RESEARCH AND DEVELOPMENT OF HUMAN RESOURCES

28 October 1953

853

CONTENTS

Publication No. L54-50

INDUSTRIAL COLLEGE OF THE ARMED FORCES

Washington, D. C.

Dr. Eli Ginzberg, Professor of Economics, Graduate School of Business, Columbia University, was born in New York City. He received his A.B. degree in 1931, A.M. in 1932, and Ph.D. in 1933 at Columbia University and was Cutting traveling fellow, Columbia, 1933-34; director of research, economics, and group behavior, 1939-42, research on occupational choice, 1948-49, and director of the conservation of human resources, 1950 to date. In 1941 he was director of research, United Jewish Appeal; consultant Executive Office of the President, 1942; consultant on group advancement of psychiatry, 1946 -; director, State Hospital Study, N. Y., 1948-49. He was a member of the Committee of Wartime Requirements for Scientific and Specialized Personnel in 1942; medical consultant, Hoover Commission, 1946-48; adviser, Committee on Chronic Illness, 1950 -; director of research, National Manpower Council 1951 -; U. S. delegate on reparations for nonrepatriable refugees, Five Power Conference, 1946. During the years 1942-44, he was special assistant to the chief statistician, U. S. Department of War; to the director of Hospital Division, Surgeon General Office, 1944; director of Resources Analysis Division, 1944-46; Department consultant, U. S. Army, 1946 -; member of the Medical Advisory Board, U. S. Secretary of War, 1946-48. He is also a member of the Economics Association; Academy of Political Science; American Friends of Hebrew Universities (director); and A.A.A.S. He has published several books in the fields of economics and human resources. This is his first lecture at the Industrial College of the Armed Forces.

RESEARCH AND DEVELOPMENT OF HUMAN RESOURCES

28 October 1953

COLONEL PRICE: Admiral Hague, gentlemen: In a recent lecture on manpower resources the speaker referred to the two reports of the National Manpower Council that have been circulating among the class during the last few weeks--"Student Deferment and National Manpower Policy" and "A Policy for Scientific and Professional Manpower"--and to another book, "The Uneducated," by Dr. Eli Ginzberg and Dr. Douglas W. Bray. Many of you have seen this book. In fact, it is dedicated to Major General Howard McC. Snyder who is with us as a guest.

We are fortunate indeed this morning in having with us to discuss "Research and Development of Human Resources" Dr. Eli Ginzberg, Professor of Economics, Director of the Conservation of Human Resources project at Columbia University, and Director of research for the National Manpower Council. Dr. Ginzberg.

DR. GINZBERG: Admiral Hague, gentlemen: I do not know whether I am in fact an authority on human resources, but I think it is fair to say I am an authority on one resource--myself--and I warn you not to be impressed by the flattering introduction of Colonel Price.

When I received Colonel Van Way's invitation to speak, I was at a Swiss mountain resort writing memoranda and recovering from a trip to the Near East for the State Department and the Army. A request to talk to you on a subject that I supposedly knew much about appeared attractive and I accepted. Colonel Van Way wrote that he was enclosing a list of the subjects he wanted me to cover, but my secretary, who forwarded his letter, failed to include the outline. When I finally saw it, I decided that had it not been for the high altitude in Switzerland, I would never have accepted. This is the recommended scope of my talk: trends in social science research which have a direct bearing on military manpower matters, evaluation of the results of such social science research, and an estimate of the prospects of social science research both in and out of universities.

It is clearly easier to make such an assignment than to meet it. I will therefore ask your indulgence.

I have lectured to the armed services often enough to know that the payoff comes not in the first hour of formal presentation but in the question period which follows. The main purpose of my remarks, which I will group under three major headings, is to stimulate discussion in the question period.

I want to tell you, first, something about the environment of social science research; second, I will describe four areas in which social scientists are working and which I believe should have some relation to your interests; third, I will rise to the bait and indicate some of my appraisals of how social science research can contribute in the future to the solution of military manpower problems.

