

A Quick Refresher Course in Macroeconomics

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Introduction

TWENTY YEARS AGO, it was 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 "How should policy respond 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' (1937) interpretation of John Maynard Keynes' (1936) once revolutionary vision of the economy. Because the IS-LM model took the price level as given, a Phillips curve of some sort was appended to explain the adjustment of prices. Some thought the Phillips curve had the natural rate property, implying that the economy was self-correcting in the long run.

At the more applied level, this consensus was embodied in the large-scale macroeconometric models, such as the MIT-Penn-Social Science Research

Council (MPS) model. The job of refining these models generated many dissertations. Private and public decision makers confidently used the models to forecast important economic time series and to evaluate the effects of alternative macroeconomic policies.

Today, macroeconomists are much less sure of their answers. The IS-LM model rarely finds its way into scholarly journals; some economists view the model as a relic of a bygone age and no longer bother to teach it. The large-scale macroeconometric models are mentioned only occasionally at academic conferences, often with derision. A graduate student today is unlikely to devote his dissertation to improving some small sector of the MPS model.

In contrast to this radical change in the way academic macroeconomists view their field of study, applied macroeconomists have not substantially changed the way they analyze the economy. The IS-LM model, augmented by the Phillips curve, continues to provide the best way

to interpret discussions of economic policy in the press and among policy makers. Economists in business and government continue to use the large-scale macroeconomic models for forecasting and policy analysis. The theoretical developments of the past twenty years have had relatively little impact on applied macroeconomics.

Why is there such a great disparity between academic and applied macroeconomics? The view of some academics is that practitioners have simply fallen behind the state of the art, that they continue to use obsolete models because they have not kept up with the quickly advancing field. Yet this self-serving view is suspect, for it violates a fundamental property of economic equilibrium: It assumes that a profit opportunity remains unexploited. If recent developments in macroeconomics were useful for applied work, they should have been adopted. The observation that recent developments have had little impact on applied macroeconomics creates at least the presumption that these developments are of little use to applied macroeconomists.

One might be tempted to conclude that, because the macroeconomic research of the past 20 years has had little impact on applied economists, the research has no value. Yet this conclusion also is unwarranted. The past 20 years have been a fertile time for macroeconomics. Recent developments have just not been of the sort that can be quickly adopted by applied economists.

A. *A Parable for Macroeconomics*

A tale from the history of science is helpful for understanding the current state of macroeconomics. Because I am not an historian of science, I cannot vouch for its accuracy. But regardless of whether it is true in detail, the story serves nicely as a parable for macroeconomics today.

Approximately five centuries ago, Nicholas Copernicus suggested that the sun, rather than the earth, is the center of the planetary system. At the time, he mistakenly thought that the planets followed circular orbits; 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 superior.

Now imagine yourself, alternatively, as an academic astronomer and as an applied astronomer when Copernicus first published. If you had been an academic astronomer, you would have devoted your research to improving the Copernican system. The Copernican system held out the greater promise for understanding the movements of the planets in a simple and intellectually 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 functional separation between academic and applied astronomers was reasonable and, indeed, optimal.

In this paper I survey 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, it is to show how several recent developments point the way toward a better understanding of the economy, just as Copernicus' suggestion of the heliocentric system pointed the

way toward a better understanding of planetary motion. 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 be of great practical use in the 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.

B. *The Breakdown of the Consensus*

The consensus in macroeconomics that prevailed until the early 1970s faltered because of two flaws, one empirical and one theoretical. The empirical flaw was that the consensus view could not adequately cope with the rising rates of inflation and unemployment experienced during the 1970s. The theoretical flaw was that the consensus view left a chasm between microeconomic principles and macroeconomic practice that was too great to be intellectually satisfying.

These two flaws came together most dramatically and most profoundly in the famous prediction of Milton Friedman (1968) and Edmund Phelps (1968). According to the unadorned Phillips curve, one could achieve and 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 tried to exploit it. They reasoned that the equilibrium, or natural, rate 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: Inflation rose without a permanent reduc-

tion in unemployment.

