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ABSTRACT

This paper discusses the scientific methods that guided the economic research of Simon Kuznets, with particular stress on his approach to measurement and theory. The paper closes with the transcription of a brief autobiographical talk by Kuznets at a dinner in honor of his eightieth birthday.

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SOME NOTES ON THE SCIENTIFIC METHODS OF SIMON KUZNETS¹

"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 and me at Harvard University in the early 1970s. I was startled when he said it, since our profession thrives 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.

Four 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 other aspects, it was his concern with the great policy issues of his age. My emphasis on this point may surprise those who are familiar with Kuznets's work, since he never became directly involved in those highly politicized disputes over economic policy that often split the profession into partisan camps. Moreover, many of the problems on which he worked, such as the relationship between the rate of population growth and 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, Kuznets 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 upon

these issues was an urgent task of economists. He saw economics as an empirical science aimed at disclosing the factors which 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 irreversable 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 Hobson, Hilferding and Lenin, became widely accepted by professional economists during the 1930s, and Hansen's suggestion that a correct fiscal policy could bring an end to secular stagnation, despite a certain optimism, seemed to endorse the view that secular stagnation was the natural condition of free market economies in the twentieth century (Abramovitz 1952).

Kuznets broke new ground in several respects when he set out to describe the phenomenon he called modern economic growth. Such growth was not a lucky accident, the outcome of a fortunate but ephemeral conjunction of circumstances. It was, rather, the central feature of a new historical epoch marked by the application of science to industry and possessing other characteristics that gave it unity and set it apart from the epochs that preceded it (1966, p. 2). Among the primary features of modern economic growth were sustained rises in output per capita or per worker accompanied by increases in population and by sweeping changes in the structure of the

economy. When Kuznets first began his work on economic growth in the mid 1920s, not all the processes that he later identified had worked themselves out. Europe and America were still passing through the demographic and epidemiological transitions (U.S. life expectancy at birth in 1920 was still under 55 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 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.

To Kuznets, accurate description of the characteristics of modern economic growth and of the factors that tended to promote or retard growth were necessary not only for the continued prosperity of the developed nations but also to formulate policies that would close the enormous gap in per capita income that had arisen between the developed and the less developed nations. Much of his work was directed toward measuring and explaining differing patterns in the inequality of the distribution of income, across and within nations, over time. He believed that at low levels of per capita income, economic growth tended to increase inequality of the distribution of income, but at intermediate and higher levels, growth reduced inequality. On this question, as on so many others, Kuznets sought to distinguish factors affecting the income distribution that were more or less inescapable consequences of the dynamics of population or income growth from those that were amenable to current policy.

The last point touches on a second aspect of Kuznets's approach: his concern with the role of long-term factors in the determination of current economic performance. In his view many current economic opportunities and problems were determined by economic conditions and relationships that evolved slowly, often taking many decades to work out. At a time when Keynes declared that "In the long run we are all dead," an aphorism reiterated by many economists not only during the 1930s but during the 1940s and 1950s, Kuznets continued to call attention to the role of long-term factors that had to be taken into account by policy makers, factors which led him to conclude that the opportunities for returning to high employment levels and rapid economic growth were greater than generally believed.

Current social problems, Kuznets emphasized, are often the result of past growth -- the consequence of past desirable attainments, which at a later time produce socially undesirable consequences that require remedial policy action. Of his numerous illustrations of this principle, one is particularly cogent: the explosion of population growth in the less developed nations of Asia, Africa, Oceania, and Latin America in the quarter century following World War II. This population explosion threatened to thwart efforts to raise per capita incomes from their dismally low levels because birth rates remained traditionally high, while public health policies and improved nutrition cut death rates in these regions by more than 50 percent in less than a generation. One obvious solution to the problem was to reduce fertility, yet there was a web of traditional patterns of behavior and beliefs that tended to keep fertility high. Nevertheless, Kuznets believed that properly designed public policies could hasten the social and ideological changes required to reduce fertility and to lead

these societies to prefer a greater investment in a fewer number of children. Such a program required not only government and private campaigns to disseminate the technology of birth control but a restructuring of social and economic incentives that would provide rewards for families with fewer children.