What are the central features of the environment of social science research? Roughly, "social science" includes the fields of anthropology, economics, political science, and the not very well defined areas of social psychology and sociology.

The term "research" is more difficult to define. In reviewing my preparation for this lecture this morning, I noted: Social science research is a systematic method for increasing understanding of the way in which men live in groups. This may not be very useful, but at least it provides a point of departure.

Who engages in research and how? I think, by and large, that a true researcher is a man who has made a commitment to himself to devote his working life, his career, to increasing knowledge. In our society such a career is usually associated with university teaching. From my experience--and the whole of my life has been spent in a university environment--the best research people are those to whom research is central and teaching peripheral. A man's commitment to research must be for a long period, if not for life, because new knowledge does not come easily. True research cannot be a hit and run affair.

What are some of the characteristics of social science research, and how does it differ from what goes on in the physical or chemical laboratory? It is next to impossible to do research on social problems that involves certain kinds of direct experimentation on human beings, for one of the central concepts of life in the Western World is that human beings may not be controlled directly, and certainly not for experimental purposes. This is a major handicap to the social scientist.

From one viewpoint, everyone is a social scientist since everyone can observe and reflect on the actions of others. This attitude becomes quite clear when a Congressman says, "I know just as much about how people behave as you do, professor." In fact he may even know more. Any intelligent man can acquire a great deal of knowledge about how people act, and if he is at all reflective, he can reach important conclusions about human behavior. But many who lack experience and knowledge and many others who lack objectivity fall back on prejudice. Consequently, there is a galaxy of fixed opinion as to the important principles of human behavior. This is another characteristic of the environment in which social research operates.

There are two important approaches that, though related to research, in my opinion at least, are not research. A tremendous amount of effort devoted to accumulating data sometimes goes under the misnomer of research. The collection and tabulation of data--I do not care how valuable the information--is not research, but only a preliminary and essential step. A tremendous amount of effort goes into the collection of census material, but without analysis the figures add little to our understanding of the United States.

Another activity which is mistakenly called research is the construction of theoretical models of human behavior, frequently by people who have had good training in mathematics. A model may aid in the discovery of new knowledge, but model building, by itself, is not research. Models can prove themselves only when they are put to work to explain concrete situations. At this point, unfortunately, they frequently fail.

Approximately since the end of World War I, two major factors have been working to change the nature and the scale of social science research. Social scientists have become increasingly committed to studying problems empirically. A tremendous and growing amount of energy is being devoted to enlarging the factual bases of the several social disciplines.

Second, we have become increasingly impressed by the fact that the individual sciences I mentioned at the outset--economics, anthropology, political science, sociology, and the rest--are seldom able by themselves to illuminate significant aspects of human behavior. More and more research people have found it desirable, in fact essential, to undertake interdisciplinary research, involving the collaboration on the same problem of people with different training.

Because of the intensified effort to improve the factual bases of social science, and because of the recognition that no one individual controls all the knowledge and techniques required for significant results, social science research has become much more elaborate and difficult. At the same time, the promise of important returns has become much greater.

Social science research is formally financed by business, foundations, and Government. I will not deal with business sponsorship beyond saying that it is overwhelmingly concerned with supporting practical work, that is, the development of specific tests for the selection of personnel and similar techniques that appear to have a direct bearing on the operating problems of business. Some large companies are just beginning to underwrite social science research of a fundamental nature without worrying about whether the results will be immediately profitable to them.

The foundations have been the principal supporters of social science research. I looked the other day at the last annual report of the Rockefeller Foundation, which spends 4 million dollars a year for social science research. The Ford Foundation, which is primarily concerned with social science and education, spends about 25 million dollars annually.

As late as 1940 the Federal Government gave practically no support to social science research. Now, however, governmental funds going to the universities for research total about 350 million dollars a year. Most of this sum is spent for research in the natural sciences. Psychology and social psychology, according to the most recent estimates, receive about 11 million dollars. Expenditures on the other social sciences are no greater than 10 million dollars. The sum of 20 million dollars of Federal funds for social science research in American universities is probably a reasonable first approximation.

