

What They Were Thinking Then: The Consequences for Macroeconomics during the Past 60 Years

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When I started graduate school in 1962 at the Massachusetts Institute of Technology, a particular version of Keynesian economics was ascendant, both within the field of economics and in the broader society. Just three years later (in December 1965), *Time* magazine put Keynes on the cover, even including a presumed endorsement from Milton Friedman: “[W]e are all Keynesians now.” (More accurately, as Friedman [1966] quickly protested, he had offered only the Delphic pronouncement: “In one sense, we are all Keynesians now; in another, nobody is any longer a Keynesian.”) *Time* was the country’s most influential news-magazine at the time. Its embrace of Keynes was especially noteworthy because the magazine had always emphatically supported “the free enterprise system,” whereas Keynes and his disciples had long been accused of socialist leanings. At Harvard, for example, Alvin Hansen and his fellow Keynesians had been attacked by an alumni association called Veritas, for their alleged communism (Skousen 1992, 21; Dobbs 1960). With its cover-story endorsement of Keynes, *Time* about-faced on Keynes, while still sticking to its guns regarding free enterprise; now that aggregate demand management freed the economy from busts, free enterprise could be further empowered to provide a new era of prosperity. It appeared that the Keynesians had obtained a complete win.

The primary public policy lesson of Keynesian economics—that we now knew how to respond to economic downturns—had been a hard-won fight. It had been fought for decades, with high stakes: nothing less than the maintenance of full employment, rather than lapses into Great Depression. As I was beginning graduate

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school, Cambridge, Massachusetts, under the leadership of MIT's Paul Samuelson, had taken over from Cambridge, England, as the center for Keynesian economics. The adoption of this message was also extremely personal to me: its propagation was then, at age 22, my self-assigned life's mission. It continues to this very day: 57 years later.

This article begins with a review of the two main textbook approaches that had evolved by the early 1960s to incorporate the musings of Keynes: the Keynesian cross from Samuelson's (1948) introductory textbook and the complete, well fleshed-out model in Gardner Ackley's (1961) advanced macro textbook. This Keynesian-neoclassical synthesis followed a pattern set by Hicks (1937) by focusing on certain elements of Keynes, while setting aside others. Some potential weaknesses of the specific approach in these models were, at least vaguely, sensed at the time. For example, Hicks had, at least obliquely, mentioned the neglect of inflation expectations. In other cases, the model left out topics that Keynes had treated as important, such as the dangers of financial crises and the role of social norms in wage bargaining, and what these topics implied about the potential importance of multiple equilibria in macroeconomic outcomes. However, the Keynesian-neoclassical synthesis of the 1960s was flexible enough that it encouraged a large body of work. The article will show that this work was based on a style that I call "one-deviation-at-a-time" (a phrase adapted from Caballero 2010). As I will demonstrate, one-deviation-at-a-time constraints have had real consequences for macroeconomics. For example, they have resulted in lack of attention to financial crashes as a macro topic; they have also resulted in the omission of plausible models with very different core conclusions regarding the effectiveness of macro stabilization.

My concerns can be expressed in the terminology of Thomas Kuhn (1962). What was the dominant paradigm for macroeconomics in the early 1960s? What were its vulnerabilities? What was the resistance to addressing these vulnerabilities? Do these vulnerabilities still remain? I shall address these questions regarding the field of macroeconomics from two intertwined perspectives: my perception of what they were thinking as I began graduate school at MIT in 1962, and my view as I look back on the developments in macroeconomics over the past 57 years.

Early 1960s: Macro at MIT and Acceptance of Keynes

Kuhn (1962) also tells us that the textbooks of a scientific field are fertile hunting ground for its paradigms. Macro at the MIT of 1962 was a tale of two textbooks. At the time, Paul Samuelson of MIT was the world's most famous living economist, known for his *Foundations of Economic Analysis* (Samuelson 1947) and for his numerous articles (Samuelson 1966), but especially for his bestselling introductory textbook (Samuelson 1948). Its early editions began with macroeconomics based on the keystone "Keynesian cross" diagram of Samuelson's invention (Pearce and Hoover 1995), which was the uncontested heart of macroeconomics at MIT. (As a reminder, the Keynesian cross plots income on the horizontal axis and

expenditures on the vertical axis. Equilibrium occurs where income and expenditures are equal—along a 45-degree line from the origin—but this equilibrium could occur either as a “deflationary gap” below full employment, or at full employment, or as an “inflationary gap” above full employment.) The analysis behind the figure explored the consequences of observing that, as Keynes (1936) had claimed, equilibrium income occurs where desired savings equals desired investment. Any difference between desired savings and desired investment would represent a gap between production and sales, which, in turn, is undesired inventory accumulation. Production then becomes a natural equilibrator: producers with unintended inventory *accumulations* will decrease production; those with unintended *decumulations* will increase production.

