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RECENT DEVELOPMENTS IN MACROECONOMICS: A VERY QUICK REFRESHER COURSE

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ABSTRACT

This paper outlines the major developments in macroeconomics over the past two decades. It examines the reasons for the breakdown in the consensus view of the 1960s and how this breakdown has guided research in macroeconomics. The introduction and importance of "rational expectations" are discussed, as are recent advances within the new classical and new Keynesian paradigms.

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Fifteen or twenty years ago, it was much easier being a student of macroeconomics. Macroeconomists felt more sure of the answers they gave to questions such as, "What causes output and employment to fluctuate?" and "What is the correct policy response to these fluctuations?"

At the textbook level, the accepted model of the economy was the IS-LM model. It was little changed from John Hicks's (1937) interpretation of John Maynard Keynes's (1936) once revolutionary vision of the macroeconomy. The IS-LM model takes prices as given. To explain the adjustment of prices, a Phillips curve of some sort was appended. Perhaps the Phillips curve even had the natural rate property, allowing the economy to be self-correcting in the long run.

At the more applied level, this consensus was embodied in the large-scale macroeconometric models, such as the MPS model or the DRI model. The job of refining these models generated many PhD dissertations. Private and public decision-makers confidently used these models to forecast important economic time series and to evaluate the impact of alternative macroeconomic policies.

Today, macroeconomists are much less sure of their answers. At some schools, the IS-LM model is not even taught at the graduate level; it is thought to be the relic of a bygone age. At most schools, the large-scale macroeconometric models are

mentioned only briefly. A graduate student today is unlikely to devote his dissertation to improving some small sector of the MPS model.

In contrast to this major change in the way academic macroeconomists view their field of study, macroeconomists in business and government have not substantially changed the way they analyze the economy. They continue to use the large-scale macroeconometric models for forecasting and policy analysis. The theoretical developments of the past fifteen years have had relatively little impact on applied macroeconomics.

Why is there such a great disparity between academic macroeconomics and applied macroeconomics? The view of many academics is that applied macroeconomists have simply fallen behind the state of the art, that they continue to use obsolete models simply because they have not kept up with the quickly advancing field. This self-righteous view cannot be correct, however, for it clearly violates a fundamental property of economic equilibrium: it assumes a profit opportunity remains unexploited. If recent developments in macroeconomics are useful for applied work, they should have been adopted. The observation that recent developments have had little impact on applied macroeconomics is <u>prima facie</u> evidence that these developments are of little use to applied macroeconomists.

One might be tempted to reach just the opposite conclusion: the fact that the macroeconomic research of the past fifteen years has had little impact on applied economists implies the

research has no value. Yet this conclusion is also unwarranted. The past fifteen years have been a very fertile time for macroeconomics. Unfortunately, however, recent developments have not been of the sort that can be quickly adopted by applied macroeconomists.

An analogy from the history of science may be helpful for understanding the current state of macroeconomics.[1] It was approximately five centuries ago when Copernicus suggested that the sun, rather than the earth, is the center of the planetary system. At the time, he mistakenly suggested that the planets followed circular orbits around the sun; we now know that these orbits are actually elliptical. Compared to the then prevailing geocentric system of Ptolemy, the original Copernican system was more elegant and, ultimately, it proved more useful. But at the time it was proposed and for many years thereafter, the Copernican system did not work as well as the Ptolemaic system. For predicting the positions of the planets, the Ptolemaic system was clearly superior.

Now imagine yourself, alternatively, as an academic astronomer and as an applied astronomer at the time right after Copernicus. If you had been an academic astronomer, you would have devoted your research to improving the Copernican system. The Copernican system held out the greatest promise for understanding the movements of the planets in the simplest and intellectually most satisfying way. Yet if you had been an applied astronomer, you would have continued to use the Ptolemaic

system. It would have been foolhardy to navigate your ship by the more promising yet less accurate Copernican system. Given the state of knowledge immediately after Copernicus, a complete separation between academic and applied astronomers was reasonable and, indeed, optimal.

This paper surveys some of the recent developments in macroeconomics. My intended audience includes those applied economists in business and government who often view recent research with a combination of amusement, puzzlement, and disdain. My goal is not to proselytize. Rather, my goal is to show how several recent developments in macroeconomics point the way toward a better understanding of the economy, just as Copernicus's suggestion of the heliocentric system pointed the way toward a better understanding of the planets. Yet just as Copernicus did not see his vision fully realized in his lifetime, we should not expect these recent developments, no matter how promising, to yield high returns in the very near future. In the long run, however, many of these developments will profoundly change the way all economists think about the economy and economic policy.

