

This PDF is a selection from an out-of-print volume from the
National Bureau of Economic Research

Volume Title: Economic Research and the Development of Economic
Science and Public Policy

Volume Author/Editor: NBER

Volume Publisher: NBER

Volume ISBN: 0-87014-115-5

Volume URL: <http://www.nber.org/books/unkn46-1>

Publication Date: June 1946

Chapter Title: Empirical Research and the Development of Economic
Science

Chapter Author: Wesley C. Mitchell

Chapter URL: <http://www.nber.org/chapters/c9701>

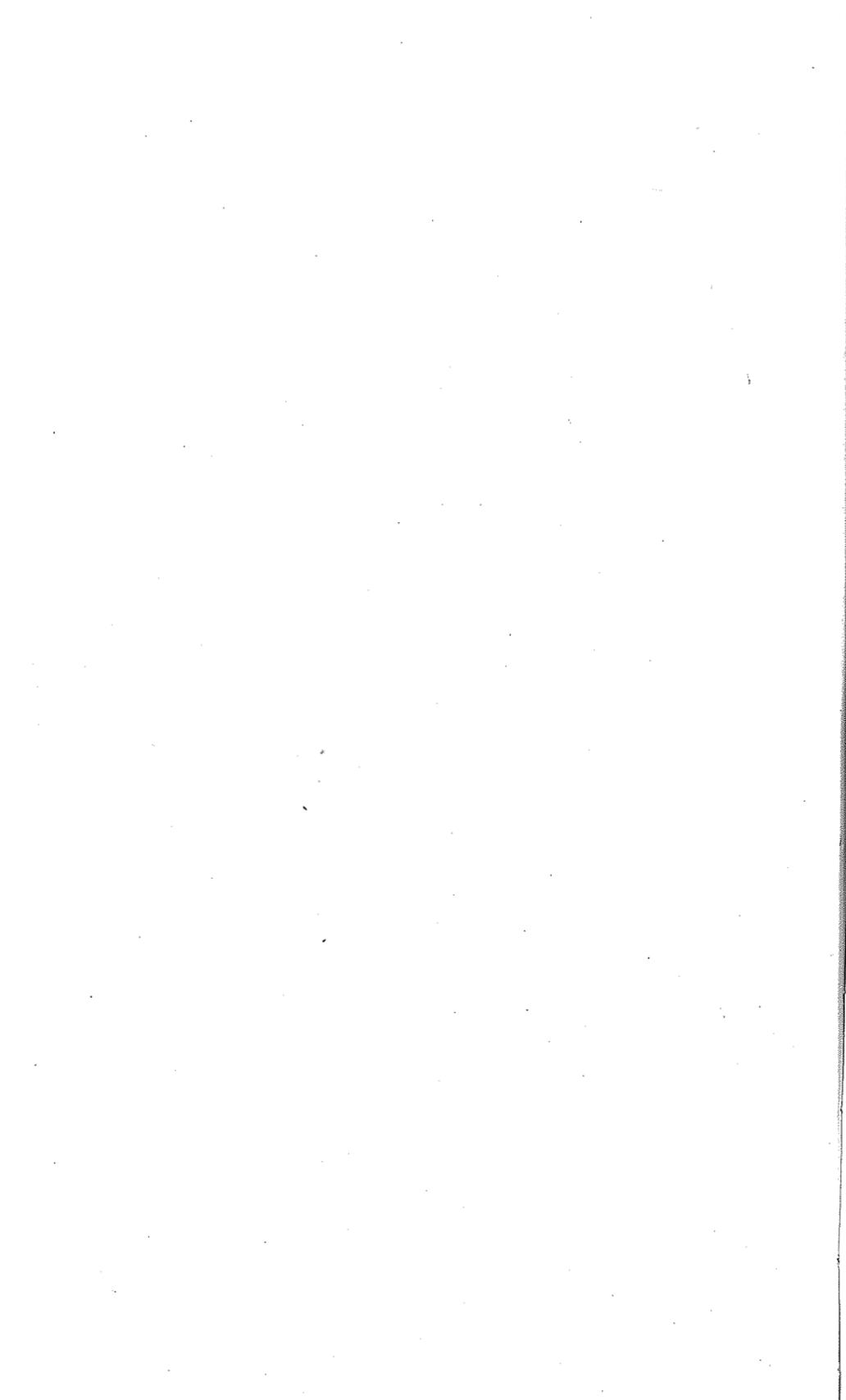
Chapter pages in book: (p. 1 - 20)

