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INTRODUCTION

by Simon Kuznets

I

THE two papers that follow are the first product of a cooperative venture initiated by the International Association in 1950. The venture is aimed at the assembly, review, and analysis of estimates of national income, wealth, and their components for countries for which adequate data extend over at least half a century and thus permit observation of longer-term trends. Since work in this field is further advanced in the United States, the data and their implications could be discussed more fully; and since the two papers form a unit it seemed desirable to publish them in a separate volume. Shorter papers on data for France and Japan will be published as part of *Income and Wealth Series III*. Work by the members of the group continues, promising to bring into review estimates for other countries (Great Britain, Germany, Italy, the Netherlands, Sweden, Australia), and to extend the analysis for countries whose records were only partly covered.

Consistent and fully articulated sets of estimates of income, wealth, and their components, for periods long enough to reveal the level and structure of the nation's economic growth, are not available for any country – no matter how statistically minded. What we find are approximations that vary in completeness, accuracy, and comparability over time. The estimates, currently prepared or inherited from the past, are an amalgam of basic data, plausible inferences, and fortified guesses – in almost no case subject to rigorous tests of statistical accuracy. Their assembly, review, and interpretation require familiarity not only with the basic economic and other statistics of the country, but also with that part of its economic literature that attempts quantitative formulation of aggregates, and with its economic history, which is the background against which inferences suggested by the imperfect data can be checked. For this reason the cooperative effort of the International Association is so organized that the scholar of a given country, who knows its data and its history, undertakes the assembly and

review of the estimates for his country with the hope that the results can then be used by other scholars with a minimum danger of misinterpretation. For the same reason the task is long and can never be completely discharged, but some addition to the stock of comparable long-term estimates of national income, wealth, and their components for a few countries, and some enrichment of our knowledge of the level and structure of economic growth of a few nations, can be expected.

Even this necessarily limited addition to our meager stock of established knowledge is to be desired, despite the effort required. For the contribution of the estimates and analysis sought here to the study of economic growth of nations seems to me to have a type of strategic value for which there is no apparent substitute. Furthermore, greater emphasis on the use of national income and wealth measures in the study of longer-term changes might minimize the dangers which recent concentration on their use in connection with day-to-day problems of government or business policy carries with it; and might also be salutary for current uses, or rather misuses, of the measures in comparisons among 'developed' and 'underdeveloped' countries. It is to a brief sketch of the role of long-range estimates and analysis of national income and wealth in the study of economic growth and in the field of income and wealth research that the remainder of this Introduction is devoted.

II

The spectacular shifts during recent decades in the relative position of nations, and the violent conflicts among them that were both causes and consequences of these shifts, have rekindled interest in the subject of economic growth. 'Rekindled' is perhaps too mild a term: it may be more accurate to say that the complex of problems connected with economic growth was pushed to the forefront by recent events to confront unprepared scholars and wider groups with an acute intellectual emergency. Professional economists have long since abandoned the theories of economic growth of their Classical ancestors, for the good and sufficient reasons that they were naïve, unhistorical, and were proved invalid by subsequent events; have rejected, largely for the same reasons, the Marxian doctrine of the growth and decay of industrial capitalism; have found little constructive

promise in the methodological polemics and empirical studies of the Historical school and its successors; and, finally, have ignored the problem of economic growth in recent economic theory and discourse. They now find themselves in a situation where the basic limiting assumptions of their analysis are like shifting sands; and where the problems that can be handled under these assumptions (largely short-term changes and impacts under conditions of relative stability of both physical and social technology) are dwarfed into insignificance by the processes of economic growth and decay and the attendant conflicts among nations. The wider circles, who did not and do not concern themselves with the abracadabra of technical economics, have had the impression, perhaps inherited from the assured days of Classical or Marxian economics, that the factors making for economic growth of nations are well-known; that the pattern of growth is well charted; and that any country that wanted to could follow the path of economic progress by adopting well-established remedies. In the halcyon days of the nineteenth century it was widely felt that the pattern of Great Britain, and perhaps of the United States, could be followed by other countries if the wish were strong enough; and most probably *would* be followed in the course of time. The events of the twentieth century tended to destroy this belief in the universal applicability of the growth pattern established by Great Britain, or even by its immediate followers, as the key toward economic success. More than that, they destroyed the notions that obstacles to economic and social progress can be surmounted easily and that such progress is inevitable.

