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ence, particularly new stretches or newly measured ones, is set by the balancing of gains from a formal set of parameters against losses. The losses are not slight if the formal model conceals revealing deviations by impounding them in anonymous variance, or if modifying any part of the model involves long scrutiny and heavy costs. When the changing economic reality is teaching us something new at a rapid rate, the costs of formalizing prior limited knowledge by dint of a variety of assumptions (substitutes for knowledge that is lacking) may be too heavy, and the gain from having results amenable to formal tests of uncertain relevance may be too slight.

### 3. RECENT TRENDS IN QUANTITATIVE ECONOMIC RESEARCH

Quantitative economic research on the broader aspects of the national economy, dealing with the growth and structure of national product, its origin and distribution, is dependent upon a supply of primary data sufficient for a variety of meaningful economic measures. These, combined with complementary data and relevant hypotheses, can then be interpreted and eventually serve as a foundation for at least partial generalizations and tentative predictions. In turning to trends in quantitative research of this broad type since World War II in this country, I must limit the discussion to studies that employ national product and related aggregates, largely for the analysis of short- and long-term changes in the performance of national economies. This performance is viewed in relation to the commonly accepted goals, e.g., adequate growth, freedom from disturbing fluctuations, equitable distribution of gains, the least painful distribu-

tion of losses, and the like. Much of what can be said of this type of quantitative research on the broader aggregates for national economies (and their major components) applies equally to quantitative research dealing with some important economic institutions that are part of the framework of national economies, or with activities of the major policy agencies, or with the special problems imposed by international relations (rather than viewing the "rest of the world" as a minor sector in the national product total). But since my knowledge of these other areas is slight, I shall limit my review to the field with which I am more, if not fully, familiar—a field to which the National Bureau has contributed much in the past, and the prospects and problems of which must be considered in any discussion of the National Bureau's programs for the future.

#### **Acceleration in the Supply of Primary Data, Economic Measures, and the Pace of Quantitative Research**

A look back over the period since the middle 1940's, and comparing it with the interwar period, conveys a strong impression of an acceleration in the supply of primary data, of economic measures, and of the pace of quantitative research related to the national economy, in this country and elsewhere. Output of primary data is not easily measured, because reduction to comparable units is feasible only for the most elementary types of information (e.g., the number of people counted). This is also true of the generation of economic measures, which, as already indicated, may be complex amalgams of primary data with economic concepts and theories, and with wide quality and significance differentials. As

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to pace of quantitative research, some measure of input and gross output can be secured, if quality differentials are disregarded; but the effort may not be worthwhile, and in any case is not feasible here. I am thus limited to impressions, supported by some evidence.

Let me begin with the most telling set of economic measures, that of national product, its components, and the related totals, for this country. The initial study by the National Bureau in the early 1920's made a significant contribution in attempting to provide some answers to the major question of the day, the distribution of income; and this contribution marked a substantial advance over the earlier work of several individual scholars. The notable expansion of the National Bureau's work and indeed of any work in this field in this country did not come until the 1930's. It was largely in response to the acute need to take stock of an economy afflicted by a major depression and to a changing emphasis on the problems of investment and savings generated by the writings of John Maynard Keynes.

But the major acceleration in the supply of *continuous* estimates of national product, in increasing detail and with an increasingly solid foundation in a variety of primary data, began when estimation was taken over by the Department of Commerce; and when, soon after World War II, the results became a frequently used guide to public and much of private policy. This acceleration was stimulated by the increasing contribution of economic scholarship outside the government, which generated the flow-of-funds approach, the input-output analysis, new devices for sampling and summarization, and a host of other intellectual innovations that served to facilitate and to stimulate an ever-increasing flow of economic measures,

produced in rising proportions by the government agencies. To be sure, this type of acceleration can hardly be measured by percentage rates of increase: after all, when one starts with nothing or very little, the initial proportional increase can be enormous and cannot be exceeded later. Nor does any government initiate economic measures that have not already been explored, and their worth demonstrated, by individual scholars or research agencies that are the main carriers of innovative research. The acceleration of which I speak was an enormous increase in the volume of acceptable economic measures, in increasing detail for any given time and in growing coverage of the historical experience. In this case, the possible economies of scale, in terms of facilitating significant economic research, are so great that linear measures of quantities and proportions are hardly appropriate. Since national product estimates are closely linked with other economic measures that may have an independent value for other uses (price indexes, money supply, labor force and employment, and the like), acceleration in the supply of measures relating to national product and its significant components must have meant acceleration in the supply of other economic measures as well.

