A CRITIQUE OF SOME ASSUMPTIONS APPARENTLY UNDERLYING SOCIAL SCIENTIFIC ANALYSIS, WITH PARTICULAR REFERENCE TO THE FIELD OF CRIMINAL JUSTICE

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ABSTRACT

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by Wayne B. Hanewicz

The relative success of experimental research in the physical and natural sciences seems to have stimulated similar methodological endeavor in the social sciences. That is, there is an attempt to utilize those methods which have been, and are now being, utilized in the former academic disciplines. There remain, however, a number of questions concerning the nature of these methods, which questions appear to be even more complex within the context of social phenomenon. Indeed, some relatively recent experimental results appear to lend credence to philosophical doctrines, both contemporary and non-contemporary, which suggest that some of these methods may have limited utility in reaching a thorough understanding of a social reality apart from the one in which the researcher finds himself.

Closely related to these basic issues, and a major theme of this thesis, is the question of concept formulation. Generally speaking, it is suggested that concepts are human constructions which signify a perception of some aspect of social reality. However, since the theory of

social reality presented here is that of a totally interrelated or "wholistic" one, these constructions may be rather arbitrarily delimited extractions; that is, the delimitation could have taken place at some other points within the sphere of social reality. Consequently, although these concepts may signify certain phenomena, it may be difficult or impossible to ascertain how accurately they represent such phenomena.

Since the general field of criminal justice is a relative newcomer to the arena of "scientific" methodology, it may be productive at this early, malleable stage to re-examine these methods and the nature of the concepts involved in their utilization. It may well be that the very concept of a "field" of "criminal justice" should undergo significant modification. If, after sufficient analysis of these issues, one finds that some of these methods will not lead to an "understanding" of an "event" or a "reality," then their continued utilization in their present form may be brought into question. This, in effect, is what has herein been attempted.

Approved: Raymond G Galvin

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By

Wayne B. Hanewicz

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DEDICATION

Dedicated to Sitoo and Jidoo, and to Amey D. F. Gamf, some of whom, in their own way and to varying degree, have had a more profound effect upon my life than they may realize.

Wayne B. Hanewicz

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CHAPTER I

GENERAL INTRODUCTORY AND BACKGROUND INFORMATION

Introduction

There is an increasing tendency on the part of both scholars and practitioners in the field of police administration to attempt to undertake "scientific studies" of phenomena or relationships which appear to fall within the confines of that field. The necessity for "objective" analysis in such studies is of overriding concern, and is equaled only by the demand for "value-free judgments." For the most part, the general feeling seems to be that there is "something out there" which can be "discovered" if the correct methodological guidelines are followed.

The application of a "scientific" method to the analysis of some phenomenon is a rather complex undertaking; the use of mathematical and other types of "purely scientific" principles is not without procedural and substantive problems. The issue is made considerably more complex when such study is directed toward the analysis of "social" phenomena, of which "police" phenomena are a part. It appears as though certain basic assumptions have been made regarding human thought processes and concept formulation, both of which provide the framework for any methodological

inquiry, which are subject to considerable question. One of the major purposes of this thesis is to once again raise these questions.

Due to a relative lack of "scientific" research in the field of police administration, it appears that students of this academic discipline have not yet been dramatically confronted with these issues in the same way that physicists or biologists have. However, with the trend toward the development of a comparative method, these questions may again acquire major proportions. Although comparative method per se is not the major object of this discussion, nevertheless, because of the relationship it may have with the questions at issue, a rather short review of discernible trends in this area may be useful as a means of clarification.

A Short Review of Comparative Method in the Social Sciences

The development of, or, more correctly, the idea to develop, a comparative method for utilization within the context of cross-national and/or cross-cultural analysis of police systems is a most recent phenomenon. Indeed, with regard to analysis of police systems per se (even in a very broad sense), one finds oneself in a most virgin area of academic endeavor. While most students of police administration are, at least, not unfamiliar with the term "comparative," the term "methodology" does not appear

to enjoy equal notoriety; and when one combines both terms, one finds the combination enjoying even less notoriety. Whether this situation be the result of a lack of interest, a lack of time due to the pressing domestic problems in the area of police administration, or, what is more likely, a combination of both, is not of prime interest to this writer; the resulting situation itself, however, is deserving of discussion.

As of this time, there does not appear to be any "comparative method" to speak of in the area of police administration, whether it be in regard to inter- or intra-national and/or cross-cultural studies; notwithstanding the fact that there appears to be some indication of a growing interest in the development of a "comparative method" per se to deal with problems of cross-national and cross-cultural studies in police administration.[1] For the most part, however, comparative studies in the area of police administration have been descriptive and/or historical more than conceptual, with the respective authors undertaking an analysis, sometimes cursory and sometimes "in-depth," of some particular police system or, more rarely, of a number of police systems. Among the

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[1]See, for example, Raymond T. Galvin, The Study of Comparative Police Administration, A paper presented at the American Society of Criminology Meeting, Berkeley, California, December, 1965.

descriptive works of other police systems, Great Britain has probably enjoyed the most popularity as a topic country. Hewitt's work provides a general description of police administration within that country, although it probably could be more useful as a reference book for certain statistical, and legal and constitutional information.[2] In addition, of course, some indigenous literature is also available and can provide descriptive and/or historical information for a respective police system.[3] Robert Foran's book provides an interesting historical analysis of the Kenya police[4] as does Wood's for the police of Natal.[5] Early in this century, Fosdick presented a kind of comparative analysis of European police systems,[6] but it was not what could be called an example

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[2]William H. Hewitt, British Police Administration (Springfield: Charles C. Thomas, 1965).

[3]See, for example, John Coatman, Police (London: Oxford University Press, 1959), or, G. T. O'Brien, The Australian Police Forces (Melbourne: Oxford University Press, 1960). There are also a number of books which are available in foreign languages, Germany being fairly prolific in this area.

[4]Robert W. Foran, The Kenya Police 1887-1960 (London: Rovert Hale Lmtd., 1962).

[5]Arthur A. Wood, Natal, Past and Present (Devon, England: Stockwell, 1962).

[6]Raymond B. Fosdick, European Police Systems (New York: The Century Co., 1915).

of comparative methodology. Finally, and more recently, Cramer's work provides a simultaneous description of a number of police systems throughout the world,[7] but, again, this is not looked upon as a development of a comparative method.*

If one wishes to view a police system as encompassing more than purely "administrative" concepts, one expands the literary base from which to draw relevant material. For example, the area of comparative criminal law has been, and continues to be, a rather popular subject among legal scholars. Further, this closely related academic discipline seems to offer more in terms of a comparative method than the traditional police literature. That is, there appears to be some progress in formulating concepts for the purpose of cross-national and/or cross-cultural study; the very enlightening series of articles which appeared in the Journal of Criminal Law,

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[7]James Cramer, The World's Police (London: Cassel and Co., Ltd., 1964). Although Cramer does devote a few of the initial chapters to a discussion of the historical development of policing in a few different countries, it does not appear to be an attempt to develop any kind of method which would allow for further comparative analysis along the same lines which he follows.

*For point of clarification, the comparative method as used here suggests, among other things, the construction or formulation of concepts which are sufficiently flexible to allow for their use, in a productive manner, in cross-national and/or cross-cultural study.

Criminology, and Police Science[8] is indicative of this change in comparative perspective. Anthropology and law have combined to produce some very productive material in this respect, although it must be admitted that anthropologists have been developing a comparative method for at least two decades now. Llewellyn and Hoebel's piece on social control among the Cheyenne[9] must certainly be viewed as a forerunner to a comparative method. Similarly, M. Fortes and Evans-Pritchard's African Policital Systems[10] provided firm groundwork for the development of a comparative method, as did Gluckman's Politics, Law, and Ritual in Tribal Society.[11] More recently, Marc Swartz, et al., have developed a new conceptual framework for the comparative analysis of political systems, both "primitive" and "developed."[12]

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[8]The Journal of Criminal Law, Criminology, and Police Science, 51:2, July-August, 1960; 51:4, November-December, 1960; 51:1, May-June, 1961.

[9]K. N. Llewellyn and E. A. Hoebel, The Cheyenne Way (Norman, Oklahoma: University of Oklahoma Press, 1941).

[10]M. Fortes and Evans-Pritchard (eds.), African Political Systems (London: Oxford University Press, 1940).

[11]Max Gluckman, Politics, Law, and Ritual in Tribal Society (Chicago: Aldine Publishing Co., 1965).

[12]Marc J. Swartz, et al., Political Anthropology (Chicago: Aldine Publishing Co., 1966).

Trends in the General Field of Political Science

The developments in anthropology have coincided with, have indeed influenced, similar methodological developments in the other social sciences in general and political science in particular. Because of the obviously close relationship between political science and public administration (of which police administration is viewed as a part), developments or trends in these fields have had, or should have in the future, significant repercussions with respect to similar developments in police administration. Consequently, a brief sketch of these developments with regard to comparative methodology may lend more coherence to the later discussion of developing a comparative method for analysis of police systems.

When one turns to policital science and public administration (insofar as they can, in fact, be so neatly separated), one begins to discern the development of some tangible comparative method that appears to have particular relevance to students of comparative police administration. There is a great deal of interest on the part of political scientists and public administrators to develop a comparative method which will provide working concepts that will produce empirical data for the testing of hypotheses and discovering of "general principles" of administration. Indicative of this trend is the following statement made by Robert Dahl as early as 1947.

> No science of public administration is possible unless . . . there is a body of comparative studies from which it may be possible to discover principles and generalities that transcend national boundaries and peculiar historical experience.[13]

Similarly, the need for a comparative method to supplant the traditional approaches to the study of political and administrative systems was suggested by Dwight Waldo in the following statement:

> The case against "traditional" study of comparative government is put as follows: that it was culture-bound, limiting itself for the most part to countries of Western Europe or to areas affected by Western political institutions; that it was legallistic and formalistic, limiting itself to examination of documents (often of the formal, static aspect of governmental institutions, unduly neglecting informal arrangements and indeed the whole political-social-economic-cultural context of laws and formal institutions); that it was preponderantly descriptive rather than problem-solving, explanatory or analytic; that it failed to probe far enough to discover functional equivalence . . . ; and that it was not genuinely comparative--i.e., that its basic descriptive categories were inadequate and confusing, that most students dealt with only one country (or at best proceeded country-by-country), and that there were really no concepts or techniques for determining either similarity or difference, especially if study moved beyond a strict Western orbit.[14]

As is suggested by the above statement by Waldo, political scientists and public administrators have long

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[13]Robert A. Dahl, "The Science of Public Administration," Public Administration Review, Winter, 1947, p. 11.

[14]Dwight Waldo, Political Science in the United States of America; A Trend Report (Paris: United Nation's Educational, Scientific, and Cultural Organization, 1956), p. 63.

recognized the limited usefulness of purely "descriptive" and/or "historical" studies in supplying those kinds of data which would provide a firm basis for comparative studies of the actual goings-on within political and/or public administrative systems. (It will be recalled at this point that this is predominantly the type of literature which is now circulating within the ranks of police administration students.) This traditional, preponderantly descriptive, approach ". . . did not lend itself to the development of theories and the testing of hypotheses and the compilation of significant data. It was limited to the description of the forms of government of foreign political systems."[15] The applicability of that criticism to contemporary methodology in the field of comparative police administration is all too plain!

The development of more flexible and meaningful concepts for comparative political studies was in evidence more than a decade ago. Marion J. Levy's The Structure of Society[16] attempted to uncover certain general characteristics common to all societies, thereby facilitating conceptual formulation around those characteristics for use

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[15]Roy C. Macridis and Bernard E. Brown, Comparative Politics: Notes and Readings (Homewood, Ill.: The Dorsey Press, 1964), p. 3.

[16]Marion J. Levy, Jr., The Structure of Society (Princeton: Princeton University Press, 1952).

in comparative analysis. Roy Macridis[17] and Gabriel Almond[18] produced their respective works, both of which are specifically devoted to the comparative study of political systems per se. Similarly, the "Industria-Agraria" model of Fred Riggs[19] should not be overlooked in this short historical, developmental sketch, for his model represents a significant attempt to formulate working concepts for comparative political studies. Very obviously the list is not very complete, nor can it be said to be very representative, but it does indicate that there has been, as there now is, a definite trend toward developing suitable methods for comparative analysis.

It should be emphasized that these developments have not been as distinct as this, rather obviously imcomplete, historical sketch may have suggested. "Anthropology," "political science," "public administration," "social science," "criminal justice," etc. are terms which represent some kind of intellectual specialization in the pursuit of certain kinds of knowledge; the real phenomena are not as distinct as their linguistic symbols suggest. On

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[17]Roy C. Macridis, The Study of Comparative Government (Garden City, New Jersey: Doubleday Short Studies in Political Science, 1955).

[18]Gabriel Almond, "Comparative Political Systems," Journal of Politics, August, 1956, pp. 391-409.

[19]Fred Riggs, "Agraria and Industria: Toward a Typology of Comparative Administration," in William Siffin (ed.), Toward the Comparative Study of Public Administration (Bloomington: Indiana University, 1957), pp. 23-116.

the contrary, the assumption of this writer is that there is a very significant interrelationship among developments in all of these fields, and between them and similar developments in the field of comparative police administration. In all probability, students of comparative police administration will draw heavily from experiences in these disciplines in attempting to construct a workable comparative method; in one sense, this heavy reliance is apparently a necessity, for the development of any truly comparative method has been, and will continue to be, the result of interdisciplinary cooperation. This apparent necessity, however, should not diminish the importance of the concomitant necessity to critically analyze the material which will be the object of this interdisciplinary borrowing.

The field of comparative police administration, then, is in its infancy, and, like its human counterpart, it is quite malleable. To the extent that students of comparative police administration draw material from students of other disciplines concerned with similar academic endeavor, to that extent, the former students must make an assumption that the work of the latter is basically valid and capable of being modified to meet the demands of police administrative analysis. Generally, this paper will be concerned with both these and some other basic assumptions that have been, or appear to be in the process of being, made with regard to the formulation of concepts and construction of

a method for social scientific analysis in general and police administration in particular.

Statement of the Problem

As the preceding discussion should suggest, the emphasis in the fields of political science and public administration, insofar as comparative studies are concerned, is on the formulation of concepts and subsequent development of a method sufficiently flexible to allow for cross-national and/or cross-cultural analysis. Moreover, as has also been suggested, developments in these academic disciplines will necessarily have significant repercussions for similar attempts in the field of police administration. In all of these and related fields the key words are "concepts" and "method." "It is not saying too much that ours has become the methodological century in the social sciences."[20] Indeed, the very terms "comparative government" and "comparative public administration," being defined as "lines of approach involving perspective, methodology, and data . . ."[21] (emphasis supplied), reflect a characteristic method which is inherent in their very definition.

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[20]Arnold Brecht, Political Theory: The Foundations of Twentieth-Century Political Thought (Princeton: Princeton University Press, 1959), p. 5.

[21]Macridis, op. cit., pp. 4-5.

If the development of a suitable method, in general, is a major objective of comparativists in the social sciences, particularly in the field of political science (in the very broad sense), then "scientific" is the prerequisite for acceptable participation in such a development. Everywhere the emphasis is on the "scientific approach" to the problems of comparative analysis in the social sciences, and if the assumption of a necessarily close relationship between police administration and political science is accepted, one can expect to hear similar appeals for a "scientific method" for comparative police analysis.

The general assumption seems to be that the social sciences can utilize those methods for analysis that have been developed, and are now being utilized, by biologists, chemists, mathematicians and physicists. The "scientific" method, in this sense, is supposed to allow for a thorough and "objective" analysis of nature. As Holten says, there is a generally ". . . accepted thema of the unlimited possibility of doing science, the belief that nature is inexhaustibly knowable."[22] Apparently, it is felt that if one follows such a "scientific" methodology, one can expect the same degree of "success" enjoyed by students of the "pure" sciences.

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[22]Gerald Holten, Science and Culture (Boston: Beacon Press, 1965), p. xxiv.

Although no attempt is made to mimic every detailed aspect of the method of the "pure" sciences, nevertheless, it is felt that general principles underlying this method can be validly used as basic premises for a comparative method in the social sciences. More specifically, it is not uncommon to hear one talk about the elimination of "value judgments" in undertaking comparative analysis in order that "objectivity" will result; the assumption being, of course, that "value judgments" can in fact be eliminated.[23] Another advantage commonly attached to the use of "scientific method" in the social sciences is that general hypotheses can be "empirically" tested and validated or invalidated, thereby allowing for appropriate modification of the hypotheses for future testing, and so on until theory adequately represents reality.[24] Symbolically, the ultimate objective would be to validly state some social relationship or phenomenon thus:

Given: A = Some social condition
B = Some "resulting" social condition
X_1 = Variable
X_2 = Variable
X_3, etc.

Then: A (provided X_1, X_2, etc.) = B.

The assumption here is, basically, twofold: (1) that social

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[23]Harold MacIver, The Pursuit of Happiness, A Philosophy for Modern Living (New York: 1955), pp. 157, 170, and 173.

[24]Macridis, op. cit., pp. 4-5.

phenomena are sufficiently distinct to be capable of being perceived as elements in a cause-effect relationship, and (2) that all the variables (X_n) involved can be isolated and measured to determine their significance with regard to the social phenomenon under study. This thesis will question these assumptions!

In general, then, there appears to be a tendency on the part of social scientists to draw heavily from the methodology of the "pure" sciences in order to develop a "scientific" method for use by the former academicians. Efforts in political science and, more specifically, public administration now reflect this predominant concern with "scientific" methodology and the formulation of a comparative methodology based upon the experiences in the "pure" sciences. "If science is essentially a matter of method, then a major value of comparative administration lies in its contribution to an increasingly scientific approach."[25]

What is the supposed objective of comparative method in general and particularly of utilizing a "scientific" method for undertaking these studies? Two have already been touched upon, namely, (1) the introduction of "objectivity" and the elimination, or reduction of the significance of, "value judgments," and (2) the gathering of "empirical" data for the testing and retesting of hypotheses.

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[25]Siffin, op. cit., p. 16.

In addition, however, it might be said another objective appears to be to facilitate the formulation of general principles which are applicable "across the board," regardless of national and/or cultural variation. In this respect, of course, comparative studies pose the problem of generality and the need for obtaining and assimilating data which have often been considered beyond the scope of traditional political science.[26] Generalization is recognized as necessary if for no other reason than the fact that one cannot experiment on all aspects of reality,[27] but there is a question as to how far and in respect to what kinds of social phenomena one can generalize.

To recapitulate, the basic problems with respect to the development of a comparative method for utilization in cross-national and cross-cultural police analysis, and the ones to which this writer will, directly or indirectly, devote the proceeding discussion, are these. First, and this is probably the most basic, to what extent can students in the social sciences draw from the methodological experience in the "pure" sciences? To what extent should such a borrowing take place? Second, to the extent that the

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[26] Ibid., p. 1.

