto in the early 1970s. This interest was pursued further when I joined the Australian National University and began

working with John Pitchford and various students and coauthors, culminating in our edited book, Pitchford and Turnovsky (1977). These were early days in the application of control methods to economics, and it has always been a great source of pride to me that the ANU, despite its remoteness (at the time), was right at the forefront of these developments.

2. Large versus small models

I have always worked with “small analytical models”, being primarily interested in the insights that they have to offer in helping us understand economic principles and processes, with a particular interest in dynamics. Several years ago I was assigned the job of discussing a set of papers in a session at the annual American Economic Association meetings entitled “Complexity and Dynamic in Macroeconomics: Alternatives to the DSGE Models”. The four papers in that session presented diverse criticisms of the state of macroeconomics and proposed progressing in a variety of directions.¹ Overall, these papers were arguing for more complex models, suggesting that the computing facilities now available offer these opportunities. That may well be the case. Nevertheless, this reminded me of the 1960s and the development of large econometric models, specifically the Brookings Model and its international extension to the LINK Project.² While this activity generated great excitement at the time, the payoff has turned out to be limited and I am not convinced that this approach has had any significant lasting impact, at least insofar as its influence on subsequent academic developments is concerned.

Although research in developing more complex models is to be welcomed, I argued in my comments that research employing small models should not be abandoned. Rather, to quote the title of a section of the book edited by David Colander (2006), which formed the basis for this AEA session, I favored a strategy of “edging away from the Dynamic Stochastic General Equilibrium (DSGE) model”.

In advocating for the importance of small models, I do not mean to denigrate the role of large models. On the contrary, larger, more complete, models are clearly necessary to approximate the

¹ The papers were Brock et al. (2008), Colander et al. (2008), LeBaron and Tesfatsion (2008), and Hoover et al. (2008).

² See Duesenberry et al (1965, 1969). The LINK project was associated primarily with the Wharton School at the University of Pennsylvania. Large models were also developed and continue to be developed in large research organizations such as Central Banks and other agencies that have the resources required to develop and sustain them.

complexity of the modern national economy. They are, therefore, entirely natural and appropriate for research being done at central banks, treasuries, and other agencies that are concerned with setting out the specifics of macroeconomic policies (rather than their stylized representation) and evaluating the detailed consequences of their implementation. But it remains important that larger models should try to incorporate the insights provided by small models, and that they not come at the price of obscuring our intuitive understanding of the interactions that they are describing.

3. Some advantages of small models

When I began studying technical economics in the mid 1960s, the focus was on developing models with the objective of using them to try and help us understand the economic responses to specific structural or policy changes. The typical approach was to begin with a small model and then to build upon it in various dimensions, trying to make it more realistic and more all-encompassing. That, for example, is the strategy I adopted in my 1977 Cambridge University Press book, Turnovsky (1977). I began with the simplest static model, with fixed prices, basically the standard IS-LM model. The model was then expanded in several directions by introducing sequentially: (i) inflation and inflationary expectations, via a Phillips curve, (ii) financial asset accumulation, (iii) physical capital accumulation, (iv) international transactions, including asset flows, and (v) finally active government stabilization policy, including optimal policy making.

But the focus always was on analyzing a particular model and deriving its economic implications, understanding the underlying economic mechanisms, and seeing if, and how, they were modified as the model was enriched. The term “transmission mechanism” was frequently applied when describing the channels through which policy and structural changes drive the evolution of the economy. There was little attempt to relate the implications to the empirical evidence, although there was some.³ I think that this was the conventional method of that time and I believe is a fair characterization of, say, Burmeister and Dobell (1970), which was the leading treatise on growth theory at that time.

³ Two examples of this are, first, the stylized facts of growth theory originally enunciated by Kaldor (1961) and summarized by Burmeister and Dobell (1970, p.65). A second example is the early work on speeds of convergence and adjustments of the neoclassical growth model, extensive references to which are provided by Turnovsky (2002).

