Models, Measurement, and "Universal Patterns": Jan Tinbergen and Development Planning without Theory

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In a special issue of *History of Political Economy* devoted to Robert Solow and the development of growth theory, Brian Snowdon (2009, 243) expressed the view that "development economics and growth theory evolve[d] along separate paths for almost three decades in the post-1956 period." Solow himself espoused this view, as he made clear in an interview Snowdon and Howard Vane conducted with him in 1998. Solow also offered an explanation, ascribing the difference to distinct temperaments. Thus:

> On the whole the personality types in the profession who became interested in economic development were not model builders. They were collectors of data and generalizers from rough empirical data, like Simon Kuznets; or they were like Ted Schultz, really deeply into underdeveloped agriculture, or they were people interested in history and backwardness for its own sake. That sort of temperament is not suited to model building. Growth theory, *par excellence*, yielded to model building. . . . The people who got interested in the theory of economic growth were interested in model building. (quoted in Snowdon 2009, 243; quoted in Snowdon and Vane 1999, 275)

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Solow's multiple-temperaments account undeniably has traction. That being so, however, the challenge for the historian is to account for individuals for whom it does *not* hold. One such who, almost uniquely, was among the most prominent in both categories—development planning *and* growth modeling—was Jan Tinbergen.

Although the practice of mathematical modeling was new in the 1940s and 1950s, and Solow contributed in 1956 in a manner that defined further analysis of this sort (Halsmayer 2014), a case can also be made for viewing Tinbergen as the founder of this very practice (Boumans 2005). Moreover, concentrating on the progress of growth modeling alone, Tinbergen may be assigned temporal precedence, his 1942 model being mathematically equivalent to the 1957 Solow model (Boumans 2009). The main difference between Tinbergen and Solow, however, was not so much one of temperament; it had to do more with Tinbergen's simply seeing no point in applying abstract models to deal with issues in economic development. This distinctive attitude was emphasized by Bent Hansen in his characterization of Tinbergen's contributions upon the latter's being awarded, jointly with Ragnar Frisch, a Nobel Prize for the *development and application of dynamic models*. Thus Hansen (1969, 332):

> Tinbergen's scientific contributions to the theory and practices of long-term economic planning for growth are typically Tinbergian in the sense that he has been looking for simple crude methods that "work" under primitive conditions of policy-making in underdeveloped countries. Here . . . the contrast with contemporary work by mathematical economists, especially in the United States, is striking. Although he is himself an excellent mathematician, Tinbergen took little part in the discussion of topics like optimal growth rates, turnpike theorems and dynamic efficiency. Being essentially an extension of modern welfare theory, these refinements had little practical relevance for development planning. Tinbergen's long-term planning models were designed on the basic assumptions that only a minimum of statistical information is available, and that the skill of planners, administrators and politicians is limited.

What Hansen dubs "typically Tinbergian" actually applies in one sense to most of the economists who began work in the new field of economic development in the 1950s. A widely perceived barrier to applying to "underdeveloped" countries analyses of growth in industrial, capitalistic economies was a lack of economic data to use in assessing the perfor-

mance of the less developed. It was also the case that systems for measuring the economic performance of a country's economy were still in their own early stages of development and had been built for just a few developed countries. Tinbergen, for example, had created the first macroeconometric model of the Netherlands in 1936 and a second, of the United States, in 1939, but after the war these had to be redesigned to turn them into more appropriate tools for country-specific policy purposes. Wassily Leontief developed his input-output table for the United States, and it came to be used in running the war economy there in the early 1940s; but a decade later—thus in the McCarthy era—its strong association with planning meant that it was unlikely to become a key economic policy tool in the United States (Kohli 2001). Yet a third measurement system was that of National Income Accounting. This system was encouraged in Depression-strapped 1930s America. During World War II, National Income Accounting was specifically adapted by Richard Stone and James Meade to running the war economy of Great Britain, and that of the United States by Simon Kuznets (Comim 2001; Tily 2009). But as soon as attempts were made to apply this system to countries in Central Africa in the late 1940s and early 1950s, it became clear how difficult it would be to transpose a framework based on the experience of Western economies to economies whose social and cultural norms were very different from those of the West (Morgan 2011).

Politics aside, by the early 1950s it was apparent that all three sorts of systems for measuring economic performance would have to be built separately, country by country, and that even then it would be many years before they could be used for policy purposes, not least because trustworthy data with the necessary intra- and intereconomy coverage simply were not widely available.

