In the Kingdom of Solovia: The Rise of Growth Economics at MIT, 1956–70

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1. Growthmanship

In the early 1960s the Massachusetts Institute of Technology (MIT) was the native land of the "growthmen." Its leading light, Paul Samuelson (1948a), had published a pathbreaking undergraduate textbook, *Economics: An Introductory Analysis*. The book was striking partly because it gave pride of place to the analysis of national income and outlined the vision of the "mixed economy" in which Keynesian demand management would secure full employment and the price system operating under the usual neoclassical analysis would govern allocation (Pearce and Hoover 1995). (By the third edition this stance had gained a name: "the neoclassical synthesis" [Samuelson 1955, vi].) The focus was on the short run—avoiding a replay of the Great Depression and combating the anticipated postwar slump loomed large in Samuelson's vision. He paid no attention

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to economic growth per se. In the sixth edition of *Economics*, Samuelson (1964, vi) added a "new chapter on the *theory* of growth." Samuelson had become a growthman.

Richard Nixon coined "growthmanship" in the bitterly contested presidential election of 1960. No less a Cold Warrior than Nixon, John F. Kennedy had previously harped on the (later discredited) "missile gap" with the Soviet Union. And putting Vice President Nixon on the defensive with regard to the economic stewardship of the Eisenhower administration, he saw an economic gap as well: Western Europe and, especially, the Soviet Union appeared to have grown economically far faster than United States (Solow [1962] 1964, 101). Nixon used "growthman" to criticize Kennedy's single-mindedness in allowing growth to dominate the political debate (Tobin 1964; Arndt 1978, chap. 5; Collins 2000, chap. 1).

Growthmanship was originally informed by policy concerns, rather than formal economic theory (Collins 2000, chap. 1). Walt Rostow, a colleague of Samuelson's at MIT, became a national security adviser in the Kennedy administration. But Rostow was an economic historian, not a theorist. His book *The Stages of Growth: A Non-Communist Manifesto* (1960) influenced the formulation of American security policy (Lodewijks 1991). Yet, as its subtitle suggests, the practical concern of winning the Cold War dominated formal economic analysis. Samuelson (1964, 723) himself acknowledged the Soviet challenge and criticized the belief—presumably based on the notion of diminishing returns—that the Soviet Union's rate of growth would have to slow down as its economy approached American levels of economic development as "wishful thinking" and "rightwing determinism." It was characteristic, however, of Samuelson and MIT at this period to emphasize the utility of economic analysis for policy. Indeed, what perhaps most distinguished *Economics* from earlier textbooks was the way in which even an elementary exposition was informed by formal analysis. In the sixth edition, Samuelson drew on the work on growth theory of his younger colleague Robert Solow (1956)—an indication that growthmanship was taking an analytical turn. Solow ([1962] 1964, 101–3), who also worked in the Kennedy administration, had happily adopted Nixon's term of abuse: he was a "growthman," and "growthmanship has become respectable."

The MIT economists were thus growthmen in two senses: in seeing growth as an absolutely central policy imperative and in seeing the theory of growth as a focus for economic research. The first fed the second. Evsey Domar (1957, 14–15), a growth economist and expert on the economy of the Soviet Union, captured the policy imperative: economic growth was the "fashion of the day" because, when "an aggressive part of the world is strongly and quite successfully committed to rapid growth, the other can disregard this objective only if it is tired of its own existence as a society." Growth theory itself was a substantive field in economics, at least since the late 1940s. What the MIT growthmen added was a distinctive style of analysis that made it easier to address the dominant policy concerns in tractable formal models. Solow's (1956) model was the perfect exemplar of the MIT style. It provided the central framework for the subsequent developments in growth theory and secured MIT as the center of the universe in the golden age of growth theory in the 1960s. To Eytan Sheshinski (1990, 41), an MIT graduate student from 1963 to 1966, the "Solovian equipment" was a "natural monopoly with a non-duplicable resource" operating in high gear throughout the decade. Both the centrality of Solow's growth model and the policy imperative are captured nicely in the opening of Edmund Phelps's "A Fable for Growthmen":

> Once upon a time the Kingdom of Solovia was gripped by a great debate. "This is a growing economy but it can grow faster," many argued. "Sustainable growth is best," came the reply, "and that can come only from natural forces."
>
> A few called the debate growthmanship. But most thought it would be healthy if it led to a better understanding of Solovian growth. So the King appointed a task force to learn the facts of Solovian economic life. (Phelps 1961, 638)

2. Founding the Kingdom

Various historians have documented the development of growth theory in the 1950s (e.g., Boianovsky and Hoover 2009a; Halsmayer and Hoover 2013; and Halsmayer, this volume). Solow (1956) and the Australian economist Trevor Swan (1956) each developed independently what is essentially the same model, now often referred to as the *neoclassical growth model* (Dimand and Spencer 2009). The single-sector neoclassical growth model appears to be an elegant theoretical exercise in which aggregate economic growth is characterized by a single first-order differential equation. But Solow's version arose from empirical and pedagogical concerns.[1]

1. The remainder of this section is informed by, and complements, Halsmayer, this volume.

Solow, who was the student of Wassily Leontief at Harvard, had worked on the empirical implementation of Leontief's input-output analysis, constructing large multisectoral tables for the United States. In the early 1950s Solow, who had taken up a post at MIT, worked with Samuelson to analyze the theory of dynamic input-output analysis. This work was tied closely to work on activity analysis and linear von Neumann growth models, some of which informed Dorfman, Samuelson, and Solow's *Linear Programing and Economic Analysis* (1958). While much of this work was theoretical, it was empirically motivated and supported partly under defense contracts.

Solow's (1956) model grew out this work. He regarded its core to be a "collapsed production function" that helped him to characterize and explore the causal dynamics of the aggregate economy in a transparent way for his MIT engineering students. Solow regarded the model as a tool—a drastic simplification of the complexities that he had explored with Leontief and Samuelson, but not a denial of them, and not a turning away from empirical motivations. He used the model to teach craftsmanship to provide a basis for "reconnaissance exercises" to understand what factors really matter for how the economy behaves. An early exercise—not in fact by Solow himself but by Swan (1956), his codiscoverer of the neoclassical growth model—was a surprise: the rate of growth in the neoclassical growth model is independent of the rate of investment (or savings).