In contrast, today the emphasis is much more on relating the model to the data, and motivating it in terms of explaining observed empirical episodes and, in some cases, “puzzles”. Whereas in the past, the model itself, or the novelty of the analytical methodology employed, may have sufficed to generate interest in its own right, that is far less so today. Now, the model is primarily a vehicle employed to study a specific economic problem. The importance of the analysis rests entirely on the significance of the economic problem and the appropriateness of the model to address that issue, rather than in any intrinsic interest in the model, itself, or the formal technical skills that it may embody. This reflects a natural maturing of the discipline, and is a consequence of the substantial advances made to macrodynamic theory since the pioneering days of Samuelson and Hicks, and of course is to be welcomed.

In my view, the main advantage of small models is that they focus our thinking, enabling us to identify regularities that would surely be obscured in large models. These regularities may emerge from the underlying theory or they may be motivated by empirical observations, and while they tend to be stylized, they provide insights into the fundamental underlying economic forces. This is particularly true when it comes to characterizing the underlying dynamics, which is always a difficult task. The complexity of large models may force us to restrict attention to only their stationary equilibria, preventing us from investigating their associated transitional paths, which if convergence speeds are slow (as they often are) may in fact may be far more relevant. Finally, small models may provide benchmarks that may serve as natural starting points, both for further analytical investigations, as well as stimulating research directed at examining their empirical relevance. Here are a few prominent examples taken from a range of areas.

3.1 Rational expectations, “Lucas critique”, and policy neutrality

The theory of policy that originated with Tinbergen (1952) and Phillips (1954, 1957) was based on small analytical models, providing insights into the relationships between targets and instruments, and the dynamic adjustments induced by various types of policy rules. As illustrated by Turnovsky (2011), the rules proposed by Phillips were closely related to the class of optimal stabilization rules derived by control engineers. The subsequent introduction of rational

expectations into macro models by Sargent and Wallace (1975, 1976), Lucas (1976), and others also employed small analytical models. Typically, the formal solution of rational expectations equilibria are computationally very complicated, and by employing small models, these authors were able to bring out the main insights in a transparent way. Of these, I take there to be two. First, is the distinction between “forward-looking” and “backward-looking” dynamics and their appropriate resolution. Perhaps because macrodynamics, as introduced by Samuelson (1947) with his background in physics, always assumed that all dynamics were sluggish, evolving from some given starting point, economists were often confused how to approach saddlepoint dynamics, having its elements of both stability and instability.⁴ One of the main contributions of rational expectations was to identify the appropriate way to handle this, something that was made crystal clear by Sargent and Wallace (1973) in their analysis of the Cagan (1956) model of hyperinflation.

The second insight was with regard to policy analysis. One of the features of rational expectations, is that it is forward-looking, incorporating the agent’s information regarding the structure of the economy. In particular, rational expectations include the agent’s perception of policy as part of the economic environment. Lucas (1976) made the profound observation that, in these circumstances, for policymakers to conduct policy under the assumption that the coefficients describing the evolution of the economy remain fixed and invariant with respect to its chosen policy is not rational. This dependence needs to be taken into account in determining the optimal stabilization policy rule. As a related observation, the Lucas Critique calls into question the practice of econometrically estimating the parameters of a reduced form equation. This is because as the policy varies, so do the structural parameters, and consequently the assumption that they remain fixed over a sample period is inappropriate.

The issue of policy neutrality goes further. In their influential article, Sargent and Wallace (1976) provided an example to show that under rational expectations only unanticipated policy changes can have real effects, so that any feedback policy rule, such as the rules proposed by Phillips, will have no effect on output. It turns out that this “policy neutrality” proposition, as it

⁴ As in interesting observation on differences in perspectives, mathematicians often refer to “saddlepoint instability” whereas economists describe the same system as being “saddlepoint stable”.

became known, and which is potentially most damaging to attempts to engage in active stabilization policy, is highly sensitive to model specification, and in particular to the timing of expectations and the information set upon which they are based. It is fair to say that these overly strong policy conclusions are no longer taken very seriously. Nevertheless, the extensive discussion of policy neutrality of 1970s did serve the purpose of focusing research in stabilization policy on the role of expectations and information, which undoubtedly helped in our understanding of the design and effectiveness of policy rules.