What was true of the United States, when one compares the post-World War II situation with that of the interwar period, appears to have been true of other countries that we now classify as economically developed. The assumption of official responsibility for continuous, detailed, and acceptable estimation of national product and its components has been largely a World War II or post-World War II phenomenon in most de-

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veloped countries of the world.<sup>4</sup> The extension of official income estimation was even more striking for the less developed countries of the world. Many of them attained political independence only after World War II, and their governments then faced the problems of assuring adequate levels of performance and growth of the economy, in the concern for which even approximate estimates of aggregate product and its major components seemed indispensable. The explosive expansion in the supply of national product measures the world over in the post-World War II years can be seen by comparing the summaries of these estimates by the United Nations in the late 1940's or early 1950's with what is available in the massive *Yearbook of National Accounts Statistics* for recent years; or by comparing the latter with the League of Nations publications in the 1930's. A similar explosive increase occurred in the supply of data on the balance of international payments in the international compendia of the IMF; on population in the *Demographic Yearbook* of the United Nations; on labor force in the publications of the ILO; on agriculture in those of the FAO; and on education and health in the yearbooks of other UN agencies. Even the Communist countries, after a temporary blackout, began to release more meaningful economic measures after World War II. Communist China and a few of its satellites are significant exceptions. There, if an increased supply is produced

<sup>4</sup> Previously an occasional elaborate estimate had been prepared, but without a continuous follow-up. See Paul Studenski, *The Income of Nations*, New York, New York University Press, 1958, the first 150 pages of which cover the developments before World War I; see also the United Nations, Statistical Office, *National Income Statistics, 1938-47*, New York, 1948, and *National Income Statistics, 1938-1948*, New York, 1950.

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by and available to the government, it is withheld from circulation. The exception is of some significance to the relation between supply of economic measures produced under governmental auspices and quantitative research.

If we accept the impression of a marked acceleration after World War II in the supply of national product estimates and of a wide variety of related economic measures, occurring both in this country and in many other countries, two questions arise.

First, does this also mean an acceleration in the supply of primary data—over and beyond the usual accretion associated with continuation of the old patterns of census-taking, periodic reporting, etc.; or does it mean merely more intensive reworking of the stock of primary data growing at the rate prevailing in the earlier years? In a sense it does not matter which answer is valid. Primary data existed before, but may not have been utilized as raw material for meaningful economic measures. This situation was not untypical of the nineteenth and early twentieth centuries, when data were being published in census volumes and statistical compendia but only gathered dust. During that period quantitative-research-oriented economists (or governments) made little attempt to convert the data into meaningful economic measures, thereby testing their quality and relevance to economic analysis. Until they are so tested, the unused primary data have little value for economic research and analysis. In that sense the initial utilization of existing primary data for formulation of acceptable economic measures (such as estimates of national product and its significant components) is like the supply of new primary data. It provides a base for economic analysis and quantitative research not previously available.

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Yet, there must also have been an acceleration in the supply of basic primary data: some in response to pressures generated by the accelerated attempts to provide meaningful economic measures, and others associated with the factors that explain the more rapid pace of quantitative economic research. If greater attempts were made to combine primary data with other information, and to convert them into economic measures acceptable in terms of the underlying basic assumptions and theoretical hypotheses, and if in this process the available primary data proved deficient, such attempts acted as a stimulus to supplement the existing primary data. In that sense, work on a comprehensive and articulated estimate like that of national incomes provides an incentive for, and a guide to, the collection of missing primary data—especially if they are crucial. Furthermore, if the technology of collecting and processing primary data improves markedly, as it did in recent decades, the resulting reduction in the real costs of deriving the indispensable summaries may, all other conditions being equal, permit a corresponding acceleration in the collection and tabulation of new primary data. The acceleration in the supply of such comprehensive and basic economic measures as national product and its components must have also meant an acceleration in the flow of primary data—even in some developed countries like the United States and the Scandinavian countries where periodic collection of various basic nationwide statistics has been a practice since the late eighteenth and early nineteenth centuries. There is little doubt about the acceleration in the flow of primary data after World War II in the less developed countries and in many of those developed nations that

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were not distinguished by adequate data collection in the pre-World War II days.

The second question, whether the pace of quantitative economic research and perhaps of all economic research also accelerated, is, despite some fuzziness in the definition of a rate of output of economic research, at least answerable in terms of inputs. If the supply of primary data and that of economic measures accelerated, one could certainly argue that the pace of economic research was also accelerating. And, unless the total volume of economic research ceased to grow, or unless the increasing supply of measures and primary data had a *diminishing* effect on “quantization” of economic analysis, one would expect an acceleration in the pace of *quantitative* economic research. Moreover, one would expect that in free market economies, in addition to individual scholars, organized research institutes, and governmental and quasi-governmental agencies, at least the larger business units and trade unions would undertake systematic, quantitative research.