Kuznets pointed out that this urgently needed program to reduce fertility would have its negative as well as its positive side. Since it was those in upper income brackets who would respond most rapidly to the new incentives, the immediate impact of a campaign to reduce birth rates would be to increase the inequality of the income distribution. This initial impact could be overcome by a determined effort to change the social and economic conditions of the lower classes in a way that would promote their interest in smaller families. Yet as the experience of the U.S. and other developed nations has shown, the success of the program to curtail fertility is bound, much further down the line, to create a new set of problems, similar to those which are currently at the center of the modern women's movement: the restructuring of society in such a way as to promote equal opportunity for women in all occupational markets.

Economic growth creates social problems because it is profoundly disruptive to traditional values and religious beliefs, to longstanding social and family patterns of organization, and to numerous monopolies of privilege. Despite the fact that modern economic growth has brought with it tremendous increases in longevity and good health, has brought to the lower classes standards of living as well as social and economic opportunities previously available only to a tiny minority, and has greatly reduced the inequality in the income distribution of developed nations, the social restructuring of society required by modern economic growth has been

fiercely resisted -- sometimes because of an unwillingness to give up traditional values and ways of life, sometimes by entrenched classes determined to protect their ancient privileges. Because of the complex responses to change and because the epoch of modern economic growth was still unfolding, many aspects of the social restructuring that was under way were still obscure and difficult to predict (Kuznets 1966, p. 15). As late as 1972 Kuznets felt compelled to point out that despite the multitude of tentative partial generalizations, cross-sectional studies, and econometric exercises, there was as yet no "tested generalization, significantly specific to permit the quantitative prediction of aggregate growth, or even of changes in the structural parameters in the course of growth" (1972, p. 58).

The difficulty of predicting the future relates to two methodological problems with which Kuznets continually struggled: How long a period of observation is needed to identify the underlying process at work in any specific aspect of economic growth? How can one determine whether such a process, once identified, is sufficiently stable to provide a reliable basis for prediction? These problems are illustrated by an issue on which Kuznets was the preeminent investigator of his age, the interrelationship between demographic processes and modern economic growth.

Kuznets considered the acceleration of population growth during the nineteenth century not only as 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 rate 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 (1966, pp. 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, Kuznets did not anticipate the explosive entry of women into the labor force during the next quarter century, nor the new ideology that would facilitate that development (1966, pp. 193-195).

A third 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 policy makers inside and outside of 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 analytical and mechanical, required to process and analyze the data (1972, p. 39).

In explaining both the enormous growth of economic research between 1930 and 1970, and the direction that it took, Kuznets emphasized the importance of the interaction between these three factors, rather than the ascendancy 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.² 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" (1972, p. 42). Since the theory indicated that the depression could recur unless the government was continuously concerned with insuring a sufficiently high level of final demand, government policy moved heavily in a Keynesian direction. 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, of the size distribution of income, and of other macro variables as a means of monitoring economic performance and of guiding government intervention.

Kuznets emphasized the critical role played by academic research on 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 they 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.

The increased importance placed on economic measurement was also promoted by the enormous strides made since World War II in methods of collecting and summarizing primary data, as well as in analyzing them. In this connection Kuznets emphasized not only the enormous advances in computer technology and in methods of statistical inference, but also the advances in the mathematical modelling of both simple and complex socioeconomic behavior. Just as he viewed Keynesian theory as a great stimulus to quantitative economic research, he viewed the post-war explosion of economic models as having the potential to promote more well-defined empirical research and eventually to increase the body of empirically tested and confirmed economic theory (1972, pp. 54-58).