All this, including other related issues such as “time consistency”, was facilitated by working with small models and the ability to derive closed-form solutions. I am not sure that even today’s enhanced computational facilities enable us to solve high order rational expectations models, with their combination of numerous backward-looking and forward-looking variables.⁵

3.2 Monetary policy and the exchange rate under flexible exchange rates

Using a small four equation model Dornbusch (1976) showed that while a 1% expansion in the money supply will lead to a 1% long-run depreciation in the nominal exchange rate, due to the sluggishness of prices, on impact this will lead to a more than 1% exchange rate depreciation. In other words, in the short run the exchange rate will overshoot its long-run response. This was an important insight into the consequences for monetary policy of moving to a flexible exchange rate system, following the breakdown of the Bretton Woods system in the 1970s. It showed how, with flexible exchange rates, a domestic monetary expansion could destabilize the exchange rate. Dornbusch’s analysis demonstrated the issue very effectively and led to a flood of papers, both theoretical and empirical, investigating the theoretical robustness of the proposition and its empirical significance. By employing a parsimonious model, Dornbusch was able to highlight the role of key, but previously largely ignored, elements such as exchange rate expectations and market adjustments, thereby enhancing our understanding of monetary policy under flexible exchange rates. These insights would surely not have been detected in a large scale model.

⁵ For a good overview of some of the computational methods available to solve both linear and nonlinear macrodynamic models see Marimon and Scott (1999).

3.3 Some examples from public and corporate finance

In a seminal article, Barro (1974) showed that under a number of stylized conditions debt financing by the government of its deficit is equivalent to taxation. This is because rational forward-looking individuals recognize that increased borrowing by the government today has to be repaid with higher taxes in the future. The recognition of this relationship has led to the focus on the intertemporal dimensions of fiscal policy and the realization that any sustainable fiscal policy must be conducted within a fully specified intertemporal framework.

Somewhat related to this is the extensive literature relating to the optimal tax on capital. Chamley (1986) and Judd (1985) showed that in the long run, income from capital should not be taxed. The argument is simply demonstrated in a Ramsey model. This is another controversial proposition that has played a central role in the optimal tax literature, where its robustness has been, and continues to be, scrutinized under alternative assumptions and scenarios.

As a final example, I mention the Modigliani and Miller theorem. One of the major issues in corporate finance is the cost of capital. Using a simple arbitrage argument, Modigliani and Miller (1958) showed that under certain stylized conditions, the firm's cost of capital is independent of its financial structure. Dividend policy may also be irrelevant. Like the other examples, this has led to a voluminous literature generalizing the proposition, deriving it under more general conditions, and testing its robustness, as well as its empirical relevance.

The propositions that I have summarized above (and there are many others) all yield very sharp predictions and serve as important benchmarks in their respective fields. While they are never robust in their pure form, they all highlight the key underlying economic forces in operation, and in doing so help our intuition and understanding. Furthermore, they all provide a focal point in setting the ensuing research agenda, both in terms of advancing the theory and its empirical testing. It is extremely doubtful that one could have detected the regularities identified in these propositions from large models, or developed the intuition and insights that they provide.

4. Calibration versus simulations

As macro models have been enriched and their dynamic dimension increased, even small models have become less tractable analytically. For example, a macrodynamic model which includes two capital goods, say, physical capital and human capital, will generally lead to a fourth-order equilibrium system involving the two state variables and their associated shadow values. A well-behaved dynamic system requires the linearized system to have two stable and two unstable eigenvalues. However, the determination of the root structure of a fourth-order system having a lot of interdependence between the variables, and therefore the characterization of its dynamics, can be very tedious and ultimately not particularly informative. I can recall having spent several frustrating weeks trying to determine the root structure of such systems. And even if successful in obtaining the required necessary and sufficient conditions for stability to prevail, it often turned out that the resulting constraints on the parameters were quite complex and not particularly illuminating, without some knowledge of the likely parameter values.

Thus, if a workable macrodynamic model is too large to provide insights into its dynamic structure, it is more fruitful to employ numerical simulations.⁶ In this regard, the basic augmented DSGE/Ramsey model calibrates quite successfully. For example, the one-sector neoclassical growth model with private and public capital and endogenous labor supply can replicate the main empirical ratios relatively easily. In addition, it also seems to yield reasonably plausible dynamics, in terms of properties such as convergence speeds, and its implications for fiscal policy. In short, this type of model should be taken seriously, as many proponents of real business cycle models have argued.