Some evidence is at hand concerning inputs into research. Although a thorough assembly and analysis of such information is beyond the limits of this paper, I can cite some figures available for this country. The number of doctoral degrees in economics (including agricultural economics) granted by American universities rose from 117 per year for the decade from the mid-1920's to the mid-1930's to 132 per year in the next decade; it then jumped to 250 per year in the six years from 1946 through 1951, to 313 in 1957, and was as high as 680 in 1967.<sup>5</sup> While a rising proportion of Ph.D. degrees in

<sup>5</sup> For the earlier years see Howard G. Bowen, “Graduate Education in Economics,” *American Economic Review*, vol. XLIII, no. 4, part

economics may have been awarded to foreign students, the acceleration shown above would hardly be reduced significantly by a reasonable adjustment for this factor. The United States members of the American Economic Association (excluding subscribers), estimated to be less than 2.5 thousand in 1920–1924, rose to about 4 thousand by 1944–1946, and reached 15.6 thousand in 1969.<sup>6</sup> The impression is that the number of people actively interested in economics, particularly those highly qualified for the pursuit of economic research, has grown at an accelerated rate since World War II, and presumably made possible a higher pace of economic research, including quantitative. (The above statement disregards the higher level of training of the more recent products of graduate education.) A similar impression of acceleration after the late 1930's, and particularly after World War II, is produced by the marked expansion in the size of the older professional journals, and by the addition of a number of new ones.

But it would hardly be useful to try to document further these rather strong impressions of acceleration in the supply of data, of economic measures, and in the pace of quantitative economic research. The illustrations and comments above should suffice to yield a conclusion most relevant to any consideration of the National Bureau's program for the future. In the early 1920's, and perhaps through the first fifteen to twenty years of its

2, September 1953, Table 41, pp. 209–10; for the later years see the Behavioral and Social Sciences Survey Committee, *The Behavioral and Social Sciences: Outlook and Needs*, Englewood Cliffs, N.J., 1969, Table 9-5, p. 146.

<sup>6</sup> See the *American Economic Review*, 1969 *Handbook of the American Economic Association*, vol. LIX, no. 6, January 1970, Table 2, pp. 593–594, and Table 4, p. 596.

existence, the National Bureau was one of the few loci of quantitative economic research in this country. However, while remaining a major center for quantitative economic research, in recent decades it has become a much smaller part, proportionately, of the total resources devoted to such research, particularly when one includes, as one should, research done by government agencies. Incidentally, the number of books published by the National Bureau also indicates a rapid acceleration (see NBER, *Publications 1920–1970*, March 1970). Publications in the general and special series grew from 66 for 1921–1945 to 115 for 1946–1969; with conference volumes and occasional and technical papers included, the number of titles is 102 for the earlier period, and 279 for the later. Even so, the rapid expansion in the volume of quantitative economic research, and the resources invested in it in the country at large, calls for a careful scrutiny of the particular contribution that the National Bureau is best prepared to make.

### **Causes of Acceleration**

Of more interest than detailed evidence on the post-World War II acceleration in the supply of primary data and economic measures, and the pace of quantitative economic research, are the factors that might explain such an acceleration. The explanation, tentative as it must be, should tell us much about the relation of economic research to the major problems that it is meant to resolve; help us to identify the problems that still urgently demand, if not solutions (which may be out of reach), at least the amelioration that economic research and policy may provide; and permit us to project the

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possible problems and tasks of research for the future.

Since supply of primary data, production of economic measures, and the pace of quantitative economic research are interrelated, an explanation of the acceleration in one of these complexes explains to a large extent the acceleration in the others. Likewise, the initiative taken by one social agency, e.g., the government, in intensifying the flow of data, measures, and perhaps the pace of research, stimulates a higher pace of research among other agencies, such as academic economists, nongovernmental research institutions, and business firms or trade unions. Yet it is next to impossible to treat these interrelated complexes as one. At the risk of drawing artificial distinctions, we will discuss (i) the assumption of wider responsibility by government for growth, stability, and equity in the national economy, and consider what seem to us the major factors that brought about this increased responsibility, particularly in the older, free market, developed economies. Since these factors also stimulate scholarly work, we consider (ii) how the response of scholarly research to emerging problems interplays with the change in the scope of government responsibility. We then conclude (iii) by considering the changes in tools, analytical and mechanical, that, by increasing the efficiency of inputs, may have contributed to the acceleration in the pace of output of economic research.

(i) The governments of a large proportion of the countries of the world, unweighted or weighted by population or product, had, by the late 1960's, professed to assume responsibility for greater economic growth, full and stable employment, greater equity, and the like. That

proportion is larger than in the mid-1940's; and larger by far than in the early 1920's, when only the newly organized and struggling USSR could be so classified. This does not mean that governments were not always interested in facilitating such goals. But it was generally assumed in the free market economies that, given a minimal framework of political, legal, and social stability, greater economic growth, stability, and equity could be achieved without, rather than with, active government intervention. Moreover, there was doubt that, if government did assume direct overt responsibility for such broad aspects of economic performance, it would know what action to take to discharge such a responsibility.

