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Introduction

I

THIS is the last in a series of monographs resulting from an inquiry initiated by the National Bureau of Economic Research in 1950, with the financial assistance of the Life Insurance Association of America.[1] The inquiry examines long-term trends in capital formation and financing in the United States, and is organized primarily around the principal capital using sectors of the economy—agriculture, mining and manufacturing, the regulated industries (public utilities), nonfarm residential real estate, and governments. The analysis for each sector summarizes the major trends in real capital formation from 1870 (or the earliest year for which data are available) and in financing

[1] The others, all published by Princeton for the National Bureau of Economic Research, are: *Capital Formation in Residential Real Estate: Trends and Prospects*, by Leo Grebler, David M. Blank, and Louis Winnick (1956); *Capital in Agriculture: Its Formation and Financing since 1870*, by Alvin S. Tostlebe (1957); *Financial Intermediaries in the American Economy since 1900*, by Raymond W. Goldsmith (1958); *Capital in Transportation, Communications, and Public Utilities: Its Formation and Financing*, by Melville J. Ulmer (1960); *Capital in Manufacturing and Mining: Its Formation and Financing*, by Daniel Creamer, Sergei Dobrovolsky, and Israel Borenstein (1960); and *Trends in Government Financing*, by Morris A. Copeland (1961).

Some of the findings had previously been presented in part or in preliminary form in a series of occasional and technical papers published by the National Bureau: Leo Grebler, *The Role of Federal Credit Aids in Residential Construction*, Occasional Paper 39 (1953); Daniel Creamer, *Capital and Output Trends in Manufacturing Industries, 1880–1948*, Occasional Paper 41 (1954); Raymond W. Goldsmith, *The Share of Financial Intermediaries in National Wealth and National Assets, 1900–1949*, Occasional Paper 42 (1954); Melville J. Ulmer, *Trends and Cycles in Capital Formation by United States Railroads, 1870–1950*, Occasional Paper 43 (1954); Alvin S. Tostlebe, *The Growth of Physical Capital in Agriculture, 1870–1950*, Occasional Paper 44 (1954); Israel Borenstein, *Capital and Output Trends in Mining Industries, 1870–1948*, Occasional Paper 45 (1954); David M. Blank, *The Volume of Residential Construction, 1889–1950*, Technical Paper 9 (1954).

from 1900 (the earliest practicable date), and the factors determining those trends; and, so far as possible, suggests the significance of those factors for the future. In addition to the five sector studies, the inquiry comprises two others. The first deals with trends in financing channeled through intermediate financial institutions and attempts to link the major types of institutions with the various groups of capital users. This, the second, utilizes the results of all the other studies within a framework provided by countrywide estimates of national product and its relevant components and of assets and debts, and draws upon estimates and findings not covered in the other monographs.

II

A proper view of trends in capital formation and in its major components requires comparison with national product, from which the savings embodied in capital formation are drawn, and for the production of which capital investment is made. Although the five sector monographs—on agriculture, manufacturing and mining, the regulated industries, nonfarm residential real estate, and governments—do not cover the whole economy, the capital investment and output of those sectors account for a substantial part of the national totals of capital formation and product. Yet it was necessary in this summary monograph to provide a set of continuous and comparable estimates of national capital formation and national product, covering the decades since the 1870's. Of capital formation, several components are distinguished: construction, further subdivided into nonfarm residential, government, and all other; producers' durable equipment; net changes in inventories; and net changes in claims against foreign countries. Within national product, in addition to the distinction between capital formation and flow of goods to consumers, there is the distribution of the latter among services and commodities of differing durability. The task involved the preparation of a fairly elaborate framework of estimates: both capital formation and national product were estimated with and without allowance for consumption of durable capital; these totals and their components were expressed in both current and constant prices; for some of the totals several statistical variants were calculated; and important related series such as population and labor force had to be estimated. In addition, in order to distinguish between the long-term trends and the swings or alternations in the rate of growth, we needed continuous estimates for successive periods short enough to permit study of changes in the rate of growth.

A similar task was involved in securing a nationwide view of trends in financing—even though the statistical picture of that aspect extends back only to 1900. The sector monographs on capital formation and financing do not cover the entire economy, and, furthermore, the differing supply of data available results in varying degrees of detail and coverage. The monograph on financial intermediaries presents a more comprehensive picture, but it stresses external financing and the changing shares of various groups of intermediaries in the latter. Consequently, it was necessary to organize and supplement the data on financing by countrywide estimates covering both internal and external financing, a task for which Raymond Goldsmith's monumental study of saving in the United States and his work at the National Bureau in recent years were invaluable.