[27] Henri Poincare, "Hypotheses in Physics," in Philip P. Wiener, Readings in the Philosophy of Science (New York : Charles Scribner's Sons, 1954), p. 33.

former can draw from the experiences of the latter, what assumptions must be made regarding the utilization of the "scientific method" even within the context of "pure" scientific research? Third, will any of these methods really supply a thorough or accurate understanding of social reality? Finally, what are the prospects for developing a truly useful comparative method for police or any kind of social analysis?

Generally speaking, discussion will proceed along these lines, although not necessarily in that general order. In addition, discussion will not necessarily be directed specifically to a given question, although the material brought out will have rather obvious relevance to all of them. Finally, the bulk of the thesis will be devoted to pointing up the interrelationships of a number of implications of these basic questions. In general, discussion will be centered around these questions rather than being directed at them.

Definitions

For reasons which will hopefully become clearer as the thesis progresses, with two exceptions, no attempt is made at "defining" most concepts utilized here. Since "defining" a given concept requires the utilization of a process of conceptual extraction (to be explained later in the thesis), which process is the object of a major portion of this thesis, it would be rather self-defeating to utilize the

process in formulating "definitions" prior to the discussion. In addition, those concepts which may require clarification, e.g., "conceptual extraction," "system of reality," etc., are objects of rather extended discussion within the body of the thesis. Concise definitions at this point would be more confusing than clafiying.

The two exceptions noted above are "pure sciences" and "field."

Pure Sciences: Those academic disciplines which may be represented by the terms "chemistry," "physics," mathematics," "biology," and other closely related terms (i.e., "biochemistry," "mathematical physics," etc).

Field: That grouping of phenomena or relationships which is seen as comprising a distinct area for academic or practical endeavor.

Literature Review

It is noted that there is no attempt to extensively review the literature of the "field." There are a number of reasons for this intentional inaction. First, one of the major issues raised in this thesis deals with the utility of the perpetuation of a "police field" as such. In effect, it is suggested that the "field" is confined by a rather arbitrary means of circumscription, and, in reality, a great deal of information concerning almost every aspect of human existence would be a significant contribution to the understanding of the relationships or

events grouped under a given "field." Consequently, if an intentional attempt is made to review all the literature in the "field," the thesis immediately becomes subject to the justifiable criticism of gross self-contradiction.

The second reason for the absence of a literature review of "the field" follows directly from the first. Given the writer's general train of thought and belief in the artificiality of a "field," it becomes difficult to discern a "field" at which the thesis is directed. Rather, the thesis is directed at the very concept of "field," and involves concepts which accrue from, and clarify the nature of, a number of "fields." Consequently, rather than an extensive literature review of a "field," relevant material was drawn from the literature of many, sometimes apparently "unrelated, fields."

In brief, the anticipated result is not a thesis which only discusses the issues related to the artificiality of "fields." Rather, an attempt was made to construct a thesis which both discusses and itself manifests the major theme of the significantly interrelated relevancy of many "fields."

Organization of the Remainder of the Thesis

Chapter II will be devoted to a discussion of research methodology as utilized in what have traditionally been labeled as the "pure" sciences, i.e., chemistry, physics, etc. Here the emphasis will be on exploring those aspects

of the method that seem to be so attractive to the social scientists who envisage the many advantages associated with it. For instance, there will be discussion of such topics as mathematical systems, the question of "certainty" in "scientific laws," and the relation of recent experiments in quantum physics to the possibility of "objectively" undertaking research.

Chapter III will begin a discussion of Method in general, and it will be particularly concerned with the questions of concept formulation, "objectivity," "value judgments," and with the issue of whether or not there are such things as "facts." Here, as throughout the thesis, an attempt will be made to relate the immediate discussion to the preceding one, for the conclusions will be based on a thorough understanding of the material already presented.

Continued discussion of isolated aspects of social scientific methodology is undertaken in Chapter IV. Emphasis will be placed on a concern for such things as the general theory of causation and the related theory of multiple-causation, the development of, and the rationale for, an interdisciplinary approach to the analysis of social phenomena, and a short discussion of the utility and problems associated with the use of models. Here again, attempts will be made to relate the discussion to the material presented up to that point.

Chapter V will be the concluding chapter of the thesis. Here the discussion will involve logical implications which may accrue from the material and conclusions presented in the preceding chapters; where productive, their relationship to the development of a comparative method will be likewise discussed. No attempt will be made to formulate such a method, for this would take at least another thesis. Rather, this chapter, as indeed the entire thesis, will be a critique of the method in general, with particular attention to those aspects of social scientific methodology which appear to raise the most significant questions with regard to the utilization of that, or any, method.

It is in this final chapter that the writer will most obviously put forth personal conclusions which he feels to be justified in view of the preceding chapters. It is to be emphasized that conclusions drawn from material dealing with the kinds of topics which are herein dealt with cannot, for the most part, be "proved" as a mathematical problem might be; indeed, the very concept of "proof" is indirectly discussed and questioned to some extent within the context of the thesis. Rather, the question is one of <u>justification</u> as opposed to "proof," and it is toward this end that references from philosophy, physics, and other "unrelated fields" are cited.

CHAPTER II

A DISCUSSION OF SOME OF THE ELEMENTS OF "SCIENTIFIC" METHOD

Since there is a general trend among social scientists to draw from the experiences of students of the "pure" sciences, it would seem appropriate to undertake a discussion of what seems to be implied by the term "science" in general, and the "scientific method" in particular. In addition, it is felt that it would be beneficial to discuss some recent advancements in the "pure" sciences and relate, at least in a cursory manner, the implications of these advancements to those elements of the "scientific method" which seem to be the most heavily relied upon by the social scientists. It should be noted that each of the areas to be discussed could easily be the sole topic of an entire thesis; given spatial and intellectual limitations, however, such an undertaking would be impractical. Consequently, discussion will center only around those elements which seem closely related to the general theme of the thesis.

Science

It would appear appropriate, indeed necessary, at this point to discuss some characteristics of the term "science," with particular emphasis on its relation to method. In fact, it is this writer's opinion, and an attempt will be made to show, that the term "science" implies the use of a particular method, although characteristics, goals, and techniques of this method vary, or ought to vary, from one particular "science" to another.

The term "science" has been attached to so many academic disciplines, particularly within the last fifty years, that one can no more equate it with a particular discipline as was the case during the nineteenth century and, to some extent, the twentieth. The traditionally "pure" (including natural) sciences, i.e., biology, chemistry, physics, and mathematics, no longer enjoy the same monopolistic use of the term as was once the case; for psychology, politics, sociology, law, and public administration, among others, have made it known that they too are now "sciences." What has happened within these disciplines that now makes them "sciences"?

The term "science" appears to imply, among other things, an inclination toward the formulation of general laws which can be used, primarily, for the prediction of future events. It is felt that if one is able to predict future events, one can possibly modify a given human

situation to the extent that that situation is related in some manner to the events liable to prediction.

> However we may define the word science in some philosophical or epistemological system, it is clear that it begins with the use of previous observation for the prediction of future events.[1]

Another purpose of these general laws, the ramifications of which will be gone into in more detail at a later time, appears to be that of tying together the various phenomena which have been extracted from their totality for purposes of analysis.[2] That is, they are supposed to provide some semblance of order to perceived phenomena in order that rational thought regarding those phenomena may be facilitated. Actually, these two general purposes are closely interrelated, if, indeed, not sometimes interdependent. For, in most cases, accurate prediction is best accomplished only after a thorough understanding of the interrelationships of the events involved; while in other cases, the process of prediction does aid in tying together conceptually distinct phenomena. In general, then, it can be said that:

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[1]Bronislaw Malinowski, "A Scientific Theory of Culture," in Philip P. Wiener (ed.), Readings in the Philosophy of Science (New York: Charles Scribner's Sons, 1954), p. 391.

[2]Albert Einstein, "The Method of Science," in Edward H. Madden (ed.), The Structure of Scientific Thought (Boston: Houghton Mifflin Co., 1960), p. 83.

> The function of science, in this sense of the word (broadest), is to establish general laws covering the behavior of empirical events or objects with which the science in question is concerned, and thereby to enable us to connect together our knowledge of the separately known events, and to make reliable prediction of events as yet unknown.[3]

At this point, it should be apparent that both the "pure" sciences and the "social" sciences share a common denominator in the formulation and utilization of general laws for predictive purposes and to facilitate comprehension of the interrelationships of events. There is yet another characteristic of a "science" which is shared, or certainly has the possibility of being shared, to some extent by all the disciplines, namely, experimentation, and more experimentation. The crucial issue with regard to experimentation revolves around the question of control, and notwithstanding the interdisciplinary use of experimentation in general, it is at this point, involving the question of control, that experimentation in the physical sciences and experimentation in the social sciences start to part company; for control suggests variables, and the social scientists have more than their share of variables. In the physical sciences, for the most part,* relatively

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[3]Richard Bevan Braithwaite, Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science (New York: Harper and Row, 1953), p. 1.

*Developments in quantum mechanics suggest that all the variables may not be subject to control in the sense of being liable to experimental manipulation.
This will be discussed at another point in this chapter.

rigid control of variables can be obtained; but it is another matter with experimentation in the field of human behavior. In contrasting experiments in the physical sciences with those in the social sciences, Brecht states:

> Yet rarely, if ever, is it possible to concoct such favorable experimental conditions in the social sciences. Even when external conditions can be arranged in line with strict postulates, many variables enter the experiment because of the human factor involved.[4]

The social sciences, of course, do not have a monopoly over this basic problem; the question appears to be one of degree. The effect of the variable differential on the relative degree of predictive reliability and amenability to operational manipulation can be seen in the following set of statements, which may be classified as "laws," generally speaking, in their respective fields.

Mathematical Statement: The hypotenuse of a right triangle is equal to the square root of the sum of the squares of the other two sides.

Bio-chemical Statement: If the brain of a human being is deprived of oxygen for 8-10 minutes, other bodily functions will cease shortly thereafter.

Social Scientific Statement: In times of administrative stress, the organization will tend toward an authoritarian type of leadership.

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[4]Arnold Brecht, Political Theory: The Foundations of Twentieth-Century Political Thought (Princeton: Princeton University Press, 1959), p. 94.

It becomes apparent that the mathematical statement expresses a relationship the outcome of which is certain, at least within the context of that mathematical system. The bio-chemical statement also expresses a predictible relationship between events, but there is an interjection of a small amount of ambiguity. For instance, what are the "bodily functions"? Although organs and cells may cease to function, atomic interaction will continue to take place. This atomic observation is not, of course, particularly significant; but it <u>is</u> significant that the relationships are not seen as clearly as was the case in the mathematical statement. The social scientific statement is even more ambiguous. Although the relationship itself may or may not be true, there is considerable problem in defining the terms so as to include all the variables that may affect the relationship which is being expressed. The variable issue can truly be seen as being one of degree <u>and</u> perspective.

As mentioned previously, all the sciences have at least one common denominator in the inclination toward, and the recognized necessity for, the formulation of general laws. In addition, experimentation may be another common denominator, although the problem of control does appear to create a significantly more complex problem for the social sciences than is generally the case with the other sciences.

In addition to these two general characteristics, there are some other features of the "pure" scientific method which, because of social scientific interest in them, deserve discussion. Reference is specifically made to the role of mathematics and mathematical systems and "laws" in scientific method. Although a discussion in this area may appear at first glance to be unnecessary or "unrelated," nevertheless, because such techniques appear to be the objects of some rather rampant social scientific larceny, some comments would appear to be in order. No attempt will be made to analyze their present role in the social sciences, for, in view of the literature available,[5] this would require at least another thesis if any kind of professional dignity is to be maintained. Rather, discussion will center around those characteristics of mathematical systems and laws which, in this writer's opinion, would appear to have rather significant repercussions for social scientific utilization.

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[5]See, for example, Paul F. Lazerfeld, Mathematical Thinking in the Social Sciences (Glencoe, Ill.: The Free Press, 1954); or Frank Harary, Robert Z. Norman, and Dorwin Cartwright, Structural Models: An Introduction to the Theory of Directed Graphs (New York: John Wiley and Sons, Inc., 1965); or Roy G. Francis, The Rhetoric of Science: A Methodological Discussion of the Two-by-Two Table (Minneapolis: University of Minnesota Press, 1961).

Mathematics and Mathematical Systems

> As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.
>
> Albert Einstein

As has been suggested, there is a great deal of interest on the part of social scientists in the possibility of utilizing mathematical techniques in social research. There appear to be, basically, three reasons for this interest:

1. The apparent success of utilizing mathematical techniques in natural scientific research appears to be a lure for many social scientists,
2. The prestige and charm of mathematical work is a temptation for many social practitioners,
3. There is a felt need for a more precise language among social scientists.[6]

No attempt is made to directly assess the merits of these "drawing cards." Discussion is centered around the general area of mathematics as utilized in the "scientific method" and the implications that accrue therefrom.

One of the most obvious characteristics of "pure" scientific methodology is the constant reliance upon mathematics or its basic principles in undertaking almost any

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[6]Lazerfeld, op. cit., p. 3.

type of experiment. Mathematical systems provide a kind of logical "absoluteness" within a given system that, generally, cannot be contradicted as long as the required manipulative techniques are carried out correctly. It is strictly a logical process. The terms, devoid of any inherent meaning, are manipulated according to an accepted "modus operandi" until the necessary result is obtained. The new Euclidean postulate in geometry is a good example; If A, B, and C are points on a straight line l, and if B lies between A and C, then B also lies between C and A. This is simple, logical, and absolutely certain within that system!

There is a conspicuous absence here of empirical content, the characteristic element with which sciences other than mathematics must deal. Indeed, "A mathematical truth is irrefutably certain just because it is devoid of factual, or empirical content."[7] When one introduces any degree of empirical content into the system by attaching some meaning to the symbols, the process being referred to as semantical interpretation, one should not thereby expect that the mathematical system will "prove" the factual "truth" of the empirical proposition under analysis, for this is not a direct function of the mathematical system, it is a function of empirical observation; ". . .mathematics deals with symbols--not with the objects which the symbols

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[7]Carl G. Hempel, "Geometry and Empirical Science," in Wiener, op. cit., p. 43.

represent."[8] This is true not only of the social sciences, but of all empirical sciences. Again, and at the risk of redundancy, empirical interpretation of mathematical symbols is not subject to mathematically conclusive proof; only when the factual content is discarded can absolute "proof" be mathematically shown to exist.

Mathematical symbols are abstractions, they have empirical meaning only insofar as such meaning is attached to them by someone, be he sociologist, economist, physicist, or chemist. There is, therefore, a distinctive difference between the symbol and its attached meaning, they are not one and the same. The physicist-philosopher, Pierre Duhem, in discussing this point with regard to the physicist, states that,

> Between the concrete facts, as the physicist observes them, and the numerical symbols by which these facts are represented in the calculations of the theorists, there is an extremely great difference.[9]

The point is that the certainty and absoluteness which characterize a pure mathematical system do not carry over, in the same respect or to the same extent, into an empirical field, whether it be physics, chemistry, or what have you. One cannot "prove" that "opportunity + desire = crime" in the same way one "proves" that 2 + 4 = 6.

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[8] S. S. Stevens, "Psychology and the Science of Science," in Wiener, op. cit., p. 175.

[9] Pierre Duhem, "Mathematical Deduction and Physical Theory," in Wiener, op. cit., p. 27.

Mathematical induction will not "prove" the "truth" of an empirical proposition.

> What we have called the "truth" of an empirical proposition is something which can never be absolute. Repeated tests of an objective sentence can add to its probability, but never clinch its certainty. Induction, as Hume pointed out, is not a watertight method of proving anything empirical.[10]

The mathematician Carl G. Hempel discusses this point at length, and his conclusion is deserving of quotation in toto.

> The most distinctive characteristic which differentiates mathematics from the various branches of empirical science, and which accounts for its fame as the queen of the sciences, is no doubt the peculiar certainty and necessity of its results. No proposition in even the most advanced parts of empirical science can ever attain this status; a hypothesis concerning "matters of empirical fact" can at best acquire what is loosely called a high probability or a high degree of confirmation on the basis of the relevant evidence available; but however well it may have been confirmed by careful tests, the possibility can never be precluded that it will have to be discarded later in the light of new disconfirming evidence. Thus, all theories and hypotheses of empirical science share this provisional character of being established "until further notice," whereas a mathematical theorem, once proved, is established once and for all. . .[11]

If one accepts as necessary the less than certain results which accrue from the interjection of empirical content into a mathematical system, one must further become aware of the "degrees of uncertainty," so to speak,

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[10] S. S. Stevens, op. cit., p. 175.

[11] Carl G. Hempel, op. cit., pp. 41-42.

which may result from the interjection of different kinds of empirical content. For an empirically "contaminated" mathematical system to retain a relatively significant degree of certainty, ". . . the system of postulates has to be made so complete that all the required propositions can be deduced from it by purely logical means."[12] This suggests that, within a given set of empirical propositions which are going to be "mathematically proved," account must be taken of all the possible variables which may influence the factual outcome. Within the realm of the traditionally "pure" sciences, there is a good probability that such a state can be attained or, at least, closely complied with, quantum physical experimentation being a seemingly notable exception. On the other hand, there are so many variables involved in social scientific research, and they are of such a nature, that such a state seems to be out of reach for the methods now being utilized by students of that academic discipline. That is, it appears easier to identify and control the possible effects of "heat" or "light" than it is to similarly control "attitudes" or "thought." Although the "too many variables" argument is admittedly an old one, unlike other types of aging, time does not necessarily diminish the virility of arguments.

It appears, at this point, that the kind of certainty which is so characteristic of pure mathematical

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[12] Ibid., p. 42.

systems cannot be attained in the empirical field, any empirical field. One of the reasons for this appears to be the lack of universality with regard to the definition and meaning of empirical facts which is so necessary for mathematical techniques to be successfully utilized; again, this problem is common to all the empirical sciences. In addition to the problem of universality, or, more correctly, the lack of it, there is the problem of constructing postulational systems which take account of all the variables which may be influential in the particular phenomenon under analysis; in this respect, the social sciences seem to be confronted with more formidable obstacles than is the case with the "pure" sciences. It appears less complex to determine a significantly useful relationship between a virus and the disease of polio than would be the case with "atmosphere" and "coercion." In any case, this appears to be an appropriate point to undertake a discussion of the question of "Certainty" in the pure sciences and its relationship to "Laws" within those sciences.

