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RESEARCH: PURPOSE, METHODS, SPIRIT

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RESEARCH: PURPOSE, METHODS, SPIRIT

9 September 1955

DR. REICHLEY: This morning we are going to have a talk on research. Throughout the year you will work on both committee and individual problems as a very important part of your activities here at the College. We believe a talk on the purpose, methods, and spirit of research would be of value to you. Of course our lecturer needs no introduction; so I will just say, Louis Hunter.

DR. HUNTER: Captain McCaffree, Colonel Walsh, Miss Widger, Miss Heisey, and Gentlemen: The almost automatic reaction of the typical university professor to the typical War College or Industrial College lecture subject is: "What you have given as the subject of a lecture is really the title of a course." No doubt I'm laboring under a delusion in thinking that I can somehow get around the subject of research in thirty or forty minutes, but I'll have a try at it.

You will quickly discover that I'm really trying to do two different though related things in this talk. On the one hand, I shall try to present something of the philosophy or rationale of research. On the other, I shall throw out from time to time certain practical suggestions as to how to go about doing a research job of the modest type that will be employed in our course of study.

Now, neither philosophy nor suggestions on "how to do it" are, perhaps, very useful taken separately. They are needed in combination if you are to get the best results from your effort.

I shall discuss the subject of research under several headings, to wit: the nature and purpose of research; the methods of research; the mechanics of research; and just a few comments on the spirit of research at the end.

With an audience such as this, there is no need in spending time talking about the fundamental role of research and researchers, because as we all know well, these are involved in the conduct of every major activity in the modern world. For the terms "research" and "researchers" we can substitute, for most practical purposes, the terms "science" and "scientists," for research is the basic method of science and the chief tool of the scientist.

In the Western World especially, science has played a key role in the historical developments of the past several centuries. But, of course, it is during the past generation that the practical applications of scientific research have produced such extraordinary results in our way of life, as well as in our means to death. The concrete results of scientific research are obvious to all. The methods by which these results are obtained are understood by relatively few.

What I have said so far applies especially to research in the physical sciences--physics, chemistry, biology, and their many branches, subdivisions and combinations. So far as practical accomplishments are concerned, progress in the social or behavioral sciences has been much slower, and the accomplishments are much less impressive. Yet in respect to fundamental methods, spirit, and purpose, if not in materials and techniques, the physical sciences and the social sciences are very much the same. And, of course, it is chiefly within the field of the social sciences that the subject matter of our course in economic mobilization chiefly falls. Economics, political science, social psychology, public administration, history, geography, and related fields are the disciplines upon which our course of study rests. These are all social sciences.

So much for preliminaries. Now, just what is research? How do we do it? And why do we do it?

Let me stress at the outset that actually there is nothing very mysterious or highly technical about research as a process or method. It is not something that always requires long, elaborate and highly specialized training. A lot of people do research who are not, in the usual sense of the term, researchers or scientists. In fact, research is a common, every-day operation with most of us. We engage in it, we carry out research projects, as a more or less routine thing. We think nothing of it, and certainly are not likely to dignify what we do with the label of "research." And yet in its essentials it is research.

Let's take an example familiar to every one of us. We get up one fine morning, perhaps a little late, gulp down some juice and coffee, and hurry out to the car--in the garage or at the curb. We push the starter button. The engine turns over--and over--and over; but no rewarding series of explosions occurs to show that the ol' fireball, double V-8 or rocket motor has taken over.

Of course, we usually react at first in a very unscientific manner; we refuse to believe the evidence of our senses. We keep the starter going until the battery threatens to give out. Finally we reach the inevitable conclusion. We decide that the application of the starter and a rising temper gets nowhere. So reluctantly we fall back on our common sense, put on our white coats, get out the bunsen burner and microscope, so to speak, and we're off on our research project, which might be given the informal title: "What the hell's wrong with that engine?"