*Empirical Research and the
Development of Economic Science*

WESLEY C. MITCHELL

*Professor Emeritus of Economics,
Columbia University*

*Director of Research,
National Bureau of Economic Research, 1920-1945*



ON BEHALF of the Directors and staff of the National Bureau of Economic Research, I have the pleasure of welcoming you who have come from far and near to assist in celebrating somewhat belatedly our twenty-fifth anniversary. To some among you we are indebted for moral and material aid at the beginning of our experiment when no one could foretell its issue. More of you have helped to bear the heat and burden of later days. Others have been working elsewhere at similar tasks, and we have profited by your achievements. As exemplars of our faith and fellow workers, we are grateful to you one and all, and to those you represent.

Our chief interest on this anniversary, however, concerns the future rather than the past; not so much what economic research has accomplished as the work that lies before it; less the fortunes of the National Bureau than of all who are striving after fuller knowledge of "mankind in the ordinary business of life". We believe that the effort in which we have shared with you has already made contributions of substantial value to society's knowledge of itself and of how to manage its affairs; but to us this assurance is primarily an encouragement to press further in the direction we have been following.

Today we are aware of more tasks than were in our minds at the close of World War I; we can define our problems more clearly and grasp more firmly their relations to one another; a larger and richer body of objective data is available for our use; the methods of deriving warranted conclusions from these raw materials are more varied and more powerful. Meanwhile, the

course of world affairs is making larger and more imperative demands for services of the type we try to perform. It is the National Bureau's hope that this anniversary meeting may aid economists to carry the heavier responsibilities that are being thrust upon them.

If that hope is to be realized in any measure, we may fitly begin our sessions by considering what will be demanded from us, and what we ourselves wish to supply. Traditionally our position has differed widely from that of other professions concerned with the work of the world. Lawyers, engineers, and accountants have been employed for the most part in helping to direct practical operations, private and public. They have been asked for technical advice, and their success has depended upon how the actions they recommended turned out. Economists have had a much less intimate relation with current operations. Until recently, few of us have been sought out by clients ready to pay for our counsel. We have given a great deal of advice, but most of it has been unsolicited, and much of it has been rated as free goods commonly are, or even assigned a nuisance value. We have commonly addressed, not individuals, but the public at large—especially the young section of the public enrolled in our college courses.

This position, somewhat aloof from the activities we have been studying, has not prevented the seminal minds among us from exerting a powerful influence upon the cumulative change of institutions. Adam Smith, Bentham, Malthus, Ricardo, Mill, Marx have helped to sway the course of events in ways we can specify. And aloofness has had positive advantages. Not being immersed in operating details, economists have been relatively free to follow their intellectual interests in whatever direction inclination and circumstances urged. They could deal when they chose with problems of general and abiding concern to people at large—problems that often have slight appeal to business enterprises and political administrations. Those inclined

Empirical Research and the Development of Economic Science

toward abstract speculation have been especially favored; for that type of work is least hampered by the conditions under which an academic scholar usually lives. Gifted logicians have developed beautifully articulated systems of thought without worrying much about the relation between their conclusions and what actually happens. Such restrictions as the world has imposed upon us have been mainly vague pressures to conform to the conventions prevailing among our fellow citizens—pressures to which other professions are subjected in perhaps greater degree. Yet nonconformists among us from 'the labor writers' of Ricardo's time to Thorstein Veblen have been allowed to speak their minds, though at considerable cost to themselves.

