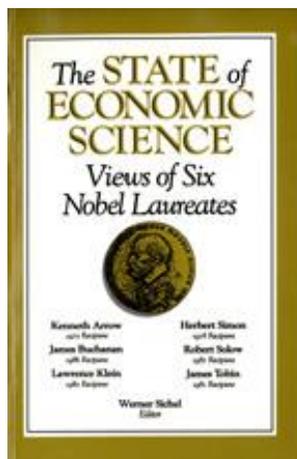

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During the past forty-five years **JAMES TOBIN** has focused on and made major contributions in the fields of macroeconomic theory, monetary theory and policy, portfolio theory, economic growth, and consumer behavior.

Professor Tobin holds A.B., M.A., and Ph.D. degrees from Harvard University. He has been awarded more than fifteen honorary degrees from U.S. and foreign universities. After spending two post-doctoral years at Harvard as a Junior Fellow and a year at Cambridge University, Dr. Tobin joined the faculty of Yale University in 1950. At Yale he served for seven years as Director of the Cowles Foundation for Research in Economics and for five years as Chairman of the Economics Department. On leave from Yale, Dr. Tobin was a Visiting Researcher at the Survey Research Center of the University of Michigan, a member of President John F. Kennedy's Council of Economic Advisers, Visiting Professor at the Institute for Development Studies of the University of Nairobi, Visiting Professor at the University of Minnesota, and Ford Visiting Research Professor at the University of California, Berkeley.

Dr. Tobin is a past president of the American Economic Association, the Econometric Society and the Eastern Economic Association. In 1955, he was awarded the John Bates Clark Medal by the American Economic Association. He is also a fellow of the American Academy of Arts and Sciences and the American Statistical Association and a member of the National Academy of Sciences and the American Philosophical Society. Dr. Tobin has been a consultant to many agencies including the Board of Governors of the Federal Reserve System, the U.S. Treasury Department, and the Congressional Budget Office. Dr. Tobin is presently a member of the Board of Trustees of the Joint Council on Economic Education and the Twentieth Century Fund and is a member of the National Academy of Sciences' Committee on the Status of Black Americans.

Professor Tobin has written several books and monographs as well as over 300 articles in journals and books. Titles of Dr. Tobin's books that are indicative of his work include: *National Income Policy*, *Financial Markets and Economic Activity*, *Essays in Economics: Macroeconomics*, *The New Economics One Decade Older*, *Essays in Economics: Consumption and Econometrics*, *Essays in Economics: Theory and Policy*, and *Macroeconomics Prices and Quantities*.



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I am pleased and honored to participate once again in this valuable series, and I congratulate the University and my hosts on its 25th birthday. The topic this year is very broad. I shall confine myself to the area of my greatest interest and experience, macroeconomic theory. It happens also to be the arena of the liveliest controversies over substance and methodology during the past 20 years. I suspect that the revolutions or counterrevolutions in macroeconomics may be the principal reasons that the organizers of this series invited the speakers to comment on the present state of economic science.

Even macroeconomic theory is too general for one essay. After a brief methodological introduction, I shall narrow my focus to a specific substantive issue, to what in my opinion is *the* fundamental issue of macroeconomics: the existence, reliability, strength, and speed of adjustments by which a market economy maintains or restores economywide equilibrium between the supplies of labor and of the products of labor and the demands for those services and goods. A half century ago, during the Great Depression, intense debate on this issue split the economics profession between John Maynard Keynes and the revolutionaries he inspired, on the one side, and the defenders of established orthodoxy, on the other. Today the same battle is rejoined, and the same

ground is contested with more powerful ammunition. Those who, like me, were young rebels in the 1930s are now, as this essay will make all too clear, on the defensive. Whatever you may think about the merits of the controversy, you may find it interesting to relate the new debate to the old one on the same issue.

I begin with a general discussion of methodologies, old and new, in macroeconomics.

