

<TI>The Solow Residual as a Black Box

<STI>Attempts at Integrating Business Cycle and Growth Theories

<AU>Tiago Mata and Francisco Louçã

The intersection between growth and business cycle theory remains a controversial subject in economics. The question posed by this article is, What role did Robert Solow's "Technical Change and the Aggregate Production Function" (1957) play in recent attempts to integrate business cycle and growth theory? We argue that the "Solow residual" was a resource given to multiple uses, at times rhetorical and symbolic, at times instrumental for theory development, at others a social artifact.

The Solow residual is herein conceived as an object. We examine the history of fluctuations in growth theory in a narrative of model building, of the addition and subtraction of elements to models. Our views are informed by the concept of the black box associated with the work of Bruno Latour and colleagues (Latour 1987; Latour and Woolgar 1979). Their notion is that theoretical and experimental artifacts can attain a status of consensual acceptance that exempts them from close examination. It is in this sense that they are said to be "closed."¹

Our use of the term black box is felicitous for another reason. The term has an established lineage in controversies over the role of innovation in economic growth. We agree with Nathan Rosenberg's (1982) judgment that "economists have long treated technological phenomena as events transpiring inside a black box. . . . [and] adhered rather strictly to a self-imposed ordinance not to inquire too seriously into what

transpires inside that box." Our story, which begins in 1982, at the time of Rosenberg's book, suggests that the box was opened to become a source of multiple puzzles. {**Au: I.e., the openers of the box knew it would raise a lot of puzzles? Or after the box was opened, only then did people see that it raised a lot of puzzles?**} But before we consider the uses given to the object, we have to observe how neoclassical growth theory canonized the Solow residual.

<A>Solow's Residual and Growth's Black Box

The influence of Solow's "Technical Change and the Aggregate Production Function" cannot be overemphasized. The piece followed closely the publication of Solow's (1956) celebrated growth model and was intended as a contribution to the literature on growth.² Its startling conclusion was that "gross output per man hour doubled over the interval [1909–49], with 87½ per cent of the increase attributable to technical change and the remaining 12½ per cent to increased use of capital." Solow humbly acknowledged that the result lacked great depth or insight. He noted soon after that

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in every-day speech, talk of technological change calls to mind primarily single discontinuous inventions, like the electric light or the automobile or the electronic computer. . . . But most of this flavor disappears in statistical aggregates.

Economists, who give the impression of having invented the idea of technological progress in the past 6 or 7 years, have something much more

pedestrian in mind, . . . “increase in output per unit of input.” It is a statistical artifact. (Solow 1963, 37)

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<FL>It was immediately understood that such a large proportion of growth left unaccounted for by the factors of production was a problematic result. Frank Hahn and R. C. Matthews (1964, 832), in their “monumental” 120-page survey of growth theory, noted that “those who used the ‘residual’ approach were well aware that the residual, as its name implies, is a catch-all and that in practice the task of research would be to break it up into its constituent components.”

The field of growth accounting blossomed in the 1960s, particularly following the lead of Edward Denison (1962, 1979, 1985; Denison and Brookings Institution 1967, 1974). As Nicholas Crafts (this volume) establishes in his survey, the residual resisted the multiple accounting models. As Christopher Freeman (1988, 2) graphically put it: “The various ‘growth accounting’ exercises, even after allowing for an entire Kamasutra of variables, generally remain with a big unexplained ‘residual’ and fail to deal with the complementarities and interactions of these variables.” Denison (1967, 283) was aware of the difficult interpretation of the residuals, which include “contributions of advances in the lag of average practice behind the best known but also the net effect of errors in the growth rates themselves, of errors in estimates of the contributions made by other sources of growth, and of omission of all sources not specifically estimated.”

Alongside the accounting exercises, in theory development, Solow’s contribution set into focus the need to formalize “innovations” (Burmeister and Dobell 1970).

Solow's own views on the multiple formulations of the aggregate production function leaned toward labor augmenting technological progress and capital "vintages" with differing productivities. Yet, despite the insight afforded by these developments, the dynamic rested in the technology component represented by the residual, "to change the rate of growth of real output per head you have to change the rate of technological progress." Significantly, such improvements stood largely outside the model's explanatory purview, as follows from the statement that policy measures "involve considerations quite outside the model we have been discussing" (Solow 1970, 77).