What do we do? We start out by checking the gas tank. Yes, plenty of gas. We check the carburetor, the spark plugs, and such other possible causes of engine failure as fall within the limits of our competence. Everything seems O.K. Then, of course, we chance to look down and see that we've forgotten to turn the ignition key. Our research is over. We turn the key, push the button, the engine starts, and away we go.

Or we may exhaust our technical resources without success and call in a garage mechanic, with his superior skill and equipment. He finds that the fuel line is blocked, the gas pump broken, or the distributor cracked. And, having located the cause, he takes the necessary corrective action.

A dozen or more other commonplace incidents of this kind will occur to you. The light bulb fails. In a more or less systematic fashion we check in turn the bulb itself, the switch, the cord, the plug, the fuse. Sooner or later, our investigation locates the cause and we take corrective action.

Now, in the process I've described in these two commonplace situations, we have the essential elements of research. As conducted by many of us, it may not be very skillful research, nor even very effective research beyond certain limits early reached. But it is research nonetheless.

Now, what are these essentials? What are these scientific methods?

In the first place, we start with a problem to be solved--a very practical and concrete problem in the instances I have cited; but in essentials no different from, say, the problem of harnessing atomic energy or developing a vaccine for polio.

The problem to be solved gives rise to a question; and a question is the starting point in all research. The basic question here may be stated in vulgar, not to say profane, terms: "Why won't the damn

engine start?" With perhaps the peripheral and rhetorical question added: "Why does the damn thing have to pick this time to break down?"

Incidentally, this characteristic reaction of the car owner--that is, virtually every one of us--is an example of what the professors describe as the anthropomorphic fallacy or the animistic delusion, something we all suffer from at times.

Actually, of course, we have to deal not simply with one question, but with a series of questions growing out of the general one with which we are dealing. "Why doesn't the engine start?" breaks down into: "Is there any gas?" "Is the battery dead?" "Is the ignition on?" and so on.

This brings us to the third step in the process of research: What are the facts, that is the facts bearing upon the problem or question in which we are interested. Frequently research is thought of as concerned entirely, or almost entirely, with digging up the facts. This is a misconception. You can dig up endless quantities of facts--doubtless facts that in themselves are very interesting and very curious, as, for example, in connection with crossword puzzles and quiz programs. And yet the process involved will have no real relation to research, that is to significant research. The facts we're interested in, in research are the facts which have a bearing (or which may have a bearing) on the problem or questions toward which the research investigation is directed.

Now, while digging up the facts is not the whole of research, it is the core of research, the "search" in research; and the validity of the end product hinges upon the accuracy of the facts obtained. If we're wrong on our facts, the investigation goes haywire. In the example of the automobile engine, suppose the gas gauge isn't working properly and reports the gas tank half full when it's actually empty. We will get nowhere fast in locating the source of trouble if we erroneously believe there is a supply of gas in the tank.

And, finally, having collected our facts and related them through analysis to our questions and to the basic problem, we check the facts by experiment. In the case of the engine, we correct the believed defect indicated by our investigation. We turn the ignition key and step on the starter. If the engine now starts properly, the correctness of our analysis and the accuracy of our research have been demonstrated.

The examples I have chosen, for their simplicity and familiarity, fall within the field of the physical sciences. The chief difference of

research in the social sciences, the field with which we are particularly concerned in our studies, is this: The final check by means of experiment is rarely practicable in the social as distinct from the physical sciences. Even totalitarian governments, with all their authority and power, are rarely in a position to do much experimenting with people in their behavior in groups.

Now, let's go back and examine a little more closely what I called the first, the initial, step in all research: the statement of the problem, which, as I have suggested, breaks down into a series of related questions. This is true of all research, whatever the field of interest, regardless of the character of the problem.

This is a very critical phase, in fact, the most critical phase, of any research project. The problem must be analyzed with great care. On the skill, on the accuracy of this analysis and the resulting statement of the questions to be answered, will depend in large measure the quality of the research, and the significance of the results. Ask trivial questions and you can get only trivial answers. Ask significant questions and you may get significant answers and results.

