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LOGICAL POSITIVISM AND THE METHODOLOGY
OF POLITICAL SCIENCE: ANALYSIS AND PROGRAM

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CHAPTER ONE

INTRODUCTION: OBJECTIVES AND ORIENTATION

The dissertation has three broad objectives. In summary, they are: (1) to delineate a philosophical basis for the analysis of methodological problems in political science; (2) to employ the philosophical basis for such analysis; (3) to present a case for and contribution to greater methodological awareness among political scientists. A central theme of the volume-- indeed, its very raison d'être-- is that even the partial achievement of these objectives would constitute a contribution to the scientific development of political science.

At the present time, this purpose is not deemed a worthy one by all concerned. Some contend that the scientific study of politics is impossible, others that it is undesirable, and still others (those who should be most content) that it is both undesirable and impossible. The moral, factual, and logical challenges represented by these claims will not be ignored--the logical issues they raise are a central concern of the dissertation.

In the first three sections of this chapter, we will discuss the three objectives. Their clarification will shed some light on the subject of the dissertation

and our general orientation toward it. A fourth, and final, section relates to the controversy mentioned above.

A. Philosophical Basis and Methodological Problems

The philosophical basis referred to in our first objective is based upon writings of philosophers belonging to the contemporary analytic movement--most prominently, those who might be called "Logical Positivists." It consists, primarily, of the philosophy of science. However, the more comprehensive system of ideas of which the philosophy of science is a part could not be wholly neglected. The philosophy of science provides us with a profound understanding of the nature of science. But science is not all of life. Its understanding is further enhanced when we are able to fit it into the rest of our experience. And this requires a complete philosophical perspective, a "metaphysics," if you please. This raises matters which belong to what has felicitously been distinguished as philosophy proper.

The need for fitting science within a broader perspective arises most prominently in our analysis of political science with respect to the question of values. For this is one way to avoid the misunderstandings that often result from a treatment of values which does not go beyond investigating their place in science. From the assertion that science cannot prove value judgments to the

conclusion that one person's or group's values are just as good as any other's, there is a mode of reasoning which defies not only logic but common sense as well. Yet it is not unusual to find this inference imputed to those who insist upon the fact-value distinction in social science.¹

At this point, it should be apparent that the author does not adhere to the absurd value judgment that all values are of equal value (what David Easton called "equalistic relativism"). Probably nobody ever has. And this is an opportune place to anticipate some conclusions of Chapters Six and Seven, and thus divest myself of other attitudes toward values which have been attributed (usually without cause) to those who (like myself) advocate scientific social science and Logical Positivist philosophy. In this way, we may avoid the stimulus generalization which -- if I am not mistaken -- accounts for much of the opposition to the objectivist (or scientific) orientation to political science.

¹It is not usually asserted so bluntly as I have stated it, but Barrington Moore, Jr. is almost as forthright when he offers as a partial explanation of what he observed to be a move toward "the moralist position" in the social sciences, the following: "I suspect that the change has a good deal to do with the discovery that the relativist and objectivist position leads to the conclusion that the social practices of Nazi Germany and Communist Russia cannot be condemned any more than cross-cousin marriage and the couvade." "The New Scholasticism and the Study of Politics," World Politics, 6, 1953, 122-138, at p. 127.

Horried at what appears to be the degradation of human values, the humanistic political scientist rejects the whole objectivist orientation. On this occasion the response is not appropriate, for here there is no such stimulus. Thus, the following assertions: It would be inconsistent with any empiricist philosophy, such as Logical Positivism, to deny the ontological existence of values. That is to say, there are values. Furthermore, not only is it true that we can, as political scientists, reasonably discuss values, but I agree with Dwight Waldo (though not with most of his reasons) that discussing value questions in much political science teaching is almost unavoidable and that we should not try to avoid it.¹ (Value judgment!)

One further denial and I will be content to wait until later to have my say on values. The emotivist theory of values, as at times advocated by a number of Logical Positivists and other analytic philosophers, whereby value judgments are interpreted as "merely the expression of emotion" seems clearly inadequate. Upon some interpretations it denies that there are values; by others it makes ethics a branch of psychology; in all versions, it does not do justice to the significance

¹"'Values' in the Political Science Curriculum," in Approaches to the Study of Politics, ed. R. Young (Evanston: Northwestern University, 1958), pp. 96-111.

of values in life.¹

The clarification of our first objective requires some explication of what we referred to as "a philosophical basis" and as "methodological problems in political science." However, we will proceed slowly. So far we have indicated that the philosophical basis is primarily the philosophy of science, but that philosophy proper is also considered where relevant. This requires two additional comments.

1) Readers familiar with contemporary philosophy will have noticed that I have taken a position on an issue which divides analytic philosophers. Many, following the dominant view of early Logical Positivists, regard philosophy and the philosophy of science as co-terminous, or at least nearly so.² I do not, and will indicate why in Chapter Three, when the basis for the distinction will be developed. However, such strictly philosophical issues will not be argued in detail. This is part of the second comment.

¹An early statement, first published in 1936, of this view of the nature of value judgments is contained in A. J. Ayer, Language, Truth, and Logic (2d ed.; New York: Dover Publications, Inc., 1946), pp. 102-114 and pp. 20-22 of the author's new introduction. For a concise review and cogent criticism of the emotivist theory by a Logical Positivist philosopher, see: M. Brodbeck, "Philosophy in America, 1900-1950" in M. Brodbeck, J. Gray, W. Metzger, American Non-Fiction 1900-1950 (Chicago: Henry Regnery Co., 1952), pp. 83-87.

²See, for example: Arthur Pap, Elements of Analytic Philosophy (New York: Macmillan, 1949), Chap. 1.

2) We have indicated our broad strategy as that of using philosophy for our purposes regarding political science. Obviously, we cannot attempt to present philosophically adequate arguments for the philosophical positions taken, though we will present enough to make them plausible--at least this will be our goal. The main test will be a pragmatic one: does the philosophical orientation clarify certain kinds of issues about political science? Thus, when I write of the philosophy of science, I do not mean to assert that there are not competing philosophies of science.¹ In one sense, there obviously are. All of the classical philosophical systems have their contemporary adherents. Witness the frequency with which we find the prefix 'neo-' followed by the old names--Platonism, Thomism, Hegelianism, etc.--in the description of contemporary orientations or schools. And most of them have something to say about the nature of science. What we can say is that there is much less that is controversial in the philosophy of science than there is in philosophy proper. Persons of quite diverse philosophical persuasion can, without inconsistency,

¹For a general account, see: A. G. Ramsperger, Philosophies of Science (New York: F. S. Crofts & Co., 1942), and the essays of E. Nagel in Sovereign Reason (Glencoe, Illinois: The Free Press, 1954).

agree in their interpretation of the nature of science.¹

Within the broad confines of analytic philosophy, the most developed philosophy of science (particularly regarding social science) which differs in important respects from that which is followed here is instrumentalism, the Deweyan variant of pragmatism. It is-- in my estimation--influential in some areas of social science and in some recent developments in political science. In another type of work, I believe it would be enlightening to examine this competing philosophy of science in its entirety within the context of its contemporary influences in social science, but this lies outside the scope of our interests on this occasion.

The philosophy of science is a certain kind of description of science; perhaps the expression "analytic description" conveys the proper idea. It is not an evaluation of the efficacy of science as a method or the truth of science as a body of knowledge. Generally speaking, we know, quite commonsensically, that scientific method works and that scientific knowledge is the best knowledge we have. Of course, any particular

¹G. Bergmann, Philosophy of Science (Madison: University of Wisconsin Press, 1957), pp. 5-7; see also R. B. Braithwaite, Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science (Cambridge: Cambridge University Press, 1955), pp. 2-9.

scientific law or theory is always open to challenge and refutation, but this is the business of the relevant special science, not of philosophy. Nor does the philosopher of science attempt to evaluate science in the sense of answering moral questions about science. Referring to the philosophy of science as a description of science is, therefore, enlightening in that it reflects its neutral, non-evaluative character. However, calling it a description may be misleading in another respect. For the philosopher of science does not describe science in the sense in which scientists describe those aspects of the world in which they are interested. To do so--i.e., to study science as a social phenomenon--is itself a scientific activity: the history, sociology, and psychology of science. Most of the general topics, and what it is that the philosophy of science attempts to accomplish are revealed in the following quotation:

The scientist uses in a rough and ready way notions of lawfulness, causality and probability. Concerned as he is with finding particular laws or specific causes for specific events, it is left for someone else to answer such questions as these: What exactly is meant by saying that one event is the cause of another? What precisely is the structure of a law of nature? How does such a law differ from so-called probability or statistical laws that preoccupy both physical and social scientists today? What is the nature of a theory? How are concepts and laws related to it and to each other, and how are they all related to the

world of experience? Science, we are told, uses now induction, now deduction; what is the nature of these methods, what is the difference between them? To answer such questions is an enterprise in logical analysis, an analysis which clarifies the basic ideas¹ and methods of the sciences.

Now we have some idea of the nature of the discipline, philosophy of science. In addition to the general discipline there are divisions or areas, like philosophy of the social, biological, or physical sciences; and, of narrower focus, philosophy of physics, psychology, and so on for all the special sciences. The distinction between the philosophy of science and these more specialized studies may be made in this way: the general discipline focuses upon those fundamental ideas and methods (or principles) found in any science; the more restricted inquiries emphasize characteristic logical problems surrounding the manifestation of these principles in each of the sciences (or groupings of them). As illustrations, let us take a glimpse at methodological problems which have special significance for (a) psychology, (b) sociology, and (c) history. (Note: we do not say that these problems are not germane to other social sciences.)

¹M. Brodbeck, "The Nature and Function of the Philosophy of Science," in H. Feigl and M. Brodbeck, eds., Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, Inc., 1953), pp. 3-7, at pp. 4-5.

(a) In their most general formulation, the principles of concept formation are the same in physics and psychology, but physics does not have to deal with the problem of "other minds." The psychologist talks about such things as feelings, beliefs, memory images, percepts of his subjects. Since these cannot be observed by anyone other than the person experiencing them--so the argument goes--how can psychological propositions employing such concepts meet the criteria of intersubjective confirmability required of all statements in science?

(b) For sociology, characteristic methodological problems surround the use of group concepts. Exactly what are the referents of such concepts as "the family," "the church," "the upper-class"? Are there group things in addition to individual things? Must these concepts be defined in terms of characteristics of individuals? If so, what is the status of sociological generalizations? Are they all "reducible" to the laws of psychology?

(c) Probably the logical questions most characteristic of history are those relating to the relevance of statements of lawfulness to the historian's stated concern for the description of unique historical events and periods. Does the historian merely describe the past or does he also make assertions about causal relations and (what amounts to the same thing) provide explanations of historical events and/or trends? And if he does the latter, must he employ laws about the social process? about individual behavior? or is he able to achieve explanation (or "understanding") without any reference to lawfulness?

I take it that it is now apparent that we have arrived (at last!) at the threshold of "methodological problems in political science." A verbal bridge will take us the rest of the way. Following linguistic habits characteristic of writing in the philosophy of science, I use the expressions 'methodology,' 'philosophy of science,' and 'logic of science' synonymously.¹ The

¹The reader is cautioned concerning the term 'methodology.' As employed here it does not encompass research procedures (techniques of science) as it does

labels refer, of course, to both a product and a process. In their latter meaning, they are encompassed by the term 'logical analysis,' which is more inclusive in that it is also another name for analytic philosophy, again as a process.

Taking our cue from the above discussion of the general discipline of the philosophy of science, and of characteristic methodological questions from three of its subdivisions--philosophy of psychology, sociology, and history--our subject may, as a first approximation, be described as another of these subdivisions. What, then, are some of the methodological questions characteristic of political science? To this question we will

in most social science usage. See, e.g., the discussion of the nature of methodology in P. G. Lazarsfeld and M. Rosenberg, The Language of Social Research: A Reader in the Methodology of Social Research (Glencoe, Illinois: Free Press, 1955), pp. 1-12 and 495-497; and A. Leiserson, "Problems of Methodology in Political Research," Political Science Quarterly, 68, 1953, 558-584, reprinted in Political Behavior: A Reader in Theory and Research (Glencoe: The Free Press, 1956), pp. 53-64, especially p. 53. My decision to use the word 'methodology' in this way could cause some confusion. I hope this note will save us from that. But this usage may help us avoid another kind of confusion later on. I refer to the tendency on the part of some to identify certain techniques of the more advanced sciences with scientific method itself, especially experimentation and quantification. The dialectic from this mistake to the conclusion that politics cannot be studied scientifically is a familiar one. (Discussion of this point is included in Chapter Five.) It should also be mentioned that I shall also follow philosophers of science by occasionally using the term 'logic' as synonymous with 'method'; 'logic' also has a narrower meaning, as in the expression 'symbolic logic'; however context will always indicate which of these meanings is intended.

now turn.

At this point we will merely list some of the specific questions which we will later attempt to answer; we will not attempt to answer all of these questions, but topics related to all of them will be discussed. Afterwards, we will comment upon one general feature of the methodology of political science which they seem to exhibit.

(a) Every political object and event is unique. Therefore, political science must be an ideographic rather than a nomothetic discipline. Is this argument a valid one? And if it is invalid, isn't there, nevertheless, a serious conflict between scientific method, with its emphasis upon generalizations, and our profound interest in specific events -- today's international crisis, the last election, the next war?

(b) In recent products of the "political behavior movement" we find such concepts as "sense of political efficacy," "perception of social class," "decision-maker's view of the world," "definition of the situation." Since the referents of such concepts are not subject to direct observation, does their use introduce the kind of mentalism and subjectivism mentioned in our previous illustration of a methodological issue in psychology?

(c) When political scientists speak of defining their discipline, are they using the same idea of definition as when they define 'power,' 'authoritarian political attitude,' or 'imperialism?' If not, exactly what are they doing when they define the discipline? We all know, do we not, what political science is? The whole thing seems stranger still when we reflect on the fact that physical scientists never seem to have regarded the definition of their disciplines as a serious problem. Are there special circumstances in political science which make the task of defining the discipline a scientifically important one?

(d) Considering the vast difference in precision and scope of scientific laws in the physical

sciences and the generalizations found in political science, is it correct to even talk about laws in political science? If it is, what are the types of statements of lawfulness most frequently found in political science?

(e) Is it possible to provide an adequate explanation of the results of an election without referring to the characteristics of individual voters? That is, can a group phenomenon be explained on its own level?

(f) The charge of "reductionism" has been made against political scientists who emphasize the use of concepts, research techniques, and theories of psychology. What precisely is meant by saying that political science, or a theory in political science has been reduced to psychology? Disregarding factual difficulties, is such reduction logically possible?

(g) There is no doubt that values influence behavior. They often influence the problems which political scientists select for research, and even their interpretation of the results of research. And, of course, the persons studied are also influenced by their values. Is there, then, a defensible sense in which we can speak of "value-free" political science? What is it?

An examination of this list reveals considerable overlap with our previous listing of general methodological issues characteristic of psychology, sociology, and history. Questions (a) and (d) here involve the same issues given earlier as typical of methodological discussions of history; (b) is an instance of the prior illustration for psychology; and (e) and (f) are, in like manner, encompassed by our examples for sociology. If our illustrations are correct, then these observations suggest what is -- in my estimation -- the most distinctive feature of the study of the methodology of political science: the range and diversity of topics which it must investigate

are, indeed, extensive; probably more extensive than such a study of any other of the social sciences. For all, or nearly all, of their major methodological issues are significant for political science, and there are others, such as those related to values, which are of greater import for political science than for any of our sister disciplines. To support these sweeping conclusions would require more than an anticipation of most of the remainder of the thesis, so we will leave them for the present as revealing conjectures. They reveal the conception of the subject guiding the selection of topics for this study. The significance for political science of the many diverse matters involved in these topics must show itself as we discuss them.

We have completed discussion of the first objective: the delineation of a philosophical basis for analysis of methodological problems in political science. By allowing ourselves to be led off in whatever direction the desire for clarity seemed to warrant, perhaps we have accomplished our intention of introducing the subject of the dissertation and our orientation toward it. This was one reason for discussing the three objectives. The other reason was to clarify the objectives themselves. And the main task in this regard was the first of them, since understanding it renders the others much less problematic. Since this has been accomplished (as far as possible, given the complexity of our subject

and the limitations of our author), the remaining remarks concerning the second and third objectives will be very brief.

B. Analysis of Methodological Problems.

The second objective is to carry out the kind of analysis mentioned in the first, which means to answer some of the illustrative questions presented above and others like them. Particular examples of the manifestations of these problems chosen for analysis will, in most cases, be taken from political science writings. But in view of the obviously wide ranging significance of the diverse methodological issues we will be considering, it should not be surprising that considerable attention will be given to matters seemingly alien to "traditional" political science. This appears all the more appropriate, however, in view of the fact that concepts and theoretical orientations of other social sciences are being employed with increasing frequency in political science.

The evidence of an increasing interdisciplinary focus by political scientists is -- as I regard it -- an encouraging sign.¹ More emphatically, in Chapter Four

¹Dwight Waldo documents this under what he described as the "political behavior movement." Political Science in the United States of America: A Trend Report (Paris: UNESCO, 1956), pp. 22-30. Representative examples of recent interdisciplinary research by political scientists may be found in: H. Eulau, S. J. Eldersveld, and M. Janowitz, eds., Political Behavior; A Reader in Theory and Research (Glencoe: Free Press, 1956). That this

I shall relate several reasons which--if I am not mistaken--make it very probable that we cannot adequately handle many of the problems with which we have been concerned without a great deal of help from the other social sciences. But in the light of the fact that these "new" orientations and approaches may result in major changes in our discipline, they require careful scrutiny.

Already the enormity of our task must be apparent. My final comments upon our second objective relate to one feature of our subject which helps us adjust to its scope, and to the way in which we will adjust to it. The feature is the interrelatedness of methodological problems. They tend to appear in clusters such that a relatively small number of issues, sometimes just one, are fundamental in the sense that their resolution is crucial for the resolution of all the rest. As we would expect, the fundamental issues are significant beyond political science--most of them relate to all science. The other issues in each grouping are more peculiar to political science. Finally, let us note that the

kind of orientation or focus is not (yet?) characteristic of most research in political science is indicated by two recent studies: C. E. Hawley and L. A. Dexter, "Recent Political Science Research in American Universities," American Political Science Review, 51, 1952, 470-85; and Charles S. Hyneman, The Study of Politics; the Present State of American Political Science (Urbana: University of Illinois Press, 1959), p. 161.

interrelatedness of methodological issues goes beyond their appearance in clusters, the clusters also form clusters, and they are all, ultimately, interrelated.

This is the feature of our subject which, I believe, helps us to adjust to its scope; it will become somewhat less abstract as I indicate its relevance to the contents of the thesis. There are -- in my estimation -- four major clusters of methodological problems in political science which may be descriptively labeled as follows: (1) concepts, (2) propositions, (3) explanation, and (4) values. We will consider all four of the major clusters and one major methodological issue which cuts across all of them (actually, it encompasses most of the fourth) -- namely, the question of the appropriateness of scientific method for the study of politics.

In light of what we said above **about** a cluster of methodological issues, we may think of each of the four just mentioned as a core, consisting of what we called the fundamental issues, surrounded by three concentric zones. The first, closest to the core, consists of methodological problems less directly related to empirical political inquiry or research; the second is made up of issues more directly related to research; and the third, or outer zone, is composed of methodological problems manifested in political research. These distinctions are admittedly vague, but I believe they will help us to describe the content

of the dissertation.

I intend to be most complete in discussing the issues at the core of each cluster and progressively more illustrative and programmatic in my treatment of issues in the zones moving out from the center. If methodological problems are as interrelated as I think they are (and as I hope to show in their analysis), then there is a sense in which I shall treat the whole subject of the methodology of political science. That is, I am claiming, in effect, that the basic ideas and style of analysis (which together make up the philosophical basis referred to in our first objective) employed in the handling of those methodological problems which I do discuss are adequate for dealing with others which I do not take up. This is the programmatic feature of the thesis--in this way it lays out a program of continued methodological analysis in political science.

As to the one major issue, I hope to treat it rather thoroughly. The fact that it cuts across every major area of methodological problems in political science will also help me to cope with the broad scope of our subject. For, during its discussion, there will be many opportunities to comment succinctly on issues which, because of self-imposed limits, could not otherwise be considered.¹

¹A comment concerning another procedural device belongs in a note. I will use the notes for some rather

C. Methodological Awareness

Our third objective, you will recall, is "to present a case for and contribution to greater methodological awareness among political scientists." This requires two comments. I don't know how much of the methodology of science it would be advisable to teach graduate students, for example. Certainly I would not argue the more the better, though the inclusion of some such training is involved in the vague idea of increasing methodological awareness. Another aspect of the idea is that the discipline should have its own methodologists. Again, I would not try to guess how many and I would very definitely not say the more the better.

To further complicate the question and at the risk of further confusing the claim concerning the utility of methodological awareness but to make my own convictions clearer, I shall try to elaborate these qualifications. Though I am convinced that raising the general level of methodological sophistication would contribute to scientific progress in the discipline, I am not at all confident concerning the advantage of further knowledge in this area for any particular political scientist. If someone has a rather narrow research interest in an area where there are fairly well established research

synoptic remarks on matters which might be more fully developed in the text of a more extensive work on our subject.

techniques, such as public opinion research or the more "legalistic" study of judicial decisions, then--it seems to me--increased knowledge of the logic of science would probably be of little or no benefit. And in the areas of highest speculation where one is so "far" from the data that he can only "leap in the dark," there is probably a point at which increased methodological awareness may ground an otherwise brilliant and creative jumper. On the other hand, for research in relatively virgin territories the task of developing novel procedures may be facilitated and the chances of success increased by the guiding percepts of scientific method. And, continuing to speculate, I believe a clear understanding of the logic of science is an almost indispensable tool for the generalist in political science who must cope with a greater range of the complexity and confusion of contemporary social science. At least as teachers, most of us are to some extent generalists. Enough said.

All of this is one of the two comments. The other is that I am aware that the best argument for more attention to methodology that can be presented here would be the achievement of our first two objectives. And it is with such evidence that I am almost exclusively concerned. However, I also want to include a brief consideration of another kind of evidence supporting the claim for the utility of greater attention to methodology. This follows in the next section.

D. The Frame of Reference of Political Science: Its
Problematic Status

We are primarily concerned with two disciplines: philosophy and political science. Those aspects of the former which are relevant to our purpose will be described, but I assume the reader is familiar with the latter. This is one reason for not attempting to present an adequate description of political science, or even of all of those aspects of it which are necessary to fully understand the subject of our inquiry. What I am interested in describing in this section is one kind of evidence which supports the conclusion that the frame of reference of political science is problematic.

Now the expression 'frame of reference' is admittedly vague, but so is the idea which I use it to express. It is this: by 'frame of reference,' in this context, I refer to the most general objectives and method(s) of attaining them which are well established in any field of inquiry.¹ In addition to the ordinary meaning of the term 'method,' as employed above it also includes

¹The qualification "in this context" was added to this definition of 'frame of reference' because the expression is so frequently used with several other meanings, such as: (1) a person's, usually implicit, criteria of judgment, (2) a synonym for 'definition of the situation,' (3) a "conceptual scheme" or "theoretical system" (David Easton), (4) any general orientation or approach to a subject. We recognize a family resemblance between the first and fourth of these meanings and the usage exemplified here.

certain presuppositions which are not questioned, are often not even consciously adhered to, but are nevertheless fundamental to a discipline. For example, some version of determinism--such as, every event has a cause or, more broadly, every event can be accounted for (explained) as an instance of a general law or theory--is part of the frame of reference for any science.¹ Whether or not scientists assert it, even if they deny it, this presupposition is revealed in their behavior. Scientists attempt to discover causes (or laws, which amounts to the same thing). When they fail, they give reasons such as the inadequacy of observational procedures, experimental design, or their selection of variables for investigation, or they give no reasons. What they never conclude is that there are no causes of the phenomena investigated. So we may say scientists "believe" determinism is true of our world, in the sense that what they do would not make sense if they did not. This

¹The controversies related to quantum physics are here disregarded. I find convincing the arguments of philosophers of science who maintain that even in this area of physics, the logic of science is not radically altered. See, for example, G. Bergmann, "The Logic of Quanta," American Journal of Physics, 15, 1947, reprinted in Feigl and Brodbeck, eds., op. cit., pp. 475-508; and E. Nagel, "The Causal Character of Modern Physical Theory," Freedom and Reason, ed. S. W. Baron (Glencoe: Free Press, 1951), pp. 244-268, also reprinted in Feigl and Brodbeck, op. cit., pp. 419-437. In any event, the supposedly "indeterministic" character of theories regarding subatomic processes does not have similar implications for theory in all those other areas of inquiry dealing with ordinary "middle-sized" things, such as the individual people and nation-states

indicates something about the notion of a frame of reference and something about science.¹

With respect to the physical sciences, the frame of reference is well established and unproblematic. That this is not the case for political science is readily apparent.² Let us take note of one impressive kind of evidence of the problematic status of the frame of reference of political science.

dealt with by political science. See Nagel, ibid., especially pp. 436-437.

¹This does not mean that determinism is a "meta-physical" presupposition or a necessary postulate of science. It is better described as a "regulative principle" or guiding precept. See, for example, A. Grünbaum, "Causality and the Science of Human Behavior," American Scientist, 40, 1952; reprinted in H. Feigl and M. Brodbeck, eds., op. cit., pp. 766-778.

²The notes in the remainder of this chapter show that political scientists have recognized this and written about it. At this point I will cite only two illustrations. In Contemporary Political Science (Paris: UNESCO, 1950), Massimo Salvadori reviewed the discussions of "method" by the contributors, and concluded: "However brief and incomplete it may be, this survey of the views expressed by certain authors of the studies published in this volume suffices to expose the confusion and ambiguity which reign in a field as fundamental as that of methodology." And it seems to me that even a cursory reading of these papers by forty-eight prominent political scientists of twenty-two countries thoroughly documents Salvadori's conclusion. After reading this and a number of similar recent statements concerning the methodology of political science, it was interesting to read the opening statement of the "Reports of the National Conference on the Science of Politics" in 1924: "Those who have been following the work of the committee on political research cannot escape the conclusion that the great need of the hour is the development of a scientific technique and methodology for political science." American Political Science Review, 18, 1924, 119-166.

The most obvious indication that the frame of reference of political science is still problematic is the continued controversy over the scientific status of the field. It would be difficult to guess how widespread the controversy is, though a number of political scientists have recently commented upon its prevalence and significance. D. G. Hichner and W. H. Harbold, in discussing the present state of political science, referred to the "intramural debates, long-standing but still continued without decision, over whether politics [i.e., the study of politics] is an art or a science."¹ James W. Prothro was no less forthright when he commented: "The author arrived at the conviction that the real question behind the current schism in political science is this: Is a political science possible? The dichotomy ... is ... into scientific versus anti-scientific schools."² And in a review of writings on political parties, F. C. Englemann takes note of a debate between M. Duverger and G. E. Lavau

¹"Politics in Perspective," Association of American Colleges Bulletin, 42, 1956, 298-309, at p. 298. It is not entirely clear whether in the quoted statement they were referring to politics or to the study of politics, though it appears that they were using the term in its ordinary sense. However, it is clear from their article that they make this assertion about both, so I have taken the liberty of interpreting the usage of the quotation to fit our present discussion. On the peculiar tendency of writers to confuse the characteristics of politics with what political science must be like, see below, Chapter Five, where I refer again to this article.

²"The Nonsense Fight Over Scientific Method: A Plea for Peace," Journal of Politics, 18, 1956, 565-

over the question of the possibility of scientific study of political parties, and comments that this could be answered "only in conjunction with the major question: Is a science of politics possible?"¹ David Easton observed that political science "is the last of all the social sciences in the United States to feel the influence of rigorous scientific procedures," and he concluded "that there is a tendency for political science to become the battle ground where the advocates and opponents of the use of scientific procedures fight out their issues."² Probably the most significant instance of such secondary evidence³ which we may cite are certain conclusions of Professor Charles S. Hyneman. In a carefully written book reporting upon his recent re-examination of the discipline he stated: "A substantial part of the intellectual conflict which plagues American political scientists is rooted in issues that are methodological in character." And in the next paragraph he adds: "The issues of methodology that

570; at pp. 567-568.

¹"A Critique of Recent Writings on Political Parties," Journal of Politics, 19, 1957, 423-40; at pp. 424-425.

²The Political System: An Inquiry into the State of Political Science (New York: Knopf, 1953), p. 48.

³I refer to these assertions as secondary evidence that the scientific status of the field is unsettled, but it should also be noted that as statements by political scientists they also constitute primary evidence.

stir American political scientists most deeply, it seems to me, are related to the scientific character of our study."¹

In the light of these statements it may seem strange that there is so little writing on the questions of whether political science is, ought to be, or can be a science. The author examined six of our leading journals² over the decade of 1949 through 1958 and found only fifteen articles (including short items, such as "Communications" in the A.P.S.R.) out of a total of more than 1500 that might reasonably be construed as directly concerned with the scientific character of the discipline, and approximately twice that number dealing with clearly related matters. This hardly constitutes evidence of a "great debate." However, I do not believe that Easton, Hyneman, and the other writers quoted were mistaken.³ The issues and their expression are more subtle than we might at first expect. Anyone investigating

¹The Study of Politics; The Present State of American Political Science (Urbana: University of Illinois Press, 1959), p. 151.

²(1) American Political Science Review; (2) Journal of Politics; (3) Political Science Quarterly; (4) Western Political Quarterly; (5) Public Administration Review; (6) World Politics.

³In this regard, J. W. Prothro suggested that the anti-science viewpoint in political science "enjoys much wider support than the pages of our professional journals might suggest." Op. cit., p. 566.

the anti-scientific orientation in political science would find much of his evidence in such writing as critical articles and book reviews which employ a style of argument that could readily be extended to oppose most any instance of scientific political research; and, most significantly, he would find evidence of a non-scientific frame of reference in much of the substantive writing in the field. I do not intend to carry out such an inquiry, though I will elaborate on the latter contention at several places in the thesis.

In my estimation, the problematic condition of the frame of reference of political science is itself adequate warrant for methodological inquiries such as that undertaken here. And the analysis of one kind of evidence of this feature of the present state of the political science discipline--the anti-science conceptions of the field entertained by some political scientists--represents one of the foremost tasks for such methodological inquiry. It is important, I believe, not just because some members of the discipline assert these views, but because they are a part of what might be called the intellectual milieu of the discipline. This assertion may be taken as an assumption of the writer, but it seems a rather safe assumption when such views not only command respect in the discipline but some of the members who assert them are regarded by many others as leading figures in the discipline. Under

these circumstances, I also have little hesitation in assuming further that the views expressed in the anti-scientific writing have contributed to the fact that an explicit commitment to scientific inquiry is less pronounced in political science than it is in the other social sciences.¹

However, even to say this much may raise controversies which I wish to avoid at this point, so permit me to retreat again, as I did once earlier, into saying that the views just expressed are perhaps most significant as indications of my choice of topics for analysis-- of why I will devote so much effort to the analysis of the anti-science arguments. There is evidence enough-- some presented above and more to be presented later-- of the prevalence of these views to justify their analysis here, even if I am wrong in my speculation regarding their influence. Actually, I will, as we proceed with the analysis of the anti-science arguments, raise a few considerations in support of the above speculation that they have had anti-science influences within the discipline. So, perhaps, this will not be left as simply an assumption.

Speaking of assumptions (I do so, even though I am aware of the ambiguity of the term), some may

¹I would add "with the exception of history," but many, not without good reason, would choose to consider history apart from the other social sciences.

think from what has been said, that I have assumed that the anti-science conceptions of political science are mistaken. I plead guilty. However, this is one assumption in the thesis that is only temporary. The matter of how temporary, or when will the arguments be given, is a matter I wish to comment upon.

In a sense, this large methodological problem has priority over all others--especially for one who is already convinced of the appropriateness of scientific method for the study of politics. For such a person will analyze all the other methodological problems in this light; we may even say that what he regards as such a problem will frequently be determined by this prior commitment. All this is, I believe, correct. Nevertheless, I think there is a good reason for my decision to deal with the arguments concerning scientific method for political inquiry in the second rather than the first half of the thesis.

There will be a good many assumptions underlying my analysis of the anti-science arguments--I don't think anyone can proceed otherwise. Of course, I must proceed from my own basic philosophical positions with respect to the other subjects of the thesis too, but I shall devote a great deal of effort before we get to such analysis to lay out that philosophical position and to support it by argument, as best I can. There will always be something left to assumption--i.e.,

something which is important but is not argued. From where I stand, this is, as it were, the way of things. So it will be advantageous to go through all this before getting to the argument over science. And, it so happens, that the other methodological issues partly blend into the exposition of the more basic philosophical ideas. Thus the order of methodological issues I have chosen.

One of the ordinary purposes of an introduction has not been mentioned, though it has been accomplished. I have given evidence of the concern of political scientists with the methodology of their field, and of the fact that they have shown this concern by writing on the subject. This thesis shares that concern, and it is intended as a contribution to that stream of writing. Its contribution--assuming it is successful--will ensue from its explicit roots in contemporary analytic philosophy. In this way, it differs from the bulk of methodological writing in political science.

The next chapter begins the discussion of the philosophical basis of the thesis by giving a general account of the philosophical movement from which these ideas developed.

CHAPTER TWO

LOGICAL POSITIVISM AND THE ANALYTIC MOVEMENT¹

Historically, the roots of analytic philosophy run as deep as the beginnings of philosophy, for philosophers have always had some interest in analysis even when most

¹There are a number of general descriptions of Logical Positivism and analytic philosophy as a whole. For the history of Logical Positivism I have relied most upon: A. J. Ayer, ed. Logical Positivism (Glencoe, Illinois: Free Press, 1959), "Editor's Introduction," pp. 3-28; P. Frank, Modern Science and Its Philosophy (Cambridge: Harvard University Press, 1950), especially the author's "Introduction, Historical Background," pp. 1-52; and J. Passmore, A Hundred Years of Philosophy (London: Gerald Duckworth & Co., 1957), especially Chaps. 6, 9, 14-17. The book edited by Ayer has also been a valuable source for some of the early papers by the founders of L. P., and it contains an extensive bibliography of not only L. P. but all analytic philosophy, (pp. 38-1446). Passmore's very readable book is, I believe, the most complete history of the development of analytic philosophy available. A. J. Ayer, Language, Truth, and Logic (2d edit.; New York: Dover Pub., Inc., 1946) remains as perhaps the most readily comprehensible introduction to the subject.

In addition to the sources cited above, I have been aided in the exposition of these philosophical ideas by: J. R. Weinberg, An Examination of Logical Positivism (London: Routledge & Kegan Paul, 1936) and V. Kraft, The Vienna Circle, trans. A. Pap (New York: Philosophical Library, 1953), orig. pub. as Der Wiener Kreis (Vienna, 1950). However, I owe most to two collections of papers by Gustav Bergmann: The Metaphysics of Logical Positivism (New York: Longmans, Green and Co., 1954) and Meaning and Existence (Madison: University of Wisconsin Press, 1959). For the present chapter the first three papers in the former and the second paper in the latter are most relevant. Additional sources are noted in footnotes.

of their efforts were speculative.¹ Today analytic philosophy is a widespread, very active movement containing within its ranks philosophers whose disagreements extend to the nature of the philosophical enterprise itself. Thus it is impossible to give a short account of analytic philosophy (or, probably, even a long one) which can claim both comprehensiveness and accuracy. This would be a good enough reason, if I did not already have a better one, for not attempting to present my own philosophical standpoint by describing those aspects of analytic philosophy with which I agree. There are no clear philosophical doctrines or even a school whose aspects I could so describe. So the approach to this task will be primarily by a straightforward argument reflecting the work of analytic philosophers rather than a description of their accomplishments. Nevertheless, in the present chapter I will give a very incomplete sweeping account of the history and present status of analytic philosophy, particularly that wing of it most appropriately labeled 'Logical Positivism.' This will do no harm

¹If anyone felt any doubt about this he need only examine the arguments of the Platonic Socrates in the early dialogues (which the scholars tell us were the most representative of Socrates himself) as he probed for the meaning of such concepts as piety, love, and courage, and compare these to the dialectic employed by an analyst such as Gilbert Ryle in some recent lectures published as Dilemmas (London: Cambridge University Press, 1954). The similarities are more impressive than the differences.

as long as we recognize it for what it is, and it will help with the exposition of my own views in the chapter which follows and give some indication of how they fit into this stream of thought.

Among persons unfamiliar with contemporary philosophy and those who are hostile to analytic philosophy, it is not unusual to find all of analytic philosophy referred to as Logical Positivism. Of course, this is not true, but like so many vague and false generalities it contains a germ of truth. So influential were the philosophical developments associated with the rise of Logical Positivism in the third decade of this century, that practically all analytic philosophers have been affected by it. For this reason and because the philosophical orientation to be developed here may properly be termed a variant of Logical Positivism, I will devote most of my attention to this branch of the movement, limiting myself to only a few remarks at the end concerning the similarities and differences among the divisions within current analytic philosophy.

Disregarding the structural history of the ideas involved, we may locate the historical origin of Logical Positivism with a group of scientists, mathematicians, and philosophers known as the Vienna Circle which was formed in 1922 around the personal leadership of Moritz Schlick of the University of Vienna. The leading philosopher members were Schlick, Rudolf Carnap (who

became the intellectual leader of the group), Otto Neurath (who was also an economist and had been a professor of physics at the University of Prague), Herbert Feigl, Friedrich Waismann, Edgar Zilzel, Victor Kraft, and at a later date Gustav Bergmann; the leading scientists and mathematicians were Philipp Frank, Karl Menger, Kurt Gödel, and Hans Hahn. In 1928 the group was legally organized as the Ernest Mach Association, and in 1929 they issued a monograph written by Carnap, Hahn and Neurath as a kind of philosophical manifesto, Wissenschaftliche Weltauffassung der Wiener Kreis¹ (The Scientific World Conception of the Vienna Circle).² The following year they took over a journal which they named Erkenntnis (Cognition),³ and it became the official mouthpiece of the positivist movement.

An international flavor and missionary zeal were evident in the Circle from its beginnings. Personal relationships of its members, the journal Erkenntnis and other publications, together with a number of international congresses were the means by which close contacts were maintained with philosophers of similar orientation in Great Britain, the Scandinavian countries,

¹(Wien: A. Wolf, 1929).

²Translation by Frank, op. cit., p. 38.

³Translation by Frank, ibid., p. 41. Seven volumes of Erkenntnis were published, 1930 to 1938; volume 9, 1939-1940, was published under the name Journal of Unified Science.

Poland, and the United States. Special mention should be made of a group in Berlin known as the "Society of Empirical Philosophy" whose leading members were Hans Reichenbach, Richard von Mises, Kurt Grelling and Carl Hempel. They worked in close association with the Viennese positivists and Reichenbach joined with Carnap in editing Erkenntnis.

Throughout the thirties the influence of the Vienna Circle and Logical Positivism spread steadily, but the more or less centralized leadership of the movement dissolved.¹ Hahn died in 1934 and Schlick was murdered by a demented student in 1936. The rise of Nazism dispersed the Vienna Circle and the Berlin Society as well. Most of the leading members came to the United States -- Carnap, Bergmann, Feigl, Frank, Hempel, Gödel, Reichenbach, von Mises -- and all, except Reichenbach who died a few years ago, are still teaching in this country. Waismann went to England where he continues to teach.

The Logical Positivists had two principal philosophical aims: first, a negative objective of eliminating metaphysics; second, the positive objective of clarifying

¹The label 'Logical Positivism' was originated in an article by H. Feigl and A. E. Blumberg written in the United States: "Logical Positivism: A New Movement in European Philosophy," Journal of Philosophy, 28, 1931, 281-296.

and securing the foundation of the sciences.¹ They saw these aims as obviously related, for part of the second involved establishing that there were no metaphysical foundations (or presuppositions) of science; of course if metaphysics were eliminated -- i.e., shown to be literally meaningless or nonsensical -- this part of the second objective would also be accomplished. Moreover, it turned out that the means for achieving the destruction of metaphysics, the verifiability criteria (theory, principle) of meaning, was also the basic doctrine for clarifying the nature of science. So it is not surprising that the verifiability principle came to be regarded as the leading tenet of Logical Positivism, and that the attempts to resolve the issues surrounding it shaped the history of this philosophy. Therefore, we will focus our attention in this brief review of the work of the Viennese positivists on these three items: the two aims and the most general means of accomplishing them. But first we must take note of what probably all analytic philosophers would regard as one of the most significant developments in contemporary philosophy, since the Logical Positivists contributed to its achievement and

¹Weinberg, op. cit., p. 1; see also the early papers reprinted in Ayer, ed., Logical Positivism, especially those by Carnap and Schlick. Throughout most of the discussion I will simplify by disregarding the differences among Logical Positivists; only toward the end do I begin to consider them.

they were profoundly influenced by it.