Overcoming these lacks may help explain why researchers in the United States such as Hollis Chenery found funding in the 1960s for empirical research into the development and growth for a whole slew of the more advanced economies. By 1969 Chenery had amassed a collection of country and cross-sectional studies, many by or with others (see the 1975 volume edited by Moshe Syrquin). A number of these studies were supported by funding received by the Harvard Project for Quantitative Research in Economic Development, in the Harvard Center for International Affairs, and coming from the National Science Foundation, and the US Agency for International Development (USAID). Chenery's studies were pathbreaking both in terms of method and in the data on which

they drew, namely, UN National Income Accounting data for sixty-one countries during 1950–64, and included specifically GDP per capita and industrial data for a common array of fifteen industries and eight industrial sectors such as electricity.

Tinbergen is our primary focus here, but by 1968 it was apparent that he was positively impressed by Chenery's multiyear set of interlocking research efforts. At the time Tinbergen was professor of economic development at the Netherlands School of Economics in Rotterdam, and he persuaded his colleagues to agree to the school's bestowing an honorary doctorate on Chenery. A text making the case for this unusual acknowledgment of work by an outsider was penned by Tinbergen (1968) in terms that reveal his own convictions as to why development was so crucial to universal well-being and peace. He did not mention growth models in the Solow-Swan or Harrod-Domar tradition, but stressed a feature he found in Chenery's work, namely, that it was "efficient" science, a term Tinbergen borrowed from Gunnar Myrdal.

This term covered the selective use of sophisticated modeling techniques to reveal the most effective allocations and sequences thereof within development plans. It also encompassed input-output analysis as a way to illuminate a country's industrial structure, and family budget studies to shed light on demand. All these features Tinbergen saw in Chenery's research; he also noted, approvingly, Chenery's support for the studies of Irma Adelman, who developed "yardsticks for some 25 social, cultural, or political indicators" (1012), even though these were unmeasurable with the tools familiar to economists. Tinbergen also applauded Chenery's astute use of concepts such as absorptive capacity in relation to efficient/effective foreign funding for development, rather than just treating balance of payments deficits as lumpen "obstacles" to the realization of industrialization.

In the late 1950s and 1960s Chenery investigated whether, in the UN series to which he had access, statistical analysis revealed what he called a "universal pattern" of industrialization. This was bold at the empirical level, and it impressed Tinbergen for focusing as it did on empirics rather than theoretical modeling. Chenery opted to estimate the effects of a limited number of exogenous causes, including in the first instance rising income per capita and, among knock-on effects therefrom, the nature and degree of shifts in domestic demand (expected to be away from domestic agriculture and toward manufactures) and in the pattern of trade (expected to be toward new, exportable manufactured goods).

Something like that pattern had been identified earlier by Simon Kuznets (1964). Chenery's work, however, was not only more inclusive; it involved sophisticated modeling of exogenous demand and supply changes and in scale and their effects, amounting to a sort of general equilibrium analysis of physical demand and supply on a scale that required the deployment of computational skills. Tinbergen correctly saw in Chenery's work a triumph of data and empirical technique that was genuinely pathbreaking, and his short 1968 article made precisely that case.

As for technical progress, Chenery and others faced what Kuznets (1964, 53) called "the technological revolution in transport and communications," which "is the basis either for a powerful material technology or for the social institutions and devices that provide the proper auspices for the new tools and production methods," though "we cannot, in the present state of our knowledge, express this stock in meaningful quantitative terms." Indeed, a project reported on by Chenery in 1969 modeled what was a general equilibrium in name only. For example, it did not include prices, and the project simply followed Kuznets in acknowledging that there had been improvement in something economists could not explain, let alone measure. Chenery modeled it by assuming that technical change is a function of GDP alone, and he employed a constant elasticity of substitution (CES) production function, tacitly assuming thereby given and neutral increases in efficiency. Just as Tinbergen stressed the limitations of data, Chenery was well aware that positing a given, neutral increase in efficiency simply left unexplained the likely human element, in particular the administrative decision-making role in development or, as his one-time teacher Kuznets (1964, 120–21) put it, "the problem of the 'how' of economic growth."

