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The Artifact and the Experimental Report

> All theory depends on assumptions which are not quite true. That is what makes it theory. The art of successful theorizing is to make the inevitable simplifying assumptions in such a way that the final results are not very sensitive. A "crucial" assumption is one on which the conclusions do depend sensitively, and it is important that crucial assumptions be reasonably realistic. When the results of a theory seem to flow specifically from a special crucial assumption, then if the assumption is dubious, the results are suspect.[1]

The "Contribution to the Theory of Economic Growth" started with a methodological preamble. While this introductory statement may sound casual, it self-confidently laid the groundwork for both dismissing the prevalent approach to theorizing about economic growth and opposing one of the best-known treatments of economic methodology at the time. In a widely read essay, Chicago's Milton Friedman had suggested that the "more significant the theory, the more unrealistic the assumptions."[2] According to the conventional reading of Friedman's odd mixture of positivist and instrumentalist rhetoric, if the predictions deduced from a theory were true, then it was possible to proceed *as if* the assumptions were true. In contrast, the "Contribution" was part of a research endeavor that was more invested in models. The preamble introduced the paper of a young, not particularly well-known economist who had been hired by the Department of Economics and Social Science at MIT seven years earlier. It directed the reader's attention, right from the outset, to a specific way of *doing* economics, an "art of theorizing." The explicit focus on theorizing as an activity contrasted decisively with the differentiation between a positive science of economic theory and a normative art of economic policy-making that had been common in the 1930s. The introductory statement hinted at a very specific conception of economics as a craft that was based on a particular set of techniques and skills and imparted a certain practical knowledge. The article did not contain any further methodological explications about "successful theorizing," how to make the "inevitable simplifying assumptions," or about which assumptions might be "reasonably realistic." It performed it.

Edited at Harvard University's Department of Economics since 1886, the prestigious *Quarterly Journal of Economics* published a diversity of contributions both in terms of topics and approaches. The issues before Spring 1956 reflected the varied state of the midcentury economics discipline: An essayistic treatment of the "Soviet capital controversy"; discussions of the American reception of Keynes's theories of interest; an account of "the problem of 'underdevelopment'" from a history of economic thought perspective; a behavioral model of rational choice; new formulations of nineteenth-century mathematical economics; an analysis of bank mergers in the United States in the early 1950s; an investigation of the consequences of David Riesman's *The Lonely Crowd* for the common framing of a rational economic man; and a statistical examination of the problem of raiding among American Unions.[3] The articles ranged over many forms of knowledge – literary descriptions, historical case studies, econometric arguments, anecdotes, algebraic equations, descriptive statistics, numerical examples, geometric reasoning, and thought experiments. Unlike most of the articles published in the *Quarterly Journal*, the "Contribution" contained a significant number of diagrams and algebraic equations in the main text. This setup conformed with what was increasingly taken as economic science. Earlier renditions of economics as a theoretical and scientific enterprise that was distinct from the nonacademic sphere did involve algebraic and diagrammatic formulations but most commonly relegated them to footnotes and appendices.[4] In contrast, the "Contribution" revolved around a mathematical model – its construction, operation, and how it reacted to a series of small experiments. Describing how the model worked and contemplating its interpretation constituted the sole focus of inquiry.

Paying attention to the forms and aesthetics of the 1956 article, this chapter showcases what it means to conceive of mathematical economic models as artifacts. The prime focus of my reading is not the paper's theoretical content or the specifics of the mathematical formulations but rather its particular combination of mathematical equations, prosaic passages, and diagrams. Only this combination constituted the "model," something that, in principle, stood for more than itself. In this sense, the following pages explore what exactly the "Contribution" presented as a model and in which way it did so. The first section analyzes the article's order of events, the specific presence of a narrative authority, and its references to objects beyond the text. It is here that the model appeared as an artifact that was constructed and at the same time attributed an autonomous existence. The second section delineates the ways in which the mathematical system of equations was presented as an economy, in particular how the assumptions of perfect competition and perfect foresight gave economic meaning to the mathematics. Most importantly, the third section concludes, this model world had to function. It was all about the making of a well-working system that could be experimented with. What the introductory preamble called "reasonably realistic" related primarily to the functioning of the model and had only loose ties to the world of economic practices. In the first place, it was about securing the very purpose of the model: Establishing and analyzing a full-employment growth equilibrium. On the whole, the "Contribution" looked straight *at* the model (constructing, setting up, and qualifying an artifact). It did not look at the world *through* the model. That there was a relation between the model and a world beyond, as tenuous as it might have been, was merely suggested by lowering any expectation that it could be practical for policy-making on its own.

Other chapters in this book ponder the power of models to angle reasoning with regard to economic measurement and policy-making. This chapter zooms in on the intriguing developments at play in processes of what is commonly called "formalization." There was one instance in which the "Contribution" looked straight *through* the model, namely when it came to presenting it as an improvement to existing economic theory. The article took up wider discussions among postwar modelers who considered much of the predominantly narrative, nonmathematical economics to be obscure and logically flawed. In the work of institutionalist economics, some modelers saw merely descriptive accounts, bordering on the non- or even anti-theoretical. Restricting proper "theory" to mathematical models, a similar verdict met the (otherwise much admired) John Maynard Keynes. His *General Theory* was widely seen to represent incomplete and vague reasoning in need of clarification. American Keynesians, as they came to be called, transformed his work into a collection of small-scale mathematical and geometrical working objects.[5] The "Contribution" treated the British Keynesian Roy F. Harrod's dynamic theory, published at the end of the 1930s, in a similar vein, presented it *as* if it was a postwar model, and turned it into a special case of the neoclassical approach. The fourth section argues that such histories of formalization fundamentally underestimate the formatting of knowledge in terms of models and the serious losses it involved.[6] The concern with sustained capitalist instability was not easily modeled through a closed system of differential or difference equations; it was first caricatured in the so-called Harrod–Domar literature and then vanished from a growth theory that constituted itself in terms of equilibrium models and, for some time, was regarded as the modeling endeavor *par excellence*. In this way, the “Contribution” was a major step both in marginalizing a different way of economic reasoning and in converting interwar thinking about dynamics, frictions, and crisis into equilibrium models of stable growth.

NARRATOLOGICAL DIMENSIONS

The narrative structure of the “Contribution” was akin to that of an experimental report. Even if readers skipped over the explicit wordings of “experiment” and “experimentation,” they could not escape being taken on a tour through the model on which they encountered an object that could be manipulated with some surprising results.[7] In the first section, the article introduced the reader to the “model of long-run growth.”[8] It laid out its specific assumptions, defined the variables, and put them in relation to each other. In a further step, it described the model’s workings and demonstrated how it reacted to various changes of parameters. The next section extended the system of equations by adding new variables showing how this would affect its fundamental outcomes. In this way, the article familiarized the reader with constructing and manipulating a mathematical model – the “art of theorizing,” which the preamble pushed so emphatically. All verbal formulations served to describe, demonstrate, and explain what happened with the model in the course of experimenting. References to any phenomena or knowledge extraneous to the system’s austere architecture were scarce. Only a short last section, under the heading “qualifications,” discussed differences between what happened in the mathematical model and the realm of economic policy-making (Figure 1.1).

The text contained a strong narrator who directly addressed the readers, especially when it came to investigating the model’s “behavior.” It encouraged them, for instance, to play with the parameters (“The reader can work out still other possibilities”[9]) or to manipulate the graphs (“The reader can draw a diagram . . . in which the growth paths pass to steeper and steeper or to flatter and flatter rays”[10]). During model experiments, the narrator shifted to the pronoun “we”: “inserting (2) in (1) we get . . .,” “we see that . . .,” “we know that . . .,” “these conclusions are . . . just what we should expect.”[11] These instructions, guidelines, and commentaries turned

A CONTRIBUTION TO THE THEORY OF ECONOMIC GROWTH

By Robert M. Solow

I. Introduction

All theory depends on assumptions which are not quite true. That is what makes it theory. The art of successful theorizing is to make the inevitable simplifying assumptions in such a way that the final results are not very sensitive.[1] A "crucial" assumption is one on which the conclusions do depend sensitively, and it is important that crucial assumptions be reasonably realistic. When the results of a theory seem to flow specifically from a special crucial assumption, then if the assumption is dubious, the results are suspect.

Figure 1.1 Title page of the "Contribution." The table of contents conveys the paper's narrative structure.
(Solow, "Contribution," 65)

the mathematical system into an autonomous object that could be used and manipulated – independently of its construction history, by others aside from the modeler. At the same time, the narrator appeared as the model constructor ("the way I have drawn," "I have elsewhere discussed").[12] At least at some points in the article, the model appeared as an economist's personal product. But it already took the first steps into autonomy, open to manipulations and other usages by, in principle, anyone. Those not familiar enough with the algebra could focus on the sections dealing with the basic outline of the model and try to work out the diagrams.