I am referring to the philosophical import of the work of such men as Peano, Frege, Russell, Whitehead and Wittgenstein which clarified the peculiar truth claims of logic and mathematics.¹ Empiricists believed that all knowledge of the world was derived from experience. Yet the propositions of mathematics and logic were regarded by everyone as true, even necessarily true, and they were apparently not based upon experience. No empiricist philosopher was very comforted by J. S. Mill's interpretation of mathematical propositions as empirical assertions which derived their characteristic certainty from the fact that they were so often confirmed by observation. The answer, simply stated, is that the statements of mathematics and logic are indeed true (and certain), but they say nothing about the world. That is to say, they are non-empirical in two senses: they are true independent of experience (or, as is sometimes said, they can be known by reason

¹An excellent account of this analysis of mathematics is given by C. G. Hempel, "On the Nature of Mathematical Truth," American Mathematical Monthly, 52, 1945; reprinted in H. Feigl and W. Sellars, eds., Readings in Philosophical Analysis (New York: Appleton-Century-Crofts, Inc., 1949), pp. 222-237. All of the men mentioned by Hempel as the main contributors to the clarification of the nature of mathematics were listed as precursors by the Vienna Circle. Frank, op. cit., p. 39. There is a discussion of the nature of these developments in mathematics and logic and their significance for Logical Positivism by Weinberg, op. cit., pp. 11-24, 69-103.

alone), and they do not say anything about what might be experienced. So it was conclusively established that the truth of mathematical and logical propositions in no way conflicts with the empiricist principle that all knowledge of the world is based upon experience.¹

It is to the lasting credit of the Vienna Circle that they recognized the philosophical significance of these developments in logic and took them to heart.² Thus, they set out to found empiricism on logical rather than psychological analysis, substituting for Hume's thesis that all ideas were traceable to sense impressions the view that all significant non-analytic sentences could be reduced to observation-statements;³ and in

¹Grossly simplified, this achievement involved three steps: (1) Peano reduced all of mathematics to simple arithmetic; (2) Russell and Whitehead in Principia Mathematica (London: Cambridge University Press, 3 vols., 1910-1913) derived arithmetic from logic; (3) Wittgenstein in his Tractatus Logico-Philosophicus (London: Kegan Paul, 1922) showed by the invention of the "truth-tables" that the truths of logic were all tautologies.

²See, e.g., R. Carnap, "The Old and the New Logic," trans. I. Levi, in the collection edited by Ayer, op. cit., pp. 133-145, which originally appeared in the first issue of Erkenntnis (1930) entitled "Die alte und die neu Logik."

³This will be explained presently. However, I should say now that in the account which follows the term 'reduced' will not be taken literally, and the above assertion would be more consistent with subsequent discussion if it were replaced by the term 'connected.' Reduction to observation-statements, or 'atomic sentences' as Wittgenstein called them, was the main idea of the meaning theory adopted by some members of the Vienna Circle from the Tractatus, but in this brief account we are not going to consider this early viewpoint, though its influence is reflected in the theories or

place of the classical positivists' rejection of metaphysics on the ground of indemonstrability and lack of utility,¹ they sought to demonstrate the impossibility of metaphysics by logical analysis, attempting to show that all metaphysical assertions were cognitively meaningless. Let us examine the character of their arguments.

All discourse can be divided into sentences of two types: those which state something about the world (or "our experience" if you wish to avoid the realist overtones) and those which do not. The first kind (called 'factual,' 'empirical,' or 'synthetic') are obviously cognitively meaningful or cognitively significant; the second, those which say nothing about the world, are either contradictions or tautologies (called 'analytic,' 'logical,' or 'formal'),² or they have no cognitive significance whatsoever. It follows that all meaningful

criteria of meaning we do consider. Actually, this represents a further simplification, for they sometimes confused the idea of a theory of meaning in the sense of an explication of what meaning is, with that of a criterion of meaning in the sense of a statement of a necessary characteristic(s) of all meaningful statements. I have disregarded this confusion. Even though I continue their loose usage of 'theory' in this context, our concern is only with meaning criteria.

¹Compte was not far from the Logical Positivists in this regard; nevertheless, he was not an important influence on the Vienna Circle. See Weinberg, op. cit., pp. 6-9.

²For example, ' $2+3=4$ ' is false and a contradiction, 'A or not-A' is true and a tautology.

statements,¹ those which can be said to be either true or false, are either analytic or factual.

The achievements in logic referred to above provided the basis for an explication of analyticity:² an analytic sentence has the form of either a logical truth (tautology) or a logical falsehood (contradiction); since it is true or false by virtue of its form alone, it says nothing about the world. For example, consider the sentence 'Either John is tall, or he is not tall.' We know that it is true without measuring John, and it does not tell us anything about John's height. It is true because it has the form of the logical truth 'P or not-P'; it is therefore a tautology.

Factual propositions assert something about the world. By making observations we can discover whether what they assert is the case, thus verifying them as true or false. So whether or not any particular sentence is factual depends upon the possibility of verifying it by observation. In condensed form, such is the reasoning

¹I have dropped the rather stilted construction 'cognitively meaningful.' References to meaning or meaningful sentences are ordinarily so understood anyway, as they should be in what follows. When I wish to refer to something else, as for example emotive meaning, I will do so explicitly. Also, the reader may have noticed that I use the words 'sentence' and 'statement' synonymously; I will also use 'proposition' in the same way. These terminological decisions will be adhered to throughout.

²Carnap, op. cit., pp. 141-143.

which leads to the verifiability criterion of factual meaning; the key words are those underlined in the last sentence. It may be formulated as follows: a sentence is factually meaningful if and only if it is possible to verify it by (empirical) observation(s).¹

How does this concern with meaning relate to the objectives we noted above? First, with respect to the elimination of metaphysics the connection is not diffi-

¹In the form: "the meaning of a statement lies in the method of its verification," the verifiability principle became a slogan for characterizing Logical Positivism. Like many slogans, by itself, this one is misleading. Nobody, as far as I know, ever subscribed to its literal interpretation that a statement's meaning is the steps one must go through in order to determine its truth or falsity. It appears this way in an influential article by Carnap, but the import of his discussion of it is, I believe, rendered more clearly by my formulation. See: Carnap, "Überwindung der Metaphysik durch Logische Analyse der Sprache," Erkenntnis, 2, 1932; trans. A. Pap and reprinted in Ayer, ed., Logical Positivism, pp. 60-81, at p. 76.

For illuminating discussions of the verifiability theory, see: Ayer, Language, Truth and Logic, pp. 5-16, 33-45; Passmore, op. cit., Chap. 16; H. Reichenbach, "The Verifiability Theory of Meaning," Proceedings of the American Academy of Arts and Sciences, 80, 1951; reprinted in Feigl and Brodbeck, eds., op. cit., pp. 93-102; C. G. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning," Revue internationale de Philosophie, 11, 1950, reprinted in L. Linsky, ed., Semantics and the Philosophy of Language (Urbana: University of Illinois Press, 1952), pp. 161-185; and "The Concept of Cognitive Significance," Proceedings of the American Academy of Arts and Sciences, 80, 1951, 61-77; the criticism of Hempel by G. Bergmann, "Comments on Professor Hempel's 'The Concept of Cognitive Significance,'" in the same volume of Proceedings, 78-86.

cult to discern. In the writing of metaphysicians we find such expressions as "the Absolute," "the being of being," "non-being," "thing in itself," "absolute spirit"; and propositions like the following:

The Absolute enters into, but is itself incapable of, evolution and progress. (F. H. Bradley)

The Absolute is the Essence. This is the same definition as the previous one that the Absolute is Being, in so far as Being likewise is simple self-relation. But it is at the same time higher, because Essence is Being that has gone into itself: that is to say, the simple self-relation (in Being) is expressly put as negation of the negative, as immanent self-mediation. (Hegel)

What is to be investigated is being only and--nothing else; being alone and further - nothing; solely being and beyond being--nothing. What about this Nothing? . . . Does the Nothing exist only because the Not, i.e., the Negation, exists? Or is it the other way around? Does negation and the Not exist only because the Nothing exists? . . . We assert: the Nothing is prior to the Not and the Negation . . . Where do we seek the Nothing? How do we find the Nothing . . . We know the Nothing . . . Anxiety reveals the Nothing. . . . That for which and because of which we were anxious, was 'really' - nothing. Indeed: the Nothing itself - as such - was present . . . What about this Nothing?--the Nothing itself nothings. (M. Heidegger)¹

¹The statement by Bradley was taken from his Appearance and Reality by Ayer as an example of "a metaphysical pseudo-proposition," Language, Truth, and Logic, p. 36. The passage by Hegel is from "The Doctrine of Essence," Encyclopaedia of the Philosophical Sciences

(I originally intended to quote only the examples used by Ayer and Carnap, when I remembered the fitting quotation from Hegel. It serves to explain (!) the use of 'the Absolute' by Bradley and it supplements so well the selection from Heidegger--for now we have "Being that has gone into itself . . ." as well as "The Nothing [which] itself nothings." Having thus accounted for both Being and Nothing, do we not have Completeness which turns into itself and completes?! Later it shall be clear that I do not accept the wholesale rejection of metaphysics characteristic of many analytic philosophers, but when it comes to writing of the kind exemplified above I at least strongly sympathize with them.)

When we see, in philosophical writing, a statement containing words like those listed above or a statement like the above illustrations, we should ask, first, whether it is analytic. (A not unreasonable query, since metaphysicians who write this way usually claim to be asserting certain or necessary propositions, and, we

(London: Oxford University Press), trans. W. Wallace and reprinted in J. Loewenberg, ed., Hegel (New York: Charles Scribner's Sons, 1929), p. 129. Heidegger's selection from Was Ist Metaphysic? is quoted here from R. Carnap, "The Elimination of Metaphysics Through Logical Analysis of Language," trans. A. Pap and reprinted in Ayer, ed., Logical Positivism, pp. 60-81, quoted from p. 69; originally entitled "Überwindung der Metaphysik durch Logische Analyse der Sprache," and published in Erkenntnis, 2, 1932. My discussion of the Logical Positivists' handling of metaphysics reflects the content of this article.

remember, analytic statements have this quality.) If so, it does not tell us anything as its writer seems to claim; if not, then it must be intended as a factual proposition, so we may legitimately inquire concerning the observations which would verify it. And if there is no possible answer to this question, as is the case with practically all of the statements called metaphysical, we must conclude that the statement is without sense, i.e., it is literally meaningless. In those cases where the relevant observations can be specified, what, other than confusion, is gained by calling them metaphysical?

I have tried to show something of the manner in which the Logical Positivists sought to accomplish one of their two major aims, the refutation of metaphysics. The constructive side of their philosophical efforts, clarification of the foundations of science, is what we know as the philosophy of science. Since this is our main preoccupation throughout the thesis and, in this, I follow the Logical Positivists, our discussion at this point will be quite succinct.

Just as metaphysics was to be discredited by its failure to measure up to the verifiability criterion of meaning, so the basic methodology or logic of science was to be clarified and secured by showing that it must always produce propositions consistent with that criterion.

Remnants of metaphysics within the sciences were, in the same manner, to be purged. As an illustration, I will give a schematic account of the analysis of the concept of causality. The task is to clarify the meaning of statements of the form 'P is the cause of Q,' where P and Q are propositions describing events such as 'This water is heated' and 'This water is boiling.' Propositions like P and Q are unproblematic; we know the observations which would verify them, but what about the longer statement? We can observe P and Q, but how can we observe the alleged causal connection between them? Clearly, we cannot; so at first it might appear that causal statements in science are just as unverifiable, just as meaningless as those of metaphysics. However, further analysis reveals that the meaning of propositions like 'P is the cause of Q' is fully rendered only by at least three propositions: a law of the form 'Whenever P, then Q' and two singular descriptive propositions, P and Q, together constituting a valid deductive argument whose conclusion is Q. In this way, use of the initially problematic term 'cause' within science is shown to be unproblematic, and any temptation to interpret scientific causality by the metaphysical notion of "necessary connection" is eliminated.¹

¹See, e.g., M. Schlick, "Causality in Everyday Life and in Recent Science," University of California Publications in Philosophy, 15, 1932; reprinted in H. Feigl and W. Sellars, eds., Readings in Philosophical Analysis (New York: Appleton-Century-Crofts, 1949), pp. 515-533.

To conclude our discussion of Logical Positivism, we will take a look at some of their major philosophical problems and -- in my estimation -- deficiencies. This will serve as a preparation for a brief comment upon the present status of analytic philosophy. In the discussion above we employed the verifiability criterion of meaning in a rough and ready way without raising any questions about it. With respect to their treatment of metaphysics Logical Positivists proceeded in the same way, but when it came to the analysis of science, serious difficulties arose. In the first place, it was soon apparent that insistence upon conclusive verifiability, the "strong" sense of the term, was too restrictive. Most obviously, it ruled out all scientific laws for they were universal propositions about the past, present, and future; all of the infinite number of cases to which they applied could not be observed. To remedy this, verification was reinterpreted in a "weak" sense, so the criterion required that a factual statement need only be capable of being rendered more or less probable by observational evidence. This would serve to admit the statements of science as well as those of common sense

and still preclude metaphysics.¹

Another kind of difficulty which arose concerning the idea of verifiability by observation was more serious, and--as I see it--the response to it by Logical Positivists (and other analytic philosophers as well) has contributed to some of the main schisms in the analytic movement today. Verifiability by observation meant verifiability by evidence consisting of, or reported by, observation-statements (basic sentences, or protocol sentences). In their earliest writings the Viennese group took it for granted that what was described by the observation-statements were experiences and, quite consistent with the classical empiricist tradition (as expressed in the writings of Locke, Berkeley, and Hume), they regarded experiences as mental states. The difficulties arose when they came to question the consistency of these views with the inter-subjective (objective) character of science. The dialectic goes something like this: the

¹See Ayer, Language, Truth and Logic, pp. 36-39. Hempel, op. cit., p. 46, argued that the verifiability criterion did not exclude metaphysical statements, for given a statement 'C or D' where C is a verifiable proposition and D is a metaphysical (unverifiable) one, then 'C or D' would be verifiable and true if C were true. (A disjunction is true when either of its component propositions is true.) I disregarded this technical difficulty, as it may be overcome by adding to the verifiability criterion the stipulation that for compound sentences it is to be applied to each of the component sentences separately. In this instance we would conclude 'C or D' is meaningful, but D alone is not. Another advantage of this construction is that we avoid the anomaly which would arise if C were false, for then all we could say of the disjunction as a whole would be that it is meaningless.

meaning and truth of scientific propositions are ultimately dependent upon observation-statements: these refer to mental states, so they are subjective in the obvious sense that nobody can observe the contents of anybody's mind but his own; science is, therefore, fundamentally subjective. No Logical Positivist would accept this conclusion, but most of them took the argument seriously. The former response was correct; the latter was mistaken. This, I hope to show incidentally in the next chapter. For now, we will take a brief look at some of the replies by Logical Positivists.

If we accept the validity of an argument but reject its conclusion, logic requires that we also reject at least one of its premises. The premise called into question here concerned the meaning (reference) of observation-statements. As noted above, it was believed that if they referred to the content of the observer's experience (mental state, sense data) subjectivity resulted; so one position taken (e.g., by Schlick) was that the observation-statements refer only to the structure of experience.¹ In favor of this view, it was argued that even though I cannot know the content of your experience when you say, e.g., that the fourth band in the rainbow is green--i.e., I cannot know whether the quality of your experience which you call

¹See Passmore, op. cit., pp. 374-378.

'green' is the same as what I call 'green,' I do know that you apply language in the same manner as I do and this shows that your experiences are structured the same as mine. Therefore, what is communicated by language is the structure of experience and this is not subjective.

This argument, as I have illustrated it, is not very convincing. It could be stated more persuasively, as it was by Locke when he distinguished primary and secondary qualities: primary qualities such as quantity, extension, and spatial location are objective constituents of the world; secondary qualities like sound and color are the products of our own minds.¹ But just as Berkeley demonstrated the speciousness of this distinction,² so did Neurath show the error in Schlick's form-content dichotomy. The spatial relations of the fourth band of a "rainbow experience" are no less a part of the content of that experience than is the color of the band. If the latter is private, subjective, incommunicable, so is the former.³ Neurath's own conception of the nature of observation-statements deliberately omitted

¹An Essay Concerning Human Understanding, ed., A. C. Fraser (Oxford: Clarendon Press, 1894), Bk. II, Chap. 8

²Principles of Human Knowledge, pars. 8-20, in The Works of George Berkeley, Bishop of Cloyne, eds., A. A. Luce and T. E. Jessop (London: Thomas Nelson & Sons, 1949).

³Passmore, op. cit., p. 378; and Ayer, "Editor's Introduction," Logical Positivism, pp. 18-20.

any reference to experience. For Neurath, the observation-statements upon which all the other statements of science are founded refer only to physical objects. These are entirely public, so there is no problem concerning the objectivity of science.¹

Neurath's interpretation of observation-statements is one way of stating one version of the thesis of physicalism. This doctrine was subject to considerable controversy among Logical Positivists but, in one form or another, most of them subscribed to it. Its discussion will complete our brief account of Logical Positivism. First, I will restate the thesis given above in another form and then indicate a more radical version of it. Consider a language in which all of the descriptive words (the other kind of words are "logical") refer ultimately to either physical objects or characteristics of physical objects; call this the "physicalist language." Physicalist thesis₁ is the assertion that all scientific propositions can be stated in the physicalist language. Thesis₂ says that a complete description of the world (everything there is) can be stated in such a language. When Phillip Frank wrote that

¹Ayer, "Editor's Introduction," Logical Positivism, p. 20. Neurath did not state the matter so simply, but this interpretation seems clearly implied in his paper: "Protocol Sentences," Erkenntnis, 3, 1932-33; reprinted in Logical Positivism, pp. 129-208.

physicalism did not represent any change in the philosophy of the Vienna Circle,¹ he must have been thinking of thesis₁. Considering physicalism as thesis₂, Julius Weinberg described it as an entirely new system of philosophy.² Allowing their different interpretations, I believe they were both correct.

Thesis₁ amounts to no more than what I regard as scientific common sense. Leaving aside details, such as the manner in which various kinds of scientific concepts are connected with terms referring directly to physical objects, all analytic philosophers would probably subscribe to it. However, thesis₂ represents a radical innovation. It is itself a metaphysics of the materialist variety, which Gustav Bergmann referred to as the "silliest of all philosophies."³ Not only does it imply that there are no values, but it involves a conception of the world devoid of mind. This is such a patent contradiction of common sense, and remembering that most Logical Positivists tend to explicitly reject all metaphysics, I hesitate to ascribe it to any of them. Nevertheless, I agree with Bergmann that "a

¹Op. cit., p. 36.

²Op. cit., p. 27 and pp. 227-288.

³"Logical Positivism, Language, and the Reconstruction of Metaphysics," Rivista Critica di Storia della Filosofia, 8, 1953, 453-481; reprinted in The Metaphysics of Logical Positivism, pp. 30-77, at p. 48.

crude metaphysics implicitly held ... is the price every philosophy that explicitly rejects metaphysics must pay."¹ However, by including the formulation of physicalism as thesis₁, I leave open the possibility that some philosophers may ascribe to it, refuse to adopt any explicit metaphysical positions and still avoid adopting, even by implication, thesis₂; i.e., they might, in effect, say, "I am interested only in the philosophy of science; I have nothing to say about metaphysics." Which particular analytic philosophers belong to either of these classes, I would rather not attempt to say. In any event, with its physicalist phase, most Logical Positivists came to regard philosophy as practically coextensive with the logic or methodology of the sciences.

This is a good point at which to begin those promised remarks describing the current divisions or groupings within analytic philosophy. Logical Positivists may be divided into those who identify philosophy with the philosophy of science, as we just mentioned, and those who do not. Following Bergmann, I will call the

¹Ibid., p. 64. See also M. Brodbeck, "Philosophy in America, 1900-1950," in M. Brodbeck, J. Gray, and W. Metzger, American Non-Fiction, 1900-1950 (Chicago: Henry Regnery Co., 1952), pp. 81-83.

first group Formalists and the second Reconstructionists.¹ I will say more about the difference between them shortly, but first we will label the other groups and take note of one obvious similarity and some related points of agreement, not only among Logical Positivists, but among all analytic philosophers. In addition to Logical Positivism there is another major wing of analytic philosophy which is often called Ordinary Language Philosophy. It is the dominant form of British philosophy. I am less interested in the divisions within it, but I shall distinguish two groups: Casuists and Therapeutic Philosophers.²

¹G. Bergmann, "Logical Positivism, Language, and the Reconstruction of Metaphysics," Revista Critico di Storia della Filosofia, 8, 1953, 453-481; reprinted in The Metaphysics of Logical Positivism, pp. 30-77. Bergmann is, without any doubt, the intellectual leader of Reconstructionism. Most of the accomplishments of this kind of philosophy are reflected in the two collections of Bergmann's papers cited in footnote 1 on the opening page of this chapter. See also the writings of such American philosophers as Nelson Goodman, Herbert Hochberg, Willard V. Quine, and the British positivist, Everett W. Hall. Rudolf Carnap is probably the most outstanding Formalist. For excellent examples of the work of Carnap and others of this group which relate to social science, see the two volumes published by (1) H. Feigl and M. Scriven, eds., The Foundations of Science and the Concepts of Psychology and Psychoanalysis (Minneapolis: University of Minnesota Press, 1956); (2) H. Feigl, M. Scriven, and G. Maxwell, eds., Concepts, Theories, and the Mind-Body Problem (Minneapolis: University of Minnesota Press, 1958).

²For a general discussion of the divisions within analytic philosophy see, in addition to Bergmann, ibid., A. Pap, Elements of Analytic Philosophy (New York: The Macmillan Co., 1949), pp. v-xi; Brodbeck, loc. cit., pp. 70-87; and Ayer, "Editor's Introduction," Logical

Now for the obviously common feature. All analytic philosophers regard philosophy as a method for achieving clarification rather than as a means of discovering some special kind of knowledge. (If anyone claimed that the clarification of our ideas does, in a reasonable sense, add to our knowledge, they would not argue.) Therefore, the name analytic philosophy; the other kind is speculative.

Logical Positivists, as we said above, distinguished their philosophy from classical empiricism and positivism by their use of logical analysis to establish similar theses. This is the most often used name for the method. Exactly what is the nature of "logical analysis" is difficult, if not impossible, to say. It is best described by illustration. I tried to illustrate it above by outlining the analysis of the concept of causality, and I discussed it with reference to the philosophy of science in the first chapter. Perhaps I can add to our

Positivism, pp. 7-10, 26-28.

For Ordinary Language Philosophy, see the summary description by Passmore, op. cit., Chap. 18. Short articles by seven members of this group are in A. J. Ayer, et. al., The Revolution in Philosophy (London, Macmillan, 1957); Ayer, by the way, is a Logical Positivist who presently probably belongs among the Reconstructionists. A more technical presentation of their views is by J. M. Urmson, Philosophical Analysis (London: Oxford University Press, 1956). For a criticism focusing upon Urmson's book, see Bergmann, "The Revolt Against Logical Atomism," The Philosophical Quarterly, 7, 1957, 323-339, and Vol. 8, 1958, 1-13; both reprinted in Bergmann Meaning and Existence, pp. 39-72. L. Wittgenstein, Philosophical Investigations (New York: The Macmillan Co., 1953), is a posthumous publication of notes written by the most influential founder of this philosophy.

understanding of it at this point by explaining what is meant by calling it "linguistic"; in this, too, all analytic philosophers agree. First, logical analysis is linguistic in the sense that it is applied to language, though it is more accurate to say that it proceeds through the analysis of language, since it is not concerned with language per se. Notice the way Logical Positivists emphasize the difference between two kinds of sentences, and the importance they attach to the verifiability criterion, a doctrine about those sentences which we call factual. Second, it is evident that philosophers say strange things like "The world is will and idea" or "There are no physical objects"; the first step is to discover what, if anything, such statements mean; this inquiry is linguistic. Third, it is evident that genuinely philosophical disputes are not resolved by collecting data or performing experiments. In this sense, they are all verbal or linguistic.¹

To say that philosophical problems are linguistic is not to say that empirical data is irrelevant to their solution; it is just that, for philosophy proper at least, we have all the data we need from ordinary experience. For philosophy of science we must have some knowledge of

¹For a more thorough discussion of this point, see Bergmann, "Logical Positivism, Language and the Reconstruction of Metaphysics," The Metaphysics of Logical Positivism, pp. 30-77, especially pp. 33-36.

science, but it is usually of a readily available and non-controversial kind. Where the latter is not true, as in the case of the most advanced frontiers of quantum physics and the largely undeveloped areas of social science, it is because in these cases philosophical and scientific problems are so easily blurred into each other. I think, by the way, that this is one reason why logical analysis in political science can be most effectively performed by a person trained in this discipline.

Limiting ourselves to cautious generalities, there are other points of agreement among analytic philosophers closely related to their common conception of philosophy as analysis. They are all empiricists of sorts; that is, they all hold that knowledge of the world stems from experience and they adhere to some kind of empiricist criterion of meaning. Therefore, they all reject, after a fashion, classical or traditional metaphysics. These broad similarities are indeed significant but in certain respects they are rather shallow; at least, they do not obscure fundamental differences both in their style of analysis and the subjects to which they apply it.

The difference among Logical Positivists is that one group, the Formalists, has, for all practical purposes, given up all concern for traditional philosophical problems; while another group, the Reconstructionists, regards them as of primary importance.

Reconstructionists, as we said, also reject traditional metaphysics; they consider it as literally nonsensical. But they regard the principal task of philosophy as the restatement of traditional philosophical problems and the reconstruction of traditional metaphysics. In the process they try to resolve the problems and recover the meaningful core of the metaphysical positions. The change brought about by the analytic movement is, as they regard it, a change in method only, though the new method often changes the old metaphysics beyond recognition.¹ Since I am myself committed to Reconstructionism, I will have occasion to illustrate its application to traditional metaphysics in the following chapter.

Both groups of Logical Positivists regard the construction of schematic or ideal languages by the use of formal logic as an important philosophical tool, though Reconstructionists are less apt to use it in philosophy of science. This is why Logical Positivists are sometimes referred to as "ideal linguists."² The contrast with the name "Ordinary Language Philosophy" is significant. British analysts are not interested in symbolic logic, neither in itself, nor as a philosophical tool. This is one connotation of their name. Another is the central place ordinary, everyday language plays in

¹Ibid., pp. 30-32.

²Ibid., p. 32.

their philosophy. On the one hand, traditional philosophical problems are to be resolved through an investigation of ordinary discourse. Philosophers become entangled in philosophical puzzles concerning the concepts referred to by such words such as 'knowledge,' 'truth,' 'existence.' Once we discover how such words are actually used, these philosophical ills are cured by showing how philosophers deviate from customary usage. Such is the linguistic therapy administered by the therapeutic group of Ordinary Language philosophers.¹

On the other hand, Casuists--the other major group of Ordinary Language philosophers--carry this concern with ordinary discourse even further. They investigate language not only as a means of curing philosophical ills, but as an important subject in its own right.²

¹An outstanding member of this school has stated the aim of their type of analysis as "that of coming to understand philosophically puzzling concepts by carefully and accurately noting the ways in which the related linguistic expressions are actually used in discourse." See P. F. Strawson, "Construction and Analysis," in Ayer, et. al., The Revolution in Philosophy, pp. 97-110, at p. 104.

²For illustrations and a short account of the manner of such inquiry, see G. J. Warnock, "Analysis and Imagination," in Ayer, et. al., The Revolution in Philosophy, pp. 111-126. Ordinary Language Philosophy has been criticized for its tendency to substitute social science for philosophy. Warnock dismisses this contention lightly as merely a question of the application of a name (p. 116). I cannot share his levity at this point; when scientific inquiry proceeds under the name of philosophy, it is not likely to be accompanied by systematic research. Philosophers have not been data collectors. In this, the British analysts still uphold the tradition.

A. J. Ayer believes the greatest contribution of Ordinary Language Philosophy has been in this area. He stated, "To my mind, the main achievement of the 'Ordinary-language school' has been their examination and dissection of the 'unscientific' uses of language."¹

Current analytic philosophy is considerably more variegated than my brief account might lead one to suspect; at the same time, the groupings within it are not so sharply distinguished as I have made them appear. However, rather than attempt to make more accurate what I have already said, I will use these final remarks to comment upon three omissions which for one reason or another did not fit into the above discussion. First, I did not include pragmatism within the analytic movement though there are some structural similarities. This was partly a matter of convenience, for to have included it would--as I see it--have required that I qualify in some way every generalization that I made about analytic philosophy. Of course, if this is true, there are other grounds for its exclusion.²

Second, I did not mention ethics when discussing Logical Positivism. The omission must have been

¹"Editor's Introduction," Logical Positivism, p. 28.

²For discussion of this point, see: Brodbeck, loc. cit., pp. 77-78 et passim; and Bergmann, loc. cit., pp. 59-63.

surprising to political scientists, for whom Logical Positivism has been considered more than anything else as a philosophical position regarding values. Therefore, it might also be surprising for some to learn that they have written very little on the subject. This might have been an excuse for omission; actually, mine was the fact that I will consider their views on values in a chapter on that topic. For now, I will only record that all analytic philosophers are, with respect to ethics, relativists in the sense that they regard the fact-value distinction as significant, and also in the non-technical sense in which a relativist is one who denies that there are moral maxims which hold for all persons at all places for all time.

Third, concerning structural history which we largely ignored, I wish to emphasize the important influence upon analytic philosophy of the Cambridge School of Analysis, predominantly Bertrand Russell, G. E. Moore, and Ludwig Wittgenstein.¹ Logical Positivism was, in part, a reaction against the prevailing idealist metaphysics (primarily Hegelian) which dominated Continental philosophy. Before the Vienna

¹Of these three men, Bergmann wrote: "Virtually all living linguistic philosophers are either directly or indirectly students of one of them." See, "Two Types of Linguistic Philosophy," The Review of Metaphysics, 5, 1952, 417-38; reprinted in The Metaphysics of Logical Positivism, pp. 106-131, at p. 106.

Circle was formed, Russell and Moore had already led a successful attack on Hegelian metaphysics in Great Britain.¹ Moore's insistence upon common sense as a philosophical starting point and his emphasis upon the necessity of achieving linguistic clarity, remain as pervasive features of analytic philosophy. The importance of Russell's work in logic has already been mentioned. Regarding his other contributions, I can only register my concurrence with those who have suggested that Russell probably contributed more than anyone else to contemporary philosophy.² Both major divisions of analytic philosophy list Wittgenstein as a founder. Logical Positivists consider his Tractatus Logico-Philosophicus³ as a basic book; the posthumously published Philosophical Investigations,⁴ which records the changes in Wittgenstein's views beginning about 1929, has

¹See Passmore, op. cit., Chap. 9. Some of their most significant papers in this regard are contained in two collections: B. Russell, Logic and Knowledge, ed. R. C. Marsh (New York: The Macmillan Co., 1956); and G. E. Moore, Philosophical Studies (London: Routledge & Kegan Paul Ltd., 1958).

²See, for example, the judgments by R. C. Marsh, op. cit., p. 365, and by Russell's biographer, Allan Wood, in a fragment of a second book he was writing about Russell at the time of his death which is included in: B. Russell, My Philosophical Development (London: George Allen & Unwin Ltd., 1959) at pp. 257 and 260.

³(London: Routledge & Kegan Paul Ltd., 1922).

⁴(New York: The Macmillan Co., 1953).

the same status for Ordinary Language philosophers. We may note that prior to writing the Tractatus, which was submitted as a Ph.D. dissertation, Wittgenstein had been a student of Russell and Moore.

In the chapter which follows, I shall, in a sense, begin anew the discussion of analytic philosophy. Only this presentation will be in the form of an argument rather than a report upon the views of others. It will not cover identical material but if some of what it contains seems already familiar, it may be that the present section was not wholly unsuccessful. And perhaps what we have covered here will become clearer during what follows. But continuing the exposition of analytic philosophy is only an incidental goal of Chapter Three, its primary goals are (1) to develop the most basic ideas of the "philosophical basis for the analysis of methodological problems in political science" referred to in Chapter One, and (2) to begin to show their efficacy for this purpose. It so happens that these goals, as I view them, require further exposition of analytic philosophy.

CHAPTER THREE

PHILOSOPHICAL BASIS:

COMMON SENSE, ANALYTIC PHILOSOPHY AND METHODOLOGY

In this chapter I shall introduce some of the most fundamental of those more or less philosophical ideas which I believe are required for the logical analysis of political science. The strategy I have chosen is to present a general point of view and what may be called a philosophical style which together reflect the Reconstructionist variant of analytic philosophy; and to proceed by employing the style to support the point of view in such a way as to include presentation of the ideas in which we are interested. At several places, the connections with political science will be made explicit.

The point of view may be summed up in the following propositions. Science and philosophy both start from common sense. Scientists take it for granted and extend it. Philosophers analyze and elucidate the propositions of common sense, but they do not dispute them. Indeed, an important task of philosophy is to clarify assertions which apparently contradict common sense, but nevertheless deserve our serious attention. To carry out such analysis is, I believe,

an excellent way of both showing what is involved in these claims for common sense and, at the same time, defending them. In what follows, we shall make use of this kind of indirect exposition and argument. By the way, one reason for calling such an approach indirect is that, as we shall see, it starts from the position it seeks to establish. But what else can it do, when the position it defends says it cannot do otherwise? What **actually** happens is that one supports some common-sense propositions by showing their connection with other more obvious ones. This is no shortcoming. It merely reflects the folly of any claim to a wholly presuppositionless philosophy.

Before proceeding with the program outlined above, one important reservation is in order. I am not going to defend the point of view expressed there as it relates to philosophy proper. I will, however, attempt to make it plausible by a few related comments at several places in the discussion and by a short illustration of the way it is manifested in the Reconstructionist approach to philosophy proper. At this point, I will only say that one important result of analytic philosophy has been the demonstration that in both science and the philosophy of science one need not say anything about philosophy proper. I hope that one result of our failure to take full advantage of

this achievement -- as evidenced in the last chapter and in some places in this one -- may be that we do not have to accept it wholly on faith or authority, and that we need not reject traditional philosophy in order to accept it on any basis.

The philosophical style is empiricist, linguistic, and analytical. We may approach it by first turning to a consideration of the language in which our knowledge is stated: i.e., to ordinary language, including both everyday language and the language of science. That we do in fact describe the world in which we live in ordinary language is not open to controversy. It is part of that common sense we all share. So is that feature of our language which enables it to function in this manner;¹ a feature that happens to be a central element in the kind of analysis I wish to introduce. We may get at it by attending to the descriptive words and inquiring into the way in which they acquire referential

¹More precisely, we are speaking of that feature which makes a system of symbols a language, but we do not need this kind of precision at this point.

meaning.¹

At first we might be tempted to say that their meaning is assigned by definitions, as in a dictionary. But if we reflect on the use of dictionaries, it is soon apparent that, though the meanings of most words may be specified in this way, all of them could not be. If we look up a term in a dictionary, then look up the terms appearing in its definition and continue in this way, the same words begin to reappear. Dictionaries are circular; they cannot be used unless we already know the meanings of many words. There is a limit to the extent to which the meanings of words can be specified verbally; ultimately we must come to some which are more directly attached to reality.² That there are such

¹We must be careful of the ambiguity of the word 'meaning'; much poor philosophy and methodology can be traced to an unexamined notion of meaning. I use it always in its referential or designative sense; to emphasize this usage, I sometimes speak of 'referential meaning.' Concerning a related subject, also note that as I speak of definitions they are always verbal. And, to emphasize this usage, I once again occasionally employ a redundancy -- 'verbal definition.' Operational definitions are in this sense verbal, they are given in language. Sometimes the non-verbal procedure of indicating what a word means by pointing is called 'ostensive definition.'

²Herbert Hochberg uses this argument to make the same point in "Axiomatic Systems, Formalization, and Scientific Theories," in Symposium on Sociological Theory, ed. L. Gross (Evanston, Illinois: Row, Peterson, and Co., 1959), pp. 412-414. For a somewhat different argument, see J. Hospers, An Introduction to Philosophical Analysis (New York: Prentice-Hall, Inc., 1954), pp. 56-62.

undefinable words in our ordinary language is evident once some of them are pointed out. Consider the following pairs of relational terms:¹ (1) above, below; (2) to the right of, to the left of; (3) before, after. The meaning of no more than one of each of these pairs can be given verbally; e.g., if you know what 'above' means, 'below' may be defined as follows: 'X is below Y' is equivalent by definition to 'Y is above X.' But try defining either without assuming the meaning of the other. It cannot be done. The color words illustrate the point even more obviously; their ordinary meaning cannot be rendered verbally. A blind man born blind could not possibly be taught the meaning of 'blue.'²

¹Relational terms refer to relational properties (characters, characteristics); the other kind are non-relational. The distinction is most precisely made syntactically: non-relational predicates have only one logical subject -- e.g., 'X is red' or 'X is square'; relational predicates require two or more logical subjects -- e.g., 'X is to the left of Z' or 'Y is between X and Z.' There is a tendency to think that relational properties are less "concrete" (more "abstract") than non-relational properties. This seems to be a result of the assumption that what is point-at-able is concrete. However, we can no more point at red than we can between. What we point to in both cases is an object or objects which exemplify the property. Historically, the suspicion of relational properties was connected with the fact that they could not be handled in Aristotle's logic.

²A clever colleague once pointed out to me that this would no longer be true if it were discovered that stimulating a particular place in the brain caused a subject to experience blue sensations. However, this would not change the nature of the word 'blue'; it would only mean that my often used illustration was no longer valid.

We have not yet described that feature of our language by virtue of which it may be used to describe our world, or -- to say the same thing differently -- the source of the referential meaning of its descriptive vocabulary. However, it is clear that we only have to answer for part of that vocabulary -- the undefined terms, for the meaning of all the rest can be given by verbal definition, and if the process of defining is continued, it ends always at the undefined terms. The answer, or the feature, is that we know the meaning of the undefined words by "direct acquaintance." In this way our language is, in a manner of speaking, tied to the world and given its descriptive capacity.

That our language is connected to reality by direct acquaintance (or immediate experience) is the fact I referred to earlier as a central element in the style of analysis I intend to introduce. Of equal importance is consideration of what it is with which we are directly acquainted. Speaking quite commonsensically, it is obvious to me (though, ultimately, each person must verify this for himself) that we are acquainted directly with two kinds of things: (1) physical objects and some of their properties (including, of course, relational properties), and (2) mental objects and some of their properties. The first includes such familiar objects

as pencils, tables and people (as the physiologist regards them);¹ and properties such as the colors of pencils, the hardness of tables, and the relation of juxtaposition exemplified by two people. The second includes such no less familiar objects as percepts, feelings, and thoughts; and properties such as the colors of percepts (i.e., mental not physical colors),² the warmth of feelings, and the relation of temporal succession exemplified by several thoughts. To say that we are directly acquainted with such things is, of course, to say both that they exist and that we know they exist. All of this is -- as I regard it -- a familiar part of

¹It is obvious that the words 'pencil,' 'table,' and 'person' are definable, yet I said we are directly acquainted with the things to which these words refer. This does not conflict with what was stated above. I only claimed that we must, logically, be directly acquainted with the things referred to by those words which cannot be defined; actually we are so acquainted with many other things.

²In our everyday language we often use the same terms to refer to both mental and physical things. We frequently make the distinction by various locutions such as the difference expressed by 'It was hot' and 'It felt hot,' or 'Mary's hair is green' and 'Mary's hair appeared to be green.' The underlined word in the first of each of these pairs refers to a physical characteristic; in the second, to a mental characteristic. It is true, of course, that the second of each of these pairs of statements is often intended to imply what is stated in the first, but what each one states is quite different. As a result of the use of such phraseology and through the help of context, this dual usage of many words does not ordinarily cause confusion, but I think it does sometimes cause trouble in the philosophy of the social sciences. We will meet with this matter below.

common sense.

It is no accident that in discussing ordinary language I also talked about common sense; they are intimately related. Having made explicit, in this way, some of what I regard as common sense, I can now elaborate the point of view expressed earlier with respect to science. To do so is to engage in the philosophy of science. Philosophy proper will be referred to later when we consider some possible objections to what I am about to say about the logic of science.

I said earlier that science starts from common sense, takes it for granted, and extends it. The basis for this claim is suggested by our discussion of ordinary language. I will state it in two parts. First, the descriptive words (synonym: concepts) of the sciences to be meaningful must be verbally connected to the terms of a basic vocabulary which we understand by direct acquaintance. (This is what philosophers call the "principle of acquaintance.") The basic vocabulary is made up of part, but not all, of the undefined terms of ordinary language. This is why I rather offhandedly remarked earlier that the language of science is included in ordinary language. Our direct acquaintance with the referents of the undefined terms of ordinary language is commonsensical. That is why I just referred to a principle of concept formation as evidence that science starts from common sense.

Incidentally, the undefined terms of ordinary language excluded from the basic vocabulary of science are those referring to mental things. This is a rather unceremonious way of asserting methodological behaviorism regarding psychology. We will discuss it in the latter part of this chapter.

Just as the meaning of scientific concepts is ultimately derived from direct acquaintance, so the evidence which supports scientific laws and theories is ultimately derived from direct observation. And what the scientist observes directly are individual facts expressible by grammatically simple sentences whose only descriptive words are those of the basic vocabulary;¹ I will call them basic sentences. Such sentences as: 'The water is placed over the fire'; 'The water boils'; 'The pointer on the instrument is coincident with the number 212'; 'The subject was placed before a stationary light in a dark room'; 'The subject responded: "The light moved four inches."' That we do indeed learn about the world by direct observations or -- to say the same thing differently -- that basic sentences are generally true, the

¹More cautiously, I should say that reports of direct observations employ only words whose referents we are acquainted with, but which may be nevertheless definable; and they may be defined in other contexts. This kind of a blur does not make any difference, for we are interested in the principle involved and I believe that is clear enough.

scientist takes for granted. It is part of the common sense from which he starts. This is the second part of what I called the basis for the assertion that science proceeds from common sense.