One thus gets the impression that in the past only narrowly defined economic problems were directly tackled and acted upon by government. Some of these dealt with monopolies and trusts, the deficiencies of the banking and credit institutions in preventing economic crises, the effects of tariffs on specific domestic industries, labor, and immigration (I am citing examples from the United States experience). These were problems created by inadequacies in the prevailing market institutions; and a great deal of economic research was generated by government when each problem was studied. But there was no strong conviction that the broader aspects of growth, stability, and equity should be a continuous and active concern of the government; there was no pressure, like that exerted today, to observe continuously the rate of growth, level of employment, degree of price and other stability, of the economy at large. Nor was there a widely shared belief (recognized in official legislation) that government has an active responsibility for these broader aspects of the national economy. It is,

of course, this shift toward greater government responsibility that resulted in the acceleration of the supply of primary data, economic measures, and the pace of economic research—insofar as government initiative, resources, and pressures were responsible.

What caused this shift? The answer to this question for Communist countries is automatic, since widespread control of the economy by the government, in turn dominated by a single political party, is of the essence—with all the familiar consequences for other economic and social institutions. The answer to the question for the many less developed countries that have only recently acquired political independence may lie in the fact that the personnel who are staffing the new governments are those few with some economic training; and these governments must play a crucial role in the economic and social modernization required to accelerate economic growth. It is for the older, developed, free market economies that the question assumes most interest. In these countries the rate of growth was more satisfactory; occasional instabilities were cushioned by a higher standard of living; and, despite inequalities, inequities were of far narrower proportions than elsewhere. What factors then exercised pressure toward assumption of wider responsibility by the government for these broader aspects of economic performance, rather than, as in the past, leaving them to the working of the market and other private economic and social institutions?

No definitive answer can be given to the question, if only because the consequence to be explained—the shift in overtly accepted responsibility by governments—is itself not precisely defined. It can range from purely political and empty eloquence to a far-reaching positive

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action by the government, involving a volume of resources that is large in relation to the rest of the economy. But if we assume that the shift in question has real substance, a number of related factors seem to provide an impetus to and an explanation of it.

Some of these factors lie in the demonstrated failure of the economic system, as it operated heretofore in these free market developed countries under conditions that were not so exceptional as to warrant the failure. The impact of the great depression of the 1930's was particularly far-reaching. If, with all the adjustments that had previously been made in many developed economies, particularly financial and monetary controls, the contraction in the capacity of these economies to exploit fully their resources could be so disastrous, the existing institutional controls were inadequate and needed to be modified. The modification might have been limited to new provisions for unemployment insurance, new plans for supplementary public works, and the like, if it were not for the Keynesian theory—partly generated by the pressure of events on scholarly analysis. This theory indicated that such a situation could recur if government did not take care to assure adequacy of final demand at a sufficiently high level. And once government had to assume an active role in supplementing private investment opportunities, questions of economic growth and the implicitly greater responsibility of government for such growth—if only in terms of its role in complementing private demand and private investment opportunities—were bound to arise. One should also note that the connection between major depressions and economic growth problems became all the more evident when the distortion of the political and economic structure in Hitler's Germany (and the growth

consequences that followed) was seen as having been made possible, in large part, by the disaffection of the population as a result of the prolonged depression.

The other factor was World War II and its aftermath. Active participation by a country in a major war is clearly an exceptional set of conditions under which the "normal" operation of market and other economic and social institutions must be drastically modified—the more so, the greater the magnitude of the conflict. The main reason, of course, is that a large volume of resources must be devoted to the ultimate social gain of avoiding defeat, and no clear connection with measurable private gain is apparent. The sovereign government must assume responsibility for mobilizing such resources, since it alone can exercise the political and legal authority—backed by social consensus—to limit activities oriented toward private gain, and free the resources required for the socially necessary war task. It is plausible to argue that the experience during a prolonged major war, the successful redirection by the government of economic activity toward a new set of purposes, makes for easier acceptance even in relatively peaceful times of a more active role of government with respect to economic growth, stability, and equity. The effect is stronger if the postwar years are characterized by international divisiveness, with a continuing active international competition in terms of precisely these broader economic aspects. The cold war has certainly contributed to the consensus in favor of greater responsibility of central government for many tasks in which the national economy plays a basic role; and in which national security and national position in the international competition had to be considered together with domestic aspects of economic per-

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formance—the adequacy of which, in itself, assumed significance for the country's international position.