If modern economic growth, Kuznets emphasized, "is, in essence, a controlled revolution in economy and society, and the revolution in society, with its internal and external ramifications, is an indispensable part of the total process, economic growth is neither fully understood, nor properly measurable and analyzable, in a study limited to traditionally defined economic variables" (128).

As our title indicates, research on planning and development, particularly by Tinbergen and Adelman, though in one sense also by Kuznets and Chenery, was "without theory." This phrasing alludes to the "Measurement without Theory" debate, initiated by Tjalling Koopmans (1947), as to what kind of empirical research can claim to be the most scientific. According to Koopmans, a scientific approach would have to be deductive; hence he

prioritized the role of theory. "Theory" had a specific meaning, which Koopmans did not define in this debate, but in a 1957 essay, "The Construction of Economic Knowledge," he defined "theory" as "a sequence of conceptual models" and a "model" as "a set of postulates, of which the implications are developed to the extent deemed worthwhile in relation to the aspects of reality expressed by the postulates" (142). By contrast, both Tinbergen and Adelman defended an inductive approach, where theory has a more modest role, namely, that of providing suggestions as to which kind of influences one may take into account once measurement has shown that they are significant. This inductive approach was prompted by pragmatic reasons: there were simply no "sets of postulates" that could cover the complexity of development. Moreover, there were no sufficiently reliable data available. For these reasons, Tinbergen developed a pragmatic approach: considering the lack of complete theory and data, he proposed a modeling strategy with the aim of designing models that should work under idiosyncratic conditions.

Tinbergen and Planning in Stages

Tinbergen saw his work on the development and application of models only as a contribution to the development of tools that were needed for an overriding purpose, namely, the reduction of poverty. This engagement was his reason for building the first macroeconometric models. In particular, the first Dutch model was meant to supply a framework for designing policies that might get the Netherlands out of its lingering 1930s recession. The model was part of a larger "plan" developed within the Dutch Labor Party. Because of the outbreak of the Second World War, the plan was never implemented, but after the war, it became the blueprint of a newly established economic policy advisory body, the Central Planning Bureau (CPB), of which Tinbergen was named the first director. The initial task of this new body was to develop a macroeconomic model of the Netherlands that could be used to design postwar economic recovery (Van den Bogaard 1999).

Tinbergen's "planning" did not mean a specific kind of policy. "Planning has nothing to do with the type of policy involved. Planning in our sense can be applied to any type of policy" (Tinbergen 1956, 10). He used this term in the sense of "designing" and "thinking ahead."

An important experience intervened, causing Tinbergen to redirect his engagement from the formulation of steps toward economic recovery for

the Dutch economy to international poverty concerns. In 1951 he was invited to the twenty-seventh meeting of the International Statistical Institute, held in New Delhi and Calcutta, India. "Although in Holland we had been hungry during 1944–45, the last winter of the occupation of Hitler's army, the poverty prevailing in India—as a normal situation—was such a contrast that it redirected my thinking and main activities" (Tinbergen 1984, 316–70). As a result, he left the CPB in 1955 to occupy a new chair for development programming at the Netherlands School of Economics in Rotterdam. This responsibility he combined with directorial responsibility for a new division of Balanced International Growth at the Netherlands Economic Institute (NEI), affiliated with the University of Rotterdam, for fundamental and applied research into the problems of developing countries (Bos 1970, 141).

At the NEI Tinbergen developed a specific, pragmatic approach to development programming, one that included "semi input-output models," thus building on Leontief's work. As we show in greater detail below, Leontief and Tinbergen brought a shared empirical and pragmatic view to planning, although, in 1950, when Tinbergen first became acquainted with Leontief's approach, he did not yet see that for planning purposes input-output models were more pragmatic than his own macroeconometric models.

In September 1950 the NEI (1953, v), "on the initiative of professor Leontief," organized a conference on input-output relations in Driebergen, Netherlands. This event took place a year after a conference on activity analysis, held in June 1949, and organized by Koopmans (Koopmans 1951; Düppe and Weintraub 2014). The "Activity Analysis" conference became famous for its introduction and discussion of linear programming. The brainchild of George Dantzig, linear programming was described by him in 1947 as an optimization technique for planning the activities of the US Air Force. The relationships between these activities and the precisely specified goals to be achieved were similar to the input-output relations of Leontief's models, and there was, in this limited sense, some overlap between the "Activity Analysis" conference and that initiated by Leontief. Neither Tinbergen nor Leontief attended the "Activity Analysis" conference, though both participated in the Driebergen meeting. Indeed, only two participants at the conference attended both, Koopmans and Oskar Morgenstern. In Driebergen Leontief presented his "Input-Output Approach in Economic Analysis" (1953). In the subsequent discussion Koopmans asked for an "indication of the class of policy problems to

