From Long-Run Utopia to Technical Expertise

Solow's Growth Model as a Multipurpose Design

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Abstract
Combining concrete policy-oriented modeling strategies of World War II with what was received as traditional neoclassical theory, in 1956 Robert Solow constructed a simple, clean, and smooth-functioning "design" model that served many different purposes. As a working object it enabled experimentation with long-run equilibrium growth. As an instrument of measurement it was applied to time series data. As a prototype it was supposed to feed into larger-scale econometric models that were, in turn, thought of as technologies for policy advice. Used as a teaching device, Solow's design became a medium of "spreading the technique," and one of the symbols for neoclassical macroeconomics that soon became associated with MIT.

Keywords: model, modeling, Robert Solow, growth theory, growth, neoclassical growth model, linear programming, dynamic programming, design model, Harvard Economic Research Project, Massachusetts Institute of Technology.

JEL Codes: B22, B23, B31, B40, O4, Z1

On January 10, 1954, the journalist and author Godfrey Blunden sent a letter to Harold Isaacs, a member of the Department of Political Science at MIT, asking for help on a novel he planned to write:

> The novel […] is about some New York and Connecticut people, among them a mathematician. I have got involved in the notion that in a highly mechanised society the "pure" mathematician is pretty much under pressure from the physicists, economists and statisticians to produce new theorems which may be of practical use in the applied fields […] I am thinking of a theorem, or a calculus, as fundamental to the analysis of our social economy as the Archimedian geometry was to the great builders, the infinitesimal [sic] calculus to the age of motion, the theory of ralativity [sic] to the atomic physicists. Which is the same thing as saying that a mathematician may (or may fail) to answer some deep need in his society. […] I just want to see how they do it. In other words I want to have a look at the kind of thing my man might be expected to turn out (Blunden to Isaacs, January 10, 1954, Box 53, File B: 7 of 7).[1]

Isaacs forwarded Blunden's letter to Robert M. Solow, a colleague in the Economics Department. Solow thought himself the right person to answer Blunden's question since "[t]he area where economic theory, mathematics, and statistics run together is exactly my line of work." Contrary to Blunden's suggestion, Solow did not emphasize the great advantages of "pure mathematics" in economics but instead pointed out the difficulties that arise when formulating "complicated economic systems in mathematical terms":

> What holds up the analysis is not any gaps in mathematical knowledge. […] Nor is it the lack of large-scale computing machinery. The bottleneck is that the functions and relationships that make up a theoretical model have to be given some empirical form and content. Information of this kind has to come from observation and statistical calculation. But it takes a year to get a year's figures on consumption and investment and inventories and so forth. And it takes many years' figures before any statistical sense can

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[1] Unless otherwise indicated, all archival material cited is taken from the Robert M. Solow Papers, collected at the Economists' Papers Project, Rare Book, Manuscript, and Special Collections Library, Duke University.

> be made: the irregular, non-systematic element in economic life is far from trivial. And by the time the years have passed, the quantities and relations one is trying to estimate will probably have changed in some substantial but unknown way. The mathematics that appears in modern economic theory is by no means elementary; but it seems unlikely that any purely mathematical discovery would set economics on its ear (Solow to Blunden, January 26, 1954, Box 53, File B: 7 of 7).

According to Solow, mathematical knowledge was indeed useful for economic analysis in a "highly mechanised society," but the crucial aspect of economic research was the empirical content given to a mathematical model. However, as Solow knew from his own experience, empirical research was tedious, time- and cost-intensive. At the time of writing the letter, he was engaged in research on dynamic programming and linear modeling. Only two years later Solow published "a simple model" in his "Contribution to the Theory of Economic Growth." This model was not the first to show a balanced growth path not entirely dependent on capital accumulation. Neither was his empirical model published in 1957 the first to claim that productivity growth in the U.S. had come from the contribution of a residual (cf. Crafts 2009, 200). Instead, what set Solow's model apart from earlier contributions was the form of the object he created: a simple, clean, and tractable, yet fully articulated and empirically usable artifact.[2] It became the standard growth model of neoclassical macroeconomics, an "engine of research" that was easy to use, adopt, extend and apply in a variety of different projects (see Boianovsky and Hoover, this volume).