"Certainty" and "Laws"

A number of students of the social sciences seem to imply in their writing that somewhere, under the confused maze of social phenomena, there lurk "scientific laws" which will supply rather clean-cut and/or absolute-type answers to various questions concerning these social phenomena. The general feeling seems to be that if one follows the

"objective" methods of the "pure" sciences one will then uncover social scientific laws of human behavior which reflect the "certainty" of "purely scientific" laws. With these propositions, there are a number of problematic implications, most of which will be discussed in more detail as the thesis progresses. For example, the assumption that scientific laws regarding some academic discipline are discoverable in the sense of existing prior to human formulation is subject to question. At this point it need only be noted that the general consensus seems to be that "laws" are formulated in such a way as to conform to the natural phenomenon under study, and not vice-versa. The implications of "objectivity" in undertaking certain kinds of physical experiments is also subject to question, particularly in the light of recent developments in quantum physics. This point will be discussed later in this chapter.

The subject of this particular section, however, is the question of "certainty" and the existence of "absolute laws" in the physical sciences.

It has been suggested earlier in this chapter that empirical propositions cannot achieve the same kind or degree of certainty which characterizes pure mathematical systems. In the empirical realm, mathematical "correlations" are many times more flexible than mathematical "proof." It has been known for quite some time that

empirical phenomena, for the most part, were best explained by statistical laws of probability as opposed to absolute laws. Although it is not uncommon to hear of the existence of universal laws which assert that in all cases without exception when conditions of a specified kind C are realized a phenomenon of a certain kind E occurs, it now seems difficult, if not impossible, to specify exactly all the conditions involved. For quantum theory now suggests that it is impossible to accurately specify at least one condition, the prediction of certain kinds of atomic events, of which, it may be noted, all other events are composed. The Hiesenburg "uncertainty principle" involving the inability of accurately predicting both the position and momentum of an electron is probably the most notorious example of quantum theory implications. It is emphasized that at this level not even statistical laws of probability can be accurately utilized.

The statistical laws of probability begin to have meaning only as more and more atomic events become involved in a given phenomenon.

> Only in the co-operation in the enormously large number of atoms do statistical laws begin to operate and control the behavior of these assembles with an accuracy increasing as the number of atoms involved increases. It is in that way that the events acquire truly orderly features. All the physical and chemical laws that are known to play an important part in the life of organisms are of this statistical kind; any other kind of lawfulness and orderliness that one might think of is being perpetually disturbed and made inoperative by the unceasing heat motion of the atoms.[13]

Schrodinger writes further of the $\sqrt{N}$ rule:

> The laws of physics and physical chemistry are inaccurate within a probable relative error of the order of $\sqrt{N}$, where N is the number of molecules that co-operate to bring about that law--to produce its validity within such regions of space or time (or both) that matter, for some consideration or some particular experiment.
>
> You see from this again that an organism must have a comparatively gross structure in order to enjoy the benefit of fairly accurate laws, both for its internal life and for its interplay with the external world. For otherwise the number of co-operating particles would be too small, the "laws" too inaccurate. The particularly exigent demand is the square root. For though a million is a reasonably large number, an accuracy of just one in one thousand is not overwhelmingly good, if a thing claims the dignity of being a "Law of Nature."[14]

One reason for this inability to accurately predict atomic events is that, at this level, the observer himself is part of the event. The very processes by which he predicts are themselves part of the event, they cannot be separated from it. On this point, the noted physicist, Max Plank, writes:

> In consequence the actual impossibility of predicting even a single occurrence accurately in classical as well as quantum physics, appears to be a natural consequence of the circumstance that man with his sense organs and measuring instruments is himself a part of nature, subject

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[13] Erwin Schrodinger, "What is Life," in Robert M. Hutchins and Mortimer J. Adler, The Great Ideas Today (Chicago: Encyclopedia Britannica, Inc., 1967), p. 381.

[14] Ibid., pp. 384-385.

> to its laws and confined within its limits, whereas the ideal intellect is free of all such limitations.[15]

"Certainty," then, is really a matter of degree, it can even be said to be relative, it depends on the observer's "threshold of improbability" if you will.

To what extent these individual atomic events influence the relatively gross phenomena with which social scientists deal is unknown. However, it should be noted that at no stage in the conceptual "growth" from atomic events to gross behavior is "certainty" substituted for "probability." Indeed, the latter term does not even begin to have meaning until one leaves the atomic level of conceptualization. It is obvious that the significance of these atomic events is closely related to the question of "probability," a term which itself is not susceptible to precise universal definition. Lest one be inclined to disregard the significance of atomic events at the macroscopic level, Shrodinger warns that

> . . . incredibly small groups of atoms, much too small to display exact statistical laws, do play a dominating role in the very orderly and lawful events within an organism.[16]

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[15]Max Plank, "The Concept of Causality in Physics," in Wiener, op. cit., p. 87. For the reader's information, the "ideal intellect" in this case is that which deals with totally abstract entities or non-empirically observable phenomena, i.e., mathematics.

[16]Schrodinger, op. cit., p. 385.

It is interesting to note that the people who work most directly with these concepts and mathematical calculations, the physicists, the chemists, the mathematicians, are not, for the most part, the same people who assert the "certainty" or "absoluteness" of scientific "laws." On the contrary, these kinds of assertions are most vociferously put forth by people who, for the most part, have not had the training and experience of working with these concepts that the student of physics or mathematics may have had. Maybe it is because of this unfamiliarity and lack of experience in working with these concepts that

> The non-physicist finds it hard to believe that really the ordinary laws of physics, which he regards as the prototype of inviolable precision, should be based on the statistical tendency of matter to go over to disorder.[17]

There appears to be a constant attempt, almost to the point of obsession, to "explain" human behavior in terms of laws which are based on the same kinds of assumptions which underlie physical laws. Indeed, in many cases the same basic conceptual framework which is found in the physical sciences reappears in the form of social or behavioral "laws." If the physicist is not sure about the "certainty" of physical laws, how can the social scientist be so sure about them? Maybe the concept of "physical

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[17] Ibid., p. 414.

laws" is not applicable to social phenomena. With regard to this point, the physicist, Schrodinger, emphasizes

> . . . that from all we have learnt about the structure of living matter, we must be prepared to find it working in a manner that cannot be reduced to the ordinary laws of physics.[18]

If only the humility of this physicist could, by some osmotic process, be passed on to many a social scientist!

Generally speaking, this writer is not in total disagreement with the proposition that if one follows, in a sufficiently similar manner, the methods utilized by students of the "pure" sciences, one may then expect to attain the same kind of "certainty" in results, although not necessarily to the same degree, that is enjoyed by the pure sciences. However, it is this writer's feeling that this "certainty" is really not quite as certain as some social scientists seem to imply. The difference between mathematical and empirical "certainty" is, as indeed the differences among the certainties" within the empirical realm are, rather significant. And it is suggested that these "certainties" will affect the reliability of both the methodology utilized and the results obtained.

Quantum Physics and "Subjectivism"

The work done, and now being undertaken, in the area of quantum physics has literally shaken the world of

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[18] Ibid., p. 418.

physical science and philosophy. The implications for philosophy in particular are relevant to this thesis, and, consequently, a short discussion will be undertaken with regard to some particularly relevant aspects of this comparatively "new" area of physical science. Basically, quantum theory appears to modify the rather neat, compact atomic model which consisted of electrons, protons, neutrons, etc. (at last count there were 100+ of these particles) manifesting spatial relationships in a manner not unlike the solar system. Quantum theory now modifies this neat picture in such a way that one now has a more accurate picture of the situation if one views these particles as bieng, in effect, units (quanta) of energy. Although the mathematical calculations and detailed analysis are far beyond the intellectual capacities of this author, nevertheless, it can be said that this basic conceptual modification seems to be the most significant general change in atomic (or, more correctly, quantum) physics.

Whence the relationship between quantum physics and methodology in the social sciences? The exact relationship is unknown; but there is one point that is deserving of discussion. As physicists refined and "re-refined" experiment after experiment with regard to quantum phenomena, they found that there was a point at which the experimenter ceased to "examine" the "experimentee," so to speak. That is, at that level of analysis "experimentor" and

"experimentee" really had no meaning in the traditional sense; there ceased to be a "subject" and an "object." The instruments, the machines, light, man himself, were interacting with the phenomena being studied, and, consequently, the experimentor became, in effect, part of that which was being "observed."

The most significant implication of this effect is that it appears to be inaccurate to speak of "objectivism" in the classical sense as being removed from the object of study; rather, the key word here is "subjectivism." The "observer" is now an integral part of the "observed," and is incapable of detachment.[19] The implications of such a situation have been recognized for some time in the field of philosophy,[20] although this writer does not portend through understanding of the relationship, if in fact there is any, between the position as advanced in the physical sciences as compared to that advanced in the realm of social philosophy. Nevertheless, the student of the social sciences should be aware of these positions and their possible implications for success (or, more correctly, failure) in undertaking such "dredging operations necessary

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[19]Richard Schlegel provides a good discussion in his Completeness in Science (New York: Appleton-Century-Crofts, 1967).

[20]Pierre Duhem suggests this, as does Willard Van Orman Quine, From a Logical Point of View: Nine Logical Philosophical Essays (Cambridge: Harvard University Press, 1953).

to remove obstacles to the development of scientific objectivity" hinted at by Martindale.[20] If the observer is, in fact, part of the system within which he functions, then there may be a social application of Goëdels theorem, which implies the impossibility of a system examining, comprehending if you will, itself. If this be true, it may be that social researchers should restrict research to the "descriptive" realm, and emphasize the creative aspect of research rather than the "explanatory."

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[20]Don Martindale (ed.), Functionalism in the Social Sciences (Philadelphia: American Academy of Political and Social Science, February, 1965), p. viii of the Introduction.

CHAPTER III

A DISCUSSION OF SOCIAL SCIENTIFIC METHOD

General Introduction

With this chapter will begin discussion of those isolated factors with which the social scientist, particularly the comparativist, will be, or should be, most directly concerned. It must be emphasized at this point that the division of this chapter into specific, sectional topical areas, such as "Concept Formulation," "Objectivity," and "Truth," was undertaken purely for reasons of chirographical expediency. The divisions, while not without justification, are, in the last analysis, rather arbitary. The action is justified for the reason that coherence of thought is thereby somewhat simplified, but arbitrary in the sense that this writer is not aware of any "scientific method" which would "objectively" justify the action taken.

The point is that the subject matter areas should be viewed as being closely interrelated. Indeed, one will find that a clear understanding of the concepts introduced in one section may be dependent upon a grasp of the concepts introduced in another section, and the reader will find that some concepts have been introduced in earlier sections in a rather superficial manner with the understanding

that the matter will be taken up later in this chapter or the thesis. Insofar as coherence of thought seems to permit, reference to appropriate concepts in other sections will be made at the time that such referral is deemed necessary. Although this procedure may result in a certain amount of repetitiveness and apparent redundancy, nevertheless, it is well worth the price if clearer understanding will result.

Concept Formulation

Above all, the researcher deals with concepts. The term is rather vague, and a definition will not be attempted here. Rather, it is hoped that a clearer understanding will result from examining some stated purposes of "concepts," and similarly examining some factors involved in their formulation. Among other purposes, concepts serve to provide some sort of order to the myriad phenomena with which the researcher is confronted, they serve to facilitate comparison, they provide for productive communication, and, in general, they provide a basic framework for human understanding. Although a detailed discussion of each of these topics would certainly not be irrelevant to the present analysis, nevertheless, such an undertaking would be beyond the proper scope of this thesis. Consequently, the present discussion will be most directly concerned with the process of conceptual formulation per se as an attempt to provide

a more manageable picture of interconnected phenomena, and, secondarily, with the implication that the discussion of the process may have for the conceptual role in comparative analysis.

A working assumption of this analysis is that the universe of phenomena, in their totality, are a totally interrelated whole, of which the human being, the researcher if you will, is a part.[1] This view is certainly suggested at the quantum physical level, and, as mentioned in the last chapter, some philosophers have maintained that a similar condition obtains on the "macroscopic" level. Since the human intellect is very probably incapable of comprehending every relationship that could exist among the universe of phenomena, the intellect constructs various "concepts" or "images" which represent those relationships which are either the sum of those capable of being grasped by the "observer" or those which hold some particular interest for him; "criminal behavior," "enforcement," and ""deviancy" are representative examples.

In any case, the "concept" is an extraction from this totality of phenomena, and this extraction, for whatever

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[1]A. N. Whitehead, Science and the Modern World (New York: The Free Press, 1925), pp. 161-162.

*The writer will not get involved, at this time, with the question of whether or not such conceptual extraction is justified or, indeed, necessary. It may very well be that such extraction is a necessary part of the functioning of "mind," although this is a rather complex philosophical issue.

reason, will not be without repercussions for a thorough understanding of the particular "event" or sum total of relationships being represented by the concept. The general proposition being suggested is simply this: The choosing of the "event" which is represented by the concept, the process involved, is, in the last analysis, basically arbitrary in nature. It is arbitrary in the sense that the particular conceptual extraction could have taken place at any other point in the universe of phenomena, been involved with any other event, without contributing any more to the observer's understanding, to his real knowledge, of the total phenomenal system of events. The interjection of the intellect in the form of "concepts" or "meaning" introduces an artificiality which is obstructive of, as Kant might call it, "pure experiencing."[2]

The following, possibly oversimplified, example may help to illustrate some of the implications of this position. Say that line AZ represents the universe of relationships, the universe as a whole, the line is solid. Further, say that a researcher is studying some phenomenon or "event" P whose conceptual symbol or symbols represent some part CE of AZ. Suppose also that another researcher is studying a phenomenon P_1 whose conceptual symbol or

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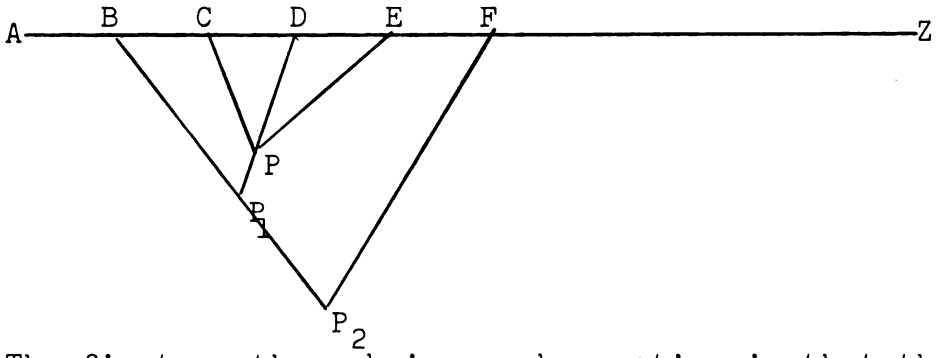
[2]Kant seems to have suggested this in some of his material, particularly in the Prolegomena to Any Future Metaphysic That May Be Presented As a Science, Academy Edition.

symbols represents some part BD of AZ. Finally suppose that a third researcher is studying the phenomenon P_2 whose conceptual symbol or symbols represent some part BF of AZ.



The first, rather obvious, observation is that there are an infinite number of possibilities ($P\infty$) for the conceptual extraction of some phenomenon. Second, since all the relationships on AZ are interrelated, no one event can be completely understood without a concomitant understanding of (1) all the phenomena, one or both of whose extreme points on AZ fall within the extreme points of the phenomenon under analysis, and (2) all phenomena, one or both of whose extreme points fall "immediately"* outside the extreme points of the phenomenon under study. These, of course, will encompass the relationships <u>directly related</u> to the relationships under study. However, when one speaks of an analysis of all the relationships <u>directly related</u> to the ones under analysis, one thereby introduces

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*On a truly solid line which represents an infinitude of relationships, it is indeed difficult to conceive of some point "immediately" outside of another point.

a note of relativity which suggests an infinite number of direct relationships which will be necessary for the thorough understanding of the relationships directly related to the ones under study. In other words, steps 1 and 2 above must now be undertaken for a complete understanding of the relationships directly or indirectly (for they will soon become directly) related to the ones under study. Using the illustration above, and again, consequently, at the risk of oversimplification, one might say that thorough understanding of P requires a thorough understanding of P_1, which requires a thorough understanding of P_2, which, in the final analysis, requires a thorough understanding of $P\infty$. Actually, a sphere would probably serve the illustrative purpose much better than the line AZ, since the real impact of $P\infty$ can be more easily grasped when something more than a two dimensional reference is utilized.* The application to the "field" of police administration can be made more obvious by substituting "law enforcement" for P, "policing" for P_1, and "social control" for P_2.

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*This, of course, is the way Einstein had envisioned the universe, that is, as a three dimensional (4 dimensional if one includes Time) geometrical figure. It served very well for depicting the "closed" nature of the universe, and it would probably serve very well for depicting the nature of reality that is envisioned by this writer. Incidentally, it seems quite possible that the similarity may be more than structural.

The important point to emphasize is that the concepts with which the researcher works represent events which have been conceptually extracted from their totality of interrelationships. Indeed, the very terms "event" and "interrelationships" are really quite inadequate since they may easily acquire a connotation of finiteness with respect to the conceptual segments involved. It does indeed seem justifiable to say that ". . . every single perception discloses on closer examination an infinite number of constituent perceptions which can never be exhaustively expressed in a judgment."[3] The utilization of the term "phenomenon" or "event" immediately restrains one's view of reality insofar as interrelationships with other "events" are concerned. Regarding this problem, Brecht writes:

> . . . events cannot be neatly separated into units that follow each other like the units of an alphabet, distinctly identifiable as separate. Every detail of what occurs is so inseparably intertwined with what had occurred before and will occur after, and even with what occurs simultaneously, that any lumping together of occurrences into bigger units called "events" is utterly arbitrary . . .[4]

If such general and all-inclusive terms as "event" or "phenomenon" fail to represent the interrelationships of reality, how much less so would such terms as "policing," "legal," or "enforcement"?

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[3]Max Weber, "'Objectivity' in Social Science," in Wiener, Philip P. (ed), Readings in The Philosophy of Science (New York: Charles Scribner's Sons, 1954), p. 330.

[4]Arnold Brecht, Political Theory; The Foundations of Twentieth-Century Political Thought (Princeton: Princeton University Press, 1959), p. 97.

There are a number of questions which present themselves at this point if one accepts the basic position suggested by the previous discussion. If the concepts in terms of which natural phenomena are comprehended are indeed "man-made inventions,"[5] how does one know how accurately one's concept represents reality? As Harre puts it:

> We know that our perceptions, are, in part, the result of the mental ordering and grouping of our sensations. It is an easy step from this fact to sceptical doubts about the degree of resemblance which we are entitled to assume that our perceptions bear to those things, properties, and processes which we want to say cause our perceptions.[6]

Further, one is always confronted with the problem that what one is observing is most probably not some elemental relationship, but, rather, a conglomeration of relationships; one is never quite sure whether what one is observing is a number of details which are combined into larger units, or larger units (wholes) which can be further subdivided into parts.[7] Actually, it seems more likely that it could be both depending upon one's perspective.