The breakdown of the Phillips curve and the prescience of Friedman and Phelps made macroeconomists ready for Robert Lucas' (1976) more comprehensive attack on the consensus view. Lucas contended that many of the empirical relations that make up the large-scale macroeconomic 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 course of the economy. Macroeconomic models treated expectations in a cavalier way, most often by resorting to plausible but arbitrary proxies. Lucas pointed out that most policy interventions change the way individuals form expectations about the future. Yet the proxies for expectations used in the macroeconomic models failed to take account of this change in expectation formation. 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 macroeconomic models were already aware of the problem Lucas defined so forcefully, that the models were nonetheless informative if used with care and judgment, and that the Lucas critique was right in principle but not important in practice. These defenses were not heeded.

As I have mentioned, the consensus in macroeconomics broke down because of two flaws. Both were crucial. Neither the empirical flaw nor the theoretical flaw was, by itself, sufficient to cause the breakdown. As an exercise in intellectual history, it is instructive to consider two counterfactuals.

Suppose the macroeconomic 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 assert, 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 macroeconomic models had performed wonderfully in the 1970s, but that Friedman, Phelps, and Lucas had nevertheless spelled out their inadequate microfoundations. In that case, the feeble foundations 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 Lucas critique might have haunted theoretical eccentrics, but the general response would have been "If it ain't broke, don't fix it."

As it turned out, however, the macroeconomic models and the consensus view did fail both empirically and theoretically. This failure led to a period of confusion, division, and excitement in macroeconomics which still continues today.

C. *Directions of Research*

Much of the research in macroeconomics during the past 20 years attempts to deal with the problems that caused the breakdown of the consensus. Economists have focused renewed and more intensive effort on building macroeconomics on a firm microeconomic foundation. Very often, the relevance of the research to current economic problems is sacrificed. To macroeconomic practitioners, much of the research must seem eso-

teric and useless. Indeed, for practical purposes, it is.

Let me divide recent developments in macroeconomics into three categories. Like most taxonomies of complex phenomena, the one I propose is imperfect. Some developments fall into more than one of the three categories, and a few 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 *expectations* in a more satisfactory way than was common 20 years ago. More careful attention to the treatment of expectations can often extract new and surprising implications from 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 using *new classical* models. These models maintain the assumption that prices continually adjust to equilibrate supply and demand. Twenty years ago, macroeconomists commonly presumed that a nonmarket-clearing theory of some sort was necessary to explain economic fluctuations. Recent research has shown that market-clearing models have much richer implications than was once thought and are not so easily dismissed.

A third category of research attempts to reconstruct macroeconomics using *new Keynesian* models. This last category is the most compatible with the textbook model that combines the IS-LM model with a modern Phillips curve. This research can be viewed¹ as attempting to put textbook Keynesian analysis on a firmer microeconomic foundation.

Expectations

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 explored 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.

A. Policy Irrelevance

One of the earliest and most controversial applications of rational expectations was made by Thomas Sargent and Neil Wallace (1975). They asserted that systematic monetary policy is irrelevant to the path of output and employment. To reach this conclusion, Sargent and Wallace merely applied rational expectations to the expectations-augmented Phillips curve of Friedman and Phelps. This Phillips curve posits that inflation that is expected does not influence unemployment, but that unexpected inflation temporarily lowers unemployment below its natural rate. The assumption of rational expectations, however, implies that people cannot be surprised by events that occur systematically or by policies that are applied in a uniform and consistent fashion. Sargent and Wallace reasoned that *systematic* monetary policy

can generate only inflation that is expected; it cannot produce unexpected inflation and therefore cannot affect unemployment. If correct as a description of the world, this result would render policy rules such as "Increase money growth when the economy looks as though it is going into a recession" ineffective.