This emphasis on the intimate interconnection between measurement and theory was the fourth, 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 theory had to be invoked in order to determine how the raw data thrown up by normal business or governmental activities had to be combined in order to create the desired measures. Since measurement was dependent on theory,

he emphasized that as theory advanced, due either to deeper insights or sounder empirical knowledge, past measures would have to be revised. Thus empirical 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 (1972, pp. 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 did not pit deductive theory against inductive theory but made use of both approaches in his work. Nor did he object to simplifying assumptions that, although a gloss on reality, facilitated an analysis without distorting it. Kuznets was, however, impatient with theorists who knew so little about the institutions or processes about which they theorized that they could not distinguish between metaphors and reality and so failed to consider the logical implications of assumptions that violently distorted the real world. One of Kuznets's repeated contributions was the demonstration that certain so-called pure theories embodied false assumptions about empirical matters, assumptions that critically affected the conclusions derived from the theories. In so doing he helped to counter the view that in theoretical work, cleverness and elegance were all that mattered. Clever economic theories that did not ultimately contribute to the bottom line, curing or keeping the economy healthy and promoting its growth, were no more useful than biomedical theories that did not ultimately contribute to fighting disease or otherwise improving people's health and longevity.

Kuznets not only used theory but sought to extend it by identifying empirical regularities that could provide the basis for new theories or to modify and extend 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 interrelationship 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 changes in vital rates on the socioeconomic characteristics of households.

I am acutely aware that the preceding comments are at best a gloss on the methods that underpinned the work of a scholar as prolific and as broad ranging in issues, and as flexible in methods, as Kuznets was. Others might have emphasized much different aspects of his approach than those that I have singled out. And enough can be written on each of these points to fill a book. Since the editor, although generous in his charge, did not give me that liberty, I have limited my desire to elaborate on Kuznets's methods to two points: his approach to what one might call the art of measurement in economics and his contribution to economic theory.

How to Measure in Economics

To many of those who studied under Kuznets, his demonstrations and discussions of the art of measurement were the most valuable aspect of their training. By the art of measurement I mean not merely statistical 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 analytical 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 judgement on these issues is developed with experience, and 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" in three different ways: first, in his lectures on economic growth where he discussed problems of measurement and gave numerous examples of good and bad attempts to measure key economic variables and relationships; 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 Kuznets varied his approach to the degree of independence desired by the student, while always serving as a sympathetic, thorough, and penetrating critic.³

At the time I took it, Kuznets's course on economic growth covered four main topics: population, technological change, long-term trends in national product and its components, and cross-sectional analysis of

differences in per capita income, industrial structure and the political and social characteristics of developed and less developed nations. On each of these topics, Kuznets defined the issues to be studied, the types of evidence available to study them, the methodological problems of obtaining from the available data the kinds of information required to resolve the issues, and the results obtained from applying different analytical and statistical procedures to different types of data (including qualitative and anecdotal information). He also interpreted the findings to date, carefully evaluating the conclusions that they could support, pointing up crucial gaps in information revealed by the studies (often suggesting how they might be closed), and carefully evaluating conflicting findings in order to determine whether the conflicts were merely the consequences of poorly conceived or poorly executed studies, or raised new issues that constituted an agenda for the next round of research.

One of the first methodological points that emerged from the course was 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 that 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 upon 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 which

affected the production of the primary data but which were difficult to ascertain and to quantify.

Throughout his lectures it was apparent that Kuznets practiced what he preached about the need to know history. He was well versed in the history of economics as a discipline, in the history of censuses and other data sources (not only in the United States and Europe but also in many less developed nations), in the history of science and technology, and in the general economic history of numerous countries. One might think that with such wide reading his grasp of any one of these topics was bound to be superficial. Yet the depth of his knowledge on each of these questions was strikingly evident.

When Kuznets dealt with the development of the Watt steam engine, for example, he not only carefully identified each of its new components but he went into considerable detail about the host of problems that Watt had to overcome. Numerous events preceding the basic design and many that came afterward were set forth, including Watts education as a mechanic, his exclusion from the guild, the opportunities opened to him when he was hired as the instrument maker for the laboratory at the University of Glasgow, the way in which his scientific cast of mind developed, his difficulty in finding machinists who could mill parts to the tolerances required by his design, the difficulties of financing both the long developmental process and the sales of expensive equipment, the advantages of his partnership with Matthew Boulton, and the persistent search for improvements in the original design, especially for adaptations that transformed the engine from a steam-powered pump into a general power source capable of driving all kinds of machinery. Kuznets did not assume that the search for generalizations about the process of invention and diffusion made details about the personalities,

beliefs, and circumstances of inventors and entrepreneurs irrelevant. Quite the contrary, he believed that the mastery of these details was a precondition for valid generalizations.