The divided state of the post-World War II world, with the major Communist country, the USSR, viewing itself as beleaguered by a hostile capitalist ring and using all its power to widen the base under its control, constituted both a threat and a challenge to the free market developed countries. It was a threat in that the revolution in the manufacture and transportation of armaments reduced markedly the protection that distance had offered previously. As a result, a warlike situation tended to be maintained, with a large volume of resources devoted by the central government to security-oriented economic production. The challenge lay in the claim of Communist countries that their's was a more effective way to attain a higher rate of economic growth, a greater degree of stability, and even wider equity. The validity of such claims had to be examined, and the overt and hidden costs appraised. But an even more important response to the challenge lay in a re-examination of the rate and conditions of economic growth in the free market economies themselves; and in a scrutiny of the capacity of the older institutions to operate under the changed conditions of the new and divided world. For example, would validity still attach to the theory held by the established institutions, that the promotion of competition would result in success for the more able and productive members in the race, if there was an ever-present threat of being called away from this competition into national defense, or of being destroyed in an atomic holocaust before the competition was completed? Much of the shift toward a welfare state in the older established and developed free market economies was the result of a natural

trend from legal to political, and from political to economic equality. However, much of the shift in the post-World War II years was also the result of a recognition that economic inequalities were no longer justifiable on the grounds of their presumptive contribution to productivity in the market-oriented part of the system, in view of the increasing weight of the required contribution to other high-priority social purposes, with respect to which certain aspects of economic inequality are destructive, not constructive. Just as it proved important to control capital investment and channel savings, without leaving the decision entirely to the private sector of the economy, so it became necessary to be concerned with inequalities in personal income generated by the competitive characteristics of private enterprise. And the underlying international, intersystem competition and challenge had obvious effects on the relations between the developed market economies and the less developed countries, many of which were former colonies. The evolution of new forms of international transfer of resources from the rich developed to the poor less developed countries also meant a new type of activity and responsibility for the government, because of the inadequacy of the ordinary channels of private enterprise.

If the Great Depression, the ensuing war, and the post-World War II intersystem competition in the world were among the more obvious factors responsible for the shift in the free market developed economies toward greater responsibility by the governments for economic growth, stability, and equity, two other groups of factors, less conspicuous and noticeable, may also have contributed. One was connected with the rapid development of technology, the major permissive source of mod-

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ern economic growth, to levels at which innovations could have highly potent and dangerous implications. The marked advances in mechanical power at the disposal of human beings, in rapidity of penetration into the various areas on this planet, and recently beyond it, resulted in diseconomies of mass output under private enterprise that called for remedies possible only with intervention of public authority. Moreover, and even more important, these advances created opportunities for innovations with so much power, and hence so great a danger of misuse, that it was unsafe to leave them in the hands of private enterprise, even if the latter were capable and willing to take the economic risks involved. Thus the technological innovation, war-originated, that resulted in a designed production of atomic energy, carried with it such great danger that careful government decisions were required on how its mass exploitation could be safely developed by private enterprise. Space exploration, another essentially war-originated innovation, involved such a huge volume of inputs and was so far removed from market exploitation that only the government could undertake it—although some of its by-products are already exploitable for private market purposes. The problem that would arise if and when the present work on DNA makes it possible to control the genetic capacities of future generations is another good illustration of a technological innovation that would require control by a socially responsible government. One might, in general, argue that any major technological and scientific innovation, which has dangerous by-products or bestows too much power on its users, cannot be entrusted to private enterprise without careful supervision,

and must be controlled by the government until these dangers can be observed and counteracted.

The second additional complex of factors that might have contributed to assumption by governments of a more active concern with the broader aspects of economic performance is the progress made in the very course of study of the new conditions and the new data, which resulted in revisions of the older theories that implicitly limited government responsibility to keeping order and dealing with immediate emergencies. To illustrate: if the main source of economic growth is assumed to be material capital, and hence savings, and government can only encourage such savings and their efficient utilization, its responsibility is limited to the institutional changes that serve this means of optimizing economic growth. However, if the major source of economic growth is technological change, and if the latter can be facilitated by research, particularly the type that, while basic and immensely productive in the long run, yields results that cannot be appropriated by private interests, then the government must undertake the task, or finance it. To illustrate further: the long-term growth rate can differ widely among free market economies, as it has in fact done during the postwar decades. If these differences reflect a variety of policies pursued by governments, i.e., if the process of economic growth, income distribution, and temporal stability, are not subject to some inexorable laws over which intervention by government can have no useful influence—there is no reason to *limit in advance* the concern of government with the broader aspects of economic activity.

