Volume Title: Political Arithmetic: Simon Kuznets and the Empirical Tradition in Economics

Volume Author/Editor: Robert William Fogel, Enid M. Fogel, Mark Guglielmo, and Nathaniel Grotte

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-25661-8, 978-0-226-25661-0 (cloth)

Volume URL: http://www.nber.org/books/foge12-1

Conference Date: n/a

Publication Date: March 2013

Chapter Title: The Scientific Methods of Simon Kuznets

Chapter Author(s): Robert William Fogel, Enid M. Fogel, Mark Guglielmo, Nathaniel Grotte

Chapter URL: http://www.nber.org/chapters/c12917

Chapter pages in book: (p. 89 - 105)

5 : : The Scientific Methods of Simon Kuznets

"Anyone can start a row in economics; it is much harder to find out what is really happening to the economy." Simon Kuznets made this statement during a conversation he had with Henry Rosovsky, who was then chairman of the Economics Department at Harvard University and later became dean of the Faculty of Arts and Sciences, and Robert Fogel at Harvard in the early 1970s. Fogel was startled when he said it since their profession thrived on controversy. Indeed, to many economists, cleverness in debate, rather than the applicability of the debate to any issue of the real world, is what economics is all about. To Kuznets, however, there was a real economic world, and the task of the economist was to describe it accurately and to explain it in a way that would be helpful to those who had to make economic policy.[1]

Some Aspects of Kuznets's Approach to Economics

If there was any aspect of Kuznets's approach to economics that may be said to have dominated all the others, it was his concern with the great policy issues of his age. Emphasis on this point may surprise those who are familiar with his work since he never became directly involved in those highly politicized disputes over economic policy that

1. For a general introduction to Kuznets's scientific method, see Easterlin (1989), Abramovitz (1971, 1985), Patinkin (1976), Ben-Porath (1986), Bergson (1986), and Bergson, Leibenstein, Rosovsky, and Griliches (1987).

often split the profession into partisan camps. Moreover, many of the problems on which Kuznets worked, such as the relation between the rate of population growth and the rate of technological innovation, are hardly likely to be resolved or even affected significantly by new legislation, nor did his findings on such issues enter prominently into the shifting partisan alignments of his age. Nevertheless, he recognized the importance of the points at issue in the political debates over economic policy, and he believed that the development of a reliable body of evidence bearing on these issues was an urgent task of economists. He saw economics as an empirical science aimed at disclosing the factors that affect economic performance.

It is important to keep in mind how new the issues with which Kuznets grappled during his career were when he first began to address them in the mid-1920s. The proposition that Western Europe and America had undergone an irreversible economic transformation—an industrial revolution—was not effectively enunciated until the end of the 1880s. Although optimism about the economy was widespread during the first three decades of the twentieth century, these years also spawned influential theories that economic progress was grinding to a halt. The notion of a general crisis for capitalism, set forth in the work of such socialist or radical theorists as J. A. Hobson of England, Rudolf Hilferding of Germany, and Vladimir Lenin, became widely accepted by professional economists during the 1930s.[2] Alvin Hansen, in his 1938 presidential address to the American Economic Association, suggested that a correct fiscal policy could bring an end to secular stagnation. Despite a certain optimism, that speech seemed to endorse the view that secular stagnation was the natural condition of free market economies in the twentieth century and that capitalist economies could be kept afloat only with massive government intervention (Hansen 1939; cf. Abramovitz 1952).

When Kuznets first began his work on economic growth in the mid-1920s, not all the processes that he later identified had worked

2. J. A. Hobson (1858–1940) was an English political thinker and a promoter of "New Liberalism." Rudolph Hilferding (1877–1941) was a Marxist theorist and the chief theoretician of the Social Democratic Party of Germany.

themselves out. Europe and America were still passing through their demographic and epidemiological transitions (U.S. life expectancy at birth in 1920 was still under fifty-five years), and the nature of these phenomena was not yet fully apparent. It would be another two decades before the theory of the demographic transition—which explains that declines in the death rate lead, with a lag, to comparable declines in the birth rate—was formulated, and it would be another three to four decades before it became clear that the economic advances of the last half of the nineteenth century were part of a new epoch of economic growth that was about two centuries old and that was in the process of spreading from its origins in Western Europe and in certain countries of European settlement to the impoverished nations of Africa, Asia, and Latin America.