Thus, much of the material for this summary monograph, other than that drawn from the monographs that preceded it, was estimated, and the need to present the estimates and describe the sources and methods used accounts for much of the bulk of the present volume. Indeed, the appendixes are really a statistical supplement, which sets out in detail the series on national product and capital formation, as well as the related series on population and labor force. But even in the body of the report a fair amount of space is devoted to the estimates and their derivation. Because of the detail in presentation and description throughout, the volume can be used for reference as a compendium of estimates which can be employed for a great variety of analytical purposes.

Having at hand the national estimates and the wealth of series in the preceding monographs on various sectors and on the financial intermediaries, the next task—in logical if not historical sequence—was to order and classify these data so that they would reveal the levels of and movements in capital formation and its financing. Because our concern was with long-term movements, the process of classification and arrangement called for the grouping of the data in space—distinguishing, comparing, and combining various components of capital formation and financing by type, by grossness and netness, by industrial user—and relating some of these groups to the appropriate product, population, and labor force totals. It was necessary also to group the data in time, by periods long enough to cancel the effects of short-term fluctuations associated with business cycles and to reveal, if possible, the underlying long-term movements. This task of classifying was rendered all the more difficult because we were compelled to distinguish

within the long-term movements two components: the long swings or alternations in the rate of growth, and the secular trends that underlie and transcend those swings.

Most of the discussion is devoted to the results of this ordering of the data on national product and on capital formation and its financing. The aim was to observe the long-term movements in these important aspects of the economy since the 1870's or since the beginning of this century, and to present the findings in a systematic sequence that would help us absorb this segment of experience into the background of our thinking. This apparently simple task is beset with numerous pitfalls: the component categories available in any set of estimates are often too broad to permit distinction of significantly different groups or processes, and the changes over time are often so violent and erratic that the underlying trends can be discerned only with difficulty. The major reason for space-consuming alternative groupings and discussion of them is to minimize the danger of overstating the findings, of attributing a degree of simplicity and firmness to them that is not justified by the evidence. But prone as we all are to oversimplification and generalization, this weakness may not have been entirely avoided here.

With the ordered empirical findings at hand, the next task in logical sequence was that of explanation—of attempting to show with what patterns of established generalizations a specific finding could be associated, of what general law it was a specific case. Since there are few such generalizations or laws in the field of economic knowledge, and since any linking of a given set of findings to some established theoretical tenets requires more precise knowledge of the mechanism involved than is available, adequately tested explanations are not now feasible—at least for the findings of the present inquiry. Our explanatory discussions are, perforce, only suggestions of the factors that were likely to have been operative in producing the trends and long swings found, and where practicable, the findings were reduced to the next set of immediate quantitative determinants. The result is, at best, a sketch of a possible but untested association between the findings and a set of known or reasonably acceptable general patterns of economic behavior, an indication of the directions in which specific tests of the suggested associations are to be sought, not a demonstration of the existence of such links. In short, the explanations are conjectural rather than tested, partial rather than complete, suggestive rather than definitive.

Conjectural as the explanations must be, they are essential if we wish to speculate, in a systematic fashion, on the bearing of past trends upon the prospects for some projected future. For the trends in the past are never so persistent and stable as to permit a purely statistical extrapolation into the future, even if one were willing to accept the mechanical projection of patterns of human behavior without attempting to understand why and how such patterns have come about. It is thus indispensable to identify at least some of the factors that may have produced the past trends and to evaluate their possible continuation into the future. We face the problem of projection only in the last chapter. This is, essentially, a task of controlled speculation, but one which, to be done properly, requires a systematically organized sequence of steps that could not be encompassed within this inquiry. There are, in consequence, no quantitative projections given. Instead, Chapter 10 lists the conditions for the future that would have to be carefully examined in any systematic projection, and discusses briefly some aspects of these conditions that seem relevant on the basis of general observations rather than intensive examination.

The study reported here is thus largely a compound of estimation and classification, seasoned at different levels of empirical findings with conjectural explanation, and topped off with a frosting of impressionistic speculation. But this description should be amplified in two respects.