Diagrams did not only make the model visible in a different, perhaps more accessible form than algebraic equations, they played a major role when the paper unfurled as a series of experiments. In fact, the relevant equations were only given in footnotes or were not given at all. The article's central diagram (Figure 1.2) visualized the algebraic equilibrium graphically as the point where two curves intersect in Cartesian space. By drawing variants of this first diagram, the narrator investigated the specific properties of the model, asked what happened if the curves were formed differently, and illustrated the specific properties of the model as they unfolded in the course of experimenting.

By drawing different shapes, the diagrams in Figure 1.3 showed that the stability of equilibrium depended on very particular assumptions about production. The model experiments depicted in Figure 1.4 consisted of making small changes *ceteris paribus* (that is, leaving everything else unchanged) in order to see whether the economy still found its way to the equilibrium growth path.

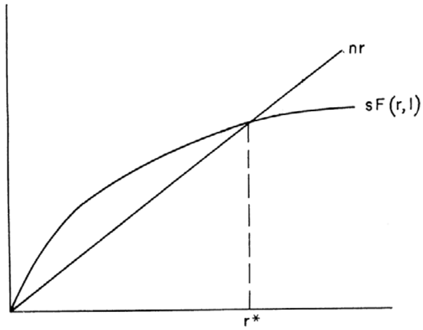
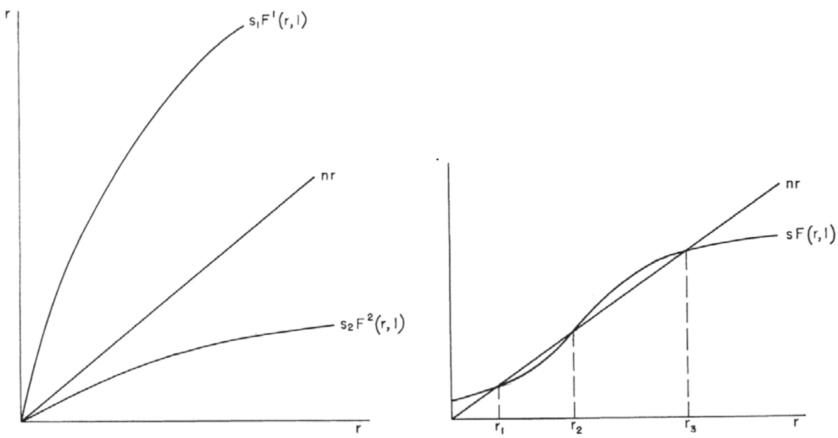


Figure 1.2 The central visualization of the model. r*, the point where the two curves intersect, signifies the equilibrium growth rate.
(Solow, "Contribution," 70)

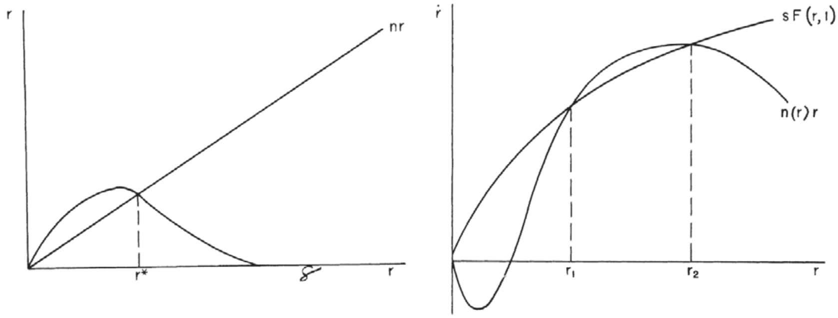


(a) (b)

Figure 1.3 Drawing different shapes.
(Solow, "Contribution," 71 and 72)

Beyond simply illustrating the equilibrium mechanism, the diagrams fostered a different level of understanding than did the mathematical equations. They were essential in capturing the workings of the mathematical artifact – in an almost literal sense. Readers were not only told how the model worked, they also had the chance to turn into model manipulators themselves and experience the sensual character of formal operations.[13]

To really get a full grasp of the model, the reader had to combine algebraic, graphic, and verbal elements.[14] Diagrams included labels for axes and graphs in symbolic form, which referred to the model's



(a) (b)

Figure 1.4 Experimenting with variables.
(Solow, "Contribution," 89 and 90)

parameters (or they did not, as the axes in Figure 1.2 show). They depicted the relations between parameters but did not further characterize them. In principle, as sociologist Andreas Langenohl has highlighted with regard to static neoclassical models, these relations could be read as depicting a synchronous ("to the same extent as"), temporal ("given that"), or causal ("if, then") relation. In the "Contribution," only the surrounding text established that relations were to be read as causal relationships in time.[15] Likewise, prosaic passages made sense of the diagrammatic manipulations as experiments with an *economic* model. At the very outset, the mathematical variables K, L, and Y were introduced with the rather colloquial terms of "capital," "labor," and "output." In this way, the mathematical system turned into a "model economy."[16]

The verbal accounts in the "Contribution" that revolved around the mathematical object were not just a descriptive part of the mathematics but qualified what was happening in the model. In this way, they were central in making mathematical economic modeling seem like the construction of new artifacts. In contrast to contemporary publications in game theory or the emerging general equilibrium theory that devised more sophisticated formal systems, here mathematical proofs occupied only very little space. Instead, there were substantial blocks of text that contained no symbols but explicated in plain language the economic rationale of what was going on. Statements such as the following made sure that mathematical equations had economic content:

Alternatively (4) [$L = L_0 e^{nt}$] can be looked at as a supply curve of labor. It says that the exponentially growing labor force is offered for employment completely inelastically. The labor supply curve is a vertical line which shifts to the right in time as the labor force grows according to (4). Then the real wage rate adjusts so that all available labor is employed, and the marginal productivity equation determines the wage rate which will actually rule.[17]

In this way, the mathematical equation turned into the behavior of the supply of labor over time. This also meant to redefine verbal terms in a very specific way: "labor force" was a vertical line, a mathematical concept. The verbal account detached everyday terms from their common meanings and filled them with new conceptual content in relation to the mathematical object. Moreover, it ensured that the system of equations made sense as a highly specific economy in which, for instance, an "exponentially growing labor force" existed. From this perspective, the ominous "assumptions" (for instance, that labor was supplied inelastically) that necessarily belonged to any model did not so much simplify, abstract, or idealize "the real world." Rather, they were crucial in providing the constructed mathematical system with some economic meaning (however detached from the world of economic practices).

Despite its formal precision, a model can still be indeterminate in the sense that it is open to various interpretations. The central component of the growth model, for instance, was a production function: $Y = F(K, L)$. The mathematical formalism spoke of some variables, homogenous somethings called "output" (Y), "capital" (K), and "labor" (L). The verbal account explicated: "There is only one commodity, output as a whole."[18] The production of aggregate output was like the production of one good. Ambiguities remained, as the article left open whether the variables were to be thought of as aggregate measures, composite numbers (comprising the production processes of several firms), or hypothetical figures of one singular firm producing one good. The model's polyvalent equations were open to different textual explications: Was K, the composite capital, to be read as the result of the actions of rational economic agents, was it more like a summary statistic, or both? This ambiguity would become central in the life of the model after it was published, both as a focal point of critique and as a major driver for the model's manifold adaptations. In the years to come, even the modeler himself looked at the model from different points of view, interpreting the its variables as being based on individual businesses' behavior in a general equilibrium in some instances and as observable aggregate entities in others.[19]

A VERY SPECIFIC ECONOMY

Taking mathematical system, verbal accounts, and diagrams together, the model in the "Contribution" portrayed a particularly smooth-running and manageable world, which later became the dominant image of growth in economic reasoning. The inputs of production (capital and labor) were both "homogenous," meaning that there was no difference, say, between manual and intellectual work or between variable or fixed capital. Moreover, there were no such things as energy, land, or any other non-augmentable resources. Output equaled the "community's" real income.[20] At every instant in time, part of the output was consumed while the rest was saved and immediately invested. This meant that savings equaled investment in new capital at every instant.

The equations for production were encapsulated by the quintessentially neoclassical assumption of *perfect competition*.[21] In this way, the model relied on the presumption of fully competitive markets in which flexible prices and wages kept supply and demand (for capital and labor) in equilibrium. The so-called marginal conditions applied, meaning that prices straightforwardly reflected relative productivity: Capital and labor could just be substituted for one another whenever relative prices changed. Consequently, there was constant full employment of both labor and capital. The rationale for this assumption was that it provided a story of why the rate of interest and the wage rate could be directly derived from the production function. Thus, money was excluded from the model, and production turned into a mechanical combination of physical inputs and physical output.