Solow's model arrived at a propitious moment for growth economics. He offered three reasons to account for the turn to growth economics after the mid-1950s: (1) the political attention given to the problem of the economic development of preindustrial areas; (2) the intellectual pursuit of extending Keynesian macroeconomics into the long run, initiated by Roy Harrod and Domar; and, tongue in cheek, (3) the feeling that "short-run macroeconomic theory is pretty well in hand . . . all that is left is the trivial job of filling in the empty boxes" (Solow [1962] 1964, 101–3; and letter from Solow to Hoover, July 8, 2013). The neoclassical model proved to be the principal tool in the analytical turn to growth and the workhorse of growth research, not only in MIT's economics department but around the world.

3. Development Economics and the Cold War

At the time of the publication of Solow's neoclassical growth model, MIT was already deeply engaged with growth as a front in the Cold War. Beyond the direct worry about the United States being eclipsed by the East Bloc—1956 was also the year that Nikita Khrushchev declared to the West, "We will bury you!"—the first world of the Western democracies competed with the second world of the communist states for the allegiance of the third world of newly independent and developing countries. A central question was, which economic system would lift the third world out of poverty? Understanding growth was the key to development economics. Rostow had joined MIT as part of the newly created Center for International Studies (CENIS) in 1951. He was joined two years later by the economist Paul Rosenstein-Rodan. The work at CENIS was not mathematical. Rostow and Rosenstein-Rodan understood the growth process to be complex and viewed developing countries as facing special hurdles that impeded any spontaneous convergence to Western standards of living. In an influential article, Rostow (1956) introduced the idea of a "take-off into self-sustained growth" (see also Rostow 1960). In a similar vein, Rosenstein-Rodan (1943) had already argued that poor economies with large agricultural sectors could be moved to a path of sustained growth only through a deliberate massive investment in industrial capital—a "Big Push."

The economists at CENIS were skeptical of the value of formal economic models as guides to development. Rosenstein-Rodan's (1961) essay (originally a CENIS working paper in 1957) did not cite Solow or any other growth theorist directly but, nonetheless, offered a sustained criticism of neoclassical growth theory. Growth models built on production functions that display constant returns to scale were deemed "completely unrealistic," since they implied "no economies of scale or of agglomeration, no entrepreneurship, no phenomenon of minimum quantum or threshold" (Rosenstein-Rodan 1961, 60). Rosenstein-Rodan based his theory of the Big Push on the assumption of indivisibilities in the production functions—particularly in the form of social overhead capital—which generated increasing returns to scale and external economies. The rate of return in each industry, he believed, is influenced by demand from complementary ones, opening up the possibility of coordination failures in which the economy becomes trapped in a low-level equilibrium. The Big Push—a policy intervention that created a coordinated quantum of investment in different sectors—aimed at internalizing the external economies to establish *balanced growth*. (See also the work of his MIT colleague Charles Kindleberger [(1958) 1965, chap. 9].)

International trade, Rosenstein-Rodan (1961, 63–65) believed, could reduce, but not eliminate, the size of the required quantum of investment.

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Here he directly challenged the relevance of Samuelson's (1948b) well-known factor-price equalization theorem: historical experience did not support its implication that wages and rates of return would converge among different countries. Nonetheless, he quoted approvingly Samuelson's (1948b) own suggestion that different production functions and technology in each country caused the observed divergences. The differences in technology were not, in Samuelson's view, a matter of the relative scarcity of knowledge, which he considered a "crypto-explanation": knowledge "is *not* an input such that the more you use of it, the less there is left" (181).

Similarly, Rosenstein-Rodan (1961) challenged another prediction of trade theory: abundant manpower should result in low wages, which should attract capital from abroad, driving up wages and income. However, "lower wages in underdeveloped countries did not attract enough capital to reduce the inequality in factor rewards nor did international trade fully achieve this effect" (66). Orthodox theory had neglected external economies, in the sense that the deficiency of social overhead capital caused "diseconomies on capital account which more than outweighed any economies on wage account" (66).[2]

The difference between the economists of the economics department and those of CENIS were less about the aims of policy or even the underlying causes of underdevelopment per se than about the appropriate methodology for addressing the messy, complex problem of underdevelopment and the relevance of highly simplified theoretical models—differences brought into clear relief in September 1960 at a conference of the International Economic Association held at Konstanz in West Germany to examine Rostow's (1956, 1960) economics of take-off into sustained growth (Rostow 1963). Solow's assessment in the final session of the conference (Rostow 1963, 468–74) illustrates the difficult dialogue between economic historians and growth theorists. Solow taxed Rostow with failing to establish an "orderly relation between economic theory and economic history," which should start with a clear distinction between initial conditions, laws of behavior and parameters. Rostow (1963, xiii–xxvi; see also 1984, 235–37) accepted the challenge: in his after-conference "introduction and epilogue," he tried to give the concept of take-off some added precision. He maintained, nonetheless, that economic theory was a "world of

2. Thirty years later, Robert Lucas (1988, 15–17) would mount a similar criticism of the neoclassical theory of trade and growth.

problems of simplicity," unable to function in the "world of organized complexity" of economic growth (Rostow 1963, xxiv).

Solow, of course, dissented—not because he challenged the notion of take-off, but because he believed that surrendering to the complex details of the growth process was to give up too soon on understanding the causal essence of the problem. He had already suggested a way for the neoclassical growth model to generate multiple equilibria and explain a poverty trap (Solow 1956, 90–91). In the basic model, the steady state is achieved at a unique capital-labor ratio (r) where the need for capital to outfit a growing workforce at that ratio (*capital widening*) exactly equals savings (or investment). In an extension of the basic model, Solow considers what happens if the rate of population growth depends on per capita income, which is itself a function of the capital-labor ratio. Population decreases for very low levels of income; it begins to increase for higher levels of income; and declines for still higher levels. Because of the variable rate of population growth, the needs for capital widening will match the available savings (or investment) at two different capital-labor ratios. Solow shows that the larger of these (r_2) is unstable, in the sense that, if the initial ratio is less than r_2, the economy will tend to return to the lower steady state at r_1, and if it is greater than r_2, the economy will move to a non-steady-state growth path in which the capital-labor ratio increases (*capital deepening*). But the lower ratio corresponds to a low per capita income. The stability of this low steady state marks it out as a poverty trap. If, on the other hand, a Big Push (to use Rosenstein-Rodan's but not Solow's term) could drive the economy above r_2, then the instability would carry the economy toward rising capital-labor ratios with rising per capita incomes—a take-off to a path of self-sustained growth. Solow remarks:

> The interesting thing about this case is that it shows how, *in the total absence of indivisibilities or of increasing returns*, a situation may still arise in which small-scale capital accumulation only leads back to stagnation but a major burst of investment can lift the system into a self-generating expansion of income and capital per head. (91; emphasis added)