The “Micro Foundations” Methodology of Modern Macroeconomics

Macroeconomics has been a distinct field in economic theory only since Keynes’s 1936 book, *The General Theory of Employment, Interest and Money*. The word “macroeconomics” itself, adopted to distinguish the study of economies as a whole from the study of households, businesses, markets, and sectors, “microeconomics,” is of even more recent vintage. The central paradigm of economic theory begins with “micro” as the calculus of rational self-interested behavior by individual decisionmakers. They determine their supplies of and demands for multitudes of commodities by maximizing their incomes of their utilities subject to the constraints of their resources. Competitive markets coordinate their choices, balancing demands for all commodities with supplies. Prices adjust to clear all markets. Through the responses of rational economic agents to these price signals, markets transmute micro selfishness and myopia into optimal social allocations of resources—as if by an “invisible hand,” to quote the famous metaphor of Adam Smith. The theory of general equilibrium, perhaps the most impressive intellectual structure in social science, gives rigorous content to Smith’s intuitive conjecture. It purports to build the whole economy from the behaviors of individual agents. But it is a framework of analysis, rather than a source of specific conclusions about the signs and magnitudes of relationships among economic variables—e.g., price and income effects on demands and supplies or effects of taxes on prices and quantities.

The shortcuts and simplifications of macroeconomics were and are the inevitable costs of getting interesting and testable propositions, of which full-blown general equilibrium theory is virtually empty. From

Keynes on, macro model builders relied on the standard paradigms of neoclassical theories of the behavior of individual agents in specifying their behavioral equations. But Keynes and his successors had to use information and hypotheses about behavior other than the implications of optimization theory. They could appeal to empirical observation, or to hunches about plausibility, to place restrictions on individual behaviors. Moreover, aggregate relationships are the results of diverse behaviors of multitudes of individual agents; a structural macro equation combines assumptions about individual behavior and assumptions about aggregation. Macro modelers also inject realism about the institutions and economic structures of the economies they are describing. Those economies did not, do not, conform to the assumptions of highbrow general equilibrium, for example, perfect and complete competitive markets.

Pure theorists naturally found macro models aesthetically unappealing and intellectually confusing. They criticized macro relationships as *ad hoc* because they were not explicitly derived from “first principles,” i.e., from optimizations by individual decisionmakers. “Micro Foundations!” was the rallying cry of the methodological counterrevolution against Keynesian economics, really against all macroeconomics. Its protagonists complained of the absence of explicit derivations of macro behavioral equations from optimization; they proposed to build a new macro solidly and clearly based on individual rationality. Only relationships with those micro foundations, they said, could be expected to be stable over the range of applications—not just forecasts but also conditional estimates of the effects of policy interventions and other exogenous variations—to which macroeconomics aspires. This viewpoint has swept the profession.

After 15 or 20 years of methodological counterrevolution, where do we stand? “What you gain on the swings you lose on the roundabouts.” Aggregation is a tough problem, so it is just finessed. It is easy to display explicit micro foundations when you assume the whole private economy can be represented as one agent, Robinson Crusoe, or as two who differ only in age and endowment, operating in competitive markets with flexible prices. But of course there are no transactions in these markets

(except in the overlapping generations model once every two-period lifetime). The immense volumes of transactions we actually observe in markets for assets and commodities are simply not explained. No heed is paid to all the problems of coordination and communication which concerned Keynes and other macro theorists—the differences between savers and investors, lenders and borrowers, bulls and bears, risk-lovers and risk-aversers, and so on.

Why the “representative agent”—Robinson Crusoe—is a less *ad hoc* and more defensible simplification than the dirty constructs of earlier macro modelers, and of today’s macroeconometricians, is beyond me. I note some biases to which this methodology leads. The single-agent abstraction makes social welfare identical with the welfare of the individual agent. It excludes by definition any discrepancies between individual and social optima, in particular the deadweight losses due to involuntary unemployment, the market failure that motivated macroeconomics at its origins 50 years ago. The methodology treats government as an alien player in a two-person game with the anthropomorphic private sector, a game in which the government incomprehensibly tries to throw the private sector off its optimal solution while the private agent tries to outwit the evil or idiot policymaker. These biases work in a conservative and Panglossian direction.

I exaggerate. An increasing number of theoretical papers using the new methodology attempt to model setups in which things do not work out for the best and in which government may even play some beneficently corrective role. I note, however, that this role is seldom a Keynesian one, because the distortion the government can correct is seldom a failure of markets to clear. Moreover, because of the methodology those papers are, like the ones that glorify the invisible hand, logical exercises rather than models that seriously try to describe real-world economies.