The failure of the old theories and notions has pointed up the need for reorientation and a beginning in this direction has in fact been made. In this connection, more careful observation and recording of the processes of long-term change as they have actually occurred seems indispensable. One of the great obstacles confronting economists concerned with the facts of varied situations was the lack of tested empirical evidence. Perhaps an even greater difficulty was the urgent pressure for the formulation of a problem that would permit a determinate solution and thus provide a rationally justifiable basis for economic policy. Adam Smith's contemptuous reference to the limited service that political arithmetic could render stemmed partly from the conviction that his basic theory of social

organization – liberty as the condition of full application of self-interest and hence of vigorous economic progress – was the key to the understanding of the basic economic problems of his age – and perhaps of ages to come. To him the empirical diversities of the past that could be revealed by political arithmetic, even disregarding its cavalier treatment of standards of empirical evidence, were largely irrelevant, since the key to the future lay exclusively in the belief in the benefits of the 'invisible' hand. And what was true of Adam Smith was equally true of his successors in the nineteenth and twentieth centuries since their firm convictions about the dominance of one or another factor, derived from a limited range of empirical reference, precluded the need or desire to record the evidence so that the full variety of experience could emerge. Economic theory, preoccupied with the analysis of the major problems of the day and attempts to rationalize alternative solutions to them, could not accumulate testable evidence. And it is for that very reason that the cumulative inheritance of economic theory and analysis in the way of a stock of established and tested knowledge is so meager.

Can we expect that with the re-emergence of the economic growth of nations as a current problem, we shall again see a flock of hastily formulated theories and generalizations? The probability is great, and the expectation is not to be deplored for problems will not wait for fully established solutions, and it is highly desirable to have as intelligent a basis for action as possible. But it may be urged that the statistical estimates and analysis of the type sought for here already provide a body of empirical evidence that should be scrutinized closely in any attempt to gauge the course and factors of economic growth, at least of the industrialized nations for which such estimates are available. Furthermore, future work in this direction, going beyond hasty statistical or qualitative generalizations, is likely to add to our knowledge of the variety of ways in which economic growth has in fact been attained. Since national income and wealth measure the aggregate activity of a nation, and its distributions among various industries, social or economic groups, types of organization (business, government, etc.), types of use and the like, provide a clear view of the anatomical structure of the nation and of its changes over time, there is little need to stress the importance of such evidence in the analysis of economic growth of nations, the cumulative value of the evidence,

and its direct bearing in the formulation of theoretical hypotheses concerning factors that set the course of economic progress or decay.

III

There is another viewpoint of the nature of economic and social change – not uncommon among scholars in the historical disciplines and akin, in some respects, to the indifferent attitude of the theorist – that discourages emphasis on statistical study of the past. According to this view, historical changes in general and economic growth in particular must be studied in terms of the motivational patterns of the active groups in society, within an institutional framework inherited from the past that changes rapidly over time and differs widely in space. The emphasis must, therefore, be on the institutional changes or differences, with particular attention to the critical junctures at which these changes assume exceptional proportions and may indicate a transition from one era to another. The analysis must deal with individual or group motivations as they drive society through its historical peregrinations. Against this background, generalizations valid over wide ranges of time and space may be impossible: the scholars' task is a continuous reinterpretation of the past in the light of changing criteria given him by the society of his age. Or, if possible, these generalizations are stylized ideal constructs not necessarily testable by canons of empirical evidence – the type found in various philosophies of history. The grandiose dynamic theories of the Classical or Marxian schools, the stage theories of some of the early members of the German Historical school, and the more sophisticated intellectual constructs of men like Max Weber, Pareto, and Schumpeter, are clearly subspecies of philosophies of history, no matter how limited in scope they may seem compared to the broader, if vaguer, generalizations of the philosophers of history *par excellence*, from Vico to Toynbee.