Despite being published in 1956, Solow's version of the Big Push reads as if he were reacting to Rosenstein-Rodan's 1957 CENIS essay (published in 1961). Looking back, Solow, in a letter to one of the present authors (Hoover) dated August 11, 2013, denies that he was familiar with Rosenstein-Rodan's earlier essay (1943), but suggests the idea of poverty traps was already well known in the mid-1950s.[3]

Solow's (1956) account of poverty traps illustrates a methodological strategy: keep the models simple, yet adapt them to the demands of the real world. Solow's remark about "other possibilities" illustrates that he regarded his analysis as a "reconnaissance exercise" and a demonstration of the capacities of his model qua tool of analysis. He did not see his models as competing with the work of the historians or development economists, but as clarifying the causal mechanisms that lay behind the facts that they documented. Those facts included the differences in income levels and growth rates between developed and underdeveloped countries. Confirming Nicholas Kaldor's (1961, 178–79) sixth "stylised fact" of economic growth, Samuelson (1964, 757) observed that "in contrast to the narrowing of income differentials within the advanced nations, the divergence between advanced and underdeveloped countries is probably now widening rather than narrowing."

What lay behind the differences between developed and developing countries? To the extent that development economists employed a formal model of growth, it was the Harrod-Domar model, which was interpreted as a neoclassical growth model with a production function with technologically fixed proportions (fixed coefficients) between capital and labor (Harrod 1939, 1948; Domar 1946).[4] The Harrod-Domar model appeared to suit the problems of development, since it readily explained situations in which labor was in excess supply and capital was a binding constraint. It connected growth rates in both the short and the long runs to increases in investment rates and opened up the possibility of a self-sustaining growth

3. On the occasion of the conferring of the degree of Doctor of Humane Letters to Rosenstein-Rodan in 1982, Solow recalled that it was not easy for the relatively small MIT Department of Economics in the 1950s to absorb CENIS, with its large number of senior scholars in one specialized field. Rosenstein-Rodan was a key figure in the process, because he "saw himself and wanted others to see him as someone in the main stream of economics, and not as the practitioner of some rather different art . . . to be measured against different standards" ("Address by Professor Robert Solow on the occasion of the conferring of the degree of Doctor of Humane Letters to P.N. Rosenstein-Rodan," December 3, 1982, Robert M. Solow Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University, box 59).

4. Verena Halsmayer and Kevin D. Hoover (2013) show that the interpretation of Harrod's model either as a growth model or as embodying a fixed-proportions production function (or, indeed, any production function) is untrue to Harrod's purpose or presentation. It remains true, however, that Harrod was widely interpreted in this way and that his analysis was typically assimilated to Domar's.

path from the massive underutilization of a poverty trap to full employment at a high "natural rate" of growth (see, e.g., Boianovsky 2010).

Some of the very features that recommend the Harrod-Domar model to development economists were the direct target of Solow's (1956) paper. In particular, the same mechanism that offered the possibility of self-sustained growth for an impoverished country seemed to suggest the fragility of steady-state growth paths, predicting that developed economies would be susceptible to self-reinforcing collapse. Solow attributed this property to the assumption of a fixed-proportions production function. In reality, he believed, the mix of factors of production responded to price signals, and a flexible-proportions production function (such as the Cobb-Douglas or constant-elasticity of substitution [CES] production functions) would not suffer from the instability problem (see Halsmayer and Hoover 2013). Domar, who had been recruited to MIT in 1958, recanted his growth models of the 1940s and endorsed Solow's approach, with its "less rigid production function" (Domar 1957, 7–8).

By the mid-1960s Samuelson (1964, 760) argued that development economists had no unified theory differing from the dominant growth model, but that they did add to it some "special features." The Solow model predicted that, given some parameters (savings and fertility rates), economies move to their steady states. Because of diminishing returns, countries with lower capital-labor ratios would have a high rate of return to capital. Hence, controlling for savings rates, poorer countries would tend to go faster and converge. But clearly, poorer countries were not converging very rapidly, if at all, toward developing countries' per capita incomes. Samuelson argued that for the "poor countries there must be added the important additional concepts of 'external economies,' 'social overhead capital,' and 'increasing returns'" (752). Neoclassical diminishing returns and constant returns to scale should be replaced in "dynamic economic development" by "*increasing* returns," which "can make it possible for the dramatic spurts and acceleration to occur in economic development" (761–62).[5] Samuelson associated those discontinuities in the development process with Rostow's take-off and, especially, with Rosen-

5. Samuelson, however, did not formalize the argument. After the matter was tackled by "new growth theory," he recalled that "the problem with increasing scale returns is that after you have said the first things about it, it is deucedly hard to find interesting second and third themes to develop. Most pretty modern mathematics lacks elegant applications to the combinatorial swamp that nonconvexity irreducibly entails. My conscience was long bothered by our guild's neglect of a subject on the ground that it is so hard to tackle" (Samuelson 2001, 499).

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stein-Rodan's Big Push—a view that focused on multiple equilibria rather than on convergence to steady states.

Thus there was substantial agreement with CENIS on the goals of policy. But in contrast to Rostow and Rosenstein-Rodan's skepticism about the theory, Solow advocated the flexible adaptation of simple, formal models to different aspects of the development problem. He recognized that the actual substantive application of economic analysis to development would have to move beyond the reconnaissance stage, and that was difficult work. As a consequence, formal growth economics and economic development would fracture into distinct fields.

On the one hand, the fracture was a typical division of labor: the development of methods belonged to growth economics; the applications to policy problems belonged to development. On the other hand, Solow's reconnaissance exercises had already begun to show that the special assumptions needed to fit the neoclassical growth model to developed and developing economies would be different, so that growth economics and development economics were also distinguished by their domain of application.

Seen this way, the debate in the late 1950s and early 1960s over the realism and relevance of the neoclassical growth model gave way to the systematic adaptation of it to the needs of development economics. At the Konstanz conference, Solow complained that no mention had been made of Arthur Lewis's (1954) concept of dual economies in which underemployment in the traditional low-productivity sectors results in an infinitely elastic supply of labor at an exogenous real wage rate (Rostow 1963, 472). By the end of our period, economists in both the economics department and CENIS were following through on Solow's preferred strategy: Stephen Marglin (1967) and Sukhamoy Chakravarty (1969), both at CENIS, and Avinash Dixit (1968a, 1968b, 1973) in the department deployed optimal control theory to investigate the choice of techniques and growth paths in economies with a surplus of labor (Burmeister and Dobell 1970, 408–9). Samuelson's preface to Chakravarty (1969) illustrates the mutually beneficial trade between growth and development economics at MIT.

