



# Algorithmic Social Sciences Research Unit

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## THE MONIAC, MODELLING, AND MACROECONOMICS

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## **The Moniac, Modeling, and Macroeconomics**

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The first time I saw the Moniac was when Abba Lerner was demonstrating it in a hotel lobby at an AEA convention in the 1970s. (Abba had acquired the US rights to the Moniac and had made a number of tapes with him lecturing about macroeconomics using the video of the Moniac as a way to give visual sensibility to his exposition.) As I walked into the hotel lobby on my way to a session, one of the Moniac's tubes sprang a leak, and colored liquid spilled onto the hotel lobby floor. After the session, I again went through the lobby, but the Moniac had disappeared.

It wasn't only there in the lobby where the Moniac disappeared; it was throughout the world. As what was seen as Keynesian economics was abandoned in the 1970s, the various Moniac machines were abandoned as well. Abba's Moniac moved with him to Queens University where I lost track of it—most likely it was abandoned in a storage room.

While the Moniac was abandoned, the macro modeling approach of which it was representative was not, and the goal of this paper is to tell the tale of how the economics profession got from the Moniac to the DSGE model, and to discuss what it lost in the process. That tale is a tale intricately involved with the relationship between economists and models, and with the way in which economists conceive of the modern macro problem.

### **Economists do it with Models**

Modern economics is about modeling, and if you want to understand economists, you must understand their relationship with models. As Jodi Begg's blog puts it, "economists do it with models." Robert Solow has provided a less graphic description of the relationship; he writes: "Today, if you ask a mainstream economist a question about almost any aspect of economic life, the response will be: suppose we model that situation and see what happens." (Solow, 1997, 90) Beginning with the principles course and extending through graduate school, much of a young economist's training is in designing and building models. That training in modeling continues through graduate school, which generally begins with a mathematical boot camp, and then moves on to graduate core courses designed almost exclusively to teach students techniques used in building formal models.

This focus on teaching modeling techniques has displaced other approaches to understanding. For example, students in the core macro course learn almost nothing about past macro literature or about macro institutions. For example, almost no graduate student has heard of, let alone read, John Law, David Laidler, Walter Bagehot or Henry Thornton. In interviews I conducted with graduate students, some didn't know how to pronounce the name, Keynes. Similarly, graduate students learn almost nothing about institutional

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issues such as the Fed Funds rates, mortgage backed securities, or even about monetary and fiscal policy. As one MIT student stated when asked about what he learned about macro policy. “I think that in the macro course we never talked about monetary or fiscal policy, although it might have been slipped in as a variable in one particular model.” (Colander 2008, pg 169).

This pedagogical focus on modeling techniques is by design. Graduate programs concentrate on training academic macroeconomic researchers; they believe (correctly in my view) that to succeed as an academic macro economist today, students don’t need to know the nuances of using monetary or fiscal policy, the institutional structure within which those policies are implemented, the history of macro policy, or the past literature on macroeconomic ideas. What they do need to know is the technique of optimal dynamic stochastic control theory, since that technique underlies the modern macro dynamic stochastic general equilibrium model that is the central model used by academic macro economists. Since publishing articles using imaginative variations of that dynamic stochastic control theory is what will lead students to succeed in academia, the successful graduate programs concentrate on teaching that technique.<sup>1</sup>

### **Alternative Uses of Models**

I am not arguing against models. I don’t know how one can do economics without models. What I am arguing is that how one uses models is what matters. Models have many uses. These uses can be pedagogical, scientific, policy, exploratory, or metaphoric, among many other possible uses.<sup>2</sup> The uses overlap, and when thinking about the usefulness of models it is necessary to be clear about the use to which the model is being put. One can define usefulness of a model only in relations to its intended use. For example, a good pedagogical model likely has quite different characteristics from a good policy model. Many discussions of models are not clear about the intended use of the model, which makes it unclear what is being debated.

In thinking about the Moniac, Phillips’ intended use of the Moniac is clear. He states specifically that the Moniac was designed as a pedagogical model. Phillips (1950) writes the “machines are intended for exposition” and are “an attempt to develop some mechanical models which may help non-mathematicians by enabling them to see the quantitative changes that occur in an inter-related system of variables following initial changes in one or more of them.” (Phillips, 283). These statements are important in understanding Phillips’ intended role of the Moniac. He did not mean it to be used by policy makers, nor did he mean it to be used to advance scientific knowledge. If its use had been to advance scientific knowledge—to explore whether a simple differential equation model is a useful way of analyzing the economy—a formal mathematical model

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<sup>1</sup> This paper was written in 2010, and what techniques are the “in techniques” will likely change in the future. What I do not expect to change is the tendency of the profession to gravitate toward teaching a single technique rather than providing an overview of a wide variety of techniques and issues.