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 Sargent-Wallace paper was important not 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 more conventional solution. 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. Rethinking macroeconomic theory to take into account how private decision makers form expectations appropriate to their environment became a major job for academic macroeconomists. It replaced work on the large-scale macroeconomic models as the primary focus of research.

B. Rules Versus Discretion

Of the many questions that have been reexamined, perhaps the most important is whether public policy should be conducted by rule or by discretion. Various authors have provided a new and often persuasive reason to be skeptical about discretionary policy when the outcome depends on the expectations of private decision makers (Finn Kydland and Edward Prescott 1977; Guillermo Calvo 1978; Fischer 1980; Robert Barro and David Gordon 1983).

The argument against discretion is illustrated most simply in an example involving not economics but politics—specifically, public policy about negotiating with terrorists over the release of hostages. The announced policy of the United States and many other nations is that the government will not negotiate over hostages. Such an announcement is intended to deter terrorists: If there is nothing to be gained from kidnapping, rational terrorists won't take hostages. But, in fact, terrorists are rational enough to know that once hostages are taken, the announced policy may have little force, and that the temptation to make some concession to obtain the hostages' release may become overwhelming. The only way to deter truly rational terrorists is somehow to take away the discretion of policy makers and commit them to a rule of never negotiating. If policy makers were truly unable to make concessions, the incentive for terrorists to take hostages would be substantially reduced.

The same problem arises less dramatically in the conduct of monetary policy. Consider the dilemma of a monetary authority concerned about both inflation and unemployment in a world governed by the expectations-augmented Phillips curve of Friedman and Phelps. The authority wants everyone to expect low inflation, so that it will face a favorable

trade-off between inflation and unemployment. But an announcement of a policy of low inflation is not credible. Once expectations are formed, the authority has an incentive to renege on its announcement in order to reduce unemployment. Private economic actors understand the incentive to renege and therefore do not believe the announcement in the first place. Just as a president facing a hostage crisis is sorely tempted to negotiate the hostages' release, a monetary authority with discretion is sorely tempted to inflate to reduce unemployment. And just as terrorists discount announced policies of never negotiating, private economic actors discount announced policies of low inflation.

The surprising implication of this analysis is that policy makers can sometimes better achieve their own goals by having their discretion taken away from them. In the case of hostages, there will be fewer hostages taken and fewer hostages killed if governments are bound to follow the seemingly harsh rule of abandoning any hostages that are taken. In the case of monetary policy, there will be lower inflation without higher unemployment if the monetary authority is committed to a policy of zero inflation.

This theory of monetary policy has a trivial but important corollary. Under one circumstance, a monetary authority with discretion achieves the same outcome as a monetary authority bound to a fixed rule of zero inflation. If the authority dislikes inflation much more than it dislikes unemployment, inflation under discretion is near zero, because the monetary authority has little incentive to inflate. This finding provides some guidance to those who have the job of appointing central bankers. An alternative to imposing a fixed rule is to appoint individuals with a fervent distaste for inflation.

The issue raised here in the context

of hostages and monetary policy is more generally called the *time inconsistency of optimal policy*. It arises in many other contexts. For example, the government may announce that it will not tax capital in order to encourage accumulation; but once the capital is in place, the government may be tempted to renege on its promise because the taxation of existing capital is nondistortionary. As another example, the government may announce that it will prosecute all tax evaders vigorously; but once the taxes have been evaded, the government may be tempted to declare a “tax amnesty” to collect some extra revenue. As a third example, the government may announce that it will give a temporary monopoly to inventors of new products to encourage innovation; but once a product has been invented, the government may be tempted to revoke the patent to eliminate the distortion of monopoly pricing. 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.

C. Rational Expectations in Empirical Work

So far I have been emphasizing developments in macroeconomic theory. But the widespread acceptance of rational expectations as a methodological tenet has also had a profound influence on empirical work. By focusing attention on how economic actors should behave under uncertainty, the rational expectations revolution has changed the way macroeconomists formulate their theories and the way they use data to test them.