which the input-output analysis gives practical answers" (NEI 1953, 24), a question he then also answered by stating that input-output analysis gives no indication as to what production methods to adopt. As Koopmans put it: "[Input-output analysis] does not in its original form help to make or predict the choice of methods of production. Similarly, the analysis in terms of prices seems to reflect mainly the distributive rôle of prices (e.g. the determination of profit rates of individual industries) rather than their allocative rôle as guides to the choice of production methods" (NEI 1953, 24).

In his own comments, Tinbergen asserted a link between his work on "macro-economic models" and the "micro-economic models" of Leontief. He noted that "whereas macro-economic models seem most appropriate for studying problems of business cycle policy and balance of payments problems, problems of war economy and bottlenecks are better dealt with by micro-economic models" (NEI 1953, 24). He went on to outline his ideas concerning economic policy as he was developing them at that time at the CPB. These were published as *On the Theory of Economic Policy* (1952) and *Economic Policy: Principles and Design* (1956). The paper presented by Koopmans (1953) at the Driebergen conference, by contrast, was a summary of "some properties of generalized input-output models, developed by various authors in some recent investigations published as contributions to 'Activity Analysis of Production and Allocation.'" This summary was largely an exposition of the new technique of linear programming.

In 1950 Tinbergen was mainly interested in developing a design for economic policy based on macroeconomic models devised at the CPB and for Dutch policy purposes. At the time he saw no use for the "micro-economic models" of Leontief. However, that changed after 1951, when he started to think of designing development programs. His later reflections appeared initially in *Design of Development* (1958). Although *Design of Development* was published only in 1958 as the second document commissioned by the Economic Development Institute of the International Bank for Reconstruction and Development, Tinbergen had completed the manuscript in 1955. It was originally written at the request of Léonard Rist, economic director of the institute and, to judge by its repeated use, was much valued as a training manual for senior administrators from developing countries. However, official publication was delayed for what Tinbergen perceived as political reasons. Recounting things thirty years later, he wrote: "At their request, I dealt with an additional example of state financing in the Dutch steel industry. The report was accepted but not published then; I am afraid the presi-

dent of the Bank at that time did not agree with the opinion I expressed" (Tinbergen 1984, 325n), involving as it did support for the state's funding of selected important industries.

Politics aside, Tinbergen, in *The Design of Development* (1958), asserted that development policy has "four principal objectives":

(i) To create the general conditions favorable to development;
(ii) To acquaint the government itself, the business community and the public generally with the potentialities and advantages of development;
(iii) To actually make a number of investments, usually of the "basic" types; and
(iv) To take measures designed to facilitate and to stimulate private activity and investment. (3)

All four objectives counted equally. Under the general conditions for development, Tinbergen included a minimum level of security and stability; a minimum number of "instruments of economic policy" available to the government; the absence of "extreme inequalities in income [inequalities being] conducive to social unrest and lack of cooperative spirit in production" (5); and the "provision of training and education at all levels" (5). To acquire useful knowledge about "development potentialities and advantages," Tinbergen insisted, "reliable statistics" are needed for production, trade, prices, government finance, and income and income distribution. Even though the available statistics all too often are incomplete and unreliable, Tinbergen emphasized that one should nonetheless attempt to arrive at both completeness and reliability: "Anything helping to supplement the available statistics therefore should be welcomed" (10).

Detailed "knowledge" of specific circumstances was required because the type of programming most appropriate to these circumstances was dependent on it. "Knowledge" included every kind of information available, whether statistics or accounts provided by experts. Knowledge was essential on the "stage of development" of a country (25), the "degree of activity and initiative in the private sector" (26), the "particular bottlenecks" a country is facing (26), the "general attitude of the people with regard to government measures: the degree of public spiritedness and the willingness to cooperate, and in the quality of administration" (27). It is also desirable to know the "quality and nature of the data available" (27).