In addition, the Air Force, and to a lesser degree the Army and the Navy spend sizable amounts for developmental work in social science within the departments. Total governmental expenditures for social science are difficult to estimate because it is very hard to draw precise boundaries around social science, or to decide where research ends and experimentation with training devices begins. If all experimental work on training devices were included, 50 to 60 million dollars a year might be a first estimate of total governmental expenditures for social science.

Having described briefly the environment in which social science research proceeds, I want to talk next about a selected group of problems on which research is currently proceeding at several universities. Obviously, I must be very selective because of my limited knowledge as well as lack of time. What I have to present represents what my staff helped me to prepare. I divided the field and asked them to find out who is working on what specific problems and where?

I am going to talk about four problem areas: the manpower pool and problems of selection for the military service; classification, assignment, and training; leadership; and selected problems that go under the term group behavior, group morale, or group dynamics.

Let us begin with problems related to the manpower pool. At Princeton, Frank A. Notestein and his associates have been concentrating on problems in demography, including comparative studies of population changes. Notestein has studied the rate of expansion of the population of Russia and of Asia. Clearly, the rate of growth of European and Asiatic populations has an important bearing on the present and future military position of the United States.

At Columbia, Abram Jaffe has been working on manpower pool problems. Together with the Assistant Commissioner of Labor Statistics, Charles D. Stewart, he has written a good book on the labor force, "Manpower Resources and Utilization." At Johns Hopkins, Clarence Long has also been working on labor force problems. He has been particularly concerned with studying how periods of mobilization have altered the flow of people into and out of active employment.

National character is being investigated by a number of researchers, particularly at Harvard under Clyde Kluckholm, an anthropologist. One might call these studies a psychoanalysis of large groups. They are focussed on efforts to unravel the traditional values of various national groups and to estimate how members of the group are likely to respond to specific pressures. These investigations are concerned also with regional "subcultures" such as that of the Spanish Americans who live in Texas or of the Tennessee mountaineers.

These studies have some relevance to military personnel problems in that they can suggest different ways of handling inductees from various sections of the country. I am sure that many of our errors in World War II in dealing with psychiatric problems resulted from failure to appreciate what is typical or normal in various American subcultures. To a New York psychiatrist with a Park Avenue practice, some soldiers from the Southern Appalachians might well give the impression that they are suffering from borderline schizophrenia because of their slouched posture, unkempt appearance, and monosyllabic answers. From their viewpoint however, it is probably the fluent well-groomed psychiatrist who is abnormal.

R. B. Cattell of the University of Illinois has studied the distribution of selected characteristics of the American population and their bearing on mobilization. He has been especially concerned with the distribution of intelligence in the population because of the very heavy demand of the armed services and the civilian economy during mobilization for highly intelligent people. He has also been investigating the distribution of sensory and perceptual abilities, in which the Air Force and Navy have a distinct interest because of their need for people with aptitude for plane spotting and aerial photography.

Joseph Lohman, formerly of the University of Chicago and now, I believe, in Washington is working on another facet of the manpower pool. He has evaluated how criminals paroled from the penitentiaries during World War II actually performed in military life. Lohman's findings suggest that the several hundred thousand who went from jail into the armed services did just about as well as the average inductee. He points out that most released criminals never commit a second serious offense. Since those who were accepted by the military were carefully selected, it is not surprising that many felons did well in the services.

Norman Q. Brill, now at the University of California at Los Angeles, and Gilbert Beebe have been concerned with neurotic predisposition and its bearing on selection for and service in the Armed Forces. Their careful studies challenge the assumption that a man who exhibits neurotic symptoms in civilian life will not make a good soldier.

The second problem area I have selected for review is "classification, assignment, and training." John C. Flanagan, who was in charge of Air Force psychological studies during World War II and is now working in Pittsburgh, has specialized in test construction, in the hope of developing a limited number of tests which can be used to assign men to one of the large number of military occupations. His goal is 50 good tests, each one of which will really distinguish those persons who possess the critical abilities required for various jobs. Flanagan insists that a major weakness in test construction has been the lack of a basis for evaluating the performance of the persons tested. Only if one understands the key aspects of a job can one develop sound tests. His critical incident technique for evaluating performance is based on description of specific behavior in instances of very good or very poor performance.