Kuznets considered the acceleration of population growth during the nineteenth century as not only one of the most important consequences of economic growth but also a major factor contributing to it. A particularly important aspect of the phenomenon was the concentration of the decline of death rates at early ages, which contributed to the reduction in fertility rates. The reduced fertility rates released a large proportion of the female labor force to gainful occupations, accelerated the transition to modern families, mobile and responsive to economic incentives, and promoted new ideologies conducive to economic growth (Kuznets 1966, 56–62). In this connection, Kuznets noted the increase in the share of women in the U.S. labor force from 17 percent in 1890 to 27 percent in 1950, which he attributed to the lower fertility rates, the shift in employment opportunities from manual to service-sector positions, and urbanization, which made organized labor markets more accessible to women. He also called attention to the fact that the most rapidly growing occupations—those in the professional, technical, clerical, sales, and other services—were the ones in which women had made the greatest inroads. Nevertheless, in the late 1950s and early 1960s, when the new women's movement was still incipient, he anticipated neither the explosive entry of women into the labor force during the next quarter century nor the new ideology that would facilitate that development (Kuznets 1966, 193–95).

Another aspect of Kuznets's method was his approach to the establishment of the priorities for empirical research in economics. At any moment, there are more issues and problems demanding the attention of economists than there are resources to address them. In Kuznets's view, the priorities for research were determined by a complex interaction of three factors: (1) the needs of policymakers inside and outside the government, particularly, the issues that they considered paramount for promoting economic growth, stability, and equity; (2) the beliefs of economists and other social scientists regarding the most effective measures for resolving the problems on this social agenda; and (3) the availability of the data needed to address these issues and the effectiveness of the tools, both analytic and mechanical, required to process and analyze the data (Kuznets 1972, 39).

In explaining both the enormous growth of economic research between 1930 and 1970 and the direction that research took, Kuznets emphasized the importance of the interaction between these three factors, rather than the ascendency of any one over the other. This expansion of economic research undoubtedly depended on the social agenda since it was largely through the government that the training of the scientific personnel, the collection of the primary data, and the financing of individual research projects were directly or indirectly promoted.[3] However, which direction this research took was heavily influenced by developments within the academic community. Thus, while the devastating impact of the Great Depression of the 1930s promoted greater government intervention in the economy, the direction that the intervention took, and the type of research that the government promoted, was greatly affected by Keynesian theory, which had gained such dominance in the scholarly community. In the absence of this influential theory, government policy "might have been limited to new provisions for unemployment insurance, new plans for public works, and the like" (Kuznets 1972, 42). Since the theory indicated that depression conditions could recur unless the government was continuously concerned with ensuring a sufficiently high level of final demand, government policy moved heavily in a Keynesian direc-

3. Foundations and other private institutions also played an important role (Kuznets 1972, 42).

tion. This interaction between social priorities and economic theory gave an enormous stimulus to the development of national income accounts, of measures of employment and unemployment, the size distribution of income, and other macrovariables, as a means of monitoring economic performance and guiding government intervention.

Kuznets emphasized the critical role played by academic research in the innovations in economic measurement adopted by government agencies in the free market economies. It was not primarily from the government bureaucracy but from the scholarly community that new approaches to measuring economic performance arose. It was not until those approaches had been advanced and explored within the scholarly community that the national income and product accounts, input-output analysis, flow-of-funds measures, and periodic sample surveys were adopted by government agencies as standard procedures on which they relied.

An emphasis on the intimate interconnection between measurement and theory was a third, and perhaps the most distinctive, aspect of Kuznets's method. Although Kuznets was a quintessential empiricist and a standard bearer for empirical research, his empiricism did not imply hostility to theory. Quite the contrary, he continually emphasized that a sound theory was needed to identify the variables that had to be measured and that theory had to be invoked in order to determine how the raw data thrown up by normal business or government activities had to be combined in order to create the desired measures. Since measurement was dependent on theory, he emphasized that as theory advanced, owing to either deeper insights or sounder empirical knowledge, past measures would have to be revised. Thus, empirical knowledge and theoretical knowledge are at any point in time only asymptotically valid, subject to changing knowledge in both areas as well as to changing social goals and values (Kuznets 1972, 18–22). In attempting to pursue his empirical objectives, Kuznets frequently encountered theoretical issues that had not yet been addressed adequately. On such occasions, he made notable contributions to theory, as in his work on the theory of national income accounting, in which he extended utility theory to issues involved in designing measures of output that reflected economic welfare.