From here on we shall continue as I indicated earlier by considering various challenges of what I have said so far. The argument will take us to philosophy, psychology, astronomy, and finally -- if we do not become too lost along the way -- to political science. Most of these places we will visit more than once, so we will not stay very long at any of them. I hope we may be saved from the superficial observations of a hurried tourist by going always with a limited purpose in mind: to add an additional strand to a single rope which may help us to secure political science to the bedrock of common sense, alongside of philosophy and the other sciences.

We will mention first a general challenge of our whole common-sense standpoint. I refer to the argument based upon the fact of perceptual error. After recounting a variety of illustrations of cases in which the ordinary evidence of our senses is mistaken -- illusions, delusions, hallucinations, etc. -- the philosophical twist comes when it is suggested that perhaps we are always mistaken. If our senses sometimes mislead us,

how do we know they do not always mislead us?

I am sure that everyone has come across this argument in one form or another. As employed against common-sense, it is easily refuted. We need only point out that it would make no sense to speak of perceptions which were mistaken if there were not also perceptions which were correct. It is only by virtue of the former that we know of the latter.¹ Consider, for example, the well-known difficulty of correctly judging the comparative length of lines by ordinary observation in the Mueller-Lyer diagram (Fig. 1).

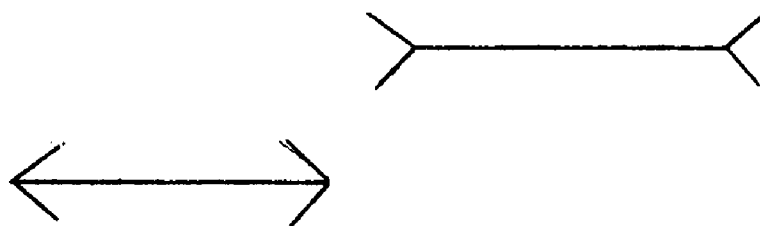


FIG. 1

The diagram creates the illusion that the lines are of different lengths. We know this is an illusion only because we know the lines are actually of the same length; and we know the latter by reason of non-illusory perception, though, in this case, its achievement requires

¹For a very thorough use of this argument against various forms of the argument from perceptual illusion, see G. Ryle, Dilemmas (Cambridge: Cambridge University Press, 1956), Chap. 7.

something like observing a ruler along with observation of the lines.

From this point the discussion may move in either of two directions. We may follow the philosophical reasoning behind the argument for skepticism of the senses which began the discussion; or we may continue a line of reasoning suggested by the example from perception psychology. We will choose the latter, though we are not finished with the former. I believe it is correct to point out, as we did, that occasional mistakes in our ordinary observations of material objects are not adequate grounds for casting general doubt upon the utility of this source of knowledge, but I do not think that these arguments were originally intended for such a purpose. To attempt to state their philosophical import may contribute to making plausible our general point of view that philosophy does not dispute common sense. So we will mark this place as one to return to.

In attempting to defend or we might say, perhaps more accurately, to explicate the assertion that science is based upon common sense, we have come across two fundamental ideas in the logic of the sciences -- the related ideas of a basic vocabulary and basic sentences. I say they are fundamental because they reappear in so many areas in the philosophy of science. Consider, for example, their central role in the analysis of the

meaning and truth of scientific propositions. In short: to be meaningful, a scientific proposition (1) must be grammatically correct (a rather obvious requirement we have not discussed) and (2) its concepts (descriptive terms) must be connected through definitions with the basic vocabulary; to be true, a scientific proposition must be consistent with relevant basic sentences.

As we said before, the basic vocabulary names things with which we are directly acquainted, and the basic sentences state facts which we directly observe. Both the ideas of "acquaintance" and "observation" involved here are clearly related to what is spoken of as sensory-perception. Recognition of this might very well lead us to attribute to the psychology of perception a propaedeutic status among the sciences. For, doesn't it investigate the validity of the claims with respect to the basic vocabulary and basic sentences which -- unless my general position is false -- are taken for granted by all of the other sciences? To see clearly that it could not possibly do so, and why it is, therefore, a mistake to attribute such importance to this field of psychology¹ may enable us to grasp more firmly

¹G. Bergmann suggests that "psychologists are especially prone to make this mistake," and that philosophers have made it, too. Philosophy of Science (Madison: University of Wisconsin Press, 1957), p. 19. I have not mentioned it at this point in order to criticize anyone; the mistaken view would serve our purpose just as well even if nobody ever held it.

the meaning and significance of the point of view under discussion. This is the line of reasoning suggested by the illustration from perception psychology mentioned above, because that illustration shows that the study of perception like all other scientific inquiry must start from common sense.

When we commented on the use of the Mueller-Lyer diagram in the investigation of perceptual illusion, we noted that in using it the psychologist operates within the bounds of common sense -- i.e., he does not, for example, compare the subjects' perceptions with his own; no, he compares them with what this ordinary physical object, the diagram, is actually like which he learns by observations, his own or those of others, perhaps aided by measuring instruments.

Before pressing these considerations, let us add some similar ones. Another perceptual phenomenon, also illusory, investigated in the psychologist's laboratory is called "autokinetic movement."¹ In these

¹For an excellent account of experimental studies of perception, see C. E. Osgood, Method and Theory in Experimental Psychology (New York: Oxford University Press, 1953), pp. 191-297. Aside from the merits of the book itself, its title is very appropriate as a reference at this point. It helps to emphasize that in the above discussion I am talking about perception in the narrow sense of an ordinarily immediate response to a physical stimulus, as it is considered in such laboratory experiments as those described; I am not speaking of the much broader idea of perception as reflected in contemporary social psychology -- see, e.g., D. Krech and R. S. Crutchfield, Theory and Problems of Social Psychology (New York: McGraw-Hill, 1948), or S. Asch, Social Psychology (New York: Prentice-Hall, 1952).

experiments the subject is placed in a completely dark room in which there is a stationary pinpoint of light. After a while, most people "see" the light move in one direction or another. As before, the psychologist describes what his subjects perceive under various conditions, but neither in this description nor in his description of those conditions does he say anything about what he perceives; he speaks commonsensically about what is there for anyone to perceive under ordinary circumstances, in this case when the room is lighted and the fixed source of the light stimulus is visible.

It seems to me that even this brief glimpse at the experimental study of perception indicates rather conclusively that even though the psychologist may investigate the effects of various conditions upon the accuracy of direct observation, he does not, and as a psychologist cannot, question the general efficacy of this means of learning about our physical environment. In like manner, he treats his subjects' language in these studies as verbalizations of perceptual responses, but his own statements are considered as unproblematic descriptions of what there is "out there," as is sometimes said. And in so far as he achieves this, he does it by adhering to the principle of acquaintance which we explained in terms of the idea of the basic

vocabulary. We may conclude, then, that perception psychology proceeds within common sense according to the logic of science; that it is not, therefore, systematically relevant to the other sciences. However, this is not to say that its findings are always irrelevant to other sciences. One rather striking case in the history of science in which they were quite relevant (or would have been were they available) will be recounted. It serves as an introduction to several major points of our argument; also I think it is intrinsically interesting.

In 1795 the astronomer royal at the Greenwich Observatory, Maskelyne, dismissed his assistant for making consistent errors in his observation of the times of stellar events. This was the first recorded recognition of what became an anomalous chapter in the history of astronomy; not only because it was also a chapter in the history of psychology.¹ For by the 1820's it was painfully evident that there was a significant and systematic difference between the observations of the times of stellar events by different astronomers. As a

¹My description is taken from E. G. Boring, A History of Experimental Psychology (2d ed.; New York: Appleton-Century-Crofts, 1950), Chap. 8.

result, during the 1830's and 1840's astronomers were measuring what came to be called "personal equations" between each other. What this meant was that two astronomers, A and B, would make identical observations together and then compute an equation such as: Time of event observed by A minus 1.1 seconds = Time of same event observed by B. Thereafter, they could use each other's observations by making corrections on the basis of the equation. However, if this were not upsetting enough for what was perhaps the most precise of the sciences, it was discovered that there were some variations in these differences between any two astronomers; and, in addition, even if the times recorded by different observers could be used together by making corrections, one could not help wondering what the actual times were!

Astronomical observations were made by the "eye and ear" method. The observer would look away from his stellar transit to note the time when a given star approached the crossline, then he would count the ticks of the clock as he watched the star cross the line. In this way, he would mark the position of the star at the second before it crossed the line and the position at the second after it crossed and, by considering the distance between the crossline and these points on either side of it, estimate the time of crossing to the

nearest tenth of a second. The difficulty which led to interest in personal equations was not a result of the need to make this kind of interpolation; it was something much more fundamental than that. Individuals differed in the time it took them to respond to both auditory and visual cues, and the effect of having to attend to both kind of stimuli at once also varied among individuals.

For the newly emerging experimental psychology, these phenomena became the subject of two types of investigation: the former issued in the study of reaction times; the latter was the basis of the "complication experiment." In time, the techniques of the psychology laboratory made it possible to measure an astronomer's observation error absolutely rather than relatively, as it was expressed by the personal equation. But the resolution of this problem for astronomy had already been accomplished by changing the manner of observation. Actually a number of different techniques were sufficiently successful, as using a device which would automatically record the time (a chronograph) when triggered by an observer at the instant that a star crossed the line in the transit; but maximum precision was achieved by photographing what would be the field of the observer with a camera electrically synchronized with a chronograph.

This particular case is rather unusual, but what it illustrates has been more or less characteristic of all of our most advanced sciences -- i.e., what may be called the achievement of an "unproblematic evidential base." By this I mean the circumstance where evidence is gathered and presented in such a way that there is no doubt about either its meaning or, for all practical purposes, its truth. The idea may be expressed methodologically in terms of the language in which evidence is stated: what psychologists sometimes call the "data-language." In general, we may say, then, that evidence is unproblematic to the extent that the data-language approximates what we have been calling basic sentences.

One possible source of confusion in this manner of characterizing evidence is worth commenting upon. Remember that basic sentences state individual facts which can be directly observed. Therefore, it is possible, often without much difficulty, to achieve practical certainty with respect to their truth -- i.e., by providing that the observations be carefully made. But notice that the observations must be made. If they were not, the meaning of the related basic sentences would be unproblematic, but, obviously, we could not make a similar claim for their truth-value. So in my statement that evidence stated in basic sentences is

unproblematic, it must be understood that the relevant observations have been made.

Consider a typical study of voting behavior of the electorate. In the report of such research, a statement such as (1) 'S said he voted for R' is evidence stated in a basic sentence, and it is an example of the fully unproblematic kind we are referring to. However, in the same research report, a statement such as (2) 'S voted for R' is a basic sentence, but it is not a report of a direct observation; sentence (2) is an inference from sentence (1), albeit a usually valid one. And it is sentences like (2), not (1), upon which the conclusions or hypotheses of voting studies are based; so it would not be correct to say that such studies proceed from a wholly unproblematic evidential base. This is one point I will refer back to shortly. Two related points are: (1) practically, it is impossible in studies of voting to achieve wholly unproblematic evidence -- i.e., to observe the act of voting; (2) logically, there is no reason why such evidence could not be obtained.

The last example took us to political science. We will stay for a moment. The case of the personal equation was given as an instance where the psychology

of perception was temporarily relevant to astronomy; it suggested the idea of unproblematic evidence, which we characterized as evidence consisting of basic sentences reporting direct observations. It has probably been achieved most fully in the science of physics where primary data usually consists of pointer readings that can be taken accurately by almost anyone without a great deal of training. Of course, in areas of inquiry where this degree of reliability and precision is achieved, not only perception psychology but all other investigations of the processes of observation are generally irrelevant.

Now I want to suggest that it is logically possible to base scientific inquiry in any area upon evidence of this kind, including all of those areas which make up the subject matter of political science. I do not want to argue this at this time; it turns out that the relevant arguments are the same as those related to the general question of whether all areas of political science are subject to scientific investigation. Instead, I wish to make some quite general and somewhat impressionistic comments upon two major kinds of consequences of the uncontroversial observations that: (1) by and large, political scientists do not state their evidence in basic sentences; and (2) for most of the phenomena they study it would be very difficult and

often practically impossible to obtain data which could be stated in this way.

1) The first kind of consequence of using evidence more or less far removed from the level of immediate observation is that a great deal of inference usually takes place before one begins to theorize in political science; and such inferences ordinarily require general knowledge which is only partially available. Our earlier example concerning some of the evidence used in voting studies may be used for illustration. In this instance, theoretical elaboration (as the formulation of hypotheses) is based on information concerning the way people voted which is arrived at by inference from their testimony. The general knowledge required for inferences of this kind consists of such propositions as: People usually tell the truth; A person is more likely to tell the truth about his voting behavior when the interviewer assures him that what he says is confidential; It is more likely that a respondent will falsify his answers in order that he may appear to have voted for the winner in an election than for the loser; etc.

In this case the evidence is not far removed from the level of direct observation, and the general knowledge required for inference is to some extent available. So the inference from what is observed by the investigator to the theoretically relevant evidence causes

little or no difficulty. But consider for a moment research concerned with the effect of various events in the sphere of international affairs upon public attitudes toward foreign policy. In such a study there might be three categories of variables: types of events, types of nation states, and types of attitudes. Each instance of theoretically relevant evidence, then, would consist of a set of values for these three variables. The inferences involved in going from the data to the attitudinal evidence, for example, would be several and **hazardous**; the knowledge required to assure their validity, imperfect and partial. Most obviously, it would be necessary to infer the distribution of attitudes in national populations on the basis of some kind of sampling. Since it would also involve, at an earlier **stage**, such considerations as possible interviewer bias, it is easy to see in this example (and actually in the voting example, as well) that findings available from investigations of the various kinds of observational processes employed are quite relevant, even essential.¹

These two illustrations relate to the more explicitly

¹For an informative discussion of this subject, see H. Guetzkow, "Interaction between Methods and Models in Social Psychology," Current Trends in the Description and Analysis of Behavior (Pittsburgh: University of Pittsburgh Press, 1958), pp. 142-175. The footnotes in this paper provide a guide to recent studies of various observational procedures.

and systematically scientific research which is carried out in political science.¹ If what I said above concerning inferences involved in arriving at theoretically relevant evidence applies to this kind of research, we can safely say that its importance for our more "traditional" investigations is considerably greater.

Before leaving the idea that inference is involved in arriving at theoretically relevant evidence in political science, I wish to guard against the possibility that someone may have read too much into what I have said. Suppose the voting research referred to is concerned with the relationship between social class membership and voting. Now even if the class and voting data were of the fully unproblematic (non-inference) kind, any hypothesis relating these two types of variables would always involve an inductive inference, and -- as we usually say -- no amount of even such excellent evidence could do more than increase its probability. This is not the sort of inference we have been discussing; we have been concerned with the inferences involved in the evidence which supports such inferences. One important distinction between political science (as well as other social sciences) on the one hand, and the physical and

¹The fact that such political research is often performed by persons of other disciplines is of no concern at this point.

biological sciences on the other, is that this particular source of difficulty and error is relatively absent in them. But no matter how practically significant, this is not a logical distinction among the sciences.

2) My remarks on the second type of consequence for political science of using evidence usually far removed from what has been directly observed by the researcher are even more general and impressionistic than those on the first; I will be very brief. This circumstance -- it seems to me -- fosters and supports erroneous views regarding the logic of political science. The comment above concerning the part it plays in the practical differences between inquiry in political science and inquiry in the more exact sciences is the most general support for this supposition. It is not difficult to be so impressed by the actual differences as to believe that there are logical differences. And all of the doctrines that I know of concerning the logic of political science which I believe to be erroneous involve the assertion of such logical differences.

One specific example is the denial of the possibility of scientific objectivity in the study of political phenomena. This mistaken claim is, I believe, supported by two circumstances in our discipline, both of which are related to the evidence problem we are

discussing. First, the more inferences an investigator makes from what he observes directly, the more likely it is that his own preferences or values with respect to the subject under investigation will influence his statement of the evidence. Secondly, where precise, relevant evidence is difficult to come by, this relative absence of an objective criterion for selecting among competing hypotheses permits a person's subjective values to influence his choice. Those who wish to deprecate the possibility of objective political research can point to instances of these two kinds of subjectivity to support their position. However, the occurrence of such instances of value bias can be accounted for within the objectivist position.¹

Philosophy proper was briefly introduced earlier in this section when we mentioned a well-known argument against the common-sense view of the world based upon the pervasive possibility of perceptual error. Then we left philosophy after pointing out the fallacy of such arguments against common sense, and went on to perception psychology, astronomy, and political science. Now we will pick up the strand of our argument regarding philosophy. Apparent denials of common sense appear

¹See below, Chapter Seven.

frequently in traditional (non-analytic) philosophy. Perhaps there are actual as well as apparent denials,¹ but with respect to any particular traditional philosophical position, where its literal interpretation conflicts with common sense (or its long arm, science), I believe one should seek another interpretation. From where I stand, anything else would amount to an outright denial of most of traditional philosophy; besides, there is a great deal of support for this approach within traditional philosophical writing. This is an essential tenet of the Reconstructionist variant of analytic philosophy. Its "proof" lies in its consequences; that is, the results of the analysis it leads to. I can only illustrate it in a rough way, though I hope that attempting to do so may add to the plausibility of the proposition that philosophy, like science, starts from common sense.

It is in philosophy proper that the style of analysis I have been trying to exhibit appears in its most distinctive (and complex) form. Its key idea is that philosophical problems may be resolved through ordinary discourse about an artificial language constructed with this purpose in mind. In philosophy proper, one uses

¹For an interesting discussion of this point which is generally consistent with the approach taken here, see G. E. Moore, "In Defense of Common Sense," Contemporary British Philosophy: Personal Statements, 2nd Series, ed. J. H. Muirhead (New York: Macmillan, 1925), pp. 193-223.

what is called an "ideal language."¹ It is not an actual language which might be spoken, but a schema or outline of a language. Using some of the distinctions introduced in our discussion of ordinary language, we may describe it as follows. It contains two kinds of words, logical and descriptive. The descriptive words name (or refer to) things, in the broadest sense of the word; the logical words do not name anything. Among the descriptive words we may distinguish two classes, defined and undefined terms. If we consider the undefined part of the descriptive vocabulary as zero level terms, then the others may be characterized as first level terms which are defined by terms of the zero level, second level terms defined by zero and first level terms, and so on. The logical words play an important role in the syntax of the schema, and in order to use it as a general tool of philosophical analysis, we would have to say a great deal about both the logical words and the syntax. For our present purposes, however, we may assume that its logical words are the ordinary English 'and,' 'or,'

¹For description and illustration of ideal-language analysis, see the two collections of papers by G. Bergmann: The Metaphysics of Logical Positivism (New York: Longmans, Green and Co., 1954) and Meaning and Existence (Madison: University of Wisconsin Press, 1960). There is an excellent brief, non-technical account of ideal-language analysis written for social scientists by H. Hochberg, "Axiomatic Systems, Formalization, and Scientific Theories," Symposium on Sociological Theory, ed. L. Gross (Evanston: Row, Peterson and Co., 1959), pp. 407-436.

'if ... then,' etc, and its syntax is English grammar.¹
 So far we have described a few features of a schematic language. To be the ideal language it must fulfill two essential conditions: (1) it must be complete in the sense that a full description of the world (or all areas of our experience) can, in principle, be stated within it; and (2) it must allow for the solution of all philosophical problems by ordinary discourse about it.²

This much, and more, may be said about the notion of an ideal language without taking a position on any philosophical issue. Notice, I did not say anything about what its undefined descriptive terms name, or how we know what they name. It turns out that many traditional philosophical positions may be reconstructed as answers to these two questions. We will briefly illustrate the use of the ideal language in this way.

Ontology and epistemology have been the two most fundamental concerns of philosophy. Together they are sometimes spoken of as "first philosophy" or metaphysics, though the latter is more often synonymous with ontology. Simply stated, the ontological question is "What exists?"; the question for epistemology is "How do we know?" Obviously the words 'exist' and 'know' are not used

¹These assumptions are actually false, but this will not affect our subsequent discussion.

²G. Bergmann, "Two Criteria for an Ideal Language," Philosophy of Science, 16, 1949, 71-74.

commonsensically. To discover what they mean and thus to understand the traditional philosophical positions regarding them is the problem; the ideal language is the means of solving it.

Consider the contrasting epistemologies -- empiricism and rationalism, and the contrasting ontologies -- realism and phenomenism. They may be explicated as statements about the undefined terms of the ideal language. Empiricism is, then, the thesis that the ideal language must be constructed on the basis of undefined descriptive terms whose meaning we know by direct acquaintance.¹ Rationalism denies that everything we know could be accounted for in such a language -- i.e., it denies that the empiricist language is the ideal language; and asserts that the ideal language must also contain undefined terms, usually complex philosophical words like 'substance,' 'cause,' or 'being,' whose meaning we know in some other manner, as by "rational intuition" or "pure reason." In this way, traditional positions regarding epistemology are reconstructed as statements about how the undefined terms achieve meaning; ontological views are expressed as assertions about what they mean, or refer to. Thus, the ontological realist claims that all undefined terms of

¹G. Bergmann states it this way in The Metaphysics of Logical Positivism (New York: Longmans, Green and Co., 1954), p. 146.

the ideal language refer to physical things¹; the phenomenalist ontology is interpreted as the assertion that the undefined terms refer only to mental things.² I will expand on what is meant by ontology in what follows.

When we referred earlier to philosophical arguments in which types of perceptual error are related to discredit our ordinary conception of the world, I said that such arguments were not originally intended as attacks upon common sense and that I would later attempt to state their actual philosophical import. I shall do this now with respect to one context of traditional philosophy in which such arguments are often found -- i.e., in writings of phenomenologists such as Bishop George Berkeley.³

In the context of Berkeley's empiricist arguments for a phenomenalist ontology the import of his references to perceptual illusions may, I believe, be rendered as follows.⁴ When we see a stick in the water, it appears

¹Ibid., pp. 153-176.

²Ibid., pp. 43-44.

³See, e.g., Three Dialogues between Hylas and Philonous (LaSalle, Illinois: Open Court Pub. Co., 1946; orig. pub., London, 1713); for use of the argument from perceptual illusion, see especially the first chapter.

⁴Ibid. See the interpretation by B. Russell in A History of Western Philosophy (New York: Simon & Schuster, 1945), Chap. 16.

bent; taken out of the water, it then appears straight. Speaking as we ordinarily do, we say that the latter is a correct perception and the former was illusory. As I interpret Berkeley, he does not disagree. What he wished to emphasize is that in both cases our observations of our own experience -- i.e., the appearances, or our sense data, were correct.¹ This supported both his empiricism and his phenomenalism. The former may be expressed roughly by saying that what we know with certainty is the content of our immediate experience, and that everything else we know can be built up from this basis. It is more precisely expressed as an assertion that we understand the undefined terms of the ideal language by direct acquaintance; that we know the truth of simple (atomic) sentences in the ideal language by direct observation; and that all other statements in the ideal language are derived (as by induction) from these atomic sentences. The latter, his ontology, is expressed by saying that the things of whose existence we are certain are mental things (sense data) as this is what we know directly, and that everything else there is may be "constructed" from these "simples." As in our earlier statement of phenomenalism, Berkeley's ontological idea of "existence" is precisely stated by asserting that the undefined terms of the ideal language

¹Berkeley, op. cit., 3rd dialogue, p. 102.

name mental things.

This brief discussion of Berkeley's philosophy elaborates somewhat on the philosophical ideas of epistemology and ontology contained in our above illustrations of ideal language analysis. With respect to the epistemological question -- "How do we know?" -- it indicates that the question asks for something like how we know that which we know most certainly and on the basis of which the rest of our knowledge might be derived. And concerning the ontological question -- "What exists?" -- it suggests that what is sought are the things of whose existence we know (in the philosophical sense), from which everything else might be "constructed." And it shows, I believe that, in an informal way, instances of perceptual illusion may be used to support the answers to these questions given by empiricism and phenomenalism, respectively. Also, I think it supports the conclusion that when the philosophical ideas of "knowing" and "existence" are expressed in terms of the ideal language, although they remain peculiar, they are not meaningless, and, although they are not commonsensical, they do not conflict with common sense.

We have given what seems to me a reasonable basis for explicating what a philosopher, such as Berkeley, may

mean by saying there are no physical objects -- in short, he says words referring to physical objects are defined terms in the ideal language. Now we are concerned with what the psychologist John B. Watson meant when he said there are no minds.¹ I put it this way to make the apparent contradiction of common sense patent;² just as patent as Berkeley's denial of the existence of physical objects.³ The meaningful core of Watson's peculiar claim may be recovered and the perplexity it engenders dispelled in a manner very similar to the way we handled Berkeley's assertion -- i.e., by interpreting it as a statement about a schematic language, not the ideal

¹By 'minds', I refer to what we usually call mental contents, such as percepts, memories, thoughts, etc.

²Watson never stated it quite so bluntly, but in some of his more extravagant attacks against "content" psychology (i.e., most of the psychology of the 19th and early 20th centuries which sought to describe and explain the content of man's consciousness), he made assertions which were tantamount to this absurd claim. For example: in the paper which E. G. Boring said founded behaviorism (op. cit., p. 643), Watson identified "thought processes" with "motor habits in the larynx" ("Psychology as the Behaviorist Views It," Psychological Review, 20, 1913, 158-177, in the note at pp. 173-174); in another paper in the same year he practically identified mind with images and then denied that there were images or any other "centrally aroused sensations" ("Image and Affection in Behavior," Journal of Philosophy, 10, 1913, 421-428, at pp. 421-423); and in the first chapter of a later book he indicated that psychology "can find no evidence" for the existence of "mind" and "consciousness" (Behaviorism [rev. ed.; New York: W. W. Norton & Co., 1930], p. 18). Bergmann suggests that Watson thought he had to assert that there are no minds in order to assert the thesis that there are no interacting minds. ("The Contribution of John B. Watson," Psychological Review, 63, 1956, 265-276, at p. 266.)

³Berkeley, op. cit. Russell wrote that Berkeley's "argument against matter is most persuasively set forth in The Dialogues of Hylas and Philonous," op. cit., p. 648.

language but what is called an "improved language" for science.¹ First, we will introduce this analytical device. This will not be difficult, as it has been implicit in our earlier discussion of the philosophy of science and its structure is the same as that of the ideal language. It differs from the ideal language in the conditions it must fulfill:² (1) it must be possible, in principle, to state all of the propositions of the sciences within it and (2) it must allow for the resolution of all philosophical problems about science by ordinary discourse about it. As we already implied above when speaking of the basic vocabulary for science, the first of these conditions is fulfilled by a "realistic" language -- i.e., one whose undefined terms name those physical objects and their qualities with which we are, commonsensically, directly acquainted. With the exception of some philosophy-proper type questions concerning mind which may arise in discussion of psychology, such a language also fulfills the second condition.

Notice two things. First, to declare that such a "realistic" language is the improved language for science (or any specific science) does not imply any ontological position. The ontological phenomenalist may very well

¹G. Bergmann, Philosophy of Science (Madison: University of Wisconsin Press, 1957), passim.

²Ibid., pp. 39-41

agree that this is the improved language; he argues that it is not the ideal language, and that it, as well as areas of our experience which it does not account for, may be stated within a phenomenalist schema.¹ Second, my use of 'direct acquaintance' in talking about the improved language is not identical with the philosophical use of that expression; when the phenomenalist says he is only "directly acquainted" with, say, sense data, he does not deny that we are, commonsensically, directly acquainted with physical objects; remember, we said the philosophical use involves notions of "certainty," "simplicity," etc. So, strictly speaking, what one says about the improved language implies nothing about philosophy proper; I will speak of it as "realistic" and "empiricistic" without intending anything about ontology or epistemology. Incidentally, this is one way of showing the distinction between philosophy of science and philosophy proper which I have alluded to a number of times.

Now we are ready for Watson. Most of what he meant by his arguments against mind may be reconstructed as the assertion that, in an improved language for psychology, all "mentalistic" concepts would be defined terms, or -- to say the same thing differently -- that the science of psychology can, in principle, be stated in a realistic

¹Bergmann, e.g., takes this position, ibid., p. 40.

language.¹ Classical psychology, which Watson was attacking, may be interpreted in this way as implying that psychology could only be stated in a phenomenalist language. These positions represent, as one can see, a different conception of the subject matter of psychology: for Watson it was overt behavior;² for classical psychology it was mind or consciousness.³

Watson's view, whether we state it in terms of a conception of the subject matter of psychology, a way of handling "mentalistic" concepts, or in terms of the idea of the improved language, is what we now know as the position of methodological behaviorism. It is, I believe, an important methodological principle of psychology and, at the same time, a necessary step toward understanding the logic of political science. For

¹In addition to this methodological position, Watson's discussion of mind also reflected certain substantive positions within psychology: (1) a radical environmentalism reflected in his denial of individual differences (see, e.g., Behaviorism, p. 270); (2) peripheralism (see, e.g., "Image and Affection in Behavior," pp. 421-423); (3) opposition to use of the technique of introspection in psychological research (see the references cited above). These substantive views are no longer of any importance; probably no psychologist today would agree with those stated in (1) and (2), and Watson himself had to admit the need for introspective reports by subjects, at least in the area of psychophysical measurement. Watson's major contribution was in the area of methodology. See Bergmann, "The Contribution of John B. Watson," p. 275.

²Watson, "Psychology as the Behaviorist Views It," p. 176. See also K. Spence, Behavior Theory and Conditioning (New Haven: Yale University Press, 1956), pp. 11-13.

³Ibid., pp. 4-7.

political science, it shows that the need to deal with such phenomena as opinions, attitudes, preferences, and the like does not raise any logical barriers against scientific inquiry;¹ nor does it necessitate the dubious inference from observable behavior to unobservable psychic qualities (i.e., the contents of other minds).² I shall make a few additional general observations concerning the importance of methodological behaviorism for political science later in this chapter; at this time, we will attempt to clarify and defend the principle in the context of psychology.

Precisely speaking, to hold that all "mentalistic" concepts in psychology must in principle be capable of introduction by definition within a realistic language, as methodological behaviorism requires, is to say that mind or consciousness is excluded from psychology. After a fashion, I believe this is true. I say it is true only after a fashion for two reasons. First, methodological behaviorism is a principle of the logic of psychology; it is not a maxim about how psychologists must proceed

¹H. J. Morgenthau, for example, raises this spurious difficulty in Scientific Man vs. Power Politics (Chicago: University of Chicago Press, 1946) at p. 129. See the review by E. Nagel, "Logic and Political Theory" in his Logic without Metaphysics (Glencoe, Illinois: Free Press, 1956), pp. 377-382.

²W. H. Harbold and D. G. Hichner refer to the difficulties resulting from the need for such inferences in pointing out the obstacles to scientific political inquiry. "Some Reflections on Method in the Study of Politics," Western Political Quarterly, 11, 1958, 753-772, at p. 756.

in order to successfully discover the laws they seek. There are no such procedural **recipes** within the philosophy of science. Second, even as a programmatic goal, it does not require that psychologists refrain from thinking of the mental contents of their subjects, it requires only that they try to define their concepts in terms of publicly observable data; for characteristically psychological concepts, this means overt behavior (including verbal reports), physical features of the subject's environment (including the behavior of other organisms), and physiological characteristics.¹

To defend methodological behaviorism, therefore, does not require that we attempt an inventory of psychological concepts to show that their definitions meet the requirements of this principle; this is not even true. It requires only that we show, in a general way, that all psychological concepts might be so defined and why it is that this logical feature is important. To begin with the latter, we need only point out that a minimal requisite for the objectivity required by science is that evidential statements be capable of inter-subjective (interscientist) confirmation; that this requirement is

¹K. W. Spence, "The Postulates and Methods of 'Behaviorism,'" Psychological Review, 55, 1948; reprinted in H. Feigl and M. Brodbeck, eds., Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, Inc., 1953), pp. 571-584, at p. 578. See also G. Bergmann, "Theoretical Psychology," Annual Review of Psychology, 4, 1953, 435-458, at p. 435.

fulfilled by statements in a realistic language; that it is not fulfilled in the alternative language for psychology -- namely, one in which phenomenal terms are permitted (i.e., terms which refer to psychic qualities or, as we have usually called them, mental things). I need not inform anyone -- but it does no harm to mention -- that only one person can directly confirm a statement about someone's mental state and that is the person of whom we are speaking.

Someone might object at this point: "Your argument only holds against using evidence statements describing mental states; it does not show why it is objectionable to introduce statements describing mental states which are inferred from statements in a realistic language describing behavior." I would answer in two steps. First, I would make the observation that in order to make such inferences we would need cross-connection laws relating behavior states to mental states.¹ Secondly, I would point out that, although there is a good commonsensical basis for believing that there are such lawful relations, the original argument against the scientific acceptability

¹That scientific laws are logically required in order to validly infer one empirical state of affairs from another is generally accepted among philosophers of science. Since it is -- as I see it -- the core idea of scientific explanation, we will discuss the basis for it in Chapter Four.

of evidential statements describing mental states (which our objector accepted) has the same force against such cross-connection laws. What would be the evidence for the mental side of the connection?

So far I believe I have shown why methodological behaviorism is important -- at least for those who interpret science as I do -- and, perhaps, these remarks have also contributed to its plausibility. I shall give one direct line of argument toward the latter purpose. As we noted above, the core idea of methodological behaviorism is that all of the concepts of psychology are in principle capable of translation into the realist language. Let us consider for a moment the field of clinical psychology, since the concepts employed there appear to be the least likely candidates for inclusion in the realist language. My strategy is that if we can relate an approach which supports our principle in that area, then the case for its general validity will be strengthened.

In clinical psychology phenomenological concepts are frequently used and objective definitions are generally not yet available.¹ However, if we observe the way a psychoanalyst, for example, employs such

¹Bergmann, "Theoretical Psychology," p. 437; E. Nagel, "Methodological Issues in Psychoanalytic Theory," Psychoanalysis, Scientific Method, and Philosophy, ed. S. Hook (New York: New York University Press, 1959), pp. 38-56, especially pp. 39-47.

concepts we see that he does so on the basis of the overt behavior of his patients, mainly their verbal behavior. It thus becomes evident that he has some objective criteria of applicability for his concepts. Now one way of construing the idea of the definition of a term is a statement of the criteria of applicability of that term.¹ Therefore, it seems apparent to me that if the psychoanalysts' criteria of applicability, no matter how vague they may be, could be made explicit, they would constitute definitions of their concepts consistent with the principle of methodological behaviorism.²

In most cases the attributes described by such definitions would be behavioral dispositions. E. Frenkel-Brunswik stated this view very clearly when discussing Freudian concepts:

From the standpoint of the logic of science, unconscious tendencies are a special case of latent or "dispositional" characteristics. They are comparable to such physical characteristics as magnetism, provided that we do not insist on assigning them to "the mind" in a metaphysical sense. Such composite terms as "unconscious hostility" or "dependency" describe a disposition

¹C. G. Hempel, Fundamentals of Concept Formation in Empirical Science; International Encyclopedia of Unified Science, Vol. II, No. 7 (Chicago: University of Chicago Press), p. 41, et passim.

²Bergmann, loc. cit.

to display aggression or dependence under specified conditions, for example, in therapy.¹

It needs to be added that there is nothing unusual about dispositional concepts in science. As Hempel has stated, "the vocabulary of empirical science abounds in disposition terms, such as 'elastic', 'conductor of heat', 'fissionable', 'catalyzer', 'phototropic', 'recessive trait', 'vasoconstrictor', 'introvert', 'somatotonic', 'matriarchate'."²

It is my impression that among psychologists with an orientation toward cognition theory (Gestaltists, phenomenologists, etc.) the considerations we have introduced regarding methodological behaviorism are frequently misunderstood.³ I raise this matter because it suggests

¹"Confirmation of Psychoanalytic Theories," The Validation of Scientific Theories, ed. P. Frank (Boston: The Beacon Press, 1956), pp. 97-115; quoted from p. 99.

²Op. cit., p. 24.

³Allowing for some blurred edges, there is a dichotomy among contemporary psychologists which may be expressed in terms of a tendency to prefer either cognition or stimulus-response type theories. The distinction is expressed and each part of it described in the following articles: W. W. Lambert, "Stimulus-Response Contiguity and Reinforcement Theory in Social Psychology," in Handbook of Social Psychology, ed. G. Lindzey (Cambridge: Addison-Wesley Pub. Co., 1954), Vol. I, pp. 57-90; M. Scheerer "Cognition Theory," ibid., pp. 91-137; D. T. Campbell, "Social Attitudes and Other Acquired Behavioral Dispositions," August 1959 version (mimeo) of a chapter to appear in: S. Koch, ed. Psychology: A Study of Science, Vol. VI: Investigations of Man As Socius: Their Place in Psychology and the Social Sciences (New York: McGraw-Hill, in press).

a few remarks of some relevance to our concern, since the use of a cognition type psychology is dominant in recent political behavior research.¹

A failure to appreciate what we have said so far concerning methodological behaviorism is, I believe, reflected in a comment by M. Scheerer; when comparing cognition and stimulus-response (S-R) theories in psychology, he wrote: "Cognition theorists are no less empirically minded in the search for empirical ultimates. But for them these ultimates include phenomenal data as long as they are tied to objective procedures."² This tie, which Scheerer refers to, must be either lawful or definitional. If it is lawful, it requires laws of the kind mentioned above -- i.e., laws connecting physical (behavioral or physiological) states and phenomenal states, but the data concerning the latter which is needed to support such laws could not be of the objective kind. If the tie is definitional,

¹It would be extremely difficult to document this claim since it refers to such a vast literature. As long as my reference to "cognition type psychology" is understood in the general way that it is discussed by Scheerer, op. cit., and Campbell, op. cit., I do not believe that anyone familiar with this literature would dispute the matter. Two recent books illustrate the point: A. Campbell, et. al., The American Voter (New York: John Wiley & Sons, Inc., 1960), see especially Chap. 2; J. G. March and H. A. Simon, Organizations (New York: John Wiley & Sons, Inc., 1958), see especially pp. 9-11, 138-139.

²Op. cit., p. 92, italics mine.

then the "phenomenal data" would not be phenomenal (mental) -- i.e., the terms so defined would not actually refer to phenomenal data. I believe the latter is actually the case.

This is indicated by the fact that in such writing the supposedly "mentalistic" terms -- 'definition of the situation,' 'view of the world,' 'cathectic orientation,' 'cognitive map,' 'perceived _____,' etc. -- are not introduced as undefined terms; they are ordinarily highly defined in a way that is quite consistent with methodological behaviorism.¹ If this is true, then there is no logical difference between S-R psychology and cognition psychology. The differences are differences of approach within psychology, and each probably has its own advantages for the investigation of different areas.²

In this short digression I have not demonstrated, but I have brought forth a number of considerations which support the conclusion that political scientists

¹Though what I would call definitions are often called "indicators" by those who write from the point of view of cognition psychology, I do not think this makes any difference for the discussion above.

²To argue this conclusion is outside the scope of our study and the competence of this writer. Fortunately, it has already been done by D. T. Campbell, *op. cit.* See also, K. W. Spence, "Theoretical Interpretations of Learning," Handbook of Experimental Psychology, ed. S. S. Stevens (New York: John Wiley & Sons, Inc., 1951), pp. 690-729.

who adopt either a cognitive or an S-R theoretical orientation when employing psychology in their work, do not thereby take a position on or raise methodological questions. It follows, then, that the rejection of S-R psychology does not entail a rejection of methodological behaviorism. This obvious point is worth making because S-R psychology is also called "behaviorism," and as I suggested earlier political scientists tend to reject this kind of psychology.¹

Also we should notice that what has been called the "political behavior approach" in our discipline is not connected with any particular kind of psychological theory such as behaviorism. Thus, in a PROD editorial entitled "What is Political Behavior?" the editor, who apparently considers himself a "political behaviorist," when indicating what the political behavior approach is not, stated:

Nor is it the fulfillment of the theories of the behaviorist school of psychology, to which, in our

¹That behaviorism as a theoretical orientation within psychology is not the same thing as methodological behaviorism is reflected in Professor Bergmann's observation that "virtually every American psychologist, whether he knows it or not, is nowadays a methodological behaviorist." "The Contribution of John B. Watson," p. 270. At another place, Bergmann has also observed that "all Logical Positivists ... are behaviorists," i.e., methodological behaviorists. The Metaphysics of Logical Positivism, at p. 19; see pp. 19-21 and pp. 171-174 for his explanation and argument.

mind, it would be fatal for political scientists to subscribe.¹

Though the strength of his opposition to what he calls "the behaviorist school of psychology" creates some doubt in my mind about what he means by this expression, I believe this statement illustrates our point.²

The political behavior approach does not entail behaviorist psychology, but I think there is reason to suggest that it does entail methodological behaviorism. Even though there is some ambiguity concerning the meaning of "political behavior" as an approach or type of inquiry, it seems to me that the most prominent feature of what is normally designated by this expression is an attempt to employ scientific method in the study of politics. Another feature is its orientation toward psychology -- i.e., there is a tendency for such work

¹Political Research Organization and Design, Vol. I, No. 6, 1958, pp. 42-43; quote from p. 43. At p. 42, the author implies that he is a "political behaviorist," but that this expression is ambiguous, and he says, "we merely prefer that political behavior be regarded as nothing save political science as some of us would like it to be."

²A comment by David Easton is also relevant: "Behavioral research -- remembering that we are using the adjective without thereby imputing outmoded behaviorist views to contemporary political psychology" The Political System: An Inquiry into the State of Political Science (New York: Knopf, 1953), p. 202; see also p. 151.

to focus upon individual behavior.¹ Methodological behaviorism is, with respect to methodology, equivalent to scientific psychology. Thus, I suggest a connection between methodological and political behaviorism.