What is most relevant in this connection is that, while the progress of tested study of observable growth be-

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havior of even free market economies tends to remove limits on the possible contribution of government, it also tends to weaken any theories that claim inexorable laws of behavior for private enterprise or the indubitable virtue of some ideological forms of social and economic organization. And this abandonment of dogmatic notions leads both to an acceleration of quantitative research (and of other types of research) and to the pragmatic initiative by governments in assuming responsibility for the basic performance of the economy. It is hardly an accident that Adam Smith was scornful of what political arithmetic could teach him, not so much because its empirical standards were so low but because he firmly believed that the truth of what he thought made for the wealth of nations needed no empirical support. To be sure, his thesis, with its implications, was based in part on the historical experience of Western European societies as he saw them; and he was quite sensitive to the realistic conditions that might limit the full application of the principle to specific problems. But the impression remains that it was only with Malthus, whose thesis was applied to sources of poverty (rather than of wealth), that the search for preventive checks and a more consistent interest in observational data began. It is no accident that, in Communist countries, the dogmatic conviction concerning the rightness of a special type of control and organization of the economy is accompanied not merely by neglect of various aspects of quantitative economic analysis but also by direct prohibition of the wide range of quantitative economic study pursued in societies less committed in this fashion. The variety of growth experience revealed by further study in the United States and other developed countries, and the abandonment of sim-

plistic and dogmatic notions concerning causes of economic progress and related requirements as to income distribution, or tolerance of income instabilities, contributed to the more pragmatic, and hence potentially wider, concern of governments with economic growth, stability, and equity.

(ii) The problems and challenges posed by domestic and international history that induced governments of free market economies to assume wider responsibility for the economy were naturally a stimulus also to economists and research institutions outside of government. If the more intensive analysis of new data and measures established a more pragmatic view, one less constrained by dogmatic notions of what government activity could contribute, it was clearly the type of research pursued by nongovernment agencies that could have this effect of weakening old theoretical notions and viewpoints. In general, and because it is too closely involved with current problems, economic research under government auspices is likely to be conservative, following established definitions and measures and applying them to current problems, often accepting without question the bases of the measures themselves. Few economic measures have been adopted by government agencies in the older free market societies until they had been advanced and experimented with in the scholarly literature. The experience in this country with estimates of national income and product, input-output approaches, flow-of-funds measures, and even periodic sample surveys, clearly illustrates the sequence from academic and research institute scholarship to government use.

But the stimulus to quantitative economic research

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provided by the expanded responsibility and activities of the governments is only part of the story. I have the impression that, in this country (and perhaps elsewhere), the acceleration in quantitative economic research was not only the result of government expansion, either directly or through the granting of funds. After all, major economic problems that called for more economic research, quantitative research included, have always existed, but they have not apparently produced the acceleration in the pace of economic research suggested by the increase in numbers of potential scholars in this country, cited above.

In fact, the acceleration in the numbers of trained economists was part of a much wider movement, affecting all levels of advanced study in a wide variety of disciplines. Limiting ourselves to the numbers of Ph.D. degrees awarded, as a rough indication of the new potential research scholars, we find that the *total* accelerated at a more rapid rate than the number of economists—the latter forming but a small fraction, ranging from about 3.5 to about 6 per cent of the total. The average share of Ph.D. degrees in economics in the total (arithmetic means of annual shares) was 5.8 per cent in the decade from the mid-1920's to the mid-1930's; 4.8 per cent in the following decade; back to 5.7 per cent in the six years from 1946 through 1951 (the period when the great acceleration in numbers began); dropped to 3.6 per cent in 1957; and was only 3.4 per cent in 1967 (see the sources cited above). Furthermore, from 1957 to 1967, the share of economics Ph.D.'s in the total for all behavioral and social sciences declined from 18.8 to 17.4 per cent; and political science, psychology and educational psychology, and sociology (including rural so-

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ciology) showed a greater rise than economics (the latter including agricultural economics).<sup>7</sup> This suggests that the acceleration in numbers of trained potential contributors to economic research was part of a wider movement, and cannot be attributed to the nature of the new economic problems or to the greater responsibility of government for the broader aspects of the economy.

The causes of this wider expansion in higher education, particularly in its most advanced stages, would form a fascinating topic for investigation. Offhand, one would expect to see it as a response to the much higher valuation placed on its contribution to society and to the individual—a reflection of the connection between the recent technological innovations and advanced stages of scientific and experimental knowledge, and the more advanced methods of social and behavioral sciences, as applied to policy problems within both the public and private sectors. This connection would be reflected in the higher compensation to individuals with advanced knowledge in the arts and sciences, as well as in the extensive financial help to graduate students and other advanced learners. Economic inducements at the undergraduate level of education must also have been greater,

<sup>7</sup> See the sources cited in footnotes 5 and 6. With the annual number of Ph.D.'s in economics increasing from 132 in the decade from 1934–35 to 1944–45 to 680 in 1967 and the growth in the total number of Ph.D.'s even greater, the contribution of the larger numbers of the relevant age groups must have been quite small. The most relevant group, men age 25–34, increased from 1940 to 1970 by less than 20 per cent, much less than the total population because of the low birth rates in the late 1930's and early 1940's. For the underlying data see *Historical Statistics of the United States*, Washington, D.C., 1960; *Continuation to 1962*, Washington, D.C., 1965; and *Statistical Abstract of the United States, 1971*, Washington, D.C., 1971.