4. Setting the Agenda

Formal modeling of economic growth at MIT was never an abstract enterprise, but was always motivated by underlying policy concerns, a respect for empirical results, and a goal of providing illuminating and relevant tools. But the models were artifacts in their own right, subject to their own

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constraints and imperatives (see Halsmayer, this volume). Much of the 1960s was devoted to the formal elaboration of the models, but based on an agenda set in the second half of the 1950s.

In the short, final section of his article on the "policy implications" of his growth model, Solow (1956, 93) cited Samuelson in support of the view that the rate of growth should be one of the targets of macroeconomic policy. Samuelson ([1956] 1969, 73) claimed that "with proper fiscal and monetary policies, our economy can have full employment and whatever rate of capital formation and growth it wants"—a position that he viewed as an element of the "neoclassical synthesis" (Samuelson 1961, 807). One fault of the Harrod-Domar model, in Samuelson's view, was that, by regarding technology as fixed, it suggested that economic policy would be helpless to deepen capital and raise per capita income, once surplus labor had been absorbed, and that, once fully employed, society must "accept the growth rate that fate metes out to it" (Samuelson 1961, 809n1; see also 1964, 788). Samuelson (1961, 809n1) argues that it is "misleading to think [as implied by the Harrod-Domar model] that growth is fatalistically determined by some simple formula involving the saving and capital-output ratios." That formula allows a warranted rate of growth that exceeds the natural rate of growth of population and, therefore, cannot last long:

> If deepening of capital were impossible, so that the capital-output ratio could never be increased, fiscal policy would have to be set fatalistically at so expansionary a level as to bring down the percentage of income saved to the level set by the rate of population growth and the capital-output ratio. Society would have to accept passively this fate-given growth rate (plus what technical change will itself bring). (Samuelson 1961, 809n1)

Here Samuelson appears to argue that it is the neoclassical growth model with its flexible capital-output ratio, rather than the Harrod-Domar model, that offers hope for growth-promoting macroeconomic policy. He viewed the Kennedy administration's investment tax credit of 1962 as an application of that conception (Samuelson 1964, 787).

In retrospect, Samuelson appears to have muddled different issues. A flexible-proportions production function does permit capital deepening not available in the Harrod-Domar model. But the neoclassical model, unlike the Harrod-Domar model, builds in the convergence of the warranted rate of growth, given by savings and production technology, to a steady-state natural rate of growth, given by population and the rate of technical progress. At this point, Samuelson's optimism about growth policy seems not to have fully taken on board two challenges exposed by the neoclassical growth model itself.

The first was the "stunning result" (Stiglitz 1990, 52) that the rate of growth in the neoclassical growth model is independent of the rate of savings. In his own paper, the codiscoverer of the neoclassical growth model, Swan (1956, 337–38), had already noted what was only implicit in Solow's analysis: increases in investment (or savings) rates can boost the rate of growth initially and deepen capital, but diminishing returns to factors of production imply that the increased rate of growth plays itself out when the extra output is just sufficient to cover the needs of capital widening.[6] At that point, the economy is returned to the same steady-state growth rate, though at a higher capital-output ratio. A few years later, James Meade ([1961] 1962, 42–45, 110–13) further elaborated what was regarded as a "most paradoxical conclusion." Still, the point took a while to sink in.

Swan (1956, 338–39) remarked that the "anti-accumulation" implications of the neoclassical growth model could be mitigated if the rate of technological progress depended on capital accumulation. Investment could generate external economies, which would render the social yield of capital higher than its private value and bring about increasing returns to scale. Moreover, the rate of population growth might not be independent of the rate of accumulation, which was the "distinctively classical answer." Solow (1956) had already made a related point in his discussion of the poverty trap (see section 3).

The second challenge to Samuelson's optimism about growth-promoting policy was framed by Solow's brief, but influential, foray into empirical research. Using the constant-returns-to-scale production function that formed the heart of the neoclassical growth model, Solow (1957) developed an accounting framework that attributed definite fractions of US economic growth between 1909 and 1949 to the different factors of production. He interpreted the unattributed share of growth as technical progress. The result was deeply surprising to those who saw growth as driven principally by capital accumulation: the growth in technical progress accounted for the lion's share of overall growth in US gross national product. The challenge to the neoclassical growth model was simply that technical progress had hitherto been taken to be an exogenous factor, and an

6. Swan visited MIT in 1958.

exogenous factor does not amount to a substantive explanation. The lack of explanatory force was highlighted by the common practice of referring to technical progress measured this way as a "residual."[7]

Solow (1960) suggested a modification to the neoclassical growth model that he hoped would simultaneously address both challenges. In Solow's 1957 paper, technical progress is *disembodied*—that is, it adds to output merely through the passage of time, as if it did not have to be physically incorporated into the means of production. Solow (1960, 90) sought to relax the assumption that "the pace of investment has no influence on the rate at which technique improves." He allowed the rate of *technological knowledge* progress in this disembodied manner, but modeled it as adding to *productive capacity* only to the degree that it was embodied in new capital goods. The productivity of any particular piece of capital would depend, therefore, on its *vintage*. Faster investment would raise the rate of growth in the economy by accelerating the incorporation of new knowledge into the capital stock, answering the first challenge. And the residual would be—at least partly—explained as an endogenous response to investment rates. Of course, the underlying rate of improvement in technical know-how itself remained unexplained.

Samuelson and Solow were both optimistic about the "new view" of investment (Phelps 1962).[8] Based on his idea of vintage capital, Solow (1962) constructed econometric estimates of the rates of investment necessary to accelerate American economic growth in the following decade. His vintage-capital model and these estimates were used to provide the benchmark for the chapter on growth in the 1962 *Economic Report* of the Council of Economic Advisers, which Solow drafted in his capacity as a staff economist (see Solow 2000).

Optimism that this "new view" of investment would meet the two challenges exposed by the neoclassical growth model was soon dashed. Phelps (1962), who visited MIT in 1962–63, demonstrated that in a vintage-capi-

7. The practice of referring to the component of the measured growth rate of output per capita unaccounted for by capital deepening as the "residual" may be due to Domar (1961, 709). Kindleberger ([1958] 1965, 150) ascribed to Rosenstein-Rodan the notion that the "residual growth" is the result of external economies of scale. Later, in the literature on real business cycle models, the "residual" becomes the "Solow residual" (see Hartley, Hoover, and Salyer 1997, 1998).