¹These comments on the political behavior approach are supported by the following discussions of it: E. M. Kirkpatrick, "The 'Political Behavior' Approach," PROD, Vol. II, No. 2, pp. 9-13; D. Waldo, Political Science in the United States of America: A Trend Report (Paris: UNESCO, 1956), pp. 20-30; D. B. Truman, "The Impact on Political Science of the Revolution in the Behavioral Sciences," Research Frontiers in Politics and Government (Washington: Brookings Institution, 1955).

Both in its explicit commitment to scientific inquiry and its tendency to emphasize individual behavior, I think the political behavior approach may be distinguished from what is, also somewhat ambiguously, called "traditional" political science. However, the distinction is -- in my estimation -- one of degree, though I think the degrees of difference are significant. But, then again, the matter is actually so complex that I claim little precision for the latter remarks. As vague suggestions, they are also supported by the articles listed above.

CHAPTER FOUR

CONCEPTS, PROPOSITIONS, AND EXPLANATION

In the last chapter we introduced the basic ideas and the style of analysis which we described as "a philosophical basis for the analysis of methodological problems in political science." By what must have sometimes seemed a circuitous dialectical route we managed to bring the philosophical basis to bear upon several matters directly related to political science. This chapter seeks to continue the argument for the relevance of the philosophical base by showing its connection with what was earlier described as three major clusters of methodological problems in political science -- concepts, propositions, and explanation.

Recalling our idea of a cluster of methodological problems, we said that these problems become more specifically related to political inquiry as we move out from their central core. Our strategy will be to reason from the philosophical basis to the core of each of these groupings of issues, and to continue out from the center to an extent which is sufficient to connect up the philosophical basis with political science. This and the remaining objectives of the chapter may be summarized as follows: (1) to relate the philosophical basis to political science; (2) as

part of (1), to analyze a few illustrative issues from the methodology of political science; (3) to provide a foundation for problems discussed in the remainder of the thesis; (4) to suggest, mainly implicitly but also explicitly, directions for further methodological analysis. The last of these objectives suggests what must have been evident from the scope of our task -- the import of this chapter for the analysis of methodological problems in political science is mainly preparatory and programmatic.

A. Concepts.

The subject matter of political science consists of such things as courts, voters, states, political leaders, legislatures, judges, and the like. Political scientists seek to describe and explain some of the characteristics and behavior of such political objects. One requisite feature of political inquiry is, then, that the words employed must refer to these things -- they must refer to political reality. The first principle of scientific concept formation states the condition which must be met in order for political concepts to fulfill this requirement: in short, they must be defined by terms whose referents are directly observable.¹ Are the concepts used in political

¹C. G. Hempel, Fundamentals of Concept Formation in Empirical Science; International Encyclopedia of Unified Science (Chicago: University of Chicago Press, 1952), Vol. II, No. 7, pp. 20-23.

science capable of being defined according to this principle? We raised this question in the last chapter. Let us return to our earlier discussion, at the same time restating this principle of scientific concept formation in the way that it was discussed there.

Thus, we observed in the last chapter that part of the task of defending the possibility of scientific political science required that we "show that the concepts that occur in political science may be introduced into the language of science as we characterized it" in an ideal way, which we later called the "realist" language: i.e., "a language whose descriptive terms are all ultimately definable in terms of a basic vocabulary which names only physical objects and some of their properties -- things with which we are directly acquainted." And we said that two kinds of concepts required special attention: (1) "mentalistic" concepts and (2) group concepts. Then, when we introduced the principle of methodological behaviorism which is the basis for the clarification of the first of these types of concepts, we restated this part of the essential task in another, and I think more appropriate manner, as that of showing "that the need to deal with such phenomena as opinions, attitudes, preferences, and the like does not raise any logical barriers against scientific inquiry." We shall now attempt to give a

partial analysis of group concepts which supports the conclusion that the presence of group phenomena in the subject matter of political science does not raise logical barriers against scientific inquiry. This is a way of focusing our discussion; our main purpose is to employ methodological analysis to achieve a fuller understanding of the nature of group concepts used in political science.

For some purposes, it is convenient to divide the subject matter of the social sciences in general and, for our particular concerns, of political science into (1) individuals and their properties, and (2) groups and their properties. The first of these categories is clear enough. The second, because it is the focus of our present discussion and because there are more restricted usages of the term 'group,' needs a few comments. I will take the required definitions from an excellent discussion of our present topic.

A group is an aggregate of individuals standing in certain descriptive relations to each other.¹

And a group property is one which

is attributed to a group collectively, so that the group itself is logically the subject of the proposition, rather than distributively, in which case "each and every" member of the group

¹M. Brodbeck, "Methodological Individualisms: Definition and Reduction," Philosophy of Science, 25, 1958, 1-22, at p. 2; my italics. Our discussion of group concepts owes much to this paper.

could logically be the subject
of the proposition ... ¹

Thus, the statement 'United States Senators are at least 30 years of age' attributes an individual property distributively to each member of the group it mentions; but the statement 'The average age of Senators is decreasing' attributes a group property collectively to the same group. Further examples of groups and group properties are given immediately below.

In the broad sense defined above, political science is obviously to a large extent the study of group phenomena. Therefore, writing in the field abounds with group concepts: concepts which refer to groups such as political parties, electorates, nations, legislatures, international organizations, publics; and concepts which refer to group properties such as democratic, industrialized, and totalitarian as characteristics of states or societies, and conservative, fragmented, and responsible as attributes of political parties. One patent feature of such group things is that, by and large, they are not directly observable. (Actually, one cannot directly observe any of those listed above.) With respect to group concepts, such as those I just listed, it follows that we are not directly acquainted with their (referential) meaning. So within the

¹Ibid.

scientific language as characterized earlier, group concepts would have to be defined terms. But, are they definable in such a language? The name of the position which answers in the affirmative, and which I believe is correct, is "methodological individualism."¹

Let us spell out more fully the principle of methodological individualism as it applies to political science. First, it asserts that the above characterization of a group as "an aggregate of individuals standing in certain descriptive relations to each other" is adequate for the groups investigated in political science. Second, it claims that all the properties of such groups represent some combination of properties of individuals making up the groups. Therefore, it holds, third, that it is in principle possible to define all group concepts in terms of individual concepts.² Since individual concepts either themselves refer to what we can observe directly or they are defined by terms which so refer, group concepts are, then, admissible into the language of science.

This is the position. What can we say in its support? First, let us note a consequence of its

¹Ibid., p. 3.

²This needs to be qualified for some group concepts such as 'state,' which usually refers not only to an aggregate of individuals with certain complex properties but also to a territory. It will be apparent that such a qualification does not affect our argument; the point to notice in this case is that the meaning of terms referring to territories which may appear in the definitions of the names of states is unproblematic.

denial. To claim that group concepts are not definable in terms which refer to individuals amounts to the hypostatization of unobservable group entities exhibiting unobservable properties. Thus the denial of methodological individualism raises issues which run deeper than the methodology of the social sciences; as Professor Brodbeck concluded:

Philosophically, the holistic assumption that there are group properties over and above the individuals making up the group, their properties, and relations among them is counter to empiricism. For the latter holds that all terms must ultimately refer to what is observable, directly or indirectly, and what we observe are people and their characteristics not supra-individual groups and their characteristics.¹

This is reason enough for an empiricist to adopt the position of methodological individualism. As to its appropriateness as a methodological principle for political science, some additional, though not conclusive, support may be derived from the further consideration of group concepts and group phenomena as they appear in political science which follows.

Let us consider, first, a relatively simple group and one of its properties. Thus the "electorate of Monroe County, Indiana" is a group of which one may correctly predicate the property of "a strong disposition to elect Republicans." It is quite clear that this group

¹Ibid.

is an aggregate of individuals, each of whom is a voter or potential voter, and all of whom are related to each other as residents of the same county. And the group concept referring to the above property is definable in terms referring to individual voting dispositions, their strengths, and their distribution among the aggregate. It is apparent, then, that the two group concepts in this illustration are consistent with methodological individualism. Also the use of concepts like these in political inquiry ordinarily raises no serious difficulties concerning meaning. These last two circumstances are no doubt related, but, as we shall see in a moment, consistency with the principle of methodological individualism is not a sufficient condition for the formulation of precise concepts.

Other examples, which are in some respects similar to those given above, are the group concepts contained in the proposition 'The Republican Party is conservative.' Although the concepts involved here are more ambiguous and less precise (more vague) than our first illustrations, I think a moment's reflection shows that as ordinarily used they, too, are definable in individual terms. The expression 'Republican Party' is ambiguous; it has been used in the literature with at least four different meanings: (1) individuals in the electorate who usually vote for Republican candidates; (2) a "group of more or less professional political workers"; (3) "groups

within the government"; and (4) a group composed of all three of the above.¹ So, actually, the expression 'Republican Party' is used for at least four different concepts, but it is clear that each of these may be defined in terms referring to the individuals who compose the groups they designate. The concept of 'conservatism' as used to describe a political party is vague, but if we could spell out its definition fully, it would no doubt consist of individual terms naming dispositions to behave in specified ways, and it would include some indication, more or less determinate, of the distribution of these dispositions among the members of the party.

Though the group concepts discussed in our last illustration were in certain respects complex, they must be numbered among the relatively simple ones employed in political science. Before proceeding to more problematic group concepts, a relevant consideration concerning group phenomena which may be illustrated by the examples discussed so far will be entered; any additional preparation will be helpful before plunging into the difficulties in store for us below.

Notice that the property of conservatism, and other attributes as well, are relatively stable

¹V. O. Key, Politics, Parties, and Pressure Groups (4th ed., rev.; New York: Thomas Y. Crowell Co., 1958), pp. 180-182.

characteristics of the Republican Party; they endure even though the individuals who compose the party change.¹ The same is true of most of the groups investigated by political scientists. This circumstance might lead a student of politics to assume, contrary to methodological individualism, that a group is something other than (or in addition to) its constituent individuals and their characteristics. However, further consideration of the instance of the conservatism of the Republican Party supports the claim that this assumption is at least not necessary (even if it is not always obviously false) in order to account for enduring group properties. Thus it is easy to see that the Republican Party probably retains this quality because of such facts as the following: people with conservative political attitudes are attracted to it; as a result of these and other dispositions of those who are already members, new recruits tend to learn such attitudes; also members with conservative attitudes probably have a better opportunity to advance to positions of leadership within the party. To attribute these features to this group in no way conflicts with methodological individualism.

This one example -- assuming my analysis is correct -- obviously does not settle the matter in favor of the

¹This is, I believe, the distinctive feature which is the basis for speaking of such groups and their properties as institutions.

principle I am defending, but it helps. And it must be evident by now that our subject is so complex and the relevant factual data so incomplete, that I must be brief and suggestive rather than thorough and conclusive. We will continue in this fashion.

The two group properties discussed above, the conservatism of a party and the disposition of a county to elect candidates of a party, are, as I indicated, the resultants of certain distributions of individual properties among their members. Such group concepts¹ have been designated as "statistical."² Some other group concepts which refer to distributions of individual properties involve in their definitions some averaging process and are, therefore, more clearly statistical. An obvious example would be a concept such as 'the average income of Congressmen'; H. J. Eysenck's concept of 'cohesion' of political groups based upon the standard deviation of attitude test scores of their members is another example.³ Statistical concepts, since they rather clearly refer to distributions of individual characteristics, present no real problem for methodological

¹Notice that for the remaining discussion of group concepts, I shall not be so careful as I have been to distinguish group concepts from the properties to which they refer; however, where there is any chance of confusion from this practice I will make the distinction.

²Brodbeck, op. cit., p. 4

³The Psychology of Politics (London: Routledge & Kegan Paul, 1954), pp. 138-140.

individualism. Though the provision of adequate definitions for them does sometimes present difficulties, the use of such concepts in political inquiry does not ordinarily raise the serious problems frequently related to another kind of group concept, not yet discussed.

I am referring to such concepts as 'democratic,' 'totalitarian,' and 'having more power than' which are used to characterize states, and concepts like 'aggression' and 'imperialism' which are used to describe the behavior of states; also concepts like 'revolution,' 'rising nationalism,' 'industrialization,' and 'urbanization' which are used to refer to certain large scale social phenomena. Such concepts, as presently used, are never fully defined in terms of the properties and behavior of individuals. And, practically, it is doubtful whether the meaning of most of them could be fully specified in this way. Yet, as they appear in political science writing, such terms are in most instances meaningful. However, their meaning is rarely precise. As Professor Brodbeck observed, there is a "penumbra of vagueness" about them.¹

What bearing does the presence of, I would say the need for, such concepts have upon the methodology of political science? One possible conclusion, namely, that methodological individualism is a principle which

¹Op. cit., p. 5.

cannot be adhered to in the study of all political phenomena, is I believe clearly unjustified. That issue, as we saw, runs too deep to permit this conclusion on such a basis. Especially, in view of the notorious vagueness of these "non-statistical" group concepts.¹ Moreover, both of the features of these terms which might be summarized by saying that they are vague but meaningful, may be accounted for in a way that is fully consistent with methodological individualism. I will suggest how this might be done, not so much because this is a way of defending our principle, as because it is a way of further explaining why one might come to doubt it in political science. I will then suggest some practical advantages which ensue from a firm grasp of the principle.

Let us consider the evidence employed in research at the group level in political science. One interesting feature of it is that it is rarely, and in many areas it is never, the result of direct observations by political scientists. If a political scientist were studying the relations between, say, industrialization and the stability of political systems, much of his evidence, perhaps all

¹I will call this second category of group concepts non-statistical, though it is evident that a clear definition of the meaning of at least some of them, as presently employed in political science writing, would be of the kind we called statistical. For example, statements about nationalism usually may be interpreted as though this concept referred to certain attitudes distributed among the populace of a nation.

of it, would be taken from history books. , But even for contemporary cases, his most likely sources would be public and governmental documents. And if he were investigating some problem involving contemporary foreign policy he would not be likely to go out and observe individual behavior, though in this case, more than in the latter, it is easier to see how he might. The point is that we can, and we usually do, obtain information about group phenomena without observing individuals. Quentin Gibson suggested that it is this fact which suggests that groups are something other than the individuals who compose them. But, he adds, "such a view, however, does not follow from this fact."¹ He argues this position by going through various group facts and showing how they may be interpreted as facts about individuals.²

I will only suggest that the fact that we can get data about groups without observing individuals is not evidence against methodological individualism. We can find out what a certain foreign policy of the United States is without observing any individual, even if it is true, as I feel certain that it is, that a "foreign policy of the United States" is no more than a highly

¹Q. Gibson, The Logic of Social Inquiry (London: Routledge & Kegan Paul, 1960), p. 96.

²Ibid., Chap. 9.

complex combination of attributes of individuals.¹ And, under these circumstances, we may use group terms to characterize this foreign policy even though we cannot spell out exactly what it is. That is, we may use vague, not fully precise, group terms which are, nevertheless, meaningful.

What, then, are the practical advantages of adhering to methodological individualism in our handling of group concepts? Notice first, though, before I make these suggestions, that methodological individualism is a principle of the logic of science, it is not a procedural rule. To adhere to it does not mean that one must avoid all group concepts which he cannot fully define in individual terms. As Professor Brodbeck wrote:

The most that we can ask of the social scientist whose subject-matter requires him to use such "open" concepts [i.e., group concepts which have not been fully defined] is that he keep the principle of methodological individualism firmly in mind as a devoutly to be wished-for consummation, an ideal to be approximated as closely as possible. This should at least help assure that nevermore will he dally with suspect group-minds and impersonal "forces," economic or otherwise;

¹With respect to group characteristics which may obviously be described by statistical concepts, it is obvious both that we frequently get information about such group attributes without observing individuals and that these group attributes consist only of a combination of individual characteristics. For example, see S. Lubell, The Future of American Politics (New York: Doubleday, 1951), in which counties are characterized by their political affiliations on the basis of aggregative voting data, but it is clear that such groups and their characteristics are nothing but individuals (and, perhaps, a geographical area).

nevermore will nonobservable properties be attributed to equally nonobservable group entities. At the same time, he will not by methodological fiat be struck dumb about matters on which there is, no matter how imprecisely, a great deal to be said.¹

Our kind of advantage of the acceptance of this methodological principle is, then, the avoidance of some of the difficulties which might result from its rejection. I don't think that political science writing can be said to suffer to any significant extent from the most philosophically objectionable uses of group terms referred to by Brodbeck.² However, I do believe that some of our writing which deals with very macroscopic phenomena, as in the fields of international relations and comparative government, could be improved by a greater awareness of the first principle of scientific concept formation, and

¹Op. cit., p. 6.

²Ernest Nagel, however, has observed that "a hypostatic transformation of a complex system of relations between individual human beings into a self-subsisting entity capable of exercising causal influence ... is a recurrent theme in the history of social thought. Thus, political theorists have argued that a people possesses a "general will" that is distinct from the wills of its individual members and that may not even be an object of explicit awareness for the latter; psychologists have postulated "group minds" to account for ethnic and racial differences; sociologists have attributed a "psyche" to mobs in order to explain mass hysteria The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace & World, Inc., 1961), p. 537.

Closer to our interests, political scientists have pointed out difficulties that arise in various contexts from the "reification" of group concepts. See, for example, J. G. March and H. A. Simon, Organizations (New York: Wiley, 1958), p. 165.

methodological individualism certainly impresses that principle upon us with respect to group concepts.¹ For example, the distinction between definitions and empirical propositions is a crucial one, but this distinction is easily blurred when employing group concepts which have not been adequately defined; as a consequence, supposedly empirical propositions may be rendered tautological.² This suggests a kind of methodological analysis which may reveal conceptual problems in political inquiry at the group level and at the same time contribute to their resolution.

The practical advantages of a positive nature which result from the acceptance of methodological individualism arise from the fact that it shows clearly the direct

¹On conceptual problems in the study of international politics, see: R. C. Snyder, "Toward Greater Order in the Study of International Politics," World Politics, 8, 1955, 461-478; and with respect to the concept of 'power,' D. Sullivan, "The Concept of Power in International Relations," a paper delivered at the meeting of the Midwest Conference of Political Scientists at Indiana University, Bloomington, in May, 1960. These papers illustrate problems arising, in part, from inadequacies of definition which might have been avoided if more attention had been given to empirical import (the requirement set out in the principle of concept formation referred to above).

²For methodological investigations which show the importance of this difficulty with respect to functional analysis in social science, especially at the societal level, see the following: C. G. Hempel, "The Logic of Functional Analysis," in Symposium on Sociological Theory, ed. L. Gross (Evanston: Row, Peterson and Co., 1959), pp. 271-307, especially pp. 291-295; E. Nagel, "A Formalization of Functionalism," in Logic Without Metaphysics (Glencoe: Free Press, 1956), pp. 247-283, especially, p. 273.

connection between the study of group phenomena and the study of individual behavior. A political scientist concerned with inquiry at the macroscopic level who thinks of and attempts to define his group concepts in terms of individual concepts can more readily see the relevance for his own work of the findings of social sciences which focus upon individual behavior. Within political science, the principle of methodological individualism clarifies the link between the "traditionalists'" emphasis upon the study of political institutions, i.e., their focus upon inquiry at the group level, and the "political behaviorists'" focus upon individual behavior. Thus we arrive in the discussion of methodological individualism in this chapter at the same point where we terminated our discussion of methodological behaviorism in the last chapter; at the same time we have suggested in the present chapter the logical basis for the integration of two orientations in political science which were distinguished in our earlier discussion.

Both methodological behaviorism and methodological individualism are expressions of what we called the first principle of concept formation; the latter expresses it with respect to psychological concepts, the former with respect to group concepts. Our discussion of concept

formation so far has, then concerned itself only with the requirement that concepts have empirical import. This is the feature of concept formation with which most of the methodological writing about concepts is concerned. However, within any field of inquiry one must consider more than just the empirical import of his concepts; this something more has been aptly called theoretical significance.¹

The difference between empirical import and theoretical significance and the reasons why one must concern himself with both at the same time may be brought out by an illustration. Consider the concept 'legislative-ratio,' which is defined as follows: the 'legislative-ratio' is the number of members of the lower house of any legislature divided by the sum of twenty-seven plus the number of members of the upper house. After some simple qualifications indicating how this should be applied to other than two house legislatures, one would have no difficulty in computing the legislative-ratio for any legislature; that is, the concept has clear empirical import; it is even quantitative! Yet we can be almost certain that it is totally useless for the student of legislative behavior. Why is this? In short, it is because it is highly

¹Hempel, op. cit., pp. 39-50.

unlikely that the legislative-ratio is regularly (or lawfully) related to anything else; that is, the concept is not likely to play a part in any law or theory; it has no theoretical significance.

This brief exposition of the idea of theoretical significance introduces the second principle of concept formation in empirical science. Concepts without empirical import are empty; concepts which lack theoretical significance are useless. Relevant concepts in political science must, therefore, be consistent with both of these principles. An exposition of the significance of this claim and its defense are included in the next section.¹

B. Propositions and Explanation.

The claim I just made regarding relevant concepts in political science is a consequence of a broader thesis which may be stated as follows: political scientists, in order to accomplish the purposes which they have been pursuing, must seek reliable general knowledge in the form of scientific laws. I am not saying that political scientists have not

¹For further consideration of matters pertaining to the logical analysis of political concepts, see Chapter Five, Section C, Topic 2 and Chapter Seven, Section B, Topic 1. At these two places there is discussion of certain problems related to the distinction of qualitative and quantitative concepts, and "value-freighted" concepts.

sought knowledge of this kind, or even that they have not achieved some of it. But it seems to be undeniable that a great many of them (I would say most, but there is no need to make this claim for our purposes) have not made what might be called an informed commitment to this objective. This alone would justify talking about the thesis stated above; it would be gratuitous indeed to say that I intend to prove it, in any sense of this difficult word; actually I have, at this point, other reasons for talking about it.

Let me explain. In outline, the above thesis is based upon the following line of reasoning: political scientists are committed to the explanation (and, at least implicitly, to the prediction) of political phenomena; explanation (and prediction) logically requires scientific laws; therefore, the thesis stated above. It will be necessary to give some attention to the first of these premises, but the second premise is the crucial one. To clarify its meaning and support its validity requires an explication of what is designated by the two terms in it which we underlined -- explanation, and laws; and to do this, we must use such basic concepts as "fact," "generalization," "cause." All of these things which I believe must be included in any full treatment of the methodology of political science, so I wish to

at least mention them; also, some of them are required as preparation for analyses to be undertaken in the chapters which follow. What better way to introduce them and suggest their relevance to political science than as part of an argument for the thesis that political scientists must seek to discover scientific laws in order to achieve their purposes?

My strategy is this. I will begin by explaining abstractly and succinctly (and I hope clearly) the basic ideas of the methodology of science referred to above.¹ Obviously, I cannot explain them thoroughly; entire books have been written on the subject of explanation alone. I will try to say enough for our purposes here and in the chapters that follow. Then, I will attempt to show the relevance of these ideas, first to the thesis asserted above, and, then to additional kinds of methodological analysis in political science.

There are two kinds of propositions: analytic

¹There has been a great deal of writing on these subjects by philosophers of science and to the extent that we shall discuss them there is substantial agreement among most philosophers of science. As to the manner of presentation here, it owes most to G. Bergmann, Philosophy of Science (Madison: University of Wisconsin Press, 1957); and C. G. Hempel, "The Function of General Laws in History," Journal of Philosophy, 39, 1942; reprinted in H. Feigl and W. Sellars, Readings in Philosophical Analysis (New York: Appleton-Century-Crofts, Inc., 1949), pp. 459-471.

propositions and synthetic or factual propositions. Let us quote from our earlier discussion of this distinction in Chapter Two: "an analytic sentence has the form of either a logical truth (tautology) or a logical falsehood (contradiction); since it is true or false by virtue of its form alone, it says nothing about the world. ... Factual propositions assert something about the world."¹ We are only interested here in factual propositions. As we indicated earlier, and just quoted, they make assertions about the world. What they assert or state are facts.

Factual propositions or statements (remember, I use these words synonymously) may be divided into two kinds: singular propositions and general propositions or generalizations. Singular propositions state individual facts. Scientific descriptions, in one important meaning of this term, consist of a series of singular statements.² Generalizations state **general**

¹Above, p. 40.

²They may also contain statements which logicians call "accidental generalities." Accidental generalities are statements in a generalized form which are capable of a full restatement by a finite number of singular statements joined by 'and.' For example, the statement 'All the voters in Jackson County, Mississippi are white people' may be broken down into a long conjunction of singular statements describing each of the voters in that county. By contrast, the statement 'All voters favor candidates whom they believe will advance their economic interests' could not be restated in this way; it refers to all voters past, present, and future. The fact that it is false is irrelevant to this statement about its form.

facts. All scientific explanations contain one or more factual generalizations. This is one way of distinguishing description and explanation. It also reflects, I think rather clearly, the sense in which it might be said that science is entirely descriptive.

We arrived at explanation, our intended destination, but we do not yet have all the tools needed to help us understand it. Let us back off for a moment. What I mean by a fact is a state of affairs in the world: (1) an object having a certain property, (2) certain properties, appearing together, always, or a certain percentage of the time, (3) an event taking place, (4) one kind of event regularly following another, and so on. Illustrations (1) and (3) are kinds of individual facts; illustrations (2) and (4) are kinds of general facts. Factual statements, singular or general, are true if the facts they describe exist as described. This is discovered, immediately or ultimately, through observation. Scientific laws are factual generalizations which are true.¹

¹Some people would prefer to say they are "highly probable." But what they have in mind can, I believe, be stated just as clearly for the conception of a scientific law stated above. Thus I would say that a law is true, but whether or not any particular statement is a law is, at any particular time, more or less probable depending upon the available evidence. Both views of lawfulness thus reflect the basic, but usually not entirely clear, idea that no empirical statements are "certain." At the same time, the view I have taken reflects more obviously the fundamental idea that all empirical statements are either true or false.

We will comment further on the notion of a scientific law as we proceed. Now let us take a synoptic view of the nature of scientific explanation. Consider the following simple illustration. There is a pot of boiling water. We ask, why does it boil? The explanation follows:¹

- (1) This is water.
- (2) This is being heated.
- (3) Water if heated boils.
- (4) This boils.

Notice the following points: the three statements in the explanation, (1), (2), and (3), make up a deductive argument whose conclusion, (4), describes the event to be explained; statement (3) is a general law which in a sense "connects" (1) and (2) with (4); by virtue of this "connection" we call (1) and (2) the cause of (4). This shows the empirical and logical requirements for a fully adequate scientific explanation of an event.² It must contain at least one general law and one or more singular empirical statements describing conditions, antecedent or simultaneous with the event to be explained, such that a statement describing the event to be

¹This illustration is taken from Bergmann, *op. cit.*, p. 76; the discussion of the illustration is based upon Bergmann, pp. 75-84.

²To avoid any possible confusion, one should probably speak of explaining aspects of events or individual facts, since an event literally consists of a nearly infinite number of individual facts. Having said this, I will continue to speak of explaining events.

explained is a deductive consequence. Notice one thing further about this illustration. If we had observed the facts stated by (1) and (2) before the water boiled, and if we had known (3), then we could have predicted this event before it happened. This shows the logical similarity of explanation and prediction.¹

This introduces the basic ideas in which we are interested. The main point is that some reference to general laws is logically required for an adequate explanation or prediction; also statements asserting causal relations, since they amount to the same thing as an explanation, involve general laws -- i.e., if someone asserts that heating water causes it to boil, he in effect asserts the generalization that water if heated boils, for it is only by virtue of the latter that one may correctly assert the former. To argue the latter claim is the same as arguing for the claim that laws are required for explanation. We will discuss the matter below in the context of a consideration of explanations taken from political science writing. But first I wish to consider an illustration of explanation which differs in some important respects from the kind of explanation illustrated above.

¹As I discussed it, explanation was of an individual fact. The explanation of general facts or laws has the same form. In this case, one law is explained by deducing it from more general laws.

Consider the following illustration used by
C. G. Hempel:

when Johnny comes down with the measles, this might be explained by pointing out that he caught the disease from his sister, who is just recovering from it. The particular antecedent facts here invoked are that of Johnny's exposure and, let us assume, the further fact that Johnny had not had the measles previously. But to connect these with the event to be explained, we cannot adduce a general law to the effect that under the specified circumstances, the measles is invariably transmitted to the exposed person: what can be asserted is only a high probability (in the sense of statistical frequency) of transmission. The same type of argument can be used also for predicting or postdicting the occurrence of a case of the measles.¹

Perhaps, the simplest way to state the difference between this explanation and the one in our first illustration is in terms of the general laws upon which they depend. In the first case, a universal law was used; in this instance statistical laws were employed. The distinction may be simply indicated as follows: a universal law has the form 'If A, then B, always'; the schema of a statistical law is 'If A, then B, with a certain probability.'

My rendering of the form of a statistical law

¹"The Theoretician's Dilemma: A Study in the Logic of Theory Construction," in Minnesota Studies in the Philosophy of Science, Vol. II, Concepts, Theories, and the Mind-Body Problem, eds., H. Feigl, M. Scriven, and G. Maxwell (Minneapolis: University of Minnesota Press, 1958), pp. 37-98, at p. 38.

requires two comments. First, the statistical law need not actually include "a certain probability" in the sense of specifying a numerical value indicating the frequency with which instances of A are followed (or accompanied) by instances of B. This could not be done in the examples of statistical laws referred to in the above illustration. The indication of probability may be only that there is a general tendency for B to be associated with A. Of course, the explanatory power of such a law decreases as this indication of probability becomes less determinate. The second comment is that what I have designated as a "statistical" law is often called a "probability" law. I chose the label I did because all laws, including universal laws, are spoken of as more or less probable according to their degree of confirmation. Thus statistical laws are probablistic in two senses; to call them statistical helps to keep these senses distinct.

To return to our two kinds of explanation, let us briefly note one other obvious difference which is a consequence of the different kinds of laws employed, and one fundamental similarity. In the first type, which is based upon universal laws, we can say that if the propositions contained in the explanation are true, then a statement describing the event to be explained is certain to follow. That is the pattern of the

explanation is deductive. We recognize that the distinguishing feature of a valid deductive argument is that if the premises are true, the conclusion must be true. In the second type there is no such logical guarantee; its pattern is inductive; and, in an inductive argument the truth of the premises do not assure the truth of the conclusion. The fundamental similarity is this: both types of explanation employ general laws.

With this as a background, we turn to the subject of explanation in political science. I wish to suggest that, in a significant sense, probably every political science text ever written contains attempts to provide explanations for some political phenomena. That is, they have sought to explain some political facts by connecting them with other facts. Yet the explicit statement of empirical generalizations in political science writing is definitely the exception rather than the rule. I shall not attempt to substantiate these sweeping claims; what I shall do is try to clarify them somewhat. And I wish to present some argument for the view that these attempts to explain political facts by connecting them with other facts logically entails some reference to empirical generalities. This does not mean that the student of politics should not try to do the best he can to provide explanations and even predictions in areas where he is unable to

formulate the generalities which I maintain are logically required for adequate explanation and prediction; or even that he should explicitly state the relevant generalizations on every such occasion when he is able to do so.

The most frequent indication of explanation in political science writing when generalizations are not explicitly mentioned is the presence of what may be termed "connecting-words" -- i.e., words which imply a connection among facts, usually a causal connection. To illustrate, I will quote a number of statements from a book which also exhibits explanations in a form approximating that described above. I say this to make clear that my use of statements from this book implies no criticism of the author. Actually, my present purposes would probably be served as well by statements about political matters conjured up from my own imagination, for once what I have called "connecting-words" are pointed out in statements, the significance I have claimed for them is obvious. Consider, then, the following statements in which the "connecting-words" have been underlined:¹

- (1) England became unified at an early date, when Italy was completely divided; hence constitutionalism

¹C. J. Friedrich, Constitutional Government and Democracy (rev. ed.; New York: Ginn and Co., 1950).

could make substantial headway in England in the seventeenth century, but not in Italy. (p. 10)

- (2) The consolidation which followed was almost entirely lost during the Wars of the Roses, which in turn paved the way for Tudor absolutism. (p. 11)
- (3) Enduring common objectives engender organization. (p. 23)
- (4) The revolution of 1848 in France challenged the power of financial and industrial capital, and while its premature, radical experiments with socialism led to the Bonapartist reaction, it nevertheless heralded the coming of labor into its own. (p. 31)

Taken out of context the full significance of these statements cannot be understood, but I think they will, nevertheless, serve the purpose for which they were introduced. They all assert some causal connection among facts, and, in each case, it is the underlined "connecting-words" which are instrumental in making these assertions. Thus statement (1) asserts a causal relation between the development of constitutionalism and the unification of England. Actually, in the text this causal hypothesis is explicitly stated as follows: "only a firmly established government is capable of being constitutionalized."¹ In statement (2) the expression paved the way indicates that there was some causal connection between certain wars and Tudor absolutism. With respect to statements (3) and (4), to

¹Ibid., p. 10.

engender, lead to, or herald something no doubt means to make some causal contribution to bringing that something about. This illustrates only a few of the "connection-words" which usually indicate causal or explanatory statements, even though there is no explicit reference to generalizations. Others might be suggested, such as: 'on account of,' 'for this reason,' 'because,' 'consequently,' and the like. The use of such words in political science writing to assert connections among facts is, I suggest, one indication of the widespread presence of causal or explanatory propositions.

Predictions, like explanations, logically entail a reference to general propositions. I suggested earlier that political scientists have been committed to the prediction, as well as explanation, of political phenomena. What I meant by that claim is this. Political scientists have devoted much effort to recommending public policy and governmental reform. Such recommendations contain at least an implicit prediction -- namely, that if the recommendations are adopted, certain more or less well understood goals will be maximized. And this involves the assertion of a causal connection between one set of facts, those embodied in the recommendations, and another set of facts, those which constitute the goals.

At the beginning of this section, I stated my intentions in terms of two related tasks: (1) to support the broad thesis that political scientists must seek reliable knowledge in the form of scientific laws in order to achieve their purposes; and, with this task as a kind of vehicle, (2) to discuss some basic aspects of the methodology of science related to propositions and explanation. I regard the second task as completed. In so far as we have been able to show that political scientists are engaged in explanation and prediction of the kind described earlier, it may be said that we have supported the broad thesis repeated above. For one who believes that thesis, as I do, the question of the logical possibility of scientific political science becomes a paramount issue. The remainder of the thesis may be regarded as primarily concerned with the many ramifications of that issue; but those ramifications are so extensive that we shall have occasion to discuss many other lesser methodological problems in political science.

CHAPTER FIVE

ON THE POSSIBILITY OF SCIENTIFIC POLITICAL SCIENCE: AN ANALYSIS OF ANTI-SCIENCE ARGUMENTS

Writing on the question of the possibility of a science of politics in 1929, William Y. Elliott listed two dominant themes which "constitute the main grounds generally asserted for denying a scientific nature to the study of human activity in general ... and to politics in particular": (1) political science is an ideographic discipline, "since like history it must concern itself with an undistorted picture of a concrete reality in which events are unique";¹ (2) political science is a normative discipline and a subject "cannot be a science in so far as it is normative".² Taking these **assertions** broadly including all of their many ramifications and associated doctrines, as Elliott interprets them, I

¹W. Y. Elliott, "The Possibility of a Science of Politics: With Special Attention to Methods Suggested by William B. Munro and George E. G. Catlin", in S. A. Rice, ed., Methods in Social Science (Chicago: University of Chicago Press, 1931), p. 70.

²Ibid., p. 70; quoted from Levy-Bruhl, La Morale et la Science des Moeurs (Paris: F. Alcan, 1913), Chap. 2, especially pp. 11-14.

believe he has correctly expressed the traditional arguments.¹ The second set of arguments, those surrounding values, have been -- in my estimation -- more influential; we will attend to them below in Chapter Seven. In the present Chapter, we will examine the complex of views bound up in the assertion that political science is an ideographic discipline; for expository purposes we will divide them into three categories, the first of which deals directly with this claim.

A. Political Science is Ideographic.

A recent expression of this conception of political science by David G. Smith² is almost identical with Elliott's characterization of the doctrine. According to Smith, political science is one of the ideographic sciences

¹It was not Professor Elliott's intention to clarify and evaluate these doctrines. At times he seems to accept the anti-science arguments -- e.g., in opposing W. B. Munro's proposal that political scientists turn to physics for their method ("Physics and Politics; an old analogy revised," American Political Science Review, 22, 1928, 1-11), he asserts, "Social happenings have the historical character of unique events, and history does not, like physical nature repeat itself." (p. 79) Thus Catlin, in commenting upon the article, directs his arguments in favor of a science of politics against Elliott. (G. E. G. Catlin, commentary following Elliott's article, pp. 92-94). But in all fairness, we should observe that Elliott's position is probably most correctly described, as he described it himself in places (especially pp. 80, 87, 91), as the view that political science is partly science and partly philosophy, and that the possible achievements of the scientific part are severely limited by the complexity of the subject matter.

²"Political Science and Political Theory," American Political Science Review, 51, 1957; 734-746.

which "deal with unique, more or less extended temporal events, seeking to represent them fully and exhaustively rather than abstractly." It is therefore distinct from the "nomothetic sciences, corresponding roughly to the physical sciences, (which) search for abstract, universal laws."¹ Smith thus expresses the traditional argument: scientific method is appropriate for the study of repeat-able physical events and processes; it is not adequate for the study of individual persons and social events which are unique and non-recurrent.² For the moment, we will refrain from commenting upon the ambiguity of the ideas of "uniqueness" and "exhaustive description." Instead I will relate a story told by Professor Gustav Bergmann when discussing this and similar issues.³ It is, I believe, quite

¹Ibid., p. 735.

²There is by now a fairly large literature concerned with the analysis of the traditional arguments against the possibility of social science. For this one -- we may call it the argument from uniqueness -- see: G. Bergmann, "Holism, Historicism, and Emergence," Philosophy of Science, 11, 1944, 209-221; E. Nagel, "The Logic of Historical Analysis," Scientific Monthly, 74, 1952, reprinted in H. Feigl and M. Brodbeck, eds., Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, 1953), pp. 688-700; A. Grunbaum, "Causality and the Science of Human Behavior," H. Feigl and M. Brodbeck, op. cit., pp. 766-777. Our discussion follows the same mode of analysis exemplified in these articles.

³In a course on the philosophy of social science given at Northwestern University, Evanston, Illinois, in the summer session, 1958.

instructive.

During the invasion of Germany in World War II it was our policy to attempt to capture enemy research laboratories before they could be destroyed by their retreating armies. In one of these, as the story goes, there was an unusually intricate, immense electronic apparatus which was apparently intact, but there were no instruction manuals or any other indication of either what it was for or how it operated. At first American engineers were genuinely perplexed. Here was a piece of equipment which was, as far as they were concerned, indeed unique. Yet, after many false starts and several exasperated suggestions that it may be only a confused conglomeration of electrical circuits and mechanical gadgets left behind on purpose by the Germans to inflict mental anguish upon their enemies, the enigma was solved. The engineers discovered its purpose and exactly how it operated. That is, they explained the unique. How could they do this? Practically, it involved discovery of the types of component electrical circuits and mechanical parts and the way in which these were combined. Logically, it consisted of the application of known elementary laws and composition rules (laws of another kind). The elementary laws accounted for the nature of the components; the composition rules explained the result of their combination.

This is something of an oversimplification (and the story may be fictitious) but it shows us two things:

first, it illustrates an empirically meaningful sense of uniqueness -- i.e., uniqueness through complexity (through a new configuration of universal characteristics); and, second, it shows one way physical science must and can also deal with unique phenomena.

But we did not have to be so elaborate. Isn't it enough to refute the argument from uniqueness to merely point out that in the broad sense in which every person and social event is unique, so is every physical object and event -- each tick of my watch as well as every eclipse of the sun? When an argument against the possibility of using scientific method in political science would be just as effective against its use in physics, we cannot take it seriously. Furthermore, when we attend to the language used in describing anything we find common names and predicates designating properties (or universals) which are usually exemplified by many different individual things (or particulars). That is to say, our descriptions tend to emphasize the repeatable characteristics of objects and events, not their uniqueness.¹

¹Morris Cohen referred to language in this way when deprecating emphasis upon the uniqueness of historical events. He wrote, "The absolutely unique, that which has no element in common with anything else is indescribable, since all description and all analysis are in terms of predicates, class concepts, or repeatable relations," The Meaning of History (LaSalle, Illinois: Open Court Pub. Co., 1947), p. 84.

A simple example will illustrate both the generality of our descriptive vocabulary and the empirically meaningful idea of uniqueness referred to above. Consider the description of the American Presidency given by Clinton Rossiter in The American Presidency.¹ Rossiter describes the office in terms of ten roles which it exemplifies: chief of state, chief executive, chief diplomat, chief of party, "the Leader of a Coalition of Free Nations," etc. Notice four things. First, Rossiter refers to a specific (particular) office by merely using its label or proper name -- 'the American Presidency.' Second, he describes that office by attributing characteristics, the roles, to it which are exemplified by many other political offices in different countries: i.e., he employs descriptive concepts which refer to properties (universals). Third, the description as a whole delineates an office which probably is as a matter of fact unique. Fourth, this illustrates the two ways in which uniqueness enters our descriptions: (1) logically, through the use of proper names²; (2) factually, by describing a sufficient number

¹(New York: The New American Library, 1956), Chap. 1.

²A complete account of this subject would also include reference to two other ways of referring to particulars: (1) by demonstrative pronouns like 'this' or 'that'; (2) by the use of a "definite description", such as 'the man who was president of the United States in 1953', which is, in a sense, equivalent to the proper name, 'Dwight D. Eisenhower.'

of the general features of something so that there is nothing else which exemplifies all of them. It is a fundamental tenet of empiricist philosophy that any particular thing may be so distinguished without describing "all" of its characteristics. We shall see in a moment that this is denied within the philosophical tradition from which Smith derived his distinction between nomothetic and ideographic sciences.