The residual's resilience led to comfort in the belief that "innovation" was an indistinct and impenetrable stream whose source was in the domain of other branches of economics. The Solow residual a {**Aus: Is there a verb missing here? "residual was a"?**} statistical artifact, seemingly decided on the division of labor between growth and other theoretical quarters. This occurred despite Solow's (1985) misgivings about sharply drawn divisions of labor or dogmatic acceptance of neoclassical theory. Solow's work had gained a life of its own in the research of other authors and was autonomous of his intentions or interpretations.³

<A>Shocks and Business Cycle's Black Box

In separate quarters Robert Lucas and his coauthors were championing in the 1970s a new approach to macroeconomics.⁴ It was a model-building approach, where

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a "theory" is not a collection of assertions about the behavior of the actual

economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy. A “good” model, from this point of view, will not be exactly more “real” than a poor one, but will provide better imitations. (Lucas 1980, 697)

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<FL>The early literature commenting on new classical macroeconomics has emphasized the rational expectations hypothesis (Kantor 1979; Laidler 1981; Tobin 1981; Begg 1982; Blaug 1980; Sheffrin 1983),⁵ but to Lucas the discriminating feature of his models was a new notion of equilibrium. Models were “constructed so as to predict how agents with stable tastes and technology will choose to respond to a new situation” (Lucas 1977, 12; emphasis in original). Lucas and Sargent (1979, 7) attributed earlier notions of equilibrium to a distant legacy of John Maynard Keynes and his General Theory, stating that “the term equilibrium was thought to refer to a system at rest,” but “the meaning of the term equilibrium has changed so dramatically that a theorist of the 1930s would not recognize it. An economy following a multivariate stochastic process is now routinely described as being in equilibrium, by which is meant nothing more than that at each point in time” that markets clear and agents act in their own self-interest.⁶

Influential papers by Lucas (1972, 1975) embodied this program. Lucas’s models describe a stationary process around a deterministic time trend representing a “natural rate of growth,” whereas serially correlated variations of the levels of the economy motivated by exogenous random shocks create the cycles. The solution for the whole economy indicates the properties of the cyclical component, as agents confound

variations of relative and general price levels (Dore 1993, 68, 75). **{Au: Please add Dore 1993 to the references.}** The economic intuition is that agents confound unanticipated monetary shocks, with no real effects, with shocks in relative prices, implying substitution effects. The exogenous shocks have no permanent influence on the behavior of the system.

The seminal and canonical paper on real business cycles is Finn Kydland and Edward C. Prescott's "Time to Build and Aggregate Fluctuations" (1982).⁷ The authors' stated purpose was to "integrate growth and business cycle theory," conceiving real shocks as their distinction from Lucas's business cycle models. As they later stated, "Economic activity in industrial market economies is characterized by sustained growth. Lucas defines business cycles as deviations of real GNP from trend. . . . But Lucas does not define trend, so his definition of business cycle deviation is incomplete" (Kydland and Prescott 1990, 53) **{Au: There is no page 53 in Kydland and Prescott 1990 or in any of the other Kydland-Prescott works in your reference list. Can you double-check the citation?}**. Kydland and Prescott's (1982) "time-to-build" production function was composed of a persistent and a transitory element, interpreted as a technological shock.⁸ Production was explicitly modeled with agents not only optimizing the investment-consumption decision but also allocating time between employment and leisure.⁹

John Long and Charles Plosser (1983) gave the new class of models the label of "Real Business cycles."¹⁰ Following work by Charles Nelson and Plosser (1982), they proposed a Cobb-Douglas production function with a single stochastic component,

undermining the notion of a deterministic trend and replacing it by a nonstationary stochastic process with no tendency to return to a deterministic path.¹¹ The Long and Plosser model relaxed the assumption of the autocorrelation of shocks. Defining an input-output structure they examined the propagation of a shock and an individual's smoothing of windfall capital gains. They later realized that serial correlation was critical to produce fluctuations (King, Plosser, and Rebelo 1988). It is significant that the Long and Plosser piece was never as influential as the Kydland and Prescott model. Although the authors simulated the time paths of their model economy to reveal cycle-like movements, this result did not seek to match the economic record. The model was illustrative but unpersuasive in the new classical sense of providing an "imitation" of the real economy.

The real business cycle models developed from Lucas's program.¹² They assumed calibration in the place of econometric estimation, Kydland and Prescott (1982) comparing the time paths of their simulations with quarterly data for the U.S. postwar economy.¹³ Puzzle solving was focused on tweaking the models' specifications to replicate the autocorrelation and comovements in U.S. time series data.¹⁴ The sources of fluctuations were not misperceptions but objective changes in the aggregate productivity function. The dynamic stochastic behavior of the model-worlds emerged out of microeconomic principles of maximization and out of a situation in which all markets are permanently cleared.