The policy response implicit within the model was that fiscal policy could be used to overcome a situation in which a high level of desired savings was leading to a macroeconomic equilibrium below full employment. Keynes had solved this problem by invoking income—a variable other than prices—as the major macroeconomic equilibrator. Thus, that $C + I + G$ “Keynesian cross” was the key response to doubts regarding the possibility of an unemployment equilibrium. At the same time, the model showed that increases in government spending, or decreases in taxes, could be expansionary. The great public policy question of the day—how to fight underemployment—had thus also been solved. This message, significantly homegrown at MIT, was revolutionary relative to the thinking of the early 1930s, when economists could reach no clear consensus regarding how to restore full employment.

While Samuelson’s Keynesian cross diagram described the basic determinants of equilibrium income, economists had also felt it necessary to fill out the model so as also to include the determination of the aggregate price level, asset prices (as inverse to the interest rate), and wages. The determination of all three was in the standard model of the time, as well described in a second textbook, Ackley’s (1961) *Macroeconomic Theory*.¹ The model was rooted in an aggregate-demand/aggregate-supply equilibrium. Aggregate demand was determined by the Keynesian IS-LM equilibrium. Aggregate demand had a downward slope with respect to price, because with a given money supply, lower prices would accommodate both higher real transactions and higher real “speculative demand.” On the other side, aggregate supply had an upward slope with respect to the price level, because with a fixed money wage, at higher prices, competitive firms would find it profitable to put more labor to work. Equilibrium in the economy occurred at the price level and national income at which aggregate demand equaled aggregate supply. Wages were fixed at any moment in time, but they responded to aggregate demand according to a Phillips curve relationship: the lower the unemployment rate, the higher was nominal-wage growth.

¹ Ackley’s primary academic affiliation during his career was the University of Michigan, but in the 1960s, his textbook took on added prominence from his role as a member of the Council of Economic Advisers from 1962 to 1964, and then as chair from 1964 to 1968.

By the time I came to MIT, this “model,” which Samuelson called “the Keynesian-neoclassical synthesis” (Blanchard 1991), had become accepted wisdom. For MIT graduate students at this time, the state of macroeconomics conveyed to us was that the issue of macro stabilization had been resolved; it was no longer a desirable topic for theoretical research. Indeed, the first-term macro PhD class safely could be relegated to a faculty member outside of the field of macroeconomics, with strong reliance on the Ackley textbook.

Given that the adaptation of Keynes to include neoclassical supply had resolved the determination of the price level and asset prices (as inverse to the interest rate), it only remained to turn the model over to the econometricians. In the 1960s, their models would be applied to the task of “getting America moving again” after the high unemployment of the late 1950s. Samuelson and friends would be advisors to the new Kennedy administration. It was further understood among us graduate students that the ongoing research in business cycle macroeconomics was beyond our pay grade. At MIT, Albert Ando and Edwin Kuh were contributing to the large-scale Brookings model of the US economy (Duesenberry et al. 1965), but the collaborative nature of this research with high-level contributors from other universities and from Brookings posed a hurdle too high for us.

Instead, MIT graduate students with a macroeconomics bent flocked into growth theory: pushed by the presumed end of straight macro as researchable, while also being pulled by Robert Solow’s recent research. That especially included the role of the “residual,” rather than capital accumulation, as the main driver of productivity growth (Solow 1957).²

But proclamations of the end of macroeconomics were premature. As we would learn, that standard model failed to answer some fundamental questions.

The Keynesian-Neoclassical Synthesis and the Duty to Defend It

In summary, here’s what they were thinking about macroeconomic stabilization and business cycles at MIT in the early 1960s. A hard and important problem, where a lack of understanding had led to the long continuation of the Great Depression, was now solved. In the form of the Keynesian-neoclassical synthesis, it offered a solution that could also inform policy even after the economy had recovered. Questioning the model then would be dangerous: it might divert the public, already misguided by their misunderstandings of economics, away from the expansionary fiscal policy needed to hold unemployment at bay.