The Breakdown of the Consensus

The consensus in macroeconomics that prevailed until the early 1970s faltered for two reasons, one empirical and one theoretical. The empirical reason is that the consensus view did not adequately cope with the rising rates of inflation and

unemployment experienced during the 1970s. The theoretical reason is that the chasm between microeconomic principles and macroeconomic practice was too great to be intellectually satisfying.

These two reasons came together most obviously and most profoundly in the famous prediction of Milton Friedman (1968) and Edmund Phelps (1968). According to the unadorned Phillips curve, one could maintain a permanently low level of unemployment merely by tolerating a permanently high level of inflation. In the late 1960s, when the consensus view was still in its heyday, Friedman and Phelps argued from microeconomic principles that this empirical relationship between inflation and unemployment would break down if policy-makers attempted to exploit it. After all, the equilibrium level of unemployment should depend on labor supply, labor demand, optimal search times, and other microeconomic considerations, not on the average rate of money growth. Subsequent events proved Friedman and Phelps correct, as inflation rose without any permanent reduction in unemployment.

The breakdown of the Phillips curve and the prescience of Friedman and Phelps made macroeconomists ready for Robert Lucas's (1976) more comprehensive attack on the consensus view. Lucas pointed out that many of the empirical relationships that make up the large scale macroeconometric models were no better founded on microeconomic principles than was the Phillips curve. In particular, the decisions that determine most macroeconomic variables, such as consumption and investment, depend crucially

on expectations of the future state of the economy. Macroeconometric models treated expectations in a very cavalier way, most often making up plausible but arbitrary proxies. Lucas pointed out that an important feature of most policy interventions is that they change the way individuals form expectations about the future. Yet the proxies for expectations used in the macroeconometric models failed to take account of this feature. Lucas concluded, therefore, that these models should not be used to evaluate the impact of alternative policies.

The "Lucas critique" became the rallying cry for those young turks intent on destroying the consensus. Defenders of the consensus argued that users of macroeconometric models were already aware of the problem Lucas pointed out so forcefully, that the models were nonetheless informative if used with care and judgement, and that the Lucas critique was right in principle but not important in practice. These defenses were not heeded.

As I have mentioned, there were two reasons for the breakdown of the consensus. Both were crucial. Neither the empirical reason nor the theoretical reason was, by itself, sufficient to cause this breakdown. As an exercise in intellectual history, it is instructive to consider two counterfactuals.

Suppose the macroeconometric models had failed to explain the events of the 1970s, but macroeconomists had felt confident in the theoretical underpinning of these models. Undoubtedly the

events could have been explained away. As defenders of the consensus view often emphasize, much of the stagflationary 1970s can be attributed to the OPEC supply shocks. The remainder could always have been attributed to a few large residuals. Heteroskedasticity has never been a reason to throw out an otherwise good model.

Alternatively, suppose the macroeconometric models had performed wonderfully in the 1970s, but that Friedman, Phelps, and Lucas had correctly pointed out the inadequate microfoundations of these models. In this case, the absence of microfoundations would have disturbed only the theoretically obsessive. The prediction of Friedman and Phelps would have been forgotten, even if it had never been put to a test. The correct response to the Lucas critique would have been, "If it ain't broke, don't fix it."

As it turned out, however, the macroeconometric models and the consensus view did fail us both empirically and theoretically. This failure caused a period of confusion, division, and excitement in macroeconomics which is still continuing today.

Directions of Research

Much of the research in macroeconomics over the past fifteen years attempts to deal with the problems that caused the breakdown of the consensus. Renewed and more intensive effort has been directed at the attempt to build macroeconomics on a

firm microeconomic foundation. Very often, the relevance of the research to current macroeconomic problems is sacrificed. To macroeconomic practitioners, much of the research must seem esoteric and useless. Indeed, for practical purposes, it is.

Let me divide recent developments in macroeconomics into three catagories. Like most taxonomies of complex phenomena, the one I propose is highly imperfect. Some developments fall into more than one of the three catagories, while others fall naturally into none of them. Yet the taxonomy is useful, for it helps in understanding the motivation and goals of the research programs undertaken by many academic macroeconomists in recent years.