But aloofness has also its drawbacks. They have been felt especially by economists whose temperaments inclined them toward empirical research. Men of this ilk have always been numerous in our profession—folk who wanted to learn all they could about actual processes, from levying taxes to caring for the poor. They have needed contacts, assistance, and familiarity with prevailing practices not readily available to many scholars. At best the materials they sought have been hard to assemble, and hard to analyze. Yet, despite all handicaps, students of money and banking, foreign trade, public finance, transportation, monopolies and trusts, labor problems, and the like have gathered and made meaningful imposing bodies of information.

Unfortunately, these specialists often worked without much benefit of economic theory, just as theorists often worked without benefit of much factual knowledge. Both types of effort were the poorer for lack of integration with the other. Some economists have acted as if finding facts and understanding them were separable activities. The theorists like Adam Smith, Malthus, and Marshall who have been avid students of factual data, and the empirical investigators like Jevons who have been keen theorists, are not very numerous. Even these men often kept their two interests apart in their writings.

All of us, I fancy, sense a change in the position of our profession—a change that from slow beginnings has recently become rapid, and promises to continue at a more moderate pace through the future we can foresee. Conditions over which we have little control are sucking us into more active participation in the conduct of affairs, lessening our aloofness, adding to our responsibilities. And this change is reacting upon our own conceptions of the type of work we should do, leading us to seek a better blending of empirical with speculative inquiries.

I take the change to be basically a secular movement emanating from the cumulative growth of natural science and its applications to everyday life. The arts of production, and still more the arts of destruction, are visibly increasing the interdependence of men. Both within nations and among nations, individuals, business enterprises, and governments are realizing that their fortunes depend upon the fortunes of a widening circle of other individuals, enterprises, and governments. Less and less are people able to observe personally, let alone forecast, the conditions they should prepare to meet. More and more must people rely upon information gathered by others. Nor is the difficulty confined to finding out what is happening in many quarters. To assess reports and decide what courses of action promise the best results requires trained analysts, among whom economists are becoming more numerous, relatively as well as absolutely.

While this growing demand for the services of economists has the earmarks of a secular movement, it is obviously subject to random perturbations, of which the most spectacular have been due to war. Just as modern technology bends the economic activities of peace to its requirements, so it bends the activities of war. Economic mobilization becomes essential to victory. The freest of nations let their governments plan and supervise the use of resources, demanding, rather than merely acquiescing in, the temporary subordination of private initiative to systematic regulation. The framing and execution of a national plan, the continual readjusting of its parts to meet unforeseen developments,

Empirical Research and the Development of Economic Science

call for a galaxy of talents, among which those of economists have a respectable rank.¹

But just what do economists contribute to a nation's war effort that is not expected from lawyers, engineers, accountants, or business men? Mobilization of resources means the use of the nation's labor, technical skills, industrial equipment, land, mines, financial system, and organizing talent to produce the goods required for victory in the optimum combination of kinds and quantities within specified periods, and to distribute the enormous costs of this vast effort in a manner compatible with current notions of fairness. Economists know less about almost all details of this job than do men of other crafts. The kinds of commodities and services required for making war are determined by military, naval, and aeronautical experts. What is needed to maintain civilian health and morale is better known to physiologists, psychologists, sociologists, and perhaps politicians. How to produce the goods required by the armed forces and civilians is a problem for farmers, engineers, and business organizers. How to adjust the new rules of cooperation to our wonted scheme of institutions in an orderly fashion is a task for lawyers. Bankers must handle the placing of loans and the hugely swollen stream of payments flowing from heavy borrowings, taxes, and governmental disbursements. But I need not go on listing the numberless demands of war that can be supplied best by men of other skills. So far as I can see, there is just one job our discipline has fitted us to perform better than any other group: determining how different elements in the economic mobilization should be adjusted to one another.