<sup>2</sup> See Epstein, (2008) for a discussion of 16 different uses of models.

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would have been much preferable. For a mathematician, writing down a compact differential equation model would have far more efficiently conveyed all the information that the Moniac conveyed.

The reason Phillips felt that the Moniac was useful was that most economists at the time were not mathematically sophisticated; they needed a pedagogical crutch showing them how models of the economy involving differential equations work. That's why he designed the Moniac as a mechanical hydraulic model using transparent plastic tanks and tubes as his analog computer rather than using an easier-to-construct electronic analog model; the hydraulic model allowed a much better visual presentation of the dynamic process to the non-mathematician.

By the standards of the time, the underlying analytic model of the Moniac was highly advanced. Whereas most of economist's formal models of the time were simple algebraic comparative static models, the underlying structure of the Moniac was dynamic, and it visually related stock and flow equilibria in an elegant manner. But the underlying dynamics and assumed interrelationships in the Moniac were by a mathematician's standards even of that time, pedestrian. The tractable analytic model underlying the Moniac ruled out enormous possible complex interactions, which could have fundamentally changed the model's policy results. Before one related the Moniac's results to policy, one had to adjust for the ruled-out complex interactions heuristically. If one had tried to develop a visual representation of a non-linear system, one would have had a much more complicated Moniac model in which the pressure created by the flows could be too great for the tensile strength of the tubes, or connections, so that catastrophic leaks were commonplace. Such a "catastrophe Moniac" model could have been used for research since the analytics of such models would have been too complicated to solve analytically. Such a model would have suggested that policies were needed to deal with such unexpected catastrophes. The goal of policy in that model would have been preventing breakdown and triage, not damping oscillations. In my view, such a catastrophe model would have better captured Keynes' ideas.

#### **"A" Model vs. "the" Model**

I do not know Phillips' view on whether the Moniac could be adjusted so that it, or some variation of it, could be directly useful in policy. He certainly talked about policy in his article, but that policy discussion was mainly expository in nature. I interpret his Moniac as a way of summarizing policy that followed from the standard macro model of the time. In my view he did not see the Moniac as a simplification of *the* model of the economy; he simply saw it as *a* model that highlighted some important aspects of a macroeconomy.

If Phillips had been following a Keynesian methodology, he would have been clear that The Moniac was "a", not "the", model. I say this because Keynes was clear that there was not just one model of the economy. Keynes put models in a secondary role to interpretation. He writes:

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Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world... Good economists are scarce because the gift for using "vigilant observation" to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one. (Keynes, 1938)

In taking this view, Keynes was placing himself in a broader Classical methodological tradition of modeling. For classical economists, policy was an art and models were aids to intuition. Models did not provide a stand-alone representation of the economy nor a direct guide to policy. Classical economists, and those neoclassical economists staying within that Classical methodological tradition, (which includes Marshall, Robbins, Pigou, JM Keynes and many other economists through the 1950s), followed, to varying degrees, a "strict separation" methodology in which theoretical economic models were not seen as arriving directly at policy conclusions.<sup>3</sup> They did not offer policy advice based on a model. Nassau Senior, the earliest Classical economist who took a strong interest in methodology, clearly stated this classical economist strict separation approach. He writes:

(An economist's) conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice. That privilege belongs to the writer or statesman who has considered all the causes which may promote or impede the general welfare of those whom he addresses, not to the theorist who has considered only one, though among the most important of those causes. The business of a Political Economist is neither to recommend nor to dissuade, but to state general principles, which it is fatal to neglect, but neither advisable, nor perhaps practicable, to use as the sole, or even the principle, guides in the actual conduct of affairs. (Senior 1836: 2-3)

In this Classical methodology, models were tools that provided indirect insight into policy, not direct insight. They required a skilled applied economist with "vigilant observation" to be useful in policy. Policy's domain was a separate branch of economics—the art of economics. (J.N Keynes, 1891). Economic artists were needed to integrate theoretical insights from models with other insights, and that integration required an intricate consumer's understanding of the model, reasoned judgments about limitations imposed by a model's assumptions—judgments that had to be informed by institutional knowledge and non-economic factors. Policy required broadly trained economists whose expertise went far beyond economic modeling, rather than a narrowly trained technical economists.

Since economic models had no direct relevance for policy, models were seen as guides not maps. Classical models were often simple tautologies; they included the quantity theory of money, Say's Law, Ricardian equivalence, and the dichotomy between

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<sup>3</sup> See Colander, 2005, 2009, forthcoming, for elaboration of this point.