My aim in the present article is to understand what made constructing and using such a model plausible, and what this way of doing economics might have signified. Combining concrete policy-oriented modeling strategies of World War II with what was received as traditional neoclassical theory, Solow's

[2] The same year, Trevor Swan published a very similar model. When eventually getting to know Swan's work, Solow was intrigued: "I must tell you that I can't remember when I've enjoyed a piece of economics so much. It was sheer pleasure" (Solow to Swan, April 1, 1957, Box 60, File S: 2 of 7). However, priority is not at issue in the present article; Swan's work was not part of Solow's background. The fact that the model was initially coined "the Solow model" is itself a question of crediting, that is, in turn, linked to the rise of MIT to become one of the centers of economics in the second half of the 20th century. The part that Solow's model played in this process will be addressed in section 6.

model worked as a "design" that served multiple purposes. In what follows I will focus on the various uses Solow's design model was put to, the associated modeling practices, and the respective professional identity of the economist as – what Solow called in retrospect – an "engineer in the design sense" (Interview Solow 2011).[3]

Godfrey Blunden never finished his novel. Had he done so, he might have told the story of the postwar development of a specific way of doing economics, one that combined simple mathematical models with econometrics, but was less complex, and less laborious than large-scale empirical research. A way of doing economics that created small and tractable artifacts that were easy to investigate, to extend, and to apply, yet embodied high hopes of producing useful, and practical knowledge for economic governance.

1 The "Collapsed Production Function"

We might, when we look at economic modeling as a research practice that consists in the construction of artifacts, through tracking the components of a specific model, get a grip on the specificities of the model as well as on the context out of which it developed. The "collapsed production function" was a major component of Solow's 1956 model. In 1948 and 1949, while a graduate student in economics at Harvard, Solow worked at the Harvard Economic Research Project (HERP), calculating the first set of capital coefficients for input-output models.[4] In the late 1940s input-output research at HERP was directed at

[3] My analysis builds on recent literature on economic modeling as a practice, and the notion of economic models as quasi-material objects: Morgan (2012), Boumans (2005), Morrison and Morgan (1999), and, with a focus on the social construction of models, Breslau and Yonay (1999). On the history of scientific objects see e.g. Daston (2007) and Daston (2000); on the somewhat related notion of (techno)scientific objects see Nordmann (2011) and Bensaude-Vincent et al. (2011); on material models see Chadarevian and Hopwood (2004).
[4] See Solow (1987) as well as Leontief (1953, vii). The HERP had been established on grounds of the collaboration between the U.S. Bureau of Labor Statistics with Wassily Leontief and his input-output analysis and was financed by the Air-Force funded interagency project SCOOP (cf. Kohli 2001, 205).

investigating the feasibility of wartime mobilization plans (Preliminary Report of the Final Progress Report 1950, Leontief Papers, HUG 4517.5, Box 8, Folder: Progress Reports). To that end, Leontief and his assistants produced large input-output tables of 500 economic sectors that were meant to "visualize" the national economy as a system of industries mutually interrelated via their "capital structure." The data stemmed from what Leontief described as "direct empirical information" (Leontief 1953, 14): In addition to corporation- and government agency-reports, papers in trade journals, and engineering handbooks, HERP gathered data from production officials, design engineers, technicians, and other experts.[5] Turned into matrix form, the input-output tables enabled the formulation of linear equations systems of so-called "technical" production functions with linear technologies. These were understood to provide a "more realistic" account of production than the smooth differentiable production function of "traditional neoclassical theory" (cf. Mirowski 1989, ch. 6; Boumans 2012, 8; Backhouse 2012a, 42).[6]

This concern with observability and measurement was abandoned at the Cowles Commission's Activity Analysis Conference of 1949. At the same time Solow calculated capital coefficients at HERP, Tjalling C. Koopmans, recently appointed research director of the Cowles Commission, organized a conference, financed by the RAND Corporation, that brought together over 50 participants from different fields.[7] The conference aspired to show the "unity of [the participants'] major lines of thought" in what was considered the fundamental problem of "normative economics: the best allocation of limited means toward desired ends" (Koopmans 1951, 1). Linear programming techniques had recently been developed

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[5] For an insider's account of how research at the HERP was done see Carter (2012).

[6] The label "traditional theory" referred to the received theory of production from the 1930s, consisting basically of Cassel's version of the Walrasian system. Solow for instance mentioned Schneider (1934) as one of the most commonly used textbooks at the time (Interview Solow 2011).

[7] Thomas (this volume) also provides a short account of the conference and shows how economists selectively used OR's decision models in order to model economic dynamics. Düppe and Weintraub (2014) focus on the emergence of a new community of mathematical economists that took mathematical values of rigor and axiomatization for granted while disengaging with the intellectual values that had shaped economics departments before World War II. Backhouse (2012a) puts Paul Samuelson's relation to the conference in the center and shows that he had been working on the relationship between linear models and smooth, differentiable production function models since he started at RAND in 1949.

in wartime research in order to work out efficient ways to achieve specified objectives. The major aim of the conference participants was now to investigate how far these techniques might shed new light on traditional economic theory. The production of a whole economy was looked at as an "efficient combination of activities" (Koopmans 1951a, 33) in the form of a system of linear equations where commodities are transformed into each other at constant returns to scale.