At Yale University, the center of formal learning theory in the United States, the pioneering work of Clark Hull is now being carried on under the leadership of Neil Miller. Yale investigators have been working with rats, but they have applied many of their conclusions to human beings. A symposium on the relationship between learning theory and the military has been published recently under the auspices of the Research and Development Board, Department of Defense. Miller, a contributor to the symposium, made the point that it is important to differentiate motivation during the learning stage from motivation in performance. Many people who do poorly in training may eventually do quite well when forced to perform.

The Narkel Foundation has sponsored exploratory studies on the nonintellectual elements of achievement. Although intelligence is very important for preliminary classification, other qualities help to determine an individual's level of performance in a particular assignment. This research is aimed at discovering what these qualities are.

The third problem area is leadership. The most ambitious studies of leadership are being conducted under the direction of Carroll Shartle and Ralph Stogdill at Ohio State University. They plan to spend 10 years studying the types of people who make good leaders, the best means of selecting them, the characteristics of good leadership performance, and similar questions.

The initial work at Ohio State on leadership has been focussed on the different kinds of leadership functions. Even within the military, leaders are required to perform a wide range of different types of work. The Ohio group is also investigating differences in the degree of responsibility carried by individuals, the ways in which different leaders operate, and the characteristics of leadership groups. Their approach is heavily statistical; one of their reports refers to a series of tables containing 15,000 correlation coefficients. Impressed with the complexity of the leadership problem, the group is placing heavy reliance on calculating machines.

Fillmore Sanford, Executive Secretary of the American Psychological Association, has prepared an excellent article reviewing research on the psychology of military leadership. Sanford concludes that no one really knows how to pick military leaders. He maintains that ratings of junior officers by their superiors are not an accurate measure of how the junior officers will perform the functions of a military leader. He points out that the situations that leaders have to meet often change rapidly. Consequently, fixed principles governing how a leader should behave involve the danger of failing to recognize the need for flexibility in the leader's role. Sanford finds that most of the research on military leadership leaves much to be desired.

W. E. Henry of the University of Chicago has studied business executives through the use of modern projective techniques in an effort to learn about the emotional forces in the lives of executives. As far as I can judge from the work to date, the use of these elaborate techniques has not yet added significantly to our understanding of how to determine the presence of particular personality characteristics in business leaders or of the characteristics that a leader should possess.

The fourth area of research that I wish to describe briefly is group behavior or group dynamics. Probably the largest center of empirical research in this area is the Survey Research Center at the University of Michigan, directed by Rensis Likert, formerly of the Department of Agriculture. The Michigan group has been liberally supported by grants from the Navy.

The Michigan investigators have directed much of their attention to the influence of different supervisory patterns on group productivity. They believe that the quality of the interpersonal relations between the supervisor and the group is more important in determining productivity than pay, job security, and related matters. They have stressed group participation in the determination of work patterns on the theory that greater involvement leads to higher output.

Another ambitious long-range study of group behavior is directed by E. Wight Bakke, at the Yale University Labor-Management Center.

Bakke has set himself the task of redefining the major principles of human behavior. His ultimate goal is to develop a systematic framework for studying the whole of human social behavior. Like many other social scientists, Bakke seems to be devoting much of his effort to substituting for concepts in current usage complex terms that have been coined for the occasion. In former days economists said that labor was concerned with wages. In the new parlance one finds an elaborate discussion of reward systems. We used to talk about administration; now we talk about communication.

At the Massachusetts Institute of Technology, Alex Bavelas has been working to analyze the effectiveness of simple models of communications patterns. He is concerned, for instance, with understanding what happens in a straight-line communication setup, for in modern large-scale organizations one determinant of efficiency is the way in which orders are transmitted downward and responses upward. This is clearly not a new problem in administration, but the development of mathematical techniques represents a new method of attack. The question remains, however, whether models are adequate for the study of the complexities of reality.