To have the problem clearly defined and the questions clearly stated is to have your research task half done.

Well, you possibly will not quarrel with this principle, but will reply: "Okay, but how do you know how to ask the right questions?" There is no easy answer to this one.

General intelligence is quite a help in solving the problem of asking the right questions. But we need more than this. We need more than general intelligence. Generally we need to have a fair general knowledge and understanding of a subject to be able to ask meaningful questions about matters relating to it. After all, much of the skill of the doctor, engineer, military man, mechanic, or any other expert consists in his ability to determine what the problem really is, and to raise the kind of questions which will get at the root of the difficulties represented by the problem. Yet, with intelligence and a little hard work one can usually supply something of the knowledge and understanding of a subject needed as a basis for asking meaningful questions. This is especially true of the social sciences in which we all qualify as experts on the amateur level.

Now, before leaving what I have called the first stage in any research, I would like to comment on two practical matters. My emphasis

on raising the right kind of questions may be a bit confusing in view of the rather common academic practice, here at the Industrial College as elsewhere, of stating a research project not in the form of a question or problem, but simply as a topic.

Very shortly you will be handed a list as long as your arm of topics for term papers, from which you may make selection, or if you prefer substitute a topic of your own contrivance. Here are a few samples of last year's term paper subjects: The Mobility of the Worker in Partial or Full Mobilization, U. S. Tax Policies and Foreign Mineral Resources, Recommended Guidance for Establishment of a Mobilization Reserve, Electric Power and Economic Mobilization, Subcontracting in Military Procurement. As you see, each one of these term paper problems is stated in the form of a topic. Very obviously, the student researcher, starting with a topic of this sort, is on his own. Since he is not provided with a problem, as a question explicitly stated as such, his first task is to find and define a problem or question within the limits of the topic as stated.

A final comment I have to make on this matter of defining the problem takes the form of a practical suggestion. Don't simply give the matter of the problem and the related questions brief and casual thought and then go on to the next step in research. Get it down in writing. Get it down quickly: "The central problem or problems in this topic is so-and-so or such-and-such." The questions to be answered are: "1, 2, 3." Think this matter of the problem over carefully and then put your ideas down on paper, answer briefly and crudely after you have thought it through. With most of us there is nothing so conducive to vague and hazy thinking, and research, as simply carrying ideas along in our heads without committing them to paper.

All right, then, the problem is defined, and the questions stated. Now for the facts essential to answering the questions and dealing with the problem. The heart of research is, after all, search. The great bulk of the pick-and-shovel work of research is in locating and digging out the pertinent facts.

The first question is, of course, where to look for the facts. The answer to this question requires considerable work and usually not a little ingenuity, imagination, and, of course, intelligence. There are some fairly routine approaches that will take one usually quite a way into the subject. Let's see what these are.

A fairly sage general assumption with most research topics is that you are not the first who has given attention to the subject. So you begin, you look first to see what has been done. You explore the existing literature, in the way of books, articles, monographs, reports, and the like.

Now, there are, as you know, a whole series of professions dedicated to the business of helping you in locating this material. There are librarians, cataloguers, bibliographers, indexers, and so on. There are not only bibliographies, but bibliographies of bibliographies. There are card catalogues and shelf lists. There are book reviews and book review digests.

Frequently the material you may be looking for will be in the form of articles in periodicals and newspapers. There are a variety of periodical indexes, general and special, at your disposal. Newspaper indexes are something else again. I know of only one, but it is a good and useful one. It is an index prepared for the leading newspaper in this country--the New York Times.

However, it's not my purpose here to take you by the hand and lead you through the great mass of bibliographic aids. I simply remind you that such aids exist, for the assistance of the researcher. And, of course, libraries and library staffs exist for the purpose of assisting the reader and researcher to find the materials that they are looking for among all kinds of published material. But, while library staffs, such as ours, can do and will do much to help a researcher in locating sources of published materials, the responsibility for the research rests squarely with the researcher. He can't pass the buck on to someone else.