Kuznets not only used theory but also sought to extend it by identifying empirical regularities that could provide the basis for new theories or by modifying and extending existing ones. In this connection, he made notable contributions to the theory of technological change, the theory of industrialization and other aspects of long-term structural changes in modern economies, the theory of economic cycles, the theory of the size distribution of income, the theory of the interrelation between population change and economic growth, the theory of capital formation (including the role of variations in saving rates over the life cycle), and the theory of the effect of changes in vital statistics on the socioeconomic characteristics of households.

How to Measure in Economics

To many of those who have studied the work of Kuznets, his demonstrations and discussions of the art of measurement are the most valuable aspects of his legacy. By *the art of measurement*, we mean not merely statistical theory and econometric theory, which are important but quite adequately conveyed in papers and books. A far more difficult question in practice is how to apply statistical methods and economic models to the incomplete and biased data with which economists normally work and still produce reliable estimates of key economic variables and parameters. That question cannot be answered by a simple rule because economic data are so variable in quality and because the circumstances under which a given set of defects in the data are tolerable depends on the issues that are being addressed, on the statistical and analytic procedures that are being employed, and on the sensitivity of the results to systematic errors in the data, to the choice of behavioral models, and to the choice of statistical procedures.

Good judgment on these issues is developed with experience, and, both in his writing and in his class lectures, Kuznets tried to convey his rich experience on these matters in the same way that doctors use rounds to teach medical students the art of diagnosing illnesses. Kuznets conducted his "rounds" with his students at Pennsylvania, Johns Hopkins, and Harvard in three different ways: first, in his lec-

tures on economic growth, where he discussed problems of measurement and gave numerous examples of good and bad attempts to measure key economic variables and relations; second, in his seminar on the application of quantitative methods to the analysis of time series, which was largely a laboratory course in which students applied various procedures to typical bodies of economic data and collectively discussed the problems and interpreted the outcomes; third, in his supervision of dissertations, during which he varied his approach according to the degree of independence desired by the student while always serving as a sympathetic, thorough, and penetrating critic.

Kuznets held that, while the statistical analysis of quantitative data was a powerful instrument in the study of long-term changes in the economies of nations, it provided no magical solutions. Quite the contrary, it was filled with pitfalls that had entrapped some of the most able investigators (virtually no one was immune), and, even when the data were good, the procedures appropriate, and the results fairly unambiguous, great care had to be exercised in drawing conclusions about the domain to which the findings applied and the predictions that could reliably be based on them. High on his list of major dangers was the superficial acceptance of primary data without an adequate understanding of the circumstances under which the data were produced. Adequate understanding involved detailed historical knowledge of the changing institutions, conventions, and practices that affected the production of the primary data but were difficult to ascertain and quantify.

Another point high on Kuznets's list of major dangers was the easy assumption that the good fit of a mathematical model to the data made that model an adequate description of the significant features of the data. Because of the limitations of data, especially in time series, many mathematical models, varying in complexity and structure, may give fairly good fits to a given body of data. Nor can Occam's razor (the theory that holds that simpler explanations are preferable to complex ones) be glibly invoked to settle such issues since it is possible that the curve giving the best fit incorrectly leads to the conclusion that the data were generated by a simple process, an elegant "law" of behavior embodied in a single equation, when in fact they were

generated by several distinct processes that are badly distorted by the simple function.

Kuznets's comments on methods were always deeply embedded in a more general evaluation of the substantive findings of a particular investigation. Thus, whether a given body of data was good or bad depended not only on the inherent limitations of the data set but also on the types of measures that were being constructed from it and the issues to which these measures were addressed. Consequently, his evaluation of the validity of substantive findings tended less to be cast as simply right or wrong, although this was sometimes the judgment, and more often focused on the reliability of the results (usually expressed as the probable range of error in the estimates).