It may very well be that, as Cohen suggests,[8] the formulated concepts are, in fact, reality; that it is more

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[5]L. L. Thurstone, "Multiple-Factor Analysis," in Wiener, (ed.), op. cit., p. 193.

[6]R. Harre, Matter and Method (New York: St. Martin's Press, 1964), p. 4.

[7]Brecht, op. cit., p. 39.

[8]Morris R. Cohen, "The Place of Logic in the Law," Introduction to Law: Selected Essays Reprinted From the Harvard Law Review (Cambridge: The Harvard Law Review Association, 1965), p. 188.

realistic to view concepts as themselves being the "reality" and to end our search for some "reality" apart from the concepts. Certainly, Mannheim's observation that " . . . human thought-processes concerned with the same world produce divergent conceptions of that world" can be viewed as suggesting a similar conclusion. However, assuming this position, how does one explain the observation that in many cases scientists, working independently of each other, often arrive at similar conclusions regarding some aspect of "reality"? These issues will be discussed further, either directly or indirectly, as the thesis progresses.

Although the relevance to comparative method, with particular reference to criminal justice, can be readily deduced from the discussion, the implications may be explicated further. The rather general, and very basic, term "legal behavior" is indicative of the problems foreshadowed by the previous discussion. Can behavior really be so distinctly separated into "legal" and other kinds? How does one draw the line between "legal" and other types of social behavior? By separating "legal" behavior from other types of behavior, has not one thereby "staked out" one's position on the line AZ, and has not one thereby raised the issues ($P\infty$) connected with such action? Again, can the administration of a given organization be truly understood in vacuo? Of course not, and no contemporary student of public administration would suggest such a

proposition. However, when one begins to examine those phenomena most directly related to the administration of a particular organization, one then becomes involved in the "P∞" problem, and any line that one may draw with respect to the most directly related phenomena will be, in the last analysis, arbitary.

These are significant questions which need analysis. If one accepts their implications, then one should question oneself regarding the conceptual framework from which one pursues social scientific study. Further, if one accepts the implications, how does one justify research from the present conceptual framework? Brecht offers in justification for continuing in this manner the fact that the social sciences need research, now, and that the present framework is all we have to work with in undertaking social analysis.[9] Although there is, obviously, wide consensus on the validity of this basic justification, somehow, this just does not seem to sufficiently meet the problem. As Brecht himself had earlier written:

> Agreement does not necessarily prove the validity of a theory; human history is to a considerable extent a history of erroneous agreements.[10]

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[9]Brecht, op. cit., p. 79.

[10]Ibid., p. 11.

Objectivity, Part I

"Objectivity" has become an end in itself within the realm of social research. Its apotheosis has probably resulted from, among other things, its close association with experimental methodology in the "pure" sciences. Due to the rather charismatic quality of the term, and due similarly to the position it now occupies in the hierarchy of social scientific values, one would be remiss not to undertake an analysis of the term in any discussion of social scientific methodology. To facilitate the presentation of material, the following discussion has been divided into two (2) general questions; (1) Can one approach a given analysis of some phenomenon in the absence of any preconceived ideas regarding certain aspects of that analysis?, and (2) What is meant, indeed, what can be meant, by the proposition that one must "objectively" observe certain kinds of phenomena? These two questions, obviously, are closely related; indeed, their very separation may appear quite illogical and wanting of adequate justification. Admittedly, this is a valid criticism, but this writer could think of no better approach which would take into account the rather subtle distinction between general method and particular observation which is suggested by the division. Hopefully, the distinction, however subtle, will become more easily discernible as the discussion progresses, for if, and as, this occurs, the justification for the initial division may become self-evident.

It is not uncommon to hear it said that one must approach a particular research project with an "open mind," or "objectively," or without any "preconceived ideas" that may "distort" the "true" picture. One must be aware of drawing conclusions before they have been "verified" by the "facts." With regard to general questions of method, the fact that the human mind is most likely incapable of manifesting itself without the use of ideas is not worthy of discussion; it is simply unrealistic to speak in terms of a mind without ideas. Proceeding on this admittedly empirically unverifiable assumption, it would seem that a more proper question might be, "Do such preconceived ideas serve any useful purpose in scientific research?" The answer appears to be in the affirmative.

To begin with, without the existence of pre-constructed concepts, field research would be undertaken rather indiscriminately. There would be no orientation with regard to the gathering of data, indeed, one could not tell what the relevant data would be.

> Scientific achievements are rarely made by those who start with an open mind without any knowledge or anticipation of nature. In order to find something we must look for it . . . without any anticipatory idea . . . we do not know what facts to look for and cannot recognize what is relevant to the inquiry.[11]

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[11]Pauline V. Young, Scientific Social Surveys and Research, 4th Ed. (Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1966), p. 109.

Within the present conceptual framework of the social sciences, the characteristics of experimentation necessitate some kind of selective orientation for information gathering. In addition, pre-experimental thought may give some indication of where to go to find certain types of information, and, in some cases, there may be some indication of what types of information may be expected to be available.

> It is a complete misunderstanding of the requirements of scientific research to begin immediately with indiscriminate field research. Such research can and should be well prepared by hard thinking on the basis of available experiences, including those of the thinker himself. Such preparatory work may lead us to good guesses of what we can or cannot expect to find . . . [12]

Generally, the need for this kind of preparatory work is accepted by students of the social sciences; preconceived ideas, in that sense of the word, are accepted as necessary. When one is able to clearly discern and, in effect, to some degree manipulate these types of preconceptions, the problems do not appear to be particularly significant. However, there are other kinds of preconceptions which are necessarily involved in this type of operation, and which may not be so easily recognizable. These types of preconceptions provide the basic conceptual structure by which one's thought processes operate, and,

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[12] Brecht, op. cit., p. 392.

consequently, these kinds of preconceptions are not readily identifiable. The problem is made even more complex when analysis or research of some kind is being undertaken within the context of a social environment in which this basic conceptual structure is shared by the other inhabitants.

In the field of comparative analysis, where there is a rather good probability that one may be studying phenomena which are manifested in an environment whose inhabitants share a basic conceptual structure with regard to thought processes that is different from that of the observer, the issue becomes at the same time both significantly more subtle and subtly more significant. This problem is not peculiar to the social sciences; it is a potential problem for all empirical sciences. It is particularly operative whenever meaning is attached to any symbol. The physicist-philosopher Henri Poincare states the problem rather clearly.

> It is often said experiments must be made without a preconceived idea. That is impossible. Not only would it make all experiment barren, but that would be attempted which could not be done. Everyone carries in his mind his own conception of the world, of which he cannot so easily rid himself. We must, for instance, use language; and our language is made up only of preconceived ideas and cannot be otherwise. Only these are unconscious preconceived ideas, and thousand times more dangerous than the others.[13]

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[13]Henri Poincare, "Hypotheses in Physics," Wiener (ed.), op. cit., p. 33.

As more and more "knowledge" is accumulated as a result of research based upon some one conceptual thought structure, the more difficult it becomes for one who shares such a structure to recognize the element of relativity which pervades it. So long as one remains unaware of one's attachment to a basic conceptual thought structure, so long does one remain so attached to one's way of thinking

> . . . that the ways of thinking which are perceived in other groups are regarded as curiosities, errors, ambiguities, or heresies. At this stage one does not doubt either the correctness of one's own traditions of thought or the unity and uniformity of thought in general.[14]

As mentioned previously, the implications of this position should be carefully weighed by the comparativist, for there are ramifications for all disciplines of comparative social scientific analysis. In the field of political science, for instance, our sometimes overly emotional attachment with basic democratic principles has frequently hindered real understanding of the role of elements of other political systems.[15] In the field of police administration, the experience in post-war Japan presented the problem in a fashion which was all too clear to the American police administrators who were involved with "democratizing" the Japanese police system. In the

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[14]Karl Mannheim, "The Sociology of Knowledge," Wiener (ed.), op. cit., p. 352.

[15]Brecht, op. cit., p. 32.

broader field of criminal justice, one's basic conceptions of "trial," "law-morality,' etc., may prevent one from fully comprehending systems that deal with similar phenomena in other cultures. Indeed, the very term "criminal justice" is a concept which may not have significant meaning within the context of another culture.

On Words

The discussion has now become so indirectly concerned with "words" per se, that a short expository departure from the analysis of "objectivity" at this point seems almost necessary to maintain coherence of thought. The problem of the ambiguity and relativity of meaning of words is, or should be, most familiar to social scientists in the comparative field. Where comparative analysis is undertaken in a cross-cultural context, the "word problem" or "language problem" takes on added proportions. Although it is a potential problem in almost any academic endeavor with empirical overtones, word ambiguity is a particularly sticky issue in the social sciences, where the subject matter appears to be a little more susceptible to such problems than is generally the case with the subject matter of, say, physics or chemistry.

Political scientists and public administrators, due to their relatively recent interest in comparative analysis and consequent lack of experience, have found the problem

particularly sticky. This fact, added to the burden created by the nature of the subject matter dealt with, presents a rather formidable obstacle for the undertaking of valid comparative analysis. As Brecht states:

> Most words (verbal symbols) are ambiguous; they cover a great variety of facts or values, or of both. This is especially so in the political field, where terms such as freedom, equality, democracy, socialism, patriotism, loyalty, power, authority, order, carry many different meanings.[16]

The fields of law and legal philosophy, insofar as one may wish to distinguish, is notorious with respect to semantical problems. Felix Frankfurter, in discussing the difficulties which arise from statutory interpretation even within the context of one's own culture, feels that:

> The problem arises from the very nature of words. They are symbols of meaning. But unlike mathematical symbols, the phrasing of a document, especially a complicated enactment, seldom attains more than approximate precision. If individual words are inexact symbols, with shifting variables, their configuration can hardly achieve invariant meaning or assured definition.[17]

Similarly, Karl Llewellyn feels that similar problems may have indirectly affected the effective study of legal behavior. He emphasized ". . . the limitations of rules, of precepts, or words, when made the focus, the <u>center</u> <u>of</u>

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[16]Brecht, <u>op. cit.</u>, p. 57. In passing, it might be noted that Brecht suggests the use of mathematical symbols to replace such terms.

[17]Felix Frankfurter, "Some Reflections On The Reading of Statutes," <u>Essays On Jurisprudence From the Columbia Law Review</u> (New York: Columbia University Press, 1963), p. 44.

reference, in thinking about law."[18] In critically discussing the traditional approach to the study of legal phenomena, which has striking similarity with the traditional approach in political science, public administration, or police administration, he goes on to say:

> The traditional approach is in terms of words; it centers on words; it has the utmost difficulty in getting beyond words. If nothing be said about behavior, the tacit assumption is that the words do reflect behavior.[19]

The point to emphasize is that the researcher should look beyond the verbal symbols, the words, in analysis of some social phenomenon. So much for words.

In summary of this section, it is noted that it is unrealistic to think in terms of approaching some particular piece of research in a state of mind which is devoid of preconceived ideas. In fact, it is difficult to conceive of "mind" as existing apart from "ideas." Not only would it be a rather frustrating experience to attempt to reach such an "ideal" state, but it is questionable whether such a state would, in fact, be "ideal." For preconceived ideas are not only helpful in delimiting the area of study and indicating what the relevant data may be, but they are, in addition, simply necessary, whether or not one finds them helpful.

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[18] Karl N. Llewellyn, "A Realistic Jurisprudence--The Next Step," Essays On Jurisprudence From the Columbia Law Review, op. cit., p. 153.

[19] Ibid., p. 161.

Over and above the problem of these rather readily observable preconceptions, one is faced with the additional problem posed by the not-so-readily discernible preconceptions, the very basic ones which influence both the information gathering process and the construction of other concepts. They are, in effect, basic assumptions of "the nature of things" with regard to certain aspects of reality; in the field of physics, "space" and "time" are examples. To recognize and work with these "preconceptions" requires a good deal of very hard, but very basic, thinking. At this stage of development in the social sciences, consensus has not yet been attained on what these most basic concepts, in fact, are; and until such consensus has been attained, one may never know to what extent and in what respect one is being "non-objective" in approaching analysis of social phenomena.

Objectivity, Part II

As utilized in the social sciences, the proposition that one should "objectively" study something seems to carry with it an assumption that the something to be studied is capable of "being" apart from any human meaning which may be attached to it; the position is certinaly not dissimilar to Heinrich Rickert's "Reality" and "Meaning" distinction.[20]

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[20]See Brecht, op. cit., p. 209 for a short discussion of Rickert's thesis.

From the assumption that something can "be" apart from human meaning, it appears that "objectivity," in the sense herein used, would refer to a process by which the human, the researcher, would view the phenomenon from a state of mind devoid of any preconceived meaning. There are a number of problems inherent in this position, some of which were discussed in the preceding section. It is the purpose of this section to analyze some implications that may not have been brought out in that section.

Among other things, the above position suggests that there is a "subject," to wit, the researcher, and an "object," to wit, the phenomenon to be studied; in short, the classical "subject-object" distinction is at the base of the position. At this point one is referred to the discussion on quantum physics which suggested that at the quantum level of analysis there could be no "subject" and "object," both were one and the same. At the quantum level of observation, the key word is not "objectivism," but, rather, "subjectivism," although, upon closer analysis, even that term seems inadequate. The question was also raised at that point as to what implications the conclusions reached regarding observation of quantum events might have for the macroscopically oriented social scientific research activities; and, again, it must be stated that the exact relation is unknown, or, at least, not generally agreed upon. However, it was noted that conclusions similar to

those reached regarding quantum analysis have in fact been reached by some philosophers with regard to the "social universe," if you will. This view is generally put forth as the "wholistic" view, and, indeed, the P∞ problem discussed previously is based upon this position.

If one accepts the "wholistic" position, there are some rather significant implications for social scientific "objectivity," at least in the sense that the term is being herein used. Assuming that one is part of the entire system of events occurring in the universe one calls "reality," then it would seem that one would be incapable of completely comprehending the system of which one is a part, since one could not comprehend the act of one's comprehension. In this sense, then, one is <u>always</u> <u>both</u> "subject" and "object," and one can realize a "subject-object" distinction with respect to oneself only to a degree, specifically, only to the degree that one is "willing" to sacrifice full comprehension of the total system.

This discussion could proceed for some time without all of the ramifications being fully analyzed; the entire question is much too complex to be thoroughly analyzed by this writer and in this thesis. It is mentioned here only as a stimulus to thought, in the hope that some students of the social sciences will become aware that one of the basic premises of certain types of social scientific research,

namely, that there is a "subject-object" distinction, is vulnerable to considerable question.

Regarding the question of "objectivity," there is, in addition to the assumption of a "subject-object" distinction, the assumption that the mind is capable of comprehending certain phenomena, of receiving certain sense impressions, in a totally passive manner. That is, in a manner that will attach no "artificial" meaning or characteristics to the conglomeration of sense impressions received. This attempt to attain purely descriptive statements of "facts" appears to have the potential for causing a great deal of frustration among social scientists. This seems to be particularly true in view of the previous discussion which centered on preconceived ideas and the influence of one's basic conceptual structure on thought processes. It appears that this basic conceptual structure, these unconscious assumptions, mitigate against purely descriptive statements.

> . . . contrary to what some empiricist philosophers seem to have held, "observation-description" are not written on the face of events to be transferred directly into language, but are already "interpretations" of events, and the kind of interpretation depends on the framework of assumptions of a language community.[21]

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[21]Mary B. Hesse, Models and Analogies in Science (New York: Sheed and Ward, 1963), pp. 16-17.

Observation of some event is not simply passive; the literature seems to suggest that observation involves a number of elemental processes, and is influenced by a number of factors.

> Empirical observation of even the simplest fact is a complicated process. In its course the mind is not merely a receiving instrument; it cooperates in various ways, intentionally singling out the objects and sub-objects of observation, using many conceptions and experiences previously piled up in it, and affecting--sometimes sharpening, sometimes corrupting--the accuracy of observation through expectations or through some preconceived shape of configuration.[22]

There is, then, something more to observation than physiological reaction. "Meaning," although possibly having a physiological basis, cannot be understood solely in those terms; "meaning" is not as susceptible to manipulation in the same way that physiological processes are.

> Observing is an experience. A retinal reaction or an olfactory or tactile reaction is only a physical state--a photochemical or pressure-sensitive excitation. Physiologists have not always distinguished experiences from physical states. People see, not their eyes. Cameras and eyeballs are blind. Attempts to locate within the organs of sight (or within the neurological reticulum behind the eyes), some nameable called "seeing" or "observing" may be summarily dismissed. There is more to seeing than meets the eyeball. And there is much more to scientific observation than merely standing alert with sense organs "at the ready!"[23]

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[22]Brecht, op. cit., p. 32.

[23]Norwood Russel Hanson, "Observation and Interpretation," Morgenbesser, Sidney, Philosophy of Science Today (New York: Basic Books, Inc., 1962), p. 91.

It appears that one cannot escape the effects of the totality of one's experiences. The processes by which we observe are significantly influenced by past experiences as well as by physiological states. Both past experiences (in the broadest sense) and physiological states are involved in a joint process which may be called "observation," and the significance of their respective roles probably varies with the phenomenon being observed.[24] For example, it would seem that in those instances where touch and smell may be utilized as an aid in observation, physiological factors would probably play a more significant role than if one were observing some pure abstraction. Specifically, physiological processes would probably play a more direct role for a geologist in analyzing a piece of rock than they would for a social scientist observing some type of "legal behavior"; the social scientist does not "see" legal behavior in the same sense that a geologist "sees" a piece of rock. This suggests that, however contrary to "objectivism" it may seem, past experiences, etc. play a relatively more significant role in "seeing" social phenomena than would be the case in observing those types of things with which biologists and chemists, for instance, work most frequently. However "unobjective" this position may appear, the conclusion does not seem to be unjustified.

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[24] Harre, op. cit., p. 52.

However one may try, one cannot escape the influence that the totality of one's past experiences will have on one's mode of thought and the process by which one gives meaning to what is sensually received. Actually, the phrase "past experiences" is not quite adequate unless taken very strictly, for the social milieu within which one functions will have similar effects on the relatively indiscernible assumptions which influence one's thought process. Within the context of comparative analysis, particularly where cross-cultural overtones may be evident, the issue obviously becomes many times more significant. Karl Popper discusses these problems under the general heading of "the sociology of knowledge."