From this viewpoint the possible value of statistical measurement and analysis of social aggregates is limited indeed, since statistical tools can serve to measure only results *en gros* and cannot reveal the underlying motivations and aspirations of the human agents and of the institutional factors at play. What is worse, they necessarily drown the strategic, the revolutionary,

the dynamic, in the mass actions of groups and thus tend to obscure the elements that may most deserve emphasis. Measurement, by its very nature, requires qualitative comparability: one cannot apply quantitative gauges to elements that are qualitatively diverse, and some important economic and social forces may never be susceptible of measurement. The emphasis upon and requirement of measurability may, obviously, yield a distorted picture of historical and economic change; and may well lend an appearance of uniformity, stability, and continuity where the essential elements are diversity, qualitative change, and revolutionary breaks.

In so far as this viewpoint emphasizes the limitations of statistical measurement and analysis in revealing the forces in and the processes of economic growth of nations, one cannot but agree and sympathize with it. That these limitations should be stressed is evident when one considers the dangers of statistical arithmetic running wild and roughshod over major institutional and historical differences, and tying quantities in mechanical regression analysis in which a line connects, say, the capital-product ratios for the United States with those for Honduras or, for that matter, China or India. But the excesses of zeal of some modern political arithmeticians should not obscure the value of statistical estimation and analysis when used in full recognition of their proper limits. Such exuberance in applying powerful tools with disregard of their limitations is not uncommon in the early stages of any approach: it is not a necessary concomitant through life.

The fact of the matter is that, popular impressions to the contrary, statistical study of economic aggregates, even for short-term changes, let alone long-term growth and decay of nations, is in its infancy. Because of the recent origin of the basic data and because of conditions in the institutional organization of social study unfavorable to empirical, and particularly, quantitative research, its pursuit has been exceedingly limited and its fruits so far necessarily meager. As a result, there exists only incomplete awareness of its potentialities. Yet even the bare beginnings indicate that this type of statistical research can serve to revise many notions derived by canons of apparent 'reasonableness' and entrenched by dint of unopposed repetition; and to raise pregnant questions otherwise but dimly seen. Any example may be suspect because of its specificity, but to

cite one: it was somewhat of a surprise to find, first, that the proportion of the national product of the United States saved and invested since the 1870's was so limited – apparently smaller than in some other countries which were at a lower economic level and did not have as vigorous a growth; and, second, that the fraction saved, while on the whole stable over the longer run, tended to decline with a marked secular rise in per capita product. This finding and what it portends for the analysis of factors in the economic growth of the United States has already been foreshadowed in the extensive discussion of the role of the standard of living, the productive value of the population and its training, and the availability of natural resources, but there is a sizable gain in having the finding in quantitative terms that can be tested, even if only roughly.

It is exactly this property that gives its high value to statistical measurement and analysis of economic aggregates: the validity of the results can be tested and easy comparison and manipulation are possible. Granted that the limitations of measurability exclude many forces and factors underlying economic and social growth from statistical view; that only results can be measured, and in a fashion that may conceal the significant and revolutionary changes or phases. The statistical approach does, however, record results that can be tested, accepted, and cumulated and to that extent is an improvement over verbal descriptions and documentary annotation of the succession of individual historical events, or the imaginative interpretations of 'spirit of civilization', 'historical style', or 'challenge and response'. One may also expect that statistical measurement will penetrate deeper into the fabric and structure of economic and social aggregates as data and analysis of components accumulate – to the point where the connections among and empirical constraints upon the various groups in a country's economic *system* will become known. And results thus recorded and established do form the *conditions* within which the unmeasurable strategic motivations or ideals must operate.

In short, statistical measurement and analysis of economic aggregates promise to provide the hard and tested skeleton of observation of the past – one, it seems, that cannot be obtained by other means. The accumulation of tested observations is essential to a framework within which theoretical hypotheses and analysis must be formulated, against which they must be

tested, and from which they may gain some meaning. Around that same framework the verbal and documentary account of institutional and other historical changes may be organized, and the role of the latter as results of specific antecedents or causes of specific consequences assayed.

