

Learning Dynamics: An Essay

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1. INTRODUCTION

The debate in the 1950's over the realism of assumptions underlying macroeconomic models was won by Milton Friedman (1953) and his intellectual allies, in part via the powerful example of the billiards player. An observer, watching the billiards player, might reasonably conclude that a good model of play as it unfolded on the billiards table was going to have to involve reasonably detailed knowledge of physics. Yet, paradoxically, one might ask the billiards player if he has any knowledge of physics which is applied in his playing of the game, and he would say that such knowledge was not part of the way the game is played. Instead, intuition and experience seem to play the largest role. The observer might nevertheless remain convinced that physics was going to have to be part of the formal model of how play actually unfolds on the billiards table. Friedman concluded that the realism of assumptions was irrelevant in judging the fitness of economic models. Predictive content—positive economics—was the preferred criterion.

The realism of assumptions was much less of a concern in the general equilibrium theory of Arrow, Debreu, and McKenzie. These authors were concerned primarily with an idealized existence question: Was it logically consistent to think of all markets as being in equilibrium simultaneously, and if so, what else would have to be true

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of such an economy? In the economies they studied, however, a new question arose. The new question was, How is equilibrium achieved? Are we imagining that, at least initially, trade takes place at disequilibrium prices? If so, how is it that that dynamic process eventually leads to the general equilibrium which is the focus of study? These questions are fundamental to all general equilibrium theory, because equilibrium cannot be produced out of thin air. There must be forces at work, if general equilibrium theory is to be successful, which propel the economy toward the fixed points of the equilibrium maps that form the basis for all modern macroeconomics.

The initial answer to this question was the tâtonnement process. There were schemes, it was argued, that described a plausible dynamic process that could possibly lead to the general equilibrium which is the focus of study. Equilibrium could be achieved, even if initially the equilibrium price vector was an unknown quantity. This was comforting news, but counterexamples soon showed that the tâtonnement process could go badly awry. In the counterexamples, the process cycled, and the equilibrium price vector was never found. Equilibrium might exist, but it might never be achieved. This led to work attempting to refine the tâtonnement process, and to introduce alternative processes, which might lead to some type of more robust stability results for the general equilibrium systems then under study. But the research stalled. Frank Hahn would later write in the *Handbook of Mathematical Economics* that much of the work had a disturbing *ad hoc* quality, and that we economists were not much closer to understanding or resolving the stability question than when we began. Stability remained as the least satisfactory aspect of general equilibrium theory. This was the situation as the rational expectations revolution gathered steam through the 1970s.

In the new, rational expectations models, the economies continued forever, and the agents inhabiting these worlds formed expectations over some future horizon. They formed these expectations in a manner that was consistent with the nature of the world in which they operated. As serious metaphors for actual macroeconomies, these general equilibrium models were now being introduced to a much wider audience within the economics profession. Quite naturally, the same questions were asked all

over again. Among the first to be asked was, Isn't it unrealistic to assume agents are so rational? Isn't it a little preposterous to assume agents have this much foresight concerning the future? The reply that carried the day, perhaps predictably, was essentially to recall Friedman's argument concerning the billiards player. Realism of assumptions was irrelevant. Only predictive content matters for judging the fitness of theories. We make scores of assumptions in our models every day on exactly this basis.

Then came the second, deeper question that had been asked by the original general equilibrium theorists. How is rational expectations equilibrium achieved? Are we saying that agents initially do not have rational expectations, so that trade initially takes place at disequilibrium prices? What forces are put in motion which eventually push the economy toward the equilibrium which is the focus of study? The macroeconomics learning literature was an attempt to respond to this type of deeper question.

2. LEARNABLE RATIONAL EXPECTATIONS EQUILIBRIA

The early returns were not in the affirmative for rational expectations. In 1979 Stephen DeCanio wrote in the *Quarterly Journal of Economics*: "... direct computation of rational expectations by flesh and blood agents in an actual market situation is *impossible in practice*." (Emphasis in original.) Roman Frydman, writing in the 1982 *American Economic Review*, presented an impossibility theorem on convergence to rational expectations similar in spirit. These papers and others seemed to suggest that rational expectations was not an appropriate foundational concept for macroeconomics. But the tide turned. Eventually tâtonnement-like results began to appear in the literature. An influential paper by Margaret Bray (1982) suggested how the arguments might be constructed. George Evans (1985) introduced the notional time concept of expectational stability, and later greatly expanded upon the basic idea in work with Seppo Honkapohja. Albert Marcet and Thomas Sargent (1989ab) argued that a plausible learning concept, like the notion that agents obtain their expecta-

tions from an econometrician running vector autoregressions in real time using endogenously produced data, could be used to define dynamic systems in which rational expectations equilibria were locally stable. The least squares learning framework they developed could be applied to many of the economic models we care about. In this way the impossibility theorems were put to rest. There are now, without question, plausible learning models one can build in which rational expectations equilibria can be attained, even if the system is not initially at that equilibrium. Rational expectations equilibria can be achievable, learnable, expectationally stable. This was comforting, if somewhat tardy, news for the foundations of economic science, since much of macroeconomics had already come to be dominated by rational expectations.

On the other hand, and again perhaps predictably given the course of the earlier debate, counterexamples began to appear. In Bullard (1994), least squares learning can cycle about a rational expectations equilibrium in perpetuity. Guesnerie and Woodford (1989) note similar possibilities in the adaptive systems they study. And the results of the form: “plausible learning can lead to rational expectations equilibria” are not confined to the equilibria we customarily study. Indeed, exotic outcomes, such as stationary cycles and sunspot equilibria, can be achieved under plausible learning schemes, as first demonstrated in the sunspot case by Woodford (1989). In models where there is more than one fundamental equilibrium, such as a model with an externality producing three steady states, Evans and Honkapohja (1993) have shown that more than one can be expectationally stable. Generally speaking, in the department of equilibrium selection, the plausible learning models we currently use seem to rule *in* a lot of equilibria without ruling many of them *out*. In some ways, learning is not panning out very well on the equilibrium selection front, at least if one was hoping that we economists could someday be consistently, across models, selecting a single equilibrium out of many according to some learnability criterion.