An example of a topic that has been extensively reexamined in the light of rational expectations is the permanent income theory of consumption. In a seminal paper, Robert Hall (1978) pointed out

a simple and surprising implication of the theory: Changes in consumption should be unpredictable. According to the permanent income theory, consumers facing an intertemporal budget constraint try their best to smooth the path of their consumption over time. As a result, consumption reflects consumers’ expectations about their future income; consumption changes only when consumers revise these expectations. If consumers are using all available information optimally, the revisions in their expectations should be unpredictable, and so should changes in their consumption. In essence, Hall applied the logic of the efficient markets hypothesis, which economists have long used to explain the unpredictability of stock prices, to the permanent income hypothesis.

Formulated in this way, the permanent income hypothesis is easily tested. One merely regresses the change in consumption on some set of lagged variables to see if these variables can forecast changes in consumption. When Hall ran these regressions, he found, to the surprise of many economists, that the theory passed this test, at least as a first approximation. Changes in aggregate consumption from quarter to quarter are largely unpredictable. Like stock prices, consumption is close to a random walk.

To see how revolutionary Hall’s approach was, consider how an empirical researcher gauges success. Twenty years ago, empirical research on consumption most often entailed estimating consumption functions. Success was measured by how well the estimated equation fit the data; that is, success was a high R^2 . Hall turned this standard on its head, arguing that the permanent income theory is valid precisely because he found a low R^2 . This difference arises because Hall did not estimate a consumption function, but instead examined the intertemporal first-order condition of a representative

consumer to check whether this consumer was making systematic errors in optimization.

In retrospect, it is clear that Hall's contribution was more methodological than substantive. Hall concluded that the evidence strongly favored the permanent income hypothesis. Subsequent research, some of which has followed Hall's approach, has found that current income has a stronger influence on consumption than the permanent income hypothesis predicts (Marjorie Flavin 1981; Hall and Frederic Mishkin 1982; John Campbell and Gregory Mankiw 1989, 1990; Chris Carroll and Lawrence Summers 1989). There remains much controversy about the validity of the permanent income hypothesis, but there is little doubt that Hall changed forever the terms of the debate.

Once revolutionary, the rational expectations approach to empirical work is now standard. It finds its most advanced development in the Euler equation methods that evolved from Hall's work on consumption. Researchers have applied these methods to study labor supply, labor demand, spending on consumer durables, business fixed investment, and inventory accumulation. Although these new techniques are unlikely to replace old-fashioned econometric approaches completely, they have earned a permanent place in the empirical economist's toolbox.

New Classical Macroeconomics

Because Lucas' initial attack on standard macroeconomic practice emphasized the inadequate way expectations were treated, the first task facing macroeconomists was to learn how to deal with the foresight of rational economic agents. At the early stages of the new classical revolution, some economists believed that the macroeconomic 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 too optimistic: There was much more work to be done. The goal of the new classical revolution was to rebuild macroeconomics beginning with microeconomic primitives of preferences and technology. The new classical economists pursued this goal while maintaining the axioms that individuals always optimize and, more controversially, that markets always clear.

A. Imperfect Information

The earliest new classical models had the aim of generating a monetary business cycle. To do this, they departed slightly from the Walrasian paradigm by assuming imperfect information regarding prices (Lucas 1972, 1973). Individuals were assumed to be more aware of the prices of the goods they produce than they are of the prices of the goods they purchase. They therefore tend to confuse movements in the overall price level (which should not matter) with movements in relative prices (which should matter). An unanticipated inflation leads individuals to infer that the relative prices of the goods they produce are temporarily high, which induces them to increase the quantity supplied. This story thus implies that output depends on the deviation of inflation from expected inflation. In this way, the assumption of imperfect information was used to generate the expectations-augmented Phillips curve of Friedman and Phelps.

