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A PHILOSOPHICAL EVALUATION OF JEROME S. BRUNER'S PSYCHOLOGY
IN RELATION TO THE THEORY AND PRACTICE
OF SOCIAL STUDIES TEACHING

DISSERTATION

Presented in Partial Fulfillment of the Requirements for
the Degree Doctor of Philosophy in the Graduate
School of The Ohio State University

By
William Howard Paynter, B.S. Ed., M.A.

* * * * *

The Ohio State University
1974

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# TABLE OF CONTENTS

<p>| ACKNOWLEDGMENTS                                      | ii    |
| VITA                                                | iii   |
| Chapter                                             |       |
| I. SCOPE, PURPOSE, AND METHODS OF THE STUDY          | 1     |
| Introductory                                        |       |
| Historical Background                               |       |
| Purpose and Methods of the Study                    |       |
| Significance of the Methods of the                 |       |
| Study for the Philosophy of Education               |       |
| Concluding Summary                                  |       |
| II. BRUNERIAN COGNITIVE PSYCHOLOGY                   | 28    |
| Introductory                                        |       |
| Methodological Orientation                          |       |
| Brunerian Work on Perception                        |       |
| Early Work on Cognition                             |       |
| Later Work on Cognition                             |       |
| Concluding Summary                                  |       |
| III. EPISTEMOLOGICAL CONSIDERATIONS                  | 79    |
| Introductory                                        |       |
| Features of Analytic Philosophy                     |       |
| Logical Pattern in Explanation                      |       |
| Covering-law Model                                  |       |
| Probablistic Model                                  |       |
| Functional Model                                    |       |
| Genetic Model                                        |       |
| Enthymematic Form                                   |       |
| Logical Pattern in Laws                             |       |
| Laws of Kinds                                       |       |
| Laws of Invariable Sequence                         |       |
| Statistical Laws                                    |       |
| Laws of Functional Dependence                       |       |</p>
<table>
<thead>
<tr>
<th>Chapter</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>Linkage to Observation</td>
<td></td>
</tr>
<tr>
<td>Concluding Summary</td>
<td></td>
</tr>
<tr>
<td>IV. EPISTEMOLOGICAL CRITIQUE OF BRUNERIAN COGNITIVE PSYCHOLOGY</td>
<td>119</td>
</tr>
<tr>
<td>Introductory</td>
<td></td>
</tr>
<tr>
<td>Two Groups of Related Philosophical Issues</td>
<td></td>
</tr>
<tr>
<td>The Functions of Categorizing</td>
<td></td>
</tr>
<tr>
<td>The Conditions of Generic Categorizing</td>
<td></td>
</tr>
<tr>
<td>Early Work Contrasted With Later</td>
<td></td>
</tr>
<tr>
<td>&quot;Laws&quot; of Representation</td>
<td></td>
</tr>
<tr>
<td>Identity as a Condition of &quot;Conservation&quot;</td>
<td></td>
</tr>
<tr>
<td>Representational Conflict and &quot;Growth&quot;</td>
<td></td>
</tr>
<tr>
<td>Concluding Summary</td>
<td></td>
</tr>
<tr>
<td>V. SOME PEDAGOGICAL IMPLICATIONS OF THE STUDY</td>
<td>152</td>
</tr>
<tr>
<td>Introductory</td>
<td></td>
</tr>
<tr>
<td>General Pedagogical Considerations</td>
<td></td>
</tr>
<tr>
<td>Consideration of Man--A Course of Study</td>
<td></td>
</tr>
<tr>
<td>Concluding Summary</td>
<td></td>
</tr>
<tr>
<td>BIBLIOGRAPHY</td>
<td>161</td>
</tr>
</tbody>
</table>
CHAPTER I

SCOPE, PURPOSE, AND METHODS OF THE STUDY

Introductory

In this chapter, the nature of the scope, purpose, and methods of the present study will be discussed. A very brief sketch of historical background relevant to the study will be offered first. Then, a precise statement of the purpose and methods of the study will be attempted. Finally, the significance of the methods of the study for the philosophy of education will be discussed.

Historical Background

Among the more distinctive features of the literature of social studies education in the 1960s and early 1970s has been the expressions in it by the profession with regard to the desirability of change. Now let it be said at once that, historically speaking, generally those who have contributed to the literature of social studies education have nearly always been a malcontented bunch, so far as the educational status quo is concerned. And, perhaps, they have been so with good reason. For candor would compel one
to say that what has ordinarily passed for social studies instruction in America's schools and colleges induces at least a mild sense of despair in the sophisticated observer, rather than an overwhelming sense of euphoria.\footnote{The present writer's despair with social studies instruction stems primarily from the fact that there is a lack of basic instructional research findings relevant to the task of teaching social studies. Given the relative paucity of significant research findings, in a sense the social studies teacher is forced to operate almost blind in the classroom. On these matters, see especially, Lawrence E. Metcalf, "Research on Teaching the Social Studies," in N. L. Gage, ed., Handbook of Research on Teaching (Chicago: Rand McNally Co., 1965), pp. 929-965, and James P. Shaver and A. Guy Larkins, "Research on Teaching Social Studies," in Robert M. Travers, ed., Second Handbook of Research on Teaching (Chicago: Rand McNally Co., 1973), pp. 1243-1262. But, see also Egon G. Cuba, "The Failure of Educational Evaluation," in Carol H. Weiss, ed., Evaluating Action Programs (Boston: Allyn and Bacon, 1972), pp. 250-266.} Couple this with the traditional optimism regarding the possibility of social change characteristic of unalienated segments of the American people, and one might venture the hypothesis that, if social studies literature were examined, say, for any decade since 1900, there would be a high probability of finding a "change motif." What is distinctive about the literature of the 1960s and early 1970s, then, probably is largely a matter of degree, not of kind.

It is the urgency, the vigor, the enthusiasm, of writers in the past ten or so years that needs to be noted. Perhaps, a sense of this feature of social studies
literature can be grasped by the inspection of a few sentences from an article written in 1965 by the late Hilda Taba.

Education is going through a truly revolutionary period. Research and experimentation is moving along on all dimensions of the educational process. A massive revision of curriculum is under way which is changing, not only the method of organizing curriculum content, but also the very conceptions of content and the perspective toward what is important in it. While the area of social sciences has been a latecomer to the scene of experimentation and study, the revolution in this area will probably be even more pronounced than it has been in science and mathematics.

Studies of learning have shifted from the laboratory to the classroom and are producing a new and a more dynamic view of learning. . . . Such studies should eventually produce both an articulated theory of instruction and a concept of teaching which should make it possible to plan instructional processes scientifically and to direct these strategies to differentiated targets of learning. . . .

No doubt, one could quarrel with Professor Taba about her interpretations of the recent educational scene. Whether the term "revolutionary" is appropriate to the context in which she uses it, for example, is debatable. Also, whether research could possibly move on all dimensions of "the educational process," simultaneously, is open to question. But, even if she is guilty of overstatement--indeed, perhaps because she is guilty of overstatement--Professor Taba certainly has caught the flavor of the excitement in the

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social studies literature of the 1960s and early 1970s in a delightful way.3

Even to those who survey the recent educational literature in a cursory manner, it becomes obvious that one of the writers in the forefront of the movement for change in the social studies is Jerome S. Bruner. In fact, it could be argued with some force that Bruner's studies of human cognition, together with those of his associates at the Harvard Center for Cognitive Studies, have served to energize the ferment of the 1960s and early 1970s. But, if the work of Bruner and his associates has been considerably influential, it has also been considerably controversial—at least in its pedagogical implications.

The pedagogical controversy surrounding Bruner's work seems to center mainly on his notion of "structure" in fields of knowledge. In one of his smaller volumes, The

Process of Education, Bruner broadly outlined his belief in
the pedagogical importance of this notion.

Teaching specific topics or skills without making clear their context in the broader fundamental structure of a field of knowledge is uneconomical in several deep senses. In the first place, such teaching makes it exceedingly difficult for the student to generalize from what he has learned to what he will encounter later. In the second place, learning that has fallen short of a grasp of general principles has little reward in terms of intellectual excitement. The best way to create interest in a subject is to render it worth knowing, which means to make the knowledge gained usable in one's thinking beyond the situation in which the learning has occurred. Third, knowledge one has acquired without sufficient structure to tie it together is knowledge that is likely to be forgotten.4

Curriculum workers sympathetic with Bruner's views have used them to serve as a rationale for, among other things, a "retooling" of subject matter, to use an interesting metaphor from a recent work, so as to "... illuminate base structure, and to insure that knowledge which generates knowledge takes priority over knowledge which does not."5

Bruner's critics, on the other hand, have attacked his notion of structure on various grounds. One critic, for example, points out that although the notion has in recent years gained immense stature as an educational "watchword"


its meaning is susceptible of manifold interpretations. Another critic boldly states that Bruner fails to give a "clear and specific definition" of the notion of structure, and that, in any event, Bruner's notion is unsound. He then proceeds to suggest his own definition, and to carry on from there. Still another critic, this one perhaps the most forceful and cogent in the debate, feels that a serious consideration of Bruner's notion of structure by those who build curricula would ultimately lead to the removal of history from the primary and secondary social studies. This, because (1) history seems to have no structure in the relevant sense, and (2) even if someone could formulate such a structure of history, it is doubtful that competent students of history would generally agree as to either its veracity or utility. This critic feels, moreover, that there are good reasons for keeping history in the social studies curriculum, and that Bruner's notion of structure may be basically inappropriate to the social studies.

Although the reaction to Bruner's work on cognition

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has been mixed in American pedagogical circles, the reaction of his fellow psychologists throughout the world has generally been quite favorable. A multitude of reviews of his work and that of his associates acknowledge its importance in breaking new and reopening old "psychological ground." Psychologists like Frederic Bartlett, Fritz Heider, and Jean Piaget have extolled Bruner's work as brilliant and original. This is not to say, however, that the work of Bruner and his associates has totally escaped criticism from psychologists. A discipline which contains within it the views of scholars as widely divergent as does psychology makes it almost impossible to please everybody. But, even the most critical of Bruner's colleagues who are interested in his work acknowledge merit in what he has done. Consider, for example, the stinging 1957 review by David Rapaport of Bruner, et al., *A Study of Thinking*.

The bold, the novel, and the stimulating are drowned in this volume not only by an eclectic tendency to serve too many gods (information theory, game theory, ego psychology, Brunswik's probabilistic ecology, etc.), by an ostentatious erudition, and by catering to what is modish, but also by a curious blend of the forthright, the didactic, the circumstantially cumbersome, the outright repetitious, and the preciously urbane. The authors display an uncannily sure hand at "snatching defeat from victory."9

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Moreover, and finally, many psychologists aware of Bruner's work and generally sympathetic with it seem to feel that it has substantial pedagogical implications.¹⁰

**Purpose and Methods of the Study**

The present writer wishes to plunge into the highly controversial discussion among educators as to the pedagogical utility of Bruner's psychology. Thus, the fundamental purpose of the present study is to determine in an explicitly rational way whether or not the theories of human cognition formulated by Jerome S. Bruner and his associates at Harvard University constitute a helpful guide for the social studies teacher in his attempts to teach youngsters and to build curricula for the purpose of teaching. The expression "explicitly rational way" in this statement of purpose is, of course, ambiguous. Accordingly, the following remarks are offered to clarify it.

Basically, in this study we will attempt to find out whether the psychological explanations of human cognition formulated by Bruner and his associates are true. Knowledge

as regards the truth (or scientific warrant) of Bruner's psychology is crucial to the social studies teacher who wishes to use the work of Bruner and his associates as a guide to teaching and curriculum building. For, plainly, if the psychological explanations of Bruner and his associates are false, or if their present formulation is so indeterminate as to preclude ascertaining their credibility, then they would not form a firm foundation on which to base either teaching or curriculum.

For an educator to raise questions about the scientific warrant of the psychological explanations of a respected psychologist and his associates is ostensibly outrageous, for at least two reasons. First, such questions impugn the scientific competence of the psychologists. And, second, such questions cast doubt on psychology's status as a scientific discipline. But, in view of the substantive and methodological controversies which currently plague psychology, it seems appropriate that such questions be raised.

It is beyond the scope of the present study to go into much detail regarding the substantive and methodological controversies of contemporary psychology. However, details may be found in, e.g., A. J. Ayer, *Man as a Subject for Science* (London: The Athlone Press), and Robert Brown, *Explanation in Social Science* (Chicago: Aldine Publishing Co., 1963).
consider the following rough sketch of the issues involved.

Broadly conceived, it is the purpose of psychology to attempt in a scientific way to explain and predict the behavior of organisms. It would not be a gross oversimplification to say that modern psychologists generally assume that any given instant of behavior is the joint product of two sorts of factors: (1) environmental stimuli currently impinging upon the sensory receptors of the organism, and (2) internal states of the organism. Moreover, the relative contribution of these two sets of variables to the determination of behavior probably varies greatly for behavior of different kinds. While knowledge of local stimulus conditions contributes substantially to accurate prediction of some classes of instinctive and conditioned behavior, in the case of verbal (i.e., linguistic) behavior knowledge of the stimulus situation often affords only slight grounds for predicting what an organism will do.

A characteristic feature of academic (as against clinical) psychology, considered as a discipline, is that nearly all of the theories dealing with human behavior within the discipline are marked by the coexistence of competing theories. There is, for example, not just one theory of human cognition, but at least four. Each of the four, moreover, represents a radically different choice of what is relevant and important in the human behavior to be described and explained. There is not one theory of human
learning, but a half dozen. There is not one theory of human personality, but many. And so the story goes. Academic psychology, like its sister social scientific disciplines, is, indeed, marked by schools of thought, each distinguished by alternative assumptions for inquiry made and mode of analysis employed, and each of which selects from the intimidating complexities of subject matter the small fraction of the whole with which it can deal.