Lucas (1987, 46–47) was supportive of the new generation, in 1987 calling Kydland and Prescott's work the "current frontier in business cycle research" and

adding that they offer “a formulation that combines intelligible general equilibrium theory with an operational, empirical seriousness. . . . I would like to call this progress.”¹⁵ Lucas was critical to the extent that the “model focuses exclusively on real (as opposed to monetary) neoclassical considerations, which I think is a mistake.”¹⁶

By locating the emergence of real business cycle models in the context of new classical macroeconomics, we aim to show its genetic distance from growth theory. Solow 1957 was not mentioned in the two seminal pieces of real business cycle models (Kydland and Prescott 1982; Long and Plosser 1983). Solow was on record as quite dismissive of the new classical enterprise. In his presidential lecture to the American Economic Association, he portrayed the equilibrium outlook as unreasonable, saying it is “legitimate to wonder why the unemployed do not feel themselves to be engaged in voluntary intertemporal substitution, and why they queue up in such numbers when legitimate jobs of their usual kind are offered during a recession” (Solow 1980, 7).

Solow was not engaged by the enterprise but that did not mean that his work was not influential. Although real business cycle models emerged from the new classical program, its adoption of the neoclassical production function borrowed from Solow. The goal was to integrate business cycle and growth, a prospect that Solow (1982) shared. Solow and the new classical concurred that the “technology” component of the production function was its dynamic and independent element.

<A>The Black Box Challenged

The notion of shock entered the discourse on the growing economy in the 1980s. Later

in the decade, real business cycle theory's attempts to straddle growth and fluctuations became an item of controversy.¹⁷ Our aim in this section is to highlight how the interpretation of the Solow residual became a crucial item of that debate.

The opening salvo of the controversy was fired by Prescott at a Carnegie-Rochester Conference on Public Policy in 1985, with an essay titled "Theory Ahead of Business Cycle Measurement" (Prescott 1986b), in which he offered an updated version of his 1982 model with Kydland. While Kydland and Prescott (1982) had studied variance and correlation for the output of different sectors of the economy, the new empirical target was to simulate the dynamics for the American economy post-Korean War for such aggregate variables as investment, inventory stock, capital stock, consumption, hours of work, productivity, and real interest rate. Prescott judged a satisfactory match between his simulation and the record, but he found discrepancies in labor's sensitivity to changes in output, the recorded elasticity being well below the model's solution. Prescott considered the mismatch to be due to empirical measurement lagging behind theory development.¹⁸

Contemporaneously, at a National Bureau of Economic Research (NBER) conference in March 1986 in Cambridge, Massachusetts, Martin Eichenbaum and Kenneth Singleton (1986) offered an assessment of monetary shocks (changes to M1) to explain variance of aggregate output. They concluded that monetary shocks had an insignificant causal power, which led them by default to endorse an explanation through real shocks. Real business cycle models were supplanting Lucas's money models. At this triumphant stage, Solow's work receives mention. Prescott (1986) {Au:

1986a or 1986b?} credited Solow and Trevor Swan as the originators of growth theory from which he claimed business cycle theory partly derived. But more salient than ancestor worship was Prescott's use of Solow's (1957) method to obtain a measurement of technology.¹⁹ Prescott concluded that economic fluctuations were optimal responses to uncertainty. Policy should be devoted to the institutional study of technology and an increase of its rate of change, in effect a proposition that echoed Solow's 1970 lectures. The policy brief of 1980s business cycle theory was aligned with that of 1960s neoclassical growth theory.

It was Lawrence Summers, speaking to the NBER Economic Fluctuations Group, who engaged with Prescott's overview (Summers 1986). Summers's list of objections sought to turn Prescott's paper on its head: "Theory was ahead of measurement" because theory had been constructed with unobservables that forever eluded definitive study. Not all of Summers's objections are of interest to our argument. The second objection, "Where are the shocks?" zoomed in on Prescott's interpretation of changes in total factor productivity as technological shocks albeit with a small measurement error. Summers asked for historical examples to assert the validity of this hypothesis and offered an alternative interpretation where changes in the Solow residual reflected businesses hoarding labor in response to economic downturns.

Both Prescott's and Summers's articles were published as a debate in the Quarterly Review of the Federal Reserve Bank of Minneapolis, and Prescott (1986) was given the chance to reply to Summers. To the charge that no technological shocks were discernable in the historical record, Prescott called on Eugen Slutsky's finding that

“some stable low-order linear stochastic difference equations have been known to generate cycles. They do not have a few large shocks, they have small shocks, one every period.” Stating that economists lacked a theory of technological change, Prescott called on Solow (1957) to argue that given the “finding that more than 75 percent of the changes in per capital output are accounted for by changes in the technology parameter, the evidence for variation in the rate of technological change is strong.” Summers’s argument of labor hoarding was classed as a measurement problem in the labor component of the production function, distinct from the measurement of technology. Solow’s authoritative findings were at this point a rhetorical resource in defense of real business cycle models (Hartley 2006).