We have learned—perhaps it is our greatest achievement—to envisage the economy as a whole composed of many parts, each

¹ There is a spice of paradox in this wartime reliance upon a profession whose members have usually been critical of governmental interference with private affairs. But the paradox is superficial. The argument for *laissez faire* rested on the assumption that economic activities are undertaken to satisfy the multiform desires of the entire population. Every individual was supposed to be the best judge of what he wants, and of how best to use his resources. Alter the hierarchy of values by assuming that most citizens rank the attaining of victory first and it becomes logical to entrust management to a common agency under the direction of officials chosen by majority vote.

of which influences and is influenced by every other part. Walras' system of simultaneous equations was one method of presenting this concept. Marshall's motto, "The many in the one, and the one in the many", puts the essential notion into words. Our detailed work, whether speculative or empirical, has been to solve problems of many variables, reducing them to a form susceptible of analysis. For example, the supply and demand framework for explaining prices embraces ideally all the factors affecting the quantities of goods that traders in a given market, during a given period, will be willing to sell at different prices, and all the factors influencing the quantities buyers are willing to acquire at different prices. Prices we conceive to constitute a system of interrelated items, even when competition is far from perfect. The flow of money payments is to us a circuit flow of disbursements by enterprises for the agents of production and of spendings by consumers for the goods they have helped to make—a flow complicated by various eddies. When some change in existing arrangements is proposed, our minds fasten immediately upon the effects this change will have upon other factors, directly or indirectly, immediately or after a time; we think also about how these consequences will react upon the initial change. To officials charged with the job of mobilizing the resources of a country for war, men with this habit of mind are useful aides. Obvious as the concept of the interdependence of all economic activities seems to us, it is not part of the working equipment of many lawyers, business men, or engineers, if the able and patriotic dollar-a-year men I have collaborated with are a fair sample.

But an economist plunged into war work meets unaccustomed requirements. First, instead of working at problems of his own choosing, he works on problems assigned him by others. His intellectual initiative, like the business initiative of an enterpriser, must be subordinated to the nation's program. Second, almost all the assignments given him call for quantitative treatment, and close attention to the time factor. 'How much?' 'How soon?' are essential questions that must be answered, roughly at

Empirical Research and the Development of Economic Science

worst, the more precisely the better. Third, almost all the jobs involve forecasting. That hazardous duty cannot be discharged by merely extrapolating curves. The economist must reason things out, and do his reasoning on the disturbing assumption that other things will *not* remain the same. He must try to determine the most important developments in process or in prospect that will influence the factor with which he is concerned, and face the complications they introduce into his problem. Logical thinking is quite as essential as good estimating. In short, he must blend empirical with speculative procedures. Finally, the economist as warrior must assume responsibility for being right or wrong regarding facts. His quantitative estimates must be based upon the best information available, and he must be ready to meet vigorous criticism of his figures by interested parties. His theorizing cannot be confined to a statement of 'tendencies' that may be overborne by factors impounded in *ceteris paribus*; it must carry through to conclusions about what will happen under actual conditions. The serene aloofness with which he has been wont to point out proper courses of action to others, and to pass judgment on their errors, gives place to a hurlyburly in which he is attempting to form sensible decisions, and is getting blamed for his own errors.

The end of active fighting on a world scale has reduced governmental demands for the services of economists, but increased the demand for their services as teachers and employees of business enterprises. Yet we are not returning to the situation as it stood before the war. More carefully than before must a bitterly impoverished world husband its inadequate resources. The nations that are relatively well off are sharing with the nations whose needs are direst. While instituting relief measures we hope will be temporary, we are striving to develop a world economy that will fitly complement and support world political organization. The United States, perhaps in greater degree than other democracies, faces novel problems of world trade and finance.

A yet heavier load will be imposed upon the federal authorities if the American people insist that government assume responsibility for maintaining employment at high levels. So far as I can see, there is slight prospect that the governmental need of economists will shrink to the level of New Deal days, let alone the level of the New Era that collapsed in 1929.