I want to call your attention also to the large laboratory of social relations at Harvard under Samuel Stouffer. The Harvard group is working on a large number of different problems, but little has been published as yet, and it is too early to estimate what will emerge. Among the investigations under way are studies of attitudes, values, social mobility, and the general theory of action.

I also want to mention the work going on under Donald MacKinnon at the University of California at Berkeley. This is an ambitious project concerned with discovering why some people are effective and happy while others are not. Their approach has been an elaborate reconstruction of the experiences of individuals from early childhood to adulthood. They began with a group of graduate students, and have sought to reconstruct their developmental experiences to discern why some are better adjusted than others.

I must also refer to the work of my colleagues at Columbia in the Bureau of Applied Social Research and to our own large-scale studies on the conservation of human resources, but for obvious reasons I prefer not to elaborate on them.

These examples indicate that there is considerable activity at Princeton, Johns Hopkins, Harvard, Illinois, Northwestern, Pittsburgh, Yale, Ohio State, Michigan State, Columbia, and many other institutions which I have passed by for lack of time or knowledge. I should emphasize that much of my presentation refers to projects that are still under way. It is much too early to appraise definitively most of this

work, but my own view is that much of it will fail to pay off. Many undertakings appear to have gotten off on the wrong foot and there is little chance that corrections can be made in time.

This is not surprising. The discovery of new knowledge is always very difficult. The number of able people in research is also severely limited. Undoubtedly, some of this work is going to prove valuable, but a great deal of wasted effort is inevitable.

The major weakness, in my opinion, grows out of the fact that investigators are trying to do too much with too little. The present level of knowledge in the several social sciences does not justify some of the very ambitious projects which are under way, such as the attempt to develop a comprehensive theory of human behavior. The preconditions for the development of such principles are surely not present now, nor are they likely to become available in the near future. Much hard work must first be completed.

As a result of deficiencies in fundamental knowledge, specific answers to limited questions are often very difficult to find. In the absence of solid knowledge about the factors leading to superior performance, including the influence of personality factors, investigations into how to pick better leaders are foredoomed to failure. We are caught on the horns of a dilemma. On one hand, it is difficult to find general principles because we lack basic knowledge; on the other hand, it is next to impossible to secure answers to important practical questions because prevailing theory is inadequate. This is not pleasant, but I believe that it is a correct appraisal of where we stand.

The situation is further complicated by the fact that we are spending a great deal of money in an attempt to force the issue, particularly to get quick practical answers to pressing problems. Since 1940 the Federal Government has become a major sponsor of social science research. Now large amounts of money are not necessarily a boon since they may lead only to forcing the wrong issues. It all depends on whether the recipient of the money is on the right track. If he is, you will advance. But whether he is or not, he gains prestige from the grant; he gains power by hiring more staff. If he is on the wrong track, the additional money will simply compound error.

It is understandable that the armed services desire help from the social sciences in improving their methods of selection, training, and leadership. It is harder to understand the optimistic belief that by spending large amounts of money the armed services will receive a great deal of valuable help very quickly.

A related problem grows out of the overemphasis in the United States on empirical research, much of which is not research at all but

the accumulation of huge bodies of data. This is not likely to be a rewarding operation while theory remains inadequate. But it does offer an investigator an escape. It is much easier to amass facts and figures than it is to ask what they mean. Kinsey is the perfect example. I estimate that he spent 98 percent of his time and energy collecting and tabulating his materials and 2 percent interpreting them.

The armed services have an additional problem in that they find it difficult to attract top people to direct their research programs. Consequently, outside consultants determine the expenditure of research funds, at the same time that they, their students, and their colleagues are the beneficiaries of the money.

These very negative comments are prompted by the belief that it is important to know what is not possible as well as what is. I do not mean to imply that the military can receive no benefit from social science, but I do wish to emphasize that the military depends on the state of knowledge in the society at large. There is not much the Armed Forces can do to find answers to selection, motivation and performance problems independent of the present level of general knowledge about such problems. Military personnel problems are as dependent upon basic social theory as the weapons development program is upon basic knowledge in mathematics and physics.