To create a program satisfying the four objectives listed earlier, an appropriate method must be chosen. That choice, according to Tinbergen,

depends on "experts and the nature of data available" (20). Linear programming was mentioned as one of the useful methods, but he emphasized that it was a complex technique, to be used only by experts such as Chenery. And there was more against the technique. For linear programming, one needs a model of how individuals in an economy choose and behave, but as Tinbergen (1958, 81) concluded, such "an exact system of equations does not exist, and if it did it would be very complicated. It would be dynamic and micro-economic. For practical purposes, it will be necessary to simplify without, however, affecting essential features. Two problems then seem outstanding: what degree of *aggregation* can we apply, and what simplified picture of *development* can we use?"

In addition, for the "appraisal" of the consequences of a project or program, "accounting prices representing the 'true values'" (82) would be needed. The problem here is how to calculate them. Tinbergen (1958, 84) noted that "especially in this field, scientific development is fast and . . . new methods are being continually launched. Some of them are of a very complicated mathematical nature and require a large quantity of data; others are less exact and easier to handle. Again, what should be recommended will have to depend on the details of the situation in a given country."

The necessary "accounting prices" could be calculated by linear programming, though this would require a detailed mathematical model and much data. Or they would have to be determined in another way. This alternative way was not further specified in Tinbergen's *Design of Development*, but in his later *Central Planning* (1964), he discussed in much greater detail how to acquire the needed information about an economy. To do it, one needs "contacts with ministries, lower public authorities, regional or sectoral planning agencies, business organizations, trade unions, and research institutions" (Tinbergen 1964, 14). The following quotation details in what way these experts might be involved:

> The instrument of outside contacts is, as a rule, the *meeting*, although there are simpler methods such as the letter or the telephone conversation.
>
> Meetings serving primarily the purpose of informing the planners may take one of two main forms. We first have the meeting where outsiders supply factual and numerical information, which in principle must be statistical data, including coefficients, e.g. the supply elasticity of agriculture. This type of information can best be supplied by experts of the sector concerned, agriculture in our example; but these experts may be interested parties, which creates a problem as to the reliability of the information. The contacts between sector experts and general (or

central) experts in planning also create an interesting problem as to the proper questions to be asked of the sector experts. The data solicited should in fact be information about the coefficients rather than about the variables; in our example, the supply elasticity rather than the quantity to be supplied. The latter, being an unknown of the planning problem, should properly be found by the solution of a number of simultaneous equations, which is the task of the central planner. In other words, the sector expert should not be allowed to decide on the production figure, because that depends on other coefficients—the price level, the income level, and so on—on which he is not an expert.

The second type of meeting for information purposes is one where the central planners discuss with other experts the uncertain elements in the relations they use and the methods they apply. This is more like a scientific discussion, an exchange of views, and this is very often necessary in a new field like planning. (97–98)

The first type of information here is supplied by experts and is strikingly similar to Leontief's idea of "direct observations," in place even of official statistics (Leontief et al. 1953). Therefore, before we proceed with Tinbergen, we should first have a closer look at Leontief's empirical approach.

Leontief presented his approach most explicitly in his 1970 presidential address to the American Economic Association. There he noted the importance of knowledge about the "structural relationships" that govern an economic system. But "in contrast to most physical sciences," these relationships are "in a state of constant flux" (Leontief 1971, 3). Therefore, this system of relationships needs a "steady flow of new data" and knowledge beyond the traditional domain of economics: "The pursuit of a more fundamental understanding of the process of production inevitably leads into the area of engineering sciences. To penetrate below the skin-thin surface of conventional consumption to develop a systematic study of the structural characteristics and of the functioning of households, an area in which description and analysis of social, anthropological and demographic factors must obviously occupy the center of the stage" (4).

To obtain this type of information, however, direct observation was considered more appropriate than what Leontief called "indirect statistical inference," which was what was practiced at the Cowles Commission. Such inference would just be "circular," neither widening nor deepening the empirical foundations of economic analysis, because we then construct models in which prices, outputs, rates of saving and investment are

explained in terms of production functions, consumption functions, and other structural relationships. However, to measure the parameters of these relationships, we use the magnitudes of prices, outputs, and other variables, in circular fashion.

This view had always underpinned Leontief's input-output analysis. Since the 1930s at Harvard, to collect data necessary to fill the input-output table, Leontief, with his assistants, wrote letters, "called up industries, particularly firms which were engaged in the distribution of commodities, and got the data from them" (quoted in Foley 1998, 121; Carter and Petri 1989). It should be stressed that not just anyone was asked for their observations. They asked only engineers, technicians, and other experts on a relevant sector or component in the economic system, such as ironmasters and specialists in animal husbandry.