Furthermore, the recent history of academic psychology has been marked by a methodological controversy regarding the legitimacy of employing, or even acknowledging the existence of, internal state variables in describing and explaining the behavior of human organisms. For reasons that need not be discussed here, those psychologists who have held that the use of internal state variables is illegitimate have, for the most part until very recently, held the upper hand in the argument. But many modern psychologists have "liberalized" their attitudes in regard to this matter. This new flexibility has, no doubt, resulted from the realization on the part of many psychologists that, so far as the behavior of human beings is concerned, the explanatory import of most generally

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12Cogent summaries of the debate to which I am referring may be found in the essays by Sigmund Koch and R. E. MacLeod in T. W. Wann, ed., Behaviorism and Phenomenology (Chicago: The University of Chicago Press, 1964), pp. 1-45 and 47-78, respectively.
accepted psychological theories is trivial. Yet, there is still another perhaps more important reason. It is the evaporation of previously firm philosophical support for a methodological position which holds that the employment of internal state variables in theory formulation is illegitimate. In regard to this, Sigmund Koch has argued that, throughout the history of psychology, psychologists have looked to external sources in the scholarly culture, such as the natural sciences and the philosophy of science, for a sense of direction; and that they have typically embraced policies long out of date in those very sources. He remarks of contemporary psychology that,

. . . The emerging redefinition of knowledge is already at a phase, in its understanding of the particularities of inquiry, which renders markedly obsolete that view of science still regulative of inquiring practice in psychology. This can be said in utter literalness, for the view in question was imported, with undisguised gratitude, from the philosophy of science and related sources some three decades ago but, while remaining more or less congealed in psychology, was subjected to such attrition in the areas of its origin that in those areas it can no longer properly be said to exist. Psychology is thus in the unenviable position of standing on philosophical foundations which began to be vacated by philosophy almost as soon as the former had borrowed them.¹³

Given the conditions of academic psychology just described, it becomes apparent that the task of the evaluation of any psychological explanation about human

subjects as to its credibility, as to its truth, is very difficult. However, such a task seems not to be impossible. And, in the judgment of the present writer, for psychological explanations with much potential educational significance, it is essential that such evaluation be attempted. The main question, however, is "How does one go about it?"

One sort of question which may be asked about any explanation put forward in psychology is "Has it been well tested?" Plainly, since psychology is an empirical science, this question calls for an assessment of the quality of experimental and other kinds of evidence available for the explanation. Moreover, this question calls for the application of the criterion of consistency. In other words, the relationship between the explanation and the relevant aspects of the total body of knowledge of psychology needs consideration. It is a logical necessity that the explanation be either consistent or inconsistent with the relevant aspects of its parent discipline. If the explanation is inconsistent, then two alternatives present themselves: either (1) the relevant aspects of the parent discipline must be revised so as to maintain consistency, or (2) the explanation must be rejected. A decision to adopt the first alternative, needless to say, would necessitate an evaluation of the evidential support for those parts of the parent
discipline which would need revision if the explanation were accepted as warranted.

Prefactory to asking whether an explanation has been well tested, however, a less obvious question of an epistemological nature needs to be put: "Does the explanation possess the logical qualifications necessary for even being submitted for serious testing?" This question calls for an enumeration of the criteria for testability of explanations in psychology (or any other empirical science, for that matter), and a judgment as to whether the explanation meets these standards. Since the canons for the testability of an empirical scientific explanation are to be found in the literature of the philosophy of science, getting an answer to this question would entail ferreting out the canons and making them explicit. Then, the psychological explanation would need to be measured against the epistemological canons. In doing this it would, of course, be absurdly pedantic to apply to a psychological explanation the yardstick of rigor and precision necessary for explanations in current mathematical and experimental physics. Proper allowance would need to be made for the notorious difficulties encountered in all inquiries into distinctively human conduct, and for what is perhaps an inevitable fuzziness of all generalizations about human behavior.

It is the epistemological question that the present writer wishes to put regarding the psychological work of
Bruner and his associates. Does Bruner's theoretical work on human cognition possess the logical qualifications necessary for being submitted to serious empirical testing? The question is deceptively simple. Getting an answer to it will occupy us throughout a large portion of this study.

As a practical matter, in order to obtain an answer to the epistemological question it will first be necessary to state the psychological explanations of Bruner and his associates in a succinct form. This is necessitated because Bruner's psychological explanations of human cognition are scattered across some five major and several important minor works. (The task of succinctly stating Brunerian theory will occupy us in Chapter II.)

Having stated the theoretical work of Bruner and his associates in a succinct form, a survey of relevant philosophical literature and an enumeration of canons for scientific explanation will be offered (Chapter III). Then an evaluation of the testability of the Brunerian theoretical work will be attempted (Chapter IV). Finally, (in Chapter V) we will turn to an assessment of the potential pedagogical utility of the Brunerian psychological explanations for social studies, in view of the results of the testability evaluation. Thus, the major tasks of the present study have been delineated.
Significance of the Methods of the Study for the Philosophy of Education

Before we attack the major tasks of the present study, it is worth noting the significance for the philosophy of education of the epistemological methods that will be here employed.

Contemporary philosophers of education have been engaged in a good deal of soul-searching regarding the nature of their discipline and the relationships, if any, that hold between it and the general discipline of philosophy, the discipline of education, and educational practice. Attempts at answering questions such as "How philosophical can philosophy of education be?" and "Is a philosophy of education necessary?" are typical of their current discourse.

What exactly has prompted all this self-assessment at this point in history is not entirely clear. However, one cannot help but suspect that it is related to a major trend in the type of philosophizing operant in the twentieth century philosophy of the English-speaking world--viz., "analysis." Philosophy in most American universities today, certainly in the British ones, has virtually come to mean the analytic enterprise. It seems that for the younger generation of students of philosophy, at any rate, the ideas of the analytic movement are thought to be by far the most exciting and promising.
Two characteristic features of analytic philosophy are germane to the problem we are now considering. The first is that there is substantial ambivalence on the part of analytic philosophers with regard to whether or not their movement constitutes a radical departure in approach from "traditional" philosophy. Some analysts are prone to contrast their movement as "revolutionary" with respect to "traditional" philosophy. Others seem to hold that their approach is similar to "traditional" philosophy in substance, if not in form. In any event, there can be no doubt that their movement is different. For example, "traditional" philosophers handled, say, the problem of truth, by attempting rather straightforwardly to answer questions such as, "What is truth?" and "What, if any, relations hold between the world of experience (i.e., extralinguistic facts) and true beliefs or expressions?" There emerged from these attempts at answering such questions broad conceptions of truth such as the idealistic and the pragmatic. Analysts, on the other hand, frame different questions regarding these matters. Take ordinary language analysts as an illustration. In the words of one of the most able, the late J. L. Austin, they tend to think that

... our common stock of words embodies all the distinctions men have found worth drawing, and the connections they have found worth marking, in the lifetimes of many generations: these surely are likely to be more numerous, more sound, since they have stood up to the long test of the survival of
the fittest, and more subtle, at least in all ordinary and reasonably practical matters, than any you or I are likely to think up in our armchairs of an afternoon—the most favored alternative method.14

They also believe that the most promising way to become clear about what our basic concepts are is to determine as accurately as possible the rules of use of words that express such concepts. That is, their aim, as Austin briefly suggests, is to discover "... what we should say when and so why and what we should mean..."15

The way to achieve clarity about "the nature of truth," as the philosophers of an earlier generation would phrase it, is, if this program is adopted, to examine carefully the rules of use for "true" and the words related to its meaning (e.g., "proposition" or "assert"). The investigations needed to achieve this go far beyond conventional lexicography; for instance, it is necessary to determine whether there are absolutely clear or so-called paradigm situations, in which refusal to apply the word "true" would be a sign merely of linguistic ignorance or ineptitude. That the achievement of their aim is much more difficult than would be anticipated is shown by the disputes in the ranks of ordinary language philosophers about the correct outcomes of such linguistic research. Moreover, these disputes are

15Ibid., p. 129.
often intensively technical to the point of being esoteric. 16

At this point, the discussion of the first characteristic feature of the analytic movement (how its approach differs from that of "traditional" philosophy) dissolves into a discussion of the second—which is that the human content and cultural significance of analytic philosophy is far from clear. Western philosophy in styles other than the analytic may not get one very far, but its relevance to man and his practical problems seems unassailable; analytic philosophy may be credited with dramatic achievements (particularly in the philosophy of science), but just what significance attaches to such progressively technical argument is open to debate.

To understand why the significance of analytic philosophy is questionable, two things must be grasped. It first must be realized that initially the over-all aim of analytic philosophy, at least insofar as the keen concern about ordinary linguistic usage is involved with it, had a metaphysical target. Which is but to say that the late Ludwig Wittgenstein, generally acknowledged to be among the most important of the founders of the movement, called attention in his later writings to the crucial role the

16 The esoteric flavor of such disputes is aptly demonstrated in P. F. Strawson, ed., Philosophical Logic (London: Oxford University Press), passim.
improper use of ordinary words played in creating perplexity about most, if indeed not all, metaphysical issues. On his view, careful attention to the rules of use of ordinary words might serve a "therapeutic" function, in that it would cause such issues, as it were, to vanish; problematic issues stated properly would cease to be problematic. (You really do not argue with a recalcitrant metaphysician, you "cure" him.) That this line of argument has had a great appeal among philosophers is shown by the fact that a substantial portion of the literature written by English-speaking philosophers during the past forty years consists of attempts at its implementation.

This doctrine has been modified and more widely applied by Wittgenstein's followers. The modifications, although they are not entirely clear to at least the present writer, consist in duly noting that (1) the rules of ordinary usage are not in any real sense ultimate, and that, (2) therefore, it is possible to reform ordinary language. Thus modified, the Wittgenstein doctrine has been applied to the philosophical fields of ethics and epistemology, as well as to metaphysics, in the hope of achieving the "therapeutic" aim together with a more positive aim of redefining the issues to be disputed in the other fields.

\(^{17}\)See, e.g., Austin, op. cit., p. 38.
The second thing that must be realized is that there is at least one other distinguishable group of analysts keenly concerned with language who, although they may not in any strict sense be followers of Wittgenstein, have emerged more or less out of the logical positivist traditions in philosophy. Their concern with language relates to their somewhat larger interest in a range of epistemological problems that may be rather loosely designated as "the philosophy of science." More specifically, they are concerned about problems which arise in the attempt to understand the intellectual products of scientific inquiry as embodied in the explicitly formulated statements of scientists. Their discourse is usually addressed to the ways in which the conclusions of scientific inquiry are linked to the empirical evidence on which they may be based (e.g., problems of definition and measurement), to logical principles involved in the assessment of evidence and in the acceptance of conclusions (e.g., problems of the canons of probable inference), or to the structure of ideas embedded in scientific conclusions as well as of the systems of statements to which the conclusions belong (e.g., problems of the character of scientific explanations and the role of theories).

In short, the modern analytic philosophical scene consists of roughly two groups of philosophers. On the one hand, there are those who are concerned with eliminating
linguistic confusions in and redefining the issues of the main branches of philosophy—ethics, epistemology, and metaphysics. On the other, there are those who are intimately analyzing the linguistic properties of the world's—mainly natural—scientific literature. Moreover, the work of both groups has taken on an almost purely intellectual character. It has become "academic" in the rather pejorative sense of the word. But, however abstruse and seemingly irrelevant their work appears to be to the uninitiated, both groups remain convinced that their work is justified, if for different reasons.

The description of the nature of analytic philosophy just broadly outlined may seem to the philosophic layman as burdened with obscure distinctions. And so it sometimes is. But the purpose of the description is to show the predicament of the contemporary educational philosopher, if he looks to modern analysis for help in formulating his philosophical program. The point is that, if he looks, he will not find much. What will be found mainly is substantial quantities of writings about the major modes of argument in ethics, metaphysics, and epistemology, but not the philosophical arguments as such—together with lots of discourse about scientific locutions. To be sure, there are many techniques of linguistic analysis exhibited in these writings. Such techniques, considered in themselves, are edifying; which is but to say that all this should not be
taken to suggest that analytic philosophy is completely sterile. Nevertheless, crucial material necessary for the building of philosophies of education is conspicuously absent.

And, here is the real rub: to the extent that the contemporary educational philosopher finds the arguments of analysis convincing and, thereby, has a strong sense of identification with the mainstream of empiricism in philosophy, the greater will be his reluctance to opt for other brands of modern philosophy which could be more helpful to him. So, too, the more intense his criticism is likely to be of the venerable traditions in the writings of the philosophy of education. Both nonanalytic modern philosophy and traditional philosophy of education are likely to evoke in him more of a sense of suspicion than support.

Empirically oriented modern philosophers of education have suggested a number of ingenious ways out of their present predicament. These suggestions may be thought of as being of two fundamental kinds—although, in doing so, one idealizes somewhat. Exponents of the first kind of proposal argue that the content of philosophy of education be limited to applications of the techniques of linguistic analysis to the parlance of educators. On this view, words and phrases commonly employed by educators, such as "teaching," "subject matter," or "learning by experience," may be examined
fruitfully from the perspective of analysis. Presumably, proposals that stress the intrinsic merit of analytic methods thus applied are advocated on the grounds that (1) educators are prone to fuzzy verbalizing and the making of ex cathedra moral pronouncements; that (2) a useful sort of "therapeutic" function may be achieved if educators are made aware of their linguistic frivolity; and that (3) in any case, the techniques of linguistic analysis can aid educators in attempts to think clearly and critically.

Proposals of the second fundamental kind are more elaborate. Exponents of them seem to suggest that distinctively philosophical arguments pertaining to education should be subjected to linguistic analysis within their specific educational contexts, and that this is the special province of the philosophy of education. On this view, many philosophical arguments have education bearings, and it is the job of educational philosophers to spin them out. For example, epistemic words like "know" or "evidence" need to be explicitly related to psychological and pedagogical words like "teaching" and "believe." The rationale for positions of this kind seems to run something like this: (1) "Distinctively philosophical arguments legitimately may be brought to bear on educational matters— in fact, they have an important role to play. Epistemology, for example, while it provides no actual method (or path) to knowledge, does articulate the standards with which the results of any
method must comply if those results are justifiably to be termed knowledge. And, to the extent that an educational task is to impart knowledge, the standards for knowledge seem indispensable in determining what shall be imparted."

Parenthetically, it needs to be remarked at this point that there is nothing particularly new about this argument; in various forms it has nearly always been held by philosophers of education. (2) "Although (1) is true, the trouble is that philosophical arguments cannot be brought to bear on education in a fruitful way unless they are cleared of the evil of linguistic confusions. Indeed, to attempt the application of philosophy to education without an eye to linguistic confusions would constitute a grievous error." (3) "So, the proper formula for the philosophy of education is somehow to work at applying philosophical arguments to educational matters, while at the same time attempting to rid the philosophy of relevant linguistic confusions."