There was a certain tension at the conference: on the one hand, if one approach was dominant, it was Leontief's input-output analysis. On the other hand, Leontief's most prominent concern, namely how to gather and deal with data in order to give "a description of the capital structure of the economy" (Progress Report 1950, op. cit.), was almost absent (cf. Backhouse 2012a, 29-34). Since HERP's capital coefficients were taken to be given by engineers and therefore would not change, they entered input-output analysis as fixed. The participants of the Activity Analysis Conference considered the fixed coefficient form too narrow an assumption, thereby brushing aside the issue of measurement. Paul Samuelson for instance redefined the fixed coefficients as equilibrium positions: "*all desirable substitutions have already been made by the competitive market*, and no variation in the composition of final output or in the total quantity of labor will give rise to price change or substitution" (Samuelson 1951, 142-3). In his substitution theorem Samuelson interpreted HERP's coefficients as the solution to a system of substitutable production functions, as a social optimum that was independent of the market – changes in final output or the total quantity of labor would not give rise to "price change or substitution." Samuelson identified that optimum with the equilibrium of neoclassical production theory: the relative equilibrium prices are identical to the "shadow prices" or "efficiency prices," i.e., the dual variables of linear programming as given via the input coefficient matrix. In this way, the model became possible to integrate with traditional neoclassical theory (cf. Backhouse 2012a, 18-9; Akhabbar 2006), paving the way for Solow's growth model.

In 1951 Robert Solow received an invitation from Charles J. Hitch to spend some time at the RAND Corporation to work on "problems connected with dynamics of input/output models" (Herr to Solow, April 25, 1951; Box 59, File Rand Corporation, 2 of 3).[8] Solow had been recruited to MIT the year before, and, in close cooperation with Samuelson, had started to investigate "optimization over time in Leontief and other dynamic input-output models," including the stability of growth systems and "optimal capital programming over time" (Solow to Friedman, October 25, 1954, Box 55, File F: 3 of 3). Linear systems showed efficiently working production in which equilibrium was divorced from market activities. With the duality theorems of linear programming this equilibrium was interpreted as an outcome of competitive market processes, but lacked any price, interest, and wage dynamics. Solow's "[e]conomic intuition" said that this was "all wrong" (Solow 1953-54, 79). Consequently, he aimed to develop an appropriate price theory for dynamical input-output systems.

In the course of "simplifying" a dynamic input-output system to teach to his students at MIT, he came up with the idea of a "one-commodity-world" (Solow 1953-54, 75). HERP's input-output models were already based on the concept of dividing industries into several sub-industries "such that the achieved detailed information of sub-industries could then be 'collapsed' into a single aggregate industry so as to reduce the amount of necessary computations" (Progress Report 1950, op. cit.). Similarly, Solow collapsed various capital inputs in a single index, resulting in the "collapsed production function" that showed output as a function of inputs of labor and "capital-in-general." Unlike HERP's input-output tables, this production function was not about "visualizing" the capital structure of a national economy; and unlike the heterogeneous capital-goods models that Samuelson and Solow worked on at the time, it was

[8] Solow had received almost all his professional training at Harvard with the exception of one year he spent at Columbia University since "it was impossible to obtain anything beyond an elementary education in mathematical statistics at Harvard" (Application for a NSF senior postdoctoral fellowship October 10, 1962, Box 58, file N: 1 of 2). With the financial support of a Social Science Research Council Pre-Doctoral Fellowship he attended courses by Abraham Wald, Jacob Wolfowitz and T.W. Anderson in mathematical statistics while working on his dissertation on income distribution. Solow's advisors were Wassily Leontief and Guy Orcutt, both of whom dealt with detailed empirical schemes.

not supposed to provide a normative solution for capital programming models (cf. Backhouse, this volume (MIT and the other Cambridge)).[9] The structural matrix of HERP had been transformed and redefined into an entirely different object, a handy device for integrating the price-wage-interest dynamics – the "causal dynamics" – in what Solow already called his "neoclassical model."

2 Solow's 1956 growth model

In 1956 the *Quarterly Journal of Economics* published "A Contribution to the Theory of Economic Growth." In the spirit of the unification endeavors of the Cowles Commission's Activity Analysis Conference, which put forth the formal similarities between linear programming models and "traditional economic theory," Solow had identified a "strong analogy" between "Leontief's and Mr. Harrod's system" (Solow 1953-54, 74). Building on this analogy he now presented his own aggregative model as "a model of long-run growth which accepts all the Harrod-Domar assumptions except that of fixed proportions" (Solow 1956, 66). The "Harrod-Domar model" was one of the dominant research sites in the immediate postwar era – "countless craftsmen" had made it the "most over-worked tool in economics" (Tobin to Solow, March 19, 1959, Box 61, File T: 2 of 2). By dropping the assumption that production takes place under conditions of fixed proportions, which Solow claimed to have found in both Leontief's and Harrod's work, he aimed at repudiating the so-called "knife-edge" of the "Harrod-Domar line of thought."[10]

The collapsed production function showed a model economy producing a single commodity (Y)

[9] Samuelson and Solow (1956) extended Ramsey's growth model to the case of numerous consumers and heterogeneous capital goods. As a by-product of their dynamic programming work the authors showed that the necessary condition for optimality over time was identical in the cases of one capital good and many capital goods, thus arguing for a heuristic use of a homogenous capital substance. See Wulwick (1990, 20-34) and Sent (1998, 40). On the development of Dorfman, Samuelson and Solow (1958) see Backhouse (2012b).