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the real and nominal sector. These models were used by Classical economists to correct logical fallacies that lay people often made when thinking about macroeconomic issues. Their models and laws were not used to directly move to policy advice. For that reason their models did not prevent Classical economists from holding policy views that seemed to not fit the model. Thus, for example Jean Baptiste Say, who, as the creator of Say's Law, supposedly didn't believe general unemployment could exist, could write:<sup>4</sup>

In the first place my attention is fixed by the inquiry, so important to the present interests of society: What is the cause of the general glut of all the markets in the world, to which merchandise is incessantly carried to be sold at a loss? What is the reason that in the interior of every state, notwithstanding a desire of action adapted to all the developments of industry, there exists universally a difficulty of finding lucrative employments? And when the cause of this chronic disease is found, by what means is it to be remedied? On these questions depend the tranquility and happiness of nations (Say 1821, p. 2).

The reality is that Classical economists knew quite well that unemployment and depressions could exist, and were trying to deal with both in their thinking about policy. Many economists, including Keynes, advised government spending solutions for the depression long before they had developed a formal model showing how that spending might help the economy.

Similarly, the quantity theory did not prevent banking school economists arguing that banking institutions were central to the financial system, and could cause an economic crisis quite independently of any change in the money supply. Thus, for example, it did not stop Walter Bagehot (1877) from supporting active intervention of the central bank in times of financial crisis. In like fashion, Ricardian equivalence, which showed logically that the method of government finance did not matter, did not prevent Ricardo, and Classical economists generally from believing that in practice government financing mattered a lot, nor did it stop classical economists from arguing that generally sound finance—budget balance, was a policy that governments should follow. In the classical methodology the usefulness of the model could only be determined with an accompanying set of instructions which said when it was to be applied and when it wasn't.

When the Moniac is considered within this Classical methodological approach, it is seen as a useful pedagogical model, but not necessarily as an especially useful policy model. The reason the Moniac is of limited usefulness for policy involves the deviation of its assumptions from those that would be needed to capture the real world it is describing. One important deviation is that the liquids in the Moniac do not capture the problems presented by purposeful forward looking agents. Such agents would likely operate quite differently than the liquids in the Moniac.

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<sup>4</sup> For a discussion of Say, see Per Jonsson (1995).

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A second important deviation of the Moniac from the real world economy is that the underlying differential equations of the Moniac assume very simple linear dynamics, and there is no reason to assume that that is the case in the real world. As Robert May (1976) pointed out, even simple mathematical models often have highly complex dynamics, so given the complicated nature of the economy, a highly complex dynamics would be expected, not the simple dynamics captured by the Moniac. Before one can translate the implications of the Moniac to real-world policy the likelihood of complex dynamics needs to be accounted for. If the Moniac's results are adjusted for these and other relevant deviations of its assumptions from the real world, it might be useful in policy. Without such adjustments, it is not.

### **Keynesian Economics and the Economics of Keynes**

Neither of the above arguments about the limitations of the Moniac would have been news to Keynes. He carefully spelled out in the *General Theory* that radical uncertainty on the part of agents underlay his world view, and that purposeful agents had to be taken into account in thinking about policy. Similarly, he saw the economy as highly complex. Thus the Moniac did not even come close to capturing his implicit model of the macro economy. He would see it as “a” model rather than “the” model. Because Keynes' implicit model of the economy was so much more complex than the Moniac, my belief is that he would have been very hesitant to draw any direct policy conclusions from it. That's why most of his policy recommendations, such as spending on government works, and devaluation of the currency, did not change with the *General Theory*.

The *General Theory* should be seen primarily a theoretical, not a policy book in the sense that it advocated a specific policy for the time. As Peter Clarke (1988) points out, while writing of the *General Theory*, Keynes came to believe that Classical economists had made a serious mistake in their implicit theoretical thinking about the aggregate equilibrium. Classical economists assumed a unique long-run stationary state that involved an equilibrium between aggregate supply and demand. Keynes came to believe that that assumption of a unique long-run equilibrium was not helpful for describing the macroeconomy, especially in a depression. The *General Theory* was an attempt to reformulate Classical theory to be consistent with a multiple equilibrium vision in which complex dynamics could lead the economy to many different equilibria. He reasoned that given the enormous amount of uncertainty in the economy, if purposeful agents and complex dynamics interacted in a certain way, a model using the assumption of a unique stationary state equilibrium was not helpful because that the dynamic forces necessary to drive the economy to that unique equilibrium, even if it could be shown to exist, were too weak to be relevant for most policy issues.