I once saw this implicit, but rarely expressed, message stated openly. It took place in a seminar on money and banking, taught by Samuelson in spring 1964. His idiosyncratic teaching style was to ramble through class with his reflections of the

²It also included his modeling of capital of different quality: that capital of later vintage would be the carrier of economic growth, and that the usage of capital would be flexible at the time of construction (putty), but inflexible afterward (hard-baked clay) (Solow 1962a, b).

moment; at the end, usually running into overtime, he would give his take on the syllabus topic for the day. In one of his class-time ramblings, Samuelson told us of a special problem regarding the Phillips curve.

An article by him and Solow had plotted the rate of change of hourly earnings and unemployment by year for the US economy. They said that this plot gave the policy trade-off between inflation and unemployment. Zero inflation would entail about 5.5 percent unemployment; 4.5 percent inflation would result in unemployment of 3 percent (Samuelson and Solow 1960, 192–93). The heart of US aggregate macro policy was to choose the optimal trade-off between unemployment and inflation over this range.

But Samuelson alerted the class to a potential problem with this conventional wisdom. Maybe, he said—just maybe—with high levels of employment, inflationary wage and price changes that would occur would feed back into higher inflationary expectations. And, if those higher expectations themselves were added onto the wage (and price) changes, inflation would accelerate. Thus, the trade-off would not be between unemployment and a constant *level* of inflation; it would be between unemployment and the *acceleration* of inflation.

Samuelson revealed his own take on this proposition. Belief in it would result in contractionary policies: aimed at keeping inflation low, thereby resulting in high unemployment. Belief in this accelerationist theory, if it was erroneous, would then have a high cost. The decreased inflation would, mainly, just relieve a nuisance (who *really* cares very much if we pay high prices to ourselves?);³ however, the resultant increases in unemployment would put people out of work, and production would be lower. Asymmetrically, if policymakers did not believe in the accelerationist theory—yet it turned out to be correct—the cost of that error would be slight. The higher inflation that resulted from the erroneous belief would cause only small losses in welfare (we were paying those high prices to ourselves), and furthermore, the loss from the high unemployment when inflation had to be prodded back to normal would be largely offset by the more than full employment from the times when inflation had been on the rise.

Then it happened. Just three and a half years later, Samuelson's chickens came home to roost. Milton Friedman (1968) would make this accelerationist argument the theme of his American Economic Association presidential address. Together with the stagflation that occurred shortly thereafter, this address exploded the field of macroeconomics. Later, Samuelson would publicly confess that he had lost sleep over this possibility. Here in the *Journal of Economic Perspectives*, Samuelson (1997, 156) wrote: "In Camelot [that is, Kennedy administration] counsels I was at first too pessimistic about the possibility of stagflation ahead. . . . Alas, by 1965 and for 15 years, my fears proved all too prescient."

³Of course, this is not taking into account Friedman's (1969) concerns that inflation would act like a tax on the use of money, with consequent deadweight losses in welfare. Rightly or wrongly, those losses were viewed as small relative to the reduction in welfare from higher unemployment.

As I look back on it now, I see that in that class-time musing, Samuelson had entrusted us students with the major unspoken secret of the Temple of Keynesian Macroeconomics. He had also revealed the ill consequences that would occur if that secret became known. No graduate student at MIT took on the accelerationist view of the Phillips curve as a research topic. For us students in the Temple, that would have been unthinkable: it was our moral duty to keep the secret entrusted to us. So on to growth theory, where little damage could be done, for example, by proofs that there was no steady state for an economy without Cobb–Douglas production functions and without labor-augmenting technical change, which was the subject of my first (joint) paper (G. Akerlof and Nordhaus 1967). But that turn to growth theory was an unfortunate choice, because the Keynesian-neoclassical synthesis was not the end of macroeconomics. Much fundamental work remained to be done.

The seeds of the macro of the 1960s, with its sins of omission, can even be seen as early as the immediate, Big Bang aftermath of the publication of *The General Theory* (Keynes 1936). In his famous review, “Mr. Keynes and the ‘Classics,’” John Hicks (1937) introduced the IS-LM analysis that would set the tone for the incorporation of Keynes into mainstream economics. The overtone of that essay is the concept of IS-LM analysis, which Hicks called his “little apparatus.” But “Mr. Keynes and the ‘Classics’” also had an undertone: that Keynes had overclaimed the originality and generality of his work. Hicks’s second sentence says that Keynes had written a “*Dunciad*”—that is, Keynes had called his fellow economists “dunces.” Hicks (1937, 147) said that those fellow economists were “bewildered,” because they had not understood that they ascribed to the beliefs attributed to them in *The General Theory*. For example, while Keynes claimed that he had disproved Say’s law, he offered no example of any contemporary economist who was such a dunce as to have this belief. Even so, Keynes was correct, Hicks argued, because Say’s law was implicit in the standard economic theory of the time—that is, as long as all economic equilibria were attained with only prices as the equilibrators of supply and demand, an underemployment equilibrium was impossible. Thus, Hicks (1937) is effectively claiming that the dunces of the economics profession were only in disagreement with Keynes insofar as they had not yet seen the special cases described by his “little apparatus” of IS-LM. His rescue mission would empower those maligned economics professor dunces to take the IS-LM jewel home, as a useful ornament to their classical macro models.