> The sociology of knowledge argues that scientific thought, and especially thought on social and political matters, does not proceed in a vacuum, but in a socially conditioned atmosphere. It is influenced largely by unconscious or subconscious elements. These elements remain hidden from the thinker's observing eye because they form, as it were, the very place which he inhabits, his social habitat. The social habitat of the thinker determines a whole system of opinions and theories which appear to him as unquestionably true or self-evident. They appear to him as if they were logically and trivially true, such as, for example, the sentence "all tables are tables." This is why he is not even aware of having made any assumptions at all. But that he has made assumptions can be seen if we compare him with a thinker who lives in a very different social habitat; for he too will proceed from a system of apparently unquestionable assumptions, but from a very different one; and it may be so different that no intellectual bridge may exist and no compromise be possible between these two systems.[25]

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[25]Karl Popper, "The Sociology of Knowledge," Wiener (ed.), op. cit., p. 358.

At this point a short recapitulation is called for. The above discussion suggests that there can be no true "objectivity" either in the sense of an absence of preconceived opinions or ideas or in the sense of passive observation of events with no attachment of meaning. It appears that there are always preconceived ideas within the mind of the researcher, and he cannot help but attach meanings to observable phenomena. In addition, in most cases the underlying basis from which such meanings accrue will not be discernible by the observer, for they will be the result of his totality of experiences from which he cannot divorce himself for purposes of analysis. Every meaning, every viewpoint, is, in a sense, both his own and not his own. It was probably, basically, for this reason that Max Weber stated the case, although possibly a little harshly, when he pointed out that:

> There is no absolutely "objective" scientific analysis of culture--or put perhaps more narrowly but certainly not essentially differently for our purposes--of "social phenomenon" independent of special and "one-sided" viewpoints according to which--expressly or tacitly, consciously or unconsciously--they are selected, analyzed and organized for expository purposes."[26]

If pure scientific objectivity seems to be an impossibility, and if all viewpoints regarding social phenomena have sometimes rather significant personal connotations, can there be any "real" phenomena to observe? Does,

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[26]Max Weber, op. cit., p. 325.

indeed can, "objectivity" have a useful meaning? This writer is not qualified to answer the first question at this time. As to the second, at least a statement or two can be made. If one of the characteristics of any scientific method is that it allows for confirmation or rejection by a process of open and public (not necessarily in the liberal political sense) re-experimentation, then maybe

> . . . what we call "scientific objectivity" is not a product of individual scientists' impartiality (since it has been suggested that this may be unrealistic to assume) but a product of the social or public character of scientific method . . .[27]

Here one becomes involved in what Brecht calls "inter-subjectively transmissible knowledge," a concept to be touched on again later in this chapter. At this point, of course, one becomes involved with the question of "truth" and "fact" (again, to be discussed later in this chapter), for they are rather obviously closely related to the question of "objectivity." For now, suffice it to say that the social scientist, particularly the comparativist, should be prepared to accept a workable definition of "objectivity" which reflects considerably less absoluteness and purity than what appears to have been the case in much of the literature which has been put forth thus far.

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[27]Karl Popper, op. cit., p. 362.

Value

The question of "value" is, of course, closely related to that of "objectivity," although the former appears even less susceptible to concise definition than the latter. As was the case with preconceived ideas, most social scientists appear to accept the influence of "value" factors as a necessary, and in some cases useful, condition in the preliminary steps of research. Few contemporary scientists would maintain, as Martindale suggests, that ". . . it is the essence of the scientific mode of thought that it analyze its problems dispassionately in a value-neutral manner."[28] A great many more social scientists feel that value judgments are properly utilized in determining goals, although a number of these may disagree about the use of such judgments in the determination of means; Young's position that research must be pursued "objectively" only after value decisions have been made regarding problem identification, experimental design, etc., is indicative of this basic position.[29]

Social scientists, then, seem to suggest that "value" judgments differ from judgments which are "objectively" made; indicative of this is Brecht's statement, "The

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[28]Don Martindale (ed.), "Functionalism in The Social Sciences" (American Academy of Political and Social Sciences: Philadelphia, February, 1965), p. VIII, Introduction.

[29]Young, op. cit., p. 127.

judgment that something is true or false is no value judgment."[30] The true scientific method per se, is supposedly characterized by "objective" data gathering, and value judgments are influential only in the penumbra of methodology so to speak, or so they say. Indeed, it is maintained that the use of "objective scientific methods," renders one incapable of proving, in an intersubjectively transmissible manner, ". . . the validity of ultimate value judgments beyond the clarifications of their implications and consequences."[31] The distinction is that clear!

Upon closer analysis, however, the distinction does not appear to be really all that clear. Can "value" judgments, in fact, be so clearly distinguishable from other kinds of judgments. Within the context of scientific method, the attempt at separation, for other than purely theoretical analysis, appears to be rather self-defeating. For as John Dewey had pointed out, an attempt toward "scientific objectivity" presupposes ". . . the sincere aim to judge truly," and for this reason any "scientific judgment" is always, in the last analysis, a value judgment.[32] Further, if one accepts the implications of the sociology of

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[30]Brecht, op. cit., p. 265.

[31]Ibid., p. 131.

[32]John Dewey, Problems of Men (New York: Philosophical Library, 1946), p. 228.

knowledge doctrine, one should be prepared to recognize the possibility that "value" judgments, as with other kinds (insofar as one wishes to clearly distinguish), are an inherent part of the thought process of the observer, from which it appears impossible to escape. With this in mind, the impact of Young's statement is more keenly felt.

> In the last analysis it is the experimenter who makes the choices and decisions on what, when, and how to use the complicated system of measuring instruments. These decisions are a scientist's evaluation, and therefore there can be no value-free research or value-free observation.[33]

Finally, there are some implications that follow from the position, maintained previously, that "value" judgments cannot be "objectively" verified by "scientific method." Those who put so much faith in the scientific method and point to all that it has been, and most assuredly will be, able to conquer, must be prepared to accept the possibility that all or most "value" judgments are, in fact, quite "objective," since it may only be a matter of time before an acceptable method will be devised that could "scientifically" verify their "objectivity." Even if this were not the case for all such value judgments, still, how could one know which value judgments will be capable of being "objectively" dealt with in the future? Or, on the other hand, one should be prepared to

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[33]Young, op. cit., p. 182.

accept the possibility that all judgments are, in fact, "value" judgments, of which some may be further termed "objective" because of the existence of some method which complies with some generally accepted rules of procedure. In short, the distinction between "value" and other types of judgments does not appear to be as clearcut as is implied by some social scientists.

Nor does it appear realistic to suppose that "objective" observations are made in vacuuo without recourse to some value premise. The data observed must have some meaning for the observer if the latter is going to utilize the former in some constructive manner, and meaning, as previously suggested, implies recourse to some value premise. It may be quantitatively useful to carry out the arithmatic operation 2+2=4 without attaching any particularly significant meaning to 2 or 4. But, one cannot manipulate social scientific symbols in a similar manner without the attachment of some meaning both prior and subsequent to the arithmatic operation. For in the social scientific sphere, quantitative elements are not as significant as the qualitative ones. Scriven's criticism of the ". . . arid escapism of the so-called 'empirical' school of political science . . ." with their "pristine but pointless evaluations" appears to be based on the position that meaning is necessary for extracting "crucial criteria" from the mass

of observed data.[34] In other words, empirical data can take on meaning for the observer only insofar as they are related to some value premise, and this seems particularly true with regard to the social sciences. The discussion by Weber on this point is quoted at length.

> The significance of a configuration of cultural phenomena and the basis of this significance cannot however be derived and rendered intelligible by a system of analytical laws (Gesetzesbegriffen), however perfect it may be, since the significance of cultural events presupposes a value-orientation toward these events. The concept of culture is a value-concept. Empirical reality becomes "culture" to us because and insofar as we relate it to value ideas. It includes those segments and only those segments of reality which have become significant to us because of this value-relevance. Only a small portion of existing concrete reality is colored by our value conditioned interest and it alone is significant to us. It is significant because it reveals relationships which are important to us due to their connection with our values . . . We cannot discover, however, what is meaningful to us by means of a "presuppositionless" investigation of empirical data. Rather, perception of its meaningfulness to us is the presupposition of its becoming an object of investigation.[35]

As mentioned previously, many social scientists seem to feel that value judgments can and should be reserved for deciding upon ultimate goals and objectives, but having done that, the researcher then "objectively" proceeds to gather the data.[36] In addition to the short discussion

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[34]Michael Scriven, "Science, Fact, and Value," Morgenbesser (ed.), op. cit., p. 185.

[35]Weber, op. cit., p. 329.

[36]Harold D. Laswell, "The Policy Orientation," D. Lerner and H. D. Laswell (eds), The Policy Science, Recent Developments in Scope and Method (Stanford University Press, 1951), pp. 9 and 11. Brecht also takes the position throughout the whole of his book.

earlier, there is now one further implication which seems to be suggested by the preceding analysis of "meaning," particularly with respect to Weber's statement. If one accepts the position that the basic structure of the research project is the result of value judgments,[37] and if one also accepts Weber's position that data without the interjection of the observer's value system are meaningless, then "objectivity" can be easily construed as having meaning only within the subjectively (in the sense of non-objectively, personally, although not necessarily intentionally) constructed system and only to the observer whose values provided the premise for the construction of that system. In other words, pure "objectivity," at best, may have meaning only for the particular observer and only within the context of the study being undertaken.

The theory, of course, is far from tight, and a number of questions can be raised regarding it. The only purpose in expounding it is to raise some very basic questions about the nature of "objectivity" and its relation to "value" judgments. It is hoped that this discussion has considerably confused the whole issue, for confusion may stimulate more thought. Heretofore, the issue has been relatively clear, in some cases too clear, in the minds of many social scientists, particularly regarding students of

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[37]See Footnote 33.

comparative political, public, and police organization. One of the reasons, probably, is that the latter academic areas are relatively new (infant in the case of comparative police administration) and much is yet to be learned. However, in their haste to learn, students of these fields should not accept without question some of the basic elements of social methodology, or, for that matter, any empirical methodology. For the issues are far from clear, and they are very far from resolved.

Fact

A discussion of "objectivity" or value judgments would not be complete without a similar discussion regarding the nature of "facts." For most, if not necessarily all, proponents of the "objective scientific method" presuppose the existence of "facts" which one can perceive if one is "objective" enough. The following quote by Brecht, apparently made in an attempt to justify the use of the term "fact," is indicative of this position.

> So long as we do not abandon the speculative idea that there is a reality which no human senses have yet observed or may ever be able to observe--be it a star whose light has not reached us or has not been made noticeable and may never become noticeable, or be it some metaphysical reality--so long do we need in our vocabulary for this reality a term that is independent of the state of our knowledge; . . .[38]

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[38]Brecht, op. cit., p. 50.

Although such terms as "speculative" and "metaphysical" in his statement do not tend to thoroughly convince one of it, actually, Brecht appears quite certain that there are such things as "facts." In fact, one of the criteria for proving facts to be facts is the acceptance of the apparently observed facts as actual facts![39]

Notwithstanding the certainty with which Brecht approaches the issue, and the frequency with which it is utilized in the social scientific literature, the term "fact" seems to be among the most difficult to define.[40] The problem seems to revolve around the condition that once a phenomenon has been physically received into the sense organs, it must then be given meaning so that it can be truly "observed," this distinction between physiological states and "experiencing" being discussed earlier in this chapter. In addition, the structure of the thought processes and cultural and social environmental variation contribute their share to the issue; as Young puts it, "Facts expressed in words assume different meanings to different people, depending on their past experience as well as on the many things with which they associate the facts and the words."[41] Physiological reaction, thought processes, cultural and

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[39] Ibid., p. 49.

[40] Young, op. cit., p. 10.

[41] Ibid.

social environmental variation, and the intangible called "meaning" all combine to create a problem significantly more complex than might initially appear to be the case upon scanning some of the social scientific literature.

In view of these difficulties, one may easily be tempted to take the extreme position that there is simply no such thing as a "fact." However, how would one then account for the observation, mentioned earlier, that in many cases scientists, working independently (both physically and culturally) of each other, and on the same subject matter, arrive at very similar conclusions? One of the more plausible explanations seems to be that, although there may be some personal variation as to what the "facts" really are, intersubjective compatibility or agreement on certain aspects of the phenomenon concerned may help to solidify an image of the "fact" which is capable of consensus with regard to the observers involved. Surprisingly, Brecht expounds upon this position at length, terming it "consubjectivity," and even accepts it as a basis for his general theory concerning the "intersubjective transmissibility of knowledge."* The discussion fits so well with the present analysis, that it is quoted in toto.

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*Here is an example of the much too common phenomenon where one accepts a position which has implications that seemingly undermine the theory that one goes on to espouse, but one goes on without resolving, or, in many cases without attempting to resolve, the contradiction. In this case, "consubjectivity" certainly does not suggest the absoluteness that is necessary to observe "facts as actual facts," yet both positions are put forth by Brecht.

> An empirical observation may be limited to a single situation or be repeated in essentially similar situations. It may be "extrospective," focusing on the outside, or "introspective," focusing on the inner self. It is always, strictly speaking, observation by one person, and by one person only. Other persons may make similar observations, but never identical ones, because the terms "I" and "Here" refer to different persons and places in each case.
>
> Even though observations by two or more persons are never identical the "what" observed may, of course, be so, if due allowance is made for the different "angle" or "perspective." However, to accept identity of the object (the "what") observed by several persons means to accept a fundamental presupposition: the truth of the common-sense assumption that one and the same thing often cause parallel impressions in different human beings, and that, therefore, there is a broad sphere of what is best called "consubjectivity."[42]

Similarly, Schrodinger's amusing, but pertinent, example of viewing a tree is worth quoting.

> . . . there is a tree there outside my window, but I could not really see the tree. By some cunning device of which only the initial, relatively simple steps are explored, the real tree throws an image of itself into my consciousness, and that is what I perceive. If you stand by my side and look at the same tree, the latter manages to throw an image into your soul as well. I see my tree and you see yours (remarkably like mine), and what the tree itself is we do not know.[43]

The theory of intersubjective verification, "consubjectivity" if you will, appears to have considerable

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[42]Brecht, op. cit., p. 33.

[43]Erwin Schrodinger, What is Life, Hutchins, Robert M. and Adler, Mortimer J., The Great Ideas Today (Chicago: Encyclopaedia Britannica, Inc., 1967), p. 425.

merit. Probably its most valuable asset is that of flexibility. It does not deny the existence of some "factual world," although it is not difficult to conjure up visions of being condemned to Plato's cave. On the other hand, it does not deny the significance of a personal interpretation of these "facts"; on the contrary, it apparently does allow for the influence of those factors subsumed under the theory of the sociology of knowledge in providing for the utilization of intersubjective verification in consolidating some image of a "fact." Finally, it does provide some type of acceptable justification for the apparently radical theoretical and practical changes which have occurred over the centuries.

Whatever the merit, it nevertheless appears to be one of those theories that is incapable of empirical "proof" in the sense that most other types of theories appear to be. In the last analysis, one either accepts it or rejects it on grounds that one considers to justify one's decision. It appears, at this stage of methodological and/or conceptual development, to be not unlike one of those deep-seated preferences that Holmes had talked about. For those who appreciate the style of a great legal philosopher,

> I love granite rock and barberry bushes, no doubt because with them were my earliest joys that reach back through the past eternity of my life. But while one's own experience thus makes certain preferences dogmatic for oneself, recognition of how they came to be so leaves one able to see that others, poor souls, may be equally dogmatic about something else . . . Deep-seated preferences cannot be argued about--you cannot argue a man into liking a glass of beer . . .

CHAPTER IV

A DISCUSSION OF SOCIAL SCIENTIFIC METHOD, PART II

Chapter IV will be devoted to a consideration of the concepts of causation and multiple-causation, the notion of an interdisciplinary perspective, and to some of the problems of "model-building." Throughout the chapter emphasis will be placed on the interrelationships of these topics with each other and, in most cases, with many concepts already discussed in previous chapters. For instance, the discussion in Chapter III on concept formulation will have obvious relevance to the preceding sections, as, indeed, will the general theory of reality around which the entire thesis is oriented. Again, as has been the case thus far, none of the sections will provide as thorough a discussion of the particular subject-matter areas as could logically be undertaken; rather, the main point is to raise som justifiable questions with regard to the utility of these concepts and the validity of conclusions based upon their use.

Causation

As Chapter III may have suggested, there are a number of very basic concepts which are incorporated into one's thought processes in such a way that they are not easily

discoverable. One of the reasons for them being so difficult to uncover is that they form the conceptual structure for some very basic assumptions regarding certain aspects of reality, the truth of which seem self-evident. To question the validity of these basic assumptions is to likewise question the validity of conclusions which were based upon data gathered from the conceptual framework of which these assumptions were a part.

In the traditional "purely physical sciences," the notions of space, time, and matter are some examples of these kinds of concepts. The physical theory of relativity certainly had a significant impact on the traditional notions of space and time, as did the atomic theory on the traditional notions of matter. The impact that the general theory of relativity has had, and is continuing to have, on generally accepted notions of aging, particularly with regard to the human process, is tumultuous; it is indeed difficult to conceive of a reversal of the aging process. Similarly, with regard to the atomic theory, one could imagine how difficult it must have been for so many people to so significantly modify their idea of a piece of rock as being some solid chunk of matter. How difficult it must have been to conceive of a rock as consisting of particles which were constantly in motion; how difficult today to conceive of that same piece of rock as consisting not of particles, but of waves of energy.

The preceding paragraph of thought is not meant to be overly rhetorical, rather, it is meant to point out how difficult it may be to earnestly question some concepts which have heretofore been assumed to be self-evidently true. Such questioning seems to strike, directly or indirectly, at the very foundations of one's experience, at the myriad justifications to which one refers for so many of one's actions. "Causation" is one of those concepts. Its grip on thought processes is so tight that it can be difficult to recognize, and, consequently, it can pose a serious problem to any student of any empirical science. This writer will attempt no final definition or explanation in this thesis, as it is highly unlikely that such an objective could be attained here. Rather, the scope of this discussion will be limited to clarifying the concept and its implications only to the extent that a discussion of multiple-causation can be adequately undertaken.

Generally speaking, "causation" implies, at minimum, some kind of relationship between some given events. As Brecht puts it,

> Cause-effect is the conventional name for the interrelation between two successive events where the occurrence of the earlier is regarded as a condition for that of the latter.[1]

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[1]Brecht, op. cit., p. 74

In other words, if one can validly state that every time A occurs B will thus obtain, it would probably be acceptable to state that one has discovered a cause-effect relationship, at least classically speaking. Actually, it really is not quite that simple, as one may run into considerable difficulty in attempting to define exactly what A and B both include or preclude. In reference to this general problem, Brecht writes of historical causal relations:

> Limited knowledge of relevant events, plurality of causal factors, limited possibility for actors in history to forsee the effects, and the limited ability of the historian to list all the possible alternatives of action and their potential effects, plus the immense number of other variables involved in the analysis of history--all this makes the causal interpretation of macro-history a free field for almost unlimited adventures in speculative thought, while the possibilities of verification are narrowly limited.[2]

Although it was made in direct reference to "macro-history," the basic line of thought in the above statement is definitely relevant to the "present"; for one need only ask, "At what point does an event become 'history'?" All the obstacles in the path of accurate causal interpretations of macro-history are similarly present in the macroscopic social sphere of the present. Rather than proceed with what would eventually develop into tangential discussion, suffice it here to convey the very general picture of a relationship between events which is such that one always

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[2] Ibid., p. 91.

precedes, or, more correctly, has a high degree of probability of preceding, the other. This general idea will suffice to proceed with a discussion of the concept of "multiple-causation."