Another point high on Kuznets's list of major dangers was the easy assumption that a good fit of a mathematical model to the data made it 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 be glibly invoked to settle such issues, since it is possible that the curve which gives 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 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 judgement, but more often focused on reliability of the results (usually expressed as the probable range of error in the estimates -- not just t-values, but a more fundamental assessment which included judgements of the probable influence of systematic errors in the underlying data as well as errors introduced by the selection of the

behavioral models and statistical techniques), and on their domain of applicability.

Although he placed great emphasis on the development of data bases of the highest quality (i.e. those least afflicted by sample selection biases, by definitional changes which 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 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, he 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 analytical 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 did not object to regressions per se (his students frequently used them with his blessings), but only to statistical procedures that were inappropriate (especially too restrictive) for the issues under study and that presumed too much about imperfect data. He considered it misleading to attach too much importance to R^2 and t statistics, when the systematic biases in the data overwhelmed sampling variability.

He 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. The case that impressed me most was his discussion early in the course of Raymond Pearl's contention that a simple logistic curve summarized tendencies so stable in human populations that it represented a law of the population growth.⁴ Pearl's theory was suggested by experiments with fruit flies raised in closed containers which show that with increasing density and a fixed food supply, the growth of the population was well described by a logistic curve. 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 statistical aspects of the argument, but I focus here on some of his statistical points. Although the data that Pearl gathered to test his theories came from a fairly exhaustive list of the nations for which such data were available at the time, they 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 12 or less observations per country; consequently, "good fits" in the sense of a high R^2 did not mean they 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 to social and cultural conditions. Since the logistic curve has three segments (convex from above, linear, and concave from above), it would give 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 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 a thorough consideration of such relevant external evidence as the nature of the societies that yielded the data, and of the conventions followed by the agencies that gathered, processed and published them.

The results, he 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. To be a good quantitative economist, then, required not only logical and technical cleverness, but also a substantial knowledge of recent and more distant history. Although he admired cleverness and technical proficiency, I believe that 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 relationship 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. They 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 through 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 40 years between 1799 and 1839, which 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 the following identity:

$$(1) \quad \bar{Y} = \rho(\lambda_a W_a + \lambda_n W_n)$$

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

W_n = output per worker in nonagriculture

Marshalling the available fragments of data, Kuznets surmised that even if there had been no increase in W_a or W_n over the period 1799 to 1839, the rise in ρ and the rise in λ_n relative to λ_a should jointly have led to about a 19 percent increase in per capita income since W_n/W_a , as indicated by Martin's data, was equal to about 5. He then went on to marshal fragmentary data suggesting that W_a and W_n had both probably risen, contrary to the implication of Martin's series, so that even Kuznets's exercise probably underestimated the total growth of per capita income during 1799-1839.

Kuznets's exercise on Martin's data touched off a major stream of research involving numerous investigators that have greatly illuminated the course of U.S. economic growth prior to 1840 (Engerman and Gallman 1983). It was characteristic of Kuznets that he considered the mathematics underlying his computations so obvious that he never made equation (1) 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 David's influential paper in 1967, 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. In the most difficult of circumstances, Kuznets pointed out, such as those which 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 of the advanced nations on 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 I 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 marshal 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 data in order to see if 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.