The addition of classical supply of output to the model had further advantages from a research perspective. It could be easily nested within a general equilibrium model. Such models are, by nature, fractal—in the sense that they keep the same form, irrespective of the number of markets.⁴ This fractal form allowed the econometricians—in due course with their computers—to go to town. The Keynesian-neoclassical model was easily expandable to many-equation, many-sector models of the economy. Furthermore, this model could be seasoned with bells

⁴This meaning of “fractal” is from <http://mathworld.wolfram.com/fractal.html>.

and whistles, such as monopolistic competition, monopsonistic labor supply, and staggered contracts, according to taste. Time-series models such as ARMA (autoregressive moving average) or ARIMA (autoregressive integrated moving average) models were waiting in the wings to add elegant dynamics that appeared to have considerable generality. As a further merit, this model also had a unique equilibrium, so that its comparative statics was also unique.

The range of possible extensions of the Keynesian-neoclassical synthesis has then encouraged a research agenda of “one deviation at a time” and looking at how such deviations shifted the equilibrium. At the same time, it has also discouraged non-neoclassical thinking, which would require more fundamental changes to the model.

Financial Crashes and the Economy

In the push to assure acceptance of the dominant-paradigm Keynesian-neoclassical synthesis, a major macroeconomic question was left unresolved: what is the basic reason for very hard times that especially require fiscal or monetary stimulus? Keynesians such as Hicks (1950), Kaldor (1940), and Samuelson (1939) offered answers to that question—based on small deviations from the standard framework—with dynamic multiplier-accelerator models of investment and savings.

But in chapter 12 of *The General Theory*, on “the state of long-term expectation,” Keynes (1936) had suggested a very different analysis of boom and bust: those bad times result largely from financial fragility. Famously, in that chapter, Keynes (1936, 140) analogized stock prices to the outcomes of a newspaper beauty competition. Entrants to that contest would choose the most attractive faces from a batch of photographs—with prizes awarded not to those who picked the most attractive by some external standard, but instead to those who chose the faces picked by the greatest number of *other* contestants. The outcomes of such a contest did not necessarily depend on the “fundamentals”—that is, individual views regarding the faces. Instead, contestants would be trying to deduce what other contestants would be taking into account about what others were also thinking.

Again, we can look to Hicks (1937) in “Mr. Keynes and the ‘Classics’” as the harbinger regarding how mainstream economics would handle these verbal musings in *The General Theory*. The first sentence of that article says: “IT WILL BE ADMITTED [capitalization in the original] by the least charitable reader that the entertainment value of Mr. Keynes’ *General Theory of Employment* is considerably enhanced by its satiric aspect.” Those opening words were not just an odd aside: on the contrary, Hicks’s major purpose was the rescue of IS-LM from *The General Theory*’s mess-heap of “satiric entertainment.” That first sentence was thus a dog-whistle warning sign against paying much attention to those ruminations from Keynes on such topics as beauty contests.

But Keynes (1936) had included the beauty contest metaphor for a very good reason, which would differentiate his original approach from the neoclassical

synthesis versions of the Keynesian model that emerged. The realm of possible (Nash) equilibria in the beauty contest is truly remarkable. Any “point” in that beauty contest—that is, any one of the faces—could be an equilibrium. The equilibrium that was reached would not just depend upon fundamentals regarding which was the “prettiest” (Keynes 1936, 140) face. *Every one* of the faces could be a (Nash) equilibrium. It all depended on how others thought that others would vote.

In concert with modern game theory, finance theorists have evolved a sophisticated understanding of why Keynes’s beauty contest is central to the theory of financial crash.⁵ A very simple game (adapted from Atkeson 2001)⁶ describes the skeleton of such models. In this game players have two choices: to continue to hold an asset or to sell it. With economic conditions sufficiently strong, the incentives to hold may be so strong that it pays to do so, even if everyone else sells. In this case, there is only one equilibrium: everyone holds, and the price of the asset remains high. With economic conditions at the opposite extreme, the incentives to hold may be so weak that it pays to sell, even if everyone else keeps it. Again, there is only one equilibrium: but this time everyone sells. But between these two extremes, in such a game, there is the possibility of an intermediate range with a threshold level of holders. If the number of holders exceeds the threshold, it pays to hold; if that number is below the threshold, it pays to sell. If such a model describes asset markets, financial equilibria are likely to be fragile for two reasons. First, within the intermediate region, there may be chain-reaction changes—from above the threshold to below—in people’s expectations regarding whether others will hold or sell. Second, the economic environment may change, which drives the model from the safe, always-hold region into the fragile region of sometimes hold/sometimes sell (depending on the threshold).