Needless to say, this formula sketches the parameters of an ambitious program.

The results of the proposals of the aforementioned two fundamental kinds are at the present time multiple and
interesting, but extraordinarily difficult to assess. It would serve no useful purpose, here, to attempt their detailed assessment. Suffice it to say that, from the present writer's viewpoint, these results are not totally satisfactory, and that this state of affairs has served, in part, as a stimulus for undertaking the present investigation.

If the epistemological methods of the present study are successfully employed, the bearing and utility of analytic philosophy on education and its problems will be demonstrated. This, because (1) the canons for the testability of scientific explanations are articulated in the analytic philosophy of science; (2) these canons will be employed in this study to determine the testability of Brunerian cognitive psychology; (3) the testability of Brunerian cognitive psychology is a matter that has a direct bearing on the question of whether Bruner's cognitive explanations are credible; and (4) the credibility of Brunerian cognitive psychology is of crucial relevance to any social studies teacher wishing to use it as a guide to...

18 Let the reader who doubts the credibility of this statement evaluate for himself the results exhibited in two books. See Israel Scheffler, Conditions of Knowledge (Chicago: Scott Foresman Co., 1965), and B. Othanel Smith and Robert H. Ennis, eds., Language and Concepts in Education (Chicago: Rand McNally Co., 1961). Scheffler's book is an example of the first kind of proposal; Smith and Ennis exemplifies the second.
teaching and curriculum building. Thus, in view of the problems that have been encountered among analytic philosophers of education in relating analytic philosophy to educational matters, the epistemological methods of the present study have significance for the philosophy of education.

**Concluding Summary**

This study proposes to subject the cognitive psychological explanations of Jerome S. Bruner and his associates to an epistemological critique in order to determine their truth. The truth of Bruner's cognitive psychology is a matter of some importance to social studies teachers who wish to use it as a baseline to guide their teaching and curriculum building. They need to know whether they are on firm ground in utilizing Bruner's work.

This study will draw upon analytic philosophy in seeking to accomplish its purpose. This has significance for the philosophy of education.
CHAPTER II

BRUNERIAN COGNITIVE PSYCHOLOGY

Introductory

In this chapter, first, the methodological orientation of Bruner and his associates is considered. Then, the psychological work of the Brunerians is succinctly described in both its earlier and later phases. In the description of both the earlier and later work, particular attention is given to the major explanations embedded therein. Relevant controlled experiments are also described.

Methodological Orientation

Broadly speaking, the work of Bruner and his associates may be tagged as behaviorist in tenor. A few words of clarification on this point are needed for two reasons. First, because the term "behaviorist" (and/or "behaviorism") is not consistently used by social scientists today. Second, because Bruner and his associates might cringe at being classified under this rubric.

As here understood, the standpoint in contemporary social science known as behaviorism is a modification of the
program of research adopted by a great many academic psychologists during the second decade of this century. The earlier program was the expression of a widespread revolt against the vagueness and unreliability of psychological data obtained by systematic introspective analyses of mental states.\(^1\) In their earliest formulations, proponents of behaviorism recommended the total rejection of introspection as a technique for psychological inquiry, and their announced aim was to investigate human behavior in the manner of inquiries into chemical processes or into the behavior of animals—without any appeal to the contents of human consciousness. Furthermore, some of these early proponents of behaviorism advanced distinctive views on substantive psychological issues (e.g., Edward L. Thorndike's famous "law of effect"), although the simple-minded mechanistic theories they adopted were not entailed by their rejection of introspection. It is worth notice, moreover, that even early exponents of this radical form of

\(^{1}\) "Introspection" as used here is meant to denote introspective activity of the methodical kind, typical for example of Wilhelm Wundt, rather than the casual introspective activity typical of psychologists like, say, William James. Cf. Peter McKellar, "The Method of Introspection," in Jordan M. Scher, ed., Theories of the Mind (New York: The Free Press of Glencoe, 1962), pp. 619-644. McKellar notes that Wundt demanded of his students that they undergo rigorous training in introspective techniques, and that only after some 10,000 "introspective exercises" were a student's introspections judged to be suitable material to be used in any published report from the Leipzig laboratory!
behaviorism did not unequivocally deny the existence of conscious mental states, and that their rejection of systematic introspection, in favor of the study of overt human behavior, was controlled mainly by a methodological concern to base human psychology on publicly observable data.\(^2\)

In any event, behaviorism has undergone important transformations since its inception, and there are few social scientists currently admitting to classification as behaviorists who subscribe to the earlier version's unqualified condemnation of introspection. On the contrary, professed behaviorists today generally accept introspective reports by experimental subjects, not necessarily as statements directly about private psychic states of the subjects, but as observable verbal responses that the subjects make under given (hopefully controlled) conditions. Accordingly, introspective reports are included among the objective data upon which psychological generalizations are to be founded. Moreover, contemporary behaviorists operating within this more liberal methodological framework have been investigating many areas of human behavior, individual and social. They

\(^2\)See, e.g., John B. Watson, "Psychology as the Behaviorist Sees It," Psychological Review 20 (1913), pp. 464-476. Alexander Rustow, a German sociologist of the humanist school, put it succinctly when he said, "Behaviorism is when you pretend to be dumber than you are." Quoted in Dankwart A. Rustow, "Relevance in Social Science, or the Proper Study of Mankind," The American Scholar 40 (1971), p. 491.
have proposed a number of special explanations to account for these phenomena—explanations that often differ from one another and also differ from the explanations associated with the earlier behaviorists. None of these more recently suggested explanations, however, is known to be adequate for explaining more than relatively small aspects of human conduct. So, behaviorism, like most schools of thought in academic psychology, tends to be a diversified program for research, rather than a school committed to some particular body of substantive theory. Considered, then, as a program for research, behaviorism, by placing a methodological premium upon intersubjectively observable data, thereby insists that any valid explanation of mental phenomena be evidentially linked in some way to overt behavior of the human organism.

It is in this broad, methodological sense that Bruner and his associates may be classified as behaviorists. And, it is in this broad sense that the Brunerians are at one with more conservative contemporary psychologists, like, for example, B. F. Skinner. But it needs to be underscored that mere methodological agreement still leaves room for major disagreements about substantive issues. In any event, the inquiries of Bruner and his associates are characterized by methodical and controlled experimentation with human and animal subjects, and by sensitivity to empirical data. Moreover, they appear to be guided by a
philosophically sophisticated conception of the nature of empirical scientific theoretical knowledge, although it is difficult to speak with certainty on this point. On their view, to use Bruner's words,

We know now that theory is more than a general description of what happens or a statement of probabilities of what might or might not happen— even when it claims to be nothing more than that, as in some of the newer behavioral sciences. It entails, explicitly or implicitly, a model of what it is that one is theorizing about, a set of propositions that, taken in ensemble, yield occasional predictions about things. Armed with a theory, one is guided toward what one will treat as data, is predisposed to treat some data as more relevant than others. A theory is also a way of stating tersely what one already knows without the burden of detail. In this sense it is a canny and economical way of keeping in mind a vast amount while thinking about a very little.3

Guided by such a behavioristic orientation, Bruner and his collaborators have formulated a large body of explanatory and semi-explanatory hypotheses about the human organism.

The substantive work germane to the present study has been produced over the past fifteen to twenty-five years, depending upon where one wishes to place the historical markers. It is, for the most part, a joint product of Bruner, his colleagues, and his students. It can be sorted into three compartments. Listed in chronological order, the first has to do with selectivity in human perception; the second with strategies for human perception; the second with strategies for human

conceptualizing; and the third with instrumental conceptualizing, mainly among children. The work in perceptual selectivity bears only marginally on pedagogical matters, and thus will be largely omitted from consideration in the present study. But, to the extent that these three compartments are not intellectually watertight, it needs consideration.

**Brunerian Work on Perception**

It is not so much Bruner's individual substantive contributions to the theory of perceptual processes that are pertinent to the present study, but rather his views on the nature of concepts fruitful for the description and explanation of human perception. As regards the nature of such concepts, Bruner has written extensively, in collaboration with his colleague, George S. Klein.

In an essay, vintage 1959, entitled "The Functions of Perceiving: New Look Retrospect," Bruner and Klein argue that, among other things, the post World War II work by psychologists in the field of perceptual research is dominated by a concern to move away from accounts of human perception that tend to ignore internal variables of the organism. They note that, indeed, modern students of perception tend to find the positing of internal variables to be quite fruitful. The essay emphasizes the role played in the then current "New Look" theories of perception by
concepts which denote internal variables, like "set," "need," "interest," "value," and "selective programming." But, more than this, even the neurophysiologists of sensation and perception, the essay notes, postulate internal variables. They frequently find it useful, for example, to think of the human sensory system as a complicated mechanism programmed selectively to accept environmental stimuli for purposes of transmitting them (as electrical impulses) to the brain. Moreover, Bruner and Klein suggest that ethologists, students of unlearned behavior, postulate internal variables.

The point that has been made about contact with neurophysiology can be paralleled by one about contact with the work of the ethologists. Certainly the emphasis of Tinbergen on the innate releasing mechanism and its effect on selectivity suggests that the general model of internally regulated programs of perceptual registration holds even for the simplest organisms and that the receptive system is scarcely a matter of passive registration once a traditionally adequate stimulus has impinged. Indeed, the concept of adequate stimulus is something that needs restatement, not just in terms of the capacity for reception of end organs, but in terms of programmed readiness of the entire receptive system.

In all, this essay makes it plain that Bruner and Klein see the tendency among perceptual theorists to posit internal variables to be both fruitful and healthy.

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5 Ibid., p. 66.
The point in elaborating on this particular essay is that it is indicative of the fact that Bruner is quite comfortable with explanations of perceptual processes which contain within them terms that refer to internal states of the human organism. Often, in fact, Bruner himself formulated this kind of explanation of perceptual processes. It needs to be taken into account, moreover, that Bruner changed the focus of his psychological research in the 1950s from perceptual to cognitive processes. Thus, he took his favorable disposition toward internal variables, as it were, with him from one sub-field of inquiry to another. This point should be considered, however, only with the caveat that distinctions between the two sub-fields are by no means sharp. In any event, let us now move to an examination of the first substantial piece of work produced by Bruner and his associates in the area of cognitive studies.

**Early Work on Cognition**

The substantial piece of work referred to is contained in two documents. The first is a book, *A Study of Thinking*, first published in 1956; the second a paper,  

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entitled "Going Beyond the Information Given," presented to a symposium on cognition held in 1955 at the University of Colorado. The book reports on and discusses some twenty laboratory experiments on concept attainment; the paper concentrates on the theory of the creation and acquisition of conceptual systems in the human organism. Taken together with the paper, A Study of Thinking probably marks a significant turning point in Bruner's intellectual biography—a major shift in research interest from perception to cognition.

A Study of Thinking begins with the observation that the normal environment of a human organism is immensely complicated. There are more than seven million discriminable colors, alone, for example. In such an environment, Bruner and his collaborators note, man must categorize objects and events, or be well-nigh paralyzed by the complexity. "To categorize is to render discriminably different things equivalent, to group the objects and events and people around us into classes, and to respond to them in terms of their class membership rather than their uniqueness."
The categories into which objects, events, and people are sorted, according to Bruner, are of two basic types: identity and equivalence. "Identity categorization may be defined as classing a variety of stimuli as forms of the same thing." A speck sighted on the ocean's horizon surmounted by a plume of smoke is identified as a particular ship; so, too, a towering transatlantic liner seen at a dock. Equivalence categorization, in contrast, amounts to classifying a set of discriminably different things as the same kind of thing. "Books," "fruits," "people," and "tools" are all words which denote equivalence categories.

Categories (Bruner also calls them "classes" and "concepts") are not discoverable in the sense of their being present out in the organism's environment, thus to be the objects of a search. Instead, they are creations of man, responses to experience. They are shared by and transmitted among men. Many categories that are learned, moreover, are deeply reflective of the culture in which the human organism is socialized.

The process of categorization accomplished by the human organism, occurs, Bruner argues, at the level of immediate perceptual experience, and at a deeper, conceptual level, where stimulus objects are not immediately perceived.

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9 Ibid., p. 2.
10 Ibid., p. 4.
(This is a distinction that Bruner finds difficult to make plain.) An example of categorization at the former level could be the classifying of an object of a certain color, shape, size, and texture as an apple. An illustration of categorization at the latter level could be the classifying together of all prime numbers by virtue of whether they meet the criterion of non-divisibility. "In the perceptual case, the relevant attributes are more immediately given by which we judge categorical identity of an object, at least in simple perceptual situations."

Bruner and his associates advance the hypothesis that the functions of categorization in the human organism are at least fivefold. First, categorization reduces the complexity of the environment for the organism. Second, categorization is the means by which objects in the world of human experience are identified. The act of identifying an object is, in fact, an act of categorizing. Third, categorization reduces the necessity for constant learning on the organism's part. The abstraction of defining properties makes future acts of categorization possible without the benefit of further learning. Fourth, it provides direction for instrumental activity. Presumably, to know that a substance is poison, for example, implies an

\footnote{Ibid., p. 9.}

\footnote{Ibid., pp. 11-15.}
appropriate way of dealing with it. Finally, categorization permits the opportunity for ordering and relating classes of events--i.e., for the formation of category systems. Such category systems are very important for the human organism, according to Bruner, because they enable it to "go beyond" what is directly observed. For example, to be able to categorize certain atmospheric phenomena as "dark clouds," and to know that they are causally related to other classes of atmospheric phenomena, say, "rain," is also to know that "dark clouds portend rain."

This Brunerian's chief concern in A Study of Thinking is to understand how people come to recognize that a certain array of discriminable attributes is indicative of a given equivalence category--the attributes of which have in some way already been determined. The sort of activity involved in such recognition is called by the authors the process of "concept attainment." In a meticulous fashion, they contrast the notion of "concept attainment" with another notion, more commonly used by psychologists, "concept formation." Concept formation, the authors tell us, involves the creation of categories in response to experiential events, either on the spot or in some delayed way. About this latter sort of creative activity they have little to say, for, as they pointedly emphasize, it is beyond the scope of their inquiry. In any event, the authors frame three questions to guide their investigation: (1) "How do
people achieve the information necessary for isolating and learning a concept?" (2) "How do they retain information gained from encounters with possibly relevant events so that it may be useful later?" (3) "How is retained information transformed so that it may be rendered useful for testing an hypothesis still unborn at the moment of encountering new information?"  