I learned about sensitivity analysis during the course of my dissertation. In order to estimate the social savings of railroads in the interregional distribution of grain and meat, it was necessary to know the total amount of each commodity shipped during 1890 from each primary market and the total receipts at each secondary market. The outshipments from the 10 midwestern primary markets could be obtained directly from the annual

reports of the boards of trade in each of these cities, but the reports did not list either the specific destinations or the specific quantities assigned to each destination. It occurred to me that I could fill the gap by estimating the required consumption of each commodity in each secondary market and then subtract out local production (using the disappearance procedures of the U.S.D.A. to convert stocks into net flows available for human consumption) to obtain the import requirements as a residual. Although feasible, it was a laborious task which required information on the boundaries of over a hundred secondary markets; budget studies by regions with considerable detail on consumption by age, sex, and occupation; information on live weights of animals as well as coefficients needed to convert live weights into dressed equivalents, and a host of other details. After many weeks of searching in libraries at Johns Hopkins and the Library of Congress, and of lengthy calculations on old-fashioned mechanical computers, I finally produced a set of estimates, one that I was quite prepared to defend on conceptual grounds. So I proudly presented them to Kuznets. He looked my tables over carefully and said: "Very interesting, Mr. Fogel. What kind of figures do you obtain when you estimate the requirements of secondary markets by another procedure?" "What other procedure," I asked. "Think about it for a while, Mr. Fogel, and I am sure that something will occur to you. Then let me see the results." With some hard thinking and further searching in the sources, I discovered an alternate way of estimating requirements in two of the major secondary markets. The results in these markets were close enough to the original estimates to satisfy Kuznets. And that, as I have often told my students, is how I learned about sensitivity analysis.

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 interrelationship between population change and economic growth, is provided by the author index of The Determinants and Consequences of Population Trends: New Summary of Findings on Interaction of Demographic, Economic and Social Factors. Prepared by a United Nations 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 Donald J. Bogue, Colin Clark, Ansley J. Coale, Richard A. Easterlin, Phillip M. Houser, Edgar M. Hoover, Charles P. Kindleberger, W.A. Lewis, Thomas R. Malthus, H. Myint, Gunnar Myrdal, Frank W. Notestein, Alfred Sauvey, Joseph J. Spengler, Dorothy S. Thomas, Irene B. Tauber, and B.T. Urlanis. The citations to Kuznets, however, exceed those to any of these specialists, usually by large margins. They even exceed the citations to such collective authors as F.A.O., I.L.O., O.E.C.D., and W.H.O. Indeed, only the combined agencies of the United Nations have more citations than Kuznets.

Since the interrelationship between population and economic growth is only one of the major themes on which Kuznets theorized, it is obvious that I cannot comment in detail on the substance of his numerous contributions. So I want only to present some brief comments about his approach to theory. In this connection it is useful to begin with a distinction that Kuznets often made between a partial and a general theory of economic growth. By a partial theory Kuznets 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 Kuznets 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 not only needed to encompass each of these major elements but to describe the feedback mechanisms that linked them together in a dynamic context.

Kuznets recognized that such a 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, but 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 (1955), exemplifies his approach to such partial theories. It was in this paper that Kuznets set forth the hypotheses 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), growth reduced inequality. That hypothesis, which has come to be known in

the literature as the "inverted-U hypothesis," set off a large train of both theoretical and empirical research aimed at elaborating the hypothesis and at testing it empirically. The hypothesis 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 1984a, 1984b, 1987; cf. Fei, Ranis, and Kuo 1978).

It is interesting to note that Kuznets's 1955 paper has not only been treated as an important theoretical paper but also as providing empirical support for the inverted-U hypothesis (Fields 1980, pp. 78, 84). This is a rather 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 that created pressures both to increase and to reduce inequality. The paper 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 a mathematical form (since the computations he presented to illustrate the process implied a set of equations), but Kuznets 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. Numerical examples also made his argument accessible to a wider range of readers. Since there was nothing in the model which 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 Kuznets's repeated warnings about the fragility of the data that suggested 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, so that caution had to be exercised in the conclusions that were drawn from his theory. Nevertheless, Kuznets's caveats were jettisoned and his hypothesis was 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 Kuznets was trying to uncover and to characterize.