Three examples illustrate the centrality of the preceding skeleton for leading models of financial crash: bank runs, fire sales with collateralized loans, and currency speculations. In a *bank run* model (like that of Diamond and Dybvig 1983), if only the usual transactors are making withdrawals, there is not much reason to line up at the bank. But if others are lining up out of fears of its insolvency, there is reason to rush to be among those first in line to retrieve one’s deposit. In models of *fire sale crashes with collateralized loans* (Shleifer and Vishny 1997, 2011),⁷ short-term lenders hold collateral from leveraged borrowers. Absent a decline in the value of the collateral, the borrowers do not have special need to sell it. But once such declines have begun, forced sales can trigger a vicious cycle of further declines in asset values and further forced sales. In models of *currency speculation* (as in Morris and Shin 1998),

⁵These models especially involve the role of credit, whose role in the business cycle had been earlier emphasized by Minsky (1975). Credit often involves the following feature: if I give credit to a firm, I am better off (safer) if you also give it credit, and vice versa. As explained further below, this feature of credit plays a major role in financial crash, since it can engender chain reactions both in the buildup and then in the withdrawal of credit.

⁶Atkeson, in turn, is commenting on Morris and Shin (2001).

⁷In exactly the same spirit, but with rather different modeling, Kiyotaki and Moore (1997) have shown how debt and collateral can amplify the effects of productivity shocks.

the payoff to withdrawal into a foreign currency is negative if other speculators do not make that move. However, the payoff to such withdrawal to a foreign currency is likely quite high if a large number of speculators also withdraw.

Each of these three examples of financial crash shares a crucial feature: those who continue to hold a position, or to withdraw from that position, are positively related to the numbers of those who take the same action. But such representation of financial markets had no substantial place in mainstream macroeconomics in the period leading up to the Great Recession. In the aftermath of the 2008 crash, Ricardo Caballero (2010, in this journal) explained why macroeconomists had not predicted it. He said that macroeconomists had a modeling aesthetic based variously on either dynamic stochastic general equilibrium or neo-Keynesian models, and while researchers permitted themselves to depart from such baseline models, they allowed themselves only one deviation at a time. I agree.

But as the preceding description shows, standard models of financial crash differ fundamentally from the standard macro models. Equilibria of those “neoclassical synthesis” models are stable at the point where aggregate demand equals aggregate supply. A small (ε) shift in aggregate demand, or in aggregate supply, produces a small (proportional to ε) shift in the equilibrium. On the contrary, again following Atkeson (2001), the skeleton crash models have the opposite behavior in the region of interest (that is, in the region where the crash occurs). In that region, a small decrease in the number of those who hold their financial assets, or a small increase in the number of those who sell (or perhaps just wish to sell) their financial assets, can produce a crash. These two types of model, then, typically do not fit well together. On the one side, the neoclassical models tend to have unique equilibria. On the other side, multiple equilibria are natural in beauty contest models. Keynes’s example is especially stark, as any one of the “faces”—that is any “point” in the competition—could be an equilibrium. The two types of model thus do not easily nest each other.

Between modern macroeconomic models and the models of financial crash, the incentive structures are also correspondingly different. In dynamic stochastic general equilibrium or neo-Keynesian models, I want you to have the *opposite* behavior from myself. If there is an additional supplier, I am more likely to make a purchase because I can buy at a lower price; correspondingly, if there are additional demanders, I am more likely to become a supplier, because I can sell at a higher price. But in contrast, in the models of financial crash, additional sellers of assets cause not additional buyers but rather additional sellers. Figuratively, they too join the line to withdraw their money at the bank; they too make a fire sale of their collateral; or they too dump their currency to take advantage of the devaluation that is increasingly likely as the number of others who are dumping increases. It is thus difficult, if not impossible, to produce an aesthetically pleasing model that combines the two types of equilibrium. Such a model would be the equivalent of chicken ice cream.