8. Samuelson (1964, 737) suggested as much in his observation that "innovations can be introduced faster in a society which is performing *net* investment in addition to the gross investment . . . the society which gets to try out more new things will run that much ahead of the one which does little or no saving."

tal model, a once-and-for-all change in the saving ratio cannot permanently alter the age distribution of capital. Thus it can accelerate the rate of growth in the transition between steady states in the vintage model, which might make a substantial difference over a ten- or twenty-year period, but the steady rate is still determined by the rates of growth of population and of the underlying (exogenous) technical knowledge (Solow [1962] 1964, 108; 1970, 55). Solow ([1968] 1969, 94–97) concluded from cross-section and historical evidence that "fast-growing countries are high-investment countries," despite the theoretical irrelevance of the investment ratio for the determination of long-run growth.

5. All the King's Men

The two challenges were unmet. But they set a twofold agenda for research into formal growth models in the Kingdom of Solovia in the 1960s. One focus would be optimal growth, which had three aspects: (1) the optimal capital-labor ratio; (2) whether it was susceptible to policy intervention—as it was not in the earlier neoclassical models—the optimal growth rate; and (3) the optimal rate of transition between steady states. The second (and not completely independent) focus was to explain or endogenize technical progress.

Rather than Solow and Samuelson, it was MIT graduate students, most supervised by Solow, who implemented the research agenda.[9] MIT also served as a beacon for growth economics, attracting important visitors and new faculty. Karl Shell, a Stanford PhD who had worked with Kenneth Arrow and Hiro Uzawa (on optimal growth), joined the MIT department in 1964 (and remained until 1968). He recalls that

> the MIT pyramid was inverted. At times I was the sole member below full rank. . . . Since the senior faculty was so busy (in Washington and in those capital debates with Cambridge, England), the graduate students had a lot of time for me. (Shell 2001, 708)

Shell recounts a two-way relationship in which students were the sources of ideas, as well as the targets of instruction.

9. Solow supervised the doctoral dissertations on growth of Ronald Findlay (1960), Peter Diamond (1963), Eytan Sheshinski (1966), Joseph Stiglitz (1966), George Akerlof (1966), William Nordhaus (1967b), and Avinash Dixit (1968a). Samuelson supervised David Levhari (1964) and Edwin Burmeister (1965).

5.1. Optimal Growth

How should we evaluate economic policy in a growth model? What could it mean to target optimal growth? As we have seen, the versions of the neoclassical growth model available around 1960 provided no lever for the *rate* of economic growth in the long run. Policy—to the degree that it could influence the rate of investment, savings, or population growth–could influence capital deepening. The question of optimal growth in the long run could, therefore, be turned into a question not about the rate of growth but about the best capital-labor ratio. Phelps (1961) proposed the "golden rule of growth." Taking consumption per capita as the desideratum, Phelps showed that it was maximized when the capital-labor ratio was set to a point such that the rate of profit (identified with the marginal product of capital or the real rate of interest) equaled the natural rate of growth (i.e., the sum of the rates of growth of population and technical progress). The academic year 1962–63 was a "golden year for Golden-Rulers at MIT"; Christian von Weizsäcker, Christopher Bliss, and others "proved all kinds of theorems" in Economics 14.123 (Advanced Economic Theory)—the course taught jointly by Solow and Phelps (Samuelson 1965a, 487n).[10]

The normative target of Phelps's golden-rule analysis, maximum consumption per capita, is straightforward but not economically natural. In a world with capital, there is a trade-off between consumption today and consumption tomorrow over which different preferences are possible. As was soon appreciated, it is most naturally an intertemporal utility maximization problem (e.g., Pearce 1962, 1093). This problem had been solved—though not in the context of a growth model—in Frank Ramsey's (1928) work on optimal saving. Ramsey's paper was well known to Samuelson, Solow, and others at MIT (see Duarte 2009, 170, 172).

Intertemporal utility maximization in a growth model was technically challenging. The translation from the Russian of the book on optimal control by L. S. Pontryagin and his associates in 1962 immediately changed the landscape of growth economics—not only at MIT but elsewhere (Wulwick 1995, 418–21). Edwin Burmeister (2009, 38), a student in Economics

10. From Samuelson's (1964, 748) perspective, the golden rule was a special case of John von Neumann's theorem that a self-reproducing, linear economy has a maximum balanced-growth path, or *turnpike*. If short-run policy could move the economy to the turnpike, it could grow optimally toward some target, at which point short-run policy could move the distribution of production and income to a desired configuration (Dorfman, Samuelson, and Solow 1958, 329–34).

14.123, recalled that the subject of the lecture on December 12, 1962, was optimality problems in capital theory:

> We were shown how to use the Maximum Principle of Pontryagin to solve the famous Ramsey problem of optimal saving over time. In this lecture we covered essentially everything that is contained in the famous David Cass paper that was not published until July 1965!

At Stanford, Uzawa and his students David Cass and Karl Shell quickly incorporated Pontryagin's methods into their own work on the theory of optimal growth. A volume edited by Shell (1967a) demonstrated the application of Pontryagin's technique to a variety of issues in the theory of optimal economic growth. Once Shell joined the MIT faculty, he organized a seminar on optimal economic growth in the 1965–66 academic year, which included several presentations by MIT students. Reactions to Shell's conference volume, however, were mixed. Frank Hahn (1968, 561) complained that the enthusiasm for the new technique often led to neglecting the economics of the models: "Very little is said by any of the authors about the choice of the maximand. Indeed there is something of an unseemly haste to get down to the Hamiltonian."[11]

Samuelson's original vision of applying the neoclassical synthesis to growth models had invoked macroeconomic policy, but the literature on optimal growth focused in the main on the appropriate rates of investment—often with the artificial but convenient invocation of a central planner. The MIT economists Duncan Foley and Miguel Sidrauski (1970, 1971) turned back to the original problem by constructing a growth model in which consumption and investment goods were produced in distinct sectors and in which the effects of alternative fiscal and monetary policies on the non-steady-state growth path of flow and stock variables could be investigated. To achieve the optimal growth paths for consumption and investment while maintaining stable consumer prices, fiscal and monetary policies must induce the capital goods sector to produce investment goods at

11. Hahn was one of the participants at another conference (and monthlong workshop) on optimal growth, led by Arrow in Stanford in July 1965 (see Kenneth J. Arrow Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University, box 28; McKenzie 1999). Conference participants also included Lionel McKenzie, Roy Radner, Tjalling Koopmans, James Mirrlees, Uzawa, and Sheshinski (the only one from MIT), among others. Sheshinski presented a joint paper (with Burmeister) about reswitching. David Warsh (2006, 155–56) reports that optimal growth papers by some young MIT scholars were coldly received at the conference. Apparently, those papers were not included in the Stanford conference program, and were later discussed at the MIT 1965–66 seminar run by Shell.

the optimal rate. The framework presupposes policies operating through private markets, as in the neoclassical synthesis, rather than through a central planner, as in the most common understanding of Ramsey's (1928) original framework for analyzing optimal savings.[12]

5.2. Technical Progress

The focus of research on optimal growth turned out to be on optimal level of the capital-output ratio, and did nothing to advance normative guidance with respect to the rate of steady-state growth. The second item on the MIT agenda, while mainly aiming to open the black box of exogenous technical progress, also offered hope of connecting the rate of growth with economic policy and, so, vindicating Samuelson's initial optimism about the scope of economic policy.