There were many reasons to assume that coordination failure could lead to the economy being stuck at an undesirable equilibrium. To talk about policy when the economy was stuck in such an equilibrium one needed a model of an economy that could get stuck in a unfavorable equilibrium. Whether that model was a short run or a long run model didn't matter for policy if getting out of the undesirable equilibrium was the

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primary policy concern that the model was being used for. A more general theory would have to include an analysis of these issues of complex dynamics, and in his *General Theory* Keynes spelled out a vision of that more general Classical theory.

Keynes' theoretical insight could have marked a fundamental change in macroeconomic thinking. It would have been a substantial addition to the Classical model, and in many ways Keynes is best seen as a precursor to viewing the economy as what today we would call a complex system whose stability is maintained by institutions created by agents, not by inherent natural forces. That theoretical vision of the macro economy as a complex system guided his policy recommendations—no specific model did. That is why I say that Keynes' policy views should not be seen as flowing from an optimal control model. Given the analytic modeling techniques available to him, the macroeconomy was beyond formal modeling, and policy would have to be guided by a combination of insights from a variety of models blended with intuition and institutional knowledge.

The profession did not follow this Classical/Keynesian methodology. Instead the Keynesian methodological approach of having multiple models, and of separating policy recommendations from models, evolved into a neo-Keynesian/neoclassical methodological approach in which policy and models were combined. As that happened, debates about policy became seen as debates about models.

This blending of models and policy prescriptions muddled the debate about models. This neoclassical methodological approach did not distinguish between pedagogical models and policy models, and did not follow a strict separation between scientific economics and policy analysis. The model of the economy that an economist was working on moved from being “a” model to being “the” model, and that model was directly applied to policy.

#### **Abba Lerner and the Moniac**

While many economists played a role in this methodological shift, perhaps the best representative of this shift was Abba Lerner, whose *Economics of Control* (1944) served as a template for economic pedagogy in both micro and macro in the 1940s and 1950s.<sup>5</sup> In micro Lerner presented the policy problem as a technical problem of getting the economy to meet optimality rules that followed from a general equilibrium model of the economy. In macro, policy was presented as meeting the rules of functional finance, which Lerner contrasted with the Classical rules of sound finance. In doing so he turned what Phillips saw as a pedagogical model into an optimal control policy model, and presented the Moniac (and the differential equation model that it visually represented) as a model that conveyed definitive macro policy rules.

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<sup>5</sup> I have discussed the microeconomic aspects of Lerner and teaching in Colander, (2005). Here I concentrate on the macroeconomic aspects.

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The Moniac fit perfectly into Lernerian specification of macro policy. Monetary and fiscal policy could be guided by rules. Policy makers simply turned the monetary and fiscal steering wheels and the economy followed. True, there might be lags but those were second order issues. Over time these second order issues were seen as more and more important. First, fine tuning was abandoned, and then the steering wheel metaphor changed from a car to a supertanker that took miles to turn. But the control metaphor inherent in the Moniac has remained for economists through the 1970s, and for many economists today, the metaphor still captures their vision of macro policy. For non-mathematically oriented students such a world view pedagogically can be neatly captured by the Moniac.

An important problem with this metaphor is that it includes no consideration of purposeful agents or complex dynamics, which means that if policy is too closely related to the model, the results following from a model could be highly misleading. For those using a Classical methodology in which policy involves a blending of models, good sense, institutional knowledge and good instincts. Lerner was seen as a potential problem because he lacked the judgment necessary to know when to apply models to policy and when not to do so. For example, in a letter to Lionel Robbins about Lerner, Keynes wrote “He is very learned and has an acute and subtle mind. But it is not easy to get him to take a broad view of a problem and he is apt to lack judgment and intuition, so that, if there is any fault in his logic, there is nothing to prevent it from leading him to preposterous conclusions” (Keynes, 1932) Similarly, at a seminar at the Federal Reserve, where Lerner had argued that the government should use deficit spending to get the US economy out of the depression, Keynes attacked Lerner, and argued that his suggested policies were totally wrong. (Colander, 1984)

### **The Change in Pedagogy**

While Lerner’s “optimal control” approach to policy may have been inappropriate to directly apply to real-world policy discussions, for pedagogy it was compelling. Paul Samuelson picked up on Lerner’s approach in both micro and macro and made it central to his principles textbook.<sup>6</sup> Samuelson’s textbook was enormously successful, and it became the template for all future principles of economics texts, just as Marshall’s had in the 1890s. Over time, this change in the principles text had a major change in economist’s methodology. Students’ vision became shaped by this optimal control methodological approach, rather than by Marshall’s more eclectic methodological approach, and as those students became economists, the Classical/Keynesian methodological approach, which saw models are only indirectly useful in policy, disappeared, and was replaced by the neoclassical methodological approach that saw models as directly useful in policy.