IV

The study of long-term changes in national income and wealth has great bearing upon research in the field of income and wealth proper. As the time span of statistical observation extends, as correspondingly a wider range of institutional and other changes is encompassed, all the basic problems underlying national income and wealth concepts are perceptibly magnified. Many illustrations are provided in the two papers and in some the magnitudes involved are suggested. But the effects can be clearly seen even in a brief allusion and without the support of statistical estimates. For example, the problem, in defining economic activity proper and distinguishing it from life in general, of the doubtful cases of the services of a housewife and the work within a consuming household of other members of the family, is ordinarily resolved in short-term studies by exclusion. Over a short period, no major changes in the relative contribution of these groups can be assumed; for a period ordinarily relating to the present or the immediate past of an industrial country, the role of such activities *vis-à-vis* the market-bound activities, is quite limited; and in view of the difficulties of measurement involved, exclusion is a justifiable expedient. But when a long period is considered, particularly for a country in which, with industrialization and urbanization, the functions of the household shrink markedly, the exclusion of household activity imparts a significant bias to the secular trend. The problem must, therefore, be tackled afresh, and some constructive solution found or the reasons for exclusion re-examined and redefended.

Other problems that must be reviewed are the questions of valuation, of netness or grossness, of the validity of certain component categories. The relevance of market prices as a yardstick; the difficulties of handling situations where new goods emerge for which no past prices exist and old goods disappear for which there are no current prices; the problem of price

differentials at any given time for one and the same good (among different social classes or between rural and urban groups) – all are brought into much sharper focus when one considers a long historical period in which marked changes may occur in the role of the market, in the composition of the goods basket, and in the structure of prices among different groups in the population; and the easy solutions of these questions in short-term studies cannot be blithely accepted. Likewise, the questions whether the conventional measures of capital consumption mean in fact that much reduction in productive capacity, and how to treat the intermediate services of government, are quantitatively much more important in connection with estimates extending over a long period; as are questions concerning the distinction between ultimate consumption and capital formation, or the invariance of the line distinguishing between agriculture and manufacturing, or between manufacturing and trade.

Indeed, the basic problems underlying national income and wealth measurement assume such proportions with the extension of the period of observation that beyond a certain point, measurement on a continuous basis may be deemed impossible. Where that point is placed is a matter of judgment, which is likely to be influenced by the intellectual habits and the range of interest of the individual. Among many national income and wealth students, actively engaged in estimation and acutely aware of the gaps in the data and of the elements of arbitrariness involved in deriving totals and components even for recent years, there is a tendency to shrink from long-term estimates. This tendency is rationalized by references to the increasing inadequacy of the data as one goes back in time and to the increasing discontinuity in social and economic conditions, which makes the world of say 1870 or even 1900 not comparable with that of 1950.

The reaction to such a tendency can at best be only a different judgment, since the argument cannot be resolved except by actual experiment for a specific country and period. But it may be urged that adequacy of data must be judged in terms of the uses of results: the detail needed for the study of annual fluctuations may be superfluous in a study of long-term changes. It may also be urged that any diagnosis of discontinuity puts the burden of proof on the author: we were all human beings in 1870 and 1950, with the same basic needs and aspirations; and

although our specific formulations of them may have changed, in both years the economic universe was one in which means were scarce relative to desirable ends.

A positive approach to the use of long-range estimates of national income, wealth, and their components seems indispensable because we patently need tested information about our past, and hence about the conditions that govern our choices today. It seems desirable also for whatever influence it may have on research in the field of national income and wealth proper, precisely because it brings all basic problems in this field into sharp focus. It acts as a reminder to all scholars, even those whose activity is limited to work on the current years, that their estimates and analysis imply decisions concerning the basic problems of inclusion, valuation, netness, and the like which, innocuous compromises on the surface, involve a position that perhaps cannot easily be defended, when fully recognized. Finally, it should provide, in the long run, a stimulus toward a more thorough treatment of the problems than is involved in the easy-going compromises accepted in much of the current work on short-term estimates.

This last point seems to me particularly worth stressing. The recent increased emphasis on the use of national income and wealth estimates in the day-to-day work of governments has put a premium on the use of conventions which are not likely to invite probing and disagreement. These conventions are extremely useful in obtaining short-term consensus and allowing the ponderous wheels of government activity to rotate smoothly. But they are likely to bear all the stigmata of short-run perspectives in time and space, since governments are concerned with today and tomorrow, not with the longer past, and are primarily interested in the problems of their own country rather than of a wider sphere. In the process, too, there is considerable danger that the economist may become a bureaucrat and the economic statistician an accountant. Concern with the longer-range national income and wealth estimates widens the historical perspective and accentuates the basic problems; and in considering them, the economist is reminded that he must test his concepts and analysis against a sufficiently long background of historical changes, and the economic statistician that he must test his tools and conclusions within a wider framework of measurable historical variance.