Although this theory of the business cycle received much attention in the 1970s, it has attracted few adherents in more recent years. The reason for its decline in popularity is not clear. Critics argue that confusion about the price level cannot plausibly be so great as to generate the large changes in output and em-

ployment observed over the business cycle. The empirical evidence has also been generally unfavorable (Barro and Zvi Hercowitz 1980; Mishkin 1983). But there is no completely compelling evidence that explains why this approach has been so widely abandoned.

B. *Real Business Cycles*

Those working in the new classical tradition have recently been emphasizing “real” business cycle theory (John Long and Charles Plosser 1983; Barro and Robert King 1984; Prescott 1986). This theory proceeds from the assumption that there are large random fluctuations in the rate of technological change. Because these fluctuations in technology lead to fluctuations in relative prices, individuals rationally alter their labor supply and consumption. The business cycle is, according to this theory, the natural and efficient response of the economy to changes in the available production technology.

The strengths of real business cycle models are that they are highly parsimonious and, at the same time, rigorously founded on microeconomic principles. They are often standard intertemporal general equilibrium models, common in the study of economic growth, amended only slightly to include random changes in technology. These models mimic the behavior of important economic time series surprisingly well. Edward Prescott provocatively concludes that the business cycle is not a puzzle; rather, because economic fluctuations are a natural implication of standard growth models, it would be a puzzle if we did not observe business cycles.

Real business cycle theory contrasts sharply with the consensus view of the 1960s. I will mention briefly three assumptions of these models that 20 years ago would have been considered ridiculous and that today remain controversial.

First, real business cycle theory assumes that the economy experiences large and sudden changes in the available production technology. Many real business cycle models explain recessions as periods of technological regress—that is, declines in society’s technological ability. Critics argue that large changes in technology, and especially technological regress, are implausible (Summers 1986; Mankiw 1989). It is a more common presumption that technological progress occurs gradually.

Second, real business cycle theory assumes that fluctuations in employment reflect changes in the amount people want to work. Because employment fluctuates substantially while the determinants of labor supply—the real wage and the real interest rate—vary only slightly, these models require that leisure be highly substitutable over time. This assumption conflicts with many econometric studies of labor supply using data on individuals, which typically find small intertemporal elasticities of substitution (Joseph Altonji 1986). It also conflicts with the strong prior beliefs of many economists that high unemployment in recessions is largely involuntary.

Third, real business cycle theory assumes—and this is the assumption from which the theory derives its name—that monetary policy is irrelevant for economic fluctuations. Before real business cycle theory entered the debate in the early 1980s, almost all macroeconomists agreed on one proposition: Money matters. Although there was controversy about whether systematic monetary policy could stabilize the economy, it was universally accepted that bad monetary policy could be destabilizing. Real business cycle theorists have challenged that view using the old Keynesian argument that any correlation of money with output arises because the money supply is endogenous (King and Plosser 1984). They

also give little weight to anecdotal evidence on the effects of monetary policy—like the Volcker disinflation of the early 1980s—that seems to shape the views of many other economists.

C. Sectoral Shifts

Another new classical approach to the business cycle is the sectoral shift theory, which emphasizes the costly adjustment of labor among sectors (David Lilien 1982; Fischer Black 1987). Like real business cycle theory, the sectoral shift theory observes the classical dichotomy by giving no role to monetary disturbances. But unlike real business cycle theory, it departs slightly from the Walrasian paradigm by assuming that when a worker moves from one sector to another, a period of unemployment is required, perhaps for job search. According to the sectoral shift theory, recessions are periods during which there are more sectoral shocks and thus a greater need for sectoral adjustment.