to widen appreciably the base from which the movement into more advanced training was possible. Such wider inducements were probably provided by differential returns in connection with years of education and by lower real costs associated with a higher level of per capita product and income for an increasing proportion of the population. One could also argue that, given the high-income propensity of demand for education as a consumption good, higher per capita income alone, without the favorable economic differentials associated with years of education, would have brought about an acceleration of secondary and higher education in the post-World War II period, after the backlog of the depressed 1930's and the restrictive conditions of World War II. One could also argue that the demand for education as a *consumption* good was particularly high in recent decades, when better understanding of the events in those years of rapid change placed high value on education—not as a means of earning extra income but as a means of orienting oneself in a difficult world. If such orientation required the equivalent of a good college education, the latter incidentally provided a rapidly widening base for the more advanced types of training and preparation for research, reducing in a sense the cost of information needed for such choices.

Adequate analysis of the acceleration of advanced education in almost all disciplines, of its effect on the pace of research productive of current and future innovations, and of the several reasons for it, would involve comparative study of a number of developed economies that have institutions permitting effective free choice by individuals. Such a study is beyond the limits of this paper and of my capacities. But it is useful to note here

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that the quickening in the growth of numbers of new potential contributors to economic research, including the quantitative, is part of a broader movement, not limited to the effects of a shift toward greater government responsibility even in the free market economies. The implications for our topic are twofold.

First, regardless of the possible continuation of such acceleration, the proximate future may be affected by the innovational changes that the larger body of research scholars is likely to generate. And the rate at which it generates such innovations may rise, even if the marginal contribution of the larger body is lower than that of a smaller body (although it need not be, in a relatively inexhaustible field), so long as the decline in marginal efficiency is more than compensated for by the larger numbers and possible economies of scale. Thus, as we look ahead to the problems of economic research, we ought to be thinking in terms of a *greater* rate of scientific and technological innovation—rather than assume that the high rate of the immediate past represents a kind of plateau or is transitory.

The second implication is more closely related to the possible immediate prospects for economic research. If the numbers of new, highly trained, potential research scholars accelerated more rapidly in related social science and behavioral disciplines than in economics, the research *output* of these disciplines may also have accelerated. Since this output is closely related to the economic, particularly in the analysis of long-term problems of growth and structure, it would presumably have validity and relevance for economic research proper. Indeed, the increasing concern of quantitative economic studies with demographic aspects of economic growth, with the

effects of political structure, with discrimination and stratification, clearly indicates that the nature of the problems is breaking down the traditionally narrow discipline boundaries. One, therefore, must consider the possibility that the output of research, quantitative and other, in the related social sciences and behavioral disciplines has accelerated substantially, and that its more effective utilization may be an important prerequisite in any considered plans and prospective programs for economic research.

(iii) There is little question that, over the twenty-five years since World War II, tremendous strides have been made in the methods of collecting and summarizing primary data, and subjecting it to elaborate computations and manipulations of increasing complexity. Strides have also been made in developing rules of inference from quantitative data, and in building models, simple or complex, that simulate what appear to be the most relevant structural and behavior characteristics of the national economies or of significant sectors within them, thus providing more formal and more discriminating alternatives for organizing and interpreting a vast variety of economic measures. Given, in addition, the acceleration in the numbers of potential research economists with increasingly advanced training, we could assume an increased pace of quantitative economic research. Indeed, we might even interpret it as an acceleration in the *output* of economic research, if we could assume that, over the last two decades and a half, neither the rise in efficiency nor the complexity of the analytical problems with reference to which we can judge the "finishedness" of the output of quantitative economic research had changed from what it had been in the past.

The above statement refers to three, quite distinct, possible sources of rise in efficiency of research inputs. The first is the material technology for collecting and handling primary data and for any derivative measures. Considering the major technological changes after World War II in the field of communication, calculation, and reproduction of information (some based on war-induced types of innovation), one may conclude that the contribution from this source to efficiency of inputs in quantitative research grew at a distinctly higher rate in recent than in the pre-World War II decades.

The second source is represented by the rapid advances that have been made in the theory of inferences from quantitative data. The latter are now being viewed (as they are in much of statistical theory) as reflections of a formally structured universe that can be reduced by analysis to a mixture of invariant and variant components. Distinctions are thus made between approximations to central tendencies and normal variance, or the different structure of small sample variances, or the application of mathematical tools to cyclical and trend components and the like, or the application of other mathematical tools of particular promise to specific economic behavior patterns that lend themselves to meaningful constraints. The resulting tools—some statistical, some econometric, some elements in formal economic theory—can hardly be reduced to comparable units for measuring the additions over the last twenty-five years against those in the preceding decades; and my knowledge of the field is far too limited to permit tenable judgments. Yet I have the impression, supported by chronology of advance in the theory of sampling, design, and statistical inference, the emergence and flowering of econometrics as a subdiscipline, and developments in

mathematical economics, that there is an acceleration in the rate at which the stock of these tools has grown. It seems to me that this stock has proved, after sufficient testing, to have substantially increased the efficiency of post-World War II quantitative economic research.