This brings us to another aspect of this traditional complex of arguments. I refer to the idea of an "exhaustive description." Witness the quotation above (p. 146) in which Smith speaks of "seeking to represent them [political events] exhaustively rather than abstractly." The idea is that science abstracts and therefore distorts, but in studying social phenomena we must depict events in all their "concrete uniqueness." Again a consideration of the language of descriptions reveals that, though we may use proper names to label things, we use general concepts to describe them, and, in this way, we describe only some of their characteristics. Indeed the goal of "exhaustive description" is an idle fancy. Consider the simplest physical object. Among its characteristics is the fact that there are other particular objects at certain spatio-temporal distances from it and that these objects have certain characteristics including additional relational properties. As a consequence, to exhaustively describe

anything is tantamount to describing everything -- past, present, and future.¹ The pattern of reasoning is a familiar one. It is the dialectic of German idealism. Hegel did not shrink from its consequences: a corollary of the Hegelian doctrine of "internality" of all relations is "the view that one cannot have competent knowledge of anything unless one knows everything."² It is interesting that Smith derives his conception of political science from the post-Hegelian German philosophers of history -- Wilhelm Windelband and Heinrich Rickert.³ We need not consider the philosophical doctrine; in the words of Professor Nagel: "It will suffice here to note that, were the doctrine sound, not only would every historical [or political] account ever written be condemned as a

¹In addition to the references given at the beginning of this discussion, see Carl G. Hempel, "Symposium: Problems of Concept and Theory Formation in the Social Sciences II," in American Philosophical Association, Eastern Division, Vol. 1, Science, Language and Human Rights (Philadelphia: University of Pennsylvania Press, 1952), pp. 65-86, especially pp. 74-75. We should note that there is a scientifically significant sense of complete or exhaustive description. It is, however, always relative to a theory. Thus, we may call a description of the state of a system complete when it specifies the values of all of the variables mentioned in the theory describing that system. For example, relative to the Newtonian theory of mass points (prerelativistic celestial mechanics), a description of the mass, position, and velocity of the celestial bodies making up our solar system is a complete description of that system. See, G. Bergmann, Philosophy of Science (Madison: University of Wisconsin Press, 1957), Chap. 2, especially pp. 84-115.

²Nagel, op. cit., p. 692.

³Op. cit., p. 735.

necessarily mutilated and distorted version of what has happened, but a similar valuation would have to be placed on all science, and indeed on all analytical discourse."¹

Usually assertions of the kind we are considering -- that social sciences are ideographic; that they deal with the unique; that abstraction must be avoided in favor of exhaustive description -- are accompanied by advocacy of some special methodology. Most often, it is some form of empathic understanding or verstehen. Such "understanding" is no doubt useful, especially in a heuristic way, in social science, but it "does not, however, add to our store of knowledge ... nor does it serve as a means of verification."² David Smith does not explicitly advocate verstehen in this article; however, he does advocate a special method. It is not clear what this "method" is, but the following quotation gives us some idea:

Lastly, much of the knowledge sought by the ideographic sciences is not, to adopt John Wisdom's terms, "discovery by investigation," but "discovery by reflection." Such discoveries are not "in the facts," but are the

¹Op. cit., p. 692.

²T. Abel, "The Operation Called Verstehen," American Journal of Sociology, 54, 1948; reprinted in Feigl and Brodbeck, eds., op. cit., pp. 677-687, at p. 687.

result of perspective, insight and grasp. In political studies, a statement such as "The national parties are collections of state and local machines," is a homely example. They are the kind of discoveries reported in the literature of psychoanalysis, in the novel, or in the imaginative treatment of social themes. They are rooted in fact and matured opinion, and seek to summarize experience and insight and to tie knowledge and meaning together.¹

It is difficult to decide where to start in the analysis of such a claim. We might begin by observing that the claim is perplexing, that it is in need of analysis. What could he mean by saying that the sentence 'The national parties are collections of state and local machines' does not state something which is "in the facts"? Where else is it? Either the parties are this way, which is to say the sentence states what the facts are, or they are not.² But it may be that he does not say this. Perhaps he is only asserting that the discovery of these facts is not itself "in the facts." If this is what he intends, and I suspect that it is, then he has chosen a very ambiguous way of asserting a commonplace. Namely, that there are the facts and there is the discovery of them; that these are two things and not one. And that the whole process of discovery

¹Smith, op. cit., p. 736.

²For a similar criticism of Smith, see: A. A. Rogow, "Comment on Smith and Apter: or Whatever Happened to the Great Issues?" American Political Science Review, 51, 1957, 763-775; especially pp. 766-767.

involves more than merely observing, that it also involves such things as "perspective, insight, and grasp."

I chose this interpretation of the passage because it is the only one -- in my estimation -- which makes sense, and also because there are several other passages in the article which suggest the author's concern for the importance of creativity in the study of human behavior.¹ Moreover, this may account for his likening the discovery about American political parties to "discoveries reported in the literature of psychoanalysis, in the novel, or in the imaginative treatment of social themes," since these are usually taken to be the result of much creative imagination. This suggests what appears to be the basis for some of the confusion in Smith's paper.² He fails to appreciate the distinction between the origin of ideas or the hypotheses which express them, on the one hand, and the processes related to their confirmation or testing, on the other. Following Hans Reichenbach we will call these the context of discovery and the context of justification.³ The former may, and probably often does, require **much creative** imagination, insight (and even "grasp!"); the latter requires systematic observation, though it would

¹Smith, op. cit., see, e.g., p. 741, the second paragraph and footnote, and p. 745, the third paragraph.

²Rogow (op. cit.) points out some rather patent shortcomings of other aspects of Smith's article which have not been mentioned here.

³Experience and Prediction (Chicago: University of Chicago Press, 1938), pp. 6-7.

be a mistake to neglect the fact that the devising of appropriate observational techniques may also require a great deal of ingenuity. Finally, we should note that all of this is just as true whether we are studying political parties, mental illness, or cosmic-ray reactions upon atomic particles. Recognition of the importance of creativity in an area of inquiry is not evidence of the inapplicability of the techniques and methodology of science to such inquiry. Smith's point in this regard -- if he has one, and I think he has, even though he does not explicitly state it -- is that the less developed a science is, the more need there is for creativity in pursuing it.

B. Political Science is Art.

The next closely related anti-science conception of the discipline we will consider may be summarized by the assertion that the study of politics is an art, not a science. This view is also -- if I am not mistaken -- based in part upon a concern for the significance of the more creative aspects of the study of politics and a similar failure to distinguish the contexts of discovery and justification. It is also based upon the blurring of at least one other fundamental, though perfectly obvious, distinction: politics and the study of politics. Both a failure to maintain these two distinctions and a

concern for creativity in the study of politics seem to be an important part of an early article by Professor A. T. Mason, which is primarily concerned with showing that "politics [and the study of politics?] is art rather than science."¹ Mason indicates that he is concerned with the reasons "for the comparatively backward state of the social sciences" in general and of political science, in particular.² One reason offered is "the peculiar nature of politics itself."³ Here it is obvious that he is speaking of politics, not the study of politics. But in the next paragraph he writes: "It is doubtful whether politics can ever be a science as botany, chemistry, or mathematics are sciences."⁴ Somebody might say this about political science, but would they say it about politics? And, still concerned with the reasons for the backward state of the study of politics, he observes that "rarely has political speculation been motivated by true scientific purpose."⁵ There is no doubt that he was speaking of politics, not its study, when he wrote: "The art of politics consists also in controlling, by whatever means

¹"Politics: Science or Art?" Southwestern Social Science Quarterly, 16, No. 3 (December, 1935), 1-10, p. 10.

²Ibid., p. 1.

³Ibid., p. 3.

⁴Ibid., italics mine.

⁵Ibid.

will work, the indifferent or supine element in society."¹
 And it seems he was referring to the study of politics in
 the following passage:

The conclusion is that politics is a
 science only in a very limited sense.
 In the political process there is no
 way of controlling conditions, a
 control so necessary for the establish-
 ment of scientific conclusions.²

Mason is concerned with both politics and its study,
 but how can we discern when he is talking of one, the
 other, or both in this concluding passage?

Politics is art rather than science,
 an art which has never been codified
 or completely explained; nor can it
 really be learned by people lacking
 in what may be called political sense,
 which presupposes native talent and
 highly intuitive technique. Any
 concrete political situation, however
 seemingly transparent, cannot be dealt
with by reason alone; it can only be
grasped by a process similar to
artistic perception. How, in a given
 situation to seize power or extend
 one's dominion; how to ... [etc.].
 Such knowledge is gained by instinct
 and experience; reason helps little
or not at all.³

Most of the words which I have italicized are apparently
 intended to express the non-scientific characteristics of
 the study of politics. I will reserve further comment on
 Mason's argument until we have reviewed a more recent

¹Ibid., p. 8, italics mine.

²Ibid., p. 10, italics mine.

³Ibid., italics mine.

paper on the subject by D. G. Hichner and W. H. Harbold;¹ they state explicitly the main argument which is more or less implicit in Mason's paper.

In order to elaborate on their assertion that "the study of politics today is in a state of considerable uncertainty," Hichner and Harbold refer to the existence among students of politics of "intramural debates, long-standing but still continued without decision, over whether politics [italics mine] is an art or a science."² It may be recalled that when I referred to the latter statement in another context earlier (p. 24), I expressed some doubt concerning their use of the term 'politics.' If we rely upon contextual cues it appears that they were using the word in its ordinary sense, but then their assertion -- it seems to me -- is patently false. Are there political scientists who claim that politics is a science? I have never read or heard this claim;³ they do

¹"Politics in Perspective," Association of American Colleges Bulletin, 42, 1956, 298-309.

²Ibid., p. 298.

³Having remembered that Professor Charles S. Hyneman had not mentioned any debate over whether politics is (or ought to be) a science in his examination of the political science discipline, The Study of Politics: The Present State of American Political Science (Urbana: University of Illinois Press, 1959), I asked him about this subject in a recent conversation (December, 1960). He said that he had never come across the claim that politics is a science; that he was certainly not aware of any debate over this. Furthermore, it is appropriate to mention, Professor Hyneman made the wholly unsolicited comment that he had noticed a peculiar tendency of some writers to use the word 'politics' to mean the study of politics, and sometimes to use it in such a way that it was not clear which meaning was intended.

not mention anyone who ever took this position. Now there are people who have thought it worthwhile to point out that politics is not a science, and to present evidence and argument in support of this view¹ -- both Mason in the article discussed above and Hichner and Harbold in this one are examples. These writers, and I believe others as well, were not particularly concerned with proving that politics is not a science, but they apparently felt that this circumstance was at least congenial to their views regarding the study of politics. We saw this in the case of Mason; the following quotations show it even more clearly for Hichner and Harbold:

Politics certainly was at first, and to a large extent remains, an eminently practical art -- "the art of the possible."²

¹In The American Science of Politics: Its Origins and Conditions (Berkeley: University of California Press, 1959), Bernard Crick "seeks to explain the special plausibility to American students of politics of the view that politics can be understood (and perhaps practiced) by 'the method of the natural sciences.'" (Italics mine, p.v.) This is Crick's stated objective; in so far as he sticks to it, his book belongs to the sociology of knowledge, not to the methodology of science. To take the former for the latter, or to draw conclusions about the latter from the former, is a mistake -- it is an instance of what is appropriately called the "genetic fallacy." In Crick's attack upon the appropriateness of scientific method for the study of politics, he is -- if I am not mistaken -- frequently guilty of this logical blunder. Crick's arguments are difficult to analyze. He seems to emphasize literary merit at the expense of logical analysis; where the account seems to demand argument, he is apt to substitute a well-turned phrase. I could, I believe, document these charges but this would require more than footnoting; I would have to show it by analysis. At many places in the thesis I have had to make choices about inclusion and exclusion. In Crick's case I have chosen to take him at his word. The sociology of political science is not its logical analysis; I am interested only in the latter.

²Op. cit., p. 303.

We have thus sought to show that politics as a field of study must be approached with a method adapted to the nature of its object, the governmental process. As this may be seen to involve art, science and philosophy, so must the study of politics employ art, science¹ and philosophy to understand it.

Unlike Mason, Hichner and Harbold do not -- except possibly in the instance cited earlier -- blur the distinction between politics and the study of politics, and in this way proceed to attribute characteristics of the former to the latter. Instead, they seem to argue explicitly what appears to be implicit in Mason's paper: namely, the doctrine that the study of politics to be successful must exhibit certain features of politics. The passages just quoted exhibit this kind of reasoning quite clearly. But would they really claim that a method of inquiry must reflect the characteristics of its object? Does research in the phenomena of extra-sensory perception (assuming there is such a thing) require extra-sensory perception? Or, perhaps somewhat more to the point, must research seeking to discover social conditions associated with the prominence of various art forms be done "artistically"? Of course, the last two questions are rhetorical. The answers are patently negative. Perhaps it would be a better critical strategy and maybe it would be more correct -- I know it would be more kind -- to interpret the

¹Ibid., p. 309.

second of the above quotations as primarily stylistic, a kind of rhetorical summing up of, rather than an argument for, a point of view. If this is the case, then I believe the burden of Hichner and Harbold's argument rests upon the same logical blunder that David Smith seemed to commit -- the failure to distinguish the contexts of discovery and justification. Mason is also guilty of this error, so I will comment again on his paper at this point.

Consider the following passage from Hichner and Harbold, which I believe represents their main argument:

To some degree a share of the political scientist's attention must be devoted to the art of governing as such, not only because this aspect of the subject does not -- or will not yet -- lend itself to scientific inquiry, but because it seems capable of revelation by a kind of impressionistic treatment akin to artistic perception. It obliges the political scientist, in consequence, to concern himself with matters of taste, balance and harmony.¹

and recall part of a previous quotation from Mason:

Any concrete political situation ... cannot be dealt with by reason alone; it can only be grasped by a process similar to artistic expression.²

For reasons such as those expressed in the above quotations the study of politics is said to require the use of art and, for Hichner and Harbold, philosophy in

¹Ibid., pp. 303-304, italics mine.

²Above, p.157, italics mine.

some manner which makes the methodology of political science radically different from that of the natural sciences.¹ However, the crucial question concerning the views reflected in these quotations is this: how do you determine the truth or falsity of what has been "grasped" or arrived at by "revelation," in both cases "by a process similar to [or "akin to"] artistic perception" (or by any other process!)? Of course, there have been and there still are systems of thought within which this question would be rejected. In some kinds of metaphysics and theology something like "intuition" or "revelation" may be regarded as a sufficient guarantee of the truth of what is arrived at in this way. But -- as I see it -- this alternative is not open to a political scientist, and the writers we have been discussing probably would not choose it, if it were. The answer was, I believe, adequately developed earlier, in Chapter Three.. For the present it may be summarized

¹For a later statement by Harbold and Hichner, see their "Some Reflections on Method in the Study of Politics," Western Political Quarterly, 11, 1958, 753-773. In this article they concluded that political science should be a combination of "history, philosophy, and empirical research." As they put it: "political inquiry ... must be a synthesis of these three modes of thought and study." (p. 773). This appears to represent a changed conception of the discipline, but immediately afterward they add: "If asked for rules for the creation of that synthesis we can only suggest that science itself is perhaps an art." If I understand this paper correctly, their conclusions concerning the nature of political science are about the same as those of the earlier paper. However, their arguments are different. We will briefly consider them below.

as follows: the truth or falsity of ideas about political phenomena must be determined by a more or less systematic and more or less direct confrontation of the facts, regardless of the origin of those ideas.

I wish to raise one more consideration before presenting my concluding remarks on the point under discussion. As I said, I believe the problem of determining truth or falsity is the crucial one here, but a logically prior question is actually of equal significance for our general question concerning the appropriateness of scientific method for the study of politics. In the present context, we may raise that question by asking: What are the standards or principles which must be adhered to in order to adequately state what one has "grasped," achieved by "artistic perception," intuited, etc.? This is what is usually discussed under the heading of "the principles of concept formation," as it was in Chapter Four. Here it will suffice to say that an adequate statement is one which permits testing by some kind of "confrontation of the facts" or evidence -- i.e., a statement whose truth value is determinable.

The question I just introduced concerns meaning; immediately before I introduced the question of truth. It is the manner in which these two related subjects are handled which is -- in my estimation -- an excellent indicator of the scientific status of any area of

intellectual activity. I am not suggesting that the test is what is said about meaning and truth; there is no reason why scientists should speak in the abstract about either of these subjects. I am referring to the conceptions or standards of meaning and truth reflected in what is done in the area under consideration. Those summarized above from our earlier discussions in Chapters Three and Four are, I believe, appropriate for any field of scientific inquiry. Needless to say, I maintain that they are appropriate for the study of politics. But the point here is that the scientific conceptions of meaning and truth are not in any way inconsistent with the use of artistic perception, grasp, any process for achieving revelation about governing, or any other way of arriving at ideas about politics. Grasp, operationalizing, intuition, and systematic observation may all be part of any actual scientific inquiry. I made the same point earlier when discussing David Smith's failure to distinguish the contexts of discovery and justification. Now I make it with respect to apparently the same mistake by Mason and Hichner and Harbold. Something so fundamental that is frequently disregarded is worth repeating.

It is apparent from our discussion so far that some writers who emphasize the place of art in the study of politics, do so in a somewhat confused manner. And I have

tried to show that this confusion plays a part in some anti-science conceptions of political science. However, there is what might be called a meaningful core of such conceptions of the discipline. I have already commented upon one aspect of it: a recognition of the importance of creativity in the study of politics.¹ Another is a reflection of the prominent concern among political scientists for seeking solutions to practical problems. Just as the term 'art' connotes creativity, it is also used to refer to practice.² An emphasis upon both creativity and practical problems seems to reappear frequently in writing opposed to scientific political science. It is clearly evident in the papers by Smith and Mason discussed above, and in those referred to below which found their anti-science arguments upon issues

¹For a discussion of the importance of art as creative imagination within the framework of scientific social science, see, for example, Robert Redfield, "The Art of Social Science," The American Journal of Sociology, 54, 1948, 181-190.

²See, for example, Quincy Wright's discussion of "The Arts of International Relations," Chap. 8 in The Study of International Relations (New York: Appleton-Century-Crofts, 1955).

related to values.¹

What I want to suggest at this point is that some of the intellectual foundations of **views** which deny the appropriateness of scientific method for the study of politics are rooted in an orientation toward practical problems. I believe some connection between this kind of orientation and most such views could be demonstrated. I shall only attempt to show one structural connection with one kind of anti-science argument.

The kind of anti-science arguments to which I am referring are, once again, those related to the conception of political science as an ideographic discipline. So it is not surprising that a quotation from the paper we discussed earlier with respect to that doctrine should provide an illustration of the present point. Consider the

¹See Chapter Seven. For another example, see Hans J. Morgenthau, Scientific Man vs. Power Politics (Chicago: University of Chicago Press, 1946), and the critical review by Ernest Nagel in Logic Without Metaphysics (Glencoe: Free Press, 1956), pp. 377-382. Morgenthau opposes the scientific study of international politics primarily on the grounds that the subject is such that only the "more than scientific-man," the statesman, is able to correctly interpret it through a kind of creative insight gained from practical experience; and, consequently, because scientific study cannot resolve actual problems that arise in "politics among nations." Everyone will recognize the phrase I just enclosed in quotes as the name of one of Professor Morgenthau's books which, in my estimation, is certainly a contribution to the science of international relations. This is an example of a general impression I received from my study of writing on the methodology of political science: political scientists frequently do better in pursuit of their subject than they do in writing about their subject.

following passage from David Smith's article:

Political science is not a "behavioral" science, concerned with general laws. The subject-matter of politics is basically the specific problem and particular institution: a constitution for post war Germany, the present goals of Soviet foreign policy, the legislative process in Congress, or the reform of a city or state government.¹

This illustrates what is sometimes regarded as a fundamental conflict between a concern for particular events, institutions, or problems and the scientific frame of reference which emphasizes the discovery of general knowledge. As a matter of fact, regardless of whether or not there is a conflict, there surely is a difference. Anyone seeking to discover lawful relations, the principal goal of almost every science, would rarely focus his attention upon a single instance of what he is studying. The intensive study of a single case is more likely to occur when that case is approached for some practical purpose. This is the structural connection to which I referred. The quoted passage shows it clearly. Simplifying to make it stand out, the argument might be made as follows: (1) political science has as its object the solution of practical problems;

¹Op. cit., p. 737. In view of our prior discussion, it may be of interest to notice this author's use of the word 'politics' to refer to political science in the second sentence of this quotation. On this occasion, there is no ambiguity; the context makes perfectly clear what is intended. But, I must say, every instance of such usage leaves me a little uneasy, if not suspicious that an essential distinction is about to be forgotten.

(2) practical problems related always to more or less specific events, institutions, policies, etc.; (3) scientific method is primarily a way of discovering general laws; therefore, (4) scientific method is not appropriate for political science. We do not have to deny any premises of the argument, though I would certainly question the first one, in order to refuse to accept its conclusion. My indirect argument proceeds in this way.

It so happens that in the social sciences, unlike the natural sciences, there is no separation between what we may for emphasis call pure science and engineering. But this does not mean either that there are no precise logical distinctions between these two activities or that they are unrelated. Both the distinctions and the relations may be drawn in a number of ways. For now, I shall only view them from the standpoint of the logic of science. Here the distinction is between the acquisition of knowledge in the form of laws and theories, and the use of such knowledge. The relation is implied by the way I stated the distinction: practice logically requires the use of this kind of knowledge. To demonstrate this, first one must show that all practical activities involve, in a significant sense, prediction; and, second, that prediction requires general knowledge -- i.e., knowledge of lawful relations.¹

¹See, e.g., Q. Gibson, The Logic of Social Inquiry (London: Routledge and Kegan Paul, 1960), pp. 17 and 200-204.

But this is not a matter for argument at this time. Argument for the latter was included in the last chapter, and I don't think the former really needs to be argued.

Actually, the assertion that practical activities involve prediction may very well be tautological. Would we call an activity practical unless it were done with some end in view, and doesn't any such action involve a prediction that it will contribute to that end? Even though I will give no further argument for these claims, an illustration of their application in the present context may be helpful.

Suppose we are, as political scientists, called upon to make recommendations to state legislators who are interested in improving the assessment of personal property taxes. We might very well recommend that the position of tax assessor be filled through a civil service examination emphasizing professional qualifications, rather than by political appointment. The point is that there is here at least an implicit prediction that if the job is filled one way rather than another the consequences will be different, and they will be different in a way related to certain understood (if not explicitly stated) criteria of improved tax assessment. And if anyone asked us why this is the case, our answer must -- it seems to me -- involve some appeal to general knowledge, even if it is only a statement to the effect that these improvements have resulted from similar changes in other states. In this instance, the reference to general knowledge would still

be implicit -- namely, the unstated generalization that there is a lawful connection between a certain kind of change and a certain kind of improvement.¹ The information about experience in other states would constitute evidence for the generalization.

A further comment on the claim that some form of generalization is logically required as an intermediate step in the inference from what happened in some states to what will probably happen in the state in which we are interested may avoid one type of possible misunderstanding. When I say that prediction logically requires the use of generalizations (or, strictly speaking, scientific laws), I do not mean that in a case such as the one described above, a political scientist is somehow obligated to state generalizations in support of his advice for remedying certain tax problems, or even that his performance would necessarily be improved if he did attempt to state them. Indeed, if the only basis for the advice in this case was the experiences of other states, nothing would be added by attempting to explicitly generalize from this evidence. Nor am I asserting that the political scientist in this case has consciously or even unconsciously made such a generalization. Asserting this logical requirement is neither practical advice nor psychological analysis. Its

¹Of course, the generalization need not assert a simple causal connection, it may only assert a tendency for these things to be associated.

wider recognition might have practical effects; it would, I believe, contribute toward the theoretical development of the discipline. A consideration in support of the latter possibility **will** be related at the end of our discussion of the present topic.

The counter position to the thesis that the objective of resolving particular practical problems constitutes an adequate basis for rejecting the scientific frame of reference is clear. Such an objective demands the kind of knowledge which can be derived only through inquiry proceeding within that frame of reference. Actually, it is possible to accept this, and, nevertheless, assert that political science should not be a science. Thus, one might argue that the sole function of the discipline is to render services with respect to matters related to government and politics -- i.e., to put knowledge to practical use.

Against such a view I would raise three considerations. First, most of the knowledge required in order to adequately deal with the problems in which political scientists are interested is not yet available. So a wholly engineering-type political science would have to expediently make do with whatever knowledge is available, as we must indeed do at any particular time, and wait on the discovery of additional relevant knowledge by the other social sciences in order to improve its performance. Second, the general knowledge available in political science and the other

social sciences is rarely of a kind which can be applied directly to practical problems. More or less research is almost always needed to acquire additional knowledge specifically related to the particular problem at hand. Finally, it is obvious that even some of our most practically oriented political scientists show by their own research activities that they are not prepared to accept such a limited function.

I have finished discussing the one aspect of the relations between the practical concerns of political **scientists** and the scientific study of politics which we set out to examine. In doing so, I emphasized one source of conflict between these two aspects of the discipline -- namely, the support or, more precisely, pseudo-support which a realization of the importance in the discipline of attending to practical problems lends to some anti-scientific views concerning the study of politics. And I have also asserted a relation of dependence of our more practical activities upon the theoretical achievements of the discipline. There is an important relation in the other direction which we should mention before leaving this subject, but first it is appropriate that we take note of another and considerably more extensive kind of discord between the practical functions and scientific progress within the discipline.

Several writers have recently commented upon the matter. For example, David Easton stated that "the vast

bulk of research [in political science] has dealt with problems of reformative or applied as against pure science,"¹ and he concluded:

To the extent that excessive attention to questions of political reform siphons off resources that might be devoted to the search for uniformities in political relations, the development of research towards theory is thereby retarded.²

Another passage in which Easton makes the same point is interesting because in this one he speculates about the reasons for what he regards as an overemphasis upon the practical.

Today in political science there is little clear distinction between pure and applied research; in the same classroom and in the same research worker attention must be divided between the two. Efforts at application are always initially the more appealing and in a short-run sense, because of the tremendous difficulties confronting pure research, the more easily undertaken. The study of causal relations as a distinct enterprise has therefore been jeopardized and the whole problem of systematic theory has been driven deep into the recesses of political research, where it is scarcely visible.³

And V. O. Key was concerned with the same problem when, after considering the many practical demands made upon the discipline, he asserted:

¹The Political System: An Inquiry into the State of Political Science (New York: Knopf, 1953), p. 81.

²Ibid., p. 86.

³Ibid., p. 88.

The quality of our work may have improved, but there can be no doubt that the development of our discipline lags behind our requirements. A basic limiting factor is the amount of manpower that we allocate to the business of research and inquiry. Precisely what that quantity is one cannot say, but it is plain enough that we devote the most meager resources to the tasks of political inquiry.¹

Then in the following passage Professor Key suggested another reason for inadequate scientific progress in the discipline:

As most of the branches of our discipline come to be characterized by a focus on political behavior, instead of the unique qualities of constitutions, charters, or practices, it becomes embarrassingly apparent that we need to exert ourselves to move from the description of the particular toward the formulation of modest general propositions. A modest general proposition need not deal with a modest or insignificant problem. Our work still bears the **marks** of its origins in history and law, disciplines dedicated in peculiar degree to the analysis of the particular. Our journals are still in large measure filled with treatments of particular events, institutions, practices. Often these are well done, even ingeniously done, yet they add absolutely nothing new by way of general idea. They stand alone as isolated accounts of peculiar events or situations. They remain unconnected with what has been learned before; from their nature they will remain unconnected with what is learned later.²

¹"The State of the Discipline," The American Political Science Review, 52, 1958, 961-971, at p. 969.

²Ibid., p. 965.

A preoccupation with the resolution of practical problems is one thing; scientific inquiry to discover knowledge of political affairs is something else.¹ Both of the writers cited above believe that our resources have been inordinately allocated to the former at the expense of the latter. And the passages quoted from their writings suggest some reasons for this circumstance. It is my impression that the relation between a concern for particular political phenomena, whether or not this concern is of an immediately practical nature, and the kind of general knowledge pursued through scientific inquiry is not generally understood within the discipline. The reasoning behind this judgment is brought out well by an analogy with the field of medicine. In this field, everyone who is deeply concerned with curing the ills of his fellows does

¹This is an appropriate place to interject a qualification for the purpose of protecting myself against the accusation of a certain kind of simple-mindedness that I have not wholly avoided in the text. The practical activities I have been speaking of in this section are of the rather "pure" variety; what Professor Hyneman listed as one of "four kinds of enterprise" which "American political scientists are doing and have been doing in recent years," namely: "They give advice on current issues of public policy and participate in the formation and execution of public policy." The Study of Politics, p. 4. Of course, these activities are sometimes mixed with a kind of inquiry, such that the residues of the whole activity contribute toward the purely scientific part of our discipline. And I have not considered in the above discussion what is perhaps the largest of our practically oriented writing: the research and writing directed toward immediate practical problems which might be included in the above category of work if any political practitioner had asked for it. As I see it, much of this work amounts to a giving of advice by political scientists to one another about what others should do, which is not to say that some of it does not ultimately serve practical purposes. (Hyneman made a similar observation in The Study of Politics, p. 107.) It is the large amount of this work, I suspect, of which David Easton and V. O. Key are most critical.

not go out and minister to sick people; some go into the laboratories to do basic research. One reason for this is, no doubt, because the relation between the practical goals in the field of medicine and theoretical research are clearly understood. What I am suggesting, and I wish to emphasize that it is only a suggestion which I shall not attempt to substantiate, is that if these relations were more fully understood in political science, there would be more effort devoted to the scientific development of the subject.

One more brief comment and we will have finished with the relation of pure and practical political science.¹ This

¹Of course, we have not exhausted the many aspects of this subject which might be discussed. For a number of other related facets of the subject, see, for example: Hyneman, The Study of Politics, pp. 9-17 and 165-173. However, I believe we have mentioned some of the major methodological issues which arise in this connection. And others, such as value problems and the nature of means-end statements, are taken up at other places in the thesis. But there is one point of view within the discipline which our failure to consider might be regarded as a glaring omission. I refer to the claim that political science is or ought to be a "policy science." As I see it, this viewpoint consists primarily of a suggestion about research priorities in the discipline, entirely based upon considerations of outstanding practical problems or problems likely to arise. In this sense the advocacy of "policy science" is methodologically innocent. Of course, the same kind of methodological issues which arise in the context of any discussion of practical concerns within the discipline might arise in a discussion of this thesis. Personally, I have never found anything novel or interesting about the "policy science" idea. It seems to me that political scientists as a group have generally taken a profound interest in contemporary problems related to their discipline, and that they have also devoted much attention to the policy making process, a focus of research which "policy science" advocates have emphasized. Perhaps, their suggestion that all aspects of relevant sciences be brought together to attack the problems with which they are concerned constitutes a new emphasis. For the "policy science" idea, see D. Lerner and H. D. Lasswell, The Policy Sciences (Stanford: Stanford University Press, 1951), especially Lasswell's introductory statement, pp. 3-15.

relates to what I believe to be an interesting positive contribution from our practical to our theoretical concerns. If it is true, as I asserted -- and this is not the least bit original -- that practical activities always involve, in some way or other, the application of general beliefs about politics, then the success or failure of such applications might be expected to constitute some test of the truth of those beliefs. To put the matter a little differently, and perhaps more impressively, whenever political scientists give advice to political practitioners and they act accordingly, something approximating an experiment has taken place. Of course, such "natural experiments" do not approximate very closely the controlled experimentation which is so important in most of the physical and biological sciences, but it seems to me that their potential value in a field such as political science, where experimentation is generally thought to be practically impossible, is great.¹ I am convinced that they have already provided a valuable service in refining our knowledge in several areas of the discipline, especially in the study of public administration. And I suspect that they would be of even greater help if the logic of scientific prediction were more widely understood; one must be aware that he is applying generalizations in practical cases in order to use these applications as a test

¹For a more general discussion of this idea, see Gibson, op. cit., pp. 198-200. On questions surrounding the possibility of experimentation in political science see below, Section C of this chapter.

of the generalizations.¹

C. Political Science is Difficult.

This topic heading is a rather imprecise way of asserting a commonplace, though an important commonplace, which has been established in a somewhat precise fashion in the context of arguments in support of anti-scientific conceptions of political science. The arguments are based upon certain features of both political inquiry and the subject matter of such inquiry. Pointing out these features is of some significance: it shows some of the reasons why the pursuit of reliable general knowledge about politics is difficult; it helps to explain why there is a relative paucity of such knowledge. However, this kind of analysis does not support the conclusions for which it has been invoked in anti-science writing. That is, the arguments just referred to are invalid. I hope to show this for what I believe to be the most often employed of such arguments, but -- unlike our treatment of anti-science arguments above -- I shall not discuss the views of any individual writer. Instead, each of the arguments examined

¹For a recent discussion of the dual purpose of bureaus of government research in advancing both practical and theoretical interests, which is consistent with the suggestions above, see: V. Ostrom, "Public Policy Studies; an Approach to Governmental Research," in The Research Function of University Bureaus and Institutes for Government-Related Research, ed. D. Waldo (Berkeley: University of California, 1960), pp. 159-178; and the comments by E. S. Redford, pp. 179-182.

will be stated in a form which is more simple and direct than the form in which they are likely to be found in the literature. But I do not intend to flog deceased horses -- references to specific writers who employ arguments which at least approximate those analyzed will be found in the notes. As a matter of fact, I, for one, would be indeed happy if the animals discussed below were thoroughly dead.

1. Probably the most frequently mentioned limitation upon scientific political inquiry is the alleged impossibility of experimentation.¹ This claim is somewhat ambiguous, for exactly what the characteristics required for a particular empirical investigation to qualify as an "experiment" within the meaning intended by those who make the claim is not entirely clear. However, if we take it to mean what Ernest Nagel called a "controlled experiment" -- i.e., a procedure in which "the experimenter can manipulate at will ... certain features in a situation

¹J. G. Kemeny, "A Philosopher Looks at Political Science," The Journal of Conflict Resolution, 4, 1960, 292-302, at p. 299. Three of the four writers whose anti-science views were discussed above assert this limitation. Mason wrote that "there can never be a political experiment in the scientific sense." Op. cit., p. 2. Harbold and Hichner stated: "It is virtually impossible ... to establish anything resembling satisfactory experimental conditions." Western Political Quarterly, 11, 1958, pp. 757-758. I would maintain that this was part of the argument in support of the anti-science view of these writers, though their writing does not provide an adequate basis for imputing to them the simplified argument from no experiment to no science.

[which he produces] ... so that by repeatedly varying some of them ... but keeping the others constant, the observer can study the effects of such changes upon the phenomenon" under investigation,¹ then there would probably be general agreement that experimentation in political inquiry is practically impossible. We will assume that this is so; the qualifications, especially regarding less strict definition of 'experimentation,' will be introduced as we proceed. The point I wish to make is that any argument from the impossibility of experimentation in an area of study to the non-scientific status of inquiry in that area is an invalid argument. Perhaps this is obvious; I think it is. And one way of substantiating this view is also rather obvious: it only needs to be pointed out that geology and astronomy are both sciences, that at least the latter science is a highly developed one, and that experimentation is not employed in either of them.

However, another way of showing the invalidity of the simple argument we are considering is not so obvious, and it is -- in my estimation -- of considerable import for our present purposes. The fallacy of the argument from no experiment to no science is that it confuses scientific method or the logic of science with one of the techniques

¹E. Nagel, The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace and World, Inc., 1961), pp. 450-451. This book contains an excellent discussion of experimentation in the social sciences: see especially pp. 450-459; pp. 459-502 are also relevant.

or procedures of science. No matter how important experimentation is to those sciences in which it is used, it is only one of the techniques of science. It manifests the logic of science; it is not part of the logic. To see this, one need only recognize that logically it makes no difference whether a scientist produces a situation in a laboratory, such that he can manipulate some feature (condition, factor, variable) and observe its effects, or he finds the situation in nature and the change whose effects he wishes to observe takes place without any effort on his part.¹ Practically, of course, one must ordinarily resort to the relatively "artificial" use of the laboratory. But, also practically, in political science we must ordinarily find our "experimental" circumstances in "nature." As M. Duverger did, for example, when he used the case of electoral reform in Belgium as a kind of "experimental" support of hypotheses relating electoral systems and party systems. Thus, as he describes the evidence, in every European country except Belgium which had a two-party system and "the simple-majority single-ballot system" of election, the rise to prominence of a third (socialist) party was accompanied by the demise of one of the old major parties and the reestablishment of the two-party system, and Belgium was the only one of these countries which changed its electoral system by introducing

¹Ibid., p. 453.

proportional representation.¹ This particular example, though it does not in any way approximate the procedures involved in the strict notion of experimentation, does exemplify, even if in a somewhat crude fashion, the same logic which is exemplified by one form of the controlled experiment, the form which J. S. Mill termed the Method of Difference.² This is, no doubt, why such investigations

¹M. Duverger, Political Parties: Their Organization and Activity in the Modern State (London: Methuen, 1954), p. 213. I assume the reader is familiar with the hypotheses concerned here. As Duverger stated them: (1) "the simple-majority single-ballot system favours the two-party system" (p. 217); (2) "the simple-majority system with second ballot and proportional representation favour multi-partism" (p. 239). The experience of Belgium in contrast with the other countries is evidence for both hypotheses. The explicit statement of a large number of hypotheses together with the use of such patterns of scientific reasoning as the one illustrated above are noteworthy features of Duverger's book. However, the exemplification of such features of scientific inquiry (or any others) does not guarantee scientific achievements. A recent study raises serious doubts concerning the significance of the two hypotheses stated above: J. G. Grumm, "Theories of Electoral Systems," Midwest Journal of Political Science, 2, 1958, 357-376; and for a general critique of Duverger's book see A. B. Wildavsky, "A Methodological Critique of Duverger's Political Parties," Journal of Politics, 21, 1959, 303-318. Cf., F. C. Engelmann, "A Critique of Recent Writings on Political Parties," Journal of Politics, 19, 1957, 423-440.

²Nagel, loc. cit., p. 454; see also, E. Nagel, ed., John Stuart Mill's Philosophy of Scientific Method (New York: Hafner Pub. Co., 1950), pp. 214-216.

John Stuart Mill's Philosophy of Scientific Method (New York: Hafner Pub. Co., 1950), pp. 214-216.

are sometimes referred to as "natural experiments."¹

One further consideration is in order, though I shall only mention it. Even though its applicability is still severely limited, there is already a great deal of research in social science that might reasonably be called experimental and much of it deals with phenomena that might reasonably be termed political. I am not referring to so-called "natural experiments" which, as we noted, are not at all experimental in the procedural sense of experimentation (which is, as I see it, the only precise meaning of this aspect of science). I am referring to small group research and "field experiments." Small group studies approximate to some extent both aspects of controlled experiments -- (1) the "artificial" production of a situation in a laboratory-like setting, and (2) the manipulation of certain factors in order to observe their

¹See, e.g., J. R. P. French, "Experiments in Field Settings," Research Methods in the Behavioral Sciences, eds., L. Festinger and D. Katz (New York: The Dryden Press, 1953), pp. 98-135; French speaks of the "natural experiment" as research "in which the researcher opportunistically capitalizes upon some on-going changes and studies their effects in an experimental design," or, he adds, "if these natural changes have already occurred by the time the social scientist arrives on the scene, it may still be possible to gather sufficient data after the fact to fill out the design of a crude ex post facto experiment." (p. 99) It is this "crude ex post facto experiment" which has been of greatest importance in political science, especially in areas like "comparative government" and "international relations."

effects.¹ Such research, which has been carried on for more than two decades, has been applied extensively to politically relevant subjects: leadership, communication, organization, and the like.²

¹Small group experiments are, of course, far different in important respects from controlled experiments in the physical sciences: "real" situations are not approximated nearly to the extent that they are in the Chemistry laboratory, for example; the control of relevant variables is considerably less and, consequently, replication of experiments are usually quite inexact; the extent to which the factors investigated can be manipulated is very limited; and there are a number of other relative shortcomings related to precision of measurement, effects of the experiment upon the subject, etc. See: Nagel, The Structure of Science, pp. 450-459; Gibson, op. cit., p. 199; L. Festinger, "Laboratory Experiments," in L. Festinger and D. Katz, eds., op. cit., pp. 136-172.

²Three collections of articles reporting small group research contain many studies in the category of the "politically relevant": H. Guetzkow, ed., Groups, Leadership and Men: Research in Human Relations (Pittsburgh: Carnegie Press, 1951); D. Cartwright and A. Zander, eds., Group Dynamics: Research and Theory (Evanston: Row, Peterson and Co., 1953); A. P. Hare, E. F. Borgatta, and R. F. Bales, Small Groups: Studies in Social Interaction (New York: Knopf, 1955). Having cited this material which represents, given all the qualifications I have mentioned both in the text and the last note, experimentation dealing in many cases with, broadly speaking, political phenomena, it seems appropriate that I suggest a further qualification in the form of an opinion. On the basis of my reading of the small group literature, which has included a sampling of at least most types of such studies and a rather thorough coverage of the studies on leadership up to 1955, I could not name even one instance of such an experiment which has provided significant evidence for a political hypothesis: i.e., one which has served for political science the primary function which experimentation serves in the natural sciences. But I don't intend this opinion as disparaging of such research. The people who do it seem well aware of its present limitations, and aim for gradual development of its significance. Moreover, there is some evidence that small group studies have already contributed to political science in other ways. See, e.g., H. A. Simon, "Recent Advances in Organization Theory," in Research Frontiers in Politics and Government (Washington: The Brookings Institution, 1955), pp. 23-44. For an incisive account of recent improvements in the logical design of such research, see H. Guetzkow, "Building Models About Small Groups," in Approaches to the Study of Politics, ed. Roland Young (Evanston: Northwestern University Press, 1958), pp. 265-281.