Although he placed great emphasis on the development of databases of the highest quality (i.e., those least afflicted by sample selection biases, by definitional changes that led to lumping data that are intrinsically different in some important dimension into the same category, etc.), Kuznets was not a purist who insisted on working only with "perfect" data. Since no data set is ever perfect, his emphasis was on how to exploit the data at hand in order to extract from them whatever useful information they might contain. But then the effect of the limitations of the data on the resulting analysis had to be specified, with some results treated as conjectural and still others merely as illustrative computations. Providing that they were carried out with due caution regarding the nature of the results, such preliminary analyses were useful because they increased the likelihood of upgrading the available data sets or closing gaps in them by demonstrating the social usefulness of such efforts. Indeed, Kuznets viewed the preliminary analysis of the available data as an essential part of an asymptotic process of discovery, during which both the underlying data sets and the analytic procedures were perfected and made more suitable to the resolution of the substantive issues.

Like many other statisticians, Kuznets worried about imposing so much structure on the data that the a priori assumptions of the investigation overwhelmed whatever information there was in the data. He was skeptical about fitting simple (two- or three-parameter) curves to data sets with relatively few observations of questionable quality.

Consequently, he tended to work with frequency distributions, usually in either one-way or two-way classifications, rather than with regressions.

Kuznets had numerous horror stories of how very able investigators had been misled by relying too heavily on a priori assumptions of what the world was really like and on arguments by analogy as well as by misplaced confidence in formal measures of goodness of fit. A case in point is his discussion of Raymond Pearl's contention that a simple logistic curve summarized tendencies so stable in human populations that it represented a law of population growth. Pearl, a noted biologist and statistician and the author of *The Biology of Population Growth* (1925), conducted experiments with fruit flies raised in closed containers that show that, with increasing density and a fixed food supply, the growth of the population was well described by a logistic curve (an S-shaped curve that starts off gradually, then rises steeply, then evens out). Using Malthusian types of arguments, he contended that the analogy applied to man because space is also limited on earth. He then proceeded to fit logistic curves to data for various populations and, with one or two exceptions that he explained as special cases, obtained apparently good fits. Pearl also showed that one of the conditions for a logistic curve to be applicable, a decline in birth rates as population density increases, was supported by cross-sectional regressions on U.S. cities between birth rates and two density measures, after controlling for city size and per capita wealth or income. One implication of Pearl's findings was that population growth moved in long cycles, with population increasing until it came close to its asymptote. It hovered at this asymptote until some exogenous factor caused the asymptote to shift.

Kuznets carefully discussed both the a priori and the statistical aspects of the argument, pointing out that, although Pearl gathered the data to test his theories from a fairly exhaustive list of those nations for which such data were available, represented on that list were mainly Western nations at relatively high levels of economic development. The observations were primarily for the period from the early or mid-nineteenth century to 1920, and, since they were usually decennial estimates, there were generally about twelve or fewer observations per

country; consequently, good fits in the sense of a high R^2 did not mean that the results were significant. Even if the fits were statistically significant, however, they did not necessarily justify the conclusion that the underlying process was well described by a logistic curve or provide the basis for a law invariant regardless of social and cultural conditions. Since the logistic curve has three segments (convex from above, linear, and concave from above), it would give a good fit to data sets that were strictly linear as well as to those that were strictly increasing at a decreasing rate or strictly increasing at an increasing rate. Examination of the underlying data revealed such segmentation to be pretty much the case.

Kuznets's manner of discussing these examples was nearly as important as the substance of his points. There was no attempt to demean Pearl or to puff up his own image. His aim was to demonstrate both the possibilities and the limitations of quantitative methods in the social sciences. Valuable as they were, such methods did not provide easy, let alone automatic, solutions to otherwise difficult problems. No matter how high-powered the technique, the results it yielded had to be carefully evaluated not only by looking at such internal evidence as the scatter of observations around the fitted curve but also by thoroughly considering such relevant external evidence as the nature of the societies that yielded the data and the conventions followed by the agencies that gathered, processed, and published them.