Even the individual’s optimization problem is simplified and specialized in the interests of analytic tractability. Utility and production functions take parametric forms. By convention, equations are linear or log linear or are so approximated. The whole point of “micro foundations” is to find stable relationships that survive policy variations, exogenous

shocks, and the passage of time. But we have no basis for empirical confidence that an individual's utility function, for example, remains the same over her life, independently of her actual experience and environment. We certainly have no basis for assuming a utility function with a constant rate of relative risk aversion as a stable basis for both intertemporal choices and choices involving risk.

In journals, seminars, conferences, and classrooms, macroeconomic discussion has become a babble of parables. A macro theorist has become a story-teller who constructs a mythical economy in which institutions and environments conspire so that rational behavior in price-cleared markets comes up with observable outcomes that in one or two respects conform to stylized facts of the real world. In other respects, however, there is no resemblance of the mythical economy to the real world. Consequently there is a big gulf between academic macroeconomics and the macroeconomics oriented to contemporary events and policies.

Price Rigidity—The Essential Basis of Keynesian Macroeconomics?

Keynesian economics, at least old-fashioned Keynesian economics, is almost always described as dependent on *nominal price rigidity*. (The word "price" may be interpreted generically, to include nominal wage rates.) Whether the crucial rigidity characterizes labor markets or product markets or both is an interesting but secondary issue. In any case, nominal price rigidity is said to be necessary to enable monetary policies and other nominal macroeconomic shocks to affect real aggregate demand, in particular to cause real aggregate demand to deviate downward from real aggregate supply.

I could document the prevalence of this interpretation of Keynesian economics by quoting from textbooks, old and new, Keynesian and anti-Keynesian. I prefer to quote from a recent paper by three young stars of the American economics profession:

In the early 1980s, the Keynesian view of business cycles was in trouble. The problem was not new empirical evidence against Keynesian theories, but weakness in the theories

themselves. According to the Keynesian view, fluctuations in output arise largely from fluctuations in nominal aggregate demand. These changes in demand have real effects because nominal prices and wages are rigid. But in Keynesian models of the 1970s, the crucial nominal rigidities were assumed rather than explained—assumed directly, as in disequilibrium models, or introduced through theoretically arbitrary assumptions about labor contracts. Indeed it was clearly in the interests of agents to eliminate the rigidities they were assumed to create. . . . Thus the 1970s and early 1980s saw many economists turn away from Keynesian theories and toward new classical models with flexible wages and prices.¹

I quote from this paper because the authors profess sympathy for Keynesian economics and propose to overcome its theoretical flaws by deriving rigidities from “micro foundations,” that is, from rational optimizing behaviors of individuals. They style themselves “New Keynesians.”

These writers, and many others of their generation, accept the methodology of the neoclassical counterrevolution, but they are impressed by the evidence that Keynesian macroeconomics fits empirical observations better than new classical business cycle theories. After all, Keynesian economics was originally inspired by the Great Depression, for which the orthodoxies of the day had no explanations and no remedies. I believe that the depth and duration of the two most recent recessions, 1974-75 and 1979-82 (longer in Europe), have similarly helped to discredit the revival of these classical orthodoxies a half century later.

Laudable though the New Keynesians’ research program is, I shall argue that it is misguided. It is based on a misunderstanding of Keynes himself and of old Keynesian economics. The New Keynesians have accepted the terms of the debate formulated by the anti-Keynesian “new-classical” counterrevolutionaries. Both sides of the contemporary debate misrepresent and exaggerate the role of price rigidity and of nominal (as opposed to real) demand shocks in Keynesian macroeconomics.

Do Flexible Prices Fully Absorb Demand Shocks Instantaneously?

First, John Maynard Keynes, in his *General Theory*, did not postulate price rigidity, or even, money wage rigidity, in the ordinary common sense meaning of the word. It is true that some teachers and some writers of elementary textbooks drew backward L's in output/price space or employment/money-wage space. The wage or price is constant below full employment or full employment output. At those values aggregate labor and product supplies become perfectly inelastic, vertical in those diagrams. The *General Theory*, Book V, says that price will rise relative to money wage as output and employment increase, because the real wage follows marginal productivity down. (In postulating diminishing marginal productivity and countercyclicality of real wage rates, Keynes was leaning over backwards to be classical. The proposition was challenged on empirical grounds almost immediately. Keynes accepted the criticism and observed correctly that his general case was strengthened if expansion could occur without declines in real wages.) The same Book V anticipates that the money wage itself will rise as aggregate employment approaches full employment.