The first step in research then, is to search for the materials that have already been published. You seek and find out what has already been done. The existing literature, in the way of books, monographs, reports, articles, and so forth, are usually referred to as secondary sources. Of course, the amount of work that this step requires depends on how familiar you are already with the field. The professional in the field--the economist, the historian, the political scientist, and so on, in the case of the social sciences--is likely to know pretty well at the outset of his research just what literature is already available or, at least, he will know where to look for it. The amateur will probably depend largely, if not entirely, upon the existing literature for his raw materials, his facts; and locating this literature may take quite a bit of time and effort. The amateur mostly picks the brains of the existing authorities, which usually means the professional experts who have written in the field.

But whenever it is feasible, whenever time and resources and skill permit, the amateur should supplement, check, and add to what others have written on the subject; and go directly himself to what are called primary sources. This isn't very feasible in many cases, as in the research required for term papers. But when you get into the more elaborate research projects, such for example, as M.A. and Ph.D. theses, it is essential to go, in part at least, to the primary sources, when these are available.

What are these "primary sources"? Take a street fight or brawl. A newspaper account of a street fight is a secondary account--the report of a "third party" of what took place. To get to primary sources, one would have to go to actual participants in, or direct witnesses of, the affair.

I think you can see that the distinction between secondary and primary sources is by and large simply the distinction between second-hand and first-hand material. It doesn't follow that the researcher must always himself go out and dig up and collect the primary data, by interviews with people who have direct personal knowledge of the materials in question, or by circulating questionnaires. In a wide variety of fields, great quantities of primary data in statistical form are available to the researcher--in government reports and publications, and in statistical and other reports of a variety of private agencies. There are also autobiographies, letters, published or unpublished, court records, and so on.

I think you can see that every subject of research, every problem of research, presents its own difficulties and its own problems of locating sources and finding facts. As suggested earlier, it's up to the researcher to locate the material, wherever it may be. He must locate as much material as may be essential to provide satisfactory answers to the questions he has raised.

This search takes time, it takes patience, it takes intelligence, and it takes imagination as well. And there is no substitute for these things, unless, of course, the researcher gets impatient, says "To hell with it," and passes the job on to someone else, as frequently in fact can be done.

The solution of the research problem, the answers to the questions raised, will be no better than the materials, the facts, on which they are based, with respect both to accuracy and completeness.

When the research data is at hand, the next step, of course, is to write it up in an orderly and coherent manner. The researcher may feel that he has an amount of material quite adequate for the requirements of his problem. He may feel that when he has collected the essential data, and, in his own mind, reached conclusions, that the job is done. But there still remains the necessity for organizing the data, and putting it into the form of an organized report, paper, or article. And, as most of you know, this is often the worst part of the job. But it is an indispensable one. It is no good to have information just in your own mind. You have to make it available to others. Also you may not have your facts as adequately in your own mind as you think you have if you haven't put them down on paper in a systematic and logical manner.

However, don't make the mistake of thinking that the preparation of the report from the data is nothing more than a job of English composition. The data have to be analyzed. They have to be evaluated. They must be related to the basic questions and the problem with which the research is concerned. Very often the hardest and most careful thinking done on a research job is done in this final stage.

Now let's turn briefly to what I referred to earlier as the mechanics of research. This is not really a very difficult or complicated subject. But while the mechanics of research in the social sciences are relatively simple, they will give much trouble if carelessly handled. We have to do here primarily and especially with the details of taking and assembling notes.

What might be described as a basic principle in research may be stated as follows: Above all, use your head; but use it especially for those purposes for which it has no substitute; namely, thought. Don't use your head as a basket in which to carry things which are much safer when you put them down on paper. Get your ideas, your questions, your problem down on paper at the outset. Take notes of ideas, opinions, facts obtained from your sources. Don't trust your memory, even when you have a pretty good one.