The effect upon international comparisons is equally patent, and is also likely to be salutary. It was inevitable that with increasing detail, rising standards, and greater emphasis on servicing each nation's government, there has been, in the income and wealth field, greater concentration of effort on the study of the recent period in one's own country. We venture a guess that the *proportion* of total intellectual effort absorbed by detailed work on a recent period in the worker's country is greater now than it was say at the end of the nineteenth century or in the early twentieth. International comparisons made under such conditions are quite likely to suffer from our ignorance of other countries and other civilizations. It seems to me that a wider historical perspective in the study of one's own country, always more easily acquired than greater knowledge of other countries – particularly when familiarity with diverse basic data is required – would at least reduce the dangers of misunderstanding in international comparisons. For the problems that arise in intertemporal comparisons – over long stretches of historical time – are not unlike those involved in many international comparisons – when the countries represent widely different types of social and economic organization. If circumstances make it difficult for most of us to know 'other places', much is to be said for at least knowing 'other times'.

V

It is always embarrassing to discuss in general terms the potentialities of a specific approach in a wide field of research. One is likely to combine statements that urge the obvious and are therefore unnecessary, with claims that cannot be justified except by deeds, and that become redundant the moment the deeds are done. At least part of the discussion above may seem to the critical reader to fall into one of these categories. But it did seem useful, by way of introduction to the first results of a cooperative venture that may require substantial effort, to try to assess its role in the wider realms of the study of the economic growth of nations and of research in national income and wealth. Admittedly, this role could only be foreshadowed, not thoroughly appraised; nor was it possible, in the present connection, to explore it as fully as our meager progress already permits. But this sketch should prevent misconception of the

enterprise and indicate its promise and the hopes that inspired it.

Two cautionary comments are added. The first is suggested by the seeming contradiction between our emphasis on the testability of results of statistical estimation and measurement in the study of economic growth, and our emphasis on the accentuation of the basic problems underlying national income and wealth concepts and measures as the historical period is extended. Measurement of *economic* processes necessarily rests upon criteria of economic values and concepts, which in turn imply some basic notions derived from social theory. This necessity and the difficulty of formulating and applying these criteria and notions give rise to the basic problems in income and wealth measurement, and explain the cardinal difference between counting tons of pig iron produced and estimating the net product of the economy or even the output of capital goods. But once the criteria (or their acceptable variants) are agreed upon, measurement is possible and its results can be tested and cumulated. It is important to deny here the impression, which may have been created unintentionally by our earlier discussion, that statistical measurement of national income and wealth is independent of the constructs of economic or social theory, or even of the philosophy of history. The type of statistical measurement and analysis suggested here contributes observations to the study of economic growth whose magnitudes are controlled by certain basic criteria fundamental to economic and social organization; and it is economic and social theory that must elucidate these criteria. That they may differ from one type of social organization to another is obvious; and it also becomes patent that statistical measurement and analysis, to be done properly, require recognition of the bases that underlie measurement, i.e. those, in themselves, immeasurable elements of social organization to which earlier discussion referred. In this sense, there is complementarity not conflict, interdependence not isolation, between statistical measurement and the analysis that aims at formulating the major bases upon which economic and social organization is founded and operates.

The second caution is intended to damp optimism concerning the promise of statistical research in the field of our interest. Because the supply of basic data is necessarily limited, and limited largely to countries belonging to one type of economic and social organization; because the available intellectual

resources have always been reduced by competitive claims of other approaches; and because a large proportion of the few trained statistical economists have recently been pre-empted for work on current problems, rapid progress cannot be expected. When one observes the enormous gaps and lags in our knowledge, the slow accumulation of tested observation and analysis, the niggardliness of society and nature with resources for study of our historical past – the only source of experience available to us – prospects for marked accomplishment in the near future seem dim indeed. But the present venture will be warranted if it adds only a modicum of firm knowledge upon which we and others can build.