What is true is that Keynes and Keynesians did not expect the aggregate supply curve, plotting price against real output, to be vertical within the short run for which the Keynesian model applies. That short run they surely regarded as conditioned by the price and wage determined in previous periods.

Keynes and Keynesians used what Sir John Hicks has called the *fix-price method* as an expository device. The calculus of effective demand—spending propensities and multipliers—was a major innovative contribution of Keynesian economics, anticipating by 30 years the “disequilibrium economics” of Barro and Grossman and of the French school, Benassy, Grandmont, and Malinvaud. The variables in this calculus are real quantities, output flows and their components. It was convenient to keep effects on and of prices to one side during the exposition, and it was valid so long as prices were not completely and instantaneously clearing markets. This expository device, taken literally, doubtless contributed to the mistaken impression that absolute rigidity of prices was a necessary assumption.

The second point is more basic. The critics of Keynesian theory, friendly new Keynesians as well as hostile new classicals, take it for granted that if prices were flexible—that is, not rigid as they allege Keynes assumed—then there could be no departure at all from the real equilibrium, no departure even in the shortest run. Flexible prices would instantaneously and continuously clear all markets, for products, labor, and financial instruments. No involuntary unemployment could ever arise, no undesired excess capacity, no gap between actual and potential GNP.

The formal story is that the Walrasian Auctioneer receives all the multicommodity supply and demand schedules of the agents, including those of the monetary authority and other policymakers. These schedules refer to intertemporal as well as contemporaneous contracts and transactions. The Auctioneer, presumably using a super-computer yet to be designed and built, solves the equation system, generates the market-clearing price vector, and informs the participating agents of the transactions they have made at those prices. The next day, or the next hour, or really the next microsecond, the awesome feat is performed anew.

In this interpretation, flexibility of prices in response to shocks, and in response to news, occurs instantaneously. Prices *jump* to their new market-clearing values discontinuously, without the passage of clock time. A graph of a price time series would show discontinuities. This is certainly not what is meant by *flexibility* in common parlance. And it certainly does not take anything like *rigidity* in the common meaning of that word to believe that demand shocks will cause output to deviate, at least temporarily, from the “AS” schedule.

Anyway, if imperfect or monopolistic competition is assumed, rather than Walrasian pure competition, a Walrasian Auctioneer solution would not even exist.

Fifty years ago, no economists denied that demand shocks could at least temporarily affect output, in individual markets and in the economy at large. Keynes did not regard this possibility as problematic, and neither did his “classical” opponents. No one took the continuous competitive multimarket-clearing scenario as anything but an illustrative demonstration that in principle the system was self-consistent and solvable. As

Joseph Schumpeter, a great economist at whose feet I sat, put it, Walras's theory was the *magna charta* of economists, giving us the license to proceed in the knowledge that our quest for coherence was not a fruitless one. It was the beginning of the search, not the end. It was not then, as it seems to be now in theoretical circles, a point of reference from which any alleged departure bears the burden of proof. That is a Panglossian presumption, biasing our profession to the view that free markets are the best of all possible worlds.

Fifty years ago, and earlier, price theorists worried about *false trading*. Walras and Marshall envisaged temporary disequilibria in individual markets. Prevailing prices do not always clear the markets. They postulated dynamic rules of price adjustment (Walras) or quantity adjustment (Marshall) that would normally, but not invariably, bring supply and demand together. Stability of *general* multimarket equilibrium was especially problematic. "False trading" was recognized as a possible source of prolonged disequilibrium. Trades made at nonmarket-clearing prices change the endowments of the market participants, and thus alter their supply and demand schedules. A fashionable current term for effects of this kind is "hysteresis," a generic name for situations in which the nature of a system's equilibrium is not independent of the path the system takes when it is out of equilibrium. These problems have not been solved by later generations of theorists. They have simply been ignored, and replaced by firmer reliance on the great Auctioneer.