Although there is still much empirical work being done, the weight of the available evidence appears not to support the sectoral shift theory. If workers are unemployed voluntarily in recessions because they are moving to new jobs in other sectors, we would expect to find high unemployment coinciding with high job vacancy. Yet observed fluctuations have just the opposite pattern: High unemployment rates coincide with low levels of help wanted advertising (Katharine Abraham and Lawrence Katz 1986). Moreover, although the sectoral shift theory suggests that workers are moving between sectors during recessions, the opposite appears to be the case: The measured movement of workers is strongly procyclical (Kevin Murphy and Robert Topel 1987). These findings suggest that the sectoral shift theory is unlikely to be plausibly reconciled with observed economic fluctuations.

Advocates of the sectoral shift theory argue that evidence of this sort is not persuasive. It is possible that because the process of sectoral adjustment requires a period of high unemployment and low income, it lowers the demand for the products of all sectors. Thus, we might observe low vacancies and low movement during recessions, even if recessions are initially caused by the need to reallocate labor among sectors. In this form, it is not clear how to distinguish empirically the sectoral shift theory from real business cycle theories that emphasize economy-wide fluctuations in technology or Keynesian theories that emphasize fluctuations in aggregate demand.

New Keynesian Macroeconomics

At the same time that many macroeconomists have been attempting to explain economic fluctuations within the Walrasian paradigm, many other macroeconomists have been working within the non-Walrasian approach that has evolved from Keynes' *General Theory*. The rubric "Keynesian" is so broad and so vague that many researchers have applied the term to their theory. If there is a single theme that unites Keynesian economics, it is the belief that economic fluctuations reflect not the Pareto-efficient response of the economy to changes in tastes and technology, but rather some sort of market failure on a grand scale.

The market imperfection that recurs most frequently in Keynesian theories is the failure of wages and prices to adjust instantly to equilibrate supply and demand. Certainly, the short-run sluggishness of wages and prices was the key assumption of the consensus view of the 1960s. And the absence of an adequate theoretical justification for that assumption was one of the fatal flaws that undermined the consensus. Here I examine,

roughly in order of historical development, three recent lines of research that each in its own way emphasizes the failure of prices to clear markets. Much of this research can be viewed as attempting to resurrect the consensus view, with some modifications, by providing a cogent theoretical foundation of hard-headed microeconomic reasoning.

A. *Fixed Prices and General Disequilibrium*

Beginning with the seminal paper by Barro and Herschel Grossman (1971), much research in the 1970s used the tools of general equilibrium theory to examine how markets interact when prices are fixed at nonmarket-clearing levels. This research program was especially popular among European macroeconomists (Edmond Malinvaud 1977; John Muellbauer and Richard Portes 1978; Jean-Pascal Benassy 1982). It showed in the most rigorous terms how quantities adjust when prices cannot and how economic policies influence output and employment under fixed prices.

A significant result of these models is that the behavior of the economy depends crucially on which markets are experiencing excess demand and which are experiencing excess supply. Unemployment—an excess supply of labor—arises in two regimes. In the first regime, called *classical unemployment*, firms can sell all they want in the goods market; unemployment arises because the real wage is too high for all of the labor force to be profitably employed. In the second regime, called *Keynesian unemployment*, firms are unable to sell all they want at the going price; unemployment arises because of this quantity constraint in the goods market. The difference between these regimes highlights some important questions that recur in Keynesian theorizing. Is the key market imperfection causing high unemployment in reces-

sions located in the labor market or in the goods market? If there are imperfections in both markets, how do they interact? These questions have also received attention recently from Keynesian theorists pursuing a quite different research program, and I return to them below.

Because these general disequilibrium models were proposed prior to the breakdown of the prevailing consensus of the 1960s, they are not directly aimed at remedying the flaws that caused the breakdown. To concentrate on the implications of fixed prices, these models beg the question of why prices do not adjust to clear markets. In the wake of the new classical revolution, which appears to have had a greater impact on this side of the Atlantic, American Keynesians were less concerned with the details of quantity adjustment under fixed prices. They directed their efforts at modeling the price adjustment process.