Multiple-Causation

In view of the earlier discussion regarding the interrelationships of events, a "wholistic" reality, and the P∞ problem of conceptual extraction, the development of a theory of multiple-causation seems at least understandable. The development was probably stimulated by, among other things, (1) an explicit observation that no one event seems to cause only one other event, and (2) an implicit recognition of the artificiality of those very "events" as conceptual extractions. The first observation was common to all the empirical sciences. It was not until relatively recently, however, that the notion of multiple-causation began to gain ground in those sciences. Almost ironically, it seems as though the social sciences started to really develop the idea first. With the increase in field studies, initially by anthropologists, and later by students of other social scientific disciplines, it became more and more obvious that social phenomena could not be understood as consisting of simple one to one relationships with other phenomena. This, in turn, seemed to be the result of the recognition that if "causation" could have any real meaning

within the context of social scientific research, the concept would have to be modified in such a way as to account for the many interrelationships being encountered by social scientists who were now beginning to view social phenomena as being part of an overall process.[3] Indicative of this "processual" approach is the following statement by Young.

> A certain problematic situation or bit of behavior owes its origin or process of becoming not to one factor or set of factors but to a complex variety of factors and sequences . . . It is easy, but dangerous, to follow a "one track" explanation which leads to THE cause. It is imperative to look for a whole battery of causal factors or syndromes which generally play a significant role in bringing about complex situations.[4] (emphasis supplied)

Hence, "multiple-causation" was incorporated into the working vocabulary of the social scientist.

Insofar as a general theory of causation is utilized in the social sciences, it would appear that the species called "multiple-causation" would be most lucrative at this stage of development. Given the theory that reality is a system of interrelatioships incapable of being completely comprehended by the observer, and given the consequent proposition that any given "event" is a conceptual extraction whose interrelationships with all other possibilities

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[3]Pauline V. Young, Scientific Social Surveys and Research, 4th ed. (New Jersey: Prentice-Hall, Inc., 1066), p. 47.

[4]Ibid., p. 485.

for conceptual extraction of "events" are infinite, the appearance of a theory of "multiple-causation" is, as has been suggested, not without justification. Implying the need for a theory of "multiple-causation," Mill writes.

> Whatever affects, in an appreciable degree, any one element of the social state affects through it all the other elements. The mode of production of all social phenomena is one great case of intermixture of laws. We can never either understand in theory or command in practice the condition of a society in any one respect without taking into consideration its condition in all other respects. There is no social phenomenon which is not more or less influenced by every other part of the condition of the same society and, therefore, by every cause which is influencing any other of the contemporaneous social phenomena.[5]

The concept of "multiple-causation" is again implied by Young.

> Man lives in a socioeconomic and political world and thrives on its varied relationships. It is inconceivable that a study of bare and isolated events on any one aspect of man's life would yield any meaningful results . . . His past, present, and future activities, aspirations, motives, and attitudes influence each other and form a variegated and closely knit pattern of behavior.[6]

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[5]J. S. Mill, "On the Logic of the Social Sciences," in Wiener, op. cit., p. 258. It might be noted in passing that Mill feels that one should go on to study some aspect of reality anyway, because he feels that each one will have its own immediate or "main" determining causes. In view of the entire discussion thus far, the problems involved should be rather apparent. Here again, as was the case with Brecht, is an example of one who seems hesitant to accept the logical implications of one's own theory.

[6]Young, op. cit., p. 31.

As has been already suggested, the rise in popularity of the concepts of "social system" or "process" appears to have been contemporaneous with, and possibly related in some other way to, that of "multiple-causation"; indeed, their rise may have been interdependent. The comparatively recent appearance of the "functionalist" approach to the analysis of political behavior are (1) it sensitizes analsis to the complexity of interrelationships among social and political phenomena, and (2) it draws attention to a whole social system as a setting for political phenomena.[7] Here one sees the implication that the understanding of "causal" relationships can best be attained in a setting which takes into account the entire system of "causal" factors.

In the field of comparative police administration, the functionalist approach is in its infancy. As pointed out in Chapter I, most of the literature thus far has been rather structurally oriented, and, conseuqently, meaningful data is lacking. If the trend in the other social sciences is any indication, the "functionalist" approach will probably take on added significance as more students undertake research in the field. The question is, Has the concept of "multiple-causation" (and its relations, i.e.,

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[7]William Flannigan and Edwin Fogelman, "Functionalism in Political Science," in Martindale, op. cit., pp.

"functionalism") really provided for a more thorough understanding of social phenomena? Will it do so for students in the field of comparative police administration?

Summary

In view of the earlier discussion regarding the interrelationships of events, a "wholistic" reality, and the P∞ problem of conceptual extraction, it becomes apparent that the notion of "multiple-causation" is of a rather complex nature. Given the artificial nature of conceptual extractions, how does one conclude that any number of events (A_1, A_2, A_3, etc.) "cause" any number of other events (B_1, B_2, B_3, etc.) without fully comprehending the nature and role of any of the events concerned? Given a sphere as representing reality, and given thus the infinitude of interrelationships which are possible, it would seem that the closest one could come to taking into account the causal role of all the possible relationships would be in terms of an approximation of necessarily uncertain accuracy.

Further, as previously suggested, it would seem unrealistic to talk in terms of "specializing" in, or in some way constricting one's view to, some particular aspect of reality. For this seems to be a never-ending process of changing perspective, at least until one has specialized down to the quantum level of phenomena, and "causation" at that level of observation does not appear

to be quite as useful a concept as it may have been at the macroscopic level. With respect to the proposition that all social phenomena are a multiplicity of events, and similarly with respect to the question of perspectual change, Weber writes:

> The absolute infinitude of this multiplicity is seen to remain undiminished even where one's attention is focused on a single "object," for instance, a concrete act of exchange, as soon as we seriously attempt an exhaustive description of all the individual components of this "individual phenomenon," to say nothing of explaining it causally.[8]

In view of the improbability of ever completely understanding the events involved in a supposed causal relationship, it would appear rather difficult, at best, to validly construct such a relationship with regard to those events. In this respect, Weber again writes:

> Even with the widest imagineable knowledge of "laws," we are helpless in the face of the question: how is the causal explanation of an individual fact possible--since a description of even the smallest slice of reality can never be exhaustive? The number and types of causes which have influenced any given event are always infinite and there is nothing in the things themselves to set some of them apart as alone meriting attention.[9]

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[8] Max Weber, "'Objectivity' in Social Science," in Weiner, op. cit., p. 326.

[9] Ibid., p. 330. For Weber, the factor that sets them apart as meriting one's attention is their relevance to one's value system. See Chapter III, section on Value, for a further discussion of this relationship between "Objectivity" and "Value."

Given this general train of thought, the conceptual extraction of "events" as either "causes" or "effects" will provide little in the way of an understanding of the system of interrelationships called "reality." A notion of "cause-effect" would probably be most useful, although even here quite limited, only within the context of a much larger conceptual extraction, such as "social science," or "police administration" for example, one can "play the causal game" rather accurately (relatively speaking) if one remains eternally alert lest one forget that one is not playing with accurate representations of real interrelationships; unfortunately, reality does not distinguish between "police" and "non-police" phenomena. The question is, does "playing the game" really amount to anything more than that? If so, What?

There are similar implications for the entire theory of causation. The notions of "cause" and "effect" are human concepts that were probably developed to help explain certain relationships that were already conceptually extracted in terms of "events," these notions do not appear to be ontological properties of nature.[10] In other words,

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[10]John Dewey, Logic, The Theory of Inquiry (New York: H. Holt and Company, 1938), p. 326. It might also be noted here that much of Kant's writings suggested a similar situation. Although this writer is not thoroughly acquainted with his Critique of Pure Reason, nevertheless, references to, and extracts from, it appear to sufficiently reinforce the view presented here that it can be recommended in good faith to any reader desiring a notably more scholarly presentation of the philosophical basis and implications of such a view.

the notion of "causation" is, in effect, a heuristic principle, a signpost ". . . to help us find our bearing in a bewildering maze of occurrences . . .,"[11] but when utilized in this respect, or possibly at all, its usefulness in truly understanding the role of some relationship involved in the goings-on of nature may be subject to question. As Brecht states, "Whether causality rules the universe and what causality really is has remained a matter for scientific controversy."[12]

Interdisciplinary I--Culture

Given the notion of mlutiple causation, the need for the utilization of an "interdisciplinary" perspective logically follows. For along with the notion that some event was "caused" by a number of other events, comes the necessity for an understanding of the "causal" events, which events may transcend traditional disciplinary lines. In addition to this apparently logical development, the notion also developed that the probability of including in one's perspective all the "causal" events which may be involved in some phenomenon would be greater if very broad concepts were utilized as a basis for analysis. Hence, a term such as "culture" may be viewed as, among other things, an attempt to provide a working concept which could be

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[11]Max Plank, op. cit., p. 87.

[12]Brecht, op. cit., p. 74.

inclusive of a great many factors thought to be significant for the understanding of certain kinds of social behavior.

The concept of "culture" is said to be useful in understanding all kinds of social behavior, be it individual, organizational, or what have you. It has even been said that the work of any social scientist must begin with the investigation of culture, ". . . the ways of life which are characteristic of particular societies."[13] Chase has stated that the individual ". . . is a product of his culture; he is a living part of his group, and can be understood and appraised only in relation to it."[14] Indicative of the general feeling with regard to the utility of the concept of "culture" is Young's feeling that, "It would seem that, for practical purposes, the research student needs chiefly to examine the cultural milieu to determine what factors actually account for the particular life patterns under scrutiny."[15]

"Culture" has also been utilized in analysis of organizational behavior, both collective and individual. The relationship between an organization and the cultural milieu within which it carries on its administration has been, and will probably continue to be, the focus of much

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[13]Linton, quoted by Stuart Chase, The Proper Study of Mankind (New York: Harper & Row, 1962), p. 59.

[14]Young, op. cit., p. 62.

[15]Ibid., p. 253.

social scientific research. Indeed, some suggest that the very existence of organizations has a significant and integral function with regard to the productive maintenance of any given society. Similarly, it has been said that government in general, viewed as an organization, is best understood as a system ". . . linked organically with social structure, traditions and ideologies, culture and the environment within which it operates."[16] In general, then, most students of administration will accept the proposition that organizational studies are most productive when undertaken with reference to some concept of "culture."

As useful as the term is supposed to be, "culture" is not easily definable. It is usually defined in terms of some other equally vague concepts, which themselves admit of no unambiguous definition.* It is sometimes suggested that better understanding would result if one views culture as a dynamic process or act of becoming, as opposed to some static structure encompassing a number of social interrelationships. In this respect, Young writes:

> It is necessary to go even further and ascertain the system of values that determines the various cultural experiences and activities of the group. This is the same as saying that the research

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[16]Roy C. Macridis and Bernard E. Brown, Comparative Politics: Notes and Readings (Homewood, Ill.: The Dorsey Press, 1964), p. 4.

*Here one is reminded of K. Llewellyn's astute statement, "People are so much used to definitions--although definitions have not always been of so much use to people."

> observer should be interested in social processes and the sequence of events which make up a social system. Unless the observed phenomena are studied in relation to a social process or a particular way of life, they are incapable of explaining reality or of further social explanation of the associated elements.[17]

Harry Levin has suggested that any attempt at some universal definition of "culture" may lead to frustration. It is felt that the term takes on a different meaning with each new generation of scholars, and may even take on various meanings within the same generation of scholars who inhabit, if you will, different cultural milieus.[18] The approach appears to have a good deal of merit, and could probably find much reinforcement in the material currently being presented under the heading of "the sociology of knowledge."

In effect, then, "culture" appears to be just another example of conceptual extraction, but on a level which is much more inclusive than is the case with conceptual extractions which refer to individual kinds of behavior. As such, its usefulness in truly understanding the role of certain kinds of social phenomena which it now is supposedly useful in "understanding" is subject to considerable

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[17]Young, op. cit., p. 184.

[18]Harry Levin, "Semantics of Culture," in Gerald Holton, Science and Culture: A Study of Cohesive and Disjunctive Forces (Boston: Beacon Press, 1965). Levin's entire article suggests this situation.

question; this conclusion seems quite logical in view of the entire discussion thus far. The distinction between "cultural" and "non-cultural" influences, or, indeed, between many different "cultures," is felt to be obstructive of the acquisition of knowledge concerning the nature of social reality. The problem is recognized by a number of scholars, and some suggestions have been put forth. One of the most promising, and one that is certainly in congruence with the general theme of this thesis, is that of Herbert Read's, who, in his To Hell With Culture, says:

> . . . Culture in a natural society will not be a separate and distinguishable thing--a body of learning that can be put into books and museums and mugged up in your spare time. Just because it will not exist as a separate entity, it would be better to stop using the word "culture." We shall not need it in the future and it will only confuse the issue. Culture belongs to the past: the future will not be conscious of its culture.[19]

Interdisciplinary II

Given the development of rather broad concepts, i.e., "culture," to facilitate the comprehension of social phenomena, there remains the problem of gaining an understanding of the events or relationships that either comprise "culture" (when viewed as a process) or are included within its realm. The most recent attempt at solving the problem, or at least of mitigating the

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[19]Herbert Read, To Hell with Culture and Other Essays on Art and Society (New York: Schocken Books, 1963), pp. 12-13.

consequences of it, is seen in the development of an interdisciplinary approach to analysis. This trend is not peculiar to the social sciences, on the contrary, it is observable in some of the most "purely scientific" of the empirical disciplines; witness, for example, the Salk Institute in California, where, in addition to biologists, chemists, and physicists, there are also resident philosophers and historians, with immediate plans to add resident artists and musicians.

Nevertheless, the discussion is herein concerned with the rationale for the development of the interdisciplinary method in the social sciences, and with its real and potential utility. It might be verbally economical in the long run to quote at the outset a rather comprehensive statement that is probably most indicative of the general rationale for the utilization of the interdisciplinary method in the social sciences; Young provides just such a statement.

> The most fruitful results in research are achieved not only through an integration of social scientific techniques and method but also through a unified approach of the various scientific disciplines. Since man lives in a world of economic, industrial, political, phychological forces and social attitudes and values, it is self-evident that his responses to, and roles in, these should be studied. As a matter of fact, the Social Science Research Council was organized to promote and integrate the social sciences to each other and to the related useful arts of industry, government, and public welfare; to guard against overspecialization and isolated efforts which might result in distorted vision, especially when a section of man's world is wrenched loose from its context in the process of study.

> As we have already indicated in the discussion of social scientific laboratories, one of the most significant considerations in cooperative research is that it facilitates the study and analysis of the complex web of social-psychological-economic forces intricately interwoven in modern life. Each collaborator not only may provide a new orientation as to subject matter but also may proceed methodologically in a way which enables him to view certain particular aspects of the functional interrelationships of the various social elements.[20]

This rather lengthy statement just about covers, directly or indirectly, the range of opinion in social scientific circles with regard to an interdisciplinary approach to social scientific analysis.* Simply stated, it comes down to someting like this: No one point of view, single approach, or scientific discipline can completely account for total social reality, each is supposed to be a complement to the others. A relatively systematically coordinated approach of this kind has been lacking between many of the disciplines, and in the general field of

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[20]Young, op. cit., pp. 119-120.

*So many social scientists espouse this view that it would simply be impractical to cite all of them. Of the materials already cited, Brecht's and Chase's respective texts provide further discussion, as of course does Young's. In the field of criminology, the interdisciplinary approach is stressed by, among others, Karl Mannheim. To this end, Mannheim states:

> From the fact that it (understanding of criminal phenomena) is dependent upon the contributions of several other disciplines and from the multi-factor theory of crime causation follows the need for research in this field often to be planned and carried out by a team of scholars with widely differing backgrounds and interests. (From Mannheim's Comparative Criminology, p. 90.)

comparative criminal justice it is suggested that such an approach would be much more productive than those heretofore utilized.[21] Similarly, students of comparative public administration have begun to utilize interdisciplinary techniques, for there is now the general recognition that nature just does not manifest itself in the form of "public administrative" behavior. In this respect, Siffin writes:

> But the required data for comparative studies are not confined to the content of public administration or even of political science as we have customarily regarded them. Other disciplines are involved. The perspectives, postulates, and techniques of sociology, social psychology, anthropology, economics, and even history are to an extent germane to comparative studies in the realm of government.[22]

Discussing this same general point, within the context of comparative studies, Dahl writes:

> It follows that the study of public administration inevitably must become a much more broadly based discipline, resting not on a narrowly defined knowledge of techniques and processes, but rather extending to the varying historical, sociological, economic and other conditioning factors that give public administration its peculiar stamp in each country.[23]

Given the general theory of reality suggested by this thesis thus far, the trend toward interdisciplinary

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[21]Brecht, op. cit., p. 329.

[22]William J. Siffin (ed.), Toward the Comparative Study of Public Administration (Bloomington: Indiana University, Department of Government, 1957), p. 13.

[23]Robert A. Dahl, "The Science of Public Administration," Public Administration Review, Winter, 1947, p. 11.

cooperation seems to be an inevitable stage in methodological development. The artificial nature of traditional disciplinary distinctions is becoming more and more obvious to scholars of these respective disciplines. At a relatively recent conference of the American Academy of Arts and Sciences (around 1963 or 1964), the members,

> . . . including scientists, scholars in the humanities, creative artists, social scientists, and administrators, found fairly quickly one area of substantial agreement. It was the feeling that the relationship between the sciences and the humanities may well become considerably more strained in the immediate future.

It is interesting to note some of the remarks heard during the conference. Once scientist said, "we may not have seen anything yet of the row that is really going to develop." A social scientist stated,

> I think that the development of the in-between sphere (where both the sciences and the humanities have claims) is going to force many, many fights; they have been developing rapidly, and we have seen only the barest beginning of what is coming.

Another social scientist remarked that such a conflict may cause "the entire intellectual enterprise to come under severe attacks in the next years." Finally, a well known poet and literary critic stated, "There is a mounting sense of tension; I am quite sure that those who have stressed it have been quite right. We are at the beginning of trouble."[24]

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[24]Gerald Holton, Science and Culture: A Study of Cohesive and Disjunctive Forces (Boston: Beacon Press, 1965), pp. XI and XII provide a discussion of this meeting.