Bruner and his collaborators tend to reject a direct approach for finding answers to their guiding questions—that of simply asking people to elaborate verbally on how they come to recognize categorical distinctions. Such verbal report, the Brunerians say, provides insufficient data for making generalizations about concept attainment.  

The process of concept attainment is best studied, the Brunerians assert, by externalizing it (by making it observable) under tightly controlled conditions, and by comparing it thus externalized to perfectly rational behavior having the same objective. This is a variation of the "zero method," as Karl Popper called it, commonly used in the social sciences. This method is, perhaps, best explicated in the context of a prototypical experimental instance.

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13 Ibid., p. 51.
14 Ibid., p. 50.
**Material**: an exhibit of eighty-one cards, each representing a combination of one value of each of the following attributes—color (green, red, or black); form (square, cross, or circle); number of forms (one, two, or three); number of borders (one, two, or three).

**Subjects**: students at Harvard or Wellesley.

**Procedure**: the investigator explains to the subject what is meant by a conjunctive concept—a set of cards that share a certain set of attribute values, such as "all red cards," or "all cards containing red squares and two borders." For practice, the subject is asked to show the investigator all the examplars of one sample concept. The subject is then shown a card by the investigator that is illustrative of a certain concept. The subject is then asked to guess what the concept is on the card he has been shown, by choosing cards in the exhibit one at a time for testing. The subject is instructed that, after each of his choices, he will be told whether the card which he has chosen contains on it the concept for which he is searching. The subject is told that he may hazard an hypothesis (but only one) after each choice of card, but that he need not do so. The subject is asked to arrive at the concept as efficiently as possible.

**Record**: the sequence of choices, the stated hypotheses about the nature of the concept which the first
card exemplifies, and occasionally some introspective reports of the subjects.\textsuperscript{16}

This kind of experiment requires that the experimenter know the amount of information relevant to the task at hand conveyed to the subject by each choice of card and their combinations. Also, the subject must be fully aware of what he is expected to do, so that the experimenter can know (or infer) to which and to how many attributes the subject is attending at any given point in his choice sequence. Under these conditions, it becomes possible for the pattern of the subject's actual sequence to be compared by the experimenter to predetermined theoretically optimal choice-sequence patterns, called "ideal selection strategies." The ideal strategy that is closest to the pattern of the subject's actual sequence of choices reveals the subject's "concept attainment strategy."

The Brunerians cite four kinds of ideal selection strategies. First, "simultaneous scanning," which consists in the subject's attempting to keep in mind all hypotheses regarding possible concepts exemplified in the first positive card given to him by the experimenter, and, then, using the occasion of his choice of each following card for deducing which of the hypotheses are tenable and which have been eliminated by his choice. This is quite a difficult

\textsuperscript{16}Bruner, Goodnow, and Austin, \textit{A Study of Thinking}, p. 83.
and exacting strategy. Second, "successive scanning," which consists in the subject attempting to test only a single hypothesis suggested by the first positive card at a time. The successive scanner, then, limits his choices to those instances that provide a test of the hypothesis which he has in mind. Third, "conservative focusing," which consists in focusing on the first positive card, and, then, making a sequence of choices, each of which alters but one attribute value of the first focus card—testing, accordingly, to see whether the change yields a positive or negative instance. Those attribute values of the focus card that, when changed, still yield positive instances are not part of the concept; those that yield negative instances, when changed, are features of the concept. This strategy guarantees to the subject that each choice he makes will be informative. Finally, there is "focus gambling." The principal feature of this strategy is that the subject uses a positive instance as a focus, and then, by way of his choices, seeks to change more than one attribute value at a time. If, and only if, he is lucky, by using this technique the subject can determine the concept with a relatively small number of choices.

Although descriptions and discussions of experiments like the one just described constitute the bulk of the material in _A Study of Thinking_, there were some variations from this prototype. One variation, important at the
theoretical level, had to do with the kind of category for which subjects had to search. The authors distinguished three types of category: conjunctive, disjunctive, and relational. A conjunctive category, as was stated in description of the prototype, was one defined by the joint presence of the appropriate value of several attributes; all cards that had three red circles, for example. A disjunctive category was one which possessed a stipulated range of alternate attributes. A disjunctive category may be illustrated by the class of cards possessing three red circles, or any constituent thereof: three figures, red figures, circles, three red figures, red circles, or three circles. A relational category was one defined by a specifiable relationship between defining attributes; for example, all those cards containing the same number of figures and borders. Other variations from the prototype often had the purpose of revealing the consequences of introducing some particular factor or other into the process of concept attainment. Subjects were harassed in some experiments, for example, so as to reveal their "cognitive strain" while they went about their assigned tasks. A variety of techniques of harassment were used: among them, speeding up the pace, increasing the number of attributes and values to be searched in pursuit of correct cues, and arranging the instances encountered in an orderly as against a random form.
The Brunerians list as significant results of *A Study of Thinking* two principal findings and the discovery of three "tendencies." With regard to the principal findings, we are told that (1) it is possible systematically to describe and to evaluate (e.g., in terms of efficiency) concept attainment strategies; and that (2) it is possible to demonstrate the effect of relevant conditions on measurable aspects of concept attainment strategies. As to the "tendencies," the authors note that, in attempting to differentiate exemplars from non-exemplars of a category, subjects tend, in the absence of other information, to fall back upon cues that in the past have seemed useful, whether such cues have been useful in analogous situations or not. They note, in addition, that subjects tend to prefer working with conjunctive categories, and to use (often inappropriately) strategies for cue searching that are relevant to such concepts. Lastly, the authors note another general tendency in "... the inability or unwillingness of subjects to use information which is based on negative instances or derives from the indirect test of an hypothesis."\(^{17}\)

It is worth noting the somewhat tenuous relationship between the significant results of *A Study of Thinking*, as listed by the authors themselves, and the initial questions

\(^{17}\)Ibid., p. 237.
framed by the investigators to guide their inquiry (see pages 39-40). Put bluntly, it is, in fact, difficult to see any relationship between questions and results at all. This leads one to suspect that the questions were meant merely to sensitize the readers of *A Study of Thinking* about the nature of the experiments reported therein, rather than actually to guide the authors in their investigation. In any event, the questions are interesting, even if ambiguous, and some clues as to possible answers for them are found in Bruner's paper, "Going Beyond the Information Given." It is to an examination of this paper that we now turn.

In "Going Beyond the Information Given," Bruner is primarily interested in the problem of describing and explaining how the human organism operates when it is engaged in the process that is ordinarily, and foggily, called "inferring from the observed to the unobserved." This problem has agitated psychologists at least as far back in history as von Helmholtz, probably because the kind of phenomena in question seem to appear in even some of the more rudimentary modes of human conduct. To be a little bit more precise, what Bruner is concerned with is the familiar fact that in many, if not most, descriptions of human behavior, one is forced to admit that the organism cannot adequately be characterized as merely reacting to immediately sensed features of its environment. Rather, it seems that the organism must be described as acting in ways
which indicate that it is taking into consideration more
than what it could conceivably sense directly in its con­
fronting situation. An apple when observed by an ordinary
adult, to take a simple example that is within Bruner's
framework, is seen not merely as an object of a certain
size, shape, texture, and smell. Instead, it is also
rendered by the organism as equivalent with a class of other
objects (apples), and is subsequently viewed as possessing
other properties, which, though not immediately exhibited
in the confronting situation, nevertheless are character­
istic of objects belonging to its class. Apples can be
eaten, sliced with a knife, etc.

In undertaking to explain how a person operates when
engaged in inferring, Bruner uses the expression "coding"
to describe what an organism does when classifying its
immediate sensations, and the expression "coding system"
to indicate the product and the instrument of the coding
process. He stipulates that a coding system is a con­
tingently related set of nonspecific categories, and asserts
that a coding system amounts to a person's manner of
grouping and relating information about his world. Codes,
and systems thereof, we are told, are constantly the
subjects of reorganization and change. Moreover, they have
another dimension--that of "genericalness." The organism
can learn and/or invent codes and coding systems that have a
wider or narrower degree of applicability in its environment; the more generic the code, the wider its applicability.

From this perspective, Bruner proffers an explanation of the inferring process.

We propose that when one goes beyond the information given, one does so by virtue of being able to place the present given in a more generic coding system and that one essentially "reads off" from the coding system additional information either on the basis of learned contingent probabilities or learned principles of relating material. Much of what has been called [by psychologists] transfer of training can be fruitfully considered a case of applying learned coding systems to new events. Positive transfer represents a case where an appropriate coding system is applied to a new array of events, negative transfer being a case either of misapplication of a coding system to new events or of the absence of a coding system that may be applied.18

Bruner goes on to ask and to attempt answering the question: "Under what conditions will an organism code (learn) something in a generic manner, so as to maximize the transferability of the learning to new situations?"

Bruner's answer to this question is that conditions of four kinds may be determinant.

The first condition is "set." Bruner argues that the manner and the degree with which newly learned information is coded generically may be affected in a transient way by the situational instruction given to the organism

18Bruner, "Going Beyond the Information Given," loc. cit., p. 49.
immediately prior to coding, and, in a more permanent way, by the regimen of the organism's past experience.\textsuperscript{19}

The second condition is the organism's "need state." In drawing his conclusions about the influence of "need state" on generic coding, Bruner extrapolates from his and other psychological studies of rats and monkeys. In these studies, the animals were deprived of food for periods of time, and subsequently observed in transfer of training experiments. The experiments show that, under conditions of very high and very low levels of "drive," the animals behave in ways that indicate "concreteness of cognitive activity." The inference to be drawn from these studies appears to be that too much or too little motivation will hamper generic coding. But, Bruner qualifies such a conclusion by suggesting that, in some higher organisms, this may not always be the case.\textsuperscript{20}

The third condition--Bruner calls it "degree of mastery"--is the amount of practice involved in generic coding. Bruner seems to say that a certain amount of repetition is required in most any situation where a human organism is attempting to learn generically. As to what

\textsuperscript{19}Ibid., p. 53.

\textsuperscript{20}Ibid., p. 57.
kind of practice and how much, Bruner seems not to be sure.  

The final condition Bruner considers is "diversity of training." The expression "diversity of training" refers to the number of different illustrations of a particular generic code to which a person needs exposure in order effectively to learn the code. Bruner deals with "diversity of training" largely at the level of common sense, and suggests that, unless the learner is exposed to some different illustrations of a generic code, it is not likely that generic coding will take place.  

Let us now take stock of the views of Bruner and his associates as broadly outlined up to this point. The Brunerians seem to hold that the human organism is active and purposeful within its environment, capable of applying its previous experiences in new confronting situations. That the human organism can be so characterized, on the Brunerian's view, is due largely to its capacity to categorize the phenomena of its experience in generic codes. Again, the human organism achieves information necessary for isolating and learning categories, the Brunerians seem to say, by employing rather rational strategies of concept attainment. Information about the world may be rendered

21 Ibid., p. 60.

22 Ibid., p. 61.
useful to the organism in new situations only to the extent that it is coded generically.

It is from this view of man that emerges Bruner's stress on the pedagogical importance of the "structure of subject matter." Generally speaking, to Bruner structure in subject matter means the generic codes contained therein--coding systems that have applicability beyond the situation in which they are learned. He proposes a test as a measure of adequacy of any set of instructional propositions--"... that once they are grasped [by the learner], they permit maximum reconstruction of material to the reconstructor." But, all this is to anticipate. Suffice it to say at this point that, to almost any pedagogue, these views are bold and imaginative, when compared to an educational psychology that occupied itself for more than a generation with things like "rate of acquisition" and "rate of extinction" in learning. In any event, over a period of about sixteen years after their initial cognitive studies, Bruner and his associates have modified, refined, and extended their views. It is to these matters that we now turn.

Later Work on Cognition

The character of the Brunerian's more recent work is

\[23\text{Ibid.}, \ p. \ 66.\]

It is, perhaps, significant that the expression "cognitive growth" appears in the titles of both of these documents. The word "growth," itself, is notoriously one with protean meanings. It is sometimes used to denote a process, sometimes the product of a process. It is frequently employed as a purely descriptive term to characterize several types of change. It functions in some contexts, for example, to indicate a sequence of continuous changes eventuating in some outcome, however vaguely specified, which is somehow potentially present in earlier stages of the process. Moreover, in a narrower sense, the term is reserved for changes which are irreversible, yield only greater numerical complexity, and eventuate in modes of organization in the growing system, such that the system acquires an increased capacity for self-regulation—a larger measure of relative independence from environmental fluctuations. This is, at any rate, what embryologists seem to have in mind when they take progressive differentiation
and increased capacity for self-regulation as essential marks of growth. The point of all this elaboration on the meaning of "growth" is to show that the Brunerians are no longer concerned only with the creation and attainment of categories in the adult human organism. Rather, they are seeking to describe and explain mental processes characteristic of persons as they develop from infancy to adulthood. In other words, they have become "developmental psychologists."

It is also necessary to note, preliminarily, that in their views as to the nature of cognitive growth the Brunerians show the heavy influence of the work of Jean Piaget and his collaborators at the Rousseau Institute of the University of Geneva. Indeed, both the paper, "The Course of Cognitive Growth," and the book, Studies in Cognitive Growth, can fruitfully be thought of as reactions of the Center for Cognitive Studies to the Rousseau Institute. In any event, the point in mentioning Piaget's influence is to make clear why in the following exposition space will be devoted to elaborating on aspects of Piaget's work.

One final point before our examination begins. Piaget is known to the English-speaking scholarly world mainly for his thought provoking, but, according to some critics, disputable accounts of the quaint notions of young children about such things as enduring physical objects.
space, and causality. There exists in English no entirely satisfactory summary statement of his work. This is probably due to the sheer quantity of his writing—by one count Piaget's work consists of approximately twenty-five books and over a thousand journal pages—and because his writing often invites differing interpretations. There is some evidence which suggests that Piaget, himself, feels that Bruner has failed to understand the precise nature of some of his (Piaget's) work, and, in view of the situation just described, misinterpretation on Bruner's part is entirely possible. But, so far as the present study is concerned, the question of misinterpretation is almost purely academic. For, here, the focus is on Bruner as he understands Piaget, not on whether his interpretations are correct.