The debate remained unsettled, since in 1989 the same arguments were repeated with a new cast of protagonists in the pages of the Journal of Economic Perspectives. Plosser was cast as defender of real business cycle theory. Mentioning Solow twice in his argument, Plosser refers to Solow 1957 as a piece of empirical research revealing that “changes in productivity and technology are the major factors determining economic growth.” Further, “a crude but straightforward method [of obtaining some measure of the productivity shocks] is to follow the example provided by Solow” (Plosser 1989, 58).

Reprising Summers’s criticisms, Gregory Mankiw reestimated the Solow residual to highlight substantial yearly variation. He clarified that “if the Solow residual is a valid measure of the change in the available production technology, then recessions are periods of technological regress” (Mankiw 1989, 84). Mankiw thus argued that for the 1982 recession there had been no reduction in the residual, contradicting the real

business cycle argument. To counter the implausible interpretation, he proposed that cyclical changes in the Solow residual reflected firms' decisions to hold on to unnecessary and underutilized labor.

The interpretation of the Solow residual was center stage.²⁰ What is striking is that the Solow residual remained stable to a degree, as an object. In figure 1 there are two representations of the Solow residual. The numerical values were the same for both authors and were not in dispute. It is their readings that differed. Plosser (panel A) framed the residuals alone and hence interpreted a fluctuating time series of irregular interval, suggestive of a random walk. Mankiw, on the other hand, provided a greater time range, a much more cluttered and erratic depiction. Critically, Mankiw plotted the residuals (panel B) against the income series to stake the claim that the residual was quite literally a leftover. It was not the residual that was controversial but what it meant or, in Mankiw's terms, which was the "more appealing interpretation"?

<Figure 1 about here>

The differing interpretations of the Solow residual were not alone in demarcating the two perspectives. Probably more significant was the new Keynesians' disbelief in the automatic full employment of the Walrasian real business cycle models. Further, the latter reserved a role for monetary policy that was seemingly forbidden by the "real" emphasis of their opponents' models. Ironically, the Solow residual was not what set them apart, but what they shared. It was a convenient subject of their discussion

because it was an object that stood at the boundary between the two communities.²¹

<A>Opening the Box

As the Solow residual became a focus of debate, it moved from being a rhetorical and symbolic object to become a research puzzle. It is in relation to the opening of this black box that we can envisage the landscape of models.

Contemporaneously to the business cycle debates, the subject of neoclassical growth theory witnessed a resurgence of interest. This was largely due to the work of two Chicago men: Paul Romer and Lucas.²² In the 1985 Marshall Lectures at Cambridge University, Lucas (1988) toyed with models of growth in which infinitely living families accumulated human capital. Further, human capital was formalized to have a spillover effect on the production function. Hence, while individual economic units faced constant returns to scale, the economy had increasing returns. Such work reinterpreted the Solow “technology” residual as disembodied human capital, a sediment of collective human capital. The more significant attack on growth theory was that of Romer (1986a, 1986b). **{Au: Please add Romer 1986a and 1986b to the references.}** He too sought to ban exogenous technological change from his competitive model. He specified a production function for technology production whose goods were in some measure nonexcludable, hence creating a positive externality. And although the production function of technology faced decreasing returns, output production had increasing returns.

The initial response of real business cycle theorists to the new classical versus

new Keynesian controversy was tame. Real business cycle theorists met in June 1986 at the Portuguese Catholic University to discuss the state of their research program.²³

Kydland and Prescott (1988) reconfigured their model to allow capital utilization to vary, hence separating the service of capital from its stock measurement. The volatility of their model increased, although the nagging discrepancy between model and measured labor-output elasticity remained. Prescott (1988, 2006) never conceded to his critics that the real business cycle explanation required revision; he has consistently described the debate as settled to his advantage. In his Nobel lecture, Prescott dubbed his contribution part of a “revolution” that reunited macroeconomics with neoclassical economics. He took his 1986 findings that total factor productivity “shocks are the major contributor to fluctuations” to be “highly robust,” lining up models akin to his as evidence (Prescott 2006, 11). Still, he was left to uncomfortably argue for the magic number of labor supply elasticity equals 3. Proof was hung on that proposition.²⁴

Despite statements otherwise, Prescott has struggled with explaining the Great Depression (De Vroey and Pensieroso 2006). Following Cole and Ohanian (1999), **{Au: Please add Cole and Ohanian 1999 to the references.}** Prescott separates the 1929–33 from the post-1933 period. While changes in total factor productivity may partly account for the 1929–33 downswing, they are unable to account for the American economy’s failure to return to trend growth. Prescott (1999, 26) considered that, “given the considerable evidence against technology, monetary, or banking explanations, I am led . . . to the view that there must have been a fundamental [New Deal] change in labor market institutions and industrial policies that lowered steady-state, or normal, market

hours.” This result has been heralded as a “novel fact” given by theoretical investigation (Kehoe and Prescott 2002), but it hides an inability of the real business cycle models to provide plausible explanations of the economic historical record.