These assumptions about perfectly flexible production laid the foundation for investigating growth – in the very specific sense of a self-sustaining growth equilibrium. The relevant expression took the form of a linear differential equation. It determined "the time path of capital accumulation" by the movements of output and saving.[22] Labor was given exogenously, which meant that it was given outside the model through a constant rate of population growth. All available labor was employed: Whenever the supply of labor changed, the relative prices of capital and labor changed. Capital reacted flexibly to these changes; whatever the necessary relation to ensure efficient production, capital adapted in the appropriate way. There was "always a capital accumulation path consistent with any growth rate of the labor force."[23]

To make sense of the notion of time that came with the linear differential equation, the "Contribution" assumed *perfect foresight*. Essentially, this meant that the future development of interest rates was known in the present and allowed for perfect arbitrage over time. Differences in prices and interest rates in the future were efficiently utilized today. Plans were always realized: There was no risk, no uncertainty. Speaking with Deirdre McCloskey, a solvable linear differential equation implied "a rather brute form of time, a mere chronology." The equilibrium growth path was independent of initial conditions. The system already started to run in equilibrium. "Behind a differential equation lies the idea of a timeless process."[24] Historical dynamics, frictions, and contingency were thus excluded. The central characteristic of the time path of the growing economy was that it was, in a specific sense, mathematically clear-cut and tractable.[25]

The wondrously smooth world of perfect competition and perfect foresight featured a growth equilibrium in which capital and labor were always fully employed in an unchanging relationship.[26] This long-run equilibrium, the "balanced growth path," was proven to be stable, "whatever the initial value . . ., the system will develop *toward* a state of balanced growth."[27] If the growth rate of labor increased, the system adjusted and asymptotically "approach[ed] a state of steady proportional expansion."[28] In this economy nothing essential evolved or transformed during the growth "process." The use of a composite commodity made sure that the growing economy only changed in scale but not in composition. Accordingly, the growth rate of output could be permanently increased only through exogenous factors that were not part of the inner workings of the model: Alterations of the model government's policy, demographic changes, or "technological change."[29] All these changes came from the outside and did not modify anything inside the efficient, timeless system of physical relations.

The main purpose of the "Contribution" was to lay out the conditions that enabled the mathematical possibility of a stable equilibrium growth rate. For such a mathematical equilibrium to exist, the strict differentiation between effortlessly working production on the inside and all kinds of actual dynamics on the outside was essential. Internally consistent and manipulable, the model appeared as a small-scale mathematical object. It demonstrated a stable growth equilibrium, which came with a neat storyline about a smoothly working economy in which everything just added up. As we will see, things became messier once the narrator dared to move away from the realm of the model.

WEAK KNOWLEDGE

The epistemological conundrums of such a parsimonious model were at stake in the last section of the paper. They appeared as rather casual, seemingly mundane comments from the vantage point of the practitioner. These comments dealt with those differences between the model and the world that the modeler deemed obvious. They gave an impression of what "reasonably realistic" might mean and conjectured about the (non)uses of such a simple model for thinking about policy. While the better part of the "Contribution" was about presenting and highlighting the model's characteristics, the final paragraphs emphasized it was merely a model and hinted at the fragile character of what it had termed "experimental" knowledge in the previous pages.

Today, economic papers circling around a model usually end with a section of conclusions or policy prescriptions. This article ended with "Qualifications," a section that was quite outspoken: Neither was the economy the paper presented even approximately believed to be real, nor was the model of any use for directly drawing policy conclusions. Here, the narrator most evidently appeared as the model constructor: "I have been deliberately as neoclassical as you can get." Compared to today's standards of economic writing, this modeler was rather careful about drawing conclusions from the model that related to something beyond its confines. The absences they emphasized as relevant belonged to another realm of economic theory dealing with so-called Keynesian income analysis."[30] Clearly, the model eclipsed all kinds of "difficulties and rigidities," but it was "not [the modeler's] contention that these problems don't exist, nor that they are of no significance in the long run."[31] There was no assertion of the real-world-existence of a market creating spontaneous order as a self-regulating system. Quite in contrast to the model, which always adapted to equilibrium, the qualifying remarks highlighted that it "may take deliberate action to maintain full employment ... via tax, expenditure, and monetary policies."[32] "Economic stabilization" relating to the ups and downs of the business cycle or high unemployment rates would require different models. One could discuss how the short-run features of such "Keynesian" models might impinge on the neoclassical long-run world of growth and investigate what would happen if the conditions chosen in the paper did not apply.[33]

When it came to the relation between the artifact and policy questions, it was all too obvious that the portrayed economy was so tremendously specific that any relation to a world outside the model had to remain rather vague. This, however, did not mean that the "Contribution" was silent on economic governance. On the contrary, it situated the model within the context of short-run policies that determined, among others, the extent to which an economy was imagined to be able to achieve full employment. The advantage of such "highly abstract analysis" was that it provided a "theoretical counterpart" to "practical possibilities."[34] If short-run policies worked out, the smoothly running neoclassical world of perfect competition, this "land of the margin," provided a framework for discussing the relationship between investment and consumption, between present and future consumption, as well as the various factors that would affect that relationship.[35] In the parallel publication of another neoclassical growth model, the Australian economist Trevor Swan made explicit the belief that would become central for the so-called neoclassical synthesis and highly relevant for macroeconomic planning: "When Keynes solved 'the great puzzle of Effective Demand,' he made it possible for economists once more to study the progress of society in long-run classical terms – with a clear conscience."[36] In neither of the models was the assumption of full employment an expression of a belief that the free market would eliminate unemployment. They complemented the vision of a patchwork of models rather than one overarching mathematical framework, which could integrate all kinds of aspects (such as the short- and long-run, individual actions, and macro dynamics). What one model could not handle might be depicted in another one. However, such statements were ambiguous, as models were open to several interpretations. The catchy point that the growth model was the "neoclassical side of the coin" could easily be interpreted in different ways.[37] It could promote, for instance, the idea of an overarching framework integrating both sides of the coin, which was exactly what happened in the revival of neoclassical growth theory from the 1980s on. Another way to read the statement was that it strictly separated between the factors that were seen to promote long-run development and those that contributed to short-run issues like full employment.[38]

In an equally loose manner, the "Contribution" elaborated on the central argument of the introductory paragraph, that assumptions needed to be "reasonably realistic." In the first place, the argument was a side blow against what it called the "Harrod–Domar line of thought," the dominating strand in economic growth theory at the time.[39] Overturning its "dubious" assumption of a fixed relation between capital and labor, the article built on the alternative assumption that inputs could be substituted flexibly. What made it reasonably realistic, in the modeler's reasoning, was that in the long run it was just not reasonable that the relation of inputs was fixed. The underlying notion of what was realistic was a matter of plausibility rather than of econometric testing, empirical falsifiability, or predictive accuracy. The "Contribution" did not contain any empirical support of the model's realism; neither did it try to maintain that any of the other assumptions was particularly close to a reality. In the end, the "Qualifications" underlined that what was reasonably realistic depended on the specific purpose a model was thought to fulfill. The purpose of this model was not to present a "credible theory of investment" but to investigate a stable long-run growth equilibrium.[40] This involved the model's ability to check how the growth process worked without the assumption of flexible inputs. Before this background, reasonably realistic meant something like *as long as it does not endanger the workability of the model.* When legitimizing the chosen shape for the production function (Cobb-Douglas), for instance, the modeler argued frankly that it was "tractable" and avoided the complications of a more general shape.[41] The more practical shape involved the assumption that innovations came from changes in relative prices, happening exactly when they were due.