The results, Kuznets emphasized time and again, had meaning only if the investigator defined and studied the universe from which the data were drawn, and that required a substantial effort to discover and understand the relevant social institutions of the societies under study as well as how they were changing over time. Required to be a good quantitative economist, then, were not only logical and technical cleverness but also a substantial knowledge of recent and more distant history. Although Kuznets admired cleverness and technical proficiency, he considered the capacity to be thorough and to pursue details rigorously as a rarer quality and as a more binding constraint on good work.

In assessing the reliability of particular estimates, Kuznets emphasized the importance of systematically investigating their relation to

other series and other kinds of information that were logically related to them. He was, in this connection, a master of devising algebraic identities that brought other available data to bear on the estimates at issue in a particularly illuminating way. These identities were also marvelous devices for revealing implicit and unsupported assumptions and thus contributed to the social research agenda. A dazzling example of this skill is contained in his evaluation of the time series on U.S. national income and its sectoral distribution generated by Robert F. Martin for the period 1799–1869 (Kuznets 1952a, 1952b). What puzzled Kuznets about these widely cited figures was that they implied a decline of about 8 percent in per capita income over the forty years between 1799 and 1839, years that witnessed vigorous growth in population, a vast geographic expansion, and the introduction and initial diffusion of the steamboat, the railroad, and the factory system.

To evaluate Martin's series in the light of the available data, Kuznets employed an identity[4] that related per capita income to wages in agriculture and in the rest of the economy and to the labor force participation rate. Marshaling the available fragments of data, he surmised that, even if there had been no increase in wage rates over the period 1799–1839, the rise in nonagricultural labor relative to agricultural labor, together with the rise in the labor force participation rate, should jointly have led to about a 19 percent increase in per capita income since, as indicated by Martin's data, the ratio of nonagricultural to agricultural wages was equal to about 5. He then went on to marshal fragmentary data suggesting that both agricultural and nonagricultural wages had probably risen, contrary to the implication of Martin's series, so that even Kuznets's own exercise probably underestimated the total growth of per capita income during the period 1799–1839.

This exercise touched off a major stream of research involving numerous investigators that have greatly illuminated the course of U.S.

4. The algebraic statement of this identity is

$$\bar{Y} = \rho(\lambda_a W_a + \lambda_n W_n),$$

where $\bar{Y}$ = per capita income, ρ = the labor force participation rate, λ_a = the share of the labor force in agriculture, λ_n = the share of the labor force in nonagriculture, W_a = output per worker in agriculture, and W_n = output per worker in nonagriculture.

economic growth prior to 1840. It was characteristic of Kuznets that he considered the mathematics underlying his computations so obvious that he never made the underlying equation explicit. Although this and other Kuznetsian identities were often used by his students in teaching, the simple equation (or a variant of it) was not put into print until the publication of Paul David's influential paper in 1967 (Engerman and Gallman 1983; David 1967; see also Fogel, Galantine, and Manning 1992; and Fogel and Engerman 1992a, 1992b), more than a decade after Kuznets's original discussion of it. Subsequently, a variety of Kuznetsian and Kuznets-like identities have been set forth as differential equations and effectively exploited.

Did the numerous biases that afflicted the data sets with which economists had to work, the pitfalls of curve fitting, and the sensitivity of results to the presumed underlying behavioral models as well as to the choice of statistical procedures doom the usefulness of quantitative methods in the study of economic growth? By no means. Kuznets was neither an optimist nor a pessimist on this question but a realist and an architect of procedures needed to make the most of defective data and imperfect tools. Even in the most difficult of circumstances, he pointed out, such as those that confronted Pearl in his attempt to demonstrate that the logistic curve represented the law of human population growth, there was important information to be gleaned. What Pearl had indirectly demonstrated was that all the advanced nations for which data were available had experienced declines in their percentage rates of natural increase between 1850 and 1920. That finding was robust no matter what segment of the logistic curve Pearl had fitted to his data since it is a characteristic of the logistic function that the percentage rate of increase is always declining. This was no mean finding. It was one of the early demonstrations of what subsequent research confirmed as a major demographic feature of modern economic growth. Hidden among the oysters was a genuine pearl.