Therefore, the major interest of the military ought to be to raise the level of knowledge in the key sciences that can contribute to a better understanding of human behavior. Sound expansion can come only slowly. It is wrong to think that you can buy time, for there is no way of shortcutting the processes by which science advances. Scientific progress depends on a few good people, the inspired workers, and they are by definition very scarce indeed. In our country there are many competing outlets for able people. Relatively few go into research. The key bottleneck in social science is the limited number of first-rate research personnel.

Money can help if it is spent wisely. The granting agencies must be willing to commit themselves to long-term support of good people. The present system of annual contracts insures that at best the completed work will have to be fitted into a larger pattern. The essential need is to stimulate the pattern makers. They cannot produce within a year, or even two, or three. Research is a lifetime career. The researcher must be able to plan over a long period of time. He must be able to make mistakes and to backtrack in order to go ahead again. This is absolutely impossible under annual contracts. At present moreover all Government contracts are unduly restrictive and therefore wasteful of the research man's time and the Government's money.

Next, the granting agencies must be willing to take risks. They must tell the public, the Congress, the Chief of Staff: "If we play it safe, we will get nothing; if we take reasonable risks, we may get something." They must not depend on people who have established reputations, who are old and tired. They should try to find good people who have not yet exhausted themselves, who are not burned out. Moreover, the services should try to get a few top people to guide their social science programs for a period of years--people who have support from the military and who command the respect of civilians.

In conclusion I would like to call attention to two approaches that I think can contribute substantially to good results quickly rather than in the distant future. It is frequently overlooked that the services represent a unique laboratory where much action is strictly controlled and where detailed records are kept on great numbers of people. Now, if you have a laboratory and you have a wealth of records, you are in an excellent position to learn about people and their behavior. If the services can develop methods for studying systematically their own experience, they will be able to discover significant answers to many of their most pressing problems. The difficulties however are great. Such investigations will require good brains for the design and execution of the research and courage to face up to the findings.

My second suggestion is that research is not the only means whereby social scientists can help the military to solve its problems. I am convinced that if the services can interest a few good social scientists in their problems, help them to learn how the services operate, and build up a continuous relation with them, they will be able to appraise problems in terms of the most advanced theory that modern scholarship has produced. At present consultants are used poorly. Frequently they are not let in on the real problems of the services and frequently they do not know enough about the military organization to be of maximum help. But good consultants, properly used, can prove a great boon.

One final word: My skepticism about the value of larger and larger appropriations is not predicated on pessimism about the potential of social science research, but grows out of my deep conviction that significant progress will come slowly. Brains and integrity rather than money and machines hold the clue to future progress.

COLONEL PRICE: Dr. Ginzberg is now ready for your questions.

QUESTION: Many colleges rely pretty heavily on the scores students make on college board tests as a means of deciding whom they will take and whom they will not take. Many of them also require aptitude tests or placement tests. How effective are these tests proving to be in practice?

DR. GINZBERG: There are many ways of skinning a cat. If one inverted the age at which students qualify for college, one would find performance in college related to age at entrance--the younger the man, the better the student. High school performance is one of the best ways of predicting college ability, but it is difficult to apply because of the absence of a basis for comparing high schools. If one had a reasonable knowledge of both the high schools and the students' standing in these schools, it would generally not be necessary to have them take college board examinations. The major usefulness of such tests is to pick up the boy with good native intelligence who has had an undistinguished record in high school for any one of a number of reasons. Moreover, there are many types of tests. I would suspect that to have all college applicants write an extended theme might prove quite successful. Of course, it is important to remember that tests tell us something only about statistical averages; they can never show with certainty how a particular individual will perform.

QUESTION: Could you tell us what is being done to discover the behavior of people under atomic attack, what the results, or prospective results, are?