The idea of disembodied technical progress was not a natural one; it found its way into Solow's (1957) paper only as a strategy for measuring as a residual an economic characteristic that was otherwise hard to observe.[13] The idea that technical progress was an economically explicable element of the growth process, of course, went back to the very first chapter of *The Wealth of Nations*, and it found a powerful modern expression in Joseph Schumpeter's ([1911] 1934, 1942) analysis of capitalist development, with its emphasis on "creative destruction." Both Solow and Samuelson had been students of Schumpeter at Harvard. Samuelson credited Schumpeter as an important influence. Solow—perhaps because he first encountered Schumpeter some ten years after Samuelson, at which point Schumpeter's interests were no longer primarily theoretical—was less favorably impressed. In a comment on Samuelson's papers with Schumpeterian themes, Solow (1983, 185) recalled that "Schumpeter was a teacher of mine, too . . . but he either meant less to me or I lack adequate intellectual piety." Samuelson (1964, 742) suggested that Solow's growth model vindicated Schumpeter's argument that innovations were the engine of growth, yet he did not attempt to use Schumpeter's notion of creative destruction to model the pace of technological progress.

12. Whether Ramsey intended the single utility function of an immortal individual in his model to refer to a central planner or to a "representative agent" in the private economy is disputed among historians; see Duarte 2012, 126–30; see also Duarte 2009. An alternative demographic structure (to both Solow's and Ramsey's models) was put forward by Samuelson's (1958) overlapping generations model, later extended to growth economics by Peter Diamond (1965). That was not part of Diamond's 1963 thesis; it was written when he was teaching public finance at Berkeley in 1964. In 1966 Solow recruited Diamond to MIT.

13. It is an old strategy; see Hoover and Dowell 2002 and Duarte and Hoover 2012.

While biased technical progress was an old topic, technical change itself had not been modeled formally. Work at MIT began with visitors—Weizsäcker (1962–63) and Charles Kennedy (May 1964)—who prompted Samuelson to address the notion of *induced* factor bias in invention in some detail.

Samuelson (1965b, 1966) incorporated Kennedy's (1964) *invention-possibility frontier* into a neoclassical growth model. The invention-possibility curve represented a trade-off faced by entrepreneurs between capital-augmenting and labor-augmenting technical progress. He showed that, if the elasticity of substitution is less than one, there exists a stable balanced-growth path characterized by labor-augmenting (i.e., Harrod-neutral) technical progress and constant relative factor shares in output. Samuelson found the results compelling enough that he added a note to the seventh edition of *Economics*, claiming that the theory of induced technical progress met the challenge of explaining all the basic trends of capitalism, including increasing capital-labor ratio and increasing real wages, as well as the near constancy of the capital-output ratio and interest rates (Samuelson 1967, 721n17; see also 1966, 447–48).

Once again, the work of elaboration fell mainly to the younger generation and followed three interrelated streams: (1) the role of publicly supported research and development; (2) the role of "learning by doing"; and (3) the role of privately motivated research and development.

William Nordhaus (1967a, 1967b; 1969b, chap. 6) contributed to the first stream, addressing the role of government in promoting technical progress. Following Uzawa (1965), he assumed that central planners can allocate a fraction of the labor force to research or education and, thereby, push out the invention-possibility curve, which in turn determines the rate of technical progress. A central planner can choose the rate of capital accumulation and the rate and direction of technical progress to maximize present discounted value of consumption per capita. Having solved the central planner's problem, Nordhaus (1967a, 64–65; 1969b, 111–13) asked whether the results would carry over to a competitive market economy, only to conclude that "competition will break down." The problem was that by Euler's theorem, if technical progress resulted in increasing returns to scale, then output would fall short of factor shares in a competitive market in which all factors are paid their marginal products.

The theme of the December 1965 meetings of the American Economic Association (AEA) was "knowledge, production and innovation." At the invitation of AEA president Fritz Machlup, Solow arranged a session that included a paper by Shell (1966) outlining a new model of endogenous technical change based on the assumption that knowledge is a government-funded, pure public good (Solow to Machlup, Solow Papers, box 58). Shell (1966, 63) adopted Arrow's (1962b) view of knowledge as characterized by "inappropriability" and "indivisibility": while it may be costly to produce a new idea, "knowledge can be used by many economic units without altering its character," with but a "small cost of transmission in comparison to the cost of production" (see also Meade 1968, chap. 9; Nordhaus 1969a, 18–19). Aside from the well-known problem of public goods, that the private sector would not receive sufficient incentive to support the socially optimal quantity of technological knowledge, the model shares the characteristic of Nordhaus's model that increasing returns are incompatible with perfect competition, so that technical progress would not be compensated in the market, apparently bolstering the case for government intervention (Shell 1966, 64).

Shell's model also contained a theoretical surprise: almost every point in technology/capital-output-ratio space is unstable; the only exception was a narrow locus of points, the *saddle path*. By and large, Shell's result was unwelcome—the instability was an instance of the same property that Solow (1956) had rejected as implausible and counterempirical in the Harrod-Domar model. Citing M. Maruyama's (1960) characterization of social systems as fundamentally *morphogenic* as opposed to *morphostatic*, Shell (1966, 65) embraced the result, arguing that, while global stability was an "interesting property," it should not be considered "essential" for a growth model. Shell's (1967b, 79) model predicted that, whereas rich countries tend to grow at increasing rates, poor countries tend to decay and might need a Big Push (see also Neher 1971, 199–205). Ironically, Solow's and the economics department's methodology of simple models, to which Rostow had objected at the Konstanz conference, provided yet another rationale for the substantive policies of CENIS.