But I do have one major problem with existing calibration methods, namely that there is no well articulated methodology for choosing the required parameter values. Essentially, if the numerical simulation requires n parameters to be set, this is done by reconciling the model with n (arbitrarily chosen) conditions, taken from various stylized facts, empirical estimates, equilibrium constraints, and optimality conditions. But there are usually many other constraints that could be considered, but are not. For example, the weight on leisure relative to consumption in utility is often set at around 1.75, mainly because it delivers an allocation of time to labor of around 0.25, which is

⁶ This raises the question of the line of demarcation between “small” and “large” models. While I do not offer a formal distinction, I view a small model as being highly aggregated and stylized, and having a logical structure that can be laid out compactly and explicitly. But, even with only two state variables this may be too cumbersome for an analytical solution to provide much insight, in which case performing simulations may turn out to be more informative.

more or less consistent with empirical evidence. But this says nothing about the sensitivity of labor supply to changes in the wage rate or other determinants such as income or wealth. Many of the empirical estimates employed in macro calibration are drawn from micro studies, raising the question of their appropriateness in terms of reflecting aggregate behavior. Again, a good example of this is the difference in estimates of the elasticity of labor supply with respect to wages obtained from macro versus micro data. These issues are compounded in stochastic models, where many of the calibration analyses focus on matching the second moments of some subset of variables of interest. Ensuring that the model can mimic some particular moment of interest may require structural modifications that may introduce problems in reconciling other parts of the system with empirical evidence. And what about third and higher moments?

In short, there are many more empirical facts to be reconciled than there are parameters to be chosen. So it seems to me that we need to develop some rigorous systematic procedure that uses all available and relevant information in choosing the parameter values employed for our simulations. Finally, I hasten to add that my own use of calibrations is guilty of the criticisms I have just been making.

A couple of years ago I was at Duke University attending a conference celebrating the Solow (1956) growth model. Robert Solow presented a plenary talk at which a member of the audience asked him whether he viewed theory as an art or a science. His response to doing successful theory was summarized in the following three sentences, totaling nine words:

1. “Make it simple.”
2. “Do it right.”
3. “Make it plausible.”

I take this to be a ringing endorsement of small models, coming from one of the pre-eminent economists of all times. The most pertinent point to me in relation to the calibration issue is the third one. Much of the current literature on DSGE models aims at replicating a specific economy over some episode or time period. My view of numerical simulations is a little different. I view them as a way of conducting comparative statics or comparative dynamics, in situations where the model is

too complex to accomplish this analytically with any insight. But this needs to be done over a range of plausible and realistic parameter values. The parameters need not replicate any specific economy or period, but rather must be within the bounds of reasonableness. It is of no interest to show that an economy will evolve in some unexpected or perverse way for a set of parameters that have no conceivable relationship to reality. In Solow's words, the model should be plausible. But parameter values do vary over time and between economies, and therefore I find it useful to compare behavior between economies over a range of key, but relevant, parameter values.

From my standpoint, the important role of macroeconomic models, and particularly dynamic models, is to help us understand processes, rather than to replicate particular experiences or episodes. Most economies are influenced by many factors operating in diverse and often conflicting ways. Undertaking extensive sensitive analysis, over a wide-ranging but plausible set of parameter values, can determine the relative importance of the various influences. They help us form a clearer picture of the underlying economic forces at work and determine which may be the dominant ones. On some occasions the results of numerical simulations have even helped me identify pervasive regularities that I could then establish as formal analytical results, rather than as just parameter-specific numerical simulations. In this case, numerical simulations help tighten the theory.