DR. GINZBERG: I have at best only a slight acquaintance with this area of research. I have seen several completed studies on the way in which people reacted in the two communities where the bomb was dropped. But Japan is not the United States, so that the findings have at best only a suggestive value. Perhaps the word research is out of place. I suppose what one might try to do is make a list of the major types of disruptions that are likely consequences of an atomic attack and outline some of the more obvious responses to such disruption.

QUESTION: Do you have any opinion as to the usefulness of the military services' systems of officer evaluation?

DR. GINZBERG: There is no question that the military services could not perform their missions without evaluating their officer personnel. Granting that evaluation is necessary, I am very definitely in favor of simple rather than complex schemes, for I do not believe that our instruments are strong enough to justify very involved methods of evaluation. I would be happy to be able to differentiate between very good officers, average officers, and poor officers.

I would like to suggest one pet idea, namely, that even in a complicated organization like the military, it is possible to rely to a much greater degree than most people believe on the principle of self-selection. This means that I would try to offer all officers a wide range of options involving schooling and assignments. I would place considerable weight on the willingness of a man to make a big investment in improving himself. In short, I would like to establish an environment

which would make it relatively easy for the aggressive and competent person to stand out over the lethargic and dull person by his own actions. I am not very impressed, to put it mildly, with the objectivity and validity of rating systems which are based primarily or exclusively on evaluations by a man's superior.

QUESTION: I wonder what progress has been made in some of the older European countries in the field of social science. It seems to me that they must have had considerable experience.

DR. GINZBERG: That is a good question. Europe has had one advantage. The relative poverty of European countries has protected them from making many of the mistakes wealth has led us to make. For instance, until recently the University of London had no empirical sociologist on its staff; my teacher Wesley Mitchell, introduced the first adding machine at Oxford in the early 1930's. You can decide for yourself whether the British were gainers or losers from this "backwardness."

There can be no question that European countries have shown strength in developing social theory, since they were almost automatically preempted by lack of funds from going into large-scale empirical research. More specifically, Germany has not been a really active research center since 1934, and although there are good men in France and Italy, the real strength of the social sciences in Europe in recent years has been in Scandinavia, England, and to a lesser degree, Holland. In this connection, I would like to call your attention to the very interesting appendix to Gunnar Myrdal's famous "American Dilemma," which outlines very succinctly the reactions of a preeminent European social scientist to the American research scene.

QUESTION: Doctor, you have by inference or otherwise given us a pretty good idea of what your opinion is with respect to the level of social knowledge in our universities and in military research programs. What is your opinion of the application of the relatively low level of knowledge in social science in the field of industry? We have in the past few months listened to numerous discussions of that particular subject from the same lectern that you are at now and we are inclined to conclude that in most industries the big companies have big industrial relations departments. They must be basing their activity on this same level of knowledge that you say has been created in the universities. Do you have any comment on that?

DR. GINZBERG: I hope I will not be accused of undue cynicism when I say that it is not only the armed services or Government that is able to waste money. Industry has the same capacity. One need only recall American industry's infatuation with psychological testing after World War I and its naive belief that such testing would solve many of its

most serious problems. Confronted with serious and to some degree insoluble problems on the labor front, industry is constantly looking for easy answers, or let us just say, answers. Hence, it tends to follow any promising lead. I would say that most of the leads have not been very good. If I were lecturing to an industry group on this subject, I would point out that, like the armed services, industry has tended to place too much reliance on gadgets, too little on basic knowledge.

QUESTION: Doctor, you made two statements: one that the armed services are putting so much money into social science research that the field has drawn in a lot of pseudo social scientists--at least you implied that; second, you made the statement that to get progress in this field you have to bring in new people; you cannot take established people because they have more or less atrophied. If both of these conclusions are true, the armed services can't be very well positioned in pursuing social science research and getting new people.

DR. GINZBERG: In general, I would say that whenever the amount of money available for research exceeds the amount that trained research people require, additional expenditures will prove wasteful. It is impossible to spend money effectively at a rate beyond that required to facilitate the investigations of competent people.