Obviously it is very important to have a dependable system of taking notes. There is no one system, no perfect system of note-taking. Individual preferences vary as to the kind of notes, the amount of notes, and the manner of recording them. But be sure you have a system, and be sure the system serves the requirements of the job which you are doing.

You take notes for two purposes: first, to save your head for better purposes, as I have suggested; and to avoid errors and lapses in memory. Second, you take notes in order to support your statements by citing the sources and authorities from which you obtained your data.

Unless you have a very good memory indeed, you will need to consult these notes in the preparation of your report or paper, and in the documentation in footnotes of particular statements, quotations, and so forth, made in your report. You will also need these notes in the compilation of the bibliography which is an essential feature of most research reports.

Keep in mind always that we are dealing here with social science; that scientific knowledge rests on evidence; and that therefore we must support our statements with evidence. Evidence is just as essential in research as in the court room. And the evidence in turn, if it is an essential and vital piece of evidence, must be authenticated by a statement of the source or sources from which it was obtained.

Notes may be taken down by typewriter, or by pen--regular or ball point. Don't, if you can avoid it, use pencil. Pencil notes smear.

Notes can be taken on either paper or cards, but a quality of material capable of taking ink and standing a reasonable amount of wear and tear should be used. The sheets or cards should be of a uniform size, and preferably loose. The use of a bound notebook is adequate for some purposes, especially if the project is not too large. The advantage of separate sheets or separate cards is that when you have all your notes taken on them, you can shuffle the cards or sheets about, rearrange them in a way which is very useful. This will be very helpful when you get down to the actual preparation of the report on your research.

To take the greatest advantage of separate sheets or cards, it is important to adopt a related principle, namely, a separate note for each fact or item recorded. What constitutes an item or fact always presents something of a problem. You don't want to have information on a variety of matters on the same card. As you get into the development of your subject, you will usually find that it tends naturally to break down into a number of questions or problems or areas; and you will soon find yourself almost automatically putting notes relating to these separate items on separate cards.

The size of the sheets or cards presents another problem. Here again it is a matter partly of individual preference and partly of

convenience in handling. A full typewriter sheet of the usual kind, for example, is apt to be too large for most note-taking purpose. I think if you fold this sheet and divide it in half it is much easier to handle. A useful size card for many note-taking purposes is the 4 x 6 inch card which you can get from the supply room. The 3 x 5 inch catalogue cards that are so much used in the library is perhaps rather small for many research purposes. With this card, you may often find it necessary to use two, three or more cards to record a single long note; and this will prove inconvenient.

Next, what do you put on your note card or sheet? Let me break this down into two parts: bibliography notes, and notes proper.

A very common practice of researchers in the social science field where many books, articles, and monographs, have to be consulted is to set up a separate set of bibliography cards or sheets. The 3 x 5 library catalogue cards are very useful for this purpose.

What goes on the bibliography card? As a minimum, the full name of the author, the full title of the book or article, the place and date of publication of the book; the date of the issue, and the volume and pages occupied by the article in a newspaper or periodical. If you don't record this essential bibliographical data, if you don't make note of the new paper, book or article you are consulting, and from which you obtain essential facts and ideas, you may spend hours at the end of your research job looking up again these books and articles to get the necessary bibliographical data. So the smart thing to do is to take down this data before you take a single note from it; and then file it away on a separate card. Then when you come to making your bibliography out at the end of your report, all you have to do is to assemble these cards in alphabetical order by name of author and the job is just one of typing.

Next notes proper, you are making notes on something in the book or article you are consulting--notes of facts, ideas, and opinions.

At the top of the sheet or card, give an abbreviated title of the source, together with the name of the author, and the volume and/or page of the citation. Of course, if you don't keep separate bibliographical cards, you have to put the full bibliographical information on the note itself. Of course, if you have a number of notes from the same book, it becomes rather tiresome to repeat that full data, in place of the abbreviated data which you can use if you have a separate bibliographical card.