The last point calls attention to what we believe was the most powerful lesson that Kuznets taught about the art of measurement in economics: sensitivity analysis. It was sensitivity analysis, not clever a priori arguments, that separated robust findings from conjectures. Anyone good enough to get a Ph.D. after the mid-1950s could mar-

shal an a priori case for why one procedure should be preferred over another or why some bias in the data could be ignored. It was much harder to demonstrate that a finding based on such a priori arguments should be taken seriously since it was equally easy to construct a priori arguments proving that the designated procedure badly biased the result or that the imperfections in the data were fatal. Kuznets's solution to such problems was *sensitivity analysis*, by which he meant a careful examination of both the procedures and the data in order to see whether plausible ranges of the systematic errors in the data or the substitution of reasonable alternative estimation procedures would make a material difference in the finding. If they did not, the finding was robust; otherwise, the data added nothing to the theoretical considerations that preceded the measurement. The original conjecture was still just a conjecture.

Kuznets as a Theorist

Kuznets is one of the most important theorists since Keynes. Some measure of his impact on theory in one of the major areas of his research, the interrelation between population change and economic growth, is provided by the author index of *The Determinants and Consequences of Population Trends* (United Nations 1973–78). Prepared by a UN commission, the study summarizes and interprets the worldwide literature in this field from the earliest times to the 1970s. Among the individuals frequently cited in the author index are such innovators in demography as Ansley J. Coale, Richard A. Easterlin, Thomas R. Malthus, and the Nobel laureates W. A. Lewis and Gunnar Myrdal. The citations of Kuznets, however, exceed those of any of these specialists, usually by large margins. They even exceed the citations of such collective authors as the Food and Agriculture Organization of the United Nations, the International Labour Organization, the OECD, and the World Health Organization. Indeed, only the combined agencies of the United Nations have more citations than Kuznets.

Since the interrelation between population growth and economic growth is only one of the major themes on which Kuznets theorized,

it is possible to present only some brief comments about his approach to theory. In this connection, it is useful to begin with a distinction that he often made between a partial and a general theory of economic growth. By a *partial theory*, he meant the in-depth consideration of a few variables torn from the context of the general process of economic growth. In this connection, he welcomed the explosion of mathematical growth models that began in the late 1940s and the 1950s as a return to issues that had been so important to Smith, Malthus, and Schumpeter, thus finally overcoming the long neglect of growth theory. Yet he feared that, because of the severe aesthetic constraints placed on the issues and on the interrelations of variables by the type of mathematic modeling that was fashionable, this stream of research might rapidly dissipate without making a lasting contribution to what he considered the principal objective of theoretical work in this field: the development of a tested and confirmed general theory of growth that included a theory of technological change, of population growth, of changes in the economic structure of production, of changes in political and social organization, and of the role of international political relations. A general theory needed not only to encompass each of these major elements but also to describe the feedback mechanisms that linked them together in a dynamic context.

Kuznets recognized that such a general theory was a tall order that would probably not be accomplished in his lifetime. He not only welcomed partial models as contributions toward that goal, as long as they contributed to the ultimate object of a general theory; he himself contributed numerous partial models. His presidential address to the American Economic Association, in which he considered the impact of economic growth on the inequality of the income distribution (Kuznets 1955), exemplifies his approach to such partial theories. It was in this paper that he set forth the hypothesis that, in early stages of economic growth (i.e., at low levels of per capita income), growth tended to increase the inequality of the income distribution but that, at later stages (high levels of per capita income), it reduced inequality. That hypothesis, which has come to be known in the literature as the *inverted-U hypothesis* (or the *Kuznets curve*), set off a large train of both theoretical and empirical research aimed at elaborating it and

testing it empirically. It has been put to practical use by the World Bank, which transformed the hypothesis into an econometric model suitable for estimating the share of the world population living in poverty (Anand and Kanbur 1984, 1987, 1993; cf. Fei, Ranis, and Kuo 1978).

It is interesting to note that Kuznets's 1955 paper has been treated not only as important theoretically but also as providing empirical support for the inverted-U hypothesis (Fields 1980, 78, 84). This is a strange development since Kuznets was at pains to stress its theoretical nature, repeatedly warning that his allusions to fragmentary data were not evidence but little more than pure guesswork. Most of the paper is devoted to explicating the conflicting factors that arose during the course of growth and created pressures both to increase and to reduce inequality. It also describes processes that influenced the relative strength of the conflicting factors at different stages in the growth process.