Prescott also developed a telling interest in the work initiated by Lucas and Romer. With a former graduate student, Stephen Parente, Prescott began by establishing the “development facts” models ought to emulate (Parente and Prescott 1993). They then designed a model of national technology adoption from an ever-increasing stock of world technology (Parente and Prescott 1994). Differences in relative income shares between nations were explained by an unobservable “technological capital” parameter that was deemed embodied in the firm but invisible to the aggregate data. The authors introduced a term of “cost of technology adoption” that did most of the explanatory work. Looking at the data for Japan, they concluded that “Japan was converging to some balanced growth path and in 1974 there was regime change, that is, a persistent and unanticipated change in the magnitude of the technology adoption barrier parameter” (Parente and Prescott 1994, 310). Hence the noisy flow of technology produced cycles, and it was the institutional interruption of its flow that set growth paths.

The Plosser branch of the real business cycle research program was more adventurous in rebuilding their models. To the 1986 conference, they restated their collapse of the trend-cycle distinction and with it the emergence of stochastic growth. They used the Lucas formulation to endogenize labor-augmenting technical progress. After Lucas’s growth model had reinterpreted the “residual” as a social spillover from

learning and human capital accumulation, one could hardly conceive of shocks originating out of this steady accumulation. Instead, the impulse was located in an equally mysterious and exogenous technology component in the production of human capital. It was a new black box within the old black box. Still, the move was not inconsequential. They found that “temporary disturbances to production possibilities . . . have permanent effects on the levels of economic activity, because they permit temporary changes in the amount of resources allocated to growth. Thus, endogenous growth models generate integrated time series, even when the underlying shocks are stationary” (King, Plosser, and Rebelo 1988, 325).

An idiosyncratic strategy in real business cycle models is to embrace the ambiguity of the Solow residual. In a recent paper, Costas Azariadis and Leo Kaas (2007) propose, in the spirit of Prescott’s Nobel lecture, that dynamic general equilibrium is a “theory of everything.” In their grand scheme the Solow residual becomes an index that “conveys information not just about the aggregate technology frontier but also how far inside that frontier the economy operates.” It is said to depend on “sectoral productivities; on financial variables like debt-equity ratios, interest rates, and the distribution of equity positions, and on institutional variables like property rights of lenders” and hence wealth distribution (Azariadis and Kaas 2007, 38).

Summers’s and Mankiw’s interpretation of the Solow residual as permeable to labor hoarding, and hence a poor measure of technological change, was substantively restated by Robert Hall (1986, 1990).²⁵ Hall argued that yearly movements in the Solow residual may potentially reflect market power, increasing returns, external technical

complementarities, chronic excess capacity, unmeasured variations in work effort and hours, errors in measuring capital or output, and monopsony power in the labor market. He then added that the residual would not be sensitive to overhead labor or labor hoarding, wage smoothing, adjustment costs, or price rigidities.²⁶

The new Keynesian modeling {**Au: Do you mean just “model”?**} of the cycle has distinguished itself for an emphasis on time and sector rigidities in the determination of wage contracts, prices, and investment plans (see Mankiw and Romer 1991). There is no class of new Keynesian growth models, but a strand in endogenous growth models bears some kinship.²⁷ Peter Howitt (1986) has interpreted the core of Keynes’s message as revealing the prevalence of intertemporal coordination problems. Philippe Aghion and Howitt’s research has sought to wire the new Keynesian adjustment processes with Schumpeterian insights for a study of long-run growth (Howitt 1994). This they accomplished in two papers in the early 1990s, one focusing explicitly on the concerns of the growth debate, the other on matters of unemployment (Aghion and Howitt 1992, 1994). Their highlight is that technology is disruptive: it renders old capital obsolete, leading to a destruction of jobs. Unemployment is “job search,” as the adjustment process endemic to a changing economy. The connection between this proposal and the subject of the Solow residual and growth accounting was recently made explicit. Aghion and Howitt (2007) offer a hybrid neoclassical model with endogenous growth. The innovation dynamic is restated as an uncertain investment that is a function of the capital stock. The relationships between the factors and technology become more deeply entangled.

<A>Conclusion

On the history of models that bring growth and fluctuation into a single frame, we have focused on the uses and meanings of a particularly prominent object: the Solow residual. The significance of Solow's 1957 work arose from having stabilized a method and result, the residual as reproducible object, a black box. This object was shown to traffic liberally across doctrinal divides in economics. Once on offer, the black box had a life of its own. Its relation to the original context and to the intentions and beliefs of its originator was severed. So while Denison used the residual in ways that were surely faithful to Solow, the new classicals employed it in ways that seemed counter to Solow's outlook. While the residual had always remained a problematic result in growth accounting, its borrowing by real business cycle theorists sought to establish it as a definitive representation of technology. Furthermore, in these models it was a short-run and stochastic technology, a novel and surprising interpretation.