The next problem is what and how much to note down. How much information? How much detail? Where should you draw the line? Shall you abstract the item or condense it? How often do you want to take a direct quotation? This is a matter of judgment. Too much detail and you'll be taking notes for hours; too little, and you may find you will have to consult the book or article again when preparing your report.

I suppose a good general rule is that in most instances you extract, paraphrase, condense, or digest the essential information that you are looking. But where you find a very pertinent, very meaty or strong statement by an important authority that you think may find desirable to quote directly, then take down a statement in full verbation. But be sure you quote it correctly, and be sure you put quotation marks around it.

Still another matter that falls in part within what I have called the mechanics of research has to do with the actual writing, the preparation, of the report or paper. There is a natural tendency to perform what I have called the essential steps in research in the order in which I have taken them up this morning--defining the problem and the related questions; digging up and assembling the pertinent facts; and, finally, organizing and writing up the report. Don't let me offer a word of warning in respect to this "natural" order. It is usually, though not inevitably, a mistake to put off the job of writing till the rest of the work is done, until your search for the facts has been completed. The reason is simply this: Most of us don't really get down to hard, close thinking about a subject until we have to put our thoughts, our ideas down in writing. As you work with your assembled facts, as you develop logically the reasoning based on them, you are apt to discover that there are gaps in the facts, or that new questions arise which hadn't occurred to you when you first started on this job. These new questions may turn out to be very important, more important than the first ones; but you haven't collected facts bearing upon them.

Or you discover that some of the questions that you started out with have lost something of their significance, are not good questions or important. You may start out with, "Why do gentlemen prefer blondes?" and end up with "Do gentlemen prefer blondes?" You may start with the assumption that it is color of hair that defines the blonde, and end up with the conclusion that anatomical configuration is more important than color of hair.

Seriously, there is a real problem here. You may have, as you believed, reached the end of your research and start writing only to find that you have to go back and do a good bit of the research over.

And there may not be time to do this. The moral of all this is: start very early to get something down in writing. Don't wait to get "all the facts" in. Start a rough draft, perhaps dealing with only one part or one phase of your subject. Start it just as soon as the picture begins to take some kind of shape, some kind of form, in your mind. It doesn't have to be polished or finished writing--a rough draft will usually be sufficient. Then you can revise and shape this draft as you proceed further with your research. But in putting together this rough draft, things will begin to appear in a different light from what they did at the outset of your study.

Now, there are a lot of other topics which it would be interesting and useful to discuss here if there were time. For example, how many swallows does it take to make a summer? How many facts does it take to support a generalization or conclusion? In the pressure of time, you may try to by-pass this one, but you do so at your own peril.

Which comes first--the chicken or the egg? Do you prepare the outline before or after you write the report? Do you start with the conclusion and build your report up to support it or follow the reverse order?

I think it is safe to say that the greatest obstacle to successful research and to clear thinking in the social sciences is bias--the lack of objectivity, or impartiality in the collection of the facts, in the formulation of the problem and in reaching conclusions.

Most of you, I presume, heard Mr. Northrop's lecture on "Sources of Social Tensions." You will recall that he showed how each one of us is simply a seething mass of trapped universals. Now, a great many of Professor Northrop's trapped universals are simply prejudices--or if you prefer, preconceptions or convictions.

All this means, I presume, is simply this: each one of us has attitudes and beliefs on a wide range of political, social, and economic issues--issues, that is, falling within the domain, of the social sciences. It is not unfair to say, I believe, that most of us have acquired these attitudes and these beliefs by something less than scientific methods. This is in the very nature of things. It is both necessary and desirable in the every-day business of living. We can't research every attitude before we adopt it. But--and here is the point--such attitudes and beliefs, once adopted, inevitably result in our prejudging things that we see, hear, or read.