Once attention turns to the question of price adjustment, an incongruity of these general disequilibrium models becomes apparent. These models impose fixed prices on otherwise Walrasian economies. Yet to analyze the question of how prices adjust, it is necessary to admit that some economic actors have control over prices. Thus, one needs to go beyond the price-taking assumption of general equilibrium theory and explicitly incorporate price-setting agents, such as unions or firms that enjoy some degree of market power. Once one starts to think about an economy with price setters, however, it appears unlikely that it will behave like an economy in which prices are set by a Walrasian auctioneer who, for some unspecified reason, fails to choose equilibrium prices. Therefore, the general disequilibrium models stemming from Barro and Grossman may not provide the best framework for addressing even the issues for which they are designed, such as quantity adjustment

under fixed prices. Put simply, it seems impossible to divorce the issue of quantity adjustment from the issue of price adjustment.

B. *Labor Contracts and Sticky Wages*

Most attempts at explaining why the economy departs from the Walrasian ideal have centered on the labor market. Keynes himself emphasized the sluggish behavior of wages. Therefore, when economists skeptical of the new classical revolution tried to defend Keynesian economics, the labor market was the natural place for them to start.

A prominent line of research modeled the labor market as failing to clear because of labor contracts that specify in advance the nominal wage at which firms will be able to purchase labor (Jo Anna Gray 1976; Fischer 1977; John Taylor 1980). The primary appeal of these models is that they mirror observed institutions. Many workers are covered by formal contracts predetermining a nominal wage, and many others appear to be covered by informal agreements with employers. Incorporated into a macroeconomic model, this observation has important implications for the conduct of monetary policy. One of these implications is that the Sargent-Wallace policy-irrelevance proposition does not hold: If the nominal wage is unable to respond to economic disturbances, then monetary policy that does systematically respond to them is a potent tool for stabilizing the economy, despite the assumption of rational expectations. In essence, a fixed nominal wage gives the monetary authority control over the real wage and thus control over employment.

These models based on nominal wage contracts were criticized on three grounds. First, the existence of such 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 these Keynesian contracting models rely. Because unemployed workers value their leisure less than the firm values their labor, these contracts leave substantial and obvious gains from trade unexploited.

Second, despite the 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, as these models assume. Many workers hold lifetime jobs. In the context of a long-term relationship, a wage paid in any given period need not equal the marginal product of labor, as it would 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 observation that some wages are 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 models incorporating a predetermined nominal wage and movements along a standard, downward-sloping labor demand schedule. In most of these models, a negative shock to aggregate demand lowers the price level, raises the real wage (because 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 to have no consistent relationship with economic activity, or perhaps appear 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 the evidence.

Economists differ about whether they view these criticisms as serious. At the very least, these problems with the labor contracting models placed Keynesians on the defensive in the academic debate.

C. *Monopolistic Competition and Sticky Prices*

Dissatisfaction with models emphasizing the stickiness of nominal wages turned the attention of Keynesian macroeconomists in the 1980s away from the labor market and toward the goods market. Much effort has been devoted to examining the behavior of monopolistically competitive firms who face small “menu costs” when they change prices (Mankiw 1985; George Akerlof and Janet Yellen 1985; Michael Parkin 1986; Olivier Blanchard and Kiyotaki Nobuhiro 1987; Julio Rotemberg and Garth Saloner 1987; Laurence Ball, Mankiw, and David Romer 1989). Taken literally, these menu costs are the resources required to post new price lists. More metaphorically and more realistically, these menu costs include the time taken to inform customers, the customer annoyance caused by price changes, and the effort required even to think about a price change.

This line of research is still too new to judge how substantial its impact will be or to guess what problems will be judged most serious. What is clear now is that this emphasis on the goods market can avoid the three problems that

plagued the Keynesian model based on sticky wages alone.