One further argument in favor of allowing for variations in parameters across plausible ranges, rather than fixing them to replicate some past experience, is the fact that in some cases even small changes in parameter values may lead to major qualitative changes in the implied equilibrium. A good example of where this may occur is the standard two-sector neoclassical growth model, the dynamics of which are critically dependent upon the relative sectoral capital intensities. In this case, a small change in these capital intensities, but large enough to lead to a reversal in their relative sizes, can lead to dramatic changes in the nature of the corresponding equilibrium dynamics; see Bond et al. (1996). Thus, calibrating entirely on the basis of past observed sectoral capital intensities, means that one may fail to capture such small structural changes, should they occur, in which case the numerical simulations are likely to be grossly misleading.

Finally, a closely related issue concerns complications due to the possible existence of multiple equilibria, which arise naturally in non-linear models. In some cases these may occur in

close proximity to one another, with say two distinct equilibria corresponding to plausible calibrations, yet having very different implications. A striking example of this arises in the demographic growth model of Bommier and Lee (2003), who identify two steady-state equilibria, potentially located very close to one another, but having very different characteristics and consequences for the effects of the demographic structure on the economy. The message from all this: Calibration must be done with care and flexibility.

5. Comparative statics, calibration, and sensitivity analysis

One of the basic questions addressed by economic theory in general, and macrodynamics in particular, is to investigate the sensitivity of the system, both its stationary equilibrium and its dynamics, to changes in the underlying structure. Focusing on the steady-state equilibrium, this involves the method of comparative statics, where we analyze the response in the equilibrium to a change in some underlying parameter of interest, which may be either structural or a policy variable.

To illustrate the issue imagine that the economy is in an initial steady-state equilibrium where the steady-state value of the endogenous variable, x , which we denote by \tilde{x}_1 say is determined by

$$\tilde{x}_1 = F(A, \theta_1, \varphi_1) \tag{1}$$

where, for example A denotes the level of technology, θ is some parameter of interest, say the elasticity of labor supply, and φ is some other parameter that affects equilibrium, but is of no particular interest. We assume that in the initial equilibrium A, θ_1, φ_1 have all been set (calibrated) to yield a “plausible” equilibrium value for \tilde{x}_1 .

As in many applications, we may be interested not only in determining the effects of changes in the technology parameter, A on \tilde{x} , but also specifically in studying the sensitivity of these changes to the structural parameter of interest, θ . For example, we may be interested in determining the extent to which an increase in productivity depends upon the flexibility of labor supply. To analyze the consequences of an increase in A , starting from the initial steady state, we compute

$$d\tilde{x}_1 = F(A + dA, \theta_1, \varphi_1) - F(A, \theta_1, \varphi_1) \tag{2}$$

To determine the sensitivity of this change to the value of θ , we typically consider an alternative value, θ_2 , to which the corresponding equilibrium is

$$\tilde{x}_2 = F(A, \theta_2, \varphi_1) \quad (1')$$

and compute the differential

$$d\tilde{x}_2 = F(A + dA, \theta_2, \varphi_1) - F(A, \theta_2, \varphi_1) \quad (2')$$

To assess the impact of θ on the change due to A we then compare (2') to (2). In doing so, the only change is that of θ_1 to θ_2 , although it is also true that the new equilibrium, \tilde{x}_2 , from which the change is computed, will in general not coincide with the original one, and indeed, may no longer be 'plausible'.

In contrast to this exercise, calibrators argue that this comparison is illegitimate, and that to determine the sensitivity to variations in θ , we need to make the comparison from the same equilibrium. For this we need to recalibrate the model. In this simple example, this means that if we want to change θ_1 to θ_2 we must simultaneously change φ_1 to φ_2 so that the initial steady state equilibrium remains unchanged, namely

$$\tilde{x}_1 = F(A, \theta_2, \varphi_2) \quad (1'')$$

To consider the impact of technological change in this recalibrated economy we should then compute

$$d\tilde{x}_1 = F(A + dA, \theta_2, \varphi_2) - F(A, \theta_2, \varphi_2) \quad (2'')$$

and compare this change with (2). The problem is that the comparison between (2'') and (2) does not involve just the sensitivity to θ , the parameter of interest, but also to the change in φ , the adjustment necessary to retain the same starting point.