Focus on the second stream originated in the work of David Levhari (1964, 1966a, 1966b) and Sheshinski (1966, 1967a, 1967b), who approached technical progress not from the perspective of research and development but from Arrow's (1962a) notion of *learning by doing*. Based on, among other things, empirical evidence that productivity in the airframe industry increased regularly with the number of airframes produced, Arrow had suggested, more generally, that technological progress was an increasing function of cumulative gross investment—an unintended, though welcome, by-product of production.

If learning by doing were pervasive, then the economy as a whole would display increasing returns to scale. In contrast to typical cases, these increasing returns would be compatible with perfect competition, because the private marginal productivity of capital is less than its social marginal productivity—learning provides a positive externality uncompensated by the market. Since it did not threaten perfect competition, learning by doing was easy to slot into the neoclassical growth model (see Solow 1967, 38–41).

Levhari's and Sheshinski's models, like Arrow's (1962a) model, generated the surprising result that the rate of output growth is proportional to the rate of population growth. The mechanism is that increasing population stimulates output, which in turn stimulates technical progress (the more doing, the more learning). But diminishing returns to past investment implies that the rate of technical progress must converge to a unique rate compatible with a steady-state growth path, in the same way that capital per head must converge to a unique steady-state rate in an ordinary growth model with diminishing returns to capital. A higher population growth rate that raises the rate of technical progress acts like a higher savings rate that raises capital per head. If population growth stops, then the steady-state rate of output growth falls to zero—with labor input not growing, neither does capital, so that technical progress itself stops.

Solow discussed Arrow's model in detail in Economics 14.123 (Advanced Economic Theory) in 1962 (Burmeister 1962). He extended the model to the "linear" case in which there are no diminishing returns to accumulated production experience—a case that Arrow (1962a, 159) had mentioned but not explored. Burmeister (1962) records in his class notes: "now the savings rate determines the rate of growth." Solow made little of the linear case, which played no part in his survey of production theory (Solow 1967), but it proved to be a touchstone for the "new growth theory" thirty years later (Solow 1997).

While key work on technical progress came out of Stanford (Arrow) and later Chicago (where Uzawa had relocated from Stanford), MIT students remained important players. Shell (2001, 709) recalls: "One summer . . . Hiro [Uzawa] borrowed a dozen students, taking them to Chicago." Although, Shell continues, "friendships and rivalries were strengthened in the Chicago heat," Uzawa wrote to Lionel McKenzie, "I am afraid [the Chicago 1965] seminar might not have had any particular impact upon the students who visited from MIT" (undated memo from Uzawa to McKenzie, Solow Papers, box 58). Underwriting this assessment, Uzawa observed, "I have been enjoying my association with the group of MIT students. We have been working on a couple of projects on economic growth, although none of us have come up with anything concrete" (Uzawa to Solow, June 18, 1965, Solow Papers, box 61). But still, he was "particularly impressed by Joe Stiglitz."

Stiglitz's paper titled "Towards an Endogenous Theory of Technological Change" was presented during his academic year visiting Cambridge University (1965–66) (Stiglitz to Solow, November 2, 1965, Solow Papers, box 60). But the paper did not form part of Stiglitz's doctoral dissertation and was never published.

Shell's (1966, 68) remark in the conclusion to his AEA conference paper characterizes the third stream of research on technical progress—privately motivated research and development:

> The recent contribution of Kennedy warns us that additions to technical knowledge should not be thought of as increasing efficiency in any specified way. That is, the "bias of technical progress," whether in a stylized [competitive] economy or in a planned economy, should be a subject for economic decision.

Nordhaus (1969a), who had already addressed other aspects of technical progress in his doctoral dissertation in 1967, was the first MIT economist to take up the Kennedy-Shell challenge. Nordhaus focused on the role of an inventor, who has a monopoly over his invention for a limited period of time (a patent). The "crucial" and "questionable" assumption of the model is the relationship between the inventive input and the rate of technical change (Nordhaus 1969a, 21). The rate of growth of technology was modeled as directly related under diminishing returns to the number of inventions and inversely related to the level of technology. Through an analogous mechanism to the one by which the rate of population growth limits the rate of technical progress, the rate of output growth in Nordhaus's model limits the rate of invention. The incompatibility between a competitive market and increasing returns to scale is resolved because "information is temporarily monopolized" through patents (24). When Nordhaus presented his paper at the AEA meetings in December 1968, one discussant pointed to a cognitive dissonance: such concepts as a long-run balanced-growth path and a termination of the inventive process because of the end of population growth "do not really belong to [Nordhaus's] Schumpeterian world" (Domar 1969, 44). The other discussant defended the notion of a stable production function for technology (Stiglitz 1969, 46), which had also been attacked earlier in the session by Arrow (1969, 34).

6. The Ebb Tide of MIT Growth Theory

For the fifteen years after Solow and Swan introduced the neoclassical growth model, growth economics was a dominant field, and the MIT economics department was its beating heart. E. Roy Weintraub (1991) has described the process through which a field comes to be defined and delimited as *stabilization*. Growth economics provides a perfect case study. From the 1930s until the advent of the neoclassical growth model in 1956, growth economics had been exploratory in character, mixing various analytical strategies and substantive concerns in a heterogeneous brew. By the early 1950s development economists had begun to adopt the Harrod-Domar model as a standard approach. The neoclassical growth model challenged the analytical framework of the Harrod-Domar model, while Solow's (1957) empirical investigation based on the neoclassical growth model challenged the widespread understanding that economic growth was principally the result of investment in physical capital. The next fifteen years can be thought of as a contest between frameworks (e.g., neoclassical vs. Harrod-Domar, and neoclassical vs. Cambridge, UK; see Backhouse, this volume, "The Other Cambridge") and a process of delimiting the range of applications for different approaches. This is well illustrated by the interests and approaches of the MIT economics department and CENIS. Stabilization was, in part, a (not perfectly complete) division of labor between the methodological contribution of the department (simple, precise, formal models) and the substantive, empirical interests of the development economists and, in part, a definition of domains, with development economics and the growth economics of the already developed countries coming to be defined as distinct and separable problems.