First, the distinction drawn above between the different phases of the work is too neat, and, as everyone who has ever engaged in research knows, the logical sequence is never followed in reality. One begins an examination of the available data, already full of notions as to the important groupings and classifications that are to be followed, which in turn are guided by some explanatory hypotheses, that have been either explicitly formulated or only adumbrated, and by some ideas as to the significant persistent factors. Then, in the process of organizing the data called for by this complex of the half known and half conjectured, revisions of the initial mixture of knowledge and conjecture occur, which may—and usually do—call for more data, different classifications, and different hypotheses. Then, with the additional data and new groupings, still another revision may occur. The actual work is thus a continuous interplay of estimation, classification, explanation, and speculation—not a concentration on each successive phase in accordance with the logical sequence.

Second, the relative weight of untested judgment—of tentative choice

—increases as we move from estimation to classification to explanation and to speculation. But this element of untested judgment is also present in the results most closely connected with the observational data. The very term estimation, as distinct from measurement, indicates that untested judgment is involved in fitting the primary data—themselves subject to error—into the categories of the framework of economic analysis, from which such concepts as national product and capital formation are drawn. Moreover, the way in which we order and classify the estimates, in itself, reflects some hypotheses concerning the relations of the underlying processes: we do not attempt to exhaust all possible ways of combining the data. Conversely, explanatory hypotheses concerning the past and speculative conjectures about the future obviously carry a heavy load of empirical observation—even if we sometimes have difficulty in distinguishing and identifying it when the formulation is too general. And yet, the argument must not be carried too far. There is a clear distinction between stating that the past records reveal this or that trend or level and arguing that there is a causal association between a given finding and, say, changes in distribution of income by size or relative prices of debt and equity funds. There is an even greater distinction between statements concerning trends in the observable past and their projection into the unobservable future. We hope that these distinctions are sufficiently clear in the discussion to minimize the danger of misunderstanding.

III

An attempt to summarize here the empirical findings of the study, and the explanatory hypotheses relevant to them, would run the danger of overstatement and repetition. Chapter 9 has been expressly designed as an effective summary, and readers who do not care to go systematically through the entire volume are advised to begin with Chapter 9, and then dip into the earlier chapters or into the other monographs of the series, whenever some specific finding or hypothesis invites closer attention and scrutiny.

It may be useful, however, to list what seem to be the more important empirical findings, without attempting a complete coverage or touching upon the explanatory hypotheses suggested. As in Chapter 9, the findings are those observed for Variant III, excluding the military; and, except where noted, 1946–1955 is the terminal period.

1. Over the period since the 1870's gross capital formation (the sum of gross construction, producers' durables, net changes in inventories,

and net changes in foreign claims) accounted for a fairly constant proportion of gross national product in current prices—somewhat over a fifth. The long-term trend in the gross capital formation proportion based on volumes in constant prices was slightly downward.

2. The consumption of durable capital (construction and equipment), a measure largely of economic obsolescence rather than of physical wear and tear, grew at higher rates than did gross capital formation; and its ratio to the latter rose from about four-tenths in the early decades to almost two-thirds in the recent decade (for volumes in constant prices).

3. The proportion of net capital formation in net national product declined, for volumes in constant prices, from somewhat less than 15 per cent in the early decades to 7 per cent in the most recent; for volumes in current prices, it declined from 13 per cent to somewhat less than 9 per cent.

4. Within gross capital formation, the share of construction declined, from almost two-thirds in the early decades to over one-half in the most recent; and so did the share of net changes in inventories, from about one-seventh to about one-nineteenth. The share of producers' durable equipment increased from somewhat above one-fifth to over four-tenths, while the rather minor share of net changes in foreign claims shifted from negative to positive. These trends relate to totals in current prices, but the trends in the shares in constant prices are fairly similar.

5. In the distribution of domestic capital formation (construction, producers' durables, and net changes in inventories) by category of user, we find that, in the gross totals in constant prices, the share of governments increased, from less than one-twentieth to almost one-eighth; that of households (nonfarm residential construction) declined, from well over one-quarter to about one-sixth; and that of business firms rose, although only slightly. The movement of the shares of governments and households in gross volumes in current prices was similar to that in constant prices, but the share of business firms declined.

6. Among the major divisions of the business sector—agriculture, manufacturing and mining, and the regulated industries (public utilities)—the share of the first in net durable capital formation in constant prices showed no distinct trend, that of manufacturing and mining rose, and that of the regulated industries declined. These movements of shares in net durable capital formation do not correspond to the

trends in growth of output of these sectors: agriculture's share in their combined output (also in constant prices) declined, and those of manufacturing and mining and of the regulated industries rose.