Even as academicians, economists will not revert wholly to their old ways. Many who have shared in the heady responsibilities of helping to shape events will not again seek the aloofness they may once have cherished. The practice of thinking realistically, of trying to measure leading factors, of seeking to forecast and often to shape the course of events, will not be lightly abandoned. The sense of responsibility for seeing that conclusions have a demonstrable relation to actual problems will not evaporate. Economic theory, I fervently hope, will not be neglected; but more vigorous efforts will be made to test the assumptions on which reasoning proceeds, or the conclusions it reaches, or both, for conformity to the conditions we need to understand. Empirical workers in turn must have learned from recent experience that they cannot get significant results if they rely upon fuzzy concepts. There is better prospect than before that men who think of themselves as theorists will absorb into their work the methods and findings of realistically minded investigators, while the latter will make such free use of the concepts and procedures of theorists that no one will know on which side of the old line of demarcation he stands. In fine, the years near at hand may see the beginnings of an economics worthy to be called a science.

Rapid progress toward that goal is not to be expected. The great drawbacks of empirical research in comparison with speculative reasoning are that it is much more laborious, slower, and more dependent on financial support. The speculative reasoner must think hard; his is no easy task. But if gifted with logical acumen, he can select a set of assumptions interesting to him, and think

Empirical Research and the Development of Economic Science

out their implications by himself. If he has a funded income, or earns a living salary by work that does not exhaust his energy, he can get on without financial grants. In a relatively short time he can cover much ground, and, barring logical errors, arrive at conclusions incontestably true in the sense that they are necessary consequences of his assumptions. The empirical investigator, on the contrary, requires mass observations and considerable intimacy with actual practices; to extract the meaning from his data he needs assistants whose salaries few scholars can pay out of their own pockets. Seldom can empirical researches of moment in our field be quickly completed; they have a way of growing on one's hands, so that the most experienced workers usually underestimate the time and expense entailed by fresh undertakings. Special techniques must be employed and new ones invented on occasion. The job is not confined to the plodding accumulation and arrangement of data—though there is much of that tedious labor to be performed; there is also as much exact thinking to be done as in abstract theorizing. No certain conclusions can be expected; empirical findings are always surrounded by margins of uncertainty. Nor are the findings applicable to all times and all places; they are historically and geographically conditioned. What makes them worth striving for despite their heavy cost is that they consist of warranted statements about the world in which men have to live—statements that can be tested by others, cumulatively improved, and applied to practice.

However, the laboriousness and uncertainties of empirical research are not the basic reasons why it flourished less among our predecessors than a speculative type of theorizing. In desire to understand what actually happens, in zeal to make the world a better place to live in, no modern surpasses Adam Smith, Bentham, Malthus, Ricardo, Mill, or Marshall. But none of these masters had the opportunities open to us. Adam Smith was justified in putting little faith in the 'political arithmetic' of his day. Malthus was a born realist, and made effective use of such data as he could assemble; but they were sadly inadequate for treat-

ing many of his problems. Ricardo was an acute observer within the narrow range of his own business, but had no facilities for exploring the wider realms of farming, manufacturing, and trading in commodities. In his *Logic*, Mill emphasized "that indispensable element in all deductive sciences, Verification by Specific Experience—comparison between the conclusions of reasoning and the results of observation".² Yet Mill had neither the data nor the techniques required for following his own injunctions when he composed his *Political Economy*. Marshall, coming later, was more fortunately circumstanced. He was a good observer, a great reader of blue books, and used his wide information to excellent effect in his *Industry and Trade*. While he "felt the necessity for quantitative analysis" in economics, he devoted his *Principles* to bettering qualitative analysis because he believed that the "higher and more difficult task must wait upon the slow growth of thorough realistic statistics".³ In using the 'realistic statistics' that have grown not so slowly during the forty years since Marshall wrote the later of the two essays from which I have quoted phrases, we are following the course he pointed out—surely not a flagrant breach of continuity.