First, these new models can explain in rigorous microeconomic terms the failure of price setters to restore equilibrium. Monopolistically competitive firms do not have much incentive to cut their prices when the demand for their goods declines. Yet because of the preexisting distortion of monopoly pricing, the benefit to the society of a price cut may be large (first-order) even when the benefit to the firm is small (second-order). If firms face even a small menu cost, they might maintain their old prices, despite the substantial social loss from this price stickiness.

Second, unlike nominal wages, many of the rigid prices we observe have a clearly important function in allocating resources. For example, the prices of magazines at newsstands often remain unchanged for years at a time (Stephen Cecchetti 1986). It is hard to argue that these prices are merely installment payments within the context of a long-term relationship and therefore irrelevant.

Third, these models with menu costs do not imply a countercyclical real 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 indeterminate and 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 Keynesians now embrace an equilibrium labor market. Rather, it is more common to explain unemployment by various sorts of *real* rigidities that prevent real wages from falling to equilibrate the labor

market. It is only in explaining *nominal* rigidities and the non-neutrality of money that emphasis has turned to the goods market.

Of the many sorts of real rigidities in the labor market that have received attention, the "efficiency wage" models are probably the most popular (Yellen 1984; Jeremy Bulow and Summers 1986; Katz 1986; Joseph Stiglitz 1986). The common feature of this class of models is that firms do not reduce wages in the face of persistent unemployment because to do so would reduce productivity. Various reasons have been proposed to explain how wages may affect productivity. A sociological explanation is that lower-paid workers are less loyal to the firm. An explanation based on adverse selection is that a lower wage reduces the average quality of the work force because only the best workers quit. The most popular explanation of efficiency wages is "shirking." Because firms monitor effort imperfectly, workers sometimes shirk their responsibilities and risk getting fired; a lower wage reduced the cost of getting fired and thus raises the amount of shirking. In all of these efficiency wage theories, the impact of wages on productivity diminishes the incentive for a firm to cut wages in response to an excess supply of labor. If this productivity effect is sufficiently large, the normal competitive forces moving the labor market to the equilibrium of supply and demand are absent.

In an important paper, Laurence Ball and David Romer (1990) have shown that nominal rigidities caused by menu costs are enhanced by real rigidities such as efficiency wages. Menu costs prevent prices from falling in response to a reduction in aggregate demand. Rigidity in real wages prevents wages from falling in response to the resulting unemployment. The failure of wages to fall keeps firms' costs high and thus ensures that they

have little incentive to cut prices. Hence, although real wage rigidity alone is little help in understanding economic fluctuations because it leads only to classical unemployment and gives no role to aggregate demand, real wage rigidity together with menu costs provide a new and powerful explanation for Keynesian disequilibrium.

Conclusion

I began by suggesting that recent developments in macroeconomics are akin to the Copernican revolution in astronomy: Immediately they may have little practical value but ultimately they will point the way to a deeper 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 would look like. In the past decade, macroeconomists have taken only the first step in this process; there remains much disagreement on how to take the second step. It is undoubtedly easier to criticize the state of the art than to improve it.

Yet some developments of the past two decades are now widely accepted. Although some economists still doubt that expectations are rational, and despite the mixed evidence from surveys of expectations, the axiom of rational expectations is as firmly established in economic methodology as the axioms that firms maximize profit and households maximize utility. The debate over rules versus discretion continues, but time inconsistency is generally acknowledged to be a problem with 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.

On the crucial issue of business cycle theory, however, there appears to be lit-

tle movement toward a new consensus. The “new classicals” and the “new Keynesians” each have made substantial advances within their own paradigms. To explain economic fluctuations, new classical theorists now emphasize technological disturbances, intertemporal substitution of leisure, 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 necessary to explain fluctuations in the economy.

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 “break-throughs” of the past decade less novel. The attention of academic researchers will surely turn to other topics. Yet it is likely that 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. Twenty years from now we shall know which of these developments has the power to survive the initial debate and to permeate economists’ conceptions of how the world works.

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