7. The ratio of net capital stock (net of depreciation) to net national product (both in constant prices) first rose, from 3.2 in the early decades to 3.6 in the 1920's, and then declined, to 2.9 in 1939–1955 or to 2.5 in 1946–1955. The movement of the ratios for the various sectors of the economy differed: for agriculture, and mining and manufacturing there was also a rise through the 1920's and then a decline; for the regulated industries there was a continuous decline from the high levels of the earlier decades.

8. For the economy as a whole, the ratio of internal financing (gross retention) to total uses declined, but moderately, from 0.60 in the first decade of the century to 0.56 in the recent decade. There was an even slighter decline in the ratio of internal financing to gross capital formation, from 0.78 to 0.77.

9. The stability or slight decline in the ratio of internal financing for the country as a whole was due largely to the effects of the household (nonfarm residential construction) and federal government sectors, in both of which the ratio declined and that of external financing rose. By contrast, the trend in the ratio of internal financing in the business sector—the combined total of agriculture, nonfarm unincorporated enterprises, and business corporations—was upward, although mildly so, the ratio of internal financing to total uses of funds rising from 0.59 to 0.64. The latter trend was naturally dominated by the corporate component, by far the largest in the business sector. For nonfinancial corporations the ratio of internal financing to total uses rose from 0.55 to 0.61.

10. The rise in the ratio of internal funds was particularly marked for corporations in the regulated industries, but for mining and manufacturing corporations there was no rise when internal funds were related to total uses (rather than to gross capital formation).

11. In total external financing, the share of the federal government rose markedly and the share of the private component declined. Within the latter, however, the share of households increased substantially, that of agriculture declined appreciably, while those of nonfarm unincorporated enterprises and corporations showed only a slight decline, or stability.

12. The share of equity financing in total external financing for nonfinancial corporations declined somewhat, from about three-tenths

at the beginning of the century to about one-fifth in the recent decade. But the trend was neither marked nor consistent.

13. The share of financial intermediaries in total external financing in the country increased, from somewhat less than one-half early in the century to about two-thirds in recent years. The share of financial intermediaries in total financing in the country also rose, from about one-fifth to about three-tenths in 1946–1955 or four-tenths in 1931–1955.

14. Among financial intermediaries, the share of the banking sector in total assets declined, from an average of well over six-tenths in the first two decades of the century to somewhat less than four-tenths in recent years. The share of the insurance sector rose from less than one-eighth in the first two decades to almost four-tenths in recent years. The share of government institutions (Federal Reserve Banks, government lending institutions, government pension and security funds) rose over this period from well below one-tenth to somewhat over one-fifth.

15. The statements above refer to secular trends that underlie the long swings, the alternations in rates of growth. These long swings, about twenty years in duration on the average, are clearly observable in additions to population, immigration, gross nonfarm residential construction, gross durable capital expenditures by railroads, net changes in claims against foreign countries (capital imports and exports), and indexes of stock market prices for some groups of securities (particularly utility). We find them also in other components of national product, capital formation, and financing; but the results do not lend themselves to brief summary.

IV

In concluding this introduction, we consider a question suggested by the description in section II of the kind of compound the present study is, and by the results listed in section III. Granted that the inquiry manages to establish a number of empirical findings of the type illustrated, if they are not accounted for by a testable theory that can provide a firm basis for projections into the future or policy decisions in the present, is the study of much practical use? If we do not succeed in establishing a reliable, generally persistent pattern backed by a valid theory, that is, an association with other invariant patterns of behavior on whose persistence we can rely, do the few additional empirical findings have any value except as a matter of idle antiquarian curiosity?

The answer to such a broad question can, within the present context, be only a matter of judgment; and the question would not have been raised except that, to my mind, it reflects a common misunderstanding concerning the relation between research on social processes and its applicability to social action. In particular, the misunderstanding rests upon an inadequate view of the links between knowledge and policy; and it may not be out of place to comment upon this problem here.

We begin with the simple statement that any tested empirical knowledge, relating as it does to observable natural or social processes, is by definition potentially useful. For usefulness means exploitability of the patterns of behavior of nature and society for ends deemed positive by us as human beings. And it is difficult to see how additional knowledge about observable patterns of behavior of either nature or society can fail to be potentially useful—since every aspect of such patterns is relevant to some positive aim of mankind.