It is quite true that I do believe that the future of science always depends upon new people of ability entering the field and rising to the top to replace those who have become atrophied. One serious trouble is that when large research grants are made, they are usually given to those with established reputations, who are often past the point where they want to struggle very hard. The tendency for such a senior man is to parcel out the job among a group of youngsters. Frequently, what one buys is immaturity multiplied several times over, seasoned with a little wisdom and administrative competence. Incidentally, this holds also for the physical sciences. I discussed this problem in chapter V, "Research and Development," in "A Policy for Scientific and Professional Manpower," Columbia University Press, 1953.

QUESTION: You suggested we might use the military services as a laboratory to answer some of these questions. I would like to know how we could go about that and what we could hope to gain from it if we started at the lower level.

DR. GINZBERG: This gives me an opportunity to tell you a little about my own work dealing with poorly educated Americans, which was published in "The Uneducated," Columbia University Press, 1953. I have been around the Pentagon more than 10 years, and had become rather disgusted with the fact that prejudices and impressions had the same weight as knowledge and evidence in establishing policy concerning this group.

Some argued that the uneducated made good soldiers--not very many took this position; some said that they were fair soldiers; the majority argued that they were hopeless and that the services should not be bothered with them.

Since more than half a million uneducated men were taken into the services during World War II, I thought it was reasonable to try to find out whether they were good, fair, or poor soldiers. With the cooperation of the Adjutant General's Office and his Records Depot in St. Louis, we were able to sample the detailed military histories of a representative group of men, which, together with other data concerning their performance before and after their tour of duty, provided the required factual information for our analysis.

We have another major study under way relating to the almost one million men who had to be separated prematurely from the services during World War II for reasons of psychoneurosis or ineptitude. No one had really tried to come to grips in any adequate manner with the major problem of why so many men failed to perform effectively. Of course, it takes a strong research staff to design such a study, but the important point to note is that the armed services possess unique data.

QUESTION: How do you think we might utilize what we learn?

DR. GINZBERG: That is a good point. I think there is considerable doubletalk in the armed services, as well as in industry and elsewhere about research. All too frequently if the research findings are in conflict with prevailing prejudices and beliefs, the findings are put aside and nothing happens.

QUESTION: Have you had enough contact, Doctor, to review the career management system.

DR. GINZBERG: I would say that on the whole we were too little concerned with personnel management during World War II and that after the war, we may have gone too far in the opposite direction by accepting too elaborate and intricate systems. I am sure that it is important to introduce some rhyme and reason into the assignments and schooling of career personnel. On the other hand, it is my impression that the present career management program is too inflexible, so that when emergencies come up--and there are always emergencies--the whole plan is disregarded.

QUESTION: This is a very general question and all we can expect is a general answer, but do you feel that because of present-day easy living, higher wages, automobiles, and TV in every home, we are going into a quick degeneration in many sectors. You mentioned the 2 million

ineffectuals in the last war, unwilling to serve their country. Could you enlighten us on that situation?

DR. GINZBERG: I am not sure that I would read our history in quite the same manner as you have just done. As I recall what happened during the Civil War, a very large number of soldiers "melted away." I do not think I would like to make any generalizations about the character of the American population at any one time compared with another. All that I am willing to say is that there is apparently ample room for an improvement in our current standards of behavior. But this does not commit me to your position that we are heading downward.

It might be worthwhile to call your attention to a related problem. I believe that we are running into serious trouble in this country because of the "perversion" that is developing in our relative reward structure. When a barber in Baltimore can earn the same amount as an associate professor of physics at Johns Hopkins University--and I can multiply this example many times over--there is certain to be confusion among young people about what is important and significant in life.

The position of the military is a case in point. I am not one for arguing in favor of ever-larger defense budgets, but I do believe that we should reconsider the use which is made of the money that we have. Just as I would be in favor of fewer and better supported research projects, so I would be in favor of perhaps slightly smaller forces with men and officers properly housed, medical care and other important services provided for themselves and their families, and with enough income to lend basic prestige to their work.

COLONEL PRICE: Doctor Ginzberg, we all appreciate the stimulating discussion you have brought to us this morning.

DR. GINZBERG: It was nice to be here.

(10 Mar 1954--750)S/sgh