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<sup>6</sup> He did this for pedagogical reasons; his policy discussions were much more nuanced and far less dogmatic than Lerner’s.

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Robert Solow (1997) has nicely documented this change in pedagogy and change in the use of models. He describes the three textbook that were used in the 1940s economics courses at Harvard as follows:

Most [older texts] provide more institutional descriptions, very sensible discussions of economic policy, and serious looks at recent history as it would be seen by an economist. No one should underestimate the value of these historical reflections. They are, in a way, the application of analytic ideas. But there is a not-so subtle difference. The modern textbook presents and uses economic analysis as a tool to be directly applied to contemporary and historical situations. The students is shown how to map real events into the categories that appear on the axes of the diagrams or the terms in the equations. The older texts are simply more discursive. The underlying ideas are treated more like categories that resonate to this or that bit of history or policy; the authors ruminate more than they analyze.

One sees this clearly in the way these two books present the idea of supply and demand. This is the one piece of analysis that gets careful treatment. Characteristically, however, Garver and Hansen are very good on how one should think about different kinds of commodities—perishable or not, bought frequently or seldom, standardized or not—but the student is not encouraged to make literal use of the apparatus of supply and demand curves. ... I do not want to be misunderstood. Garver and Hansen and Slichter were serious people. Their reflections on the workings of the economy are worth reading. The inspire bursts of nostalgia; words like “civilized” came to mind. ... But it is the tone that I want to emphasize. The modern text treats economics as a collection of analytical tools to be applied quire directly to observable situations. (Solow 88-89)

As I discuss in Colander (2005), the change in economic method that Solow describes began in the 1930s and accelerated in the 1950s, as the incentives within the profession changed. More economists started getting jobs as academic economists, and their incentives for advancement were changing. Instead of gaining recognition for their general arguments, recognition was coming from published journal articles and more technical expositions of models. Nuanced discussions of the limitations of models—what Solow calls discursive writing—disappeared, and students were left with the belief that one moved directly from models to policy. Over time this led to a change in how economics was done, with economic history and heuristic, but possibly convincing, discussions of how models related to policy disappearing from the textbooks and from what was considered serious economic research. Economists were taught how to build models, not how to interpret or apply them.

### **From the NeoKeynesian Model to the DSGE Model**

Ultimately, this change in methodology undermined the simple optimal control approach to macro policy that the Moniac embodied, and led to an abandonment of what was called the Keynesian model. Because the IS/LM type models, within which class the

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Moniac belongs, were presented as “the” model rather than “a” model, these type models drew many criticisms and, starting in the mid 1970s, those criticisms led to the eventual overthrow of IS/LM type models as the appropriate scientific model. The primary focus of that overthrow was the forward-looking agent assumption that the IS/LM style models had not dealt with. Robert Lucas and other New Classicals argued, I think correctly, that if those IS/LM models were populated by forward looking agents, the policy conclusions of the model would be undermined.

The limitations of the IS/LM model was hardly news to old-style fundamentalist Keynesians, such as GLS Shackle (1974), or Alan Coddington (1983), who treated the IS/LM model as simply a pedagogical model and not a policy model. These and who followed a Classical/Keynesian methodological approach of adjusting models for their assumptions before they arrived at policy results. The old-style Keynesian response to New Classicals was essentially—you are right, so give us a better model that captures the complexities of the economy. Until you do, the IS/LM models, used judiciously, still can serve as the best we can do. But that old style Keynesian justification was no longer acceptable, since it involved interpretations of models, not the technical nature of models.

From a policy standpoint, the profession was not provided with a better model. To replace the IS/LM type models, New Classicals offered a new class of optimal control models—dynamic stochastic general equilibrium (DSGE) models, which have become the dominant models taught in graduate macroeconomics today. Unfortunately, while solving one problem—the lack of forward looking agents in IS/LM style models—these DSGE models introduced other problems—the only way one could get a tractable DSGE model was to assume away almost all complex dynamics and agent interactions. Serious consideration of either of these issues led to intractable analytic models, and thus were not explored.

Consider rational expectations. It is natural to assume that people have rational expectations. But what rational expectations are depends on the structure of the model. If agents are uncertain about what is the correct model, or about whether others will follow the correct model, then a unique rational expectation cannot be specified. The initial rational expectations analysis was part of a broad research program with Herbert Simon to study how process affecting equilibrium. (Colander and Guthrie, 1980) It was a neat way around the process problem since it assumed all process problems away, and simply made expectations in the model "model consistent". It was abandoned in micro because it was intuitively too simple to fit economist's intuition about how micro markets works. Agents in markets had multiple models, and “model consistent” expectations would not be rational in such multiple model markets. Yet, later rational expectations became a standard assumption in aggregate macro models where the complexity of the agent interactions and the multiple possible models were even greater. It's primary justification was that it made the resulting macro model tractable.