I shall assume without trying to demonstrate that empirical research has made enough progress within the last generation to justify more extensive and more intensive efforts in the future. Happily, the workers and even the agencies, public and private, in this and other countries jointly responsible for what has been accomplished are too numerous to list on this occasion. Nor could I without long preparation and undue imposition on your patience describe the leading achievements. Let me keep to my promise and speak rather of the work that lies before us—not so much the contributions we may make toward the solution of practical problems as what we can do toward developing economic science. The greatest service we can render mankind is to

² J. S. Mill, *A System of Logic* (7th ed., London, 1868), II, p. 500.

³ *Memorials of Alfred Marshall*, edited by A. C. Pigou (Macmillan, London, 1925), pp. 301, 324.

Empirical Research and the Development of Economic Science

gain deeper insight into human behavior and the workings of economic organization. If we can progress in these fundamentals, our findings will apply to practical problems beyond counting.

As a guide, I shall use Marshall's concept of the 'national dividend', which sums up most of the factors that concern an economist. To recall his familiar words:

The net aggregate of all the commodities produced is itself the true source from which flow the demand prices for all these commodities, and therefore for the agents of production used in making them. Or, to put the same thing in another way, this national dividend is at once the aggregate net product of, and the sole source of payment for, all the agents of production within the country: it is divided up into earnings of labour; interest of capital; and lastly the producer's surplus, or rent, of land and of other differential advantages for production. It constitutes the whole of them, and the whole of it is distributed among them; and the larger it is, the larger, other things being equal, will be the share of each of them.⁴

Empirical research has vastly increased the usefulness of Marshall's concept by producing reliable measurements of the flow he defined, of the contributions coming from different industrial sources, of the chief uses to which goods are put, of the several types of income, of the number of individuals and families receiving incomes of different sizes. Research has shown that different definitions of national income, and therefore different figures, are needed for different purposes. It has gone some distance toward finding how the concept must be adjusted to the institutions prevailing in a given country at a given time. It has supplemented the figures of net national income by the scarcely less valuable estimates of gross product. Specialists in the field have developed a technical jargon of their own, which the rest of us find hard to understand. In several countries, governmental agencies have taken over the work of preparing current estimates, which are kept close to date, based in part on short time units, and made available to all. The many uses to which these mate-

⁴ *Principles of Economics* (6th ed., Macmillan, London, 1910), p. 536.

rials have been applied demonstrate the wide esteem in which they are held.

But, markedly as this work has flourished, no one is satisfied to leave matters as they stand. None of the figures is exempt from criticism; especially unsatisfactory are the distributions of income by size, and the international comparisons of average income per capita bold spirits insist upon making. Beyond the job of improving the accuracy and range of the estimates themselves lies the job of understanding the interrelations among the components of the total. The possession of figures for successive years incites us, and aids us, to conceive these problems in dynamic terms. Quantifying Marshall's concept has become a stimulus to systematic thought on problems old and new. The concept ties these problems together, because it presents production and distribution as two aspects of the same process, in which pricing plays a crucial role. When dealing with any factor in gross product or national income we can grasp its bearing upon other factors over time, and their bearing upon it.

Thus Marshall's great service in integrating economic theory promises to become an integration of the quantitative work he foresaw for the future. Just as his integration was much more than a piecing together of ideas developed by others, so the empirical researches before us will entail much more than a mere assembling of monographs upon different items of national income. We shall try to determine how these items influence one another, what types of movement occur in the totals, and how all these movements—from secular trends to seasonal variations—come about. For these analytic tasks, estimates covering appreciable periods in ever growing detail, and ever more searching methods of ascertaining the relations among time series will be invaluable. We shall find ourselves inquiring closely into the operations our data purport to represent, and so becoming better acquainted with the activities we are trying to explain. Without spending much time on discussions of methodological issues at large, we shall steadily improve the blending of quantitative

Empirical Research and the Development of Economic Science

with qualitative analysis we practiced under difficult conditions during the war.