As the claims of the new classicals came under scrutiny, so did the status and meaning of the object residual. The challenge by new Keynesians brought attention to it. The integration of growth and cycle has been shaped by the opening of this black box. Prescott has remained committed to his earlier interpretation of the Solow residual as stochastic technology. Others have sought to blanket a variety of supply shocks under the cover of the residual, abandoning attempts to decompose it. For the new Keynesians the "residual" was more evidence of market power and the need to integrate rigidities in the study of the cycle.

Solow (2007, 12) has consistently maintained the importance of the cycle in the modeling of growth, stating his worry “about the tendency of modern (American) macroeconomists to forget about the pathology of business cycles.” But in a review of the recent growth literature he does not mention the models that market themselves as integrating growth and cycle (Solow 1994). His judgment on real business cycles has remained unappreciative. Writing for the prominent humanities journal *Daedalus*, Solow (1997, 52) {**Au: Note corrected page number.**} casts real business cycle models with a minor role in the history of profession: “The bulk of the intellectual effort goes into the ways of showing that the data of observed fluctuations are compatible with the demands of the model. This is not easy because the key driving forces—irregular changes in tastes and technology—are not directly observable.” However, his closing summary is intriguing, stating that “the more adventuresome advocates of Real Business Cycle Theory have found it necessary to modify many of the clean but extreme assumptions that give formal general equilibrium theory its artificial vanilla flavor. . . . it has come closer and closer to the more or less ‘Keynesian’ model it was supposed to discredit” (52–53). These more interesting developments are in our narrative the puzzling through the Solow residual and its continued privileged role in the construction of comprehensive models of economic dynamic.

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<Begin footnotes>

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1. For a test of Latour's semiotics, see Yuval Yonay's (1994) study of institutional economics.

2. For a survey of what was in the 1950s and 1960s a vibrant field of econometric study, see Walters 1963.

3. Coupled with the subject of innovation was uncertainty. It was a difficulty that growth theory could not face and should hence ignore (Solow 1963, 15). Solow's deterministic standpoint remained unchanged, when two decades on, around the time of this narrative, Solow wrote to Frank Hahn: "Regarding . . . fluctuations, Schumpeterian innovations, and all that. I find myself driven to the uncomfortable view that probability is not the right way to handle such things as innovations or other real shocks. . . . Last thing in the world I want to spend time on is axiomatizing Knightian uncertainty. Maybe some ad-hoc device to get away from simple-minded mean-value thinking. Maybe better to dodge the whole thing" (Solow 1984.)

4. This work had its predecessors, most notably Milton Friedman. The terms of this monetarist ancestry are controversial; see Hoover 1988 for a review of that debate and Kim 1988 for an account of Austrian threads.

5. For a critical examination of rational expectations complex history, see Sent 1998.

6. Lucas's caricature of Keynes is not historically accurate. For Keynes equilibrium arose from the matching of individual plans of saving, hoarding, and

investment. It is a rather more dynamic story than Lucas portrays. On the history of equilibrium notions in economics, see Weintraub 1991. Lucas (2004) has recently suggested that his background had a great deal more Keynesianism than he avowed in print in the 1970s. For a critical assessment of Lucas's education, see Louçã 2004.

7. In a bibliometric study of the most-cited academic articles post-1970, Kydland and Prescott 1982 were ranked sixty-first, being cited 814 times (Kim, Morse, and Zingales 2006). In 1972, William Brock and Leonard Mirman published in the Journal of Economic Theory a paper presenting a basic model of growth of a single sector with stochastic impulses to the production function, and established the proof of ergodicity required for time series econometrics. Real business cycle theorists do not ignore this work. But references are sparse, centered on the peculiar contribution of the ergodicity proof and not recognizing the core idea that was present in the 1972 paper and that anticipated the strategy of real business cycle models (Prescott 1986b; Plosser 1989). It is only in a footnote that Rodolfo Manuelli (1986, 3) notices that the model was the first to use stochastic shocks in the production function. In private correspondence with one of the authors, Brock argued for a very open modeling strategy. According to this view, the way forward should be based on an experimental recipe starting with Frank Ramsey and adding stochastic shocks for uncertainty (as in Brock and Mirman 1972), disutility of labor in the production function, money, sticky price vectors, and eventually some market failures. This unorthodox outlook on orthodoxy may explain why Brock and Mirman were denied progeny of real business cycle modeling.