[10] Alternately, Solow referred to "the Harrod-Domar line of thought" and "Harrod's model." Interestingly, what Solow associated with Harrod bore hardly any resemblance to Harrod's own work, but drew instead on the Harrod-Domar literature that developed only after the war. For the reception of Harrod's work as a model of growth see Besomi (2001). For an interpretation of Solow's reading of Harrod's work see Halsmayer and Hoover (2013).

with constant returns to scale and smooth substitution between capital and labor. It was embedded in the model with the following assumptions: there is a constant savings rate (s) so that savings $S = sY$; there is "no scarce nonaugmentable resource like land"; prices and wages are fully flexible; there is constant full employment of factors of production; and *ex ante* is always equal to *ex post* investment (cf. Halsmayer and Hoover 2013, 6). In the model world of perfect foresight and perfect competition, the "fundamental equation" shows its equilibrium mechanism: $\dot{r} = sF(r, 1) - nr$ (Solow 1956, 69). The change of the capital-labor ratio r equals the difference of two terms. The first term indicates the amount of investment (equal to saving) in the model economy, while the second term indicates the amount of investment needed to maintain the work force (expanding at rate n) at the same capital-labor ratio, so that the difference between them (i.e., the amount of capital surplus to requirement) indicates the rate at which the capital-labor ratio changes. Solow then demonstrated the stability of the equilibrium value r^* by showing how the capital-labor ratio changes over time when $r \neq r^*$: "Whatever the initial value of the capital-labor ratio, the system will develop *toward* a state of balanced growth at the natural rate" (Solow 1956, 70).

The closed form of the model made graphic representation straightforward. Drawing different shapes for the productivity curve, Solow demonstrated that this stability would apply only when production took place under the "usual neoclassical conditions" of variable proportions and constant returns to scale, "[the] system can adjust to any given rate of growth of the labor force, and eventually approach a state of steady proportional expansion" (Solow 1956, 73). He backed up his conclusion with the analytical treatment of 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).

After constructing the model and investigating its properties, Solow extended his analysis by introducing small changes *ceteris paribus* in order to see whether the model economy with Cobb-Douglas production would still find its way to the stable equilibrium path (Solow 1956, 86). One of these "experiments," as Solow called them, was to introduce technological change by letting it float "down from the outside" (Solow 1959, 90). The collapsed production function was supplemented by a separate term $A(t)$ accounting for changes of technology: $F = A(t)\,F(K, L)$ (Solow 1956, 85).[11]

The key characteristic of Solow's working object was simplicity: "[a] good model makes the right strategic simplifications. In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows" (Solow 1956, 92). The decision which strategic simplifications to choose lies in the hands of the modeler, and unlike HERP's input-output models, Solow's target was not production but growth. Hence, Solow defended the assumption of homogeneous capital, but was critical of his assumption of perfect foresight.[12] By pointing out the strong assumptions of the absence of risk and uncertainty, a fixed average propensity to save, and no monetary complications, he made clear that he was dealing with a "model economy" (Solow 1956, 78): his aim was not to present a "credible theory of investment," but a simple, tractable and manipulable model of long-run equilibrium growth. To support his "experiments," Solow constructed a model that would ensure the existence of an equilibrium. As he argued in a discussion with Leontief: "The real point at issue is the consistency of a

[11] Other "experiments" encompassed the formulation of supply of labor as a function of the real wage rate and time, letting the rate of saving depend on the yield of capital, adding a personal income tax, and assuming variable population growth. Only in the last case does the model economy need a major burst of investment instead of small-scale capital accumulation; none of these changes leads the system to a "stationary state" (Solow 1956, 85-91).

[12] The concern with perfect foresight fit well with Solow's preoccupation with dynamic programming. As he stated two years before publishing the "Contribution to the Theory of Economic Growth:" "This is always a hard assumption to swallow […] The logic of this result is that in an equilibrium system with perfect foresight the future is implicit in the present, a seed literally is its own future stream of net outputs of fruit, and must be worth the present value of its own future" (Solow 1953-54, 76-7). See also Backhouse (this volume (MIT and the other Cambridge)).

model: if it will not guarantee the existence of an equilibrium solution, what confidence can we have in its other promises?" (Solow to Leontief, January 27, 1954, Box 57, File L: 2 of 2).