My reading of the "Contribution" in the preceding pages illustrated how modelers, more generally, see the key quality of their working objects not in their truthfulness but in their ability to effectively pose a problem. For the case for model organisms, it has been argued that they offered opportunities for interaction and experience because they could not be entirely controlled.[42] For mathematical models, their main quality was some kind of efficiency in expression. Or as the mathematician Marc Kac has put it: their "main role . . . is not so much to explain and predict . . . as to polarize thinking and to pose sharp questions."[43] What economists presented as "simple" models made it possible to raise precise questions due to their specific form, which imposed constraints on reasoning. The "Contribution" demonstrated the restricting and opening effects of such models, as they have been analyzed in the literature on modeling as a scientific practice. On the one hand, models determined the readers' "freedom to maneuver," as philosopher of economics Marcel Boumans has framed it: What they could do with the curves in the diagrams, how they thought with and through the model.[44] Reasoning with the neoclassical growth model was restricted to the confines of a mathematical equilibrium in a "frictionless, competitive, causal system."[45] On the other hand, despite all the strategic simplifications on which it was based, a model is more than just a well-defined version of modelers' pre-existing ideas. Even though it conforms to some extent to the modeler's intentions, she does not necessarily know the results of her investigations in advance. In model experiments, the modeler–economist and the model are, to use Mary Morgan's words, "jointly active participants."[46] She has argued that experiments just as the ones in the "Contribution" did not actually confound the modeler; the results might be unexpected but were still explicable given existing knowledge.[47] Yet playing with the model still prompted new ideas and coined new concepts. Retrospectively, Robert M. Solow, the modeler of the "Contribution" at least claimed he had been surprised that the model's steady-state rate of growth was independent of the savings rate: "I thought it was a real shocker. It is not what I expected at all."[48]

Compared to its strong presentation of the model as an experimental artifact, the article's reference to any real growing economy was rather feeble. There was no explicit attempt to show the mathematical system as a representation of some quantitatively measured entity. There was no empirical test, no reference to empirical studies. The "Contribution" can be read as making a case for much looser and more informal knowledge. Its major achievement was to separate out a smoothly running mathematical system: On the inside was a mechanism of steadily increasing physical production; on the outside were all the factors that were able to impact but never to disturb the sheltered sphere. When readers accepted the invitation to engage with the model, they ventured on an undertaking that, at least for the moment, suspended all kinds of complications and uncertainties. The many difficulties of linking the model to a real world were not simply shortcomings. They contributed decisively to the model's productivity. Proponents and critics alike built on its blunt portrayal, sought to deal with its shortcomings, provided it with different meanings, or used it as a comparative foil to build alternative worlds. The following chapters will demonstrate various interpretations and uses the model provoked. Moreover, the model lured reasoning into its narrow confines when it came to past theorizations of growth. It is here that the following section locates the suggestive power of models.

A MODEL STORY OF FORMALIZATION

For the professional readership, which the "Contribution" found rather quickly, the article signified a substantial contribution to modern growth theory and capital theory, most often mentioned alongside Trevor Swan's and James Tobin's similar models.[49] Sometimes it was singled out as the "most important paper" that contributed to a "major revision" of the widely held "Harrod–Domar" belief.[50] Such readings conformed with the article's own presentation of the neoclassical growth model as a remedy to the pitfalls of existing growth theory. The story went something like this: The conventional, so-called Harrod–Domar model dubiously assumed that the relation between capital and labor never changed (a fixed capital coefficient). Due to this assumption, it portrayed a peculiar "knife-edge," the suspicious "tightrope view of economic growth."[51] In this world, any step away from the equilibrium growth rate led inexorably to mass unemployment and depression. Against the foil of the pervasive Harrod instability, the "Contribution" assumed that, due to perfect competition, inputs were substitutable. This neoclassical angle guaranteed that deviations from the equilibrium would be self-correcting. Having corrected the problematic assumption, the model now featured stable growth instead of crisis.

Until today, the history of growth theory as it is usually told by economists – critics and supporters of neoclassical growth theory alike – follows this narrative.[52] In a joint paper with Kevin Hoover, I have argued that review essays, textbook accounts, and historical introductions to economic articles have told and retold some version of how the field progressed from a deficient first model to the neoclassical one that laid the foundation for modern growth theory.[53] One particularly widely read version of this narrative is found in one of today's major mainstream textbooks on economic growth theory. It states that Harrod "used production functions with little substitutability among the inputs to argue that the capitalist system is inherently unstable. Since [he] wrote during or immediately after the Great Depression, these arguments were received sympathetically by many economists. Although these contributions triggered a good deal of research at the time, very little of this analysis plays a role in today's thinking."[54] Likewise, historians of other fields, when they tapped into the history of economics, reverted to such potted histories.[55]

Ironically, the primary target of the "Contribution," Harrod's "Essay in Dynamic Theory" (1939), did not subscribe to the image of economic growth that it was said to uphold. Most notably, it dealt with a different time horizon focused on capitalist instabilities and was preoccupied with the question of stabilization policies.[56] It did not even contain a model in terms of a small-scale and manipulable mathematical object. Published in 1939, Harrod's essay adhered to the style and form of British Keynesian and Marshallian economics before the Second World War. It used mathematics in a limited way. The reading of Harrod's work in the "Contribution" in terms of a mathematical model meant imposing a different way of reasoning, which decisively transformed the essay's contents. Looking through the lens of mathematical modelers, the essay amounted to utterly vague thinking in need of being clarified – through a proper model.

When reading the "Contribution" and Harrod's essay side-by-side, it is difficult not to see neoclassical economics appropriating older-style Keynesian argument, providing it with a new form, and hereby substantially changing its content. The growth equations in the "Essay" provided specific reasoning tools; it even included the terminology of "tool of analysis."[57] It did present a couple of algebraic equations, which, not unlike the "Contribution," related the growth of output with capital accumulation. But neither were the variables part of a well-defined mathematical system nor was it based on any specific assumptions about the growth of labor, the conditions of production, or the factors that might influence the development of the cycle. It did mention all these aspects and wove them into verbal explanations of the cycle, but they were not part of the growth equations. Paired with some numerical examples to exemplify the argument, the essay's equations offered a loose framework for discussion of what might happen if various factors in economic life led to other circumstances than those expected by entrepreneurs. These reflections were exclusively verbal; the equations themselves were not touched in the process.[58] Due to the obvious differences, many critical readers have pointed out the crucial "misunderstandings," "misinterpretations," and "misrepresentations" in the modelers' reception of Harrod's essay.[59] The soon dominant approach to economics came with blind spots and angled historiographies as part of a larger postwar culture of reading prewar works that had made do without mathematical models as central working objects.

The specific reading of Harrod's 1939 article in terms of a growth model developed within an extensive literature on macrodynamics and growth in the immediate postwar period. During the Second World War, the resources of economics had changed decisively: New data had become available, among them national income accounts that presented coherent and clean figures of "the whole economy" (which will be central to Chapter 2), and the new mathematical techniques of wartime planning started to trickle into economic analysis and were about to fundamentally change the idea of economic dynamics (as will be discussed in Chapters 3 and 4). Most readers expressed some kind of doubt as to "what Harrod actually meant." Thomas C. Schelling, at the time a teaching fellow in economics at Harvard University, for example, wondered "whether [Harrod] meant to imply that [investment] actually would be proportionate to income, or only meant to give it formal expression."[60] They wondered about the status of the equations, criticized supposedly unclear notations, and struggled with apparently incomplete definitions.[61] Also Solow, in a letter to a colleague in Cambridge, England, spoke of his "uneasiness in this Harrod maybe–maybe land of equilibrium growth, in which one never knows anything about the behavior–dynamics of the system."[62] Akin to the way in which modelers transformed Keynes's *General Theory* into a collection of small-scale mathematical and geometrical working objects, mathematical economists brought Harrod's work into the form of a system of equations.[63]

Reinterpreters, such as William Baumol, were convinced they would merely make implicit assumptions explicit and "Mr. Harrod's system work the way he says it does."[64] A particular strategy was to use the "clearer" notations and formalisms of Evsey D. Domar's "Capital Expansion, Rate of Growth, and Employment," published in 1946 in *Econometrica*, the main outlet of the new mathematical economics.[65] In that paper, Domar had investigated capitalist difficulties in creating and maintaining full employment via a mathematical system that portrayed the relation between capital accumulation and employment.[66] In contrast to Harrod's essay, here the primary category of reasoning was an equilibrium growth rate that would ensure full employment.[67] On their clarifying, mathematical economists formulated a "complete model" with a specified production function and merged it with Domar's model.[68] In this way, Harrod's work turned into a system of difference equations. Many hands constructed the new Harrod–Domar model, which became one of economists' predominant working objects in the postwar period. Already at the end of the 1950s there was the complaint that "countless craftsmen" had turned the Harrod–Domar model into the "most over-worked tool in economics," and it continued to be widely-used.[69] Providing crucial argumentative material for justifying catch-up development policies in the 1960s, it would remain ubiquitous in development economics for the following decades.