Both comparisons raise questions. The conventional comparative static approach involves comparisons from different equilibria, while the comparison under the recalibrated model and same initial equilibrium, involves compounding the change in the parameter of interest with some other

accommodating change of no particular relevance or interest. In addition, in a more complete model there are typically many alternative ways to recalibrate the model to ensure a common initial equilibrium, and each one will have a different implication for the sensitivity to the parameter of interest, θ , depending upon their mutual interaction.

6. Should we calibrate to levels or to changes?

Before leaving the issue of calibration, I want to raise the question of whether one should calibrate to levels or changes? Typically, the DSGE models try to match moments, meaning that the authors strive to generate an equilibrium that mimics some actual data reported as levels (or ratios) as closely as possible. But given that most macrodynamic models exclude large portions of the economy I am not convinced that this is always an optimal strategy.

To take a specific example, many and probably most, economic growth models abstract from human capital, and for the issue they wish to address this may be entirely appropriate. But in reality, empirical evidence for the US and many countries suggests that human capital and its enhancement has been the single most important determinant of the growth rate in recent years. If that is the case, it does not make sense for a model that abstracts from this element to generate an equilibrium that tracks the recorded growth rate; on the contrary it should under-predict. But in that case, if one wishes to analyze the effects of some policy change on changes in the growth rate, it may still be reasonable to do so, as long as the policy changes do not impact the growth rate of human capital to any significant degree. It is the analogue to the omitted variables bias problem in econometrics.

To take an example, in a recent paper, Bruce and Turnovsky (2013) employ an endogenous growth model to analyze the effects of social security on the equilibrium growth rate. The model is a kind of Romer (1986) model and abstracts from human capital. In calibrating the model, they obtain an equilibrium growth rate of the order of 1%, which is undoubtedly on the low side. They find that the introduction of social security typically reduces the equilibrium growth rate by around 0.5 percentage points and observe that this appears to be a fairly robust result. They also feel reasonably confident that since social security probably has only a small effect on human capital that

this finding will probably carry over to an augmented model that incorporates human capital and generates a correspondingly higher equilibrium growth rate.

5. Heterogeneity

One criticism of DSGE models is that they impose the assumption of “the representative agent”. This is usually taken to mean that all agents are identical, and therefore one cannot use the model to address issues relating to wealth and income distribution. These are very important issues, particularly from the standpoint of long-term growth, and the dramatic changes in income distribution that have been associated with the rapid changes in technology. The inability to address these issues would be a serious shortcoming of modern macroeconomics.

In fact, DSGE models can handle some important forms of heterogeneity relatively easily, most notably those associated with endowments, which arguably are the most important source of heterogeneity. If, for example, we assume that agents have homothetic preferences but heterogeneous endowments in either or both of physical or human capital, it turns out that the aggregate economy is independent of distributional considerations. Representing preferences by a utility function that is homogeneous in consumption and leisure allows aggregation as in Gorman (1953) or Eisenberg (1961), and generates a representative-consumer characterization of the macroeconomic equilibrium. This enables us to address distributional issues sequentially, thereby increasing dramatically the analytical tractability. First, the dynamics of the aggregate stock of capital and labor supply are jointly determined, independently of distributional considerations. The cross-sections of individual wealth and income and their dynamics are then characterized in terms of the aggregate magnitudes. We get what Caselli and Ventura (2000) call the “representative agent model of distribution”. While this is a somewhat special model, the required homotheticity assumptions are standard in modern growth theory and the model is able to offer important insights into the growth-inequality relationship.

I have employed this approach in some of my recent work, starting with my collaboration with Cecilia García-Peñalosa, and more recently with others as well.⁷ While I find this to be useful

⁷ See e.g. García-Peñalosa and Turnovsky (2006), and Turnovsky and García-Peñalosa (2008).

and the emphasis on endowments to be particularly relevant, an alternative approach adopts the framework developed by Bewley (1977). In this approach the agents have identical endowments and the heterogeneity emerges as the result of idiosyncratic shocks, which permits approximate aggregation.⁸

A related source of heterogeneity concerns the demographic structure. The standard Ramsey growth model assumes that agents are infinitely-lived and therefore cannot address key issues pertaining to life cycle and intergenerational issues. For this purpose the Samuelson (1958)-Diamond (1965) overlapping generations model and the Blanchard (1985) “perpetual youth” model have offered important insights, although both are restrictive in different ways. Recently, progress has been made with introducing more plausible demographic structures into basic macrodynamics, and this development has again been facilitated by the use of small models; see e.g. Bommier and Lee (2003), and extensions by d’Albis (2007), Lau (2009), Gan and Lau (2010), Mierau and Turnovsky (2013).