All very well and good, except when we come to doing a research job in the field of human behavior. Prejudging, that is, prejudice, has

no place in science. And here we face, as I remarked earlier, a major obstacle to effective research in the field of the social sciences--an obstacle that has to be faced squarely and surmounted, by one means or another. Many a piece of research in the social sciences, especially at the amateur level, consists of little more than the documentation of the writer's prejudices regarding specific areas and problems in social behavior.

We can in a measure protect ourselves against our prejudices. However, it is not easy to do so. But we can get a certain measure of protection from our prejudices by searching our own minds, by trying to discover just what our prejudices are. Whatever we call them--prejudices or convictions trapped universal attitudes or preconceptions they all get in the way of seeing facts squarely for what they are. Research in the social sciences requires a kind of continuous soul-searching, to identify and, for the research job in hand, sterilizing any preconceptions which may color or bend our collection of, and thinking about the facts, This must be done if the results of our research are to have any meaning and value.

Now, finally, just a brief word on what I shall call the spirit of research. It's very important to distinguish between the form and the substance of research. The form of research--the mechanics of notes, footnotes and bibliography, the orderly arrangement and analysis of materials--can be and often are faked. Every one of us with experience as either or both student and teacher is familiar with at least some of the tricks of such faking. The more obvious and flagrant are going back into the fraternity stockpile of papers and reports; paying someone to do a paper to be handed in under your name; or swiping an article, with minor changes, from the Encyclopedia Britannica.

But there are the less obvious types of faking. Something has to be slapped together in a hurry. We read a few books and articles. We hurriedly whip the thing into shape. We gloss over our ignorance by making confident assertions. And we throw in a sprinkling of footnotes to serve as a kind of academic holy water. We don't usually fool many by this sort of thing. The important thing is not to fool ourselves about what we're doing, to think that by going through the routine mechanics of carrying on a research job we have really performed research.

The spirit of research, of course, demands accuracy. It demands more than that. It demands good faith. It demands honesty. It demands an unwillingness to make more from the facts than the facts themselves will support. The social scientist, like the physical scientist,

whether amateur or professional, dedicates his work to the task of obtaining the truth.

Now, of course, to speak of dedication to truth sounds like so much wishy-washy do-goodism. Yet such dedication is basic to advance in the social as well as the physical sciences.

Now, one final comment: It is not the purpose of this institution of higher learning, as at graduate schools in civilian institutions of higher learning to develop social scientists. Yet your year at the Industrial College would be lacking in something very essential if you should leave this place without a deeper understanding and appreciation of the scientific spirit as well as the scientific method, as applied in the study of human behavior, a deeper understanding and appreciation than perhaps you may have now. We can't all be, we don't all want to be, scientists, whether social or physical; but it is possible for every one of us to acquire and to apply in our every-day duties something of the scientific attitude, something of the scientific approach, to the study of human behavior.

Thank you very much.

DR. REICHLEY: Before I ask Louis to take the platform to answer any questions or comments you might have in your mind, I want to point out that when you get back, you will find in your student box a small book hand-out on the library. I want to call particular attention to the latter part of this hand-out--an article on Eighteen Useful Research Tools. For those of you who are not acquainted with the major bibliographical aids, this shows you what are available to researchers, such as the Industrial Arts Index, the Statistical Abstract, and so forth. This shows you many short cuts to getting information.

If you have any questions, Louis will be glad to help you.

QUESTION: I think a useful analogy for those in the military services to recapitulate your point is this outline staff study, where you have, first, the problem, then facts bearing on the problem, discussion, and conclusions. While normally this kind of work ends up with conclusions, not necessarily recommendations, yet these are what you will struggle with in staff studies. I think that epitomizes your outline of research.

DR. HUNTER: Thank you very much. It does indeed--you have given another and excellent example of research which isn't labelled as such. Let me make a comment on what sometimes happens in staff work,

whether done in civilian agencies, in the military departments or in business. There is sometimes found the view that if you are going to make a staff study, the first thing as a practical matter is to find out what topside really wants, and then build up the thing to give him what he wants. Where this is practiced we simply have further illustrations of the kind of faking of research I referred to earlier.