Two theoretical themes recur frequently in the Brunerian's more recent work. One is that the human organism's knowledge of the world in which it has to cope is based on constructed models of reality—models that can only be partially and intermittently tested for adequacy by the organism via its senses. The other is that many, if not most, of the models which constitute an important part of


the organism's intellectual equipment, rather than being invented by the organism in response to events, are acquired by it from other people as part of its cultural heritage. Bruner calls these two themes the cardinal tenets of his working point of view. He labels his working viewpoint "instrumental conceptualism."

Modeling activity on the part of the organism seems to be hypothesized by Bruner in an attempt to account for how people come to notice recurrent features of their surroundings in such a way as to be able to apply this knowledge in new situations. Traditionally, in psychological accounts for how the organism operates when applying its previous experience, the notion of "memory" has loomed large as an explanatory factor. But, according to Bruner, to dismiss this problem as "mere memory" is to misunderstand it. For the most significant thing about memory is not the storage of past experience, but rather the retrieval of what is relevant in some usable form. This is contingent upon how past experience is coded and processed so that it may indeed be relevant and usable in the present when needed.26

Bruner distinguishes three kinds of model that the organism utilizes in its daily intercourse with its

environment: enactive, ikonic, and symbolic. These three kinds of model— or "modes of representation" as Bruner sometimes calls them— must be inferred from the organism's overt behavior.

To infer a person's representation of the world, ... we design tasks that permit us to infer how he does these things. We ask him to tell us the fifty states of the Union. If he "reads out" in this order, "Maine, New Hampshire, Vermont . . . ," we can guess that the supporting representation for his recital is spatial. If the order is "Alabama, Alaska, Arizona, Arkansas, California . . . ," the support is inferred to be more list-like, ordered by an alphabetic rule.27

According to Bruner, "enactive representation" means modeling through psychomotor action, and it can be best understood as what psychologists have traditionally called "habit formation." For example: "With respect to a particular knot, we learn the act of tying it and, when we 'know' the knot, we know it by the habitual pattern of action we have mastered. The habit by which the knot is represented is serially organized, governed by some sort of schema [i.e., mental framework] that holds its successive segments together. . . ."28

In contrast to the enactive mode, "ikonic representation" means modeling via the selective organization of percepts and images. By "percepts" Bruner seems to mean the

28Ibid., p. 6.
immediate sense impressions obtained by the organism when it
directly observes events in its environment. Bruner likens
an "image" to a picture, in the sense that it is a selective
analogue of the object pictured, and only in a trivial sense
a copy of its referent. Imagery seems for Bruner to be akin
to the mental phenomena ordinarily connoted by the use of
the term "imagination." One can imagine a tree, for exam­
ple; the "image" of the tree thus in the mind's eye seems
to be the kind of thing to which Bruner wants to call our
attention.

"Symbolic representation," on Bruner's view, means
modeling reality through language. In its simplest form, it consists in using symbols to stand for the unique events and relations of experience. In more sophisticated forms, however, it involves the use of the category systems of language. By virtue of such systems, the organism is able to "rework" reality with a measure of ease. Thus, for example, "We observe an event and encode it--the dog bit the man. From this utterance we can travel to a range of possible recodings--did the dog bite the man or did he not? If he had not, what would have happened? and so on."29

Bruner reports that the three modes of representa­tion appear in the life of the organism in the order:

enactive, ikonic, symbolic. The latter two modes are dependent, we are told, on previous ones for their development. All three modes remain more or less intact throughout the life of the organism—barring such early accidents as blindness, deafness, or cortical injury. The mature organism, indeed, possesses a sort of interlocking network of enactive, ikonic, and symbolic systems. Yet, according to Bruner, as the organism ages, it loses its earlier, more innocent representational systems. This loss appears to be a necessary condition for the formulation of progressively more sophisticated models. Thus, the adult is unable to reconstruct his own mental development—he suffers from "childhood amnesia" with regard to such matters.

Essentially, the same set of experiments is reported in both *Studies in Cognitive Growth* and "The Course of Cognitive Growth." The main difference between the two reports is that *Studies in Cognitive Growth* contains more detailed descriptions of the experimental procedures. Generally speaking, all of the experiments aim at the elucidation of the nature of representation, particularly the transition of it from its ikonic to its symbolic forms. In *Studies in Cognitive Growth* a series of experiments clusters around each of nine different topics. For purposes of the present study, a sample of only three of the nine topics investigated by the Brunerians need be examined.

Rose R. Olver and Joan Rigney Hornsby report two
related experiments on the topic of "equivalence" in Studies in Cognitive Growth. They had as their purpose an examination of the relationships that hold between enactive, ikonic, and symbolic representation, and the manner in which adults and children render discriminably different things in their environment as the same or alike. Olver and Hornsby postulate that, under enactive representation, things should be seen as alike on the basis of a common role in some action; that equivalence with ikonic representation might be expected to be accomplished by grouping items according to perceptual kinship or likeness; and that, with the achievement of symbolic representation, equivalence might be expected to be governed by such grammatical principles as synonymy or syntactic substitution.

In their first experiment, the investigators gave sixty children from age six to nineteen the task of telling how an array of different items were alike. The children were each presented the words "banana" and "peach," each typed on a small white card and spoken aloud as well, and asked: "How are banana and peach alike?" The word "potato" was then added to the list, and each child was first asked, "How is potato different from banana and peach?" then, "How are banana, peach, and potato all alike?" This procedure was continued until the list of words consisted of banana, peach, potato, meat, milk, water, air, and germs. At the end of the list was included a word about which the
investigators asked only how it differed from the preceding words: for example, "stones" was presented as a final word on the banana-peach list. The investigators presented to each child a second list of words in the same manner: bell, horn, telephone, radio, newspaper, book, painting, education, and, as the contrast word, confusion.30

Olver and Hornsby analyzed the responses of the children to their questions in two different ways. They first sorted out the kinds of attributes that the children focused upon in making equivalence claims among the array of items. This analysis revealed that the children sometimes seemed to focus on the immediately perceptible features of items (e.g., color, size, shape, etc.). On other occasions, the children appeared to focus on such properties of the items as the uses to which they could possibly be put, the emotions they evoked, or their conventional class name in the English language.

The children's responses were also analyzed with respect to the nature of the grouping employed in making equivalence claims. Olver and Hornsby distinguished three kinds of groupings: those based on attributes shared by all the items grouped (e.g., "They all have skins."), those formed by using alternate features of an array of items so as to form rather loose rules for grouping (e.g., "Banana

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30Bruner et al., *Studies in Cognitive Growth*, p. 70.
and peach are both yellow, peach and potato are round, and potato and meat are served together. . . ."), and groupings formed on the basis of how items fit in a sentence of a story or a theme (e.g., "The little boy was eating a banana on the way to the store to buy some potatoes and peaches.").

Resultant from their analyses, Olver and Hornsby report that young children predominately tend to employ loose rules in equivalence categorizing, and that they tend to focus on the immediately perceptible properties of objects when so categorizing them. Older children in contrast, we are told, tend in the main to render objects as equivalent on the basis of commonly shared features. Also, older children manifest no distinct propensity toward focusing upon the immediately perceptible properties of objects to be grouped.

In their second experiment, a variant of the first, Olver and Hornsby presented their subjects with pictorial stimuli, and the subjects could deal with them in their own order. Ninety boys, ages six to eleven, were each presented with an array of forty-two water-color drawings. Their task was to select from this array a group of pictures that were "alike in some way." The drawings represented familiar objects such as a pair of scissors, a doll, a garage, a bee, a pumpkin, a taxi, a sail boat, and various articles of clothing. Each child was asked first to identify each picture to ensure that he had seen and was familiar with
all of them. Whenever any child was unable to identify a picture, he was told what it was. Each child was then asked to choose pictures that were alike in some way—"any way at all in which a group of things is the same"—and to remove them from the array. He could take as many pictures as he wanted. When he had completed each grouping, he was asked to tell how the pictures that he selected were alike. The pictures were then replaced in their original positions in the array, and the child was asked to form another group. This task was repeated ten times.\textsuperscript{31}

The children's responses in this experiment were analyzed in basically the same two ways as were those elicited in the first experiment. The findings of the second experiment were also similar to those of the first. Younger children tended to focus on the immediately perceptible attributes of the pictures in greater degree than did older children. And, the use of loose rules for equivalence classification tended to decrease with age, in favor of classifying via common features.

Also in Studies in Cognitive Growth, Bruner reports a series of experiments related to the general topic of "conservation." "Conservation" is a technical term first employed by Piaget and his associates at the Rousseau Institute. It derives its sense from the conservation laws

\textsuperscript{31}Ibid., pp. 78-79.
of the physical sciences. It is meant to denote a capacity on the part of the human organism to recognize that a particular quantity of material (e.g., water) retains its same measurable magnitude despite the changes in appearance that it might undergo.

Piaget's experiments on "conservation" have been replicated many times by other investigators.\(^\text{32}\) The experiments seem to indicate that Western children below a certain age (somewhere around seven) are usually unable to "conserve" a quantity of matter over transformations in its appearance. A typical experiment by Piaget and his associates illustrates this point with some force. A young child is presented with two beakers of the same size and shape, and told to pour exactly the same amount of colored water into each beaker. After he has done this to his satisfaction, the water from one of the beakers is poured into another glass of a different size and shape, and the child is asked if there is still the same amount. If the second beaker is taller and thinner, the child will usually say that there is more water in it than in the first, because the water level is higher. If the second glass is shorter and wider than the first, the child will ordinarily

\(^\text{32}\) Some of the more lucid replications were undertaken at the Horace Mann-Lincoln Institute of School Experimentation of the Teachers College, Columbia University. See Millie Almy et al., *Young Children's Thinking: Studies of Some Aspects of Piaget's Theory* (New York: Teachers College Press, 1966).
respond that there is less water because of the lower level. If the water from one of the beakers is poured into, say, six smaller ones, the child likely will hold that the small beakers have more water because there are more of them. In each case, the child seems to fail to grasp the invariance of the quantity of water in the face of changes in its constituent dimensions.

According to Bruner, Piaget and his associates interpret the advent in the human organism of the capacity to "conserve" as representing a crucial turning point in the course of the organism's development--a shift from a preoperational phase of thinking to the concrete operational phase. What characterizes this shift is that operations by the organism before carried out overtly now become internalized. The crucial attribute of internalized, concrete operations is that they are reversible. Thus, for example, a child who has reached the concrete operational phase and who is confronted with a situation in which he has observed changes in the constituent dimensions of some piece of material, is capable of mentally reversing the process of change that he has witnessed, of imagining the piece of material under scrutiny as having the dimensions it had before it changed. The capability of reversing operations (reversibility) enables the development of many other mental capacities, one of which Piaget calls "compensation." Piaget holds that compensation involves the multiplying of
relations. Thus, for example, a compensating child, when confronted with a tall, thin beaker and a short, fat one filled with liquid to a lower level, supposedly multiplies "greater height" by "lesser width" and comes out with "equal quantity." Bruner tells us that Piaget suggests that it is the development in a child of the reversing and compensating capability that makes "conservation" possible.33

In contrast to Piaget, Bruner maintains that, even if reversibility and compensation are necessary conditions for the advent of "conservation" in the human organism, neither factor, alone or in combination, is sufficient to produce it. On Bruner's view, another factor must be taken into account: a conception on the organism's part of identity (sameness).34

As evidence for his viewpoint, Bruner cites an experiment performed by Patricia Nair of the Center for Cognitive Studies. Nair explored the arguments children use when they solve a "conservation" task and when they do not. Her subjects were all five-year-olds from a kindergarten in a Boston suburb. They were divided into two groups: those who had shown "conservation" on a beaker type pretest (previously just described) and those who had not. Nair

34Ibid., p. 185.
transferred water from one rectangular, clear plastic tank to another that was both longer and wider than the first. Ordinarily, a five-year-old will say that there is less water in the second tank. The water level is, of course, lower in the second tank. She had a toy duck swimming in the first container, and, when the water was poured into the new container, she told the child that "the duck was taking his water with him." After moving the water and the duck, each child was asked: "Is there just as much, more, or not as much water here (pointing to the new container) as here (pointing to the original container)?" and "Is this the same water as the duck had there (pointing back)?" One-half of the children were always asked the questions in one order, and one-half in the other—indicating equality (same amount) and then identity (same water), or identity and then equality.

Nair reported that virtually all her subjects who said that the water in the two lakes was the same amount also said that it was the same water. But the reverse did not hold, for many of the children who asserted that it was the same water also judged that the two lakes did not contain the same amount. Over one-third of the "nonconservers" on the pretest, moreover, held that the water was neither the same amount nor the same water.

Upon analysis of the reasons given by children in support of their judgments of amount, Nair reported that, of
her subjects who "conserved" on the pretest, one-half used some variant of "It's the same water" as their main argument for maintaining invariance of amount. The reasons given by the children who did not start out with "conservation" were in sharp contrast. Their reasons most usually did not employ the notion of sameness—being in most cases some variant of "I poured it" or "The duck took it with him."

On the basis of these and some other analyses of the responses of Nair's subjects, Bruner concludes that children who exhibit the capacity to "conserve" have the notion of identity (sameness) within their representational systems. From the same evidence, he argues more tentatively that the recognition of identity by the human organism is a necessary, if not sufficient, condition for the recognition of quantitative equivalence.

Within the context of his experiments on "conservation," Bruner advances one of his more interesting hypotheses—that conflict between systems of representation serves as a source of impulsion for cognitive growth in the human organism. He cites as evidence for his viewpoint a complicated experiment done at the Center for Cognitive Studies by Francoise Frank.

Frank used forty children from a suburban school near Boston in her study. The children were of the ages four, five, six, and seven, and there were ten youngsters in each age category. All of the children were pretested for
"conservation" on a beaker type pretest (of the Piagetian type described on pages 63 and 64). A "standard" beaker was used in the experiment, together with four other "comparison" beakers: one of the same dimensions as the standard, one taller but of the same width as the standard, one of the same height but wider than the standard, and one both wider and taller than the standard.\textsuperscript{35}

In the first phase of the experiment, the standard beaker, half full of colored water, and one empty comparison beaker at a time were shown to the subject, and, then, placed behind a screen twelve inches wide and five inches high, so that only the tops of the beakers showed. The water was poured from the standard to the comparison beakers while both were behind the screen. The screen was never removed, so the child could not see the level of the water in the comparison beakers. After the occasion of each pouring, the child was asked whether there was still the same amount of water, and he was asked the reasons for his answer.