False trading and similar phenomena make it difficult for agents to learn the structure of the markets in which they are participating accurately enough to form rational expectations. The observations generated by disequilibria cannot teach the participants the equilibrium structure. Imagine a group of non-English-speaking foreigners from all over the world trying to learn English simply by conversing with each other.

Fifty years ago, the macroeconomic disagreement between Keynesians and classicals concerned this point. A shock occurs and takes the economy away from equilibrium. Unemployment arises, Keynesian involuntary unemployment. Would endogenous movements of prices and other macroeconomic variables return the economy to the equilibrium

from which it was jarred? Does the capitalist-market economy have reliable and quick mechanisms of adjustment?

The classical economists thought there were effective stabilizers. Keynes thought there were not. Sometimes, on some pages, he argued that there were none at all. In Book I of his *General Theory*, he envisages a whole family of equilibria, not just the classical full-employment equilibrium, but many aggregate demand equilibria with involuntary unemployment, equilibria not escaped or escapable by adjustments of prices. This indeed is the meaning of “general” in his title. Although he modifies his opening statement of his theory in later chapters, particularly chapter 19, his overall theme stands: the natural endogenous adjustment mechanisms cannot be counted on. That is why Keynes regarded macroeconomic interventions by government as essential.

The question, as Keynes saw it, was whether reductions in wages and prices would increase aggregate demand, and thus take the economy to full-employment equilibrium. His answer contained two strands. First, nominal wages would not fall rapidly in response to excess supply of labor. This strand is the one that sticks in the memory of the profession, exaggerated into assumed wage or price rigidity. Second, even if wages, and with them prices, were flexible, deflation would not increase aggregate demand and eliminate unemployment and underutilization of capital. This is the stand the profession has forgotten or neglected.

The Origins of Wage Stickiness in Keynesian Theory

I will say something about the first strand, although it is not my central topic here. It is routinely and unquestioningly supposed that Keynes attributed “money illusion” to workers. Neoclassical theorists therefore dismiss Keynesian theory out of hand. Often Keynesians accede to the charge but defend it on grounds of realism. I have come to believe that Keynes’s argument is free of the taint. And although it is not logically tight, it can be made so. Let me explain.

You will recall that Keynes’s workers were willing to accept a cut in *real* wages achieved by an increase in the price of wage goods. Yet they were not willing to take a cut in money wages. Keynes’s reason

for this asymmetry is theoretically impeccable and at the same time realistic. Workers are concerned primarily with relative wages, with how their pay compares with those to whom they regard themselves at least equal in merit. Labor markets are disaggregated and desynchronized. To any single worker or local group, a nominal wage cut appears to be a loss in relative wages; there is no assurance that others will also take cuts. On the other hand, an increase in the cost of living is the same for everybody. Workers may be perfectly prepared to receive lower real wages with unchanged relative wages, but labor market institutions give them no way to communicate this willingness.

That real wages are too high is the time-worn orthodox explanation of unemployment. If labor unions or government regulations keep them too high, unemployment is classical, not amenable to remedy by demand expansion. There is an identification problem, because the same observable symptoms are consistent with different causes. Keynes agreed that it is likely that real wages are in depressions above their full employment values. But, he argued, that is not the same thing as saying they are rigid at their high depression values. Just try expanding demand, and you will see that profit margins can be expanded and real wages reduced as necessary to make higher employment profitable to employers.

As I observed above, recovery may not require lowering of real wages. But it is still true that the way to get higher employment is to raise aggregate demand, at the same time as money wages are stuck because of concerns for relative wage parity. Those concerns do not depend on money illusion. They are certainly not irrational. They are very human, and there is a great deal of empirical evidence of their importance.

The hole in the story in the *General Theory* is that it doesn't explain how the concerns of employed workers prevail when there are unemployed workers willing to work for less pay—real, nominal, or relative. The power of insiders vis-a-vis employers and outsiders evidently derives from the costs of turnover among members of an interdependent working team. Insider power is rightly the subject of considerable theoretical and empirical inquiry right now, for example by Assar Lind-

beck and his colleagues in Stockholm. Labor economists have long observed that queues of jobseekers outside the factory gate have little effect on the wages paid to employees inside. Hard times do bring wage cuts, but generally through so damaging the financial and competitive positions of employers that they can credibly threaten layoffs of senior workers and even plant closings and bankruptcies.