There is also a serious pre-coordination problem in the learning story as it is typically told today. We begin the analysis by assuming all agents use the same

mechanical learning rule to form expectations. To what forces can we attribute this pre-coordination on the learning rule? Isn't this mode of analysis simply pushing the question of coordination one step further back? Only a few papers have considered heterogeneity in learning rules, and typically the heterogeneity is introduced on a narrow aspect of the learning rule. Undoubtedly, this has been because of tractability concerns, and also because it is not clear what the complete set of possible learning rules should look like. Should it be, for instance, the set of all possible statistical techniques an agent could use to infer from past data? How could such a set be characterized?

3. ALTERNATIVE APPROACHES

These results and concerns might sound a little disappointing, and their nature has perhaps precipitated a move in the literature toward alternative, more fundamental types of learning models. These alternative models have as their hallmark a noisy evolutionary process by which equilibrium might be achieved. Thus Ellison and Fudenberg (1995) discuss a role for social learning, Easley and Rustichini (199x) analyze choice without beliefs, and Arifovic (1996) builds models with genetic algorithm learning. These models have as a primitive agents choosing actions on a space of decision rules, preferably the entire space of feasible decision rules for the problem the agents face. As time proceeds, better decision rules—ones that deliver higher utility for their users—tend to get copied, while inferior decision rules tend to be discarded. Agents may innovate by sometimes recombining existing decision rules into new forms and by altering small portions of decision rules with small probability. Systems defined in this way are expected to evolve toward stationary equilibria in a general equilibrium setting, and the initial results from this type of research seem promising. In particular, dynamics generated in artificial economies with trial-and-error learning so defined seem to match up well against dynamics observed in analogous laboratory experiments with human subjects.

It is even possible that the rehabilitation of adaptive expectations in policy research will come via this trial-and-error learning route. Most policy work is not about

the equilibrium selection issues which are central to the macroeconomics learning literature. Instead, it is the response to a policy innovation that matters. The Fed raises its target federal funds rate today, and so what happens over the next six months? And for that type of question, standard, plausible learning schemes are a disappointment. For these schemes often imply that the policymaker can take some action today, secure in the knowledge that expectations will take some time to catch up. Thus the policy conclusions tend to be that policymakers can get away with unsustainable policies for short periods of time. This has never been a very satisfactory story about the effects of policy changes, and there is little reason to suspect it will become acceptable in the future.

But under trial-and-error learning the effects of policy intervention may have very different repercussions even though the agents are ostensibly behaving in a similar adaptive manner. The reasoning has to do with the heterogeneity in agent beliefs. In these systems, the essential vehicle for learning is that agents are imitating one another. Bad beliefs can get started, deliver high utility due to feedback effects, and then be propagated, so that bad beliefs can come to dominate the economy and send the system toward an inferior equilibrium. Here the policymaker faces a population of adaptive agents, which, instead of being a set of benign puppets to be manipulated, is instead a collective monster to be feared. A policy misstep can set off a bad reaction in the markets of the model with unintended consequences from the policymaker's point of view. Instead of being able to temporarily get away with unsustainable policies, policymakers might be wise to stay far away from anything remotely questionable for fear that markets might react badly and the economy may coordinate on a poor aggregate outcome. This is obviously speculation, but it seems that formal models of this type are not too far from realization, and such research may have more potential to explain the types of situations actual policymakers often face. One has only to read the newspapers to appreciate how important the management of expectations can be in the real world of policymaking.

4. FIFTEEN YEARS OF PROGRESS

To be able to connect the research on learning in macroeconomic models with policy in a convincing way has long simmered in the literature. Up to now, at least, working in the learning literature has meant one is always the bridesmaid, never the bride. The fundamental basis of the research has been to justify the study of general equilibrium, and to develop equilibrium selection methodologies for macroeconomists. But the picture is changing, and there are now numerous attempts to take advantage of the wide variety of results from this literature to produce novel explanations of macroeconomic phenomena. Marcet and Nicolini (1995), for instance, build a model of recurrent hyperinflation with learning playing a fundamental role. Sargent (1997) models the Phillips curve as the outcome of a complicated dance between a boundedly rational public and a boundedly rational policymaker. And Evans and Honkapohja (1997) show how temporary policy change can induce hysteresis effects when learning is included in the model in a fundamental way. I see this trend toward positive economics as a very hopeful one for this literature.

It was not always so hopeful. But it is perhaps encouraging to note that Evans and Honkapohja (1997) are far more upbeat *vis-a-vis* Hahn (1982) in assessing stability theory for general equilibrium a decade and a half later. Why is this so? Certainly much progress has been made. In addition, there appears to be less of a feeling that the absence of some kind of global stability for general equilibrium should be regarded as a fault. The attitude is more that we write down models with plausible learning playing an integral role, and we take the predictions of these models seriously as metaphors for what can actually happen in market economies. Rational expectations equilibria serve as an indispensable benchmark. But where once we might have seen a model where learning fails to converge to rational expectations, we now see a theory of excess volatility in economic variables. Instead of a disappointing multiplicity of expectationally stable equilibria, we see interesting ways in which policy might influence equilibrium coordination. And more generally, instead of learning providing a subsidiary theory justifying rational expectations equilibria, we see interesting

dynamics in their own right.

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