It is quite true that we are indeed at the beginning of what may be a tumultuous re-evaluation of the roles, objectives, and "jurisdiction" of the respective disciplines. In view of the theory of reality thus far presented, it could hardly be avoided. The artificiality of traditional disciplinary distinctions is definitely becoming apparent. Llewellyn has said that "The social sciences are not staked out like real estate,"[25] and he was certainly correct, but he did not go far enough. The statement should not have been limited to the social sciences, for all empirical sciences are subject to the same qualification, as are the "events" or particular kinds of behavior with which the various disciplines are concerned. Referring to this basic problem, Brecht aptly observes that

> The twentieth-century use of the terms "philosophy," "science," and "theory," is not definitely settled. It could not be, precisely because the interrelation between philosophy, science, and theory is one of the fundamental problems in the present crisis of scientific thinking.[26]

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[25]Karl N. Llewellyn, "A Realistic Jurisprudence--The Next Step," Essays On Jurisprudence From the Columbia Law Review (New York: Columbia University Press, 1963), p. 183.

[26]Brecht, op. cit., p. 14. Here again, however, Brecht goes on to say that there is a difference between political science and political philosophy. Yet, by the implications of some of Brecht's own statements, this distinction, however "useful" it may seem at present, will become increasingly untenable in the not too distant future.

Summary

As promising as the interdisciplinary approach may seem to be, still, it is this writer's opinion that it will provide little in the way of an ultimate understanding of reality. For the very term "interdisciplinary" suggests the existence of "disciplines," and this is the crux of the problem. In light of the theory of reality suggested here, this writer can only conclude that the development of present interdisciplinary techniques is but a stage in a process which may culminate in the general recognition that the basic process of conceptual extraction, in whatever form, is not conducive to truly effective comprehension of reality. In the end, the process will only lead to a discovery of itself. The physicist-mathematician, Hermann Weyl, suggests this very conclusion in his discussion of a "scientific" analysis of chalk marks on a blackboard.

> As scientists, we might be tempted to argue thus: "As you know, the chalk mark on the blackboard consists of molecules, these are made up of charged and uncharged elementary particles, electrons, neutrons, etc. But when we analyzed what theoretical physics means by such terms, we saw that these physical things dissolve into a symbolism that can be handled according to some rules. The symbols, however, are in the end again concrete signs, written with chalk on the blackboard. You notice the ridiculous circle . . ."[27]

It may indeed be a ridiculous circle, and the recognition of it may be a significant contribution to the "field."

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[27]Gerald Holton, op. cit., p. XXVIII of the Introduction.

Models

The use of models is widespread in the empirical sciences. Their construction and utilization for comparative studies has been academically in vogue for some time now, and it appears as though the frequency with which they are constructed and utilized will continue to increase. In the area of the social sciences one can encounter model societies,[28] model governments,[29] model bureaucracies,[30] model administrative systems,[31] and models of almost every conceivable type of phenomenon which is capable of being conceptualized by the observer. It has been said that the existence of appropriate models is a necessary prerequisite for theories to be generally predictive.[32] It has even been categorically stated that ". . . to assert a theory is to assert a model . . ."[33] With regard to the necessity

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[28]Marion J. Ley Jr., op. cit.

[29]Carl Friedrick and Leonoid Brezinski, Totalitarian Government (Cambridge: Harvard University Press, 1965).

[30]Max Weber is the one that most quickly comes to mind in this respect; his work on bureaucratic structure is well known.

[31]Fred Riggs, "Models in the Comparative Study of Public Administration," Papers in Comparative Public Administration, Special Series: Number 1 (Chicago: Comparative Administration Group, American Society For Public Administration, 1963).

[32]Mary B. Hesse, Models and Analogies in Science (New York: Sheed and Ward, 1963), p. 6.

[33]Ibid., p. 28.

for models, Young states, "In scientific studies it is necessary to isolate certain elements within the complexities and reconstruct them on a simpler model before they can be adequately examined."[34] This is only to show the ubiquity of models in the social sciences, and the present discussion is not intended to be an excursus on them. Rather, it will be limited to a short critique of those aspects of models that, in view of the general orientation of this thesis, raise considerable question with regard to their incorporation into, and the general use of, models.

Above all, models, of necessity, incorporate the use of concepts of all kinds for the purpose of categorization. "They (models) are concepts or systems of concepts (theories) in terms of which data are collected, classified and otherwise analyzed, and conclusions of some sort are formulated."[35] Concept formulation, then, is a necessary condition for model construction, and since the problems posed by the former process have already been discussed in Chapter III, it would be of limited value to rediscuss the issues here. However, it would be useful to undertake a short analysis of the relationship of concept formulation to categorization and information gathering. Simply stated, the proposition is this: Once the concepts have been formulated

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[34]Young, op. cit., p. 111.

[35]Siffin, op. cit., p. 10.

and incorporated into some model structure, there is a tendency to (1) exclude certain types of possibly (quite probably) relevant information, and (2) assume the real existence of relationships which may only be present in the artificial model.

The first effect is an indirect result of the nature of conceptual extraction. The very "selection" of the concepts and models ". . . largely determines, or in any case limits, the outcome of the study."[36] Once the artificial concepts have been formulated, there is a natural tendency to "see" things in a rather dichotomous way; either the phenomenon falls within the realm of the concept or it does not. Such an approach ". . . both excludes and includes. It makes some matter fall inside the field; it makes some fall outside. And the exclusion is almost always arbitrary."[37] The dichotomous nature of categorization resulting from conceptual extraction has been, and will remain, a formidable obstacle for effective comparative analysis. Dichotomous concepts, either alone or within the context of models, have created particularly significant problems for the students of comparative political science and public administration. With regard to this general problem, Fesler writes:

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[36] Ibid.

[37] Karl N. Llewellyn, Columbia Law Review, op. cit., p. 150.

> One of the principle obstacles (to comparative study) is the assumption that we must choose between two alternatives. We need not choose between a strong Federal government and strong state government, between absolute centralization and absolute decentralization, between bureaucratic regimentation and local self-government. The vice of such choices is manifold. Despite elaborate and repeated efforts of social scientists to marshall advantages and disadvantages of each alternative, the choice is bound to be only pseudoscientific. It forces us back on prejudices, the emotional symbolism of words, and sheer acts of faith. Once the choice between alternative positions has been made, there is set up an unscientific major premise that colors and distorts any attempt to apply scientific method to the objective facts of governmental functioning. Instead, science is mustered to rationalize a foregone conclusion.[38]

The second danger in the utilization of conceptual models is that of tacitly assuming the real existence of certain relationships because such relationships may be suggested by the model. It has been suggested by Hesse that, essentially, a model is composed of causal relationships, it is supposed to be an interconnected system of relationships.

> Certain properties are necessary or sufficient conditions for other properties, and the network of causal relations thus established will make the occurrences of one property at least tend, subject to the presence of other properties, to promote or inhibit the occurrence of another.[39]

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[38]James W. Fesler, Area and Administration (University, Alabama: University of Alabama Press, 1949), p. 156.

[39]Hesse, op. cit., p. 87.

But the property relations themselves are perceived in terms of concepts, and the dichotomous nature of so many of the latter is such that the researcher, in deciding whether or not the phenomenon under study fits, may tend to unconsciously attach to that phenomenon certain types of relationships which are part of the conceptual model with which he is working but which may, in fact, not be real. In this respect, Llewellyn writes:

> The other suggestion of a realistic approach (to analysis of socio-legal phenomena) rests on the observation that categories and concepts, once formulated and once they have entered into thought processes, tend to take on an appearance of solidity, and reality and inherent value which has no foundation in experience. More than this : although originally formulated on the model of at least some observed data, they tend, once they have entered into the organization of thinking, both to suggest the presence of corresponding data when these data are not in fact present, and to twist any fresh observations of data into conformity with the terms of the categories.[40]

Braithwaite may have been right when he said that "The price of the employment of models is eternal vigilance."[41]

Summary of Chapter

This chapter concludes the critical discussion of those aspects of "method" that, in this writer's opinion, deserve what may turn out to be a rather agonizing reappraisal. This particular chapter was concerned with the

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[40]Llewellyn, Columbia Law Review, op. cit., p. 171.

[41]Braithwaite, op. cit., p. 93.

theory of causation and its related theory of multiple-causation, with the rise of the interdisciplinary approach to social analysis, and the use of models. As should be apparent at this point, and as was suggested early in the thesis, all of these topics are interrelated, and re-evaluation of one may well necessitate re-evaluation of all. The critique thus far has been based on what appears to be a justified theory of a "wholistic" reality which cannot be fully comprehended in terms of conceptual extractions representing isolated parts of it. The final chapter will assume, for the most part, a rather thorough understanding of the material presented thus far and some of the major implications which may logically accrue therefrom; for the focus of the final chapter will be to investigate some of the ramifications of these logical implications.

CHAPTER V

CONCLUSIONS

This is the final chapter of the thesis. One of the major objectives of it will be to summarily review the major points emphasized in the material thus far, and then to attempt to draw some logical conclusions from that material. In addition, an attempt will be made to relate all of the major points heretofore presented to the general issue of methodology in the social sciences. In short, this chapter will be devoted to clarifying issues and problems with respect to social scientific research methodology, and with raising appropriate questions with regard to those issues and problems.

Implications of a Wholistic Reality

The entire thesis and its major conclusions have been based upon what has been called a "wholistic" reality. In essence, at least for purposes of this thesis, a "wholistic" reality amounts to this. The observer, any observer, is part of a system of infinitely interrelated phenomena, from which he cannot divorce himself for purposes of "objective" analysis. Actually, the term "phenomena" is not quite adequate, since it carries with it a connotation of some spatial and/or temporal finiteness which can be viewed

as not conveying an accurate picture of a truly "wholistic" reality. In view of this, it might be best to say simply that the system is a totality in its own right, it simply "is," it exists, period!

Now it must be emphasized at this point that this writer does not mean to suggest the existence of a thing (a system) apart from the human observer, some "thing" which other observers can "objectively" analyze from "outside." On the contrary, the very point is that there cannot be such a thing apart from the observer, variable perspective notwithstanding, unless the observer himself constructs it. He is actually part of it, and it is part of him; the observer is, in effect, the system. The discussion on quantum physical observation should have suggested this very conclusion.

From this theory of reality, one can draw some logical conclusions which appear to subject a number of aspects of contemporary social scientific methodology to serious question. First, the theory seems to significantly limit the real usefulness of terms like "fact," "objectivity," "value-free" judgments, etc. The following quotation from Brecht's book is an example of that type of statement, those kinds of distinctions, which, in view of a "wholistic" theory such as is presented here, can no more be accepted as a priori valid or even useful.

> . . . the scientist's acceptance of empirical laws of a factual content is based on factual observations, while his acceptance of ultimate value judgments would have to be based on a generalization, not of factual occurrences, but of opinions, and in the typical case, of conflicting opinions.[1] (emphasis supplied)

One should not conjure up an image of some social scientist running around "objectively" collecting the "facts," and then interpreting those "facts" in a "value-free" manner to arrive at the "truth." To the extent that those social scientists are attempting to attain an understanding of a "reality" which is, in effect, part of their own being, to that extent the terms with which they are working have very little utility.

A second conclusion, which is really an extension of the first, is that the process of conceptual extraction which appears to condition one's experience necessitates "value" (in the sense of non-objective) judgments. For the process of conceptual extraction takes place only in relation, and in proportion, to some value relevancy. In this respect, all judgments are "value" judgments, and the distinction between "value" and "objective" ones is ultimately rather artificial. If somehow one could "experience" without recourse to a process of conceptual extraction, a number of these problems would be solved, and "objectivity," "value," etc. would then be seen as not being all

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[1]Brecht, op. cit., p. 270.

that useful.[2] However, since this does not appear to be likely at this stage, suffice it here to clarify, at least to some extent, the relationship between the process of conceptual extraction and such terms as "value," "objectivity," etc.

One might justifiably ask as this point, Do such terms really have any menaing in view of the discussion thus far? Well, certianly they have "meaning," but it does little to recognize just that. The point is not simply that they have meaning, but that their meaning varies with the context within which they are used. In other words, they would have really useful meaning only within the context of some larger, more inclusive, conceptual extraction, and, consequently, variance in meaning might be proportional to conceptual variance. Only in this sense could there be a really useful meaning; the terms would

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[2]It should be noted here that this suggestion is something more than pure conjecture. A number of philosophers, Kant being the first one that comes to mind, do imply that a process such as that of conceptual extraction is definitely obstructive of the ascertainment of "truth." "Pure experiencing," as Kant puts it, is the only "truth."

It might also be noted that some oriental philosophies, "religions" is you wish (although this is often a misnomer), not only recognize the relation between "pure experiencing" and "truth," but their entire philosophical training is directed toward attaining a state of mind that is conducive to such experiencing. In this respect, Zen Buddhism is probably becoming the most popular, and it has been the object of a number of Western philosophical inquiries. See, for example, (1) Erich Fromm, Zen Buddhism and Psychoanalysis (New York: Harper & Row, 1960), (2) Daisetz Toitaro Suzuki, The Essentials of Zen Buddhism (London: Rider, 1963).

probably not reflect such utility when used in reference to observation of some reality apart from a conceptual extraction.

A logical implication here, and what is in fact being suggested, is that the final test of these terms, when used within the context of other conceptual extractions, might be one of simple utility, in conjunction with some kind of intersubjective verification. In other words, such terms would be utilized only to the extent that their use serves some purpose for the user. With regard to this general question of utility, John H. Randall Jr. writes:

> Beliefs are in fact, in the actual procedure of the scientist, judged to be scientifically "warranted," "verified," or proved to be "true" when they give a satisfactory solution to the specific problem to which they have been proposed as an answer. They are validated only when they have succeeded in doing what they are instituted to do. Beliefs, in scientific inquiry, are always designed to perform some determinate function. The objective criterion of their validity is the success of their functioning in the specific way in which they were designed to function. The question always is: Do they actually solve the specific and determinate problem they were designed to solve? This functional test is decisive.[3]

There is, of course, one very significant ramification to this conclusion. If a term such as "truth" or "fact" manifests real utility only within the context of some conceptually extracted world, then in any given

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[3]John Herman Randall, Jr., "History and the Social Sciences," in Wiener, op. cit., p. 315.

case what is "factual" or "true" might very well depend upon a general acceptance or belief by enough of the right kinds of people. To this end, the physicist, Max Plank, astutely writes:

> A new scientific truth does not triumph by convincing its opponents and making them see the light but rather because its opponents eventually die, and a new generation grows up that is familiar with it.[4]

In other words, whether or not such concepts as "truth" and "objectivity" have served their purpose in some way could conceivably turn out to be an intersubjectively arrived at judgment.

A "wholistic" theory of reality also has some rather significant ramifications for the notion of "multiple-causation" and the development of an interdisciplinary approach to social scientific analysis. With regard to the notion of multiple-causation, it may be better to modify it so it reads something like "multiple-influence" or "multiple-relationships." For it appears that the notion of causation, however heuristically useful it may have been previously, will, if used in too strict a fashion, mitigate against a thorough understanding of the inter-relationships which may be involved in social phenomena. It would appear that a more accurate understanding of some phenomenon would result if one looks for "relationships" rather than "causes."

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[4]Max Plank, Scientific Autobiography and Other Papers (New York: Gaynor, 1949), pp. 33-34.

The interdisciplinary approach, although enjoying a great deal of popularity at this time, is not without its problems, most of which accrue from its reliance upon the process of conceptual extraction. Nevertheless, within the context of a larger, conceptually extracted world, such an approach may have limited utility. For example, if one is studying "crime" or "criminal behavior," an interdisciplinary approach to analysis could be helpful, provided one realizes the limitations of dealing with concepts which may or may not accurately represent the interrelationships involved in some phenomenon. As suggested previously, the apotheosis of an interdisciplinary approach to empirical analysis seems to be the result of a tacit recognition of the artificiality and consequent inadequacy of disciplinary distinctions. To the extent that an interdisciplinary approach recognizes such an artificiality, to that extent, at least, it will be a relatively useful tool to the empirical investigator.

The main point with regard to all of these concepts, "objectivity," "value," fact," "causation," "social science," "philosophy," etc., is that one should see them for what they are, human concepts of necessarily uncertain accuracy. The final test of their validity is not how accurately they represent some reality, for one could never know this. In this respect, Einstein had warned that the scientist has to understand reality in the same way a man might if

he had to study a watch without opening it. "He will never be able to compare his picture with the real mechanism, and he cannot even imagine the possibility or the meaning of such a comparison."[5]

Comparison being an impossibility, the final test would again be one of utility; how well they serve the researcher in his conceptually extracted world. In other words, the concepts would have useful meaning only in relation to other concepts which make up the conceptual world of the researcher. The concept of "deviancy," for instance, will be useful only to the extent that one can refer to a working concept of "normality." Since "normality" may vary from culture to culture, and even within cultures with regard to different kinds of acts and circumstances and over a period of time, one must expect that "deviancy" will also be variable. But then, not only will these particular concepts vary in meaning, but in addition, all other concepts, adequate definitions of which are somehow dependent upon the meaning of "deviancy" or "normality," will likewise take on variable meaning; to wit, "morality," "preservation of peace," etc. And the cycle continues indefinitely. Again one notices the possibility of a "ridiculous circle."

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[5]Don K. Price, "The Established Dissenters," in Holton, op. cit., p. 127.

By attaching too much "realness" or "isness" to these concepts, one runs the risk of committing what Whitehead called "the fallacy of misplaced concreteness." In this respect, it might be best to take Brecht's advice with regard to analysis in the area of political justice.

> It is not the nature of things as such, but the human mind and human emotions reacting to the nature of things--functioning as an instrument of observation and reasoning, of predicting consequences, of shaping expectations, and of devising means to promote or counteract their realization, or engaging in metaphytical speculations--which we must examine, if we want to learn more about justice.[6]

These concepts, then, possess no inherent reality, they are referents to a set or sets of relationships which have been "selected," for some reason or another, and in a basically arbitrary manner, to be the object of one's analysis. One should recognize that the real interrelationships may not be adequately represented by, or amenable to manipulation with the ease and in the same manner as, the conceptual extractions with which one deals. The conceptual world may not necessarily be the same as, and is more susceptible to problems of perspective than, the interrelationships which comprise reality.

On this point, William James writes:

> What we say about reality thus depends on the perspective into which we throw it.
>
> Here in the field of sensation, our minds exert a certain arbitary choice. By our

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[6]Brecht, op. cit., p. 146.