3 The model as a working object

Solow's growth model was a simple, clean and transparent world, created to demonstrate not the phenomenological but the mathematical possibility of a stable long-run equilibrium, and to investigate what factors would determine this growth path. It was the simplicity of Solow's model that differentiated it from HERP's input-output tables and from the linear equation systems of activity analysis, both of which required a large amount of data and the requisite computing capacities. Solow himself called the practice of constructing and manipulating a simple model a "pen-and-pencil operation," a rather inexpensive way of doing economics compared to the empirical work done by large research groups (Solow to Nicholson, November 13, 1969, Box 58, File N: 1 of 2).[13]

Retrospectively, Solow described modeling as a trial-and-error process, a sort of "tinkering with the theory of economic growth" (Solow 1988, 308). As Morgan (2012, 25) points out, model-building is not driven by a mechanical process, and involves not only the relevant articulated scientific knowledge, but also the modeler's craft-based, tacit knowledge as well as her "intuitive, imaginative, and creative qualities." Modeling is both a craft and an art: "[W]hat model building probably isn't, is a science, in the sense that I doubt that you can reduce model building to a model of model building" (Solow Interview 2011). Hence, in the first place, Solow's model served as a "working object."[14] Interacting with the

[13] Solow retrospectively puts these two kinds of models in opposition: "Either you can work it out mathematically or, and of course this was less true when I started in the 1950s, you can do computation, which you couldn't do then, so there was a bigger premium on simplicity in the 1950s and 60s than there is now" (Interview Solow 2011, 3).

[14] Daston and Galison (1992, 85) define the working objects of science as being "not yet concepts, much less conjectures or theories; they are the materials from which concepts are formed and to which they are applied." See also Morgan (2012, 387-9).

material of the model, the modeler-economist might develop new ideas: even though Solow had shaped the aggregate production function to fit the requirements of a simple model, the model was still a new kind of object and Solow did not know the results of his manipulations in advance. Solow even claimed he was surprised that the model's steady-state rate of growth was independent of the saving rate: "I thought it was a real shocker. It is not what I expected at all" (Solow in Snowdon and Vane 1999, 275).

With modeling becoming the dominant research practice, also the attributes associated with economics as a science changed. The English economist J.R. Sargent's talk "Are American Economists better?" delivered to the Political Economy Club in Nuffield College (November 12, 1960, Box 60, File S: 1 of 7), is an expression of the epistemic virtues of the ideal new modeling economist.[15] Sargent compared US-American- with English economics, characterizing the former as "more scientific, exploratory, more professional, more serious, and borne out by the higher degree of specialisation" (Sargent talk, op. cit., 6). He complemented this characterization with an account of the personal qualities he saw in American economists. Among these he emphasized the craft-aspect of a modeling enterprise, the – indeed truly "American" – quality of an "exploratory spirit," which could "in no way be better felt than by reading consecutively Robert Solow's 'Contribution to the Theory of Economic Growth' and Mrs. Robinson's 'Accumulation of Capital'." Putting aside the all too common dismissal of Joan V. Robinson and her work, it is striking that, for Sargent, what made Solow's work special was an ability associated with the skills of the modeler as a craftsperson: "No American economist ever thinks; he uses his analytical tools to arrive at meaningful theorems" (Sargent talk, op. cit., 3). Solow would distinguish himself precisely by not following mathematical rules, but by "being careless," by experimenting with ideas and by departing "from the

[15] Eventually, the talk was published in Sargent (1963). The year after, Sargent founded the Department of Economics at the University of Warwick, modeled after the kind of "American economics" he had investigated in his talk. The department was supposed to "use, and train its students to use the critical mathematical tools needed for analysis of complex economic issues" (Muthoo 2004). On the spread of what Sargent called "American economics" see also Section 6.

standards of rigour" (Sargent talk, op. cit., 5): "What is important is that the willingness to innovate exists. The experiment may fail, or it may succeed; but something may be gained, and nothing will be lost, by trying" (Sargent talk, op. cit., 4).

4 The model as an instrument of measurement

In an extensive chapter entitled "Qualifications" Solow emphasized that policy conclusions should not be directly drawn from his model. By assuming perfect foresight he had swept aside all kinds of Keynesian rigidities:

> Everything above is the neoclassical side of the coin. Most especially it is full employment economics – in the dual aspect of equilibrium condition and frictionless, competitive, causal system. […] It is not my contention that these problems [of Keynesian income analysis] don't exist, nor that they are of no significance in the long run" (Solow 1956, 91).