Keynes did not squarely face the fact that the realistic descriptions of labor markets in his own argument were inconsistent with his assumption of pure competition in all markets. Wages are administered or negotiated prices. They are not set in impersonal auction markets. The same is true, of course, of product prices. Keynes did recognize that his theory applies to economies where the wages administered or negotiated are money wages. Things would be quite different with complete indexation.

The Weakness or Perversity of Price Effects on Aggregate Demand

The second strand in Keynes's basic argument was this: Even if money wages and prices were flexible, even if excess supplies of labor led to cuts in money wages, this flexibility would not prevent unemployment. Given a contractionary shock in aggregate demand, deflation of money wages and prices would not restore real demand to its full employment value. The classical market-clearing adjustment mechanism was, in Keynes's view, much too frail to bear the weight of macroeconomic stabilization. In fact, Keynes recommended stability rather than flexibility in money wages.

Two issues in this debate need to be distinguished. The first concerns the relation of real aggregate demand to the *price level*. The second concerns its relation to the expected *rate of change* of prices. In discussing them, I shall not distinguish between money wages and prices and their rates of change, but rather follow the assumption, conventional in this debate, that they move together. I remind you that the theoretical argument refers to a closed economy. You could think of the United States in years gone by, or of post-1992 Europe, or of the whole OECD area.

Keynes in Book I denied that real aggregate demand was related at all to the price and money wage level. In effect he turned the classical

neutrality proposition against the classicals. If all money wages and prices are lowered in the same proportion, how can real quantities demanded be any different? Thus if a real shock makes real demand deficient, how can a purely nominal price adjustment undo the damage? Actually, Keynes himself provided an answer in chapter 19. If the nominal quantity of money remains the same, its real quantity increases, interest rates fall, and real demand increases. This scenario is often called the “Keynes effect.” This mechanism would fail if demand for money became perfectly elastic with respect to interest rates—the famous liquidity trap—or if demand for goods for consumption and investment were perfectly inelastic.

Pigou and other authors provided another scenario, the “Pigou effect” or “real balance effect,” which alleges a direct effect of increased wealth, in the case at hand taking the form of the increased real value of base money, on real consumption demand (possibly also on investment demand). This does not depend on reduction of interest rates.

The theoretical fraternity has taken the Pigou effect as a decisive refutation of Keynes’s claim to have found underemployment equilibria. As long as involuntary unemployment and excess capacity push wages and prices down, there will be an equilibrium when and only when they reach so low a level that monetary wealth is so great that aggregate demand creates jobs for all willing workers.

The Pigou effect is of dubious strength, and even of uncertain sign. Most nominal assets in a modern economy are “inside” assets, that is, the debts of private agents to other private agents. They wash out in accounting aggregation, leaving only the government’s nominal debt to the private sector as new wealth. Some, if not all, of that debt is internalized by taxpayers. The base of the real balance effect is therefore quite small relative to the economy—in the United States the monetary base is only 6 percent of GNP.

That inside assets and debts wash out in accounting aggregation does not mean that the effects of price changes on their real value wash out. Price declines make creditors better off and debtors poorer. Their marginal propensities to spend from wealth need not be the same. Common sense suggests that debtors would have the higher spending propensities; that is why they are in debt. Such a differential could easily

swamp the Pigou effect. We're talking about gross amounts of 200 percent of GNP. I like to call this reverse Pigou effect a Fisher effect, because Irving Fisher emphasized the increased burden of debt resulting from (unanticipated) deflation as a major factor in depressions in general and in the Great Depression in particular. It is quite possible that this Fisher effect is stronger than the Pigou and Keynes effects combined, particularly when output and employment are low relative to capacity.²

The argument I have just made refers to *levels* of nominal wages and prices. An even more important argument refers to *rates of change*. The Keynes and Pigou effects compare high prices and low as if they were timeless alternatives, without worrying about the process of change from high to low in real time. Economists of the day argued in this way quite consciously, as required by the rules of the comparative statics game they were playing. The process of change works on aggregate demand in just the wrong direction. Greater expected deflation, or expected disinflation, is an increase in the real rate of interest, necessarily so when nominal interest rates are constrained by the zero floor of the interest on money. Here is another Fisher effect, another factor Fisher stressed in explanation of the Great Depression. Keynes stressed it too, as a pragmatic dynamic reinforcement of the lesson of his static general theory.