Among the problems treated in this fashion, first place may be given to the factors that limit national income reckoned in constant prices. In the days of the classical economists, the most important limiting factor was sometimes assumed to be scarcity of land and sometimes scarcity of capital—the latter a view still lingering in wide circles of laymen and narrow circles of economists. Later it was frequently asserted that maldistribution of wealth and income is the basic cause of poverty. Defective as they are, recent estimates of the distribution of income by size show exceedingly great disparities, but division of the totals by the number of families indicates that a leveling of incomes would not yield a satisfactory standard of living—unless this redistribution greatly increased production, instead of reducing it, as critics have argued it must inevitably do. Lord Keynes gave our interests a fresh, and, I venture to say, a more promising direction by centering attention upon the employment of the resources at hand. This orientation puts business cycles high on our agenda, which may bias my valuation of the Keynesian approach. That it meets the intellectual and practical needs of the times is proved by the extraordinary welcome given it by economists and the popular acceptance of a high and stable level of employment as the goal of economic policy, both private and public. Inquiries into ways and means of achieving this end are being energetically pushed, and they promise to expand in the near future. So seriously do we regard the issue that many believe the future of our current scheme of institutions, political and social as well as economic, hangs upon the success of private enterprise in providing 'jobs for all', all the time.

One of our tasks is searching study of resources themselves, how to conserve and how to increase them. Our predecessors seem to have thought of the resources provided by nature as imposing a presently flexible but ultimately fixed limit upon what men can

produce. We shall come to think even of natural resources as cultural products. Are they not that to all intents? The aboriginal inhabitants of this continent north of Mexico had little farm land, virtually no coal, no metal beyond bits of virgin copper, no petroleum, no electric power, no plastics. European settlers brought some of these resources with them in the form of knowledge; their descendents have invented the rest. Atomic energy is merely the latest, the most threatening, and the most promising of these discoveries. Science is beyond all comparison the greatest of resources. In trying to make economics into a genuine science, we are striving to increase the resources at the disposal of our kind. Is it not high time that we recognized this dynamic feature of our culture, ceased looking forward to a stationary state or a 'mature economy', and adopted the constructive view that our institutions must be adjusted to employ the increasing resources science has been creating decade by decade for several centuries, and never so rapidly as in our own days?

These wider explorations will inevitably swing back to studies of ourselves. That is a delicate subject. Man has always been his own most baffling problem. Economists are far from complacent about what they have contributed toward an understanding of human nature. Yet we do not and cannot operate without concepts of human nature and how it functions. They are present, whether we recognize them or not, in all our statements about what men do, or should do. In war work we were continually relying on more or less well founded ideas about how people would react to the policies we were helping to shape. All our peacetime planning, whether for a system of individual initiative or governmental regulations, assumes, tacitly or explicitly, the prevalence of certain behavior traits. Keynes' concepts of the 'propensity to consume' and 'liquidity preference', so confidently invoked today, are as patently psychological as Adam Smith's 'propensity to truck, barter and exchange', or Bentham's 'felicific calculus', or Malthus' 'instinct of procreation', or Bagehot's 'cake of custom', or the Austrians' 'marginal utility', or Edgeworth's 'indif-

Empirical Research and the Development of Economic Science

ference curves', or Veblen's 'cultural incidence of the machine process', or Schumpeter's distinction between 'routineers' and 'innovators', or Pigou's epidemics of 'over-optimism' and 'over-pessimism', or Freud's 'complexes' hiding in the 'subconscious'. The 'economic man' was a psychological caricature, deliberately drawn to facilitate speculation, but condemning it in advance to be a sketch of how creatures very differently endowed than human beings would behave. Whenever we explain what men do solely in terms of shrewd calculations of economic costs and gains we are in effect reviving the psychology of that lay figure.