Field experiments, on the other hand, approximate only the second of the two features of controlled experiments just mentioned. John R. P. French described "a field experiment as a theoretically oriented research project in which the experimenter manipulates an independent variable in some real social setting in order to test some hypothesis."¹ The classic example of such research in political science is, of course, H. F. Gosnell's Getting Out the Vote.² If we speak in terms of research relevant to the subject matter of our discipline as we did with respect to small group work, a great many studies of this kind might be cited;³ however, it seems to me that something like field experimentation has probably been of most use to political science in that area where our practical activities and theoretical interests come together -- i.e., what we discussed earlier as the occasions on which political scientists act as practitioners, as advice givers, etc., and at the same time use these experiences for theoretical purposes.⁴

The case against the view that the scientific method

¹Op. cit., p. 101.

²(Chicago: University of Chicago Press, 1927).

³See the three collections of articles cited above.

⁴Earlier I referred to such cases as "natural experiments." In light of our subsequent discussion it is clear that they approximate to some extent field experiments; perhaps, it would be most correct to say that they fall somewhere between the two.

requires experimentation is clear enough. One point of general import in our brief discussion of the false premise of an argument which might be used against the idea of scientific political science is the distinction between procedures in science and the methodology or logic of science. We came across it earlier when I indicated that we would use the word 'methodology' to mean the logic of science, in part, in order to avoid confusing this distinction; especially in view of the fact that social scientists ordinarily include both the procedures and logic of science as methodology, and sometimes they use the term even more inclusively.¹ And we will have occasion to call attention to the distinction again a number of times throughout the thesis, as in the discussion immediately following.

2. One writer made the observation that some political scientists have a "violent aversion to what is called 'quantification'";² another suggested that "most political scientists, accustomed as we are to other modes of analysis, bristle at the sight of even the most common statistical

¹See, for example, A. Leiserson, "Problems of Methodology in Political Research," Political Science Quarterly, 68, 1953, 558-584; reprinted in H. Eulau, S. E. Eldersveld, and M. Janowitz, Political Behavior: A Reader in Theory and Research (Glencoe: Free Press, 1956), pp. 53-64; for Leiserson's broad definition of 'methodology', see p. 53.

²Prothro, op. cit., p. 566.

symbol."¹ That there are among political scientists some who deprecate quantification (including the use of mathematics) is undeniable, but exactly what it is which is opposed or what the grounds are for the opposition -- these things are not at all clear.

There are strategic objections such as the argument that any attempt to apply quantitative techniques to most areas of political science will probably result in failure;² or that an emphasis upon "quantitative method" is apt to result in a disregard for the areas (or problems) of political science which are not amenable to such investigation, and the latter are always described as the most "significant" areas (or problems). But controversies of this kind are, at least in principle, resolvable through research: in this case, through the successes and failures of those who employ the disputed technique. And, furthermore, such very general questions of procedure surely do not lend themselves to categorical answers; nor are they the kind of questions about which people are likely to take radically different positions. However, with

¹V. O. Key, A Primer of Statistics for Political Scientists (New York: Crowell, 1954), p. 1.

²H. G. Morgenthau, for example, has argued that quantitative techniques are only applicable to a "narrow sphere" of our subject matter, and that "much of quantitative political science has become a pretentious collection of trivialities." "Power as a Political Concept" in Approaches to the Study of Politics, ed. R. Young, p. 70. The force of Morgenthau's deprecation of quantitative inquiry shows, however, that he is not merely disputing the efficacy of certain research techniques. His objections go much deeper and might reasonably be considered methodological.

respect to the social sciences today, as in the physical sciences of an earlier century, there are radical differences over the relevance of quantification.

In this regard, the following comments by an outstanding contemporary philosopher of science are quite appropriate:

In psychology and the other behavior sciences, where quantification has been on the rise for some time, it is still the object of overenthusiastic advocacy as well as (outside of economics) sullen resistance ... In physics, quantification coincided with its spectacular progress; hence the proclivity to take for granted that in all areas the former alone is a necessary as well as a sufficient condition of the latter. It would seem that this is one of the causes of that strained enthusiasm ... Another cause of all the stridency in favor of quantification lies, I think, in the social climate. Of late the behavior sciences have become the basis, or the alleged basis, of professions whose numerous members are rapidly acquiring managerial power and who, for better or worse, aspire to even more. Such aspiring groups are in need of prestige symbols. The white coat of the medical man is one; the mathematical formula is another. The resistance to quantification, on the other hand, is essentially a rationalization of the old bias against a science of man. Its arguments, such as they are, repeat the patterns of some earlier philosophies, which are in part built around a spurious dichotomy, quality versus quantity....¹

¹Bergmann, Philosophy of Science, p. 67.

Professor Bergmann's speculations concerning "the stridency in favor of quantification" are I believe interesting and insightful. They suggest a category of methodological mistakes which I have generally neglected -- the erroneous arguments in support of positions with which I largely agree.¹ However, it is Bergmann's last two sentences which relate most directly to our interest at this point. They support the following observation: at least some of the arguments concerning quantification in political science are related to fundamental methodological issues, and some of these are part of the anti-science tradition in political science.

The intellectual foundations of opposition to the use of quantitative procedures in political science are related to the anti-science tradition in the discipline.² I am not at all sure what the arguments are; I don't think they have ever been clearly stated. I wish to suggest two mistaken arguments which -- if I am not mistaken -- play a part in such views. Both of them assume that quantification is an essential characteristic of science, that it is an integral part of scientific method. With this as an initial premise, the arguments proceed as follows:

¹This is one of the ways in which the scope of the present study was deliberately limited.

²As a result of this same conclusion, J. W. Prothro stated: "The dichotomy, then, is not into quantitative versus qualitative ... but into scientific versus anti-scientific schools." Op. cit., pp. 567-568.

(1) it is impossible to quantify the data of political science, so political inquiry cannot be scientific; (2) the "essential" features of political phenomena are qualitative, not quantitative, therefore scientific political science must be trivial at best. I don't think anyone has explicitly stated either of these arguments;¹ perhaps they are not even implicit in views opposed to quantification in political science, as I think they are. Nevertheless, I believe their refutation will shed some light upon the import of some methodological (or more broadly "philosophical") arguments which have been or might be raised with respect to quantification in political science.

The refutation proceeds in two ways. The first line of attack is exactly the same as the argument advanced against the claim that limitations upon the possibilities of experimentation constitute grounds for argument against scientific method in political inquiry. Thus, quantification is no more one of the features of scientific method than experimentation. That is, we must invoke once again the distinction between scientific method and the techniques of science, and point out that quantification is also included among the latter; at the same time recognizing

¹I mean that I have not seen either in writing. Actually, I have heard both of them stated by political scientists. However, I only mention this "in passing," as it were. Unsubstantiated testimony by the plaintiff is of no more significance here than it is in a court of law. I trust, however, that in deciding this case the reader will take "judicial notice" of any relevant evidence from his own experience.

the great significance of quantification -- it encompasses both measurement and the concomitant opportunities for the use of mathematics -- for those sciences which have been able to employ it.¹ And, of course, in doing so we also recognize the enormous relative disadvantages which ensue for areas of inquiry in which the opportunities for quantification are limited. This brings us to the second part of our first argument. Just as we found that what is meant by experimentation is somewhat ambiguous, that there are strict and looser senses of experimentation, we must recognize that the same thing is true with respect to quantification. And just as there is a reasonable sense in which we may say that there has been experimental political inquiry, it is also true that there has been quantitative political inquiry.²

¹For an excellent concise statement of the advantages of quantification in science see: C. G. Hempel, Fundamentals of Concept Formation in Empirical Science, International Encyclopedia of Unified Science (Chicago: University of Chicago Press, 1952), II, No. 7, pp. 56-57.

²The research articles contained in two recent collections illustrate a number of different quantitative procedures employed in political inquiry: Eulau, Eldersveld, and Janowitz, op. cit.; and J. C. Wahlke and H. Eulau, Legislative Behavior: A Reader in Theory and Research (Glencoe: Free Press, 1959). For additional types of politically relevant quantitative analysis, see: K. J. Arrow, "Mathematical Models in the Social Sciences," in The Policy Sciences, eds., Lerner and Lasswell, pp. 129-154; P. F. Lazarsfeld and A. H. Barton, "Qualitative Measurement in the Social Sciences: Classification, Typologies, and Indices," ibid., pp. 155-192; L. F. Richardson, "Mathematics of War and Foreign Politics," and "Statistics of Deadly Quarrels," both in The World of Mathematics, ed. J. R. Newman (New York: Wiley, 1957); R. C. Snyder, "Game Theory and the Analysis of Political Behavior," Research Frontiers in Politics and Government (Washington: Brookings Institution, 1955), pp. 70-103; M. Shubik, ed., Readings in Game Theory and Political Behavior (Garden City: Doubleday, 1954).

Our second line of argument is more fundamental in that it provides the logical grounds for part of the first -- i.e., it supports the statement above that quantification is not part of scientific method. The argument surrounds the distinction between qualitative and quantitative. A tendency to exaggerate the difference expressed by this distinction is, I think, part of the grounds for a kind of specious support for anti-science conceptions of political science. Bergmann commented on the matter with regard to psychology:

Experience is qualitative; science is quantitative; the gulf is unbridgeable. Or, in a slightly less radical version, since experience is qualitative, the methods of the quantitative disciplines must of necessity fail in psychology.¹

If we substitute for 'experience' the expression 'the data of politics' (or 'the essential features of politics') and for 'psychology', 'political science', then we would have a restatement of the two anti-science arguments I constructed above. Then, also, Bergmann's refutation of the arguments regarding psychology would work as well against those related to political science. It consists of a clarification of the significance of the qualitative-quantitative distinction.² We cannot use Bergmann's succinct argument as he stated it because it is predicated

¹Philosophy of Science, p. 73.

²Ibid., pp. 73-74.

upon much that came before his discussion which has not preceded ours. However, accomplishing the same thing by a slightly longer route will permit us to expand my earlier reference to a "looser sense" of quantification.

One way of bringing out the significance of the distinction between quality and quantity in which we are interested is through a consideration of concepts (synonym: descriptive words). C. G. Hempel divides scientific concepts into three kinds: classificatory, comparative, and quantitative. Classificatory concepts refer to characteristics which an object either exemplifies or does not exemplify. The qualitative nature of such characteristics has never been doubted. Both comparative and quantitative concepts, in Hempel's terms, have been considered as referring to quantitative characteristics -- i.e., to characteristics which may be more or less exemplified. The distinction between them is that comparative concepts indicate only an order or ranking among objects -- i.e., they indicate whether different objects exemplify more or less of a certain quality as compared with one another. Quantitative concepts, on the other hand, indicate exactly how much of a certain quality an object exemplifies; they, therefore, always involve the use of numbers. Comparative concepts may also involve numbers, but in this case the numbers are used only in their ordinal, not in their cardinal sense.¹

¹Hempel, Fundamentals of Concept Formation ... , pp. 54-62.

As a discussion of types of scientific concepts this is terribly condensed and somewhat oversimplified. However, it suffices I think to bring out the points in which we are interested. The main point I tried to show by repeated use and underlining of the term 'quality' when speaking of the two kinds of concepts which are usually considered quantitative. At the risk of being tedious I will spell it out. Comparative concepts indicate whether certain objects have (exemplify) more or less of a quality with respect to each other, and fully quantitative concepts indicate how much of a quality an object has (exemplifies). It would make no sense to say of an object that it has more than another object, or to say of an object that it has eight units. Always one must speak of more or less of something or a certain number of units of something and this something is always a quality. It is obvious, therefore, "that every descriptive term [including quantitative terms] is fundamentally 'qualitative' and that the use of numerals in synthetic [empirical] statements is merely a logical elaboration of 'quality.'"¹

¹Bergmann, Philosophy of Science, p. 73. Bergmann expressed a more basic argument for this conclusion as follows: "Every undefined descriptive concept is 'qualitative.' The definition of every defined descriptive concept [including, of course, all quantitative concepts] contains therefore at least one 'qualitative' term." (p. 73) On the basis of our discussion of meaning in Chapter Three, the meaning and validity of this argument should be evident.

In the light of these considerations I believe we are justified in concluding with respect to methodological (or philosophical) disputes concerning quantification that, once again in the words of Professor Bergmann, "the shouting is about nothing."¹ More specifically, there is nothing in this subject that provides appropriate grounds for argument for or against scientific method in political science. As things stand, if we consider quantification in its broadest sense, which includes the use of comparative concepts, then the literature of political science obviously abounds in quantitative propositions: statements to the effect that one nation has "more power" than another, that one constitution is "less democratic" than another, that one interest group has "greater influence" or is "more partisan" than some others, and the like. There has been, on the other hand, very little use of comparative concepts involving numbers, and the use of fully quantitative concepts has been very limited indeed. Furthermore, it is probably true that most aspects of political phenomena which are studied in the various areas of political science are incapable of fully quantitative treatment. But for any particular area this cannot be determined a priori. In every instance the question of the applicability of quantitative techniques -- the possibility of quantitative measurement, if you will -- and their theoretical

¹Ibid., p. 74.

fruitfulness are matters that can only be resolved through empirical procedures. At best, logical analysis can clarify the nature of such questions, it cannot answer them; so this is an excellent place to end our discussion of quantification.¹

3. Having mentioned the importance of maintaining the distinction between the methodology and techniques of science in connection with our discussion of experimentation and quantification, it is appropriate to mention another anti-science view which may, I believe, be clarified by calling attention to this same distinction, even though it does not belong in our present category of "difficulties" in political inquiry. I refer to the rather vague claim that even though scientific method may be applicable to political inquiry, it is when applied to this field something radically different from scientific method in the natural sciences. This is part of the more general claim that it is a mistake to speak of the scientific method. Now I would not argue that there are no logical differences among the sciences. The problems connected with the status of the particle terms in physics are probably peculiar to that science; and there are logical problems related to the

¹Social scientists sometimes also include under the heading of 'quantification' or 'quantitative analysis' something quite different from what we have discussed above: the elaboration of systems of deductively related propositions -- i.e., axiomatic systems. This topic was omitted from the above discussion because I don't think it is related to the issue of the appropriateness of scientific method for political science.

"private" character of mentalistic things, the fact that the social scientist is in a sense part of the subject matter of his own field of inquiry, methodological questions related to values, and many others which are considerably more important in, if not peculiar to, the social sciences. This is not the point I am concerned with at the moment. It is that scientific method is fundamentally, in a reasonable sense of this frequently ambiguous word, the same in all sciences; that there are no radical differences of the type that are implied, or explicitly stated, in connection with the idea that one must use the expression 'scientific method' in a plural form when speaking in the same breath, as it were, of the natural sciences and political science. This is a mistake which results from a confusion of the techniques of scientific research and scientific method. The techniques of various sciences are obviously different. If the distinction between these and the methodology of science is not maintained, the inference that there is a scientific method peculiar to the social sciences or to political science is an easy one; more likely than not the distinct "method" will belong to the anti-science tradition.¹

¹See, for example, C. J. Friedrich, Constitutional Government and Democracy (Boston: Little, Brown & Co., 1941), Chap. 25. Friedrich's conclusions concerning the nature of political inquiry make it clear that he was not speaking of techniques when he wrote: "Social sciences cannot benefit from applying methods of natural sciences to them. Each field of knowledge, each science has, then, its own methods." (p. 570) Yet in support of this he offers the irrelevant observation concerning techniques that you cannot "describe the constitution of Athens with a microscope." (p. 571).

4. Another frequently mentioned difficulty for political inquiry is the complexity of its subject matter, and it, too, has been alluded to in support of anti-science conceptions of political science.¹ Exactly what is meant by "complexity" in this context is not entirely clear, but it is well enough understood so that most people would agree that it is a source of difficulty for political science; and a number of features of political phenomena have been singled out as evidence of their complexity. These include all of those mentioned above and below within our discussion of the present topic. Here I will mention only the most general of these features: the vast number of characteristics (variables) which seem to be relevant to an adequate explanation of any political event. I once heard this circumstance poetically described by a political scientist who said, "We frequently find ourselves adrift in a sea of variables." Less poetically, we may say that it is the basis for the fact that particular instances of explanation in political science are so frequently criticized on the grounds of the many possibly relevant factors which have not been taken into account.

¹Harbold and Hichner, loc. cit., refer to this as one of several obstacles to a "pure science of politics" which together constitute the grounds for their anti-science views discussed earlier.

With regard to the matter of complexity, I only wish to raise three considerations and one question. The considerations are: (1) political phenomena are no doubt complex but "it is by no means certain that they are in general more complex than physical and biological phenomena"; (2) "problems that appear to be hopelessly complex before effective ways for dealing with them are invented often lose this appearance after the inventions have been made";¹ (3) in any event, the extent to which complexity constitutes an obstacle to the development of laws, theories, and thus explanations concerning political phenomena is a matter for empirical determination. The question is this: On what legitimate grounds could anyone raise the difficulties arising from the complexity of subject matter as especially pertinent to the scientific study of politics?

5. Difficulties arise for social science from the well known fact that the investigation of human behavior may itself alter that behavior.² And there is evidence that this circumstance has been appealed to in support of the

¹Nagel, The Structure of Science, p. 505. For a defense of social science against the charge that its subject is too complex to be studied scientifically, see pp. 503-509 of Nagel's book, and Gibson, op. cit., pp. 7-10.

²My discussion of this subject follows closely the excellent analysis by Nagel, loc. cit., pp. 466-473. However, whereas Nagel speaks of social science in general, I will speak of political science.

anti-science position in political science.¹ The effect of political inquiry upon its subject matter is manifested in two ways: (1) the influence of behavior by the process of research, and (2) the modification of behavior resulting when the results of research become known: what is often referred to as the "self-fulfilling" or "self-denying prophecy." We will briefly discuss each of these in order to show that they do not present an insuperable barrier against the achievement of scientific political knowledge, that is lawful knowledge of political phenomena.

a. The first "difficulty arises because changes are produced in a subject matter by the means used to investigate that subject matter."² Stating this in these general terms enables us to grasp more readily one approach to assaying its significance for political science: that is, it points up the fact that this difficulty is present in the natural sciences as well. For example, if a thermometer is put into a liquid to measure its temperature the actual temperature of the liquid will be to some extent altered. However, on the basis of available general knowledge it is possible to estimate in advance the approximate extent of such effects and thus determine whether or not they may be discounted for the purpose at hand; if not, a manner of

¹Easton, The Political System, pp. 24-31; and Kemeny, op. cit., pp. 299-300.

²Nagel, loc. cit., p. 467.

lessening their effect is also known -- for example, by adjusting the temperature of the thermometer itself to a point where its effect will be insignificant; and if the required accuracy so warrants, other knowledge permits the effect of the thermometer to be calculated and allowed for in determining the temperature of the liquid. Oversimplified as this example is, it nevertheless illustrates the possibilities which are in principle also available for dealing with this difficulty in political inquiry. Of course, here the relevant knowledge is frequently unavailable, but there are no grounds for concluding that it is unattainable, and, furthermore, it is not always unavailable. Thus a great deal is known about the influence of interviewers, the manner of phrasing items on attitude scales, and the like. And, although such knowledge is quite imperfect, it does permit observer effects to be controlled and, to some extent, calculated in estimating the validity of data. For example, the Gallup organization uses secret ballots in collecting voter preference data because of the general knowledge we have concerning influences upon respondents in such studies.

Our first consideration, then, is that the problems arising when there are tendencies for research to influence the behavior under investigation are capable of resolution. A second consideration, which is specifically addressed to political science, is that as a matter of fact by far the

major portion of the data used in political science has not been subject to appreciable influences of this kind: aggregate data such as voting statistics, data regarding the activities of government which are a matter of public record -- legislative enactments, judicial decisions, etc., data regarding large scale features of international crises and wars; one could go on listing illustrations from almost every major area of political research.¹ As the amount of research entailing the direct observation of individual behavior increases, the potential significance of this source of data distortion will become greater; at the same time, however, the available knowledge for dealing with this difficulty is also increasing.

b. The second type of difficulty with which we are concerned, the fact that the results of research in the form of generalizations about political behavior may alter that behavior, has been more frequently mentioned in discussions of the methodology of political science than the one just considered.² Our limited purpose with respect to this feature of political inquiry is also, as stated above, to show that it does not provide any grounds for

¹Since I have indicated that I am following Nagel in the present discussion, it is appropriate to say that he does not mention this point.

²The earlier references to Easton and Kemeny deal almost exclusively with this problem. See also, Harbold and Hichner, loc. cit., p. 758; and Q. Wright, The Study of International Relations (New York: Appleton-Century-Crofts, 1955), pp. 116-120.

methodological arguments against the possibility of scientific political science; in this case, arguments against the possibility of valid generalization about political behavior. Three considerations are I believe adequate for this purpose.

First, we should notice that one easy inference from the phenomena under discussion which is congenial to the view I wish to discredit is based upon a misunderstanding. That is, it is not correct to infer that a generalization about political behavior is always falsified when knowledge of that generalization results in a change of the behavior it describes. Scientific generalizations are conditional propositions; they assert what will happen only under certain specified conditions. Suppose there is a general proposition: 'If A, B, and C, then X'; people learn of it and, since X is not to their liking, they act in such a way, that conditions A, B, and C never again arise. Assuming the generalization was true before people learned of it (i.e., that it was a law), its status would not be changed afterwards, except, of course, that it would no longer be applicable.¹ And, it is interesting to note that what we have said holds just as well if the X in

¹The same kind of analysis is relevant to an understanding of the methodological significance of the relatively rapid change which occurs in political phenomena. When certain forms of political organization, for example, cease to exist, generalizations about them are no longer applicable to the world; they are not thereby falsified.

our proposition represents a certain type of political revolution and the proposition is a behavioral law, as it does if the X represents an increase of radioactivity in the earth's atmosphere and the proposition is a physical law.

This is, however, only one kind of case where a change in behavior resulting from knowledge of a scientific generalization might be misinterpreted as a refutation of that generalization. Another kind is not so obviously mistaken. This is the case when, using our same schematic law statement, there are instances where conditions A, B, and C obtain, but the behavior represented by X does not occur. And, furthermore, in these instances the people involved refrained from X because they acted in the light of their knowledge of this generalization. Under these circumstances, it seems to me that such instances should not be interpreted as evidence against the generalization. That is, in this kind of case, it seems more reasonable to assume that the absence of any mention in the law of the condition "having knowledge of this generalization" should be taken as a sufficient indication that it does not apply to instances where this condition is present. If this argument is sound, then this second type of case where knowledge of a law changes the behavior described by that law has the same significance as the first type -- i.e., the knowledge of the law creates new situations to which

the law does not apply so that the scope of the law is limited, but its truth value is not impugned.

To those who find my argument against the second type of case less than compelling, I would say the following. If a generalization about political behavior were true in the absence of knowledge of that generalization, then another generalization exactly like this one, except for an addition to its antecedent clause which restricted it to persons who were not aware of its existence would be true without qualification; that is, it would be a scientific law. Remember, I entered this discussion primarily to show that the fact that people may modify their behavior upon learning of generalizations about it does not constitute an insuperable barrier against the development of scientific laws about political behavior; I think I have done that. This much I would say to those dissatisfied with my argument against the second type of case. However, there are two additional considerations related to the significance of the phenomena under discussion.

In the rather involved discussion above of our first consideration, I raised the possibility of restricting the scope of a generalization in order to protect it against the consequences that might follow when people learn of it. By underlining the word 'might' in this sentence, I just indicated our second consideration: as a matter of fact people do not always modify their behavior upon learning

of generalizations about it. For example, it is widely known that in democratic countries people earning relatively low incomes tend to vote for more leftist political parties, but this tendency nevertheless continues. Especially in the case of large scale social (including political) processes, there is evidence that knowledge of them does not appreciably influence the behavior underlying them. In this regard, Easton mentions the relationship between "urban concentration and industrialization" and many of the practically invariant effects of these social changes.¹

Finally, it should be mentioned that the phenomena under discussion is not anything which is especially peculiar. Modifications of behavior resulting from a knowledge of generalizations about that behavior is just another illustration of the obvious fact that one of the determinants of a person's behavior is his beliefs (or, what amounts to the same thing, his "view of the world"). There is, then, no reason why lawful knowledge could not be discovered concerning the behavioral effects of the dissemination of certain generalizations about political behavior.²

¹Op. cit., p. 29.

²I would go so far as to say that this conclusion is tacitly endorsed by practically all of the disseminators of this kind of information, including those who oppose the use of scientific method in political science. How else could one defend the proposition that courses in political science influence students to be better citizens, for example? The alternative for a teacher of political science would be an admission that there are no practical grounds to justify the continuance of the courses he teaches. Some may still argue that this does not follow, but who among the others would accept such an alternative?

Propositions embodying such knowledge might then be included in theories of political behavior. Of course, one could raise the possibility that an awareness of these additional propositions would further modify behavior. But once we have traveled this far into the realm of a priori possibilities, there is no reason to refrain from always continuing one step further, stopping only at the steps in this process which seem to reinforce our own preferences or guesses concerning the lawful character of human behavior.

My primary concern in the above paragraphs has been to put forth several considerations which support the contention that even though political inquiry may itself influence the behavior which is its subject matter, this circumstance neither supports methodological arguments against the possibility of achieving lawful knowledge about political phenomena, nor does it otherwise present insuperable barriers against the development of such knowledge. I conclude that this objective, which seems to me a rather modest one, has been accomplished. To claim more on the basis of an essentially methodological discussion would not only be immodest, it would be mistaken.

6. The final item in our survey of those difficulties or alleged difficulties which confront the student of politics and which have apparently contributed a kind of specious support for anti-science viewpoints in the discipline.

relates to limitations upon the accessibility of relevant data. Two kinds of limitations have been mentioned. First, there are practical limitations resulting from such familiar facts as the secrecy which surrounds much political activity: from the executive sessions of legislative committees to the marking of ballots in voting booths. Second, there are what may be appropriately called methodological limitations which result from the wholly private character of other men's minds.¹ Concerning the first, it is true that practical barriers to the accessibility of data account for certain features of political inquiry: they are, for example, responsible for the fact that a number of inferences frequently precede the inference from theoretically relevant data to theory in political science. And such consequences as this one (which we discussed in Chapter Three) are methodologically interesting. But I cannot see any connection between practical limitations upon the accessibility of some political data and the question of the appropriateness of scientific method for the study of politics.

On the other hand, the second alleged limitation, which asserts that certain relevant data is in principle inaccessible, does raise a methodological issue. As we indicated in Chapter Three, the question of the accessibility and relevance of data describing mental contents was a

¹Harbold and Hichner, for example, mention both kinds of limits. Loc. cit., p. 756.

central issue in psychology. I say that it was an issue because, as I also suggested in our earlier discussion, it has for all practical purposes (as in research) been resolved (at least for American psychologists). The logical formulation of the answer, you will recall is what we discussed as methodological behaviorism. There is no point in reformulating the arguments again at this time; I will stand on our earlier defense of scientific psychology in the face of this supposed barrier, transferring the conclusions of that discussion to political science.

In this Chapter I have tried to include an analysis and criticism of all anti-science conceptions of political science which are probably more or less influential in the discipline at this time, with one major exception -- those related to values.¹ I have not attempted to include

¹And a minor exception -- anti-science conceptions based entirely upon religious grounds. They are not nearly so influential in the discipline as those we have considered; they are inextricably connected with commitments so at variance with those advanced here that their analysis from our point of view would be a voluminous and inconclusive task. However, the same persons who assert religious objections to the scientific conception of political science, also give other arguments (sometimes closely related to but separable from their religious views) which we have and will consider. See, for example, the writings of John H. Hallowell whose more secular arguments are considered in Chapter Seven. For a short summary of religious objections to (or serious modifications of) the idea of scientific political science which includes discussion of Hallowell's views, see: A. Brecht, Political Theory: The Foundations of Twentieth-Century Political Thought (Princeton: Princeton University Press, 1959), pp. 271-284.

mention of every political scientist who has written in support of such views. As to the anti-science conceptions related to values, I will discuss them in Chapter Seven. The following chapter, which is primarily a preparation for that discussion, contains a more philosophical consideration of values.

CHAPTER SIX

VALUES: PHILOSOPHICAL AND METHODOLOGICAL

In the first chapter I stated that methodological problems tend to appear in clusters, and that the resolution of a relatively small number of fundamental issues in each cluster was crucial to the resolution of all the rest. The methodological problems concerning values form such a cluster; within it there is one fundamental issue -- the dichotomy of facts and values. With respect to values, then, I intend to, first, clarify and firmly establish this dichotomy; and, second, to use the ideas developed in this discussion in order to resolve some major methodological issues related to values in political science. Neither of these two primary tasks requires that we carry our analysis of values beyond the bounds of philosophy of science. This suggests a third task.

It may be recalled that when I mentioned the subject of values in the opening chapter, I suggested that a wholly philosophy-of-science type discussion of values, a discussion limited to considering their place in science, was likely to be misleading. In a moment I will further suggest that some who have limited their attention to values in this way have, as a result, committed serious

philosophical blunders. Though my concern with values in this thesis is primarily methodological, I hope to avoid both of these kinds of shortcomings. Therefore, I shall also consider values from the point of view of philosophy proper. Needless to say, I have no intention of surveying ethical philosophy. I only wish to provide a crude outline of a metaphysical conception of values which is consistent with my approach to values in political science. And, as part of this discussion, I will finish our brief survey of Logical Positivism by including some remarks concerning the characteristic Logical Positivist views on values.

These three tasks regarding values will be undertaken in the following order: philosophy proper and the distinction of fact and value are considered in that order in two sections of the present chapter; the following chapter deals with methodological questions concerning values in political science.

A. Values: Common Sense and Metaphysics.

Logical analysis is to a large extent a matter of making distinctions. Two preliminary distinctions are required to delimit our subject. The first is obvious and requires no comment: philosophers usually divide the subject of values into ethics and aesthetics; we are only going to discuss ethics. Actually, this makes little difference for the present section of this chapter, since

our discussion will be at such a general level that what is said about ethics is just as appropriate for aesthetics; we will use an illustration from aesthetics for convenience only. For the rest of the chapter and the following one, I doubt if anyone will consider my neglect of the beautiful in political science as a serious omission.

The second distinction is of greater import. It is frequently referred to as the distinction between normative ethics and metaethics.¹ Normative ethics consists essentially of value judgments: statements about what is good, right, etc. Metaethics involves no value judgments; it is concerned, loosely speaking, with the nature of value. My interest here is in metaethics only; and, as I conceive it, metaethics is an enterprise in logical analysis. With this, all Logical Positivists would agree. Noting this fact may serve as an introduction to our brief consideration of their views on the subject of values.

We may recall from our discussion in Chapter Two that a central tenet of Logical Positivist philosophy is the verifiability criterion of meaning; that under all versions of this criterion a statement to be meaningful must be

¹For an excellent discussion of this distinction and its implications with respect to the subject of human rights, see: W. K. Frankena, "Symposium: The Concept of Universal Human Rights -- II," Science, Language, and Human Rights (Philadelphia: University of Pennsylvania Press, 1952), Vol. I, pp. 189-207.

capable of observational test;¹ that, with the adoption of physicalism, the required observations must be observations of physical things. In an early paper (1932), Carnap applied this criterion to what we would ordinarily call value statements:

The objective validity of a value or norm is (even on the view of the philosophers of value) not empirically verifiable nor deducible from empirical statements; hence it cannot be asserted (in a meaningful statement) at all. In other words: Either empirical criteria are indicated for the use of "good" and "beautiful" and the rest of the predicates that are employed in the normative sciences, or they are not. In the first case, a statement concerning such a predicate turns into a factual judgment, but not a value judgment; in the second case, it becomes a pseudo-statement. It is altogether impossible to make a statement that expresses a value judgment.²

In a prior statement in the same paper, we find the same idea stated more succinctly:

In the domain of metaphysics, including all philosophy of value and normative theory, logical analysis yields the negative result that the alleged statements in this domain are entirely meaningless.³

And somewhat later (1936) A. J. Ayer wrote in a widely read book expounding Logical Positivist philosophy:

¹I have, as some may have noticed, disregarded tautologies and contradictions in this account.

²R. Carnap, "The Elimination of Metaphysics Through Logical Analysis of Language," trans. A. Pap, Logical Positivism, ed. A. J. Ayer (Glencoe: Free Press, 1959) p. 77; orig. pub. as "Überwindung der Metaphysik durch Logische Analyse der Sprache," Erkenntnis, 2, 1932.

³Ibid., pp. 60-61.

We find that ethical philosophy consists simply in saying that ethical concepts are pseudo-concepts and therefore unanalysable.¹

A number of similar statements might be quoted, but these are sufficient to indicate the most characteristic Logical Positivist view of values. With respect to the logic of science, it amounts to a vigorous assertion of the distinction of facts and values; for philosophy proper, it is a denial that there are non-empirical (or non-natural) value qualities or any special faculty such as "moral intuition" for apprehending value qualities (or gaining moral knowledge).² With these positions, I agree; but to establish them the view concerning values revealed in the above quotations denies too much; it amounts to a denial that there are values.

I wish to outline a philosophical analysis of the nature of values which accomplishes the same purposes as the Logical Positivist view described above, but does not have its shortcomings. I will also comment further on these shortcomings, but we should note the following points before proceeding. "The Vienna Circle as a whole

¹Language Truth and Logic, (2d edit.; New York: Dover, 1946), p. 112.

²The intellectual motive to maintain these positions with regard to values is clearly apparent in Ayer's discussion, ibid., especially pp. 102-107. All of these positions belong to the empiricist tradition: see M. Brodbeck, "Toward A Naturalistic 'Non-naturalistic' Ethic," Philosophical Studies, 2, 1951, 7-11.

was not very greatly interested in ethics,"¹ but in so far as it adopted a position with respect to values, I think that position is correctly described above.² If Logical Positivism is used as a description of contemporary philosophy it must be divided into Formalism and Reconstructionism, as it was earlier in Chapter Two. In this case the view just attributed to Logical Positivism is more characteristic of the Formalists. The view presented below represents a glimpse of a philosophy of value developed in the writings of Reconstructionist Logical Positivists.

¹Ayer, "Editor's Introduction," Logical Positivism, p. 22.

²M. Schlick, the founder of the Vienna Circle, wrote a small book in which he held that "the central problem of ethics concerns the causal explanation of moral behavior" -- that is, that ethics is social science, mainly psychology. Problems of Ethics, trans. D. Rynin (New York: Prentice Hall, 1939), p. 28; orig. pub. as Fragen der Ethik (Vienna: Springer, 1930). This view does not conflict with that expressed above by Carnap; the "normative sciences" which Carnap deprecated included philosophical ethics, but not "ethics" in this peculiar sense. Actually, Schlick's claim that ethics should be social science fits very well the general Logical Positivist doctrine reflected in the quotations from Carnap: O. Neurath expressed it in slogan-like fashion when he wrote, "the body of scientific propositions exhausts the sum of all meaningful statements." "Sociology and Physicalism," trans. M. Magnus and R. Raico, in Logical Positivism, ed. Ayer, p. 282; orig. pub. as "Soziologie in Physikalismus," Erkenntnis, 2, 1931-32.

With respect to Schlick's book, it is of interest to note that he not only adopted the psychological hedonism of Bentham -- i.e., the thesis that human behavior is motivated by a desire to achieve pleasure and avoid displeasure, he also seems to adopt the hedonistic ethic that people should act to maximize pleasure or happiness. In support of the latter interpretation, see: S. C. Pepper, "A Brief History of General Theory of Value," A History of Philosophical Systems, ed. V. Ferm (New York: Philosophical Library, 1950), p. 499; and V. Kraft, The Vienna Circle, trans. A. Pap (New York: Philosophical Library, 1953), p. 185.

It starts from a common-sense consideration of what is involved in judgments of fact and value. The points are easiest to make if we examine the simplest possible illustrations. Suppose that, standing before a tree, I said, "There is a tree; it has brown bark and green leaves." This represents several simple factual judgments. Let us see what is involved. There was a physical object, the tree; I looked at it and had a percept (a mental state or conscious content); on the basis of this percept or some aspects of it, I made the judgments stated above. Now suppose that I added the value judgment, "This tree is beautiful." Again, it seems to me perfectly clear, that this judgment too was based upon some aspect of the content of my consciousness. What, then, is the difference between the factual and value judgments? Still speaking commonsensically, it is this: those aspects of my mental state which led me to say that there is a tree, and it is brown and green -- all of these correspond to other things, a physical object and its physical properties, which are "out there"; that aspect of my mental state which was the grounds for my saying the tree was beautiful, we might call it a beauty datum, does not correspond to anything "out there."¹ When interested in the dichotomy of facts

¹This illustration is taken from G. Bergmann, "Ideology," *Ethics*, 61, 1951, 205-218; reprinted in The Metaphysics of Logical Positivism (New York: Longmans, Green and Co., 1954), pp. 302-303.

and values, one emphasizes the distinction; when interested in the ontological (metaphysical) status of values -- whether they "exist" -- one must recognize the similarity. The common-sense core of ethical relativism is expressed by the difference; of absolutism, by the similarity. A correct philosophical analysis captures what is valid in both of these positions. More about what our illustration illustrates after a few additional remarks about it.

The value judgment in this illustration belongs to aesthetics, but it seems clear enough that if what we have said about it is correct for aesthetic judgments, it would hold as well for ethical judgments which are almost always more complex. That is, when we judge that something, a simple act or even a governmental institution, is good there is something in our mental state at the time which is the grounds for this judgment. I would further argue, and this is a point which was not made explicit in the above illustration, that this something else, the value datum, is sui generis.

What is the proof of all this, not just this last point but what I said about value in the above illustration? I suggested that "it seems to me perfectly clear"; that it is a matter of common sense. The latter remark is a clue to the question of proof. As I stated in Chapter Three when we discussed such things, one cannot argue directly for what one regards as commonsensical, just as

one cannot argue directly for one's most basic philosophical positions. There is a direct argument which might contribute to the plausibility of this one. That is, one may suggest a consultation of experience. By a kind of introspection, I find that when I make a value judgment there is an aspect of my total conscious experience which is something in addition to what is present when I make a factual judgment. Of course, even when this tactic results in agreement it does not actually resolve the genuinely philosophical questions. It is only the beginning (and it need not be the ending for those who don't see it). To resolve the philosophical issues one must proceed dialectically, as philosophers always have, to meet the opposing arguments within the tradition and also to show how the particular position one is advocating regarding values fits with a large number of other philosophical positions which one also holds to be correct. I am not going to attempt any of these things for the philosophy of values I am suggesting.¹ That is why I said I was only going to present a glimpse of it.

In Chapter Three we outlined a way of philosophizing by talking about an ideal language. We said that the ideal language is a schematic language in which everything

¹I hope it is evident that I am not referring in this statement to the analysis of values within the context of the philosophy of science; for this, the relevant arguments are given in Section B of this chapter and in the chapter which follows.

may in principle be described; that an ontology may be stated as a claim concerning the kinds of things named by the undefined descriptive terms of such a language; that an epistemology may be stated as a claim concerning the way in which we know the (referential) meaning of the undefined terms. The philosophical position regarding values which I favor and which is fully consistent with the other philosophical positions taken in this thesis may be stated as follows: The undefined terms of the ideal language refer to mental things; we know their meaning by direct acquaintance; "the ideal language contains at least one undefined descriptive term whose interpretation is the relevant root meaning of one of the English words that occur characteristically in ethical or aesthetic judgments."¹ Thus within the context of a phenomenalist ontology and an empiricist epistemology, this position asserts that there are values. To work out the position

¹G. Bergmann, "Logical Atomism, Elementarism, and the Analysis of Value," Philosophical Studies, 2, 1951, 85-92; reprinted in The Metaphysics of Logical Positivism, pp. 243-254, quote at p. 244. This philosophical position is given a technical presentation in the latter paper and in: H. Hochberg, "Phenomena, Value, and Objectivity," Philosophical Quarterly, 8, 1958, 208-225. For a non-technical exposition of the same viewpoint, see Brodbeck, loc. cit. And for a defense of essentially the same position with respect to a number of traditional philosophical viewpoints regarding values, see E. W. Hall, What Is Value? An Essay in Philosophical Analysis (New York: D. Van Nostrand, 1956).

more fully requires a technical discussion in terms of the ideal language employing the tool of symbolic logic;¹ we will explore the position more crudely without either this tool or this schematic language.²

The philosophy of values just described asserts that a full account of reality, a metaphysics, must include values; by contrast, the implicit metaphysics of the Vienna Circle denied the existence of values. In this, as I suggested above, our view is more consistent with common sense. At the same time, our position also (like that of the Logical Positivists) avoids hypostatizing either non-empirical value qualities or any special faculty for apprehending values, and it is consistent with the fact-value distinction in the logic of science. The value qualities whose existence it asserts are phenomenal; we know them by direct acquaintance; in these respects, they do not differ from anything else at the level of direct experience -- the place where all knowledge starts. At

¹It is developed in this way in the papers by Bergmann and Hochberg referred to in the last note.