This last fed directly into Tinbergen's conviction, expressed in his 1958 *Design of Development*, that will and belief are keys to success in the face of difficulties during the application of development plans. But first those involved and responsible must acquire the relevant detailed information. Similar thinking had been articulated by Leontief (1953, 7–8) at the Driebergen conference: "Such empirical description requires many months of work by a large staff of experienced economic statisticians and experts intimately acquainted with the various branches of manufacturing, mining, agriculture, transportation, etc." According to Leontief, this type of information also determines the level of abstraction that should be aimed at. It makes no sense to ask "a manager of a steel plant or a metallurgical expert" for information on too abstract a level, for example, information about a theoretical entity such as a production function. "Hence, while the labels attached to symbolic variables and parameters of the theoretical equations tend to suggest that they could be identified with those directly observable in the real world, any attempt to do so is bound to fail" (Leontief 1982, 104). Leontief's attempts to achieve the right kinds of information require empirical work that is characterized by two pervasive concerns: disapproval of aggregate variables and an emphasis on enlarging the primary database for economic analysis with engineering and technical data (Carter and Petri 1989, 17).

To deal with this problem of getting the right kinds of data, Tinbergen, together with his colleague Hendricus Bos at NEI, in time worked out a pragmatic development program that was characterized by "planning in stages." "One of the reasons why 'planning in stages' recommended itself is the simplification of the methods which make it possible. Tinbergen and

Bos argued that this brings these methods within the reach of a larger number of experts and planning offices. The decomposition of the planning process into stages also enables a simpler explanation of the various aspects of the decisions to be taken to the political authorities and others concerned with the execution of the development policy" (Bos 1970, 143).

The first required stage consists of "a macroeconomic study of the general process of production and investment, along the lines suggested by Harrod-Domar models or by similar, somewhat more complicated models" (Tinbergen and Bos 1962, 10). "A second stage may consist then in specifying production targets for a number of sectors over a fairly long period. A third stage, if needed, may go into more detail for a shorter period, giving figures for a larger number of smaller sectors. A fourth stage may consist in 'filling the plan out' with individual projects" (Tinbergen and Bos 1962, 10). The reason given by Tinbergen and Bos for "simplifying" the programming problem by breaking it down into stages was pragmatic: "[The] programming of development requires a number of different abilities and types of knowledge. As a rule the process consists in estimating such a large number of figures that first of all considerable organizing abilities are needed" (4–5). These different stages allowed for a way to organize "the cooperation of a large number of experts" (5).

In Search of a Pragmatic Methodology for Development Economics

As we have shown, Tinbergen held Chenery's work in high esteem. The respect was mutual. Tinbergen's *Design of Development* (1958), for example, was prized by Chenery (1959) above Albert Hirschman's *Strategy of Economic Development* (1958) in an invited review of those two recently published books on development. Hirschman stressed, as a key determinant of success or failure in a plan, decision-making ability and willingness among those directly responsible to apply it. He also stressed a commitment to change by the citizens at large in a country pursuing a plan. These two factors, neither of them quantifiable, were more or less of a piece with some of the things Tinbergen urged in *The Design of Development*, and to which he subsequently gave prominence in his Nobel address (Tinbergen 1970), where he praised Adelman and Cynthia Taft Morris's introduction of multiple social and political indicators of the growth process.

Although Chenery's general focus was economic, he was also instrumental in enabling Adelman and Morris's factor analysis of development.

Adelman acknowledged this, expressing gratitude to Chenery in the preface of *Society, Politics, and Economic Development* (Adelman and Morris 1967, vii; see also Adelman, Zilberman, and Kim 2014, 7).

Adelman was one of the first economists who showed how quantitative empirical research of economic development could be widened to include noneconomic issues, a widening that has become a more accepted scope since the 1970s. In a working paper (2000) in which she reflected on fifty years of economic development, she made quite precise what this widening of scope entailed. According to her, economic development combines "(1) self-sustaining growth; (2) structural change in patterns of production; (3) technological upgrading; (4) social, political and institutional modernization; and (5) widespread improvement in the human condition" (1).