The second phase of the experiment was similar to the first, in that the same mode of arrangement and presentation of beakers was employed. This time, however, the screen was not used, and the child was asked to predict whether there would still be the same amount of water if

\textsuperscript{35}Some details of the experiment are omitted in the interest of brevity.
it were poured from the standard beaker to the comparisons. The experimenter also asked the subject to indicate with his (the subject's) finger the level to which the water would come in each of the comparison beakers if it were poured. Again, the subject was asked the reasons for his judgments. The water was never actually poured, so the child did not see the level of the water in the comparison beakers.

In the third phase of the experiment, the pairs of beakers were arranged and presented as before, but this time first in front of a screen. The child was asked to draw a line on the screen that corresponded to the level of water in the standard beaker. Then, each pair of beakers was placed behind the screen, the water poured from the standard to the comparison beaker, and the child asked if there was still the same amount of water. Each time the water was poured behind the screen, the child was also asked to draw a second line on the front of the screen that predicted the level of water in the comparison beaker, using the line he had already drawn as a reference. The screen was then taken away. For the first time in the experiment the child actually saw the level of water in each of the comparison beakers. The child was asked at this point to judge whether there was still the same amount of water as there had been before pouring. Again, his reasons for all his answers were asked.
Finally, there was a posttest, repeating the Piagetian type "conservation" test with beakers (previously described). Frank's experiment dealt entirely with a comparison of a standard beaker and one that was either equal in diameter or wider, while both the pretest and the posttest involved the comparison of a standard beaker with others that were shorter, taller, wider, narrower, or more numerous. Thus, the experimental procedure did not constitute specific training for the posttest.

Frank reported, interestingly, that the proportion of five-year-old children achieving "conservation" on the posttest in her experiment almost trebled. And, the sixes and sevens also showed a striking rise, nearly doubling their achievement of "conservation" when compared to their pretest performance. Only among the four-year-olds did the experiment seem not to have any affect whatsoever; none of the fours could "conserve" on the pretest, and none could on the posttest.

In interpreting these and other results of Frank's study, Bruner asserts that the presence of screen leads, except in the case of the four-year-olds, almost universally to the judgment that there is the same amount in the wider glass after pouring. The screen more or less forces the children's judgments to be based on an identity argument: "It's only the same water," or "You only poured it."

However, Bruner tells us, with the removal of the screen a
striking thing happens. Nearly all of the four-year-olds, who had given a "conservation" judgment when the screen was present, say: "There is more because it is higher." They seem unable to resist the visual presence of the beakers. This, however, is not the case with the older children. They resist, and justify their position by giving some variant of the statement: "Well it looks like more to drink, but it is only the same, because it is the same water and it was only poured from there to there." These reasons of the older children, Bruner argues, reveal a conflict between identity of substance and change of appearance. And, the identity schema "wins out."\(^3\)

If Bruner is here understood correctly, he seems to be saying that the "winning out" of the identity schema (symbolic representation) over changes in appearance (ikonic representation) constitutes cognitive growth. Such "growth" is indicated in the posttest results of the five, six, and seven-year-olds in Frank's experiment. Again, Bruner seems to argue that the young child, relying heavily on his usual ikonic mode of representing events, first makes the judgments that he does in the "conservation" situation because it is a situation in which for him perceptual cues are dominant. The procedures of the earlier phases of Frank's experiment cause the child to represent the events

\(^3\)Bruner et al., Studies in Cognitive Growth, pp. 196-197.
before him symbolically. In the last phase of the experiment, the child is led to represent the events before him ikonically, and thus the perceptual rendering of the events conflicts with the earlier symbolic representations. To the extent that the child, in the last phase of the experiment, tends to reject the immediately perceptible cues in his confronting situation, in favor of the earlier more compelling symbolic alternatives, he has grown, cognitively speaking. In other words, such a rejection constitutes at least a tacit acceptance on the child's part of the imperfect correlation between "appearance" and "reality," and he is therefore less likely in the future to take perceptual cues, without questioning them, as adequate representations of his experience. Thus, cognitive growth, manifested as a willingness to doubt the immediately perceptible, explains Frank's posttest results among her five, six, and seven-year-old subjects.

One last experiment reported in Studies in Cognitive Growth deserves our detailed examination. It is one of two performed and reported by Frederic A. Mosher and Joan Rigney Hornsby on the topic of "asking questions," and it is suggestive of the extent to which the Brunerians have modified and extended their views regarding "strategies" employed by the human organism when searching its environment for information.
Mosher and Hornsby used as subjects in their study seventy-seven boys, aged six to eleven, from the first, third, and sixth grades of a suburban school near Boston. They played a variant of the old parlor game of Twenty Questions with the subjects in an effort to determine how children at various ages seek information. After having been made familiar with the game of Twenty Questions by the experimenters, the children were presented with two problems in the form: "A man was driving down the road in his car, the car went off the road and hit a tree. Find out how this happened." And, "A boy leaves school in the middle of the morning. How come?" The children were told, initially, by the experimenters that they were to find the "correct" solution to the problems with which they had been presented only by asking questions that could be answered "yes" or "no." The children were told to attempt to find the solutions with as few questions as possible. The children were further instructed, initially, that the experimenters had the "correct" solutions to the problems in mind, and that they would be told by the experimenters when they (the children) discovered it.

A record was kept of the kind of and the sequence of the questions asked by the children in attempting to solve the two problems. Moreover, when the games were finished, the children were asked to describe how they, themselves, had played the games. In this context, the
children were asked specifically: (1) "Do you think that any kind of questions were better than others for getting the answer in the fewest questions?" and (2) "Do you have a system for getting the answers?" Finally, the children were also asked which of the following questions they would rather have answered at the beginning of a game: (1) "Was there anything wrong with the man?" or (2) "Did he have a heart attack?"

Mosher and Hornsby, upon analysis of the kind and sequence of questions the children asked when playing the games, report that their subjects tended predominately to employ either of two search strategies. They called one of the strategies "constraint seeking." An idealized description of this strategy is: assume that alternative possibilities are all equally likely, try to eliminate one-half of the alternatives with each question. As seen in practice, the child begins with a general question that groups a great number of specific possibilities into two domains, in one of which the correct answer must lie. For example, a child might ask: "Did it have anything to do with the car?" He then could proceed to specific questions. The other strategy was called "hypothesis scanning." When

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37 Ibid., p. 88.
38 Ibid.
employing this strategy, the child asked a series of questions, each of which tested a self-sufficient, specific hypothesis that had no necessary relation to what had gone before. The question: "Did the driver get stung by a bee?" followed by "Was there a blinding flash of lightning?" illustrates this strategy as seen in practice.

Hypothesis scanning lives entirely on positive answers. A "no" answer to the question about the bee sting, for example, is almost useless. Constraint seeking, in contrast, can utilize both positive and negative answers—for example, if nothing was the matter with the car, then some other kind of factor must be taken into account. But, Mosher and Hornsby report that sometimes children who employed the constraint seeking strategy did not efficiently use "no" answers.

In interpreting the over-all results of their study, Mosher and Hornsby report that, in general, younger children have little consciousness of the requirements of the game of Twenty Questions, save of an answer to be guessed. Among younger children hypothesis scanning predominates as a strategy. Older children, on the other hand, particularly the eleven-year-olds, tend to recognize the requirements of the game, in that they seem to be sensitive to the efficiency of successively narrowing one's questions. Among older children, we are told, constraint seeking predominates as a strategy.
The differences in approach to the game of Twenty Questions manifested between younger and older children are accounted for, in part, by Mosher and Hornsby in terms of the mode of representation employed most frequently by the human organism at different stages of its intellectual development. They seem to argue, that is, that younger children predominately employ modes of classification typical of enactive and ikonic representation. Under enactive and ikonic representation, environmental phenomena are seldom categorized on the basis of their shared common properties. Such a way of categorizing (superordinate-equivalence categorizing, the Brunerians call it) is not typical of enactive or ikonic representation, but rather of symbolic representation. Mosher and Hornsby imply that adeptness in superordinate-equivalence categorizing is a necessary condition for the human organism's effective use of a constraint seeking strategy in searching for information. As children grow older and more adept at representing the world symbolically, concomitantly they also become more facile at constraint seeking. Thus, a difference in performance between younger and older children at games such as Twenty Questions is to be expected.

Let us conclude our attempt at summarizing the more recent theoretical work of Bruner and his collaborators via a few remarks that will serve to contrast it with their earlier work of the 1950s. Central to the earlier work was
a concern with the problem of "transfer of training." It may be recalled that the earlier work had as its focus the process of concept attainment. The human capability of proficiently applying previous experience in novel confronting situations was explained by virtue of the organism's capacity to categorize the phenomena of its world, to treat new situations as equivalent with those that it had previously handled.

Like in their earlier work, the Brunerians in their more recent work are fundamentally concerned with the problem of "transfer of training." However, in their more recent work the notions of concept (category) and conceptual system (category system) are dropped as explanatory factors. The notion of representational system (models) is substituted for them. The notion of representational system, of course, can at once be seen to have certain advantages over its categorical counterpart. For one thing, models may more easily be envisaged as evolving, in the sense of becoming progressively more sophisticated, adequate, and useful as the organism ages.

In any event, it is not the purpose of the present study to discuss the relative merits of Bruner's earlier theoretical work as against the later. Instead, the purpose is here to determine whether any of Bruner's work on cognition is useful, from an empirical scientific viewpoint. Answers to questions along this line will have to await a
survey of analytic philosophical literature in which canons for empirical scientific explanation are articulated.

Concluding Summary

In both his earlier and later psychological work, Bruner seems to see the human organism as actively constructing knowledge of its world by relating incoming information to a previously acquired mental frame of reference. In his early work, Bruner conceptualizes that frame of reference as a related system of categories or codes. In his later work, Bruner's conception of frame of reference seems to have changed. Systems of ikonic, enactive, and symbolic models are substituted for systems of categories as the basic constituents of the frame.
CHAPTER III

EPISTEMOLOGICAL CONSIDERATIONS

Introductory

The main task of this chapter is to discuss canons for empirical scientific explanation. A brief discussion of some pertinent general features of analytic philosophy is offered first. Then, three epistemological canons are discussed in detail: logical pattern in scientific explanation, logical pattern in scientific laws, and observational linkage.

Features of Analytic Philosophy

In the several complicated variations in which it exists today, analytic philosophy is very difficult to characterize in a detailed way. Were it not for the fact that in the present study it will be necessary to draw heavily upon portions of the analytic philosophical tradition, particularly in the exposition of empirical scientific canons, the problem of describing modern analytic philosophy could and would cheerfully be ignored. However, some sense of the philosophical underpinnings of the present
study needs to be communicated. So, the following broadly historical and highly selective description of analytic philosophy is offered as a point of departure.¹

One aspect of analytic philosophy that is relevant to the present study can be conceived of as a product which evolved mainly out of three kinds of empiricism in American and European philosophy: logical positivism, pragmatism, and realism. It seems fair to say that the exponents of these three distinctively empirical philosophies, in their rejection of post-Kantian speculative idealism, celebrated what they conceived to be human rationality—the propensity toward responsible, reasonable, warranted belief. Realism, to begin with, counterposed the simple stubbornness of perceived particulars to the attempt to formulate a metaphysical system that would reveal the ultimate nature of the world. The idealistic metaphysic, which construed the world as essentially mental, seemed to the realists to be a piece of egotism founded on wishful thinking, and made possible only by the idealist's lack of concern with detailed realities independent of the human will.

¹Under the present circumstances, to sketch the history of analytic philosophy in detail would be tedious and unnecessary. For more detail, see John Passmore, A Hundred Years of Philosophy (New York: Basic Books, Inc., 1966). On this subject, I have borrowed freely from Israel Scheffler, Science and Subjectivity (Indianapolis: Bobbs-Merrill Co., Inc., 1967), pp. 4-8.
Pragmatism stressed not the brute stubbornness of things plainly evident to human consciousness, but rather the control of an organism's ideas by its actions and their connected consequences in its experience. It demanded that metaphysical speculations be rejected as nonsense unless they could be reconstrued as predicting differential sensible outcomes of specified operations. It insisted, furthermore, that truths acquired their warrant through publicly verifiable anticipations of the course of experience--these often contingent upon human transformation of the environment. To propound as true a belief protected from the hard test of experience flowing out of action was, for the pragmatist, willful self-assertion or self-deception.

Logical positivism, finally, emphasized the special place of language, logic, and system in mediating the control of human belief by observational phenomena. For the logical positivist, statements were cognitively meaningful only if verifiable--only if it is clear from the language of these statements how observations might conceivably make a difference to their warrant. The observational import of a statement need not be borne on its face, but may accrue to it indirectly, through appropriate logical links with a system of other relevant statements. But to assert a statement that lacks even indirect observational bearing was, for the logical positivist, irresponsible; it was to
put forward for acceptance what one could never, even conceivably, have any experiential reason for holding true.

These three empirical philosophies all had their special problems, to be sure. Realism, for example, had particular difficulty in accounting for errors and illusions—for those cases in which the direct evidence of the senses is not a sign of truth, but a mask of falsehood. It seemed able to accommodate error only by positing mediate ideational processes (i.e., "sense data") to separate realities from illusions, thus diluting its forthright appeal to the brute being of things grasped in awareness. Pragmatism, in stressing from the beginning such mediate ideational processes, ran the opposite risk of losing altogether the hard reality of things independent of the human mind. And logical positivism found the formulation of precise criteria for cognitive meaning to be a tantalizing business: set the criteria too high and perfectly respectable areas of natural science are excluded; set them too low and mysticism and superstition are included. The ironic course of logical positivism was, in fact, one of progressive liberalization to the point where nothing could be denied cognitive meaning by reference to its criteria of observational control.

But, if all these philosophies faced crucial problems in the development of their several views of objective control over assertion, still, they never wavered
in their conviction about the central significance of independent and controlling conditions that define standards of responsibility for the act of knowing. And, they all found post-Kantian idealism wanting precisely because it failed to give an adequate account of and acknowledgment to the importance of objective control.