The evolution of how we moved from rational expectations macroeconomics to New Classical economics, to DSGE models, to the current situation in macro is a story unto itself; it goes through New Keynesian economics, real business cycle economics, to

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Michael Woodford's New Consensus macroeconomics, (2009) which blends the stark assumptions of DSGE modeling purists such as Chari and Kehoe (2006) with whatever New Keynesian assumptions needed to make the "DSGE" model fit the data. These New Consensus models now serve as one of the models that large central banks use in their analysis of the economy. (Small central banks tend not to use DSGE based models, because they don't have enough staff to massage the model sufficiently to make such models even come close to fitting the data.

While, with sufficient massaging, a DSGE-based model can be made consistent with the past data, it cannot forecast the economy, or match truly out of sample data, better than back-of-the-envelope trend extrapolations or their high tech counterparts—structural or cointegrated vector autoregression (VAR) models. The justification of the DSGE model, despite its empirical failure, is that it provides insight into the restrictions that forward-looking agents place on economic variables. But, in fact, DSGE models provide only limited insight into such issues. The problem is that all current DSGE based models assume away complex dynamics, strategic agent interaction, and institutional structural stability problems in which the stability of the system depends on the institutions that agents have build up to limit activity. Yet each of these intuitively likely affects the results, and interacts with the restrictions forward looking agents place on policy issues. Even worse, the almost single-minded focus on DSGE-based models has led to dotting i's and crossing t's on DSGE models. That "dotting i research" has led economists away from studying more complex models that intuitively likely played a significant role in the recent financial crisis, and are likely to play a significant role in a future crisis.

Not only are the DSGE models doing little to advance our theoretical understanding of the economy, they are hurting our understanding of policy. In their current form, they place long-run intertemporal budget constraints on behavior of aggregates within their model, but do not demonstrate why the fallacy of composition would not undermine that logic behind that aggregate behavior.

From a policy perspective these complex dynamics, agent interaction, and uncertainty problems that current DSGE models cannot reasonably deal with are likely to be extremely important; which means that they cannot serve as a reasonable guide for policy; they have essentially assumed away the large majority of the issues that likely cause macro fluctuations.<sup>7</sup>

It would have seemed that the limitations of the DSGE model would have led to multiple macro models being developed. That did not happen. Since a single model had

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<sup>7</sup> It isn't only Keynesians who recognize this. It is also recognized by supporters of the DSGE model. For example, Robert Lucas writes, "there's a residue of things they (DSGE models) don't let us think about. They don't let us think about the U.S. experience in the 1930's or about financial crises and their real consequences in Asian and Latin America; they don't let us think very well about Japan in the 1990's." (Lucas, 2004)

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become de rigor in the profession, New Classicals offered this DSGE model as “the” macroeconomic model rather than as just “a” macroeconomic model to be used with other models and heuristic insight. As “the” macroeconomic model, the DSGE model has as many or more problem as do the IS/LM style models. Both can be massaged to fit the data, but that doesn’t mean that the model is the correct model, or that it has captured the driving forces that we need to understand for to undertake policy. That has to come from what Keynes called “vigilant observation” to choose good models. The question that isn’t asked is whether a model that integrates forward looking agents by assuming representative agents who interact in non-complex ways, a macro economy that is stochastic and subject to definite probabilistic laws, rather than one with the radical uncertainty that Keynes saw agents as facing in the economy, is a “good” model.

This brings us back to the complex systems problem that Robert May (1976) had technically pointed out, and that fundamentalist and Post Keynesians had pointed out more generally. Consider May’s insightful comments about the need for social scientists to consider complex dynamics:

In spite of the practical problems which remain to be solved, the ideas developed in this review have obvious applications in many areas. The most important applications, however, may be pedagogical.

The elegant body of mathematical theory pertaining to linear systems (Fourier analysis, orthogonal functions, and so on), and its successful application to many fundamentally linear problems in the physical sciences, tends to dominate even moderately advanced university courses in mathematics and theoretical physics. The mathematical intuition so developed ill equips the students to confront the bizarre behaviour exhibited by the simplest of discrete nonlinear systems, such as equation (3). Yet such nonlinear systems are surely the rule, not the exception outside the physical sciences....