It would have been easy for Kuznets to set forth his model in mathematical form (since the computations he presented to illustrate the process implied a set of equations), but he chose to make the same points with numerical examples. Numerical examples had two advantages over a mathematical presentation. They emphasized the limited range of the changes in the key variables and parameters needed to bring about the postulated curve, and they made his argument accessible to a wider range of readers. Since there was nothing in the model that required a long chain of reasoning to reveal some deeply buried implication, there was no reason to unnecessarily restrict his audience.

This example reveals something important both about Kuznets's approach to theory and about certain problems in the profession. Because Kuznets developed a theory consistent with the available fragmentary evidence, because he used numbers rather than algebra to set forth the theory, his paper was widely interpreted as an empirical paper despite his repeated warnings about the fragility of the data suggesting the theory. He also stressed that, even if the data turned out to be valid, they pertained to an extremely limited period of time and to exceptional historical experiences and that caution had therefore to be exercised in the conclusions drawn from his theory.

Nevertheless, his caveats were jettisoned and his hypothesis raised to the level of law, becoming the basis for numerous formal models and elaborate econometric exercises, some of which lost touch with the complex reality that he was trying to uncover and characterize.

The example calls attention to a shortcoming of current theory: the tendency to value a theory according to the type of the mathematics it employs. On this criterion, the best theory employs the most general mathematics, as free as possible from empirical or quasiempirical limitations, as the specification of the form of functions. But that criterion is purely aesthetic—equivalent to constraints that the sonnet form imposes on a poet. Aside from aesthetic considerations, such severe limitations are generally unnecessary in economics because the range of most economic variables is fairly constrained. Making use of that knowledge frequently makes it possible to solve models that cannot be solved in a purely analytic (abstract) framework. Ansley Coale, an elegant analyst, has frequently made use of the limited ranges of variation in demographic behavior to close demographic models with empirical relations and thereby manipulate models that would otherwise remain intractable. It is this flexibility in demographic modeling that in no small measure accounts for the vastly improved quality of empirical research in this field, in the face of data problems as severe as any encountered in economics proper. Kuznets was more interested in theories that proposed to describe and generalize about some aspects of the observable behavior of the economy than in those that sought the simplest set of a priori assumptions, and the weakest specification of functional relations, that could produce a particular generalization. Among the theories that he found most fruitful, but not necessarily correct, were Malthus's statements on the relation between population and economic growth, Joseph Schumpeter's theory of the business cycle, Walter Hoffmann's theory of the sequencing of industrialization, Alvin Hansen's theory of the effect of population growth on savings rates, theories about the behavior of savings over the life cycle, theories of human capital formation, theories about the factors affecting the size distribution of income, and neoclassical models of economic growth (particularly as developed by Robert Solow, Edward Denison, Zvi Griliches, and Dale Jorgenson since they implied

accounting identities that, when flexibly approached, were useful in arraying data bearing on the growth process).

Kuznets appreciated the advantages of formalizing such generalizations and of demonstrating how they could be deduced from a limited set of a priori assumptions. Such work had shown that downward-sloping demand curves, perhaps the single most important analytic and empirical tool of economics, did not require the dubious, convoluted assumptions about consumer psychology of earlier theorists but could be generated from a few simple assumptions about preference orderings. The mathematical development of the theory of consumer demand also called attention to the important distinction between income and substitution effects and had a large impact on the development of statistical procedures for the estimation of demand functions.

Yet, without in any way belittling these achievements, Kuznets feared that such formalization of theory was becoming increasingly sterile, partly as the result of an overinvestment in it. Too many papers merely explored the consequences of changing one or another assumption in a given hypothetico-deductive model. Though they pointed up the sensitivity of such models to their assumptions, they rarely served as guides to study of the real economic world. Nevertheless, these intellectual exercises acquired a vogue, and those engaged in this work developed a set of standards for judging quality that had little to do with the ultimate bearing of the models on empirical research. To avoid sterility, hypothetico-deductive modeling had to be intimately connected with, and regularly infused by, findings from empirical, experimental, and clinical research, as they normally were in the natural sciences.