This takes us back to *what they were thinking* in that primordial time when *The General Theory* was being adopted as the core of macroeconomics, which is also the

time when major mechanisms for describing financial crashes were removed from macroeconomists' deck of cards. The macroeconomics of financial crash was sent off for adoption to the field of corporate finance. There it languished, as a mostly ignored stepchild. Thus, Jean Tirole (2010) included this topic in his hefty summary of *Corporate Finance*—but only in the grab-bag last section, on “Macroeconomics and the Political Economy of Corporate Finance” (469).

After the banking reforms of the 1930s, such a division of labor between macroeconomics and finance may have been reasonable, as long as banks dominated the financial system. In this case, the major threats to financial stability would come from bank runs—and those bank runs had been rendered unlikely by the advent of deposit insurance. If depositors suspected their bank might be insolvent, deposit insurance greatly reduced their incentives to be first in line to make their withdrawals. Perhaps equally important, the supervision of banks that protected the Federal Deposit Insurance Corporation offered a further bulwark against bank runs, because it greatly reduced the chances of an insolvency.⁸ In this context, little damage could be done by macro models lacking the details of the financial system.

But exclusion of such detail (with the attendant possibility of financial crash) from standard macroeconomics *could* be a problem in a different context: if the financial system changed in fundamental ways. That was exactly the topic of Rajan's (2005) Jackson Hole talk, “Has Financial Development Made the World Riskier?” which *did* predict the crash of 2008 as it actually happened. In terms of the skeletal model, had that “financial development” beyond a well-supervised banking system with deposit insurance driven the financial system out of the safe region of always-hold? In September 2008, the answer to Rajan's question became clear: “yes, it had.”

This is a case where the path taken by macroeconomics after the publication of *The General Theory*, from the Hicks (1937) review up through the 1960s, turned out to have serious consequences. For example, the discussions about the costs and benefits regarding financial deregulation in the 1990s gave far-from-sufficient weight to the macroeconomic risks of financial crash.

Limited Perspective on the Role of Inflationary Expectations

This article has already touched on the vulnerability of the Keynesian-neoclassical synthesis to considerations of inflationary expectations. Following Friedman (1968) and also Phelps (1967, 1968), it quickly became conventional wisdom that the only “theoretically correct” representations of wage bargains added inflationary expectations one-to-one into wage adjustments.⁹ In that case, monetary policy could, at most, stabilize the economy around a single unemployment rate, the NAIRU (the

⁸Additionally, the Federal Reserve had powers to make loans to banks (and even nonbanks) under pressure, as long as they could provide sufficient collateral.

⁹In algebraic terms, Phillips curves would *only* be of the form $\dot{w}/w = f(u) + \pi^e$, where \dot{w}/w is the rate of change of wages, u is the unemployment rate, and π^e is the expected rate of inflation.

nonaccelerating inflation rate of unemployment). A few years later, Lucas (1972, 1973) and Sargent (1973) would go yet further: if those inflationary expectations, in addition, were formed according to rational expectations, systematic monetary policies could not even be used for stabilization, because expected changes in the money supply would be exactly offset by equal percentage changes in prices. With the removal of some of the extreme assumptions in the Lucas and Sargent models, New Keynesian models with just a bit of nominal rigidity—for example, with staggered nominal-wage (or nominal-price) setting—monetary policy could be stabilizing, but it still would have at most very small effect (mathematically second-order) on average employment over a business cycle.¹⁰

These results again show the vulnerability of the early 1960s version of the Keynesian-neoclassical synthesis. According to that *Time* cover story back in 1965, aggregate demand management permanently raised average employment by selectively ironing out business cycle downturns. By the late 1970s and after, there was a question: how much bathwater had been drained from the Keynesian tub?

But that takes us again to Caballero's (2010) question: did macroeconomists' one-deviation-at-a-time surrender too much, this time in regard to accelerationist modeling? Were they too quick to accede to Friedman's (1968) argument that Phillips curves would automatically be augmented one-for-one by inflationary expectations? A paper by Robert Akerlof (2016) provides the basis for revisiting this question;¹¹ it gives a general explanation of why dual equilibria are common in the enforcement of a norm. In one of the two dual equilibria, the norm is generally obeyed, and it is enforced. Why is it enforced in this equilibrium? With most people following the norm, as they do *in this equilibrium*, a violation would identify a violator as an unusual, "unreasonable," "bad" person, and so the violator would be punished. The prospect of such punishment enforces the norm. In contrast, in the other equilibrium, the norm is not obeyed, and it is not enforced. Why is it not enforced? Because most people *in this other equilibrium* violate the norm, a violator is not identified as an exceptional, "unreasonable" person. Since most other people behave in the exact same way, the violation will be excused. Thus, the norm will not be enforced.