The point of Solow's model was not "to discuss the bearing of the previous highly abstract analysis on the practical problems of economic stabilization." It was instead supposed to serve as "a theoretical counterpart" to the "practical possibilities" of maintaining full employment (Solow 1956, 93). When reacting to Robinson's early critique of the use of a homogenous capital substance (Robinson 1953-54), Solow defended the collapsed production function on pragmatic grounds: it would simply be "useful", since it enabled the measurement of Y and C in the same units (Solow 1955-56, 103).

In 1957 Solow applied his growth model to time series data. He estimated the long-term rates of technical change by using the collapsed production function as a simple factor-share device. Written in "intensive form," each point of the production function associated a level of capital-labor ratio with a level of labor productivity (i.e., the output-labor ratio). The state of technology $A(t)$ – only later referred to as "total factor productivity" – was identified as an exogenous factor. Thus, changes in $A(t)$ shift the production function. This kind of empirical work was not supposed to provide a test of Solow's 1956 model, the aggregate production function or of marginal productivity. Rather, it was a way of interpreting given time series data, assuming that they were generated from an aggregate production function and that the competitive marginal-product-relations applied (cf. Weintraub 1988, 367)[16]: if the observed factor shares were exactly constant, Solow's approach would "yield an exact Cobb-Douglas and tuck everything else into the shift factor" (Solow 1974, 121).[17] While Robinson had contested the theoretical compatibility, and logical consistency of Solow's model, Solow presented it as a *useful* instrument for measuring "technological change" enabling the estimation of "a single production function for society," as Samuelson (1962, 193) would eventually call it.

5 The model as a prototype

Like Sargent, Solow emphasized the "exploratory spirit" of his artisanal engagement as a "reconnaissance exercise":

> I am trying to express an attitude towards the building of very simple models. I don't think that models like this lead directly to prescription for policy or even to detailed diagnosis. […] They are more like reconnaissance exercises. If you want to know what it's like out there, it's all right to send two or three fellows in sneakers to find out the lay of the land and whether it will support human life. If it turns out to be worth settling, then that requires an altogether bigger operation. The job of building usable larger-scale econometric models on the basis of whatever analytical insights come from simple models is much more difficult and less glamorous (Solow 1970, 105).

Here, Solow alluded to the function of his model as a prototype: the simple model served as a closed, clean and transparent world that could be manipulated and investigated. The simplicity of the model enables the modeler-economist to enter a new, unexplored, world. If the simple model proved productive it would provide a basis for building more complex econometric models. In retrospect, Solow would describe his pen-and-pencil operation as an engineering enterprise, not in the sense of engineering the

[16] As Boumans (2005, 121) explicates: "confronting a model with phenomenological facts rather means comparing this instrument with another instrument that generated these facts." See also his examples for economic models as instruments for measurement.

[17] Although Solow talks about Cobb-Douglas in this quote, in his 1957 article he does not assume a Cobb-Douglas production function but merely any function with constant returns to scale.

macroeconomy but as “engineering in the design sense.” Economists as “engineers in the design sense” do not design the economy – they design a model economy, which serves not only as a working object and instrument for measurement, but also as a kind of prototype for more complex measuring rods (Interview Solow 2011).[18]

The image of the design model as a prototype is found in several places in the contemporary literature. Oskar Morgenstern, for instance, explicated how simple models – “with proper abstractions and simplifications” – are used to investigate “the general properties of a system by its manipulation on the basis of a theory of the system” (Morgenstern 1954, 499-500). As an example from another field, Morgenstern mentioned the model of an airplane. Instead of computing the breaking point or determining it with a real plane, a model of an airplane is “put into a wind-tunnel and is exposed to stresses until it breaks” (Morgenstern 1954, 501). Such experiments could be carried out repeatedly with all kinds of variations in the smaller model. Correspondingly, the major asset of small models is their simplicity, because, if they are more complicated, they “lose their didactic value of visualizing economic interdependencies” (Morgenstern 1954, 504).

Similarly, Solow (1956) developed three cases with three different production functions – the Harrod-Domar case, the Cobb-Douglas case, and the set of constant-returns-to-scale production functions. In order to see what effects specific changes (introduction of technological change, a personal income tax, variable population growth, etc.) would have on the dynamic model economy, Solow used the case of the Cobb-Douglas production function: “generality leads to complications, and instead I turn [...] to the tractable Cobb-Douglas function” (Solow 1956, 86). In his empirical application, Solow (1957) used a general constant-returns-to-scale production function. The Harrod-Domar case was presented as

[18] Of course, Solow’s retrospective use of the term “engineering” might be motivated by contemporary issues, e.g. in order to make a counterpoint to the “engineering” notion of Lucas. Nevertheless, “engineering in the design sense” provides a terminology that nicely captures what I believe to be a specific characteristic of his modeling practice.

exhibiting a design-flaw: the constant capital coefficient led to the "knife-edge," in which any step away from the warranted rate of growth led to collision with a full employment ceiling or to mass unemployment and depression. Therefore, neither the Harrod-Domar case nor the "Leontief production function" was seen as a suitable component of a design for larger-scale econometric models.