Development economists of the 1950s and 1960s used the term *development* in the sense of the first three elements only. Among those who followed, some included the other two in their research programs. Adelman and Morris (1967) showed empirically that economic development is multidimensional, that is, it is "an interrelated multifaceted process" and "the rate of economic growth is intimately linked to changes in social, institutional, cultural and political factors" (Adelman 2000, 2). As Adelman put it, they "rejected the Solow model, in which technology is the same across countries, in favor of a model in which technology differs and preexisting social factors play a role in the speed [of change]" (2–3). This encapsulates, too, her implicit rejection of Chenery's approach of the 1960s.

In his Nobel lecture "The Use of Models," Tinbergen (1970, 250) drew attention to "the impressive research of Irma Adelman and her collaborators" for their "simultaneous introduction of many social and political variables into models, especially for developing countries," "using factor analysis and discriminant functions in order to discover which of some thirty odd factors, measured in a heroic way, seem to play a preponderant part in the process of development." As with his own work, Tinbergen had to admit that their approach was "measurement without theory" but emphasized that it was "meaningful . . . as an exploration of a new territory of science" (250).

The 1950s and 1960s was a period in which the "scientific" standards for empirical economic research were set by the methodology of the Cowles Commission. This methodology is characterized by hypothesis testing, which implies a deductive methodology starting from a priori theoretical assumptions. This approach was expressed most strongly in Koopmans's 1947 review article "Measurement without Theory." Because of the increasing dominance of the Cowles Commission view in the 1950s

and 1960s, any other kind of empirical research was considered less scientific. That included the work of Adelman and Morris. Indeed, just such a criticism was published in 1970, written by Peter Eckstein. According to Eckstein (1970, 227), Adelman and Morris's research "neglects some of the prime rules of scientific inquiry and offers, therefore, no reliable guide to policy-making," since it did not reveal the causal structure as Cowles methodology required.

In their reply, Adelman and Morris (1970, 236) emphasized that they had not claimed to offer a "reliable guide to policy-making" and "certainly not" to have "separated out causes, effects, and joint effects." They had instead opted for an inductive approach as "most efficient in view of the extremely small quantity of validated knowledge concerning the 'laws' governing the complex interactions between economic, social, and political forces in the process of economic development and modernization" (Adelman and Morris 1970, 236). Further, they argued, "the unsatisfactory state of general theory of social change and modernization" was not the only rationale for their approach, but also "the inadequacies of factual information on developing countries, and the prohibitive cost of direct measurement" (237). Indeed, direct measurement of the relevant influences was rarely possible because of "overwhelming" data deficiencies, while choices of indirect measurements were constrained by the small number of feasible alternatives for measurement of each influence (240).

Conclusions

Development economics in the 1950s and 1960s, as Tinbergen and Adelman saw it, was a "groping in the dark." This characterization is that of Milton Friedman (1951, 112–13). It was just part of Friedman's wholesale denunciation of the Cowles Commission research program, according to which a model must be constructed for an economy as a whole. This requirement runs up against limited information and understanding of the "dynamic mechanisms at work," restrictions made worse by "limitations of resources—mental, computational, and statistical." While, on the whole, it was the US economy that Cowles researchers were modeling, the problems were even more severe for attempts to model an "underdeveloped" country for planning development.

Besides the limited knowledge of dynamic mechanisms, the lack of data was a severe problem for any attempt at modeling, whether macroeconomic, input-output, or in the tradition of national income accounting. As concerns the three main figures we have discussed—the empirical

researchers Tinbergen, Chenery, and Adelman—each developed his or her own approach to modeling: modeling in stages; modeling to capture universal patterns; and factor analysis, respectively. These attempts were marginalized in the 1970s when neoclassical growth models capturing stylized facts became the dominant approach. Of the three approaches we have discussed, Chenery's comes closest to the later dominant approach, though relatively little of his work has been studied with these issues in mind.

Tinbergen, Chenery, and Adelman shared a common inductive methodology, which might rightly be called "measurement without theory," in the sense that there was no economic theory that could help them in organizing the available messy, and often unreliable, data. Chenery saw a role for abstract mathematical models as helping inquirers to rise above such impediments, even if only temporarily, or to gain focus, as was the case with his making technical progress an exogenous variable. Tinbergen's models of the "first stage" were also intended to make planning for development tractable, but without mixing planning with such focusing devices. Adelman, for her part, preferred to search for and identify factors of development. Who and what steps were "right"? The question was put but not answered at the time in ways satisfying to all participants in development planning of the 1960s and beyond.

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