QUESTION: On this term paper, I understand that last year it was the practice to publish, I believe, about four of those. I presume the selection of those was based on the originality of the papers. Is that right?

CAPTAIN McCAFFREE: May I answer that? Each term paper is judged on its own merits. Those which are recommended for reproduction will be reread by a second reviewer. This last year, out of 134 papers turned in, there were 30-odd recommended for reproduction. They are in process now. In prior years I believe on the average it ran about 10 percent. But there is no fixed percentage. It is the merit of the paper itself.

QUESTION: Can you give us some idea how many man-hours we should put on each of these things, when we should start, and so forth? Can you give us a little guide?

DR. REICHLEY: Man-hours in relation to what? To the term paper?

QUESTION: The term paper.

DR. REICHLEY: That is impossible to answer. It is entirely dependent on the subject you are dealing with--the amount of effort you want to put into it--because it is your work. In fact, we lay down very few guide lines. We suggest the maximum length of the paper. We don't want more than ten thousand words, because with 140 papers to read, brevity and conciseness are important qualities. But there is no answer to your question as to the number of hours required to do the job. It is entirely up to you.

Gentlemen, you will, of course, take the term paper seriously, but don't worry about it. We will get the information regarding the term paper to you in October. This paper won't be due probably until about the first of May. So you will have ample time to plan for and carry out this assignment. To some of you, this term paper may sound very formidable, but actually our students find that while it requires time and

work, it does not present any real difficulty. We on the staff and faculty are here to help you all we can.

QUESTION: Will we have an opportunity to have something to say as to the selection of the topic, or do you give us a list of topics?

DR. REICHLEY: You have a very wide choice of subjects. As long as the topic proposed falls within the scope of the mission of the Industrial College of the Armed Forces, it is acceptable.

QUESTION: Do we receive these papers back, so we can tell in any way whether we botched them or not?

DR. REICHLEY: Yes. All through the term you have your faculty moderator, who will guide you and assist you to the extent you desire. You will receive assistance from the permanent staff on any report you turn in. Even on individual reports the branch chiefs will send you written commentaries. There is no formal evaluation. We don't give the papers letters or percentage or any other kind of grade.

Again, the point I want to make is that what you do has to be of value to you. Its worth is what it is worth to you. This is your opportunity as individuals, having ten months here of fairly free time, with the College guiding you, to do something that you want to do; and what you put into it is what you are going to get out of it. You don't have to convince us that any line of action or this policy or that should be adopted.

QUESTION: Does the school maintain a list of available typists?

CAPTAIN McCAFFREE: I have some information on that, but I regret that I don't feel I am at liberty to say other than that the Commandant himself is doing everything he can to assist you. At an appropriate time, if such should ever arise, we will be very happy to give you the information. But there have been active steps taken.

To get back to your question on the subject matter and the like, R&D of the Army found out that some of the term papers that had been written would be of use to them, and they sent a special messenger over here to get five of them, I believe. These they reproduced, because we didn't have the facilities to reproduce them as soon as they wanted them. I think that is the best indication of what value the term paper may be to your own service, besides to yourself.

QUESTION: One of the best sources of information to the scientist is to work with other agencies which have that information. Is there a regular procedure that you go through to authenticate yourself to these other agencies so you can get classified information from them?

DR. REICHLEY: Not with individual workers. You will get more information through the individual branch on that. Beginning with the Manpower Unit, which opens on Monday, you will get all this sort of information.

QUESTION: Is there any limit on the classification of these papers?

DR. REICHLEY: No. There is no limit. But we like to keep the classification as low as possible.

QUESTION: What is the general classification?

DR. REICHLEY: Unclassified. But you are not limited to that in any way, either in committee or individual reports or term papers.

Thank you very much.

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