In the second edition of *The Keynesian Revolution*, Lawrence Klein argued for a similar view of simple models as design tools for more complex ones. He presented the Keynesian model of income determination (with the three pillars of the propensity to save, the marginal efficiency of capital schedule and the liquidity-preference function) as a "pedagogical model" that had "teaching attributes," but provided "only a crude framework for thinking and illustrating main ideas" (Klein 1968, 193).[19] Klein noted that "intelligent discussion cannot be carried on unless this system is expanded to include fifteen to twenty or even more equations. In current econometric model construction, I am working with some macro-systems that have more than 100 equations" (Klein 1968, 223). The very simplicity that made a model useful for teaching purposes restricted its usefulness for dealing with complex data. In order to "explain the determination of output, employment, wage rate, price level, interest rate, and other variables" it had to be extended in a number of directions (Klein 1968, 212-3). The Klein-Goldberger model (1955) and the Brookings model are just two of many macroeconometric models constructed in the 1960s (cf. Nerlove 1966; Bodkin, Klein and Marwah 1991). Solow thought his simple model to provide a design for more complex constructions, among them dynamic input-output models, on which he had worked together with Samuelson in the 1950s, and multi-sectoral models such as Johansen (1960). Eventually, despite disagreeing with this line of research, Solow (1957) provided the prototype for a vast literature on "growth accounting," measuring changes in total factor productivity and the quantities of output and input (cf. Snowdon and Vane 1999, 277).[20]

[19] I will deal with Solow's model as a "teaching object" in the next section.
[20] Mata and Louça (2009) show how the "Solow's growth residual" developed a life of its own, being given to multiple uses – rhetorical, symbolic, and instrumental.

As Mirowski (1989, 328) and Sent (1998, 57) have both pointed out in slightly different contexts, engineers do, in their applications, deal with the particularities of the specific material even when working with the absence of frictions in theory. It is questionable whether the use of Solow's model as a prototypal design for larger-scale econometric models permits any kind of recalcitrance of the data. Solow's statement that "everything is tucked into Cobb-Douglas" holds for the long line of growth accounting exercises. Among others, Leontief would criticize the estimation of production functions as "methodological devices that [are] employ[ed] to avoid or cut short the use of concrete factual information" (Leontief 1983, 104).

6 The model as a teaching object

Both Morgenstern and Klein emphasized the didactic and pedagogical role of simple design models. Solow pointed to the connection between his way of modeling, and his teaching at MIT: "Remember that engineering is an academic subject, I lived my whole professional life in an engineering school, that's what MIT is, and so the notion that one of the functions of a professor of engineering is not necessarily to build a bridge but to design a bridge [...] came perfectly natural to us at MIT" (Interview with Solow, 2011; cf. Cherrier, this volume). After being involved in national planning and defense on an unprecedented scale during World War II, MIT aimed to return to its alleged primary mission: the professional education "of imaginative and creative leadership" (Report of The Committee on Educational Survey to the faculty of MIT, 1949, 4-5). To this end the humanities and social sciences should form a unified academic community with engineering, science, architecture and planning in order to launch "a coöperative attack on vitally important problems and a good educational unit for the development of leaders for a technological world" (Report Educational Survey 1949, op. cit., 46). Economics was thus considered an important part of a strong professional education not only for economists, but for businesspeople, and engineers alike.

One of the economists recruited to meet the increased teaching needs in the social sciences was Robert Solow (cf. Cherrier, this volume). In making dynamic economic issues intelligible to his engineering students, Solow had formulated the collapsed production function, the essential component of his design model. He used his simple model as a teaching device, i.e., as a "crude framework for thinking and illustrating main ideas" (Klein 1968, 193). One of the concern regarding the use of design models as teaching objects was that students and teachers too often stuck with these simple models: they were "too taken with technique and theory," and did not care enough "about the practical meaning" of what they were doing (Solow to Hunter, October 26, 1955, Box 55, File H: 2 of 3). In this respect Robert Dorfman complained to Solow about "our profession and the way it is set up to force the tremendous talents and energies of young men into made work and artificial tasks" (Dorfman to Solow, January 14, 1966, Box 53, File D: 2 of 4).[21]

Building simple, transparent and exploratory design models, and conducting econometric applications with a focus on clear-cut definitions and closed systems became the "proper" way of doing economics, and was associated with MIT from the 1960s on. MIT's students – whether destined to become modeler-economists themselves, experts in government or informed businesspersons – learned the craft: they were trained to reason with simple models, and to perceive economics as a "modeling science." As a teacher, Solow wrote shoals of letters of recommendations to help place the newly

[21] Also Joan Robinson pondered the pedagogical problems of simple models: "The dominance in neoclassical economic teaching of the concept of a production function […] has had an enervating effect upon the development of the subject, for by concentrating upon the question of the proportions of factors it had distracted attention from the more difficult but more rewarding questions of the influences governing the supplies of the factors and the causes and consequences of technical change" (Robinson 1953-54, 81).

educated modeler-economists in academia, government, business, the military, etc. He himself emphasized the "line of terrific graduate students that spread the technique" (Interview Solow 2011). For Solow, educating future teachers was the main way in which the "model building philosophy, and not only the model building philosophy, but the utility of building small, understandable, relatively transparent models, not to mimic or model the whole economy" spread (Interview Solow 2011; cf. Svorencik, this volume).