8. A 1980 paper had speculated on the introduction of preference shocks

alongside productivity ones, but the new version collapsed the former (Kydland and Prescott 1980).

9. There were four potential sources of persistence in the Kydland-Prescott model: autocorrelation of the exogenous technological shocks, time-dependent individual preferences, inventories as factors of production, and the “time-to-build” production function. K. Geert Rouwenhorst (1991) has argued that the “time-to-build” production function is insufficient to produce the cyclical behavior; for this the autocorrelation of exogenous shocks is required.

10. Long and Plosser cite the working-paper version of Kydland and Prescott 1982, but their paper did not originate as a refinement of that model. In a personal communication of January 2008 to the authors, Plosser recalled that their paper was well underway before they saw the Kydland-Prescott one, adding, “We were intentionally trying to build a real model of fluctuations using growth theory. I think KP backed into that conclusion. Thus it is fair to say the papers were developed independently at about the same time.”

11. The stochastic trend approach was further developed by James Stock and Mark Watson (1988a, 1988b), **{Au: Please add Stock and Watson 1988a and 1988b to the references.}** who hypothesize that variations in trends are responsible for an important part of the fluctuation of output in U.S. series. So did Stephen Beveridge and Nelson (1981) and Nelson and Plosser (1982), stating that business cycles can be defined as adjustments to changes in the growth paths.

12. Besides the intellectual kinship there is also a social connect. Lucas and

Prescott met at Carnegie Mellon in the late 1960s and continued to collaborate for over two decades, well into Prescott's real business cycle years (Lucas and Prescott 1971, 1974; Prescott and Lucas 1972; **{Au: Please add Lucas and Prescott 1971, 1974 and Prescott and Lucas 1972 to the references.}** Stokey, Lucas et al. 1989). **{Au: Please add Stokey, Lucas et al. 1989 to references.}** Arjo Klamer's (1984, 32–33) interview with Lucas captures this bond of friendship.

13. The calibration approach is not the only one available to the new classicals; see Cooley 1997. For an opposing view on the promise of calibration, see Hoover 1995.

14. For sympathetic surveys, see King and Rebelo 1999 and Rebelo 2005.

15. Lucas's assessment is not surprising given the close kinship between his models and Kydland and Prescott's work. The propagation side of their models is very similar; it is in **{Aus: Is this correct? Or are there other words missing here?}** the impulse element that they differ. See Lucas 1977.

16. See also the updated but largely unchanged commentary in Lucas 2007.

17. Reviewing real business cycle theory, Victor Zarnowitz (1985) did not think much of Kydland and Prescott's work.

18. It should interest the historian that Prescott had in mind a parallel between contemporary debates and the famous "measurement without theory controversy" between the Cowles Commission and the NBER. In a 1990 paper, where Kydland and Prescott examined the ancestry of their program—Mitchell, Frisch, Slutsky, and Lucas—they endorsed Tjalling Koopmans's (1947) critique of Burns and Mitchell 1946: a theory is needed before variables are selected. Yet, following Lucas's critique, they disagreed

with Koopmans's use of structural systems of equations, considering that imposing such a straitjacket was a "grave disservice" (Kydland and Prescott 1990, 4). Prescott may have been primed toward the "measurement without theory" debates by the April 1985 Carnegie Rochester conference where Salih Neftci gave a paper on the NBER's method of fluctuation analysis. The series editors read it as "the first serious reconsideration of Tjalling Koopmans's judgment, advanced more than 30 years ago, that the National Bureau methodology indulges in 'measurement without theory'" (Brunner and Meltzer 1986, 2).

19. The following year, 1987, Solow was awarded the Nobel Prize for economics. Alongside a paper by R. C. O. Matthews reviewing the laureate's contributions, Prescott wrote in the Scandinavian Journal of Economics, restating his 1986 findings. He reasserted that he had "challenged the widely held view that variations in the Solow technological parameter are too small to account for a significant amount of business cycle fluctuations" (Prescott 1988, 10).

20. Our side-by-side report of this set of controversies highlights a striking, if habitual, divide in American economics. In effect, this was another installment of the "saltwater vs. freshwater" controversy. Mankiw (1986) **{Aus: Please add page for this quotation.}** made it quite explicit: "Real business cycle theory is one of the currently 'hot' topics in macroeconomics, especially among those who live closer to lakes than to oceans."

21. The notion of "boundary object" connecting scientific communities is well established in the social study of science (Star and Griesemer 1989).

22. For a history of events, characters, and the economics of Paul Romer, see Warsh 2006.

23. The papers were only published in 1988 and substantially updated with the latest literature.

24. Prescott's reliance on the high elasticity of labor has informed his examination of why Americans' working hours differ so widely from European ones (Prescott 2004). This has sparked an independent controversy that rages on; see Faggio and Nickell 2007.