²In case anyone thinks that the use of the ideal language or its equivalent is not required to unambiguously state such philosophical positions as this one, consider the following statement which is, in a sense, entailed by that whole position: "Values exist, but tables, chairs and politicians do not exist." This statement is literally nonsensical. The sense in which I believe it is true may be expressed in terms of the ideal language, which captures the peculiar meaning of 'exist' that is implied in ontology (or metaphysics). Given our discussion of value above and our earlier discussion of ontology in Chapter Three, especially the remarks about Berkeley's denial of the "existence" of physical objects, the unambiguous restatement is readily apparent.

this level, values are objectives (more precisely, the distinction of objective and subjective does not apply here), and value statements are true or false -- i.e., facts and values are not distinguished.¹ The distinctions appear among our experiences, but not at the most basic level. They appear later on, where the phenomenal data which corresponds to physical objects constitute patterns which are different from the value data.² Then fact, value, objective, and subjective are all sorted out, so to speak.³

This kind of talk may appear a little strange; I think it does; at least it seems a little too much like introspective psychology for one who believes psychology and philosophy proper are as distinct as I believe they are. All this I believe is a penalty one must pay for discussing technical philosophical questions without using the technical apparatus. Yet I feel that the effort has not been wasted; that some idea of an important philosophical position has been communicated. At least, I think I have distinguished my own philosophical posture regarding values from another one which it resembles in some respects. And we have suggested one major philosophical shortcoming of the Logical Positivist philosophy of value. But we have not explicitly pointed out that feature of this philosophical position which has aroused the most

¹Hochberg, op. cit., p. 219.

²Ibid., pp. 220-223.

³M. Brodbeck used the metaphor of "sorting out" in this context. See "Philosophy in America, 1900-1950," in M. Brodbeck, J. Gray, and W. Metzger, American Non-Fiction, 1900-1950 (Chicago: Regnery, 1952), p. 87.

criticism. Since this feature appears in its most pronounced form in what is called the "emotive theory" of value, I will comment upon it in the context of a few remarks about that theory.

"The so-called Emotive Theory which, mainly through the work of English and American philosophers, has come to be closely associated with logical positivism"¹ does not differ philosophically from the Vienna Circle's view of values described above. It starts from the same philosophical position, then, in effect, asks the question: if value statements are meaningless, what is their significance or function? The answer given by perhaps the best known version of the emotive theory is that value statements function to express and sometimes to arouse feelings or emotions.²

What is added by this theory to the position of the Vienna Circle belongs, strictly speaking, to psychology.³ What has been considered most objectionable about it is the way in which it apparently underestimates the significance of man's value judgments and moral experience

¹Ayer, "Editor's Introduction," Logical Positivism, p. 22; Ayer is himself one of the English philosophers referred to in this quotation.

²Ayer, Language, Truth and Logic, pp. 102-114; see especially p. 108.

³This is apparent in a book which has developed that theory most fully: C. L. Stevenson, Ethics and Language (New Haven: Yale University Press, 1944).

in life.¹ This is revealed by the reckless way in which value terms are labeled as "pseudo-concepts," and the sentences in which they appear as "pseudo statements"; and by the unfortunate practice of distinguishing a sentence expressing a factual judgment from a sentence expressing a value judgment by calling the latter "meaningless."² This much was already present in the writings of the Vienna Circle, the emotive theory magnified it by adding that "ethical judgments are mere expressions of feeling" or that they "are simply expressions of emotion."³ The italicized words make the point.⁴

¹See, for example, the criticism by D. Waldo, "'Values' in the Political Science Curriculum," Approaches to the Study of Politics, ed., R. Young (Evanston: Northwestern University Press, 1958), pp. 96-111; and M. Brodbeck, loc. cit., pp. 86-87.

²See, for example, Carnap, op. cit., pp. 76-77, and Ayer, loc. cit., pp. 107 and 112.

³Ayer, loc. cit., quotes are from pp. 112 and 103, italics are mine.

⁴A Criticism of the emotive theory by Professor Herbert Feigl (who was a member of the Vienna Circle) also shows the tendency of at least some versions of this view to depreciate the significance of values. He pointed out that "the emotivist assimilation of moral issues to questions of personal taste and preference does not even begin to do justice to the nature of argument and justification in the moral realm of discourse." "Validation and Vindication: An Analysis of the Nature and the Limits of Ethical Arguments," Readings in Ethical Theory, eds., W. Sellars and J. Hospers (New York: Appleton-Century-Crofts, 1952), p. 677. This volume contains a section of seven readings on the emotive theory, pp. 391-440.

This concludes our brief discussion of Logical Positivism and the philosophy of value. In the next section I will return to the subject of the dichotomy of fact and value at the physical object level, the level at which all scientific inquiry including the scientific study of politics proceeds. At this level, I agree with the Logical Positivists of the Vienna Circle. I hope that enough has been said to indicate that an insistence upon the fact-value distinction with respect to scientific inquiry does not commit anyone to either a metaphysics barren of value or that other kind of mistake which some adherents of the emotive theory maximized.

B. Fact and Value.

Probably the most pervasive distinction in metaethical philosophy is that between absolutism and relativism. Simply stated, the absolutists claim that value qualities are objective so that statements about them are true or false; relativists deny both of these claims.¹ I think the philosophy of value suggested above reflects what is

¹The terms 'absolutism' and 'relativism' have several other meanings with regard to ethical philosophy. For example, the claim that what is good or right is the same for everyone at all times has been called "ethical absolutism," and its denial "ethical relativism." I would say such differences belong to what we distinguished earlier as normative ethics; the usage suggested above is a common one with respect to metaethics. See, for example, F. E. Oppenheim, "In Defense of Relativism," Western Political Quarterly, 8, 1955, 411-417, especially p. 411.

correct in both of these positions. At the level of immediate experience where -- in a significant sense -- all knowledge begins, the absolutists are right. Thus it was asserted that value is one of the categorical (meta-physical) features of our world. At the physical object level the relativists are right: values are subjective and value statements are neither true nor false. This is the level at which all science proceeds, and the level at which value judgments are ordinarily asserted. Our interest from here on is in the subject of values as they relate to science, so I wish to raise several additional considerations in support of relativism at the physical object level.¹

We may do this by attending to some of the differences between factual qualities and value qualities -- differences which are the basis for saying that factual qualities are objective, but value qualities are subjective. I will list them succinctly and comment afterwards.

1) With respect to the observation of factual qualities, standard conditions may be stated under which all normal observers will recognize them; the same is not true for value qualities.² Actually the case of the person who

¹I continue to use the term 'relativism' though I believe it is clear enough from our discussion so far, that I reject both traditional relativism and absolutism with regard to values.

²Brodbeck, "Toward A Naturalistic . . .," op. cit., p. 9; Hochberg, op. cit., pp. 222-223.

is not normal brings out the point. As Bertrand Russell stated:

We cannot prove, to a color-blind man, that grass is green and not red. But there are various ways of proving to him that he lacks a power of discrimination which most men possess, whereas in the case of values there are no such ways, and disagreements are much more frequent than in the case of colors.¹

2) In connection with the recognition or perception of factual qualities, one can always point to some physiological apparatus which is involved, but there is no such "machinery" for sensing value qualities.²

3) We said earlier that judgments concerning both value and factual qualities are based upon experience, but the experiences on which value judgments are based are dependent upon learning in a way that the experiences which are the basis for factual judgments are not. As Professor Brodbeck put it, "Moral training is not a process of alerting the moral sense to perceive what exists independently but, in an untutored state, is overlooked."³

4) If our value judgments change we are not tempted

¹"Science and Values," A Modern Introduction to Philosophy, eds., P. Edwards and A. Pap (Glencoe: Free Press, 1957), p. 397.

²Hochberg, op. cit., pp. 222-223.

³"Toward A Naturalistic . . .," op. cit., pp. 9-10.

to assert that something "out there" in the physical world has changed, as we are when our factual judgments are altered. For example, someone might judge that the Old Age And Survivor's Insurance program is evil; later (perhaps upon attaining retirement age), he may come to morally approve of it; this alone would not lead him to judge that the program had changed, though he may have learned more about it.

5) There are more and longer lasting disagreements about the value qualities of things than there are about their factual qualities. I mention this difference separately, even though it is, no doubt, but a manifestation of many more fundamental differences -- those listed above and others.

The distinction between factual qualities and value qualities is the core of the fact-value dichotomy. It is the basis for the three ways in which the distinction of facts and values is most frequently described by philosophers of science and political scientists writing about the methodology of their subject. All three contribute, I believe, to our understanding of this fundamental distinction. I will, therefore, describe each of them, and at the same time indicate why they are reflections of the more basic distinction between factual and value qualities.¹

¹Reference to philosophers and political scientists who distinguish facts and values in the three ways to be discussed will be given at the end of that discussion.

First, it is pointed out that factual statements are subject to confirmation or disconfirmation on the basis of observational evidence -- i.e., they are true or false depending upon what the facts are. Value statements, on the other hand, are not subject to such an observational test; they are, therefore, neither true nor false. This is very much the same distinction as that achieved by Logical Positivists through the application of the verifiability theory of meaning (as we saw earlier). It is quite correct, and I believe it is the most appropriate way of formulating the distinction in the discussion of methodological issues regarding values. To see its dependence upon the difference between factual and value qualities, one only need ask why the value statements are not subject to observational test. And if one persists in this questioning, he arrives at the answer that value terms do not have empirical referents, and this, I would say, is simply because value qualities are not factual qualities. Notice, one could make the same assertion by claiming that there are no value qualities. This, I suggested above, was the implication of the Logical Positivist view discussed earlier. Remember we are speaking now at the physical object level -- that is why I can consistently say that value terms have no empirical referent while still adhering to empiricism and claiming that there are value qualities.

Second, facts and values are distinguished in terms of means and ends. Assertions of ends are value judgments; claims concerning the means of achieving designated ends are factual. As means and ends are distinct, so are facts and values. The argument stated in the last sentence is misleading. Because means and ends are different, does not show that facts and values are different. Means-statements and ends-statements may differ, yet both may be factual. And even if we accept the premise that ends-statements express value judgments, all this still does not establish that value judgments are not just one kind of factual judgment. The point is that the distinction of means and ends as related to values is a derivative of the fact-value distinction; it is not itself the grounds for that distinction. The discussion of means and ends in the context of the subject of science and values lends additional support to the conclusion. In such discussions the point is usually made that science can resolve means questions as these are questions of fact, but science cannot resolve questions about ends because these are value questions. The word I just italicized suggests our point: it is the relevant distinction of facts and values which supplies the grounds for these statements about science; it is not the other way around.

The third way in which the distinction of fact and value is frequently made is by pointing out the difference

between what is and what ought to be, and arguing that one cannot logically derive statements about the latter from the former. This too is correct; one might almost say that it is trivially correct, in that no conclusions of a deductive argument can contain any term whatsoever that is not present in one of the premisses. However, in so far as the pointing out of this feature of logic contributes in any way -- and I think that it sometimes does -- to the clarification of the fact-value distinction with respect to methodological questions, it is important enough to mention. On the other hand, we should also notice that as an argument for the distinction, it too is dependent upon the more basic considerations raised above regarding the differences between factual and value qualities.

Briefly, the relevant arguments for the latter claim may be stated as follows. Consider the following schemata of value judgments:¹

(V-1) X is good.

(V-2) You ought to act so as to bring about X.

There are two alternatives: (1) (V-1) implies (V-2); or (2) (V-1) does not imply (V-2).² If one adopts the second

¹I think all value judgments (remember: we are neglecting aesthetics) may be rendered in one or the other of the two forms given here. Whether or not this is so makes no difference for the present argument.

²The relation between what is good and what ought to be has been a persistent issue among moral philosophers; the two alternatives stated above and various related positions have been adopted. See Hall, What Is Value? pp. 154-190.

alternative, the logical argument that one cannot derive ought-statements from is-statements does not show that value statements in the form of (V-1) are not factual statements. The first alternative looks better, but it isn't. It is true that if (V-2) is implied by (V-1) and (V-2) is not a factual statement, then (V-1) is not a factual statement. But the trouble with this approach is that it raises problems concerning the status of (V-2) itself. Ordinarily, we would say that statements like (V-2) are non-factual for they are in the imperative form and sentences stating facts are always in the declarative form. However, it is an elementary principle of logic that any statement derived from factual statements is itself factual. Under the second alternative (V-2) is derived from (V-1); therefore, if (V-1) is factual, so is (V-2). This shows, it seems to me, that any argument for the fact-value distinction based upon the difference between is and ought which adopts the first of the two alternatives noted above must presuppose the very distinction it seeks to establish.

As I stated earlier, when philosophers of science and political scientists make the fact-value distinction, they most frequently do so in one or more of the three

ways just considered.¹ Actually, in almost all of such writing there is little or no investigation of the grounds for the distinction -- it is just asserted without

¹For example, see the following works by philosophers of science: N. Campbell, What Is Science, (New York: Dover Publications, 1921), pp. 160-164; J. G. Kemeny, A Philosopher Looks At Science (New York: D. Van Nostrand, 1959); E. H. Madden, ed., The Structure of Scientific Thought (London: Routledge & Kegan Paul, 1960), p. 327; and the following by political scientists: D. Easton, The Political System: An Inquiry into the State of Political Science (New York: Knopf, 1953), p. 221; C. S. Hyneman, The Study of Politics: The Present State of American Political Science (Urbana: University of Illinois Press, 1959), pp. 109-111; H. Simon, Administrative Behavior (New York: Macmillan, 1955), pp. 45-47 -- Simon includes the basic idea "that ethical terms are not completely reducible to factual terms," p. 46; A. Brecht, Political Theory: The Foundations of Twentieth Century Political Thought (Princeton: Princeton University Press, 1959). Most of Brecht's rather large book is devoted to a study of the fact-value distinction at the physical object level -- what he refers to as "Scientific Value Relativism." Brecht makes the fact-value distinction in all three of the ways discussed above (pp. 117-135, et passim); but I think he regards as most fundamental the distinction between transmissible (factual) and non-transmissible (valuational) knowledge (pp. 113-116, and 483-484). The latter is another way of expressing our basic distinction between factual and value qualities: factual terms refer to physical qualities which are what it is that we are intersubjective about; value terms refer to value qualities which are fundamentally mentalistic and we cannot be intersubjective about these. Brecht also supports the thesis of Scientific Value Relativism in some of the indirect ways required for a complete argument. Thus he considers the implications of this view in many different contexts (e.g., with respect to ideas of "justice," Chap. 4), and he answers opposing views (Chaps. 7 and 8). Any comparison of this or the following chapter with Brecht's much longer and far-reaching treatment of values will reveal much difference in the specific arguments analyzed and an even greater difference in the style of analysis; it will, however, reveal little conflict.

supporting argument. I do not mention this as any criticism. And my argument that the three cited grounds for the differentiation of facts and values are not fully adequate for this purpose does not -- assuming it is correct -- detract from the importance of these ways of describing the distinction. I felt obligated to probe the distinction somewhat more deeply than is customary in writing of this kind, since I had previously presented a view which denied that distinction at the level of metaphysics. I hope that both our consideration of the metaphysical status of values and of the most fundamental basis for the fact-value distinction with respect to science may help us to consistently maintain that distinction in our discussion of values and political science in the next chapter.

In order to complete our discussion of the fact-value distinction, I wish to restate the essential position to be maintained in Chapter Seven in a slightly different fashion, and clarify the significance of that position by considering several kinds of "value statements." To do so involves repetition of some ideas given in Chapter Four; in methodological writing such a review is sometimes helpful. A fact, as I use the term, is a state of affairs: (1) an object having a certain

characteristic, (2) an event taking place, (3) one kind of event regularly following another kind, etc. A factual sentence (synonym: empirical proposition) states a fact. Examples (1) and (2) are individual facts; example (3) is a general fact. Singular sentences state individual facts; generalizations state general facts. If the fact, individual or general, which a factual sentence states is the case (exists), that sentence is true; if not, it is false. This is determined, immediately or ultimately, by observation. All this gives one side of the essential position: factual statements are true or false solely according to the way the world lies, so to speak; nobody's value judgments have anything to do with it.¹ The other side of the position is that one cannot say the same of value statements -- they do not assert the existence of any state of affairs; it would be incorrect to designate them as true or false.

The remaining clarification relates to what is meant by 'value statement' in the last sentence. It so happens that characteristic value (ethical, moral) language is used in a number of different kinds of statements. Four of them are, I believe, important for our purposes. Only two of these express value judgments; the others are

¹Except when factual statements are made about people's values, as, e.g., that so and so made a certain value judgment; in this case, the truth or falsity of the statement is obviously dependent upon whether or not the person made the value judgment attributed to him. I make this simple point in order to avoid any possible suggestion that the position I advocate entails the neglect of facts involving values. More about this in Chapter Seven, below.

factual. Of the two which express value judgments, only one is intended by the expression 'value statement' as used above. Just this much suggests the possibilities for confusion if these four kinds of statements are not always recognized for what they are. In the following chapter we shall uncover such confusion surrounding methodological issues regarding values in political science. Let us proceed, then, to make the distinctions.

The first of two kinds of statements which frequently employ characteristic value terms but actually express factual rather than value judgments I will refer to as statements in which value terms are used in a non-value sense. An obvious example would be the statement "Schmid is a good chess player." This merely asserts that Schmid has a certain skill; the nature of that skill is sufficiently clear that a small number of rather simple observations would determine the truth or falsity of the statement. A less obvious illustration of a statement employing the same value term in a similar non-value sense would be "The late Senator Robert A. Taft was a good Senator," when the person making the statement intended only to attribute certain skills to Senator Taft.¹ Someone might

¹One might utter exactly the same statement as a moral judgment. This multiple usage is not at all peculiar to the words used in value judgments; more or less consideration of the context of a word's use is frequently required in order to discern its meaning.

make this statement even though he morally disapproved of almost everything that Senator Taft did. What skills or abilities mark a man as a good Senator are not nearly so well understood as those which establish him as a good chess player, but the two cases are the same in the sense that they are both questions of fact not of value. Since both of these illustrations dealt with a similar type of non-value use of value language, let me give one more: it is not unusual for someone to attribute a number of factual qualities to an automobile, for example, by saying that it is a good one, or that it gives good mileage, or that it holds the road better than some other automobile.

Both the possibilities for confusion from the non-value use of value terms and a handy rule of thumb for clearing up such confusion were suggested to me by a personal experience in a seminar several years ago. When reporting on a small group study which showed that leaders of a group are superior to non-leaders in their ability to judge group opinion, I spoke of the leaders as better judges of opinion and as good judges.¹ The study was immediately attacked, not only by some fellow students

¹I recently re-examined this study and found that its authors used the expression "better than average judges" and "good judges of others' attitudes." K. Chowdhry and T. M. Newcomb, "The Relative Abilities of Leaders and Non-Leaders to Estimate Opinions of Their Groups," Journal of Abnormal and Social Psychology, 1952, 47, 51-57.

but by the professor as well, for its inclusion of value judgments in what was supposed to be objective research! There is no easy explanation of this -- the research was fully described including the design and the specific findings. The matter was never resolved to everyone's satisfaction, though I eventually satisfied some of the objectors by restating the results of the study as I described them above -- i.e., by referring to the opinion-judging ability of the leaders. The handy rule of thumb suggested is this: where the full meaning of a sentence employing value terms can be rendered by another sentence in which there are no value terms, those terms were used in the original sentence in some non-value sense. That experience was sufficient to convince me that this first kind of possibly confusing "value statement" had to be considered -- even if I had not seen the same mistake in writing.

The second kind of factual statement in which value language is sometimes used, and which, consequently, lends itself to misinterpretation as expressing a value judgment may be called a means-end statement. Strictly speaking, such means-end statements are a subclass of the class of statements using value terms in a non-value sense, but they are so important that I have set them apart as a separate category. Two simple illustrations will help to bring out the relevant points. First, a woman is

struggling to start her car when an observant passerby suggests: "You ought to turn on the ignition." Second, a doctor advises a patient with a stomach ulcer: "You ought to adhere to a bland diet." The meaning of the passerby's statement may be rendered as: "In order to start your car, you must turn on the ignition." And the doctor's advice might be restated as: "If you wish to facilitate the healing of your ulcer, then you should eat a bland diet." When our original ought-statements are so restated it is evident that they, like all means-end statements, express purely factual judgments. Actually, the doctor's statement might very well have further meaning so that it does involve a value judgment; we will come to this matter presently.

The reason I said means-end statements were important and the reason it is important that we recognize them as factual even when they are expressed in a manner characteristic of value judgments is that they appear so frequently in political science writing. I commented in Chapter Five upon the extensive amount of practically oriented writing in political science; the following quotation from Professor Hyneman's study of the discipline makes explicit the connection between this writing and means-end statements:

A review of literature produced
by American political scientists over
many decades establishes beyond doubt

that finding out how social ends can be achieved has long been a prime preoccupation of the profession. I suggested above ... that our descriptions of single entities have limited value for science because the descriptions are developed to support a particular critique or analysis which the author intends to make. I think it probable that in most cases the critique or analysis which the description supports is concerned with relationships between means and ends.¹

Since means-end statements are so prevalent in our literature and value words sometimes appear in them, it is easy to overestimate the extent to which value judgments enter into political science writing. Herbert A. Simon's comment upon this matter with respect to the field of public administration is worth quoting at length:

It is sometimes thought that, since the words "good" and "bad" often occur in sentences written by students of administration, the science of administration contains an essential ethical element. If this were true, a science of administration would be impossible, for it is impossible to choose, on an empirical basis, between ethical alternatives. Fortunately, it is not true. The terms "good" and "bad" when they occur in a study on administration are seldom employed in a purely ethical sense. Procedures are termed "good" when they are conducive to the attainment of specified objectives, "bad" when they are not conducive to such attainment. That they are, or are not, so conducive is purely a matter of fact, and it is

¹Op. cit., p. 102. Hyneman lists some particular examples of such writing at pp. 122-124, but he says, "Because preoccupation with social engineering so pervades our literature, it would be superfluous to offer an array of our efforts to influence social action." He cites examples "for the benefit of newcomers to the profession." (p. 122)

this factual element which makes up the real substance of an administrative science.¹

In line with Simon's analysis it seems to me quite clear that the correct interpretation of the "principles" of public administration is as means-end statements. Thus the full meaning of a statement like "There should be a single responsible executive head,"² might be rendered by the means-end statement: "In order to maximize efficiency,³ there should be a single responsible executive head."

I hope I have not given the impression that in every instance it is easy to distinguish means-end statements from value statements. Even in the simple example given above of a doctor's advice to his patient, I suggested that the interpretation of such a statement as a purely factual means-end proposition might be inadequate. We must take our cues from the context of the statement with which we are concerned. In this case, the statement that the ulcer patient ought to adhere to a bland diet might mean both that doing so would facilitate his return to

¹Op. cit., p. 249.

²W. B. Graves, Public Administration In A Democratic Society (Boston: D. C. Heath, 1950), p. 492.

³Luther Gulick wrote: "In the science of administration, whether public or private, the basic 'good' is efficiency." "Science, Values and Public Administration," Papers on the Science of Administration, eds., L. Gulick and L. Urwick (New York: Institute of Public Administration, 1937), p. 192.

health, and that he ought to act in order to achieve health or that health is good. That is, it might be a complex proposition asserting both a factual judgment concerning the means to an end and a value judgment concerning the worth of the end. Such a complex proposition is called an instrumental value judgment. It is the third kind of statement I wish to distinguish and the first of two types of value judgments.

The other type of value judgment is termed intrinsic; unlike instrumental value judgments, intrinsic value judgments are pure in the sense that they have no factual component; they are the value component of an instrumental value judgment. Statements expressing intrinsic value judgments are our fourth kind of statement. Only this type of statement was intended when I wrote that value statements "do not assert the existence of any state of affairs; it would be incorrect to designate them as true or false." The extent to which the same assertions are applicable to instrumental value statements is clearly evident.

The distinction between two kinds of value judgments deserves further comment -- it is a distinction which is frequently helpful in achieving clarity in discussions of values. Another way to describe the difference is to say that instrumental values are things valued as means to something else, while intrinsic values are things valued

for themselves. Thus it is easy to see how someone may value the same thing both intrinsically and instrumentally. For example, a political theorist might hold that freedom of speech is intrinsically good -- i.e., good in itself; and he may also hold that it is instrumentally good because it contributes to the growth of knowledge which is good, intrinsically and/or instrumentally.

Though the distinction itself is clear, it is frequently difficult to determine for any particular value judgment found in a piece of writing whether it is intended as intrinsic, instrumental, or both. About the best one can do is determine whether or not an author seeks to justify a value judgment by an appeal to consequences; if he does not, then -- at least for the particular book or article involved -- the value judgment must be considered intrinsic. Actually, under these circumstances it would probably be more precise to speak of a contextual intrinsic value, thus allowing for the possibility of justification by an appeal to more fundamental values. Such complications as this do not concern us greatly -- for our purpose, it is enough to note the distinction in the manner described above.

The four kinds of statements distinguished above are: (1) those in which value terms are used in a non-value sense; (2) means-end statements; (3) statements

expressing instrumental value judgments; (4) statements expressing intrinsic value judgments. The first two are purely factual statements; as we said, it is important to avoid mistaking them for value statements; the following chapter will I believe support this claim. The last two types of statements express value judgments: type (4) statements express purely value judgments; type (3) may be thought of as a combination of a factual statement like (2) and a value statement like (4).

A great deal more might have been said in the present section concerning the fact-value distinction and the four kinds of statements listed above, however I think what we have said will be sufficient preparation for the analyses in the chapter which follows.

CHAPTER SEVEN

VALUES: METHODOLOGICAL PROBLEMS IN POLITICAL SCIENCE

In our discussion of methodological objections to the appropriateness of scientific method for the study of politics in Chapter Five, I omitted those objections related to values. The present chapter is primarily a continuation and completion of that earlier discussion. Thus our analysis of methodological issues surrounding values will focus upon major variations of the theme that values are involved in politics and/or the study of politics in such a way that scientific method is not appropriate for political science. I shall argue that in these respects political science is in principle "value-free." The same thesis may be stated as follows: political science, like the physical sciences, deals with facts and facts only; it does not produce value judgments; the truth of the statements it does produce is not dependent upon value judgments.

Any short statement of such a general thesis is bound to require a great deal of unpacking. The needed qualifications and explanations will, I believe, come out in the course of our discussion in this chapter -- especially if it is read in the light of what has gone before. Thus, I have tried to show all along

my awareness that political science as a discipline and political science as a science need to be distinguished, that the latter is part of but does not exhaust the former, and that I would not argue that the discipline as a whole should be a science. So one obvious qualification to my thesis concerning the possibility of "value-free" political science, is that it does not mean that political scientists can (even in principle) do the many things they now do without asserting value judgments. It does say that they can (especially in principle, but even in practice) pursue scientific inquiry without asserting value judgments. After a few more introductory comments, we will proceed to consider the arguments which deny this.

The decision to emphasize methodological problems regarding values as they relate to the general question of the scientific status of political science does not, in my estimation, seriously restrict our coverage of such issues. I think that all of the major ones arise in the context of this focus.¹ This is not to say that

¹An observation by Professor C. S. Hyneman is pertinent at this point: "I think it no exaggeration to say that the controversy about scientific emphasis in our study and the controversy about study of values are essentially one dispute, that science and values are opposite poles in a single area of intellectual conflict." The Study of Politics: The Present State of American Political Science (Urbana: University of Illinois Press, 1959), pp. 178-179.

methodological problems regarding values arise only in this context. I can think of two important ones that arise elsewhere. First, methodological problems may arise with respect to the treatment of values as data in empirical inquiry; second, methodological inquiry is relevant to the problem of detecting covert value judgments in empirical work. However, even these two subjects will come up in our discussion. At that time I will indicate how we have already resolved the first in Chapter Three, and we will at least have an opportunity to comment upon the second.

A second introductory comment may be gleaned from two quotations. C. S. Hyneman wrote:

It will not greatly misrepresent the actual state of affairs to say that two wings of the political science profession in this country wage a continuing battle on the issue of science vs. values.¹

However, as L. A. Dexter observed, this is "a controversy which, though common enough among social scientists, is more often conducted orally than in writing."² Consequently, though I believe the issues discussed below have some prominence in the discipline, as Hyneman suggests, the views of only a few writers will be emphasized.

¹Ibid., p. 174.

²"Political Processes and Judgments of Value," American Political Science Review, 40, 1946, 294-301, at p. 294.

Finally, we will need a name for the thesis I am advocating -- it is not quite proper to refer continually to a view which is an important part of the empiricist tradition in philosophy as mine. Its opponents frequently call it "positivist," and those who advocate it "positivists." There is some historical justification for this label, so I will adopt it here, but please remember that what I mean by it is the position as developed in the last chapter. However, remember too that with respect to methodology, the subject we will be talking about, all of those who make the fact-value distinction in any of the ways reviewed in Chapter Six and advocate objective (scientific) social science may be lumped together under this same label. Also, for convenience, I will refer to all those who oppose the positivist thesis as "anti-positivists." In most cases, they are, according to our terminology of Chapter Five, members of the "anti-science" group of political scientists.

A. Values Involved in the Subject Matter.

Political science studies facts and only facts. In this it is like the physical sciences. But one outstanding difference is that among the facts studied by political scientists, there are values. Anti-positivists have, I believe, exaggerated the methodological significance of this difference. Though I hasten to admit that the

evidence which I shall discuss in this section does not firmly establish either the nature or the existence of these exaggerations. Two rather general positions concerning methodological implications of the presence of values in the subject matter of political science will be considered. It will be helpful to have a name for these facts involving values; let us call them "value-facts."¹

1. Nobody, as far as I know, has denied the significance of value-facts among the data of the social sciences generally or of political science in particular (though they are not usually referred to as "values" -- more about this later); certainly the positivist position does not deny or otherwise exclude value-facts from the category of relevant data.²

¹I first saw this expression used by T. I. Cook. See his quotation below.

²Nor did Logical Positivists of the Vienna Circle deny the existence of value-facts or their relevance for social science. They held that sentences expressing value judgments were cognitively meaningless, but they never denied that people uttered such sentences or made such judgments. See, e.g., Schlick, Problems of Ethics, trans. D. Rynin (New York: Prentice Hall, 1939), especially p. 28; orig. pub. as Fragen der Ethik (Vienna: Springer, 1930); and O. Neurath, "Sociology and Physicalism," trans. M. Magnus and R. Raico, in Logical Positivism, ed. A. J. Ayer (Glencoe: Free Press, 1959), pp. 305-307; orig. pub. as "Soziologie in Physikalismus," Erkenntnis, 2, 1931-32. It is true that I attributed to some Logical Positivists an implicit metaphysics which excluded values, but that is a very different matter.

Yet anti-positivists in political science sometimes seem to claim that the positivist position has this consequence. For example, J. H. Hallowell, a leading exponent of the anti-positivist view regarding values in political science, wrote in an article which was part of a well known controversy with the social anthropologist W. F. Whyte:

Now in challenging American political scientists to accept the perspective of positivism, Professor Whyte is simply urging them to retain a perspective which a great many of them have already accepted. His challenge does raise the question, however, whether the perspective of positivism is a sufficiently adequate one from which to observe and understand political phenomena, and whether by ruling out values as objective truths it automatically eliminates much that is pertinent, if not essential, to the understanding of any political process or behavior.¹

I will quote Whyte's reply at length because it defines quite well some aspects of the positivist's position:

I believe that the only useful sort of conceptual scheme in the social sciences is one made up of elements which are subject to first-

¹"Politics and Ethics," American Political Science Review, 38, 1944, 639-655, at p. 644; see also, p. 653 et passim. This paper is a reply to W. F. Whyte, "Instruction and Research: A Challenge to Political Scientists," ibid., 37, 1943, 692-697. The controversy over issues raised in these two papers was continued in "Politics and Ethics -- A Symposium," consisting of papers by Whyte, Hallowell, G. A. Almond, and L. A. Dexter, ibid., 40, 1946, 283-312.

hand observation and/or experimentation. The ethical values of the researcher do not meet this requirement, and therefore have no place in this scheme.

This does not mean, as Dr. Hallowell implies, that I have no scientific interest in what people believe in. I recognize, as he does, that the values held by people are an important dynamic factor in their behavior. I look upon the values of the people I study (discovered through interviewing and observation) as important research data. I feel that my own values have no place in my analysis of human behavior, and I try to disregard them as much as I possibly can.¹

Against the general claim that the positivist position entails the neglect of value-facts as data, I think Whyte's reply is adequate. A consideration of the nature of value-facts and of research about them is also relevant. This is included below in the discussion of the second anti-positivist position in the present section. Actually, Hallowell's answer to the above statement by Whyte seems to indicate that, at least as far as Hallowell is concerned, the matter of the inclusion of value-facts as data is not a significant issue:

Now human value judgments, I contend, are something more important and vital than

¹Whyte, "Politics and Ethics: A Reply to John H. Hallowell," part of "Politics and Ethics -- A Symposium," ibid., p. 304.

"research data" (whatever that may mean). They are forces that can create a better world in which to live or provide the dynamite to blow it apart.¹

It also suggests what I believe has been an important motive behind the anti-positivist position: the belief that positivist social science not only underestimates the importance of values but somehow contributes to their deterioration.²

So far I have only suggested that there is an anti-positivist thesis to the effect that the positivist position has the shortcoming of leading to or entailing the neglect of value-facts. We have not examined any actual arguments related to this position. We will, however, examine anti-positivist arguments in the next section which at least strongly suggest this criticism of the positivist perspective. That is, arguments in which it is claimed that value judgments are required in order to recognize and describe certain political phenomena, especially value-facts. Since the positivist perspective prohibits value judgments in political research (but it certainly does not prohibit value judgments), it would follow that positivists could not deal with such

¹"Politics and Ethics: A Rejoinder to William F. Whyte," ibid., p. 309.

²See the writings of Hallowell, L. Strauss, T. I. Cook, D. Waldo, and H. V. Jaffa, which are cited in this chapter. Against this view, see: F. Oppenheim, "Relativism, Absolutism, and Democracy," American Political Science Review, 44, 1950, 951-960; and "In Defense of Relativism," Western Political Quarterly, 8, 1955, 411-417.

political phenomena in their research.¹ However, this matter is complicated by the fact that some anti-positivists argue that the observation of any social facts requires value judgments. This, too, is considered in the next section.

2. Anti-positivist writers emphasize the prevalence of value-facts in the subject matter of political science. If they do this to establish that political science is not value-free in this sense, they are thoroughly correct; only, as I suggested above, nobody has ever claimed otherwise. However, in so far as they assert that the presence of values in his data requires that the political researcher assert value judgments in his conclusions, they are mistaken. I do not know if this claim can be considered an important part of the anti-positivists' position, since in their writing they more frequently claim that the study of any social facts (or any facts at all) necessitates value judgments.²

One political scientist who specifically claims (in writing) that the presence of values in the data of political science requires that value judgments appear in

¹See, L. Strauss, What Is Political Philosophy? and Other Studies (Glencoe: Free Press, 1959), pp. 20-21; and the discussion of this in Section B, below

²See below, Section B.

the conclusions of political inquiry is Thomas I. Cook -- at least he seems to argue this way. Consider the following quotations from his writings on the subject of values and political science:

Once, however, it is admitted that value-facts are relevant and objective evidence, it is impossible to exclude value judgments from the body of one's science: the social sciences are essentially normative.¹

Professor Easton, as a political philosopher, was completely aware that while it was necessary to observe and analyze disinterestedly, that very task involved an acknowledgement that men held values, and an understanding of what values they held, and why. But then a further implication followed: a science of politics had to be both normative and descriptive.²

The student of society who today needs to observe and to reflect on more rather than less facts than has been his wont, has once again to become aware that

¹"Power and Society; a Framework for Political Inquiry," Journal of Philosophy, 48, 1951, 690-701, at pp. 697-698, italics mine. This is an essay; review of H. D. Lasswell and A. Kaplan, Power and Society: A Framework for Political Inquiry (New Haven: Yale University Press, 1950).

²"The Political System: A Stubborn Search for A Science of Politics," Journal of Philosophy, 51, 1954, 128-137, at p. 134, italics mine. This is an essay-review of D. Easton, The Political System: An Inquiry into the State of Political Science (New York: Knopf, 1953).

social studies, just because they are concerned with man, are normative.¹

In an earlier paper, not one of the three from which these quotations were taken, Professor Cook advocated a separate moral science; he recommended that it be called "normative sociology" and that it should be a modern version of Aristotle's conception of "politics."² Later, in his contribution to the UNESCO study of political science, he held that the social sciences generally should include "the postulational and deductive science of values."³ At this point, I merely mention the idea of a "science of values"; our concern is with the thesis that the body of political science (or all social sciences) must contain value judgments because there are value-facts in the subject matter; that the conclusions of political research must for this reason contain value judgments. The quotations above seem to indicate that at least one anti-positivist makes this claim; the advocacy of a "science of values" by the same writer adds to the plausibility of imputing this thesis to him. It is in

¹"The Prospects of Political Science," Journal of Politics, 17, 1955, 265-274, at p. 272, italics mine.

²"Politics, Sociology, and Values," Journal of Social Philosophy, 6, 1940, 35-46, see especially p. 45.

³"The Methods of Political Science, Chiefly in the United States," Contemporary Political Science (Paris: UNESCO, 1950), pp. 75-90, see especially p. 76.

such blatant opposition to the positivist position advocated here, and, as far as I know, nobody has specifically replied to Professor Cook's thesis, that we are -- it seems to me -- compelled to consider the arguments in rebuttal.

As I see it, three separate considerations or approaches are relevant and adequate for this purpose. First we should consider the evidence and/or argument for the thesis. An examination of the five papers by Cook cited above reveals neither evidence nor argument. The first part of the thesis seems to be taken as argument for the second -- i.e., the fact that political scientists study people and people have values is the argument for the claim that political scientists must make value judgments. I am questioning the grounds for this inference. It is significant that none are offered.¹

The second approach relevant to an evaluation of this thesis consists of an analysis of the more general thesis that value judgments by political scientists are an integral part of any political inquiry. Actually, this position and the one we are considering now might

¹Professor Cook does admonish us that political scientists must make value judgments in order to avoid various undesirable consequences which would ensue if they did not (see, e.g., "The Prospects of Political Science," op. cit., pp. 271-272), but I hardly think this could be offered as grounds for the thesis under discussion which asserts that they must advance value judgments because "value facts are relevant and objective evidence."

be considered the same. However, the more general thesis does not emphasize the presence of values in the data; it emphasizes the values of the researcher. It is correctly described as more general because anti-positivist writers apply it to at least any factual inquiry in the social sciences and sometimes to the physical sciences as well. We will take it up next in Section B, though our third consideration with respect to the thesis under discussion is also relevant to an assessment of the validity of the more general thesis.

Value-facts, as we have been speaking of them, are facts involving values. This much is obvious enough. I think it is safe to say that in the anti-positivist writing on the subject one finds little more than this on the nature of such facts. Our purpose will I believe be advanced by taking a closer look at these "facts involving values," and, especially, some aspects of social research about them. This is our third consideration related to the thesis that the study of such facts requires value judgments. I will give an account of value-facts which seems to me non-controversial. I don't mean that all of those whom I am discussing in this chapter as anti-positivists would agree; perhaps none of them would. I mean that my account of value-facts will probably be acceptable to all, or nearly all, scientific social scientists. For anti-positivists, particularly in the

present case for Professor Cook, I think I can show that it would be difficult to maintain some anti-positivist views unless they did reject my account of value-facts. It will follow, then, that to the extent that this account is correct, these anti-positivist views are likely to be incorrect.

We say of an individual that he has values, that he makes value judgments, and that he engages in behavior which is, in part, determined by his values.¹ Groups, such as political parties or whole societies, are also characterized by their values; in this case one speaks of mores, norms, prevalent belief-systems, dominant ideologies, and the like. To say of a group that it has a certain value is to make a complex statement about the members of that group -- one which, if it were fully spelled out, attributes values and probably other characteristics to a certain statistical distribution of the group's members. Or, to say the same thing differently, the value concepts used in the description of groups

¹I omit the idea of things which are values. Though there are value-facts which are correctly described by a statement asserting that something is a value, this description must (within a scientific frame of reference) contain at least an implicit reference to some person or group who values that thing; the thing may be described as a value only by virtue of this relationship to the person or group. Thus, I believe my characterization of values above captures what is essential about any such value-fact. For further discussion of this point, see F. Adler, "The Value Concept in Sociology," American Journal of Sociology, 62, 1956, 272-279.

are definable in terms of individual concepts. This is merely an application of the principle of methodological individualism explained in Chapter Four. I mention it to make clear that we can make our points about value-facts by attending only to those about individuals. Our discussion will, therefore, focus upon psychology.

Let us take a brief look at the kinds of factors which enter into the explanation of individual human behavior --- what are sometimes called the "relevant variables" for such explanations. There are two general kinds: characteristics of the individual, and characteristics of the individual's environment. This much may be regarded as scientific common sense. Every approach to the explanation of behavior involves a further breakdown of the general category of individual characteristics (and, frequently, of the environmental characteristics, but I am not interested in further precision about these). I shall further divide individual characteristics into (1) primary drives or needs, (2) what the psychologists call "individual differences," and (3) acquired behavioral dispositions. As I understand them, the first two categories are biologically determined and the third, as its name indicates, is acquired. I would say learned, for my own choice among theoretical orientations within psychology is S-R learning theory. But even though I use language which is characteristic of this orientation in

psychology, the above distinctions could just as easily be made among the variables (concepts) employed within other contemporary psychological theories. That is, I think I can say this much about psychology without really adopting any particular theoretical position within scientific psychology.