The formalization of Harrod's work crucially changed the meaning of some of his concepts. Only in the form it was provided by postwar modelers, it featured the very implications they so vigorously rebutted. They introduced the assumption of a constant capital coefficient, readily referred to as the "constant capital coefficient as employed by Domar and Harrod,"[70] which led to the "straight and narrow paths from which the slightest deviation spells disaster," the "Hiccup" dynamic and its "dismal," "gloomy," and "masochistic" prophecies.[71] That literature's assumption of a constant capital coefficient was the very assumption that the "Contribution" took issue with and, as a neoclassical contribution, replaced it with a flexible capital coefficient. Harrod himself vigorously rejected the interpretation of his work as an equilibrium model of growth throughout his life. His ideas about the instability of capitalist economies were fixed into a mathematical equilibrium system, subsumed as a special case in a more general neoclassical model. "I found myself in the position of Le Bourgeois Gentilhomme who had been speaking prose all his life without knowing it. I had been fabricating 'models' without knowing it."[72]

Harrod's protest aptly captured the irony of having been turned into a precursor figure, someone who seemed to have invented the current state of affairs in earlier times – yet with some crucial flaws. Precursors have been criticized by historical epistemologists as essential figures in the "triumphal epic" of scientific progress.[73] As a "false historical object," the precursor results from replacing "the historical time" of scientific inventions with their "logical time."[74] The figure of the precursor makes it seem that concepts, objects, and techniques, which are particular to time and place, were the same all along and just became more and more refined.[75] Notably, also modeler-economists referred to *Le Bourgeois gentilhomme*, but in the affirmative, claiming that economics had always been a modeling endeavor. "Like Molière's M. Jourdain and his prose, economists have been doing linear economics for more than forty years without being conscious of it."[76] This was a quite natural assumption for economists, who were interested in modeling whatever they conceived as their object. Absorbing different kinds of knowledge in model form meant a process of highlighting and neglecting, valuing and devaluing, translating and passing over what seemed to be incomprehensible, a process that was guided by the specific requirements of mathematical systems to ensure the existence and uniqueness of equilibria.

While the implicit metanarratives might be problematic from the historian's point of view, such reinterpretations and reappropriations are integral features of research. Research, as Gaston Bachelard has emphasized, advances not by rejecting its past but rather by devising something new that cultivates a connection with its past.[77] The received history of growth theory adopts the perspective of economic research and directs its major interest at the object of "economic growth," not the contingent ways in which economists have shaped and reshaped that object. There are no losses in such narratives, just gains – more clarity, more rigor, more science. The past is recounted as a series of steps toward a *status quo*, in which "Harrod" – as personification of his deficient approach to model growth – is made out to be a precursor of "Solow" and, subsequently, of what is now conceived as modern growth theory. (The same, for that matter, happened to "Solow" in the revival of neoclassical growth theory from the 1990s onwards, as recounted in the Epilogue.) In popular accounts, the now canonical story turned into a heroic tale of the young economist Robert M. Solow who was dissatisfied with existing accounts of a crisis-prone capitalism and revolutionized the subject by creating a model that showed the stable long-run progress of economies.[78]

POWERFUL INSTRUMENTS OF MISEDUCATION

Economists like Solow or Domar pushed model-based reasoning primarily on economical grounds. Precisely because growth was "determined by the very essence of a society," including the "physical environment, political structure, incentives, educational methods, legal framework, attitude to science, to changes, to accumulation," the economist needed "highly simplified symbolic models." In addition to more general treatises, Domar argued at the beginning of the 1950s, these models provided "extremely useful instruments" to eventually arrive at a "workable theory of growth." Such focus on usefulness and workability did not fully legitimize all kinds of assumptions. In fact, Domar maintained, deciding which factors to include and leave out was "the very essence of theorizing."[79] That the design choices in the process of modeling were not merely practical ones but had profound political character is one of the most persistent strands of criticism – in particular when it came to the ideological underpinnings of neoclassical theories of production and distribution.

Soon the most famous critique of neoclassical modeling in this vein was uttered by Joan Robinson, lecturer at the University of Cambridge, England. Already world-renowned for her contributions to the economics of imperfect competition and the further development and exposition of Keynes's *General Theory*, she went on to translate Marx into academic economics. Robinson's was not a general criticism that models were unrealistic because they simplified a more complex reality. She was not opposed to modeling or mathematical reasoning in economics per se. In *The Accumulation of Capital*, she presented a formalized account of economic growth in a 1930s British economics style. As common for Marshallian economics, diagrams and formulae were safely stashed away in the appendix. While the book was received widely, several reviewers emphasized that it was a "book written for economists," which made "no concessions to the ordinary reader," relied on "equational thinking," and basically consisted of a "model," an "ideal abstraction" warranting the question of whether it was even "supposed to correspond to reality."[80] The book extended the Keynesian concept of effective demand into the long-run and investigated capital accumulation and its instabilities. Most importantly, Robinson emphasized the determining role of both historical configurations as well as political processes, which she posited against the notion of a self-sufficient economic mechanism given in definite laws. This was also one of her main arguments in an article, which, in a sweeping blow, contested the use of a homogeneous capital substance in conventional production functions.[81] Both the mathematical production functions of neoclassical theory and contemporary measurements of capital within the framework of national income accounting utterly disregarded the material realities of production. Assuming perfect competition, theoretical and empirical production functions constructed a singular entity called "capital," measurable in terms of monetary values. In contrast, Robinson's argument went, capital goods in production were physically heterogeneous and precisely not to be confused with the (money-)value of capital. That value varied with the rate of profit, which in turn was given by the rate of capital accumulation and therefore depended on social institutions (such as private property or the existence of different classes). From this perspective, excluding the social and political embeddedness of economic processes from neoclassical imaginaries made them effective instruments for obscuring political–economic realities.

The basic argument at the time for introducing mathematical models was that it equipped economic reasoning with more precision, the formalization of Harrod's work being just one example. Robinson, in contrast, argued that neoclassical models were logically inconsistent. They did not include a reasonable determination of the rate of profit, of income distribution, and the role of saving. Against the proponents of formalization she made the case that the neoclassical treatment of production was indefinite, somewhat lazy, and beset with serious intellectual flaws – precisely because it was so clear-cut and easy-going.

> The production function has been a powerful instrument of miseducation. The student of economic theory is taught to write $O = f(L, C)$ where L is a quantity of labor, C a quantity of capital and O a rate of output of commodities. He is instructed to assume all workers alike, and to measure L in man-hours of labor; he is told something about the index-number problem in choosing a unit of output; and then he is hurried on to the next question, in the hope that he will forget to ask in what units C is measured. Before he ever does ask, he has become a professor, and so sloppy habits of thought are handed on from one generation to the next.[82]

Robinson's foreboding became reality when mathematical models were widely accepted as the new epistemic standard. Ironically, as highlighted in the Epilogue, the modelers who had brought them into the world also bemoaned the restrictive thought of later generations of economists, who had, from their first steps as economics students, been drawn into model worlds.

In the mid-1950s, neoclassical economics was not yet the new mainstream, mathematical models were not yet the primary form of economic argument, and Solow was still a junior professor preoccupied with bringing together new mathematical techniques with what he thought of as conventional economic theory (Chapter 4). He seems to have already identified with a new neoclassicism and felt part of a group of modelers who he thought were entirely misunderstood. Against that background, he engaged with the more prestigious scholar's criticism. Robinson's article and Solow's riposte retrospectively turned into early episodes of what came to be known as the "Cambridge Capital Controversies." There is no reason to recount the enfolding exchange between Cambridge scholars on the two sides of the Atlantic; others have accounted for its theoretical stakes and the boundary work it contained.[83] The point here is simply to highlight the intrinsic entanglement of epistemic and political argument.

After reading Robinson's critique, Solow wrote to one of her colleagues: "I am not at all epaté by the political propaganda; but I am a little put off by the withering attacks on what may be neo-classical stinking fish in England, but bears no visible relation to any doctrine taught on this side of the Atlantic."[84] An article sought to set the record straight. In a manner that foreshadowed his argumentative style in later exchanges about the politics and ideology of economics, Solow made a case for the tool-like character of mathematical models: He defended the assumption of a one-commodity economy not by arguing that it was somewhat realistic but that it was simply "useful" to assume only one kind of physically homogeneous capital good.[85] Then output and capital could be measured "in the *same* units." He agreed with Robinson that, when measuring production processes, there was only a small range of examples for which the different capital inputs could be captured by a single index-figure of capital-in-general. But this kind of realism was not what his modeling and measuring work was about. He emphasized the difference between concepts related to the model (like the return to capital) and concepts he related to "the real world" (like the income of capitalists).[86] Similar to his idea of an assumption being "realistic enough," as discussed in the previous sections, he emphasized that the most important question was whether an assumption suited the concrete purpose of a model. With the special focus on optimality in long-run equilibrium modeling, he argued, it made no difference whether there was specific treatment of heterogenous capital or not. (A couple of years after the exchange with Robinson, Solow and Samuelson argued by way of mathematical propositions and proofs that the dynamics of a model accounting for heterogeneous capital goods could be shown in a simpler model that only dealt with one abstract capital good.[87])