One large category of research tries to model <u>expectations</u> in a more satisfactory way than was common ten or fifteen years ago. More careful attention to the treatment of expectations can often bring out new and surprising implications out of many standard models. The widespread acceptance of the axiom of rational expectations is perhaps the largest single change in macroeconomics in the past two decades.

A second category of research attempts to explain macroeconomic phenomena within the context of <u>equilibrium</u> models. By the term "equilibrium," I refer to models in which wages and prices adjust instantly to equate supply and demand. Many macroeconomists once presumed that a non-market-clearing theory was necessary to explain economic fluctuations; indeed, such a presumption accompanied the prevailing consensus of the 1960s.

In contrast, recent research has shown that intertemporal equilibrium models have much richer implications and are not so easily dismissed.

A third category of research attempts to rebuild macroeconomics within the context of disequilibrium models. This last category is the most Keynesian and the most compatible with the textbook IS-LM model. This research can be viewed as attempting to put the textbook Keynesian analysis on firmer microeconomic foundations.

Expectations I: Policy Irrelevance

The notion of rational expectations has its roots in John Muth's (1961) brilliant but long neglected paper. Economists routinely assume that firms rationally maximize profits and that consumers rationally maximize utility. It would be an act of schizophrenia not to assume that economic agents act rationally when they form their expectations of the future.

Much of the research in macroeconomics since the breakdown of the consensus has been aimed at exploring the assumption of rational expectations. By itself, the assumption of rational expectations has no empirical implication, just as the assumption of utility maximization has no direct empirical implication. Yet together with other auxiliary hypotheses, many of which predate the introduction of rational expectations and at the time seemed unobjectionable, the assumption of rational expectations can have profound and startling implications.

The result of Thomas Sargent and Neil Wallace (1975), that systematic monetary policy is irrelevant to the path of output and employment, is one of the earliest and most controversial applications of rational expectations. Sargent and Wallace merely applied rational expectations to the natural rate Phillips curve of Friedman and Phelps. This Phillips curve posits that expected inflation does not affect unemployment, but that unexpected inflation temporarily lowers unemployment below the natural rate. Since the assumption of rational expectations rules out surprising people systematically, Sargent and Wallace concluded that systematic monetary policy can affect only expected inflation, not unexpected inflation and unemployment. If correct as a description of the world, this result would render ineffective policy advice such as, "Increase money growth when the economy looks like it is going into a recession."

Much confusion once prevailed over the meaning of the Sargent-Wallace result. Policy irrelevance was sometimes said to be the implication of rational expectations per se. We now know that rational expectations is not the issue at all. As Stanley Fischer (1977) showed, it is entirely possible to construct models with rational expectations in which systematic monetary policy can stabilize the economy. Fischer's model, in which sticky wages play a crucial role, produces Keynesian policy prescriptions despite the presence of rational expectations.

The paper of Sargent and Wallace was important not primarily because of its substantive result of policy irrelevance, but

because it helped familiarize macroeconomists with the use of rational expectations. It showed that models could be solved without invoking arbitrary proxies for expectations, and that the solution with rational expectations could look very different from the solution without rational expectations. The paper by Sargent and Wallace was one of the earliest applying rational expectations to macroeconomic theory, and it illustrated vividly the potential importance of that application.

Once the attention of macroeconomists turned to the central role of expectations, many questions took on a new appearance. Adapting macroeconomic theory to take into account private decision-makers who form expectations appropriately given their environment became a major job of academic macroeconomists. It replaced work on the large-scale macroeconometric models as the primary source of topics for PhD dissertations.

Expectations II: Rules versus Discretion

Of the many questions that have been reexamined, perhaps the most important is the question of whether monetary policy should be conducted by rule or by discretion. A variety of authors, most notably Finn Kydland and Edward Prescott (1977), have provided a new and persuasive reason to be skeptical about discretion in the conduct of monetary policy. In particular, a monetary authority with discretion is likely to choose too high a rate of inflation.

Let us suppose that the world is governed by the

expectations-augmented Phillips curve of Friedman and Phelps. In particular, letting Y denote the level of output, Y* the natural rate, π the rate of inflation, and $E\pi$ the expected rate of inflation, output is determined by

(1) $\quad Y = Y^* + \alpha(\pi - E\pi).$

Output is high when inflation exceeds expected inflation, and output is low when inflation falls below expected inflation.

For simplicity, let us also suppose that the monetary authority chooses the rate of inflation. Of course, more realistically, the monetary authority controls inflation only imperfectly through use of its monetary instruments. But for the purposes of illustration, it is useful to assume that the monetary authority can control inflation perfectly.