Not only in research, but also in the everyday world of politics, and economics, we would all be better off if more people realized that simple nonlinear systems do not necessarily possess simple dynamical properties. (May, 467)

We can only start thinking about what is a good model once we realize that we are not going to have a single model that provides us with the “true” model. The world is complex and our analytical tools are limited. We need to stop treating models as truth, and more as algorithmic models that provide us not with truth, but with insight into issues. (Velupullai, 2005) This was the true Classical method, and Keynes’s method as well. It is a method that modern economics has forgotten, and which needs to be relearned.

### **Conclusion**

What does this brief history mean for the Moniac and for macroeconomics? I think it has three implications.

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The first implications that, to be useful, any model needs to come with a set of user instructions to make meaningful conclusions about the model's usefulness. Those instructions must specify the range and domain of the model, and what its assumptions and intended uses are. Only with that set of instructions can one make a reasonable judgment about whether or not it is a good model. This means that the Moniac can only be judged in relationship to the instructions about its use that accompanied it. Since we don't have any definitive instructions, no judgment can be made about whether it is a good model or a bad model.

A second implication is that if my interpretation of the evolution of macroeconomics is correct, Keynesian economics has been seriously misunderstood, both by neoKeynesians and lay people. Keynesian economics should have led macroeconomists to the study of models with both purposeful agents and complex dynamics. Models incorporating both of these aspects would have required much more complicated mathematics than most economists currently use, and likely would not have had definitive results about policy. Non-linearities, and system collapses would be expected, and researchers would have used models for stress tests, where they highlight where the weaknesses in the system are, and how redesign might avoid systemic breakdowns. Stabilization macro policy would have been supplemented by prudential macro policy—policy designed to maintain system stability, and triage policy—policy designed to fix the system after it breaks down.<sup>8</sup>

This Keynesian research program would have involved a theoretical revolution, and would be based on replacing the implicit Classical model of the macro economy arriving at a single unique equilibrium, with a more general model in which the dynamics of the system could lead to a variety of equilibria at which the economy could remain for an extended period of time. In short, what I am suggesting is that it would have led economists to a discussion of precisely the issues that May raised in the above quotation. That modeling would have required highly technical training in the mathematics of complex systems for students designing and building the models.

A third implication is that we need students to be trained in designing and thinking about those set of instructions as well as in technically modeling. The skill of choosing the appropriate model is quite different than the skill of producing models. In macro economics we are far from being able to develop models that are close approximations of the activity they are describing, or of being able to choose models on statistical criteria. We simply don't have the data, the computational machines, or the analytic technology to do it. Because of the complexities in any realistic model and the noise in the data, the close empirical fitting of macroeconomics models to reality is

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<sup>8</sup> Models could still be useful if they are seen as having a limited domain. These models could be analytic, analog, or digital models. I could even imagine a revised Moniac which was purposefully designed with tubing that was thin in areas of the economy where researchers found stress points in the real economy, and the leakages that occurred in my first introduction to the Moniac are seen as part of the modeling process.

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difficult, if not impossible. This means that formal statistical tests of various models will not provide a definitive guide for choosing among models. This lack of empirical selection mechanism means that macro models must be chosen on other grounds. It means that Keynes's rare intellectual technique of "vigilant observation" becomes key to using the model successfully.

The second and third implications seem to pull in different directions; they don't if the profession is seen as heterogeneous profession, where subgroups of economists specialize in certain tasks. Currently, the economics profession does not take advantage of specialization and division of labor. Currently the profession attempts to train "do it all" macroeconomists. Instead, it should see itself as training macro economists for different specialties. Some would be trained to develop technical models; others would be trained to interpret those models, others to teach macroeconomics, while still others would be trained to be generalists whose specialty is applying those models to policy. A few polymaths could do it all, but most would specialize. The current training of many macro economists is seriously flawed because it spends much too much time on training these economists the skills needed to produce models as compared to the skills needed to interpret or consume models that would be most useful in teaching, and applying macro models to policy.

After being presented with all these models students can turn to the really hard problem--deciding which model is appropriate for which situation. As Keynes said, "The theory of Economics does not furnish a body of settled conclusions immediately applicable to policy. It is a method rather than a doctrine, an apparatus of mind, a technique of thinking which helps its possessor to draw correct conclusions."

**Appendix: Is it Time to Power up the Moniac?**

To put my views into perspective, in this appendix I contrast them with those of Bryan Hayes, a senior writer for the *American Scientist* (Hayes, 2009) who recently wrote about the Moniac in an article that asks whether it is time to “power up the Moniac.” My interpretation differs from Hayes in two important ways: (1) the role of modeling in economics and in social science more generally, and (2) how the Moniac relates to DSGE models.