> inclusion and omissions we trace the extent; by our emphasis we mark its foreground and its background; by our order we read it in this direction or in that. We receive in short the block of marble, but we carve the statue ourselves.[7]

Developing a Social "Scientific" Method

What is a "scientific" method? Can the social scientists utilize that method which is generally utilized by students of the "pure" sciences? Attention is initially directed to the first question. It should be apparent that one would find oneself on less than sturdy grounds in answering that question in terms of "objectivity," "value-free" judgments, etc.; for it has been suggested that these concepts, upon closer analysis, do not possess the clarity that they need in order to be very meaningful for methodological study. There must be a somewhat different approach to definition. Consequently, Bronislaw Malinowski suggests that, at minimum, a definition of "science" would imply ". . . invariably the existence of general laws, a field for experiment or observation, and last, but not least, a control of academic discourse by practical application."[8] If the existence of general laws implies inexactitude, if the "field" for experiment or observation is not artificially

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[7]William James, "Pragmatism," in Robert M. Hutchins and Mortimer J. Adler, (eds.) The Great Ideas Today (Chicago: Encyclopaedia Britannica, Inc., 1964), p. 564.

[8]Bronislaw Malinowski, op. cit., p. 391.

circumscribed, and if the control of academic discourse by practical application is itself regulated by philosophical thought, then this writer would not have too many objections to such a definition, although, in a positive sense, it is felt that it does not approach the issue in a productive manner.

More than "laws" and "fields," "scientific" seems to imply the existence and utilization of hypotheses or hypothetical systems and logic. To this end, Braithwaite says:

> A scientific system consists of a set of hypotheses which form a deductive system; that is, which is arranged in such a way that from some of the hypotheses as premises all the other hypotheses logically follow.[9]

Similarly, Kaplan states that, "Scientific method is the same everywhere; it is the method of logical inference from data provided and tested by experience."[10]

This approach to a definition of "science" seems more productive than most others, and it is certainly compatible with the general orientation of this thesis. Unfortunately, however, in manifesting such compatibility it becomes susceptible to most of the criticisms heretofore presented. That

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[9]Braithwaite, op. cit., p. 12. For some reason, this definition appears as though it could be substituted in toto for that of a "model." The very interesting ramifications of such a relationship are left to the inferential abilities and interests of the reader!

[10]Abraham Kaplan, "Sociology Learns Mathematical Language," in Wiever, op. cit., p. 399.

is, it is seen as a man-made concept of necessarily uncertain valid utility in terms of the probability of providing some understanding of the system of reality presented here. In this respect, Holton writes:

> I think that we shall have to get accustomed to the idea that we must not look upon science as a "body of knowledge," but rather as a system of hypotheses; that is to say, as a system of guesses or anticipations which in principle cannot be justified, but with which we work as long as they stand up to tests, and of which we are never justified in saying that we know that they are "true" or "more or less certain" or even "probable.
>
> Our justification for these hypotheses is that they have a hold on our imagination and that they help us deal with our experience.[11]

This last point is particularly penetrating, for it both justifies the use, and implies the fallibility, of the "scientific" method in comprehending social reality in its totality. Young's statement that "Science in any form, is no panacea. . ." is certainly appropriate here. Holmes may have rather poetically made the point when he said:

> And the logical method and form flatter that longing for certainty and for repose which is in every human mind. But certainty generally is illusion, and repose is not the destiny of man.[12]

The second question which was posed at the beginning of this section was whether or not the social sciences could utilize the method generally utilized by students in

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[11]Holton, op. cit., p. XXVI of Introduction

[12]O. W. Holmes, "The Path of the Law," Harvard Law Review, 10:8 March 25, 1897, p. 62.

the "pure" sciences. If scientific method is seen as consisting of hypotheses or hypothetical systems from which logical conclusions are inferred or deduced, then such a method can certainly be utilized in the social sciences. But there is a difference between method and technique, and that difference is crucial.

> The confusion of method and technique, and the resultant identification of scientific method with the techniques of physics has hindered the advance of the social sciences not a little. For the problems of sociology are different from those of physics. There are no concepts in social phenomena comparable in simplicity and fruitfulness to the space, time, and mass of classical mechanics; experiments are difficult to perform, and even harder to control; measurement in sociological situations presents special problems from which physics is relatively free.[13]

All the sciences may utilize a method which implies some type of logical reasoning, but one cannot expect to productively use the same techniques for all the sciences or expect to find the same degree of certitude.

There are, then, a number of techniques of research which differ from one "science" to another. The appropriate technique is said to be ". . . determined both by the subjective requirements for comprehension and by the objective nature of the field to be investigated."[14] The technique of probability measurement is a good example. It is now generally accepted, as has been mentioned earlier

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[13]Kaplan, op. cit., p. 399.

[14]Weisheipl,

in this thesis, that the higher the level of conceptualization (sub-atomic, atomic, molecular, cellular, organismic, etc.) the greater the significance which is attached to the role of probability. Hence, it becomes understandable that many of the "pure" sciences can attain a greater degree of certitude in experimentation than most or all of the social sciences; in this respect, one sometimes says that there are "more variables involved," which amounts to the same thing. Discussing the role of probability in social scientific experimentation. Lazerfeld writes:

> There is a general awareness that probability does play a dominant role, explicitly or implicitly, in the study of human behavior. No one believes that behavioral laws can be as simple as the laws of the natural sciences. It is necessary to consider more variables, to begin with; but even then we can only state that certain behavioral combinations are more frequent than others. The predictions of the social scientist will always be probabilistic ones.[15]

Similarly, Brecht writes:

> Yet the possibilities of measurement in the social sciences have remained, and will continue to remain, limited because of the great number of variables, all of which cannot be included in scientific analysis, and insofar as they can, are not all measurable.[16]

Of course, as Hesse states, it is probably true that ". . . observation establishes empirical laws which become

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[15]Lazerfeld, op. cit., p. 9.

[16]Brecht, op. cit., p. 43.

almost as certain as the rational laws, when they are based upon a sufficient number of observations."[17] But what constitutes a "sufficient number of observations" may be more than the social scientists are capable of accurately making, the tool of generalization notwithstanding.*

In conclusion, there does not appear to be any reason why social scientists could not utilize some kind of "scientific" method, if they see that method as consisting of sets of hypotheses or hypothetical systems and logical inferences. If, on the other hand, there is a confusion between this kind of method and particular kinds of techniques, there will probably be a resulting frustration which will not be conducive to rational analysis of some conceptual world.

On Goals (Where To?, Why?)

Given the theory of reality which has been proposed in this thesis, it is apparent that the process of conceptual extraction which is utilized in empirical research will not

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[17]Hesse, op. cit., p. 93.

*With regard to the question of generalization, Poincare accurately states both the main advantage and problem in the following succinct statement.

> Thus, thanks to generalization, each fact observed enables us to forsee a great many others; only we must not forget that the first alone is certain; that all others are merely probable.

See Poincare, op. cit., p. 33.

lead one to an understanding of the system of which one is a part. For some reason, most empirical scientists, particularly social scientists, do not accept this position and its implications; or, as is more often the case, they explicitly accept it but implicitly reject it. It is often pointed out by the explicit opponents of this position that one need only look at the world around oneself to see tangible evidence that the sciences, both "pure" and "social," have made tremendous "advancements" toward the understanding of this reality and its manipulation; the evidence is everywhere.

The fact is, however, that such a statement is, at best, of dubious accuracy. For although one may be able to say that the social sciences have wrought change, that is not the same as saying that those sciences have wrought advancement in terms of increased understanding of some reality apart from the one within which they themselves exist and, in most cases, construct. The "advancement" about which such scientists speak implies, of course, some logical progression from X to Y, Y being "better" than X. The "improvement" of the conditions of social existence and the "increased" understanding of reality have coincided with, indeed are said to have been attributable to, a concomitant "improvement" of research methodology. In this particular case, the "progression" has often been implied as going from pure philosophy, to empirical

observation and categorization, to hypothetical and logical reasoning, to, finally, pure mathematical reasoning.

This writer suggests that the "progression," the "advancement," and the "increased understanding," amount only to change, and one cannot be sure (even in a probabilistic sense) that it amounts to anything else. As Holton writes:

> In the empirical sciences we are far from able to prove that we have been approaching an increasing understanding of the type that characterized the development of say some branches of mathematics. Our interests and tools change, but not in a linear and inevitable way. For example, the historical development from organismic science to a mechanistic and then to the mathematical style, could have taken place in the opposite direction. And the ontological status of scientific knowledge itself has been turned completely upside down since the beginning of the twentieth century. The experimental detail is now not simply the token of a real world; on the contrary, it is all that we can be more or less sure about at the moment.[18]

The very fact that new problems constantly replace old ones, or indeed are sometimes created by the "solutions" to other ones, would seem to make this conclusion at least worthy of serious consideration. If history suggests anything, it suggests that we have not been approaching an "increased understanding" of the basic issues involved in human existence; we have not even been able to agree on what they are!

It appears as though there are a number of reasons for this. One of them is that not enough people have

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[18]Holton, op. cit., pp. XXVI-XXVII of Introduction.

stopped to ask Why? Why study social phenomena? Why develop a method for comparative police analysis? If, as Macridis suggests with regard to a general comparative method, such a method will at least "identify uniqueness,"[19] then one should recognize that it will identify "uniqueness" only within the confines of one's conceptual world. Even with this recogniztion, there is still the question of Why?; one cannot escape, although one can hide, from this Why? system of questions. Since the present test of empirical methodology is its utility in someone's conceptual world, then one tacitly admits that much of one's thought and research is devoted to what amounts to "game theory" in some conceptual world. If this is the case, then it is here suggested that there needs to be some re-evaluating; for many, indeed most, social problems will not be "solved," or even identified, in this manner, "advancements" notwithstanding. In this respect, Ackerman writes:

> At the root of these problems is the preoccupation of the twentieth century SCIENTIA with function and process, and with the nature of observation, which is a cause as well as a result of the insecurities of our time. It has brought extraordinary progress in science, and radical change in the arts, but it has not sought or provided solutions to the major dilemna of human existence and behavior, nor even provided the means of assessing the value of its own achievements.[20]

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[19]Macridis and Brown, op. cit., pp. 4-5.

[20]James S. Ackerman, "On Scientia," in Holton, ob. cit., p. 22.

On the other hand, if pressed far enough, the social scientist would probably answer the "Why?" with some statement like, "It will help us understnad social reality (as opposed to some purely conceptual reality)." There is a feeling that some "it," apart from the observer, can be understood by the present social scientific method with its reliance on the process of conceptual extraction. With regard to this, Holton writes:

> The paths to an understanding of nature may be infinite . . ., and each of these paths is expected to have difficult but not insurmountable barriers. But all the paths have been vaguely thought to lead to a goal, an understanding of one nature, a delimited though no doubt complex rational corpus which some day a man's mind would be able to make his own (as the layman today says, somewhat frightened, "one great formula" that tells everything there is to know about nature).[21]

This, however, as has been implied by the entire thesis, is not possible by the process which is commonly utilized to comprehend social phenomena. All of the present social scientific approaches to methodology (be it comparative or otherwise) are based upon this process of comprehension, and, therefore, to that extent may not provide increased understanding of social reality; so whence the rationale? It is quite possible that at the innermost chamber of the social maze toward which present social scientific analysis is directed there will be nothing. In effect, one may well find oneself facing

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[21]Holton, op. cit., p. XXIV of the Introduction.

one's own "chalk marks on the blackboard," ("notice the ridiculous circle. . . "); and that might well constitute all the reality which one observer can ever really know or understand. If at any point in the series of "WHY?" questions a justifiable and logical answer or explanation cannot be given, that is the point from which analytic endeavor should proceed and to which it should be directed! If there is no "it" to discover, then it may be much more profitable to focus one's intellect on the construction of a suitable "it" which will be adequate for some small spatial-temporal coordinate of social reality. Since this construction is, in fact, what we may have been doing all the time, a most significant contribution to any "field" would simply be the recognition of it.

BIBLIOGRAPHY

BIBLIOGRAPHY

Anshen, Ruth Nanda. "Perspectives in Humanism: The Future of Tradition," in Theodosius Dobzhansky, The Biology of Ultimate Concern. New York: The New American Library, 1967.

Baum, Bernard H. Decentralization of Authority in a Bureaucracy. Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1961.

Braithwaite, Richard Bevan. Scientific Explanation: A Study of the Functions of Theory, Probability and Law in Science. New York: Harper & Row, 1953.

Brecht, Arnold. Political Theory: The Foundations of Twentieth-Century Political Thought. Princteon, N. J.: Princeton University Press, 1959.

Chase, Stuart. The Proper Study of Mankind. New York: Harper & Row, 1962.

Coatman, John. Police. London: Oxford University Press, 1959.

Cohen, Morris R. "The Place of Logic in the Law," Introduction to Law: Selected Essays Reprinted from the Harvard Law Review. Cambridge, Mass.: The Harvard Law Review Assoc., 1965.

Dahl, Robert A. "The Science of Public Administration," Public Administration Review. Winter, 1947.

Davivongsa, Shalonglap. "The Rationale for Centralization in the Thai Police Department." Master's Thesis, Michigan State University, East Lansing, 1965.

Devlin, J. D. Police Procedure, Administration, and Organization. London: Butterworths, 1966.

Dewey, John. Logic, The Theory of Inquiry. New York: H. Holt & Co., 1938.

_____. Problems of Men. New York: Philosophical Library, 1946.

Duhem, Pierre. "Mathematical Deduction and Physical Theory," Readings in Philosophy of Science: Introduction to the Foundations and Cultural Aspects of the Sciences. Edited by P. P. Wiener. New York: Charles Scribner's Sons, 1953.

Fesler, James W. Area and Administration. University, Alabama: University of Alabama Press, 1949.

Flannigan, William, and Fogelman, Edwin. In Martindale, Functionalism in the Social Sciences. Philadelphia: American Academy of Political and Social Science, February, 1965.

Fortes, M., and Evans-Pritchard, E. E. (eds.). African Political Systems. London: Oxford University Press for the International African Institute, 1946.

Fosdick, Raymond B. European Police Systems. New York: The Century Co., 1915.

Francis, Roy B. The Rhetoric of Science: A Methodological Discussion of the Two-by-Two Table. Minneapolis: University of Minnesota Press, 1961.

Fredrick, Carl, and Brezinski, Leonard. Totalitarian Government. Cambridge: Harvard University Press, 1965.

Fromm, Erich, and Suzuki, D. T. Zen Buddhism and Psychoanalysis. New York: Harper & Row, 1960.

Galvin, Raymond T. "The Study of Comparative Police Administration." Paper presented at the American Society of Criminology Meeting, Berkeley, California, Dec., 1965.

Gluckman, Max. Politics, Law, and Ritual in Tribal Society. Chicago: Aldine Publishing Co., 1965.

Gulick, Luther. "Note on the Theory of Organization." Paper on the Science of Administration. New York: Institute of Public Administration, Columbia University, 1937.

Harary, Frank, Norman, Robert A., and Cartwright, Darwin. Structural Models: An Introduction to the Theory of Directed Graphs. New York: John Wiley & Sons, Inc., 1965.

Harre, R. Matter and Method. New York: St. Martin's Press, 1964.

Hesse, Mary B. Models and Analysis in Science. New York: Sheed & Ward, 1963.

Holmes, Oliver W. "The Path of the Law," Harvard Law Review, Volume X, #8, March 25, 1897.

Holton, Gerald. Science and Culture: A Study of Cohesive and Disjunctive Forces. Boston: Beacon Press, 1965.

Hutchins, Robert M., and Adler, Mortimer J. The Great Ideas Today. Chicago: Encyclopaedia Britannica, Inc., 1964.

Laswell, Harold D. "The Policy Orientation," The Policy Science, Recent Developments in Scope and Method. H. D. Caawell and D. Lerner, eds. Stanford, 1951.

Lazerfeld, Paul F. Mathematical Thinking in the Social Sciences. Glencoe, Ill.: The Free Press, 1954.

Linssen, Robert. Living Zen. New York: Macmillan, 1958.

Llewellyn, Karl N. "A Realistic Jurisprudence--The Next Step." Essays on Jurisprudence from the Columbia Law Review. New York: Columbia University Press, 1963, pp. 149-183.

_____, and Hoebel, E. A. The Cheyenne Way. Norman, Oklahoma: University of Oklahoma Press, 1941.

Macridis, Roy C., and Brown, Bernard E. Comparative Politics: Notes and Readings. Homewood, Ill.: The Dorsey Press, 1964.

Mannheim, Hermann. Comparative Criminology. Boston: Houghton Mifflin, 1965.

Martindale, Don (ed.). Functionalism in the Social Sciences. Philadelphia: American Academy of Political and Social Science, February, 1965.

Mayer, Joseph. Social Science Principles in Light of Scientific Method. Durham, North Carolina: Duke University Press.

Morgenbesser, Sidney. Philosophy of Science Today. New York: Basic Books, Inc.

O'Brien, G. The Australian Police Forces. Melbourne: Oxford University Press, 1960.

Planck, Max. Scientific Autobiography and Other Papers. New York: Gaynor, 1949.

Quine, Willard Van Orman. From a Logical Point of View: Nine Logical Philosophical Essays. Cambridge: Harvard University Press, 1953.

Read, Herbert. To Hell with Culture and Other Essays of Art and Society. New York: Schocken Books, 1963.

Riggs, Fred W. "Models in the Comparative Study of Public Administration," Papers in Comparative Public Administration, Special Series: #1. Chicago: Comparative Administration Group, American Society For Public Administration, 1963.

Schrodinger, Erwin. "What is Life," The Great Ideas Today, Robert M. Hutchins and Mortimer J. Adler, eds. Chicago: Encyclopaedia Britannica, Inc., 1967.

Shannon, Jasper B. (ed.). The Study of Comparative Societies. New York: Appleton-Century-Crofts, Inc., 1949.

Siffin, William J. (ed.). Toward the Comparative Study of Public Administration. Bloomington: Indiana University, Department of Government, 1957.

Smith, Vincent E. (ed.). The Logic of Science. New York: St. John's University Press, 1964.

Suzuki, Daisetz Toitaro. The Essentials of Zen Buddhism. London: Rider, 1963.

Swartz, Marc J., Turner, Victor W., and Tuden, Arthur. Political Anthropology. Chicago: Aldine Publishing Co., 1966.

Waldo, Dwight. Political Science in the United States of America; A Trend Report. Paris: United Nations Educational, Scientific, and Cultural Organization, 1956.

Whitehead, A. N. Science in the Modern World. New York: The Free Press, 1925.

Wiener, Philip P. Readings in Philosophy of Science: Introduction to the Foundations and Cultural Aspects of the Sciences. New York: Charles Scribner's Sons, 1953.

Wood, Arthur A. Natal, Past and Present. Devon, England: A. H. Stockwell, 1962.

Young, Pauline V. Scientific Social Surveys and Research. Englewood Cliffs, New Jersey: Prentice Hall, Inc., 1966.