Solow's reply indicates some of the main themes of neoclassical modeling that will be treated in the next chapters. Whenever the assumptions of perfect markets or an intertemporal invisible hand were questioned, modelers' main counterargument pushed their "usefulness."[88] It was the workability of the model that counted. This was as central thread of this kind of reasoning that extended to the model's applications as an instrument of measurement (Chapter 2), highlighted its practicality in comparison to more extensive empirical work (Chapter 3), nurtured its very emergence from postwar techniques of linear modeling (Chapter 4), and produced a variety of model talk (Chapter 5). Merging both treatments of capital that Robinson had criticized (national income accounts and mathematical production functions), the model of the "Contribution" became more than simply a contribution to economic theory. Chapter 2 will deal with empirical measurements of the economy and its overall productivity. On this trajectory, the model depicted stable economies for which crises did not play any major role – prophetic images that would be crucial for the politics of growth in the decades to come. Conceived as a tool to measure the sources of growth, it turned into an essential part of the interventionist toolkit. Compared to existing approaches to measurement, the model appeared as the more efficient gauge, turning them unclear, imprecise, and confusing (not unlike Harrod's work in the preceding pages). The new style turned them into something that needed to be rectified, hereby confining all kinds of uncertainty and nonknowledge to a realm outside of the model. When Solow first used his model as an instrument for measuring growth and its sources, he referred in a footnote to Robinson's critique that hardly any "precise meaning" could be given to "capital." He alleged that he was fully aware of the artificiality of the procedure and, in the same breath, denied the necessity of discussing his modeling approach: "I would not try to justify what follows by calling on fancy theorems on aggregation and index numbers. Either this kind of aggregate economics appeals or it doesn't."[89]

Notes

1 Robert M. Solow, "A Contribution to the Theory of Economic Growth," *Quarterly Journal of Economics* 70, no. 1 (1956): 65–94, 65.

2 Milton Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), 14. For a set of diverging interpretations of Friedman's methodological stance, see the contributions to Uskali Mäki, *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (Cambridge, UK: Cambridge University Press, 2009) in particular that of Kevin D. Hoover, which departs from the following conventional reading of Friedman.

3 Alfred Zauberman, "A Note on Soviet Capital Controversy," *Quarterly Journal of Economics* 69, no. 3 (1955): 445–51; Edward Nevin, "Professor Hansen and Keynesian Interest Theory," *Quarterly Journal of Economics* 69, no. 4 (1955): 637–41; Erskine McKinley, "The Problem of 'Underdevelopment' in the English Classical School," *Quarterly Journal of Economics* 69, no. 2 (1955): 235–52; Herbert A. Simon, "A Behavioral Model of Rational Choice," *Quarterly Journal of Economics* 69, no. 1 (1955): 99–118; James M. Henderson and Richard E. Quandt, "Walras, Leontief, and the Interdependence of Economic Activities: Comment," *Quarterly Journal of Economics* 69, no. 4 (1955): 626–31; Charlotte P. Alhadeff and David A. Alhadeff, "Recent Bank Mergers," *Quarterly Journal of Economics* 69, no. 4 (1955): 503–32; Theodore Levitt, "The Lonely Crowd and the Economic Man," *Quarterly Journal of Economics* 70, no. 1 (1956): 95–116; Joseph Krislov, "The Extent and Trends of Raiding among American Unions," *Quarterly Journal of Economics* 69, no. 1 (1955): 145–52.

4 See Roger E. Backhouse, *The Ordinary Business of Life: A History of Economics from the Ancient World to the Twenty-First Century* (Princeton: Princeton University Press, 2004), 178–82; E. Roy Weintraub, *How Economics Became a Mathematical Science* (Durham, NC: Duke University Press, 2002), 22.

5 See Solow's retroactive views of Keynes in his widely-read Robert M. Solow, "How Did Economics Get That Way and What Way Did It Get," *Daedalus* 126 (2005): 87–100, as well as his associations with Schumpeter's book on business cycles: Echoing Cowles criticism leveled in 1947 in relation to the institutionalist empirical work of Mitchell, Schumpeter's book was "more like a map on the scale of one foot equals two feet: you see the potholes, but you do not learn much about the scenery," Robert M. Solow, "Heavy Thinker. Review of *Prophet of Innovation: Joseph Schumpeter and Creative Destruction* by Thomas K. McCraw," *New Republic*, May 21, 2007, available at www.newrepublic.com/article/heavy-thinker, last accessed April 12, 2024. Though Samuelson could also see "theory" from the older institutionalist perspective and credited Keynes as having created a general theory, he also described the latter's work as an "indoor guessing game", "random notes", or "obscure book", see Roger E. Backhouse, *Founder of Modern Economics: Paul A. Samuelson, Vol 1: Becoming Samuelson, 1915–1948* (Oxford: Oxford University Press, 2017), 526–9.

6 See Marcel Boumans, *How Economists Model the World into Numbers* (New York: Routledge, 2005), 14.

7 Solow, "Contribution," 88 and 80.

8 Solow, "Contribution," 65.

9 Solow, "Contribution," 91.

10 Solow, "Contribution," 82.

11 For instance, Solow, "Contribution," 67, 70, 82, and 84.

12 Solow, "Contribution," 83.

13 On the sensual character of formal operations, see Sybille Krämer, "Mathematizing Power, Formalization, and the Diagrammatical Mind or: What Does 'Computation' Mean?," *Philosophy & Technology* 27, no. 3 (2014): 345–57. On diagrams in economic reasoning, see the special issue *Thinking and Acting with Diagrams* in the *East Asian Science, Technology and Society: An International Journal* 14, no. 2 (2020), edited by Hsiang-Ke Chao and Harro Maas; Mark Blaug and Peter Lloyd,

eds., *Famous Figures and Diagrams in Economics* (Cheltenham, UK: Edward Elgar, 2010). For a case study, see Yann Giraud, "Legitimizing Napkin Drawing: The Curious Dispersion of Laffer Curves, 1978–2008," in *Representation in Scientific Practice Revisited*, edited by Catelijne Coopmans, Janet Vertesi, Michael E. Lynch, and Steve Woolgar (Cambridge, MA: MIT Press, 2014), 269–90.

14 See also Mark Blaug and Peter Lloyd, "Introduction," in *Famous Figures and Diagrams in Economics*, edited by Mark Blaug and Peter Lloyd (Cheltenham, UK: Edward Elgar, 2010).

15 Andreas Langenohl has argued that the temporal ambiguity of static neoclassical models (their ability to be interpreted statically or dynamically) created a symbolic surplus – a surplus of meaning in the sense of temporal options and potentialities: Andreas Langenohl, "Neoklassische Polychronie. Die Temporalitäten algebraischer Modelle bei Alfred Marshall," in *Forum interdisziplinäre Begriffsgeschichte* 5, no. 1, edited by Eva Axer, Eva Geulen, and Alexandra Heimes, (2016), 102–14.

16 Solow, "Contribution," 78.

17 Solow, "Contribution," 67–8. The rationale of this paragraph relies on joint work with Roger E. Backhouse; Roger E. Backhouse and Verena Halsmayer, "Mathematics and the Language of Economics" (paper presented at the Workshop "Language(s) and Language Practices in Business and the Economy," Vienna University of Economics and Business, October 23–25, 2014).

18 Solow, "Contribution," 66.

19 Cf. Matthieu Ballandonne and Goulven Rubin, "Robert Solow's Non-Walrasian Conception of Economics," *History of Political Economy* 52, no. 5 (2020): 827–61.

20 Solow, "Contribution," 66.

21 The article did add a more specific portrayal of market behavior but these "causal dynamics" that linked the equilibrium conditions for price and output were simply plugged in. In principle, the model worked without integrating the price–interest dynamics, as it was simply assumed that "the real wage and the real rental of capital adjusting instantaneously so as to clear the market" (Solow, "Contribution," 78–9).

22 Solow, "Contribution," 67.

23 Solow, "Contribution," 68.

24 Deirdre N. McCloskey, "History, Differential Equations, and the Problem of Narration," *History and Theory* 30 (1991): 21–36, 22.

25 On the varying ways economists have dealt with the trade-off between what they understood as tractability and realism, see Béatrice Cherrier, "The Price of Virtue: Some Hypotheses on How Tractability Has Shaped Economic Models," *Œconomia* 13, no. 1 (2023): 23–48.

26 The "fundamental equation" showed the change in the capital–labor ratio as the difference between two terms. The first term indicated the amount of investment (equal to savings), while the second term indicated the amount of investment needed to maintain the work force. The difference between them (the amount of capital surplus to requirement) gave the rate at which the capital–labor ratio changed.