The monetary authority likes output to be high and inflation to be low. Suppose that the preferences of the authority can be represented as

(2) $\quad U = Y - \beta\pi^2$

where the parameter β represents how much the monetary authority dislikes inflation.

Now let us compare monetary policy made by a fixed rule and monetary policy made under discretion.

First consider policy under a fixed rule. A rule binds the monetary authority to a particular level of inflation. As long as private agents understand that the authority is bound by this rule, the expected level of inflation will be the level the monetary authority is bound to produce. Since expected inflation

equals actual inflation ($E\pi=\pi$), the level of output will be at its natural rate ($Y=Y^*$).

What is the optimal rule? Since output is at its natural rate regardless of the level of inflation legislated by the rule, there is no benefit to having any inflation at all. The optimal fixed rule requires that the monetary authority produce zero inflation.

Now consider discretionary monetary policy. Under discretion, the economy works as follows:

(a) private agents form their expectations of inflation, $E\pi$;

(b) the authority chooses the actual level of inflation, π;

(c) based on expected and actual inflation, a level of output is determined.

Under this set-up, the monetary authority maximizes its objective in equation (2) subject to the constraint it faces by the Phillips curve in equation (1). When making its decision about the rate of inflation, the authority takes expected inflation as already determined.

What outcome would we expect under discretionary policy? The monetary authority chooses the level of inflation that equates the marginal benefit from the increased output to marginal cost of increased inflation. The marginal benefit is α while the marginal cost is $2\beta\pi$, regardless of the level of expected inflation. The monetary authority thus determines that the "optimal" level of inflation is

$$\pi = \alpha/(2\beta).$$

Of course, rational private agents understand the objective and constraint of the monetary authority. They therefore expect that the monetary authority will choose this level of inflation. Expected inflation equals actual inflation ($E\pi=\pi$) and output equals its natural rate ($Y=Y^*$). In this highly simplified model, optimal discretion produces more inflation than under the optimal fixed rule, while the level of output is the same. Optimal discretion is worse than the fixed rule, even though the authority was attempting to maximize its objective function (2).

It at first seems bizarre that a monetary authority can achieve a better outcome by being bound by a fixed rule. Why can't an authority with discretion mimic an authority bound by a zero inflation rule? The answer is that the authority is playing a game against private decision-makers who have rational expectations. Without being bound by a fixed rule of zero inflation, the authority is not able to get private agents to expect zero inflation.

Suppose, for example, that the monetary authority simply announces that it will follow a zero inflation policy. Such an announcement by itself cannot be credible. Once expectations of inflation are formed, the authority has the incentive to renege on its announcement in order to increase output. Private agents understand the incentive to renege and therefore do not believe the announcement in the first place.

This simple model of monetary policy has an important corollary. As I have discussed, the optimal fixed rule achieves

zero inflation. There is one circumstance in which a monetary authority with discretion achieves this outcome. If the authority dislikes inflation much more than it likes output (that is, if the parameter β is very large), inflation under discretion is near zero. This finding can provide some guidance to those who have the job of appointing central bankers. An alternative to imposing a fixed rule is to appoint an individual with an excessive distaste for inflation.

Finally, note that the issue raised here in the context of monetary policy, more generally called the time inconsistency of optimal policy, arises in many other contexts. For example, it may be optimal for a government to announce that it will not tax capital in order to encourage accumulation; but once the capital is in place, the government may wish to renege on its promise. As another example, the government may wish to announce that it will prosecute vigorously all tax evaders; but after the taxes have been evaded, the government may wish to call a "tax amnesty" to collect some extra revenue. In each case, rational agents understand the incentive for the government to renege, and this expectation affects their behavior. And in each case, the solution is to take away the government's discretionary power by binding it to a fixed policy rule.

Equilibrium

Since Lucas's initial attack on standard macroeconomic practice emphasized the inadequate way expectations were treated,

much effort at reconstruction has been devoted to learning how to deal with the foresight of private economic agents. At the early stages, during the late 1970s, it was believed that the macroeconometric models could be fixed relatively easily. It seemed that the imperfect proxies for expectations merely needed to be replaced by rational expectations. This view, it turned out, was much too optimistic. There was much more work to be done.

The goal of the new classical revolution has been to rebuild macroeconomics while maintaining the axioms that individuals always optimize and, more controversially, that markets always clear. There has been two major strands to this research program.