The third possible source of greater efficiency is the formulation and multiplication of substantive, theoretical hypotheses, relevant to observable reality, which perforce stimulate closely associated economic research. Keynesian general theory was neither an innovation in material technology of data collection or computation, nor did it constitute an advance in the methodology of statistical inference, or in econometrics, or the applied mathematical tools of the corresponding division of economic theory. Yet it proved to be a great stimulus to quantitative economic research, primarily through the emphasis that it placed on savings and capital investment, and on the factors that determined these two components of total product (viewed at the use level) and of total income (viewed at the allocation level). This statement is true also of other hypotheses and models that have emerged in discussions concerning major factors in aggregate growth—at least when these hypotheses or models specify the factors sufficiently to permit establishing their quantitative counterparts, and are not destroyed at the outset by what little is already known of certain quantitative aspects of the economic growth of nations. All such hypotheses and theories are attempts at new generalizations, either cancelling or adding to those already available; and their implications, either for broad judgments of the course of economic affairs or for more specific policy, are sufficiently intriguing to warrant quantitative analysis. A greater supply can poten-

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tially raise the efficiency of quantitative economic research by providing stimuli and guidance, and thus hold promise of more meaningful results.

We cannot state firmly that the flow of hypotheses and theories relevant to quantitative economic research dealing with broader aspects of national economies was more rapid during the post-World War II years than previously. However, we do feel that the rate of addition of such hypotheses as could serve to guide quantitative research, because they were formulated in a more easily testable way, was higher than in the past, when our knowledge of the quantitative outlines of national economies was so limited. And it may well be that the same impression would be conveyed if we were to look at many major aspects of economic analysis. If the pace was greater for theorizing about the performance of the national economy, it would have its ramifications in affecting such major aspects as industrial organization, supply of natural resources, fiscal and monetary policy and theory, and international trade and capital movements.

All of this may mean a greater pace of *intermediate* output of economic research, of economic measures, partial and partially tested hypotheses, interpretations of new stretches of economic experience, and revised interpretations of some old stretches. But what this means for the pace of *finished* output of economic research, i.e., generalizations tested and established within conditions that assure sufficient relevance in application to *realistic* prediction, either for better understanding of what is likely to occur or for better evaluation of the effects of policy, depends on one's judgments of relevance and realism. These would vary with the nature and scope of

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the economic process or structure that forms the focus of such a generalization. Thus, the generalization concerning the cyclical responsiveness of graduated personal income taxes in developed free market economies with established income tax traditions, and on the assumption of realistic conditions concerning income changes, price changes, and the like, can probably be put into a relatively finished form—given the supply of primary data, the econometric tools for analysis, and the battery of hypotheses concerning cyclical sensitivity of various types of income and the relation of the latter to income level. (This may be too optimistic a judgment, but let me use it as an illustration.) On the other hand, it is difficult to think of any 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 (of, let us say, the free market economy) that may be viewed as even approximating a “finished” product—notwithstanding the multitude of such tentative generalizations, cross-section studies, and econometric exercises. At any rate, here we are in the area of difficult judgments, which should perhaps be advanced only with elaborate documentation, not feasible in this paper. It suffices to point out, in concluding our discussion of the acceleration in the pace of quantitative economic research, that, in addition to the changed position of governments (particularly in the developed free market economies) and acceleration in the supply of highly trained potential economic research scholars, there was also a quickening of the pace of improvement in the material technology of collection and handling of quantitative data, in the methodological tools for analysis of a wide variety of complex types of measurement, and

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possibly also in the flow of theories, sufficiently well formulated to guide quantitative economic research in more "productive" directions.

#### **4. IDENTIFYING MAJOR RESEARCH PROBLEMS**

##### **Persistence of Unsolved Problems**

The greater supply of data and economic measures and the accelerated flow of hypotheses and pace of research do not mean that we are now in the happy situation of having answered all major questions and provided an adequate basis for realistic prediction and optimal economic policy. It only means that we have learned a great deal, enough perhaps to force abandonment of earlier simpler and more restrictive theories and to replace them with new hypotheses, more relevant but still based upon many simplifying and restrictive assumptions. It means that there is a basis for a greater consensus on the major changes that occurred in the economy and perhaps on some of the major factors that contributed to these changes. And it means that the greater supply of tested data and of realistic partial hypotheses permits a better evaluation of the implications of the changes as a guide to action. It also means a better choice of policy priorities and perhaps of specific policies—insofar as better knowledge of the basic framework and changes in the economy, and more tested analysis of policies, can affect both the overall priorities and specific policy choices. But acceleration in the supply of data and in the pace of research brings forth a variety of unsolved major problems calling for further