In their insistence on objectivity in the knowing act, logical positivism, pragmatism, and realism were influenced, if not inspired, by empirical science. For it is a deliberate policy of empirical science to expose its knowledge claims to the repeated challenge of critically probative observational data, procured under carefully controlled conditions. Moreover, in exalting the ideal of responsible control over assertion, these three philosophies have fed into and strengthened a common philosophy of science, with other roots as well, which has attained the status of a standard view, largely shared by the educated public, reflective scientists, and analytic philosophers alike, and laying great emphasis upon the objective character of scientific thought. It is to an exposition of the nature of this standard view, at least in its main features,
that we must now address ourselves.\textsuperscript{2}

For the sake of convenient analysis and exposition, the standard view of the philosophy of science can be divided into three principal parts. The first division is addressed to problems dealing primarily with the nature of scientific explanations: with their structures, their mutual relations, and their functions in inquiry. The second division concentrates on questions concerned with the logical structure of scientific concepts: with their development by way of diverse techniques of definition and measurement, their linkages to data of observation, and the conditions under which they are scientifically meaningful. The third division is directed to problems dealing with the nature of probable inference: principles used in weighing

\textsuperscript{2}The literature of the philosophy of science in general, and of scientific explanation in particular, is so vast and so technical as to almost preclude a short, minutely documented exposition. I have made the choice, therefore, to approach the subject rather informally, without the extensive use of footnotes and without the use of the notation of symbolic logic. In so doing, I have borrowed freely from the mode of presentation of the subject to be found in Ernest Nagel, The Structure of Science (New York: Harcourt, Brace, and World, 1961). Nearly all of the examples that I use are discussed in detail there. I have also been especially influenced by two other students of the subject: see, Richard B. Braithwaite, Scientific Explanation (New York: Harper and Row, 1960), and Carl G. Hempel, "On the 'Standard Conception' of Scientific Theories," in Michael Radner and Stephen Winokur, eds., Analyses of Theories and Methods of Physics and Psychology (Minneapolis: University of Minnesota Press, 1970), pp. 142-163.
evidence and validating inductive arguments. The purposes of the present study necessitate a detailed examination only of the standard view of the nature of scientific explanation. Accordingly, in the following exposition, attention will be devoted mainly to describing material falling into the first of the above tripartite divisions. But, of course, material central to the other divisions and requiring notice will receive at least brief attention.

Logical Pattern in Explanation

A meticulous examination of the conclusions of scientific inquiry has led analytic philosophers to argue that a distinctive aim of the scientific enterprise is to provide systematic and responsibly supported explanations. Such explanations may be offered for individual events, for recurring processes, or for invariable as well as statistical regularities. The analysts argue, moreover, that, while at any given time the various sciences differ in the emphasis that they place upon developing systematic explanations and in the degree of completeness of explanatory systems, still the quest for systematic explanations is never totally absent from any of the generally recognized scientific disciplines.

Explanations, as analytic philosophers understand the term, are answers to the question "Why?" Very little thought is needed to reveal that the word "why" is not
unambiguous, and that with varying contexts different sorts of answers are relevant responses to it. Consider the following list of examples of the word "why" and the associated explanation.

1. Why did the moisture form on the outside of the glass when it was filled with ice water yesterday? Here the phenomenon to be explained (hereafter called the "explicandum") is the occurrence of an individual event. Its explanation, in broad outlines, might run as follows. The temperature of the glass after it was filled with ice water was considerably lower than the surrounding air; and water vapor in air is in general precipitated whenever the air comes into contact with a sufficiently cold surface. This explanation could be reconstructed so as to exhibit the logical pattern of a formal deduction: with the major premise being a general statement about water vapor precipitating in the air; the minor premise being a statement about the temperature of the air surrounding the glass; and the conclusion being a statement describing the explicandum. Indeed, if the explanatory premises were formulated more fully and carefully, the deductive form would be unmistakable. However, the conclusion in this case is not a "necessary truth," in the sense that its denial is self-contradictory, and on the face of it neither are the explanatory premises. On the contrary, the premises are
statements which are presumably based on pertinent observational or experimental evidence.

2. Why did a smaller percentage of Catholics commit suicide than did Protestants in European countries during the last quarter of the nineteenth century? A well-known answer to this question is that the patterns of living of the Catholics made for a greater degree of social cohesion than did those of the Protestants, and that in general the existence of strongly knit social bonds between members of a community helps to sustain human beings during periods of personal stress. The explicandum in this case is a historical phenomenon that is statistically described, in contrast to the individual event of the previous example; and the proposed explanation does not, therefore, attempt to account for any individual suicide during the period under discussion. Moreover, although the explanatory premises are stated neither precisely nor completely, it is clear that they have a statistical content, just as does the explicandum. But, since the premises are not fully formulated, it is not quite clear just what the logical structure of the explanation is. We shall assume, however, that the implicit premises can be made explicit and that, furthermore, the explanation then exhibits a deductive form.

3. Why does ice float on water? The explicandum in this example is not a historical fact, whether individual or statistical, but a universal law which asserts an invariable
association of certain physical traits. It is familiarly explained by exhibiting it as the logical consequence of other laws—the law that the density of ice is less than that of water, the Archimedean law that fluid buoyed up a body immersed in it with a force equal to the weight of the fluid displaced by the body, and further laws concerning the conditions under which bodies subjected to forces are in equilibrium. It is worth noting that in this case, in contrast to the first two examples, the explanatory premises are statements of universal law.

4. Why does the addition of salt to water lower its freezing point? The explicandum in this case is once more a law, so that in this respect the present example does not differ from the immediately preceding one. Moreover, its current explanation consists in deducing it from the principles of thermodynamics, together with certain assumptions about heterogeneous mixtures. Thus, the present example does not differ from the previous one with regard to the formal pattern of the explanation. Nevertheless, this example is included for future reference because the explanatory premises display a distinctive feature of considerable epistemological interest. The thermodynamical principles included among the explanatory premises are assumptions much more comprehensive than any of the laws cited in the previous examples. Unlike those laws, these assumptions make use of notions, such as "energy" and
"entropy," that do not appear to be associated with any overtly fixed experimental procedures for identifying or measuring the physical properties those notions presumably represent. Assumptions of this sort are frequently called "theories" by analytical philosophers, and they are sometimes sharply distinguished from "experimental laws."

5. **Why** did Cassius plot the death of Caesar? The explicandum is once more an individual occurrence. If Plutarch is to be believed regarding this matter, the explanation is to be found in the inbred hatred which Cassius bore toward tyrants. However, this answer is obviously incomplete without a number of further general assumptions, such as some assumption concerning the way hatred is manifested in a given society by persons of a certain social rank. It is unlikely that such assumptions, if they are to be credible, can be asserted with strict universality. If the assumption is to be in agreement with known facts, it will be at best only a statistical generalization. For example, a credible generalization may assert that most men (or a certain percentage of men) of a certain sort in a certain kind of society will behave in a certain way. Accordingly, since the fact explained in this example is an individual historical occurrence, while the crucial explanatory assumption is statistical in form, the explicandum is not a deductive consequence of the explanatory premises. On the contrary, the explicandum in this
case is simply made probable by the latter. This is a distinguishing feature of the present example, and sets it off from the preceding ones. Furthermore, there is an important substantive difference between this and the previous examples, in that the explanatory premises mention a psychological disposition (e.g., an emotional state or attitude) as one of the springs of an action. Thus, this explanation is relevant to the question "Why did Cassius plot the death of Caesar?" only if there is some warrant for assuming that such dispositions do occur in the subject matter under consideration.

6. Why do human beings have lungs? The question as it stands is ambiguous, for it may be construed either as raising a problem in the historical evolution of the human species or as requesting an account of the function of the lungs in the human body at its present stage of evolutionary development. It is in this latter sense that the question is here intended. When so understood, the usual answer as supplied by current physiology calls attention to the indispensability of oxygen for the combustion of food substances in the body, and to the instrumental role of the lungs in conveying oxygen from the air to the blood and so eventually to various cells of the organism. Accordingly, the explanation describes the operation of the lungs as essential for the maintenance of certain biological activities. On the face of it, the explanation thus
displays a distinctive form. The explanation does not explicitly mention the conditions under which the complex events called "the operation of the lungs" occur. It states rather in what ways the lungs, as a specially organized part of the human body, contribute to the continuance of some other activity of the body.

7. **Why** does the English language in its current form have so many words of Latin origin? The historical fact for which an explanation is here requested is a complex set of linguistic habits exhibited by men during a somewhat loosely delimited historical period in various parts of the world. It is also important to note that the question "Why?" in the present example, unlike the questions in the preceding ones, tacitly calls for an account of how a certain system has developed into its current form from some earlier stage of the system. For the system under consideration, however, we do not possess general "dynamical laws of development," such as those available in physics, for example, for the development of a rotating gaseous mass. An admissible explanation for the development of the historical fact in question will, therefore, have to mention sequential changes over a period of time, and not merely a set of occurrences at some antecedent initial time. Accordingly, the standard explanation for that fact includes reference to the Norman Conquest of England, to the speech employed by the victors and vanquished before the Norman
Conquest, and to developments in England and elsewhere after
the Norman Conquest. Furthermore, the explanation assumes a
number of more or less vague generalizations (not always
explicitly stated, and some of them undoubtedly possessing a
statistical content) concerning ways in which speech habits
in different linguistic communities are altered when such
communities enter into stated relations with each other. In
short, the explanation requested in the present example is a
genetic one, whose structure is clearly more complex than the
structure of explanations previously illustrated. It is
worth noting that the complexity should not be attributed to
the circumstance that this explicandum happens to be a fact
of human behavior. A comparable complexity is displayed by
a genetic explanation for the fact that the salt content of
the oceans of the world is at present about three per cent
by volume.

The above list does not exhaust the types of answers
that are sometimes called "explanations." It is long
enough, however, to serve as evidence for the important
point that even answers to the limited class of questions
introduced by "Why" are not all of the same kind. The list
is long enough, also, to suggest that explanations offered
in the various sciences in response to such questions may
differ in the way in which explanatory assumptions are
related to their explicanda, so that explanations fall into
distinct logical patterns.
Analytic philosophers do, indeed, often distinguish different types of scientific explanations. Yet, it is a point at issue among analysts as to whether what seem to be different logical patterns of explanations are, in fact, only imperfectly formulated variants or limiting cases of some common pattern. In any event, the purposes of the present study make this issue irrelevant. So, let us now examine four major and ostensibly different patterns of explanation that analytic philosophers frequently identify.

**Covering-law Model**

A type of scientific explanation commonly reconstructed by the analysts has the formal structure of a deductive argument, in which the explicandum is a logically necessary consequence of the explanatory premises. In explanations of this type, the premises state a sufficient (and sometimes, though not always, a necessary) condition for the truth of the explicandum. This type of explanation is often called by the analysts the "covering-law model," and, although this expression is somewhat deceptive, we shall adhere to its usage.

It is worth noting at this point that analysts, while they identify the covering-law model, among others, as a pattern of scientific explanation, this should not be taken either as a claim that in so doing they have described an actual thought process engaged in by scientists when they
are doing their work, or as a claim that they have articulated a set of prescribed rules for the making of scientific discoveries or for finding satisfactory explanations for matters of established fact. Such an interpretation of the four models of scientific explanation would be a caricature of their intent, and, in any event, on that interpretation, the four models of explanation are preposterous. For no scientist thinks always in terms of neat syllogisms, and there are no rules of discovery or invention in science any more than there are such rules in the arts. The purpose of the analysts is to construct logically idealized versions of scientific explanation so as to facilitate their critical examination. The reconstruction of a given actual explanation may expose it to criticism by bringing unjustifiable chains of reasoning into clearer view. The ultimate standpoint of the analysts is thus epistemological; they are concerned, in the final analysis, with the logical persuasiveness of explanations. To this end, they primarily address neither the psychological background of an explanation, nor the actual process of its development, but rather the ideal elaboration of its product, as a purportedly logical argument.

The first four examples in the above list are prima facie illustrations of the covering-law model of explanation. However, there are significant differences between them that are worth reviewing. In both the first and second
examples, the explicandum is a historical fact. But, in the first, the fact is an individual event, while in the second it is a statistical phenomenon. In both these examples, the premises contain at least one lawlike assumption that is general in form, and at least one singular statement (whether individual or statistical). On the other hand, the explanation of the statistical phenomenon is distinguished by the presence of a statistical generalization in the premises.

In the third and fourth examples, the explicandum is a universal (as against statistical) law—a generalization which asserts the invariable (as against statistical) association of certain traits. However, the law in the third example is explained by deducing it from assumptions, each of which is an "experimental law," in the sense already indicated briefly. In the fourth example, on the other hand, the explanatory premises include what the analysts call "theories." While mention has been made of the fact that highly integrated and comprehensive systems of explanation are often achieved in science by the use of "theories," it will be necessary to inquire more closely as to what the characteristics are that distinguish "experimental laws" from "theories." This we shall do after having inspected the other three patterns of explanation that the analysts have identified.
Probabilistic Model

Many explanations in practically every scientific discipline are *prima facie* not of the covering-law type, since on reconstruction their explanatory premises do not formally imply their explicanda. Nevertheless, although the premises are logically insufficient to secure the truth of the explicandum, they are said to make the latter *probable*. Analysts often call this type of explanation "probabilistic."

Probabilistic explanations are usually encountered when the explanatory premises contain a statistical assumption about some class of elements, while the explicandum is a singular statement about a given individual member of that class. This type of explanation is illustrated by the fifth example on the above list. Were this explanation formulated more explicitly, it might run as follows. In ancient Rome the probability was high (e.g., greater than one-half) that an individual belonging to the upper strata of society and possessed by great hatred of tyranny would plot the death of men who were in a position to secure tyrannical power. Cassius was such a Roman and Caesar such a potential tyrant. Hence, although it does not follow that Cassius plotted the death of Caesar, it is highly probable that he did so.

It is sometimes maintained by analysts that probabilistic explanations are only temporary halfway stations on the road to covering-law explanations, and do not, therefore, constitute a distinct type. All that need be done,
so it has been suggested, is to replace the statistical assumptions in the premises with a strictly universal law—in the above illustration, for example, with a statement asserting an invariable association between certain carefully delimited sociopsychological traits (which Cassius presumably possessed) and participation in assassination plots. This suggestion is not without merit, but in many scientific disciplines it is extremely difficult to assert even with moderate plausibility strictly universal laws. Frequently, the best that can be established with some warrant is a statistical regularity. Accordingly, probabilistic explanations cannot be ignored, on pain of excluding from the discussion of the logic of explanation important areas of investigation.