27 Solow, "Contribution," emphasis in original.

28 Solow, "Contribution," 73.

29 Solow, "Contribution," 90. For those interested in the details: The linear differential equation was analytically solved for three different cases, the Harrod–Domar case

(exhibiting fixed proportions and therefore the "knife-edge" property), the Cobb–Douglas function (where the natural rate of growth equals the warranted rate as a consequence of demand–supply adjustments), and the whole family of constant-returns-to-scale production functions (differing from the Cobb–Douglas case in that production is possible with only one factor). In order to see the effects of specific changes (introduction of technological change, a personal income tax, variable population growth, etc.) on the model economy, Solow used the Cobb–Douglas case.

30 Both: Solow, "Contribution," 93.

31 Solow, "Contribution," 91.

32 All: Solow, "Contribution," 93. The relation between Keynesian policies and neoclassical growth model had been equally addressed in Evsey D. Domar, "Capital Expansion, Rate of Growth, and Employment," *Econometrica* 14, no. 2 (1946): 137–47, 145. Domar similarly argued that he offered "a theoretical point of view, without considering the numerous practical questions that the income guarantee would raise."

33 Two examples (rigid wages and liquidity preference) were given. One worked out nicely, the other identified limitations of the model and delineated a space in which other models would lead to different conclusions.

34 Solow, "Contribution," 93.

35 Solow, "Contribution," 66. Cf. Robert M. Solow, "Growth Theory and After," *American Economic Review* 78, no. 3 (1988): 307–17, 309–10.

36 On the multiple meanings of the neoclassical synthesis, see Michel De Vroey and Pedro Garcia Duarte, "In Search of Lost Time: The Neoclassical Synthesis," *The B.E. Journal of Macroeconomics* 13, no. 1 (2013): 1–31, 20–1. They also cite Solow, who kept to the conviction that "one can be a Keynesian for the short run and a neoclassical for the long run." Robert M. Solow, "Swan, Trevor W.," in *An Encyclopedia of Keynesian Economics*, edited by Thomas Cate (Northampton, MA: Edward Elgar, 2013), 594–97, 594. In fact, his theoretical work over the decades focused on the combination of the short- and long-run, in particular, the question of deviations from an equilibrium growth path. On Solow and his colleagues' attempts to bridge Keynesian and neoclassical modeling, Michaël Assous, "Solow's Struggle with Medium-Run Macroeconomics, 1956–95," *History of Political Economy* 47, no. 3 (2015): 395–417. On macroeconomic planning, see the Epilogue.

37 Solow, "Contribution," 91.

38 This is what evolutionary economist Richard R. Nelson, for instance, has argued in "Numbers and Math Are Nice, But . . .," *Biological Theory* 10, no. 3 (2015): 246–52.

39 Solow, "Contribution," 65.

40 Solow, "Contribution," 93.

41 Solow, "Contribution," 86.

42 See Reinhard Wendler on Georges Canghuilem's view that it is precisely the uncontrollability that makes a model productive: Reinhard Wendler, *Das Modellz wischen Kunst und Wissenschaft* (Munich: Wilhelm Fink, 2013), 12.

43 Marc Kac, "Some Mathematical Models in Science," *Science* 166, no. 3906 (1969): 695–9, 699, quoted in Saul Gass, "Model World: A Model Is a Model Is a Model," *Interfaces* 19, no. 3 (1989): 58–60, 60.

44 Boumans, *How Economists Model the World into Numbers*, 13.
45 Solow, "Contribution," 91. A second set of rules, according to Morgan, is determined by the subject matter, for instance, that manipulations have to be in a certain order for them to make economic sense, Mary S. Morgan, *World in the Model: How Economists Work and Think* (Cambridge: Cambridge University Press, 2012), 26. In the case of the neoclassical growth model, an example for such rules was that production could not be negative.
46 Morgan, *The World in the Model*, 256. On "epistemic artifacts" opening up new ways of thinking, see also Tarja Knuuttila, *Models as Epistemic Artefacts: Toward a Non-Representationalist Account of Scientific Representation* (Helsinki: Department of Philosophy, 2005).
47 See Morgan, *World in the Model*, 296: In contrast to laboratory experiments, model experiments are of a different materiality vis-à-vis the world they are supposed to represent. Therefore, the "surprising results of model experiments lead not to the discovery of new phenomena in the real world, but to the recognition of new things in the small world of the model, and thence to the development of new categories of things and new concepts and ideas in economics."
48 Robert Solow, quoted in Brian Snowdon and Howard R. Vane, *Conversations with Leading Economists: Interpreting Modern Macroeconomics* (Cheltenham: Elgar, 1999), 275.
49 See, for instance, I. M. D. Little, "Classical Growth," *Oxford Economic Papers* 9, no. 2 (1957): 152–77, 152, n3; H. A. John Green, "Growth Models, Capital and Stability," *The Economic Journal* 70, no. 277 (1960): 57–73, 57; Hirofumi Uzawa, "On a Two-Sector Model of Economic Growth," *The Review of Economic Studies* 29, no. 1 (1961): 40–7, 40. Swan published his model in Trevor W. Swan, "Economic Growth and Capital Accumulation," *Economic Record* 32, no. 63 (1956): 334–61, Tobin in James Tobin, "A Dynamic Aggregative Model," *Journal of Political Economy* 62, no. 2 (1955): 103–15.
50 Frank H. Hahn, "The Stability of Growth Equilibrium," *Quarterly Journal of Economics* 74, no. 2 (1960): 206–26, 206. Others spoke of "the well-known neoclassical growth model by Professor Solow," Ronald Findlay, "Economic Growth and the Distributive Shares," *The Review of Economic Studies* 27, no. 3 (1960): 167–78, 175.
51 Solow, "Contribution," 73, 91.
52 From the perspective of post-Keynesian economics, Harrod was seen to have assumed a constant savings rate, and Joan V. Robinson and Nicholas Kaldor were the ones to correct the mistake. See Daniele Besomi, "Harrod's Dynamics and the Theory of Growth: The Story of a Mistaken Attribution," *Cambridge Journal of Economics* 25 (2001): 79–96.
53 This section builds on Verena Halsmayer and Kevin D. Hoover, "Solow's Harrod: Transforming Macroeconomics Dynamics into a Model of Long-Run Growth," *European Journal for the History of Economic Theory* 23 (2016): 71–97, in particular on section 2.
54 Robert J. Barro and Xavier Sala-i-Martín, *Economic Growth* (Cambridge, MA: MIT Press, 2003), 17. Another example is found in an article by the well-known growth theorist Olivier La Grandville, "The 1956 Contribution to Economic Growth Theory by Robert Solow: A Major Landmark and Some of Its Undiscovered Riches," *Oxford Review of Economic Policy* 23, no. 1 (2007): 15–24, 16.

55 See, for instance, Alexander Nützenadel, *Stunde der Ökonomen: Wissenschaft, Politik und Expertenkultur in der Bundesrepublik 1949–1974*, Kritische Studien zur Geschichtswissenschaft, Bd. 166 (Göttingen: Vandenhoeck & Ruprecht, 2005), 77; Stephen Macekura, "Development and Economic Growth: An Intellectual History," in *History of the Future of Economic Growth: Historical Roots of Current Debates on Sustainable Degrowth*, edIted by Iris Borowy and Matthias Schmelzer (New York: Routledge, 2017), 110–28, 117.

56 R. F. Harrod, "An Essay in Dynamic Theory," *The Economic Journal* 49, no. 193 (1939), 14–33.

57 Harrod, "Essay," 33.

58 On Harrod's stance toward economics as a science and his use of mathematics, see Daniele Besomi, *The Making of Harrod's Dynamics* (London: Macmillan, 1999) and Warren Young, *Harrod and His Trade Cycle Group. The Origins and Development of the Growth Research Programme* (London: Macmillan, 1998).

59 Examples are Besomi, *Making of Harrod's Dynamics*; Athanasios Asimakopulos, "Harrod on Harrod: The Evolution of a Line of Steady Growth," *History of Political Economy* 17, no. 4 (1985), 619–35; Jan A. Kregel, "Economic Dynamics and the Theory of Steady Growth: An Historical Essay on Harrod's 'Knife Edge'," *History of Political Economy* 12, no. 1 (1980): 97–123.

60 Thomas C. Schelling, "Capital Growth and Equilibrium," *American Economic Review* 37, no. 5 (1947): 864–76, 867.

61 David Wright, a Harvard graduate, showed, for instance, that Harrod used six independent qualifications regarding the warranted rate of growth, David Wright, "Mr. Harrod and Growth Economics," *Review of Economics and Statistics* 31, no. 4 (1949): 322–8.

62 Robert Solow to Harry G. Johnson, September 28, 1953, Solow papers, box 56, file J: 1 of 2.

63 John Hicks's simple diagram reduced the *General Theory* to a relationship between output and interest rate. In the hands of Hansen, Hicks's diagram turned into the IS-LM model and became a central element of Keynesian textbooks of postwar economics. See Backhouse, *Ordinary Business of Life*, 233.