7 The "engineer in the design sense" as a technical expert

Solow's design model was used for various purposes: as a working object it served as a clean and transparent world for investigating utopian long-run equilibrium growth; as an instrument of measurement it was applied to time series data; as a teaching object it helped "spreading the technique." Used as some kind of prototype the design model was supposed to feed into larger-scale econometric models, and it was these models that lent legitimacy to the modeler-economist as a technical expert.

In a review of the Activity Analysis conference volume Solow stated that "[Koopman's] theorem is of immense significance for welfare economics, and for the possibility of a decentralized mechanism of economic planning" (Solow 1952, 427). Input-output modeling and linear programming were "technical economics proper" concerning, as Leontief would later describe it, the "effective application of mathematical methods to the operational solution of many practical economic problems" (Leontief 1965, 46). While input-output analysis models, and linear programming provided tools for directly solving "practical economic problems," such as the feasibility of production plans or the efficient allocation and utilization of economic resources on the level of a plant, an industry, or the entire economy, Solow's design model did not extend to direct application to economic problems, and the question of how the characteristics of the small, well-working design would apply to the kinds of phenomena that economic policy-makers were interested in was addressed by Solow as a matter of interpretation, involving further resources, i.e., different kinds of knowledge (Interview Solow 2011). However, the "usable larger-scale macroeconometric models" that were supposed to be built on the basis of the insights gained from the design model were indeed intended to serve as technologies for giving policy advice. With their help modeler-economists would be able to give clear answers to "what-happens-if questions" of economic governance (cf. Maas and Giraud, this volume; Porter 2006).

One of the bureaucratic bodies demanding the expertise of technical experts was the Council of Economic Advisers. With the end of the Korean War and the advent of the Eisenhower Administration, Washington had not only reverted to a less activist policy stance, but also brought the influence of economists connected with the now shunned "planning," to a minimum. This changed in the course of the 1950s, as economists established themselves as technical experts, and a new way of macroeconomic "planning with the market" developed (cf. Balisciano 1998, Goodwin 1998). With the Kennedy Administration economists had "revived their influence at the top" and were now again in a position "to influence policy from its start":

> Things are different now. Just count. Paul Samuelson and Dick Musgrave and countless others have been advising Kennedy since summer. Paul was offered the chairmanship of the Council of Economic Advisers and turned it down. It went instead to Walter Heller […] Jim Tobin will be a member of the Council of Economic Advisers and he is as good an economist as we have. Bob Roosa is undersecretary of Treasury. Speaking of RAND, Charlie Hitch […] has just been appointed Assistant Secretary of Defense (Solow to Sargent, January 5, 1961, Box 60, File S: 1 of 7).

A few months after writing this letter, Solow himself joined the CEA, chaired by Walter Heller. The kind of technical expertise that Solow and others provided came with left-liberal Keynesian views. Planning to dampen cycles, to create economic growth, low inflation, and low unemployment was a rather neutral concept; it was seen as an organizational necessity rather than a political option (cf. Düppe and Weintraub 2014, 25 with regard to Koopmans). As Solow noted in the early 1960s:

> Nowadays it has become clear that economic planning is a matter of degree, not a matter of Manchester liberalism on one side and Big Brother on the other. The government has an important guiding role even in capitalist countries, and the degree of centralization varies widely among socialist countries […] Instead of a full dress theory of economic planning, what makes more sense now is plain economic analysis and discussion of general principles, which will no doubt apply differently in different institutional contexts (Manuscript Review Dobb "Economic Growth and Planning," Box 54, D: 4 of 4).

Solow's model embodied the utopian idea that in the long run there would be another, better, world that was possible to reach through the appropriate macroeconomic planning (cf. Toye 2009, 222). With the design model developing a life of its own it became a symbol for the view that "there was nothing wrong with capitalism," even though Solow did not intend to provide an image of the long-term development of "the economy," and his interest in medium-run dynamics notwithstanding (cf. Assous 2013). He was to regret having unleashed "a standing temptation to sound like Dr. Pangloss" on behalf of the economics profession (Solow 1988, 309; cf. Mata and Louça 2009).

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