The preceding line of argument can be adapted to show why there are also likely to be dual equilibria regarding cost-of-living adjustments (COLAs) in the labor market. In one of the two equilibria, most employers are giving COLAs. Employees who are taking cues from workers' treatment in other firms will then have special reason to be aggrieved if their employer does not give them a COLA: not only has their employer denied them something they deserve, the employer is also revealed to be "unreasonable." In this equilibrium, employers have a special incentive to

¹⁰ According to Lucas (2003), the gains in average employment from stabilization in such New Keynesian models are mathematically second-order. His simulations, in which the gains in employment from stabilization were economically insignificant in size, bore out the relevance of that observation.

¹¹ Anderlini and Terlizzese (2017) have also, rather similarly, described dual equilibria, regarding levels of trust.

grant a COLA: they will want to prevent the morale consequences from appearing “unreasonable.” In contrast, in another equilibrium, in which COLAs are granted by very few firms, employers will not have the preceding incentive to grant COLAs. Why not? Employees, seeing that most other firms are not giving COLAs, will be likely to excuse a similar denial by their own employer. After all, in this case, their employer, who is just behaving like most other employers, does not stand out as “unreasonable.”

It turns out that considerable evidence accords with predictions of the dual-equilibrium model. Especially, there seem to be times when cost-of-living adjustments are generally granted (typically when inflation is high), and times when COLAs are generally denied (typically when inflation is low). As a first bit of evidence, as inflation increased in the United States over the 1960s and 1970s, the fraction of union workers covered by contracts with formal COLAs rose from approximately 22 percent in 1966 to 61 percent in 1976; then, as inflation receded, with the recession of 1981–1982, this fraction declined rapidly, back to 22 percent in 1995 (Ragan and Bratsberg 2000, 304, 306 [fig. 1]). This variation in COLA contracts contrasts with the prediction of the standard accelerationist models, in which COLAs are uniformly one-to-one with both high and low inflation.

A paper by me, William Dickens, and George Perry (G. Akerlof, Dickens, and Perry 2000) offers a second bit of evidence of times with general grant of cost-of-living adjustments and also times of general denial. Across many specifications of regressions of wages against unemployment and lagged inflation, the average sum of the coefficients on lagged inflation was 0.25 when inflation was low, which contrasts with an average sum of 0.82 when inflation was high (23). The low-inflation samples were for quarters when average inflation had been less than 3 percent in the previous five years; the high-inflation samples, for quarters when inflation had been in excess of 4 percent (22).¹²

The theory also offers a possible explanation for the direction of switches in equilibrium between times of high and low inflation.¹³ For example, a switch from a no-cost-of-living-adjustment equilibrium to a COLA equilibrium will occur if, no matter what other employers do, workers’ anger at their own employers’ failure to give COLAs is sufficient to enforce the norm. It is plausible such a level of anger will occur if inflation is sufficiently high and if unemployment is sufficiently low. The direct cost to employees of denial of a COLA increases proportionately as inflation rises,¹⁴ and workers’ anger is also more likely to be stoked the lower is

¹²Price changes regressed likewise on unemployment and lagged inflation show similar but less dramatic differences: in this case, the average sum of coefficients on lagged inflation was 0.60 at low inflation and 0.95 at high inflation (G. Akerlof, Dickens, and Perry 2000, 23). Results are similar when the inflation variable is survey-reported inflation expectations.

¹³In work in progress, Jeffrey Butler and I are building a model with this property.

¹⁴So too the gain to employers from denial of the COLA increases proportionately with inflation. But, quite plausibly, the employees’ anger, with the consequent costs they inflict on their employers, increases more with inflation than these gains to the employers. If inflation is very low, the employees are likely to ignore denial of a COLA, but as inflation increases, their anger (and consequent punishment of their

unemployment (so that their employer is doing them less of a favor by giving them a job). A similar argument explains why the equilibrium will switch in the opposite direction if inflation is sufficiently low and unemployment is sufficiently high.

Furthermore, there is an episode in US macroeconomic history when economic policy seemingly engineered a switch from an equilibrium with cost-of-living adjustments to an equilibrium without them. But it took a recession with very tight monetary policy and with very high unemployment to accomplish the change. Throughout the 1960s and 1970s, inflation escalated, as the Federal Reserve's employment targets were unrealistically high. Then, finally, in the early 1980s, Federal Reserve Chair Paul Volcker decided that enough was enough (Orphanides and Williams 2013). But the reversion to low inflation did not occur easily. In the recession of 1981–1982 that ensued, unemployment rose to 10.6 percent, and the grant of COLAs, as we have seen in the case of formal union contracts, very much moderated. Of course, there are other possible causes for the decline, including the onset of permanent threat to US manufacturing jobs, deunionization of the private sector, and the collapse of oil prices. Suggestive that there was a switch of equilibrium, a period of high employment and nonaccelerating inflation followed.¹⁵ That period is called the “Great Moderation.”