The early work in this area emphasized the role of imperfect information regarding prices. (Lucas 1972,1973) Individuals were assumed to observe the prices of the goods they produce but not the prices of the goods they purchase. They therefore confuse movements in the overall price level with movements in relative prices. An unanticipated inflation leads individuals to infer that the relative prices of the goods they produce is temporarily high, which induces them to increase the quantity supplied. This story thus generates the natural rate Phillips curve of Friedman and Phelps, in which output depends on the deviation of inflation from expected inflation.

More recent work in the equilibrium tradition has emphasized the intertemporal substitution of consumption and leisure caused

by exogenous technological disturbances. (Barro and King 1984, Long and Plosser 1983, Prescott 1986) These "real business cycle" models have the virtue of being rigorously founded on microeconomic principles: they are actually simplified, intertemporal Walrasian models. Many of the characteristics of economic time series can be surprisingly well mimicked with such models.

Real business cycle theory contrasts most sharply with the consensus view of the 1960s. Associated with this theory are the following four propositions.

(1) The economy experiences large and sudden changes in the available production technology.

(2) Leisure is highly substitutable over time.

(3) Fluctuations in employment are fully voluntary and socially optimal.

(4) Monetary policy has no ability to affect real variables, such as output and employment.

Twenty years ago, all of these claims would have been considered ridiculous. That macroeconomists now seriously entertain a theory with these features shows how radically the field has changed.[2]

Disequilibrium

At the same time that many macroeconomists have been attempting to explain economic fluctuations within the Walrasian paradigm, many other macroeconomists have been attempting to

provide justification for the non-Walrasian assumptions of textbook Keynesian economics. The failure of wages and prices to adjust instantly to equate supply and demand in all markets was the key assumption of the consensus view of the 1960s. If this assumption can be supported by hard-headed microeconomic reasoning, the consensus view can be resurrected, perhaps with some modifications.

Most attempts at explaining disequilibrium have centered on the labor market. The models of Fischer (1977) and Taylor (1980), for example, rely on the existence of labor contracts that specify in advance the nominal wage at which firms can purchase labor. The primary appeal of these models is that they mirror observed institutions. Many workers appear to be covered by contracts predetermining a nominal wage. Incorporated into a macroeconomic model, this observation has important implications for the conduct of monetary policy. In particular, monetary policy becomes a potent tool for stabilization policy, despite the assumption of rational expectations.

These models of nominal wage stickiness have been criticized on three grounds. First, the existence of such nominal wage contracts is never explained from microeconomic principles. If these nominal wage contracts are responsible for large and inefficient fluctuations in output and employment, why do workers and firms write these contracts? There has been much theoretical work studying optimal risk-sharing arrangements between firms and workers. It is clear that optimal contracting cannot produce the

nominal wage stickiness on which Fischer and Taylor, as well textbook Keynesian models, rely.

Second, despite the apparent existence of labor contracts determining nominal wages in advance, it is not obvious that these wages play an important role in the determination of employment. Many workers hold lifetime jobs. In the context of a long-term relationship, there is no reason why a wage paid in a given period should equal the marginal product of labor, as would be true in a spot market. Instead, the wage may be like an installment payment. For example, some universities pay professors' annual salary equally over nine months, while other universities pay the annual salary equally over twelve months; yet surely this difference has no relation to the work effort or marginal product of the professors over the course of the year. Similarly, the fact that the wage paid to a given worker is sticky need not imply that the allocation of labor is determined inefficiently.

Third, the cyclical behavior of the real wage does not appear consistent with the model incorporating a predetermined nominal wage and movements along a standard, downward-sloping labor demand schedule. According to the textbook story, a negative shock to aggregate demand lowers the price level, raises the real wage since the nominal wage is fixed, and thus reduces the quantity of labor demanded. To the extent that fluctuations are driven by aggregate demand, real wages should be countercyclical. Yet in the data, real wages appear acyclical or

a bit procyclical. For example, in the severe 1982 recession, which was allegedly driven by contractionary monetary policy, real wages were not very different from what they were a few years earlier or a few years later. The prediction of countercyclical real wages cannot be easily reconciled with observation.

These three problems with the view emphasizing the stickiness of nominal wages has turned the attention of Keynesian macroeconomists in the 1980s away from the labor market and towards the goods market. A "new Keynesian" view has been emerging. (See Rotemberg, 1987, for a survey.) According to this view, the problem in a recession is not that labor costs are too high but that sales are too low. This emphasis on the goods market can avoid the three problems that plagued the textbook story.