64 William J. Baumol, "Formalisation of Mr. Harrod's Model," *Economic Journal* 59, no. 236 (1949): 625–9, 629.

65 Using a strand of Marxist economics as a point of departure, Domar, who was completing a doctorate at Harvard, linked capital accumulation and employment in terms of a small-scale mathematical model.

66 Domar, "Capital Expansion, Rate of Growth, and Employment," 137. The stated idea was that, in contrast to dominating policy views at the time, the growth of the labor force and its productivity did not bring about income by itself – "the demand side of the equation [was] missing" (138). Later, Domar emphasized the enormous role played by early Soviet growth models, which were written in response to the immediate practical problems of planning. Among recent Western writers, Domar pointed among others to the works of Gustav Cassel, Michał Kalecki, Joan V. Robinson, Eric Lundberg, and Paul Sweezy. See Evsey D. Domar, "Economic Growth: An Econometric Approach," *American Economic Review* 42, no. 2 (1952): 479–95, 480, n4.

67 On Domar's understanding of the "mathematics of growth" and his modeling work, see Mauro Boianovsky, "Modeling Economic Growth: Domar on Moving

Equilibrium," *History of Political Economy* 49, no. 3 (2017): 405–36. Similar to Solow, Domar is predominantly remembered for his growth model. On Domar's specific way of narrativizing in his work on economic history, comparative economic systems, and Soviet economics, see Ibanca Anand, "Resisting Narrative Closure: The Comparative and Historical Imagination of Evsey Domar," in *Narrative in Economics: A New Turn on the Past* (supplement to *History of Political Economy* 55), edited by Mary S. Morgan and Thomas S. Stapleford (Durham, NC: Duke University Press, 2023), 497–521.

68 Baumol, "Formalisation of Mr. Harrod's Model," 625.

69 James Tobin to Robert Solow, March 19, 1959, Solow papers, box 61, file T: 2 of 2.

70 Harold Pilvin, "Full Capacity vs. Full Employment Growth," *Quarterly Journal of Economics* 67, no. 4 (1953): 545–52, 548.

71 Tobin, "Dynamic Aggregative Model"; Kenneth Boulding, "In Defense of Statics," *Quarterly Journal of Economics* 69, no. 4 (1955): 185–502, cited in Robert Eisner, "On Growth Models and the Neo-Classical Resurgence," *The Economic Journal* 68, no. 272 (1958): 707–21, 707.

72 Roy F. Harrod, "What Is a Model?," in *Value, Capital and Growth. Papers in Honour of Sir John Hicks*, edited by J. N. Wolfe (Edinburgh: Edinburgh University Press, 1968), 173–92.

73 Hélène Metzger, *Newton, Stahl, Boerhaave et la doctrine chimique* (Paris: Blanchard, 1974 [1930]), 6, cited in Cristina Chimisso and Nicholas Jardine, "Hélène Metzger on Precursors: A Historian and Philosopher of Science Confronts Her Evil Demon," *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 11, no. 2 (2021): 331–53.

74 Georges Canguilhem, *Études d'Histoire et de Philosophie des Sciences*, 3rd ed. (Paris: Vrin, 1975), 22, cited and translated in Hans-Jörg Rheinberger, "Reassessing the Historical Epistemology of Georges Canguilhem," in *Continental Philosophy of Science*, edited by Gary Gutting (Malden, MA: Blackwell, 2005), 187–97, 191.

75 See Christoph Hoffmann, *Die Arbeit der Wissenschaften* (Zürich: diaphanes, 2013), 90–5.

76 Robert Dorfman, Paul Samuelson, and Robert M. Solow, *Linear Programming and Economic Analysis* (New York: McGraw-Hill, 1958), 1. Going one step further, the same reference was also used to suggest that the long history of economic thought had always already looked at the capitalist economy as a cybernetic self-regulating system: "Mr. Jourdain, a hero of one of Molière's comedies was surprised to be told by his teacher that he spoke in prose all his life. A similar situation exists in economics and cybernetics. From the very onset of the development of the political economy, economists were engaged in problems which we define today as cybernetic problems," Oskar Lange, *Introduction to Economic Cybernetics* (Oxford: Pergamon Press, 1970), 1.

77 See Cristina Chimisso, "Narrative and Epistemology: Georges Canguilhem's Concept of Scientific Ideology," *Studies in History and Philosophy of Science Part A* 54 (2015): 64–73, 65.

78 Douglas Clement, "Interview with Robert Solow," *The Region. Banking and Policy Issues Magazine* (September 1, 2002), available at www.minneapolisfed.org/article/2002/interview-with-robert-solow, last accessed May 6, 2024.

79 Domar, "Economic Growth," 481, 484.

80 H. A. V., "Economic Fact and Theory," *The Financial Times*, September 10, 1956, JVR/xv/9.5/1; John Strachey, "Dead and Dumb Sciences," *New Statesman and Nation*, August 4, 1956, JVR/vx/9.1/2; "Capital Issues," *The Times Literary Supplement*, October 5, 1956, JVR/ xv/9.6/2, all: Joan V. Robinson papers at King's College Archive Centre at the University of Cambridge. For intellectual biographies of Robinson, see Nahid Aslanbeigui and Guy Oakes, *The Provocative Joan Robinson: The Making of a Cambridge Economist* (Durham, NC: Duke University Press, 2009); Geoffrey C. Harcourt and Prue Kerr, *Joan Robinson* (Basingstoke: Palgrave Macmillan, 2009).

81 Joan V. Robinson, "The Production Function and the Theory of Capital," *Review of Economic Studies* 21, no. 2 (1953–54): 81–106.

82 Robinson, "Production Function," 81.

83 The Controversies encompassed a series of exchanges between economists from Cambridge, England and Cambridge, Massachusetts that stretched from the mid-1950s to the 1970s, filled with intricate theoretical arguments as well as full-blown polemics. The most prominent account, in a way only making the Controversies is Geoffrey C. Harcourt, *Some Cambridge Controversies in the Theory of Capital* (Cambridge: Cambridge University Press, 1972). Tiago Mata sheds light on the aftermath of the debate that saw the formation of a "Post-Keynesian" identity, Tiago Mata, "Constructing Identity: The Post Keynesians and the Capital Controversies," *Journal of the History of Economic Thought* 26, no. 2 (2004), 241–59. Avi J. Cohen and Harcourt discuss the relevance and importance of the Controversy from a contemporary economists' viewpoint, Avi J. Cohen and Geoffrey C. Harcourt, "Whatever Happened to the Cambridge Capital Theory Controversies?" *Journal of Economic Perspectives* 17, no. 1 (2003), 199–214. A more recent contribution that focuses on the perspective of Cambridge, MA is Roger Backhouse, "MIT and the Other Cambridge," in *MIT and the Transformation of American Economics* (supplement to *History of Political Economy* 46), edited by E. Roy Weintraub (Durham, NC: Duke University Press, 2014), 252–71.

84 Solow to Johnson, September 28, 1953, Solow papers, box 56, file J: 1 of 2.

85 Robert M. Solow, "The Production Function and the Theory of Capital." *Review of Economic Studies* 23, no. 2 (1955–56): 101–8, 101.

86 Both: Solow, "Production Function," 103.

87 The argument was that the necessary condition for optimality over time was identical and, therefore, the simple model had "great heuristic value" – "even though there is no such thing as a single abstract capital substance that transmutes itself from one machine form to another like a restless reincarnating soul," Paul A. Samuelson, and Robert Solow, "A Complete Capital Model Involving Heterogeneous Capital Goods," *Quarterly Journal of Economics* 70, no. 4 (1956), 537–62, 537–8. On the relation between modeling the path of capital, and Richard E. Bellman's dynamic programming, see Nancy Wulwick, "The Mathematics of Economic Growth" (Working Paper No. 38, Jerome Levy Institute, 1990), and Esther-Mirjam Sent, "Engineering Dynamic Economics," in *New Economics and Its History* (supplement to *History of Political Economy* 29), edited by John B. Davis (Durham, NC: Duke University Press, 1998), 41–62.

88 For the idea that "the most myopic vision on the part of market participants" leads to "efficiency over long periods of time," see the textbook on linear programming

that will be treated more extensively in Chapter 4: Dorfman et al., *Linear Programming and Economic Analysis*, 321. On Joan Robinson's most famous criticism of neoclassical time, see Joan Robinson, "History versus Equilibrium," Indian Economic Journal 21, no. 3 (1974): 202–13. See also Harvey Gram and G. C. Harcourt, "Joan Robinson and MIT," *History of Political Economy* 49, no. 3 (2017): 437–50.

89 Robert M. Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics* 39, no. 3 (1957): 312–20, 312.