Going back to the earlier discussion, relative to the approach of Samuelson and Solow (1960), the dual-equilibrium model gives a new view of optimal macro policy. For them, macro policy entailed making the best choice in the trade-off between inflation and unemployment, as indicated by their estimated Phillips curve. The dual-equilibrium model gives a more nuanced view: more generally, the optimal policy is to choose such a trade-off, but with a further constraint. Inflation and employment should be kept below levels that will cause the migration of the equilibrium from denial of cost-of-living adjustments to one in which they are commonly granted.

The preceding discussion of cost-of-living adjustments to wages—when they are granted, when they are denied—is, of course very special. Of course, there are many other possible norms regarding wage setting, including many other possible norms regarding COLAs. But even if the example is only illustrative, it is also the tip of an iceberg, because it suggests more general questions: What is the role of norms

employer) is likely to increase more than proportionately. In contrast, it is likely that the benefit to the employer from the money gain from the denial will be strictly proportional to its cost to him. With these assumptions, sufficiently high inflation, *all else equal*, will result in enforcement of the norm. But the level of employment—whether high or low—can also have overriding influence on enforcement of the COLA norm.

¹⁵The dual-equilibrium model of the role of inflationary expectations then takes us back to the criticism by Lucas (2003) discussed above in footnote 10: that stabilization policies will result in only second-order increases in *average* employment—even in New Keynesian models. But there can be first-order increases in average employment if there are dual equilibria. In this model, stabilization will result in first-order increases in employment if it prevents shifts from a deny-COLA to a grant-COLA equilibrium. The reason is simple. We have seen, with the example of the 1981–1982 recession, that it took very high unemployment to drive the equilibrium back to one in which COLAs were rare. For that to occur, unemployment had to rise so high that workers would accept the denial of a perk that had become common practice.

in wage setting? And among the many different possible norms, what will be the possibilities of multiple equilibria? In chapter 2 of *The General Theory*, Keynes (1936) was emphatic that norms of wage setting—especially including nominal, rather than just real, considerations—play major roles in such bargains. Macroeconomists’ quick adoption of the accelerationists’ arguments that inflationary expectations uniformly impact wage and price changes one-for-one is thus another symptom of the dismissal of Keynes’s “satiric entertainment.” It was also another example of macroeconomists’ adherence to one-deviation-at-a-timeism, and their antipathy to multiple beauty-contest equilibria. Much important work in this area remains to be done.

Summary

The adaptation of *The General Theory* into the Keynesian-neoclassical synthesis neglected multiple vulnerabilities of the resultant model. Of course, there were reasons for this adaptation, prominently including a desire to build a professional consensus in support of activist Keynesian fiscal policy. By creating a model with an aggregate supply side that was classical in nature, and that allowed plentiful opportunities for economists to practice one-deviation-at-a-time analysis, support was indeed bolstered for Keynesian policy. But Keynesian economists became overly attached to *their* paradigm. They were dismissive of anomalous observations that indicated the need for new, more nuanced economic thinking.

The Keynesian-neoclassical synthesis that had emerged by the early 1960s put constraints on macroeconomics. Foremost, it divorced macroeconomists from working on financial stability. Luckily, after the crash of 2008, the prior work of finance economists has been belatedly acknowledged, and the subfield of macro stability has also emerged as quite possibly *the most* vibrant research frontier in economics.¹⁶ Nevertheless, macroprudential concerns remain as back matter in the textbooks. Correspondingly, macroprudential policy is undervalued in the councils of government. Yet its importance remains, given the likelihood of another crash. In these consequential ways, macroeconomists’ dismissal of “satiric entertainment” in the decades after *The General Theory* still lives with us. If there is blame to be faced, I confess that it should be placed on me as much as on anyone else. I tried, but I too was one of those macroeconomists.

■ *I thank Jeffrey Butler, Robert Johnson, Hui Tong, and Janet Yellen for invaluable comments.*

¹⁶As one indicator, see the Symposium on Financial Stability Regulation in the Winter 2019 issue of this journal. Even the briefest summary of the excellent work in this field would more than double the length of this essay.

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