The example calls attention to a shortcoming of current theory. That is 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 such empirical or quasi-empirical limitations, as the specification of the form of functions. But that criterion is purely aesthetic -- equivalent to constraints that a sonnet 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 analytical (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 relationships, 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 on some aspects of the observable behavior of the economy than those that sought the simplest set of a priori assumptions, and the weakest specification of functional relationships, that could produce a particular generalization. Among the theories that he found most fruitful, but not necessarily correct, were Malthus's statements on the relationship between population and economic growth, Schumpeter's theory of the business cycle, Hoffmann's theory of the sequencing of industrialization, 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 Solow, Denison, Griliches, and 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 analytical 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 consequence 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.

Kuznets was impatient with economists who became infatuated with elegance, and forgot that the aim of theory was to promote the search for tested knowledge about economic behavior. There was a limit to how far theory in economics could become separated from the product which the patrons of economics -- the policy makers -- demand of the discipline. Although, as I tried to indicate in the first section of this paper, he placed great emphasis on the importance of specialized research institutions and university graduate programs that were sufficiently free from government and business bureaucracies to explore novel ideas and methods, Kuznets also emphasized that all work, even the purest of theory, had ultimately to be judged by its social payoff. One may quarrel about the proper way to

measure the payoff to pure theory, but as a practical matter the resources available for such work are heavily dependent upon the volume and quality of that part of the output of scientific disciplines whose social usefulness is readily apparent to policy makers. It is not the pure theorists but the experimentalists and empiricists whose output is directly keyed to societal demands that have been most effective in convincing policy makers to support those seemingly dainty and irrelevant exercises (the pure theory) that they neither understand nor are inclined to support.

A Brief Talk by Kuznets

No one understood the social context of the rise of economics better than Kuznets. So it is fitting to close my comments on his scientific methods by presenting a brief autobiographical talk, to my knowledge the only such liberty he allowed himself. The occasion was a dinner in honor of his eightieth birthday, sponsored jointly by the economics department of Harvard University and the National Bureau of Economic Research. It was a remarkable occasion, attended by some 200 well-wishers, coming from as far away as India, and representing not only economics but other disciplines that Kuznets had influenced. Those present included numerous past presidents of the American Economic Association, the Econometrics Society, and other scholarly associations, seven Nobel prize winners, and economists who had served in government at cabinet and subcabinet levels. It was, as Henry Rosovsky pointed out, one of the most impressive assemblies of scholarly talent ever gathered in a single room. At the end of an hour of accolades showered on Kuznets with deep sincerity, he rose to respond:⁵

You probably will not be able to hear me, as usual. I am really very grateful to all of you who came here,

particularly to those who organized this festivity, and those who were so eloquent on behalf of a person whom I did not recognize. I have an inclination always to think of how much I do not know and how much I have to learn. So it is very difficult for me to recognize in your descriptions the kind of person I think I am. Furthermore, there were certain circumstances that determined my long-term research program that were not of my making and which made it easy to do what I have done. I would like briefly to describe those circumstances.

I came to this country in 1922, at the age of 21, so that most of the first quarter of my life was spent in Russia, primarily in study but also accumulating a fair amount of experience: war, revolution, two years in an institution devoted to economics, two years in the Soviet government. In fact, I ended up as the head of a section of the bureau of labor statistics of the Ukraine, and my first publication was in Russian at the ripe age of twenty. So when I came to the United States, I came with a peculiar equipment: formal training in a scientific gymnasium, a fair amount of experience with statistical research in economics, a fair amount of reading (I knew Schumpeter's work well before I came here), and a liking for orderly quantitative procedures applied to socially-oriented topics.

I spent from 1922 to 1927 working toward my Ph.D. degree at Columbia University where I met Wesley Mitchell, who had a tremendous influence on me. I then wrote my first

postdoctoral research monograph on secular movements in production and prices. In 1927 I joined the National Bureau of Economic Research at which I stayed for three and a half decades. At the National Bureau I also met Edith. We married in 1929 and she has been with me through thick and thin for 50 years now. While at the National Bureau, I was asked to teach in the statistics department at the University of Pennsylvania. I agreed because I felt I should try teaching, although I had earlier turned down an invitation from Columbia college to become an instructor there. I began teaching on a part-time basis in 1930 and became a full-time professor in 1936, while continuing at the Bureau.