The failure to recognize this ingrained potential usefulness of all tested knowledge is due partly to the wide gap in some cases between knowledge and its specific application to ends other than idle curiosity, and the links involved in bridging the gap are almost inconceivably complex. Consider the leap of imagination required of an observer who could have glimpsed the value of the application of Hertzian short waves to the electronic industry of today, or of non-Euclidean geometry (although that is not empirical knowledge pure and simple) in its later uses in applied physics.

These examples are taken from the natural sciences by design, for it is in them that the distance between knowledge and use is often great; and this may well be one of the sources of their strength. In the field of social study, unlike the others, additions to knowledge have two types of use. One is similar to the application of knowledge in the natural sciences: thus, measures of the price elasticity of demand for a commodity may be employed to estimate the likely effects of an excise tax, in consideration of tax policy, just as knowledge of resistance of materials, prospective weights, and theoretical mechanics can be used in calculating the requirements for a bridge. The other use has much less in common with the application of findings of the natural sciences, but it is pervasive in the social processes: additions to knowledge in that field become absorbed into the background of a wide variety of people, and it is against this background, professional and lay, that discussion and consideration of broad policy issues occur.

New theories and discoveries in physics and astronomy concerning cosmogony no longer agitate us, as they did our forebears in the times of the Copernican and Galilean revolutions; but the changing knowledge of the behavior patterns of society colors both the continuous succession of social doctrines and the whole milieu within which social problems are discussed and solutions decided upon.

Thus, in addition to the purely technical application of findings of the study of economic and social patterns when possible, there is the more pervasive use of those findings as a framework within which the social problems of the present and the future are considered. The time lag may be long between establishment of empirical findings and their absorption into a tested theory of reliably invariant patterns for practical technical use in projections or in estimating calculable effects under alternative policy actions (as illustrated above in connection with short-term price elasticity of demand). But the time lag is short between new findings and their use to enrich the background against which broad current problems are considered. Indeed, the danger is not that such findings will not be used for practical purposes, but rather that the results will be eagerly seized upon to yield a spate of hypotheses that claim too much generality, of new theoretical positions which have vitality because they lead to policy consequences that seem more adequate than the old. Yet, granting this ever-present danger that new empirical findings may provide a starting point for distorted use or for unwarranted dogmatic generalizations, there is a clear need for more, not less, such empirical knowledge; for a longer, not shorter historical perspective; for a more detailed, not a more aggregated, structuring of the empirical evidence; for wider interspatial and intertemporal comparisons rather than concentration on a single country and period.

In short, the most important practical use of the type of findings the present study provides, coupled with the related suggestive explanations, is to enrich the stock of tested knowledge which provides the background for much of the theorizing and decisive discussion of broad policy issues. Without such findings, the background is inadequate—one against which, for example, recent short-term changes could not be set in their longer historical perspective, and against which some extrapolation could not be fully tested, and consequently might be erroneously projected into the future. I do not mean to argue that additions to tested knowledge and to relevant hypotheses are a sufficient condition for more effective theorizing and better decisions on broad

policy issues, but they are surely necessary. And if it is true, as Lord Keynes once said, that the political leaders of today usually operate with the obsolete economic theories of yesterday, then there is surely immense value in additions to tested economic knowledge that contribute to economic theories with broader and longer validity, and thus reduce the deleterious effects of that type of cultural lag.

This potential contribution of the present study can best be visualized if it is remembered that no inquiry stands by itself. It is but another item in a whole sequence, and in a wide range of related inquiries. Its results should be utilized not in isolation, but together with all else that is known in the field—either in the way of empirical data or theoretical hypotheses linked to such data. The optimum use of the present study is as an *addition* to the economic history and analysis of the United States and of other countries, to the stock of already known hypotheses, and in as full conjunction with these other data and hypotheses as any particular problem warrants.

To put it another way: like any other empirically oriented inquiry, this one has no true beginning and no sharp end. It has no beginning because it is rooted in a variety of past studies, and is in a sense unintelligible unless it is added to much of what is already known or conjectured in the more comprehensive literature on the broad aspects of modern economic life. It has no clear end because, while we hope that it advances our knowledge a notch, its contribution can be tested only when its results have been absorbed and revised in a more extensive framework in which they will find their proper niche as well as eventual oblivion in the loss of their identity. But only in such building upon the past complex of knowledge was the study feasible; and only in its absorption in the next phase of work in the field can it make an effective contribution.