Professor D. T. Campbell has compiled a list of seventy-six different terms which have been used to name acquired behavioral dispositions. The last term in the alphabetical listing is 'value.'¹ This is the interpretation I wish to advance: the basic ingredient in value-facts is a behavioral disposition of some kind. It would not necessarily always be the same disposition -- there may be several which are regarded as values. Like all dispositional concepts, the meaning of these would be rendered by operational definitions. The task of deciding how many such concepts are needed, and of providing their definitions is wholly a scientific one; it is not methodological. But the claim that empirical value concepts are dispositional is based upon the principle of methodological behaviorism which was discussed in Chapter Three.

At the same time, from another perspective, this

¹"Social Attitudes and Other Acquired Behavioral Dispositions," August 1959 version (mimeo) of a chapter to appear in: S. Koch, ed., Psychology: A Study of A Science, Vol. VI: Investigations of Man as Socius: Their Place in Psychology and the Social Sciences (New York: McGraw Hill, in press), p. 9.

principle provides the answer to methodological questions concerning the empirical significance of value concepts, since they would not, as understood here, refer to anything directly observable. The answer for value concepts is, I believe, the same as it is for other abstract psychological concepts (as those we referred to as "mentalist" in Chapter Three), namely, that they are construed as behavioral dispositions. In this way they follow the principle of methodological behaviorism. This is why I said earlier that we had already resolved some methodological problems about values in Chapter Three.

Even a cursory examination of social research about values reveals that social scientists -- most of such research is done by psychologists and sociologists -- ordinarily treat them as behavioral dispositions.¹ It also reveals, I think, that what is referred to as 'value' by some is designated by some other dispositional term by others. In general, there are no doubt a great many synonyms among the list of seventy-six disposition

¹To support this claim in some cases would require an appeal to the arguments we gave in support of methodological behaviorism in Chapter Three. Thus it would probably be more correct to claim that behavioral dispositions represent the empirical content of value concepts in social research, while admitting that all social researchers do not explicitly handle their value concepts in this way. I think this matter will appear much less controversial if the reader will remember that I am not talking about the general topic of how social scientists conceive of value; I am referring to concepts used in describing value-facts; and I am further limiting the reference of the above claim to social scientists reporting upon empirical research.

terms compiled by Campbell, his translation of a number of them into others supports this conclusion.¹ Moreover, we find that psychologists generally do not use the term 'value' and its cognates, even when they are studying apparently the same sort of things which a few of them do designate by the value terms.²

In accordance with this, an examination of the Annual Review of Psychology³ for the years 1951 through 1960 revealed an index entry for "values" in only two of these ten volumes: two studies related to values were reviewed in the 1958 volume and six in the 1959 volume. On my examination of these studies, it was obvious that studies of the same and very similar subjects which did not employ the value terminology were reviewed in the other eight volumes. I am not going to specifically document this conclusion, however comments by the two psychologists who reviewed the value studies in the 1959 volume may be cited for support. Thus G. G. Thompson wrote of values and attitudes as though they

¹Op. cit.

²A comment by W. F. Hill is very much to the point: "Few behavioral scientists would regard values (in the empirical, not the transcendental sense) as fundamentally different from such behavioristic constructs as Hull's habit strength or Tolman's equivalence beliefs." "Learning Theory and the Acquisition of Values," Psychological Review, 67, 1960, 317-331, at p. 318. This article is an interesting "attempt to study the acquisition of values as a branch of learning theory" (p. 319).

³(Palo Alto, California: Annual Reviews Inc.)

were the same thing and he stated: "We infer that attitudes and values are predispositions to behave in overt and covert ways."¹ And R. M. Gagné said of the author of one of the articles on values which he reviewed:

He describes a number of studies in which values (or attitudes), as measured by choices may be altered by environmental circumstances to bring about increased dissonance between the attitude and the behavior, and thus greater pressure to reduce the dissonance.²

It seems to me clear that a number of different concepts are used in the description of what we have been calling value-facts. Thus in his paper, "Psychological Studies of Values," W. F. Dukes concluded:

Finally, it should be noted that such terms as attitude, interest, motive, need, sentiment, valence are often used interchangeably with value, or at least to refer to some aspect of value³

Also, an examination of the research techniques employed

¹"Developmental Psychology," ibid., vol. 10, pp. 1-42, quote from p. 13.

²"Problem Solving and Thinking," ibid., pp. 147-172, quote from p. 149, italics mine.

³Psychological Bulletin, 52, 1955, 24-51, quote from p. 25. For an interesting explication of the concept of value which distinguishes it from these related concepts, see C. Kluckhohn, "Values and Value Orientations in the Theory of Action: An Exploration in Definition and Classification," Toward a General Theory of Action, eds., T. Parsons and E. Shils (Cambridge: Harvard University Press, 1954), pp. 388-433.

in studies of values shows that they are similar, if not identical, to those employed in the study of other psychological phenomena, especially attitudes.¹

Another quotation from the paper by Dukes will serve to introduce the first point relevant to our discussion of the anti-positivist position regarding values supported by the line of discussion we have been following in the last several pages. Dukes asserted on the first page of his article that

the strength of the Titchenarian attitude that social scientists should maintain a hands-off policy toward values has been reflected in the dearth of penetrating studies of values in the psychological literature and in a general lack of psychological textbooks in which value occurs as a major concept -- even in areas like personality and social psychology.²

It seems to me that he answered his own complaint in the earlier quotation above taken from the next page of his article. At the same time, he answered anti-positivists in political science who claim that positivistic social scientists have neglected value-facts. I don't wish to argue about whether or not there is a "dearth of penetrating studies," but there certainly has been a vast

¹See W. A. Scott, "Empirical Assessment of Values and Ideologies," American Sociological Review, 24, 1959, 299-310; cf., B. F. Green, "Attitude Measurement," in G. Lindzey, ed., Handbook of Social Psychology (Cambridge: Addison-Wesley, 1954), Vol. I, pp. 335-369.

²Op. cit., p. 24.

amount of research, especially on attitudes, but also on motives, needs, etc., which, as Dukes stated and our discussion indicates, have frequently been studies of the phenomena referred to as values.

This is one relevant point our discussion so far suggests. It is not the specific purpose for which this discussion of value-facts was introduced. It was invoked to support the idea that value-facts are not fundamentally different from any other facts regarding human behavior. And this circumstance constitutes an argument against the view attributed above to T. I. Cook that research on value-facts requires the assertion of value judgments. Of course we did not have to be so elaborate; citation of the research on values together with the simple observation that in almost every case the reports of this research do not contain the assertion of any value judgments, would itself provide most of the weight of our argument. With this I cite the research and make the observation.¹ But I hoped that our longer discussion of value-facts would also lend some support to the general perspective regarding values I am attempting to advance.

¹See the studies discussed by the persons cited above, especially Dukes, op. cit. and Scott, op. cit. Actually, Dukes mentions some psychologists who believe their research provides a basis for value-judgments. The anti-positivists are not all in political science.

We have considered two anti-positivist theses based upon the fact that values are involved in the subject matter of political science. I call them anti-positivist theses, although, as we noted earlier, they are not widely proclaimed in anti-positivist writing. However, there seems to be sufficient grounds for attributing them, one each, to two leading spokesmen for this position in political science. Thus Professor Hallowell apparently claimed that the positivist perspective precludes the inclusion of value-facts in political inquiry; and Professor Cook seems to say that the study of value-facts must result in value judgments. Our discussion provides, I believe, an adequate basis for at least a tentative rejection of both of these claims; we will meet arguments in the next section which if correct would establish at least one and, perhaps, both of these positions.

B. Values Involved in the Process of Inquiry.¹

¹There has been a large amount of writing on the subject of our present section, usually under the heading of "objectivity." I have found the following most helpful: Q. Gibson, The Logic of Social Inquiry (London: Routledge & Kegan Paul, 1960), Chaps. 6 and 7; M. Mandelbaum, The Problem of Historical Relativism: An Answer to Relativism (New York: Liveright Pub. Corp., 1938), Chap. 6; G. Ryle, Dilemmas (Cambridge: Cambridge University Press, 1954), Chap. 7; Isaac Levi, "Must the Scientist Make Value Judgments," Journal of Philosophy, 57, 1960, 345-357; C. Frankel, The Case for Modern Man (New York: Harper & Bros., 1955), Chap. 7; and, in my estimation the most thorough analysis of the problem of objectivity in social science, E. Nagel, The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace, & World, 1961), Chaps. 13, 14, and 15.

The values of the political scientist play an essential role in all political inquiry. This is the claim embodied in the anti-positivist arguments considered in the present section. His values are involved, first, because certain aspects of political inquiry necessitate value judgments; and, second, they are involved through the unavoidable influence they exert at every stage of his research, from the initial formulation of a question or hypothesis to his interpretation of the results. Consequently, anti-positivists claim, explicitly, or implicitly, that the objectivity required by scientific method is unattainable in the study of politics. In this, anti-positivism regarding values becomes a part of the anti-science tradition.

Before turning to the analysis of the arguments, the troublesome idea of objectivity requires some clarification. We have already discussed what may be regarded as a minimal meaning of objectivity for science in Chapter Three. It is also, I believe, the most fundamental meaning. I refer to the requirement of an intersubjective language. In terms of our earlier discussion this may be succinctly described by saying that scientific statements must be capable, in principle, of translation into what we called the "realist" language. We have also described the same idea in both Chapters Three and Four in terms of the first principle of scientific concept

formation: the descriptive concepts of science must be verbally connected (as by definition) with other terms which directly refer to physical objects or their properties -- specifically, to simple objects or properties with which we are immediately acquainted.

One consequence of this feature of the vocabulary of science is that the descriptive propositions of science state facts, and their truth or falsity is, therefore, contingent only upon whether or not those facts exist as stated. This is one of the senses in which science is said to be impersonal, which is another connotation of objectivity. That is, scientific statements are impersonal in the sense that their meaning and truth is independent of the personal qualities of the person who utters them. It should be apparent, on the basis of our analysis of value qualities in the last chapter, that this is not true of statements which assert value judgments. Anti-positivist arguments to be considered in this section have as a consequence the doctrine that value judgments are an inextricable part of the meaning of statements about political phenomena. This is one way in which they deny the possibility of objectivity in political science.

What we have discussed so far may be called objectivity in the context of meaning. It is, as I suggested, the most fundamental aspect of the idea of objectivity

in science. Other aspects of this idea may be spoken of as objectivity in the context of truth. It makes no sense to speak of a true scientific proposition which is not objectively meaningful. On the other hand, it is obvious that to say that a statement is objectively meaningful implies only that it is true or false, it does not imply that it is true.

However, the term 'objective' does have connotations with respect to truthfulness. To say of any instance of scientific research that it is objective, at least suggests that certain kinds of errors are not present. As Quentin Gibson put it when writing of objectivity in this sense: "To be objective in the conduct of an enquiry is not to have one's beliefs influenced adversely by one's motives, by custom, or by one's social situation."¹ Adverse influences of this kind would be those which prevent one from "taking account of relevant facts and accepting the conclusion to which they point."² It is quite apparent that in this sense, one could only speak of achieving more or less objectivity. However, to the extent that one did achieve it, we know that the probability that the conclusions of his research are true is increased. Let us note carefully, though, that neither

¹Op. cit., p. 77. Objectivity as described by Gibson is what is frequently spoken of as "unbiased" research.

²Ibid.

the presence nor the absence of objectivity, as we have just been discussing it, implies any logical guarantee of either the truth or falsity of research conclusions.

There are other related connotations of the term 'objectivity' such as the public nature of the whole scientific enterprise, the honesty of scientists, and the like, but the two ideas of objectivity discussed above are the ones that concern us here. Primarily, we are interested in objectivity with respect to meaning; this is the sense of it which is bound up with the most basic principles of the methodology of science; its denial, in one way or another, by anti-positivists is -- in my estimation -- the most fundamental issue regarding values in political science. We will attempt to consider enough specific anti-positivist arguments to exhibit the major attacks upon the possibility of this kind of objectivity in political inquiry.

Objectivity in the second sense, though it frequently raises serious problems, ordinarily has no direct relevance to methodology. Generally, the problems it raises concern discovering ways of avoiding certain sources of error in research. With respect to our present subject, we may say they involve devising procedures to avoid adverse effects of the researcher's values upon his work. The difficulties of avoiding them in political science and instances of work in which they are not avoided are,

I suspect, a source of inspiration for some anti-positivist views, so one might attribute some indirect methodological significance to this practical problem of research. Finally, there is one genuine methodological issue regarding this second sense of objectivity: the issue raised by the claim that such objective **research** is, in principle, impossible to achieve in political science. So we will meet with objectivity in both senses in the course of the analysis which follows, though I am not going to show the relation between each of the arguments discussed and either of these conceptions of objectivity.

1. It is appropriate to begin our investigation of anti-positivist arguments based upon the presence of values in political inquiry with those related to the most basic process of all scientific inquiry -- observation. We will consider, first, the alleged influence of values upon perception and, second, the claim that value judgments are required for the recognition of political facts -- especially "important" political facts. Actually, the notions of perception and recognition as used in these arguments are similar, but in some respects they are different, and there are separable arguments which emphasize one of these terms or the other; so we will separate them this way.

a. Anti-positivists have sought support for their views concerning the significance of values in political science by claiming that values influence "perception."¹ A forceful assertion of the importance of the relationship of values and perception in this regard is contained in an article by the political theorist H. V. Jaffa. The substance of his argument is contained in the following paragraph:

Easton has argued, and argued very soundly, that a value-free social science is a delusion. Values have a determining influence upon everything human beings do, and political scientists are human beings. Research interests are determined by values; criteria of relevance are so determined; the ways in which we select our data are so determined. Most important of all, our ability to perceive social reality -- and what we are capable of believing is social reality -- is determined by our values. The finest passages of Easton's book are animated by this insight. Men with one set of values are actually sensitized to one set of relationships; men with certain other values are incapable of perceiving these same relationships. In truth no one, and that most assuredly includes the scientific investigator, perceives social reality except through the media of certain values, opinions which determine what he can see and how he can see it. Perception of social reality is itself social perception, "cultural apperception" the social psychologists call it. Without having some way to judge the qualities of the lenses we use to look out upon the social or political universe, we have no way of knowing which of the

¹In addition to the article discussed below, see J. H. Hallowell, "Politics and Ethics," American Political Science Review, 38, 1944, 639-655.

unlimited number of possible universes is the universe. How do we know, for example, that our ideal of an inter-cultural or transcultural social science is anything but a "value" of our culture, and that the elements in other cultures that seem to be common to all are more than an illusion inspired by our desire to find them?¹

As I see it, we cannot deal adequately with the chain of relations from values to perception to political inquiry, and the far-reaching issues raised in this passage without distinguishing two relevant meanings of the term 'perception.' We will proceed by making this distinction and applying each one to the subject of our discussion. It turns out that the final clarification of Professor Jaffa's position lies in yet another direction, but proceeding in the order just indicated will contribute most to our general topic.

In its first relevant meaning, perception may be described as a more or less immediate response to a physical stimulus; the response is said to be correct if when verbalized it accurately describes the physical stimulus. It is perception in this sense which is investigated as a branch of experimental individual

¹H. V. Jaffa, "The Case Against Political Theory," Journal of Politics, 22, 1960, 259-275; quote from p. 267. The reference to David Easton's book relates, of course, to The Political System: An Inquiry into the State of Political Science (New York: Knopf, 1953); the argument in this passage, as we shall see below, belongs to Professor Jaffa. Short quotations and references to the views of Professor Jaffa in the following discussion all relate to the passage quoted above, unless they are accompanied by a citation.

psychology. Also, this notion of perception is very close to what we ordinarily mean by 'seeing.' To say that values influence perception in this sense requires a great deal of qualification. As we said when we discussed this matter in Chapter Three, and as our everyday experience constantly demonstrates, our perceptions are ordinarily correct. Moreover, in most of the experimental investigations of perception, psychological variables which would correspond to "values" are not considered relevant. Sometimes they are, as when the psychologist speaks of attitudes and motives which influence a subject's "span of attention."¹ All this is only to urge that in an important sense of perception, a sense which is related to observation in science, it is misleading to speak, without qualification, of the "determining influence" of values.

Much was said in Chapter Three which is relevant to our present topic. It may be recalled that one of the criteria of what was referred to there as "unproblematic evidence" is that it be the result of careful observation of simple facts.² I used as an example the observation

¹In Chapter Three we cited C. E. Osgood, Method and Theory in Experimental Psychology (New York: Oxford University Press, 1953), pp. 191-297. For a more condensed account see, for example, E. G. Boring, H. S. Langfeld, and H. P. Weld, Foundations of Psychology (New York: John Wiley & Sons, 1948), Chap. 10.

²Unproblematic evidence was discussed at pp. 81-82.

of the simple fact stated by the sentence 'S said he voted for R.' Now what I, in effect, maintained then, and what I maintain now is that for the observation of such simple facts, it is not only in principle possible to eliminate any effects of the observer's values, but in practice it is usually accomplished with little difficulty. This is not to say that one's perception of even such a simple fact could not be distorted by his values. Another important point to notice, however, is that such mistakes can be detected.¹ As we shall see, Professor Jaffa's position involves a denial of these contentions.

In the consideration of evidence in Chapter Three we took note of the fact that in most of their writing political scientists do not proceed from evidence gathered by the direct observation of simple facts, and that "for most of the phenomena they study it would be very difficult and often practically impossible to obtain data" of this kind.² And I suggested that these circumstances increase the chance that their values

¹We even have some rough knowledge of the conditions under which such errors are more likely to occur; knowledge which is used, for example, in the selection and training of interviewers.

²Above p. 83 . Though such an observation needs no support, we have given it at several places -- for example, in Chapters Three and Five we mentioned practical barriers to the direct observation of political phenomena, and our discussion of group level inquiry in Chapter Four pointed to large areas of study where the above observation is clearly appropriate.

may influence their "statement of the evidence."¹ This is not quite the same as Professor Jaffa's claim that "criteria of relevance" and "the ways in which we select our data" are determined by values, but our views have something in common. How does this relate to the matter of perception?

The second meaning of 'perception' which I think may contribute to the clarification of the matter under discussion, is that which often appears in social psychology. One textbook defines the term as follows: "Perception refers to the activity of sensing, interpreting, and appreciating objects both physical and social." The author further explains that interpreting as used in this definition "involves past experience or meaning," that appreciating includes "appreciation or evaluation," and that appreciation "is really much the same as what W. I. Thomas called the 'definition of the situation.'"² It is obvious from this not atypical definition and its accompanying comments, that the concept of perception employed in social psychology is much broader than the concept used in general (or individual) psychology -- the first meaning of 'perception'

¹Above, p. 83.

²K. Young, Social Psychology (3rd ed.; New York: Appleton-Century-Crofts, Inc., 1956), p. 59.

considered above. Let us adopt subscripts to mark these distinct concepts. Perception₁ is close to "sensing" as used in the definition of perception₂. Neither concept of perception is the same as what is meant by 'observation' in science. As we said above, however, perception₁ is close to what may be called 'direct observation'; perception₂ is more like observation plus interpretation, inference, and the like. Since the latter are so frequently involved in a political scientist's description of his data, even his most basic data, one might speak of observation in political science as perception₂. It is not unusual for "values" to influence perceptions₂. Perhaps this approximates the meaning of perception used by Professor Jaffa (notice his reference to "social psychologists") and others who emphasize the significance of the influence of values upon perception in discussing objectivity in political science. Let us consider the matter in these terms.

We are speaking, then, more broadly of the influence of values upon political inquiry -- upon the selection and interpretation of data as well as its observation. In this context, one does not have to deny such influences in order to defend the possibility of objective political inquiry; we could even accept Professor Jaffa's vigorous assertion of the "determining

influence" of values (I would not, but this makes no difference for the present argument). With respect to the second part of the idea of objectivity distinguished above there is no doubt that values may have a distorting influence upon research -- that they may lead to error. But what anti-positivists like Professor Jaffa never seem to recognize is that the influence of values upon research need not always be adverse. As Professor Jaffa says, "Men with one set of values are acutely sensitized to one set of relationships," what I wish to suggest is that these relationships may not only be present in a domain of research but they may constitute theoretically significant factors -- i.e., the influence of values may be advantageous. Actually, to speak of either adverse or beneficial effects of a researcher's values implies the objectivist position, for the determination in either case requires that we be able to determine the objective truth or falsity of the conclusions of research. We will see in a moment that Professor Jaffa's position involves both the denial and the affirmation of this conclusion.

In the passage quoted, Professor Jaffa states that "values have a determining influence upon everything human beings do," and he spells this out for research activities of political scientists; earlier in the **same** passage he asserts

that "values" are an integral part of all social causation, and that the effects of any such "cause" as industrialization are utterly unpredictable apart from the "values" impelling those who are the agents in the process of industrialization.¹

Such assertions obviously entail that the influences of values upon behavior can be objectively determined. But what are we to think of such claims as these in the light of his statement in the earlier quoted passage that

without having some way to judge the qualities of the lenses we use to look out upon the social or political universe, we have no way of knowing which of the unlimited number of possible universes is the universe [?]

He cannot consistently assert such broad factual claims about human behavior or "social reality" and still maintain that it is impossible to acquire objective knowledge -- that is, he must give up his position in order to maintain it. This one cannot do, with or without "value-lenses."²

As the first passage quoted from Jaffa's article indicated, on many points concerning the influence of values upon political inquiry his own position agrees with the views of David Easton. However, Easton does

¹Op. cit., p. 264.

²For further discussion of the self-contradictory nature of denials of objectivity which are based upon the influence of values upon social research, see: Gibson, op. cit., Chap. 7; and Nagel, op. cit., pp. 498-502.

not deny the possibility of objective political knowledge. His book, which Jaffa refers to, is intended as a contribution to the development of a very general scientific theory of politics -- the kind of political theory which Jaffa argues is impossible.¹ As we have seen, the views Jaffa shares with Easton are an important part of his argument. This reflects, I believe, Easton's own exaggerated view of the significance of values in political inquiry. I will have occasion to mention the basis for this opinion later in this chapter.

b. Similar to the thesis that values influence perception is the thesis that value judgments are required for the recognition of political facts. And both theses have been used in arguments against the positivist position in political science. We just discussed the former; we will now consider two illustrations of the latter.

First, we will briefly consider such an argument by J. H. Hallowell. Thus he wrote:

The recognition of facts requires not only sensory awareness but judgments as to value and significance. As a matter of fact, it is only by fitting the data made available to him by his senses into some pre-formulated conceptual scheme that the individual is able to

¹Op. cit., p. 260 et passim.

perceive facts at all. Actually, then, when the positivist insists that to be properly scientific we must confine ourselves to a description of "positive facts" that can be observed without transcending our immediate sensory experience, he is insisting upon the impossible.¹

This serves to illustrate an instance of the claim that the recognition of facts requires value judgments, and that this circumstance is evidence against the positivist position; it serves little else here, for I must confess that I fail to comprehend the argument. If it means that one must approach the task of factual description with some prior idea of what he wishes to describe, that some focusing of attention or "set" is present when people observe facts, or that the facts included in scientific descriptions are not merely reports of our sensory experience, then I can only say that I know of no political scientist who disagrees. Professor Hallowell indicates that many political scientists have adopted the positivist position which he is criticizing,² but he does not mention any who deny what seems to be the import of the last sentence of the paragraph quoted.³ This does not establish that

¹"Politics and Ethics," p. 647.

²Ibid., pp. 639-641.

³As we mentioned earlier, this paper by Hallowell was part of a controversy with W. F. Whyte. In Whyte's response to Hallowell's charge against positivists quoted above, he wrote: "I have yet to meet a social scientist who holds the position which Dr. Hallowell is here attacking." Op. cit., p. 303.

he could not have illustrated his charge; I leave the matter open.

As to the claim that the recognition of facts requires value judgments, I fail to see how it receives any support from either the need for a "preformulated conceptual scheme" or the fact that description involves the "transcending [of] our immediate sensory experience." (I have translated 'transcending' in this context to mean that we add something to our sensory experience.) Does every "conceptual scheme" contain (imply or require) value judgments? Does the "transcending" of sensory experience in observing political facts always involve value judgments? Must one make a value judgment in order to recognize the fact that a legislator voted against a bill? I find no compelling argument for a positive answer to any of these questions in Professor Hallowell's article. I am not even sure I find any argument for them, but this may very well be only a confession of the limits of my own powers of comprehension.

For what it is worth, we might stay long enough to observe that if this broad argument holds for the involvement of value judgments in political inquiry, it evidently applies as well to inquiry in all of the sciences. (As we shall see when we refer again to this paper later on, Professor Hallowell is well aware of the inclusiveness of his position in this regard.) It is safe to say then

that by itself the broad view that the recognition of facts requires value judgments does not impugn the possibility of objective political inquiry. No argument which is equally applicable to both physics and political science could do that.

Professor Leo Strauss has also argued that the recognition of political facts, at least all important ones, requires value judgments. His argument is however more circumscribed, more particularly applicable to political facts. Its consequences are, therefore, not so readily escapable. On the other hand, its basis is -- in my estimation -- more readily comprehensible; however, it is not readily comprehensible. Thus, as part of an attack leveled against "social science positivism," Professor Strauss wrote:

It is impossible to study social phenomena, i.e., all important social phenomena, without making value judgments. ... A man who refuses to distinguish between great statesmen, mediocrities, and insane imposters may be a good bibliographer; he cannot say anything relevant about politics and political history.¹

The idea is that to recognize and describe these distinctions, these aspects of political phenomena, requires value judgments; since "scientific [positivistic] social science is incompetent to pronounce value

¹Op. cit., p. 21.

judgments, and must avoid value judgments altogether,"¹
it is precluded from taking account of these facts.

This argument involves a not too subtle fusion of fact and value; once this is demonstrated, and a way in which this fusion may be avoided is pointed out, the argument loses its force. The main point in the required analysis concerns two familiar features of some of the words used in the description of political things -- they are vague and they carry value associations or moral overtones. The terms used in Strauss's illustrations, 'great statesmen,' 'mediocrities' and 'insane imposters,' are obvious examples. In ordinary usage the word 'statesman,' let alone 'great statesman,' has no determinate empirical meaning, but it does have a clear moral meaning or connotation -- a statesman, whatever that may be, is good. Under these circumstances, to describe someone as a "statesman" involves a value judgment; and, in as much as this term does have some empirical import, such a description

¹Ibid., p. 18. This characterization of the positivist position, as I understand that position, is correct; the last part of it, however, must not be taken to mean that a positivistic social scientist is somehow prohibited from making value judgments which are not an integral part of his research; such an interpretation would be patently untrue.

involves a fusion of fact and value.¹

On these grounds, one might argue, as Professor Strauss does, that the distinction between statesmen and non-statesmen (such as "insane imposters") could not be made by anyone who refused to make value judgments; that the recognition and description of such political phenomena requires value judgments. But it is, I believe, apparent that any semblance of validity such an argument may have is derived from a sociological fact about language -- that certain words have acquired a customary evaluational significance. In any event, the positivistic political scientist can escape any of the real or imagined difficulties portrayed in this argument by either avoiding what F. S. Cohen called "value-freighted" language,² or providing adequate empirical definitions for such terms. Once one has so specified the meaning of even such terms as 'great statesmen,' 'mediocrities,' and 'insane imposters' as he is using them, he may then proceed to employ them in the description of political leaders without making

¹In a suggestive article, F. S. Cohen describes a technique of "semantic analysis" as a means of uncovering "the inarticulate value premises of others and even of ourselves." The basic idea is that people reveal their values through their choices of "eulogistic, in-between, and dyslogistic terms" under various circumstances. One triad of such terms he includes in a list of them is: "Statesman, Policy-maker, Politician." "The Reconstruction of Hidden Value Judgments: Word Choices as Value Indicators," in Symbols and Values: An Initial Study, eds., L. Bryson, et. al. (New York: Harper, 1954), pp. 545-561.

²Ibid., p. 557.

any value judgments. The fact that others may use such language to express their moral sentiments is systematically irrelevant.

Logically, then, the solution of the problems caused by value-freighted terminology is not difficult; one only need have a firm grasp of the principles of scientific concept formation. Practically, especially for political scientists, such problems are not so easily overcome. In brief, the difficulties in political science are compounded by the fact that not only are concepts used which are value-freighted in everyday usage, but some political scientists use them in the same way; not only do anti-positivists such as Professor Strauss use such terminology, but they think of this as a virtue, and criticize those who would avoid such usage.

2. On the opening page of Chapter Five, we noted W. Y. Elliott's reference to a statement by Levy-Bruhl that a subject "cannot be a science in so far as it is normative." In view of our discussion of values and the minimal objectivity required of any science, I would surely agree. However, I hasten to add that what is intended by saying that a subject is normative is not always clear. As we tried to show in the first section of this chapter, the study of normative things

in the sense of value-facts does not entail that a subject, such as political science, is normative in any sense which detracts from its scientific status. In this section we are concerned with investigating the extent to which political inquiry is "normative." Anti-positivist writers, and some others, are prone to assert that political inquiry is thoroughly value-laden. So far we have discussed the matter with respect to the most basic aspect of research -- observation. Now we shall consider the broad general claim and some of the specific arguments related to it.

a. First, we will quote three political scientists who make the general claim for value involvement. This will introduce the specific positions regarding the significance of values in political inquiry and provide a background for their subsequent examination. As we saw as part of an earlier quotation, H. V. Jaffa wrote:

Values have a determining influence upon everything human beings do and political scientists are human beings. Research interests are determined by values; criteria of relevance are so determined; the ways in which we select our data are so determined.¹

Dwight Waldo asserted the broad significance of values

¹Op. cit., p. 267, italics mine. We omit here Jaffa's claim concerning the value determination of perception which we focused upon earlier.

in research as part of a criticism of Logical Positivism.

Thus he stated:

The first methodological criticism of logical positivism is that ... it tends to obscure the inevitable role of values in selecting, guiding and interpreting empirical research.¹

J. H. Hallowell stated it this way:

The physical scientist, whom the positivist urges the social scientist to emulate, is engaged in making value judgments daily: when he decides to investigate one problem rather than others; when he decides to use one kind of apparatus for experimental purposes rather than other kinds; when he uses various kinds of standards such as mathematical accuracy, skill in handling equipment, or logical consistency, to test his own conclusions or those of others; when he decides to reject a hypothesis or to formulate a new one; when, as a matter of fact, he chooses the scientific method itself as the most appropriate for his investigation and strives to adhere to its principles.²

My intention in the pages which follow is not to argue against the proposition that values influence inquiry. No doubt such influences occur in both the natural and the social sciences, and they occur more

¹"'Values' in the Political Science Curriculum," in Approaches to the Study of Politics, ed. R. Young (Evanston: Northwestern University Press, 1958), pp. 96-111, at p. 105, italics mine.

²"Politics and Ethics," p. 647.

frequently in the latter than the former. I will not undertake to estimate the extent of such influences; my interest here is, as it has been in this chapter, in determining the significance of the involvement of values in political science with regard to the question of the possibility of objective scientific inquiry in this field. However, with respect to such broad claims for the presence of values in inquiry as those illustrated in the above quotations, I wish to raise several considerations which will -- if I am not mistaken -- show that they are misleading exaggerations.

Statements concerning the influences of values and the presence of value judgments in any science might be asserted as hypotheses of the psychology and sociology of science. Let us notice, first, that the sweeping claims represented by the illustrations given above are not stated in the manner of empirical hypotheses. They are stated much too categorically for that; the use of such expressions as "inevitable" and "determined by" with respect to such diverse and complex matters is not at all fitting for statements belonging to such underdeveloped fields of study. In a similar way, David Easton asserts that "our propositions [in research] invariably flow from some moral purpose,"¹ and he refers

¹Op. cit., p. 224, italics mine. This matter is discussed below.

to the "inextricable relation of facts and values."¹
 This language is more suggestive of the assertion of methodological principles than it is of the statement of empirical hypotheses; the statements are more like the results of logical analysis than empirical inquiry. This is only a suggestion, I do not intend to decide upon the unclear methodological status of the claims under discussion.

Dwight Waldo, for example, does appeal indirectly to evidence:

I regard it as adequately demonstrated by empirical inquiry (for example, by the sociology of knowledge) that human values guide the selection of matters for research and the interpretation of the results in "social science" (and many would say in physical science as well).²

But this, too, is clearly an overstatement. It has not been "adequately demonstrated" by any scientific inquiry that values, in the moral or ethical sense of the term, always (or even usually) influence the selection and interpretation of research. Surely, there has been no such demonstration by scientific sociology of knowledge.³
 One can find some specious support for this view in what

¹Ibid., p. 228, italics mine.

²Op. cit., p. 105

³For a general account of the present state of scientific sociology of knowledge, see R. K. Merton, Social Theory and Social Structure (rev. ed.; Glencoe: Free Press, 1957), Chap. 12.

may be called philosophical sociology of knowledge, which is, by the way, the sense in which the expression 'sociology of knowledge' has most frequently been used. I refer to that interesting mixture of philosophy and speculative social science which is most often connected with the name of Karl Mannheim. There is evidence in Waldo's article that this is the variant of sociology of knowledge to which he was referring.¹

There is one side issue which our quotation from Waldo introduces which I feel constrained to comment upon before proceeding. In the sentence quoted, Waldo criticizes the positivist view of values (specifically he criticizes Logical Positivism) for obscuring the influence of values upon empirical inquiry. He spelled out the charge as follows:

The logical distinction between a question of fact and a question of value quickly becomes not simply a useful methodological tool but a fetish which operates to obscure the many close and "organic" relationships between fact and value "in fact," i.e., in empirical reality.²

I find this criticism more than a little gratuitous.

¹Thus Waldo stated, "the so-called sociology of knowledge has 'unmasked the unmaskers' and by revealing the socio-economic roots and historical relativity of all knowledge has cast a shadow upon even the most impressive claims to objectivity and universality. 'Objective' science is in this view hardly above 'self-evident' truths." Op. cit., p. 97. This is the language of Mannheimian sociology of knowledge. For a demonstration of its logically contradictory nature, see Nagel, op. cit., pp. 498-502.

²Ibid., p. 105.

How else than through a firm insistence upon the distinction of fact and value could one hope to discover value influences upon factual research? And, I might add, how could one ever hope to avoid the possibly adverse effects of such influences, except by clearly understanding the distinction? Raising these questions does not refute Waldo's charge. But in light of the confusion invited by a failure to maintain the fact-value distinction, his emphasis upon confusion which quickly results from the distinction seems a little out of place.

Returning to the matter under discussion, I wish to make a few comments upon the broad claims regarding the involvement of values in inquiry as stated by Jaffa and Hallowell in the passages quoted above. If there is competent evidence that values sometimes influence the process of inquiry, there is surely no such support for the radical deterministic thesis asserted by Jaffa. Even to attempt to maintain it involves such a widening of the application of the term 'value' as to deprive it of any determinate meaning. But this comment presupposes that we may correctly interpret Jaffa's claim as an empirical thesis. There are grounds for this in the article. But as we saw in our discussion of the same article with respect to perception, its arguments entail a complete denial of the possibility of objective knowledge of human behavior. This renders the claims

regarding the deterministic influence of values wholly problematic.

Whether the broad thesis concerning the involvement of values in inquiry is stated in terms of the influence of values or the presence of value judgments, the writer probably intends to assert essentially the same position. However, influences are it seems to me a somewhat (but not a great deal) more difficult to detect. The real difference though is that empirical inquiry is more relevant to evaluating the claims when they are stated in terms of influences; the question concerning the presence of value judgments also has its empirical aspects, but it seems to me that in the form in which it is usually stated, its resolution turns upon wholly logical analysis. Thus Hallowell's claim (in the quotation above) concerning the ubiquity of value judgments is clearly based upon a failure to recognize (or a rejection of) both what we distinguished in the last chapter as the use of value terms in a non-value sense, and the distinction we made between factual means-end statements (or judgments) and genuine value statements.(or judgments).

This is evident enough in the passage quoted. Is it reasonable to interpret, for example, a physicist's choice of experimental apparatus as a moral judgment? Surely, in at least almost every case, this choice must

be based on a judgment of the appropriateness of the apparatus for certain experimental purposes, as whether it would measure certain things accurately -- i.e., it is based on a means-end consideration. However, both the disregard of the factual nature of means-end propositions and of the non-value senses of value terms, and also the tendency to find values everywhere is revealed in another paper by Hallowell, which we cited earlier.

In a debate with W. F. Whyte in which Hallowell was defending his "contention that the description and analysis of political behavior necessarily involves ... ethical evaluation," he wrote:

Now I confess that I do not understand what it means "to analyze political behavior in terms of the social structure of our society" if it does not mean to analyze political behavior in terms of what is good and bad for the human beings who compose our society. Professor Whyte is perturbed by the fact that I have not stated clearly enough for him whether I regard "the methods of field research" he proposes as "good or bad." He sees clearly the necessity of distinguishing between good and bad methods of research and clearly thinks that his methods are good. But if the categories of good and bad are essential to the choice of research techniques, are they not of equal validity in evaluating the fruits of research?¹

¹"Politics and Ethics: A Rejoinder to William F. Whyte," p. 308.

The answer to Hallowell's question is obvious enough. In most contexts, as in this one, the application of the terms 'good' and 'bad' to research techniques does not relate to moral evaluation, which is Hallowell's concern; they relate to such things as the reliability of the results achieved by use of the techniques; whether the techniques are efficacious in this and other ways is a matter for factual determination.

b. In the above discussion I tried to show that the broad thesis concerning the involvement of values in inquiry as maintained by some political scientists, most prominently by anti-positivists, is an exaggeration, and I attempted to point out some of the shortcomings of the reasoning associated with such views. This does not mean that values do not affect, say, political inquiry. Our argument has always been that such influences cause practical difficulties for scientific inquiry in political science, but they do not raise logical barriers against such inquiry; that objective inquiry in political science is not precluded by its involvement with values. The remaining discussion relates to issues surrounding the influence of values upon the selection of subjects for investigation.

Probably the most frequently mentioned way in which values are said to influence inquiry is in the initial selection of a topic. Out of the vast number of possible topics for research a political scientist selects those which, for one reason or another, interest him. For the purposes of the present discussion, let us agree with those who would claim that these interests always reflect his own or his society's values. Whether or not this is always true, it surely is true in many cases. That is, this is one of the several senses in which political science is not "value-free."

Political scientists who investigate the causes of war, for example, or the conditions associated with the development of "security-communities," are probably motivated by the moral purpose of contributing to the development of a peaceful world order. But why should such a moral purpose of political scientists have any more significance for the logic of political inquiry than, say, the moral purpose of a biologist seeking the cause of cancer has for the logic of his investigations. More generally,

there is no difference between any of the sciences with respect to the fact that the interests of the scientist determine what he selects for investigation. But this fact, by itself, represents no obstacle to the successful pursuit of objectively controlled inquiry in any branch of study.¹

¹Nagel, op. cit., pp. 486-487.

Let us take note of one specific anti-positivist argument which seems to challenge the conclusion contained in the above quotation. Thus it has been asserted that a social scientist's values enter as an unavoidable subjective element of social science through his selection of questions for research.

Social science is said to be a body of true propositions about social phenomena. The propositions are answers to questions. What valid answers, objectively valid answers, are, may be determined by the rules or principals of logic. But the questions depend on one's direction of interest, and hence on one's values, i.e., on subjective principles. Now it is the direction of interests, and not logic, which supplies the fundamental concepts. It is therefore not possible to divorce from each other the subjective and objective elements of social science: the objective answers receive their meaning from the subjective questions.¹

But as I consider questions answered, or partially answered by political research, I cannot make sense of this claim. For example, if we ask under what conditions do two-party systems arise, and through research discover these conditions, how would our answer receive "meaning" from our question? Suppose we valued political democracy and we believed that it could only be maintained where there was a two-party system. This was the value basis

¹Strauss, op. cit., pp. 25-26.

for the question. Now we might say that the answer receives "meaning" as it is related to values. I think this is what is meant by the argument. Assuming it is, let us notice that this subjective element of social science does not interfere with the objectivity of social science.¹

Closely related to the idea that values influence the researcher's choice of a problem for investigation is the idea that values influence his selection of data. I only take note of this in order to make the point that regardless of the influences which operate upon a political scientist's initial guesses concerning the relevance of data, the correctness of these decisions is a matter for empirical determination. We should also note that unless research moves into entirely virgin territory, these initial guesses need not be wholly uninformed; where there is any related objective knowledge at all, one can at least eliminate some possible sources of evidence.

Finally, it is appropriate that we take note of a way in which the political scientist's exercise of choice concerning what to study renders political science non "value-free" in yet another sense. Dwight Waldo has

¹For one who has tried, as I have, to read very carefully such anti-positivist writings as those cited in this chapter, the problem of interpreting their use of the term 'meaning' becomes a formidable one. My own conclusion is that where the term appears problematical it should be interpreted as approximately: "significance for values."

pointed out that the idea that the findings of political research are ethically neutral in the sense that they can be used equally well to advance any values is, at best, only a half-truth. In so far as research is designed to discover certain means-end relationships, it is likely to be most useful for advancing the values embodied in these ends.¹ Of course, this is to some extent true of all other sciences. To this extent they, too are not "value-free." But it is, I believe, especially applicable to political science where research is so frequently directed to matters of rather immediate practical concern. It is probably unnecessary to say, that this sense in which political science is not "value-free," is not directly relevant to the sense in which I maintain that political science should be "value-free."² However, it does, given certain values which most of us share, impose upon the political scientist qua political scientist a definite moral responsibility.

¹Waldo, D., Op. cit., pp. 105-106.

²The factual nature of this ought-statement is apparent once the implicit clause 'if they wish to acquire reliable knowledge' is appended to it. If anyone said I have also asserted a value judgment, in this case, I would not argue.

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