In my view, abstract macroeconomic models, such as the Moniac, the IS/LM model, or the DSGE model, are for me *not* directly applicable to policy. Thus, one cannot say that any abstract model leads to specific policy suggestions independent of context. Models are tools of experts who use them to assist them in arriving at judgments, not to provide them with definitive answers. Models serve to explicate more general macro theories, such as Classical and Keynesian theories, but both the theories and the models have to be interpreted with care. Theories cannot be seen as being fully represented by models; they include nuanced interpretation.

To understand what I mean by nuance, consider the statement by Hayes that “The basic idea in Keynesian economics policy is to counteract any oscillatory tendencies.” That may be the basic idea of Keynesian policy in the undergraduate textbook model, but it is highly debatable as to whether that is the basic policy idea in Keynes’ *General Theory*. I certainly don’t see it that way. Keynes saw himself as saving capitalism and doing much more than simply suggesting countercyclical macro policy as a way of offsetting oscillatory tendencies.

A second difference I have with Hayes concerns the role of optimal control theory in modern macroeconomics. Hayes states that optimal control theory models are out of favor within the economics profession. He writes (Hayes, 2009, 191)

[The Lucas critique] had the collateral effect of dampening enthusiasm for applications of control theory in macroeconomics. Research in the area did not end entirely, but the undertaking lost momentum, and control theory has never again been fully in the mainstream of economic thought. Nor has it become a common tool of those who put policy into practice.

Since DSGE models are definitely in favor in the macroeconomic profession, (Woodford, 2009), Hayes’ statement implies that the DSGE model is not an optimal control model. I think that is an incorrect characterization. In my view, the DSGE model is entirely grounded in optimal control theory and, in that sense, belongs in the same category of models as the Moniac. It allows for the same feedback effects as does the Moniac. What the DSGE model adds to the Moniac’s feedback loops is purposeful forward looking agents. Such agents complicate the feedback process enormously, but do not eliminate the optimal control framework. The reason is that DSGE models are still about control—it’s just that within the tubes of the Moniac, modern macro models picture purposeful rather than inert molecules. It is these purposeful agents that differentiate modeling in

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economics from modeling in physics; they make the economist's optimal control problem much more difficult to specify, solve, and empirically implement. Thus, in my view the DSGE models should be seen as an extension of optimal control theory models of which the Moniac was an early example, and not a rejection of it.<sup>9</sup>

How the DSGE model is characterized is important because the policy results that modern DSGE supporters such as Chari and Kehoe (2006) claim follow from the DSGE model are in large part due to the DSGE models optimal control framework. Specifically, that optimal control framework underlies their argument for rules over discretionary, which is the primary policy conclusion that researchers pull from the DSGE model. It is such rules that underlie support for inflation targeting and opposition to discretionary fiscal policy advocated by DSGE modelers.

What has been rejected by modern DSGE macro economics is a subclass of optimal control models that do not assume purposeful forward-looking agents, but instead just rely on aggregate flows. This subclass of models, which includes all IS/LM models as well as the Moniac, have, as scientifically valid models, I think correctly, been abandoned. Variations of these models may still have a role in policy as practical engineering models to be used in combination with judgment, but they do not serve as an acceptable general scientific model of the macro economy. I agree with that assessment.

My reasoning why this subclass of models should be given less emphasis has nothing to do with the Lucas critique. The problem with such models is that dynamic nonlinearities which are likely inherent in the coordination of the heterogeneous agents that make up the macro economy likely make the macroeconomy far too complicated to develop a tight-fitting technical feasible optimal control model with forward looking agents. Put another way, the macroeconomy needs to be seen as a complex system which continually evolves, oftentimes in unexpected and unpredictable ways. Policy in such economies is best thought about in an evolutionary framework in which the structural model in continually changing in an unpredictable ways that are not adequately captured in an as yet feasible optimal control model.

I am not arguing that we should not develop formal macroeconomic models. I am simply arguing that in macroeconomics, models cannot replace judgment; they might help guide an expert, but they cannot replace the expert. This means that in macro policy, and in social science policy more generally, we must rely on intuition and judgment using models as an aid. In this paper I hope to make my arguments clear by discussing how the profession moved from the Moniac to the DSGE model.

So, my answer to Hayes' question whether it is now time to "power up the Moniac", is "it depends." The Moniac remains a good way for showing the dynamic

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<sup>9</sup> In a skit Stanford students summarized modern macro in a sentence. The summary was "Keynes is Dead; optimal control theory; Keynes is still dead."

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flows and of providing one frame on control problems for non-mathematicians. But to be useful as a pedagogical frame, the Moniac must be supplemented by other frames as well, such as DSGE models, evolutionary models and models with complex non-linear dynamics.

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