6. Equilibrium and dynamics

Post-Walrasian economics correctly criticizes the traditionally employed notion of equilibrium. The usual practice of characterizing it as a completely stationary point where all change ceases is too restrictive. An equilibrium in which individuals move about in a balanced “offsetting” way, but with aggregate activity remaining bounded, is probably a more useful concept. Something like this was proposed by Samuelson (1947), where he defined “stability of the second kind” to be the restricted motion akin to the movement of a pendulum.

The standard procedure to analyzing dynamics involves linearizing the dynamic system around steady state and analyzing the resulting eigenvalues. While this is fine insofar as local stability is concerned, it can be misleading in that global stability may be achieved through higher order terms. For example, it is well known that a third dynamic system of the quadratic form, $\dot{x} = Ax + x'Bx$, the linear component of which may be unstable may nevertheless be bounded

⁸ See e.g. Krusell and Smith (1998), and Wang (2007). One of the earliest forms of heterogeneity to be studied was that of diverse discount rates, although the results were not particularly appealing. Bertola, Foellmi, and Zweimuller (2006) provides an excellent discussion of how heterogeneity has been introduced into small macrodynamic models.

globally and may exhibit “chaotic behavior”. In terms of conventional stability, defined in terms of convergence to a stationary point, how acceptable linearization may be depends upon the order of the system and its speed of convergence.⁹

One further related issue concerns the integration of learning dynamics with system dynamics. Most macrodynamic models assume that its parameters are constant over time and known to the agents. This of course is an extreme abstraction, and important contributions by George Evans and Seppo Honkapohja have made significant progress in analyzing the process whereby agents learn the economic structure as they observe its evolution over time.¹⁰ However, their work is always for small models involving a small number of parameters, and it is unclear how practical this would be for a large model.

7. Structure of interaction

The LeBaron and Tesfatsion paper I discussed at the AEA meeting referred to earlier employed Agent-Based Computational Economics Models. These emphasize agent-interaction and the structure of these interactions. It employs a very different approach, modeling the economy in terms of algorithms.

It is fair to say that conventional macrodynamic models are particularly weak in this respect. The standard objective function is one in which agents maximize time-separable utility that depends only on the agent’s own consumption (often plus labor supply or leisure), but independent of the environment. This may be a useful benchmark, but is quite unrealistic.

In reality, most agents care about their environment, where they fit into society, how they are doing relative to their neighbors, and so on. There is now an extensive and growing literature that attempts to model this, doing so by introducing interdependent utility functions, habits, reference consumption levels, consumption externalities, altruism, and alike. This is known generically as “keeping up with the Joneses” or “catching up with the Joneses”, depending upon whether the

⁹ This issue is addressed in some detail in the context of the one-sector neoclassical growth model by Atolia et al (2010). One unexpected insight of that analysis is that with forward-looking agents, which characterize modern macrodynamics, most of the errors from linearization are associated with the size of the initial jump and therefore occur at the beginning of the dynamic transition, rather than accumulating over time.

¹⁰ They have written numerous papers, many of which are integrated into their treatise Evans and Honkapohja (2001).

reference consumption level is related to current or past economy-wide consumption levels. Instead of consumption, these comparisons are sometimes expressed in terms of relative wealth and also leisure.¹¹

An unsatisfactory feature of almost that entire literature is that it adopts the representative agent paradigm. The standard approach is to assume that the economy comprises a large number of identical agents, each of whom takes some reference (external) consumption level as given, and maximizes his own utility conditional on that externality. The macroeconomic equilibrium is then obtained by imposing symmetry, and equating all individuals to the average consumption level. This procedure begs the following fundamental question. If all agents are identical, why does a rational agent not realize that in equilibrium everyone's consumption will be equal, in which case the rationale for comparing one's own consumption level with that of others should disappear? In other words, it would seem that heterogeneity across agents is a fundamental component of any analysis of consumption externalities, such as the effect of jealousy or keeping up with the Joneses, for the behavior of the aggregate magnitudes.¹²

In summary, modeling more general environments of interaction is clearly important, but a subject that can be usefully addressed within the context of a carefully articulated small model.