The process of stabilization throws up some characteristic markers. Already by the mid-1960s, Hahn and R. C. O. Matthews (1964) had published a famous and influential survey article—which, in fact, was issued as part of a hardcover book as well. At the same time, neoclassical growth economics became sufficiently intellectually secure that it was incorporated into Samuelson's undergraduate textbook. By the end of the 1960s, a canon had been established: in 1968–69 Solow's (1970) Radcliffe Lectures at the University of Warwick offered a consolidated account of the state of growth economics.[14] Anthologies of important articles on growth began to be published (Stiglitz and Uzawa 1969; Sen 1970). And the

14. Solow's Radcliffe Lectures follow very closely a set of lectures that he developed for his MIT course (Solow 1966).

appearance of the first graduate textbook (by two MIT PhDs), Burmeister and Dobell's *Mathematical Theories of Economic Growth* (1970), began to standardize instruction for the next generation (see also Wan 1971 [Wan was an MIT PhD] and Teixeira, this volume). Growth economics had never been an exclusively MIT project, but many of these stabilizing activities were centered at MIT, and MIT students and affiliates rapidly filled the ranks of economics departments around the world.

The future looked bright. Solow's foreword to Burmeister and Dobell's (1970, viii–ix) graduate textbook suggested that growth theorists faced a busy agenda of important problems. Yet, in retrospect, the end of the 1960s marked the high tide of growth economics. Just as the field appeared to stabilize, it also began to ebb as a preeminent research program. Why?

First, the political climate had changed radically. The Cold War and growthmanship of 1960 had yielded to the first Earth Day and environmentalism of 1970. The publication of the Club of Rome's report *The Limits to Growth*—the product of a research group in MIT's Sloan School of Management—exemplified the shift in mood (Meadows et al. 1972; see also Arndt 1978 and Collins 2000). Growth was increasingly viewed not as the optimistic project of "lifting all boats" but as a threat to life itself.[15]

A second reason for the ebbing of growth economics was the business cycle. Macroeconomic management in the Kennedy and Johnson administrations appeared to be a success. The business cycle expansion that started in 1961 proved to be the longest in US history up to that time. Economists began to ask, "Is the business cycle obsolete?" (Bronfenbrenner 1969). Such questions are leading indicators of a crash: the boom ended in December 1969. The commodity price boom, the first oil crisis of 1973, Nixon's price controls, and the perceived failure of "Keynesian" macroeconometric models to account for the stagflation of the early 1970s moved short-run macroeconomic analysis to the fore. In the sixth edition of *Economics*, Samuelson (1964, 722) had added the sentence,

15. The economics department did not simply capitulate to its colleagues in the Sloan School. Drawing on his well-developed expertise in the characteristics of the kind of multi-equation linear models that informed the *Limits to Growth*, Solow (1973) argued that the crash states that it predicted were built into its modeling assumptions independently of the empirical facts of the world and that it ignored basic economics: as resources became scarcer, their prices would rise, leading both to economies in use and to substitution into alternative factors of production. Solow's criticisms of the Harrod-Domar model found a new target. Nordhaus and Tobin (1972) and Samuelson (1976, 814–17) similarly rejected the doomsday scenarios and reaffirmed the possibility of continued growth. Nordhaus's shift into natural resources economics and climate change was a reflection that externalities are an important phenomenon, but whereas his earlier work on technical progress was on positive externalities, the later was on negative externalities (e-mail from Nordhaus to Boianovsky, May 18, 2013).

"The key word in most economic discussions these days is 'growth.'" In the ninth edition, he not only dropped the sentence but suggested that a short course could omit the chapter on growth altogether (Samuelson 1973, 731).

A third reason for the ebbing of growth economics was frustration that its early promise—precise formal models that would illuminate policy—had not been redeemed. Research strategies ran up against a variety of intellectual barriers. For example:

- For all its supposed importance and for all the effort put into modeling it, Solow (1970, 76) concluded that "very little is known about the exact connection between research expenditure and actual technological progress." Indeed, endogenous growth models put forward by Shell, Nordhaus, Levhari, and Sheshinski at MIT in the 1960s were largely invisible in the renditions of growth economics offered by Solow (1970) and Burmeister and Dobell (1970).
- One of the supposedly great advantages of the neoclassical growth model over the Harrod-Domar model was that it displayed a stable steady-state growth path. Thus, when saddle-point instability was first discovered it was thought to be "potentially fatal" to the whole theory (Dixit 1990, 6).
- Referring to the development of endogenous growth models, Stiglitz (1990, 55) recalled the "stumbling blocks" related to nonconvexities in the production function: "We knew how to construct models that 'worked,' but we felt uneasy making special assumptions" to guarantee the existence of a steady state. Sheshinski wrote that "we already knew in 1965 that a production function linear in capital leads to endogenous growth rate. But this was unattractive because of the knife-edge properties (lack of stability)" (e-mail from Sheshinski to Boianovsky, March 3, 2013). Nonconvexities are incompatible with perfect competition, but there were no aggregative models of imperfect competition available in the 1960s (cf. Nordhaus 1969b, 113).[16]

Finally, and related to the third reason, a point is reached in the history of any field when—for some time, at least—its progress slows markedly. Hahn and Matthews (1964, 890) had detected the signs early:

16. Solow (1956, 71–72) considered the possibility that the marginal product is bounded away from zero (and labor, therefore, is not essential for production). In this case, similarly to the so-called *AK* growth models of the 1990s, the long-run rate of growth becomes endogenous in respect to savings behavior. Such a "pathological" possibility was discussed by Burmeister and Dobell (1970, 33–34), but dismissed as empirically implausible.

> While not disparaging the insights that have been gained, we feel that in these areas the point of diminishing returns may have been reached. Nothing is easier than to ring the changes on more and more complicated models, without bringing in any really new ideas and without bringing the theory any nearer to casting light on the causes of the wealth of nations. The problems posed may well have intellectual fascination. But it is essentially a frivolous occupation to take a chain with links of very uneven strength and devote one's energies to strengthening and polishing the links that are already relatively strong.

Six years later, Solow (1970, 105) still argued that the effort was "not a game." Yet by 1978 he had thrown in the towel, noting the "definite signs that [growth theory] is just about played out. . . . Growth theory is now an unpromising pond for an enterprising theorist to fish in" (quoted in Solow 1991, 393).

The early 1970s did not prove to be the end of growth economics at MIT or elsewhere. Work continued on growth theory, albeit at a lower level. Development economists and specialists in productivity and technology continued to advance empirical studies. But for the time being, growth economics was no longer the field in which stellar reputations were forged. Yet intellectual tides are like natural tides: they flow as well as ebb. Growth theory revived in the mid-1980s in the work of Paul Romer (1986) and Robert Lucas (1988), as well as many others. They addressed many of the same issues that had animated the growthmen of the 1960s, but the center of growth economics was no longer the banks of the Charles River but the shores of Lake Michigan.

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