He was right to do so. In a 1975 article³ I exhibited a simple macroeconomic system, classical in the sense that it has only one equilibrium, characterized by full employment, indeed by a "natural" rate of unemployment. Given the monetary base, the price level is stable in that equilibrium. The dynamic stability of the system depends on the relative strengths of the real balance effect and the real interest effect. If the real interest effect dominates, as it well may if the real balance effect is weak and certainly will if the Fisher debt burden effect prevails, then the equilibrium is unstable. Moreover, the system could be stable locally but unstable for large displacements.

I regarded my article as supporting Keynes's intuition that price and wage flexibility are bad for real stability. I wanted to shake the profession off its conventional interpretation of Keynesian economics, according to which unemployment arises only because of a dubious asser-

tion of wage and price rigidity. I wanted to recall and reinforce the second strand of Keynes's argument, according to which unemployment is attributable to inadequate real demand, a deficiency that flexibility will not remedy. That is also what I am hoping to do here.

I am quite willing to subscribe to a meaning of *equilibrium* that excludes involuntary unemployment, and to characterize depressions as disequilibria. Either way, the Keynesian diagnosis and prescription are the same in practice.

Recently, at long last, the question whether price flexibility (in any sense short of the Walrasian Auctioneer fairy tale) is stabilizing has begun to receive serious attention. DeLong and Summers⁴ have investigated it in the Taylor staggered-contract model, amended to allow price-level and price-change effects on demand. The Taylor model results in unemployment when there are new circumstances and information, because wages and prices cannot be immediately adjusted to them. It also allows Keynesian policies to work temporarily, because the authorities can react to new circumstances and information before existing contracts are renegotiated.

DeLong and Summers simulate increased flexibility by making the periods in the staggered-contract model shorter. They find that increased flexibility in this sense frequently does make real outcomes, employment and output, more volatile, not less. The reason is the same as in my model, the Fisher real interest rate effect of inflation and deflation. Their most interesting simulation has the intuitively desirable result that in the limit perfect price flexibility—instantaneous jumps of the Walrasian solution in response to shocks—does stabilize real variables perfectly. Close to this limit, greater price flexibility means greater real stability, but farther away from it, the reverse is true.

Nominal and Real Demand Shocks

I began by calling your attention to the caricature of the Keynesian theory of business fluctuations all too generally accepted in the profession. According to that caricature, fluctuations in real output and employment arise from shocks to nominal aggregate demand, which become real shocks only because prices are rigid. Tides ebb and flow; they matter to boats only because they pass over rocks.

Keynesian theory of business fluctuations stresses shocks to real aggregate demand—investment, consumption, or government purchases. Some impulses may indeed come from the monetary side, but that does not make them purely nominal. A monetary policy action that lowers nominal interest rates also lowers real rates and affects investment demand. Likewise, a shift in production functions that raises the marginal productivity of capital stimulates investment and diminishes the demand for money at the same time. The world is not constructed in the dichotomous way assumed in the common classification of shocks as either nominal or real.

The great achievement of the *General Theory* is the theory of effective demand. Keynes's insight was that demand is constrained by amounts actually sold in markets, which may frequently be less than the amounts agents would like to sell at existing prices. This was a deeper insight than the assertion that nominal wages and prices are "rigid." I commend it to the New Keynesians as a more fruitful and important line of inquiry than the macroeconomic role of the real costs of changing nominal prices on menus, price lists, and catalogs.

NOTES

1. L. Ball, N. G. Mankiw, and D. Romer, "The New Keynesian Economics and the Output-Inflation Trade-off," *Brookings Papers on Economic Activity* 1988, 1, pp 1-2
2. I have examined the macroeconomic consequences of a dominant Fisher effect, in an IS-LM model that also has a Keynes effect, in my *Asset Accumulation and Economic Activity*, Oxford, Blackwell 1980, chapter 1.
3. J. Tobin, "Keynesian Models of Recession and Depression," *American Economic Review* 65 (May 1975), 195-202.
4. J. B. DeLong and L. H. Summers, "Is Increased Price Flexibility Stabilizing?" *American Economic Review* 76 (December 1986), 1031-44.