8. Is Modern Macroeconomics Relevant?

Ever since the financial crisis of 2008 economics has been under attack, much of this being directed more specifically at macroeconomics. Some of the most eminent economists in the profession, writing in highly visible and most prestigious newspapers, have been extremely critical of economic theory and macroeconomics in particular. In the Fall of 2009, CESifo held a conference entitled "What's Wrong with Modern Macroeconomics," devoted to this issue. Amidst the papers criticizing macroeconomics was an article by Michael Wickens (2010) defending modern macroeconomics against the critics and arguing that much of the criticism misses the point. I share Wickens' view that it is unfair to blame macroeconomics and macroeconomic theory for the events

¹¹ Consumption externalities have a very old history, dating back to Veblen (1899) and even Smith (1759). Production externalities, which are the cornerstone of the endogenous growth model, also have a long history, dating back to Marshall (1890). So these ideas are not new.

¹² For an example of such a model that preserves heterogeneity, see García-Peñalosa and Turnovsky (2008).

that occurred.¹³ I also believe that anyone conversant with modern macroeconomic dynamics should not be surprised by what happened.

One of the key components of any macrodynamic model are the transversality conditions. These can be shown to be equivalent to the intertemporal budget constraints and therefore they impose intertemporal solvency on the macroeconomic equilibrium. They are routinely assumed in our macrodynamic models, and in so doing, we are including solvency as part of the rational behavior we are imposing on the equilibrium. Maybe our preoccupation with rational behavior is a bad assumption. For the present, our concern with rational behavior and rational expectations in particular, can be defended as a natural benchmark which undoubtedly should be relaxed. In this respect recent developments associated with bounded rationality and behavioral economics are to be welcomed.

It is widely accepted that the financial crisis was caused by excess consumer borrowing in the US in the household and other sectors, together with lack of regulation. The same is true of the government. In other words, neither the private sector nor the government were observing the transversality conditions that constrain a rational macrodynamic equilibrium. Even the simplest macrodynamic model will show that when this viability constraint is violated the economy will follow an “explosive” unsustainable time path, which is a formal way of describing the manifestation of a crisis. Both macro theorists and policy economists have been aware of this for a long time.¹⁴

The financial crisis was certainly not caused by the fact that macroeconomists work with technical models. On the contrary, I continue to believe that parsimonious technical models are key to aiding our understanding of the economic process. I remember when I was a young professor in Australia in the early 1970s arguing with more senior colleagues who felt that mathematical formulations were unnecessary and that we can verbalize our analysis. I claimed then that the reason why we need formal models is help keep our logic straight, given all the competing forces operating

¹³ In his testimony before the US House of Representatives Committee on Science and Technology, Subcommittee on Investigations and Oversight V.V. Chari presents an interesting defense of modern macroeconomics and the role of theoretical models. This is available at

http://democrats.science.house.gov/Media/file/Commdocs/hearings/2010/Oversight/20july/Chari_Testimony.pdf

¹⁴ I can recall a public lecture presented at the University of Illinois in 1987 by Lester Thurow of MIT, where he warned of the long-run consequences of the US deficit, which at that time was financed largely by Japanese investors.

in the economy, particularly at the aggregate level. I believe this position is even more valid today than it was then.

In my opinion, where macro theory can be faulted is that we have accepted rationality and the application of the intertemporal solvency conditions too uncritically, albeit recognizing that these are natural benchmark restrictions. Macro theorists should probably have been more forceful in articulating the need to keep within the intertemporal budget constraint instead of taking it for granted that it will be met, as we tend to do. In addition, more effort should be devoted to understanding the consequences of violating these constraints, to analyzing and detecting alternative corrective measures to such potential violations, and assessing their feasibility and relative merits. To do this successfully will require further research in macrodynamics, for which small models will continue to play an essential role.

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