The effort to understand what actually happens will compel our profession to examine far more critically than before whatever ideas about human nature it uses. In that task also the growing abundance of mass observations and the more powerful methods of inductive inference at our disposal give us an advantage over our predecessors. We shall not have to rely so heavily as they did upon introspection; in much larger measure we shall be able to test our working hypotheses objectively. Basic observations upon the functioning of the body-mind are being provided and interpreted by physiologists and psychologists. Economists who wish to prepare themselves for fundamental inquiries can get invaluable help from this quarter, provided they undergo the necessary training. More commonplace materials are being provided by 'big business' and government. The large-scale enterprises typical of our times devote enormous sums to sales promotion. Many of their marketing campaigns are skillfully planned, pushed with vigor; the results are systematically recorded, and carefully studied. The same concern often experiments with different plans in two groups of markets believed to be representative samples. From an economist's viewpoint, this vast and varied effort is an attempt to influence the valuation processes of potential customers. When a laboratory scientist finds out how a process can be changed, he is well on his way toward finding explanations. By analyzing the records of relative success and failure in selling goods, competent investigators

should be able to learn much of scientific as well as practical interest about mass responses to different types of appeal. Is it too much to hope that these mines of information will be opened to exploitation?

Similarly, personnel managers of large enterprises are experimenting systematically with devices for determining the varying aptitudes of employees, the social and physical working conditions that lower or heighten efficiency, the effects of incentive payment schemes, and so on. Some of this experimenting is now, and more of it will be, designed expertly to yield significant conclusions about group reactions. Here is another growing body of data keenly interesting to economists who are not content to assume merely that labor is irksome.

As for government, it is supplying ever more and richer observations upon economic behavior. Our predecessors made relatively little use of the 'laws' of expenditure Engel formulated in the 1850's; the more realistic orientation of our successors will lead them to delve deeply into the better family budgets at their disposal. They will try to determine the relations between standards of living and efficiency, the competing appeals of high wage rates and security to wage earners, how people modify their patterns of spending and of saving as their incomes change. One of the great problems of the future promises to be how people will use the larger leisure that seems coming—a matter that will affect profoundly the kinds of goods demanded and produced, the places where people live, the avocations they take up, and I know not what else.

This list of opportunities for empirical research in human nature might be extended much further, but I fear that to some minds it has already strayed far beyond the limits of economics. That is true of economics as often conceived; it is not true, I think, of the economic science mankind needs. The effort to account for per capita differences among real incomes in different countries, and for per capita variations in single countries, will center attention upon men—men as producers, men as inventors

Empirical Research and the Development of Economic Science

of resources, men as consumers, men as organizers, and men as legislators. All the factors that influence efficiency in making goods, and all the factors that influence consumers' demand will be pertinent to these inquiries. As during the war, economic advisers of business enterprises and governments will be asked for more than general formulas; they will deal with definite quantities of specific goods within specific areas and specific periods; they must offer forecasts; they will be held responsible for errors. In this work they will be collaborating with men of other disciplines, absorbing what these colleagues contribute to knowledge of human behavior, and making contributions of their own.

By no means all economists will be drawn into operating tasks, private and public. Many will still be teaching, and I hope an increasing number will be enrolled in research bureaus independent of any operating agency. These two groups will be relatively free to follow their own interests. To them we may look for systematic work directed toward the increase of knowledge at large, rather than the solution of immediate problems. They will provide the general framework of ideas within which each detailed investigation finds its relations to other explorations. Empirical contributions cumulate into a science only as they are organized, and men immersed in the details of one practical issue after another have little opportunity to consider the bearings of their work upon that of others. But even the groups free to think about the economic system as a whole will share in the drive to test their assumptions and hypotheses for conformity to fact. Sufficiently aloof from the work-a-day world to view it objectively, they will still wish to understand what happens there. They, too, will be held responsible for meeting scientific standards regarding adequacy of evidence as well as logical rigor.

It is, indeed, an enticing prospect that opens before economists—much exacting labor and heavy responsibilities, but excellent materials, powerful tools, tasks to suit many talents and tempera-

WESLEY C. MITCHELL

ments. We may well envy the youngsters now beginning careers of research. They will learn from our mistakes as well as our successes, and lay plans of their own better than we can formulate. What they will do with their opportunities is for them to determine; but, while we celebrate one anniversary, we look forward to another at which their achievements and hopes will be discussed. As cordially as I welcome you today on the National Bureau's behalf, let me invite every one present to return in 1970 for another glance at the past and further planning for the future.