From 1936 until 1971, when I retired from Harvard, I continually combined graduate teaching in economics with special research work under the auspices of specialized research institutions. The National Bureau of Economic Research, the Social Science Research Council, and the Economic Growth Center at Yale University all helped to shape my research program and permitted me to pursue it in environments that promoted solid performance. At the same time, I taught at the University of Pennsylvania in both statistics and economics, at Johns Hopkins and at Harvard, which gave me an opportunity to interest the younger generation in some of the problems in which I was interested. Now I submit that the availability of specialized research institutions in this country, which

were rare abroad, and the possibility of combining that opportunity with graduate teaching at the university was one, almost indispensable, condition for the kind of sustained research program that I preferred to follow.

A second set of circumstances should be noted. There was an explosion of quantitative economic research in this country beginning with the 1930s. The U.S. government, and later governments of many other countries, began to accept responsibility for economic growth, for adequate employment, and, in general, for shaping the long-term conditions of economic life in ways in which they did not do previously. These new efforts required recourse to macro measures of the kind on which the National Bureau was working. It was within the year that I began preparing to take over the national income work of the Bureau that I was drafted by the Department of Commerce to construct the first of the official government estimates of national income, in fulfillment of a Senate resolution requesting such estimates. The same sort of pressures continued during World War II because macro measures were needed to check and shape the war production program. If the external circumstances, the concern of the government and the nation with especially urgent economic problems and policies, had not occurred when they did, there would not have been a coincidence between the measures on which I was working and what was needed. Because such measures were needed to carry out national policy, it was possible to secure cooperation

and a volume of resources that otherwise would not have been available.

As I look back on the sequence of studies that I was instrumental in completing, they began with a group of related studies of factors -- cyclical fluctuations, secular movements, seasonal variations -- that affected the development of the American economy. Then they shifted to national income for a single country, the United States. Then they shifted to a wider view, using national income estimates and their components to compare the performance of different countries in many parts of the world on an international scale. That sequence of studies would not have been feasible between 1900 and 1920 or even between 1910 and 1930. It was feasible only between 1930 and 1970.

Let me conclude by thanking you all for participating in this festivity, and I thank you not only for that. In a sense you are all collaborators, who are to be praised for whatever I am praised, and blamed for whatever I am blamed. So let me share the glory and let me share the troubles with you. And perhaps tomorrow we can return to real work.⁶

NOTES

1. Since this paper is a highly personal account of Simon Kuznets's methods, based to a large extent on recollections of conversations and seminar discussions, I have kept references and other scholarly paraphernalia to a minimum. For other views of Kuznets's approach to economics, which overlap but may not coincide exactly with mine, see Easterlin's introduction to this volume as well as Abramovitz 1971, 1985; Patinkin 1976; Ben-Porath 1986; Bergson 1986; and Bergson et al 1987. I have benefited from comments and criticisms on an earlier draft by Moses Abramovitz, Abram Bergson, Ansley Coale, Milton Friedman, Robert E. Gallman, Zvi Grilliches, Edith Kuznets, David Landes, W. Arthur Lewis, Barbara McCutcheon, Marc Nerlove, Dwight Perkins, Gustav Ranis, Samuel H. Preston, W.W. Rostow, T.W. Schultz, Robert M. Solow, Kenneth Wachter, and Nathaniel Wilcox.

2. Foundations and other private institutions also played an important role.

3. Because of schedule conflicts, I was not able to take the applications seminar, and so will confine the balance of this section to my experiences in Kuznets's growth course and during his supervision of my dissertation, which lasted for five years. However, fellow students at Johns Hopkins who took the seminar, told me how much they had learned about the art of measurement from the course.

4. Pearl was a noted biologist and statistician whose theories on population growth are summarized in his 1925 book.

5. What follows is a transcription, which I have edited lightly for publication, of a talk that Kuznets gave at Harvard University on April 25, 1981.

6. The last sentence referred to an all-day meeting on April 26 devoted to a discussion of recent developments in the international study of economic growth.

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