First, even though the problem in a recession is low sales, monopolistically competitive firms do not have much incentive to cut their prices to restore equilibrium. The benefit of a price reduction to the firm may be small (second-order) even though the benefit to society can be large (first-order). If firms face a small "menu cost" to changing prices or if they are only "near rational," they might maintain their old prices despite the substantial social loss from this price stickiness. (See Mankiw 1985, Akerlof and Yellen 1985, Blanchard and Kiyotaki 1987.) Unlike the nominal wage rigidity of the old Keynesian view, the price rigidity of the new Keynesian view does not require any

apparent substantial departure from rationality.

Second, unlike nominal wages, observed rigid prices serve an obviously important allocative function. For example, the prices of magazines at newsstands often stay unchanged for years at a time (Cecchitti 1986). One cannot argue that these prices are merely installment payments within the context of a long-term relationship.

Third, the new Keynesian view does not imply a countercyclical nominal wage. Once price rigidity is introduced as an important element to explain the response of the economy to changes in aggregate demand, real wages can be procyclical or acyclical. Moreover, if price rigidity is combined with the view that observed wages are merely installment payments, one can obtain Keynesian results while leaving the path of wages completely indeterminate and completely irrelevant.

For these reasons, the search for nominal rigidities has shifted from the labor market to the goods market. It would be incorrect to infer, however, that the new Keynesian view embraces an equilibrium labor market. Rather, the new Keynesian view explains unemployment with various sorts of <u>real</u> rigidities; it is only in explaining <u>nominal</u> rigidities and the non-neutrality of money that emphasis has turned to the goods market.[3]

Of the many sorts of real rigidities in the labor market that have received attention, the "efficiency wage" models are probably the most popular. (See the surveys of Yellen 1985, Stiglitz 1986, and Katz 1986.) The common feature of this class

of models is that firms do not reduce wages in the face of persistent unemployment, because doing so would also reduce productivity. The reason for the reduction in productivity may be that lower paid workers put out less effort, that lowering the wage reduces the average quality of workers since only the best workers quit, or a variety of other factors. In all of these stories, the forces moving the labor market to the equilibrium of supply and demand are absent.

Conclusion

I began by suggesting that recent developments in macroeconomics are akin to the Copernican revolution in astronomy: immediately having little practical value but ultimately pointing the way toward a greater understanding. Perhaps the analogy is too optimistic. Copernicus had a vision not only of what was wrong with the prevailing paradigm, but also of what a new paradigm might look like. Macroeconomists have in the past decade realized only the first part of such a vision; there remains much disagreement on the second part. It is undoubtedly easier to criticize the state of the art than to improve it.

Some developments of the past two decades are now widely accepted. The notion of rational expectations is no longer controversial among macroeconomists. Although the debate over rules versus discretion continues, time inconsistency is generally acknowledged to be a serious problem with the use of

discretionary policy. Most fundamentally, almost all macroeconomists agree that basing macroeconomics on firm microeconomic principles should be higher on the research agenda than it has been in the past.

Yet on the crucial issue of business cycle theory, there appears to be little movement toward a new consensus. The "new classicals" and the "new Keynesians" each have made substantial advancements within their own paradigms. To explain economic fluctuations, new classical theorists now emphasize the roles of technological disturbances, intertemporal substitution, and real business cycles. New Keynesian theorists now speak of monopolistic competition, menu costs, and efficiency wages. More generally, the classicals continue to believe that the business cycle can be understood within a model of frictionless markets, while the Keynesians believe that market failures of various sorts are essential to explaining fluctuations in the macroeconomy.

Recent developments in macroeconomic theory will ultimately be judged by whether they prove to be useful to applied macroeconomists. The passage of time will make efficiency wages, real business cycles, and the other "breakthroughs" of the past decade less novel. The attention of academic researchers will surely turn to other topics. Yet it is likely that at least some of these recent developments will permanently change the way in which economists of all sorts think about and discuss economic behavior and economic policy. A decade from now we will know

which of these developments has the power to persevere past the initial debate and permeate economists' conception of how the world works.

Notes

1. A caveat: Not being a historian of science, I cannot vouch for the accuracy of the details of the story. But regardless of whether it is fully accurate, the story serves nicely as a parable for macroeconomics.

2. I discuss my views on real business cycles in Mankiw (1987).

3. The interaction between nominal and real rigidities is a research topic that is beginning to be explored. See, for example, Ball and Romer (1987).

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