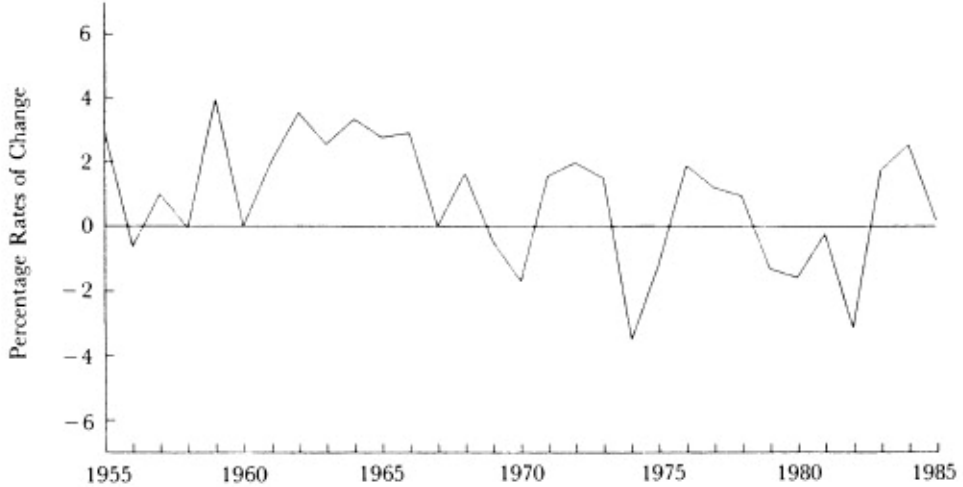
25. The new classical response to Hall was given by Matthew Shapiro (1987), comparing output and factor price measures of productivity. He interpreted the null result of no differences between the two as proof that variation in output productivity (Solow residual) was not explained by changes to demand.

26. James Hartley (2000) offers a microfoundations model that excludes the possibility of an aggregate production function. Under these conditions changes in technology will not register in the Solow residual.

27. This occurs despite the all-pervasive discredit of the trend-cum-cycle distinction. Following Nelson and Plosser 1982, Campbell and Mankiw (1987) have found that "innovations" were highly persistent in both U.S. quarterly postwar data and annual data from 1896 to 1984. Research by Shapiro and Mark Watson (1988) and King et al. (1991) have studied the contribution of changes in productivity to U.S. income data modeled as a stochastic trend: the first found its impact to be about one-third, the latter found it to be roughly two-fifths.

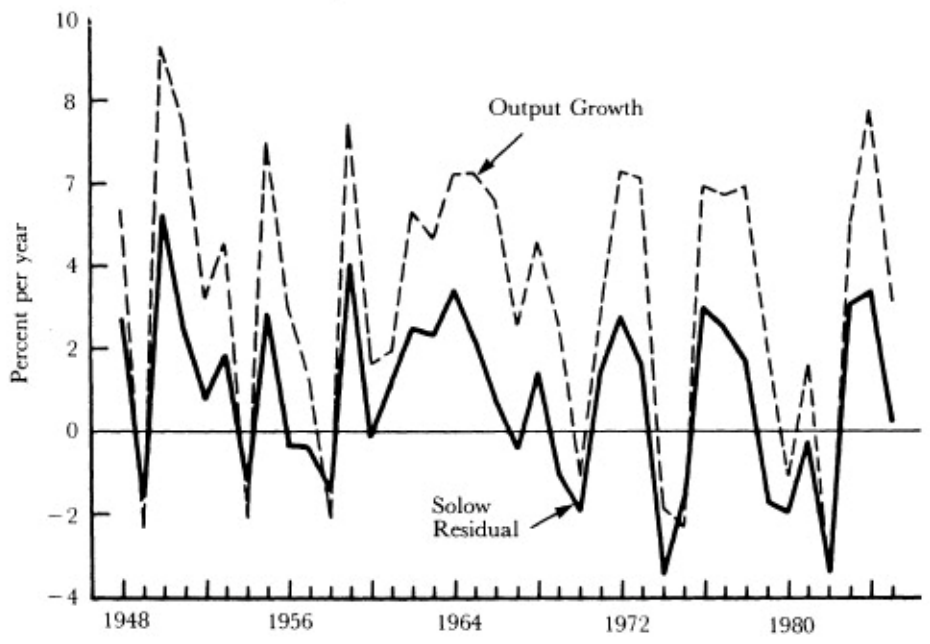
A *Figure 1*

Annual Growth Rate of Technology



B *Figure 1*

Solow Residuals and Output Growth



<Figure caption>

Figure 1 The “Solow residual” as evidence to conflicting claims: panel A taken from Plosser 1989; panel B taken from Mankiw 1989.