It is crucial not to confuse the question whether the premises of an explanation are known to be true, with the question whether an explanation is of the probabilistic type. It well could be that in no scientific explanation are the general assumptions contained in the premises known to be true, and that every such assumption can be asserted as only "probable." Even if this is so, it does not abolish the distinction between the covering-law and probabilistic types of explanation. For the distinction is based on differences in the way in which the premises and the explicanda are logically related to one another, and not on any supposed differences in our knowledge of the premises.
It should be noted, finally, that it is still an unsettled issue among analysts as to whether an explanation must contain a statistical assumption in order to be a probabilistic one, or whether nonstatistical premises may not make an explicandum "probable," in some nonstatistical sense of the word. Nor are analysts in general agreement as to how the relation between premises and explicanda is to be analyzed, even in those probabilistic explanations in which the premises are statistical and the explicanda are statements about some individual.

**Functional Model**

In many contexts of inquiry—especially, although not exclusively, in biology and in the study of human affairs—explanations take the form of indicating one or more functions that a component performs in maintaining or realizing certain traits of a system to which that component belongs. Such accounts are commonly called "functional explanations" by analysts. It is characteristic of functional explanations that they employ such typical locutions as "in order that," "for the sake of," and the like. This type of explanation is illustrated by the sixth example on the above list. The occurrence of the lungs in the human body is there explained by showing that they operate in a stated manner in order to maintain a certain
chemical process and thereby to assure a continuance of life for the body.

Function explanations, the analysts suggest, contain in them two basic notions: the notion of a system, the components of which can be clearly delineated; and the notion of a state or condition of the system, necessary for the maintenance of the system's equilibrium. A function explanation, when it is achieved, states that a particular component (e.g., the lungs) of a given system (e.g., the human body) is instrumental in maintaining the system in a stable state (e.g., life). At minimum, a full-blown function explanation must explicitly state, or at least implicitly assume, two sorts of laws: one asserting that some condition is necessary for the maintenance of a system, and another asserting that some component of the system is necessary for the fulfillment of this condition.

In the functional explanations of biology, the systems commonly examined are individual organisms; and the states of the systems that are considered include among others the survival of the organism (i.e., the condition of being a living organism), certain characteristic activities of some organ, the internal temperature of the organism, and the chemical state of some internal fluid such as the blood. There is usually no difficulty in biology in specifying what is an individual organism. Moreover, a number of easily identifiable activities carried on by
organisms (viz., the "vital functions," such as respiration and assimilation) are generally recognized to constitute the defining attributes of being alive. Similarly, adequate definitions for particular organs, for their characteristic activities, and for other states of organisms that may be investigated can normally be supplied without much trouble. Consequently, since the system and the state can be clearly specified in biology, it is intelligible to ask, and to seek an answer by experimental inquiry, whether, and if so by what mechanisms, a system is maintained in a particular state.

The situation in these respects is notoriously different in the social sciences. The systems discussed by, say, anthropologists frequently are individual societies or communities; and the states of these systems that have been of concern to them include the survival of a society, its social structure, and the norms of social behavior prescribed or overtly manifested in a society. As in biology, it is often possible in social inquiry to designate unambiguously the systems being investigated—with relative ease in primitive societies, though with increasing difficulty in more industrialized ones. On the other hand, in regard to the condition of survival by a society, there is nothing comparable in this domain to the "vital functions" of biology as defining attributes of living organisms. Societies do not literally die, although to be sure a
society may disappear because all the people who constitute it die without leaving heirs or are permanently dispersed. It is, therefore, not easy to fix upon a criterion of social survival that can have fruitful uses and not be purely arbitrary. Accordingly, adequate function explanations in the social sciences are very difficult to attain.

**Genetic Model**

One last kind of explanation that the analysts identify remains to be mentioned, although it is a debatable question as to whether it constitutes a distinctive type. Historical inquiries frequently undertake to explain why it is that a given subject of study has a certain configuration of characteristics, by describing how the subject has evolved out of some earlier configuration. Such explanations are commonly called "genetic" by analysts, and they have been given for animate as well as inanimate things, for traits of an individual as well as for characteristics of a group. The seventh example in the above list illustrates this type.

The task of a genetic explanation is to set out the sequence of major events through which some earlier system has been transformed into a later one. The explanatory premises of a genetic explanation will, therefore, necessarily contain a large number of singular statements about past events in the system under scrutiny. Not every past event in the career of the system will be mentioned.
Those events which are mentioned are selected on the basis of assumptions (frequently tacit ones) about what sort of events are causally relevant to the development of the system. Thus, in addition to singular statements the premises will also include (sometimes only implicitly) general assumptions about the causal dependencies of various kinds of events.

These general assumptions may be fairly precise developmental laws, for which independent evidence is available. This may happen when the system under study can be regarded as a member of a class of similar systems which undergo a similar evolution—for example, in the study of the development of biological traits of an individual member of some species. It is then often possible to use methods of comparative analysis to establish such developmental laws. In other cases, the general assumptions may be only vague generalizations (with, perhaps, a statistical content), and may contain no reference to some of the specific features of the subject matter under study. This often happens when the system investigated is a relatively unique one—for example, when the development of some social institution in a particular culture is studied. However, in neither case do the explanatory premises in familiar cases of genetic explanations state the sufficient conditions for the occurrence of the explicandum. What is often stated in the premises are some of the conditions which, under
circumstances generally taken for granted, are necessary for the occurrence of the phenomenon to be explained. It is, therefore, a reasonable conclusion that genetic explanations are in the main probabilistic.

**Enthymematic Form**

These four major types of explanation distinguished by analytic philosophers have been outlined, here, because they appear to correspond to real structural differences in the examples that have been surveyed, and because the classification provides a convenient framework for examining important issues that will occupy us in the next chapter. Before turning away from the outline of explanatory patterns developed in the present chapter, however, let us consider a related matter.

Analytic philosophers pointedly emphasize that many, if not most, explanations actually presented in the sciences, when they are inspected from a logical point of view, are elliptically formulated, or enthymematic in form. Which is but to say that they are logically incomplete, in a rather harmless sense. Perhaps, this point can be illustrated by a brief reconsideration of the covering-law model of explanation.

The covering-law model requires of an explanation, among other things, that the sentence in it which describes the explicandum follow logically from the explanatory
premises. It is hardly disputable, however, that many explanations presented in the sciences that analytic philosophers would reconstruct on the deductive model ostensibly fail to realize this rigorous deductive form. For example, the expansion of a piece of wire on a given occasion may be explained by citing the fact that the wire had just been heated; and it is evident that the explicandum does not follow logically from the explanatory premises as stated. However, it seems most plausible to the analysts that such a proposed explanation tacitly assumes additional premises—for example, that the wire is copper and that copper always expands when heated. When these assumptions are made explicit, the explanation does conform to the "deductive requirement" of the covering-law model.

Logical Pattern in Laws

The requirements for adequate scientific explanations considered thus far have been discussed with only incidental reference to the nature of the relations asserted by scientific laws. Yet, as it has been indicated, laws are an essential ingredient in scientific explanations. Accordingly, a brief survey of the types of laws used as explanatory premises in various sciences is in order.

Laws of Kinds

A basic and pervasive type of law is involved in the
assumption that there are "natural kinds" or "substances." Let us understand by a "determinable" a property such as color or density, which has a number of specific or "determinate" forms. Thus, among the determinate forms of the determinable color are red, blue, and yellow; and among the determinate forms of the determinable density are the density with the magnitude 100 (when measured in some standard fashion), the density with the magnitude 200, and the density with the magnitude 300. The determinate forms of a given determinable thus constitute a "related family" of properties such that every individual to which the determinable property can be ascribed must, of logical necessity, have only one of the determinate forms of the determinable. A law of the type under consideration (e.g., "There is a substance rock salt") then asserts that there are objects of various kinds, such that every object of a given kind is characterized by determinate forms of a set of determinable properties, and such that objects belonging to different kinds will differ in at least one (but usually more than one) determinate form of a common determinable. For example, to say that a given object is rock salt is to say that there is a set of determinable properties (crystalline structure, color, melting point, etc.) such that under standard conditions the object has a determinate form of each of these determinables (it has cubical crystals, it is colorless, it has a melting point of 804° C,
etc.). Accordingly, laws of this type assert that there is an invariable concomitance of determinate properties in every object that is of a certain kind.

**Laws of Invariable Sequence**

A second type of law asserts an invariable sequential order of dependence among events or properties. Two subordinate types can be distinguished. One of these is the class of causal laws, such as the law "Whenever a spark passes through a mixture of hydrogen and oxygen gas, the gases disappear and water is formed." A second subordinate type is the class of developmental laws, such as the law "The formulation of the lungs in the human embryo never precedes the formation of the circulatory system." Both subordinate types are frequent in areas of science in which quantitative methods have not been extensively introduced, although, as the examples indicate, such laws are encountered elsewhere as well. Analysts term a law "causal" apparently because the relation it asserts between the events mentioned in it supposedly satisfies four conditions. First, the relation is an invariable or uniform one, in the sense that whenever the alleged cause occurs so does the alleged effect. Second, the relation holds between events that are spatially contiguous, in the sense that the spark and the formation of water occur in approximately the same
spatial region. Third, the relation has a temporal character, in the sense that the event said to be the cause precedes the effect and is also "continuous" with the latter. And finally, the relation is "asymmetrical," in the sense that the passage of the spark through the mixture of gases is the cause of their transformation into water, but the formation of the water is not the cause of the passage of the spark. Developmental laws are not regarded as causal by analysts apparently for the reason that they generally state relations of sequential order between events separated a temporal interval of some duration. Consequently, developmental laws are sometimes regarded as representing only an incomplete analysis of the facts, on the ground that, since something may intervene after the earlier event to prevent the realization of the later one, the sequential order of events is not likely to be invariable.

Statistical Laws

A third type of law, common in the biological and social sciences as well as in physics, asserts invariable statistical relations between events or properties. An example of such a law is: "If a geometrically and physically symmetrical cube is repeatedly tossed, the probability that the cube will come to rest with a given face uppermost is 1/6." Statistical laws do not assert that
the occurrence of one event is invariably accompanied by the occurrence of some other event. They assert only that, in a sufficiently long series of trials, the occurrence of one event is accompanied by the occurrence of a second event with an invariable relative frequency.

**Laws of Functional Dependence**

A fourth type of law, characteristic of modern physical science, asserts a relation of functional dependence (in the mathematical sense of "function") between two or more variable magnitudes with stated properties or processes. Two subtypes can be distinguished. One of these is the class of numerical laws stating an interdependence between magnitudes such that a variation in any one of them is concurrent with variations in the others. An example of such a law is the Boyle-Charles law for ideal gases, that \( pV = aT \), where "\( p \)" is the pressure of the gas, "\( V \)" its volume, "\( T \)" its absolute temperature, and "\( a \)" a constant that depends on the mass and the nature of the gas under consideration. The other subtype is the class of numerical laws asserting in what manner a magnitude varies with time, and, more generally, how a change in a magnitude per unit of time is related to other magnitudes (in some cases, to temporal durations). Galileo's law for freely falling bodies in a vacuum is an illustration of such a law. It says that the change in distance per unit of time of a
freely falling body is equal to gt, where "g" is a constant and "t" is the duration of the fall. Laws that belong to this latter subtype and which formulate the structure of a temporal process are often called "dynamical laws."

**Linkage to Observation**

Scientific thought, the analysts tell us, takes its ultimate point of departure from problems suggested by observing things and events encountered in common experience; it aims to understand these observable things by discovering some kind of order in them; and its final test for the laws that serve as instruments of explanation is their concordance with such observations. Indeed, many scientific laws assert relationships between things or features of things that are commonly said to be themselves observable, whether with the unaided senses or with the help of special instruments of observation. The law that when water in an open container is heated it eventually evaporates is a law of this kind, as well as the law that the period of a simple pendulum is proportional to the square root of its length.

However, not all laws in the sciences are of this kind. In point of fact, many laws employed in some of the most impressively comprehensive explanatory systems of the physical sciences are clearly not about matters that would ordinarily be characterized as "observable," even when the
word "observable" is used as broadly as in the examples of the preceding paragraph. Thus, when the evaporation of heated water is explained in terms of assumptions about the molecular constitution of water, laws of this latter sort appear among the explanatory premises. Although we may have good observational evidence for these assumptions, neither the molecules nor their motions are capable of being observed, in the sense in which, for example, the period of a simple pendulum is said to be observable.

Analysts note the _prima facie_ difference between these two sorts of laws by calling those laws which formulate relations between observable (or experimentally determinable) traits of some subject matter "experimental laws." And, laws of the other sort, which contain in them terms that ostensibly designate nothing observable, are called "theoretical laws" (or simply "theories"). Yet, as analysts note, these labels are not free from misleading associations, and the distinction to which they refer is one that, at best, is of degree and not of kind. Thus, when a statement (e.g., "All whales suckle their young") is classified as an experimental law, it is not to be construed as asserting that the law is based on laboratory experiments or that the law happens to be one for which there is thus far no explanation. The rubric "experimental law" signifies simply that a statement so characterized asserts a relation between things (or traits of things) that are observable, in
an admittedly loose sense of "observable," and that the law can be validated by controlled observation of the things mentioned in the law. And, when a set of assumptions about the molecular constitution of liquids is called a theory, it is not to be understood as asserting those assumptions to be entirely speculative and unsupported by any cogent evidence. What is intended by this characterization is simply that those assumptions employ terms like "molecule," which ostensibly designate nothing observable (in the loose sense), and that the assumptions cannot be confirmed by experiments or observation of the things to which such terms ostensibly refer.

Moreover, it would certainly be a mistaken claim that scientific statements cited as typical illustrations of experimental laws assert relations apprehended directly or noninferentially through the various sense organs. Consider, for example, the experimental law that the velocity of sound is greater in less dense gases than in more dense ones. This law obviously assumes that there is a state of aggregation of matter known as "gas" which is to be distinguished from other states of aggregation such as liquid and solid; that gases have different densities under determinate conditions, so that under specified conditions the ratio of the weight of a gas to its volume remains constant; and that instruments for measuring weights and volumes, distances and times, exhibit certain regularities
which can be codified in definite laws, such as laws about mechanical, thermal, and optical properties of various kinds of materials. It is clear, therefore, that the very meanings of the terms occurring in the law, for example, the term "density," and in consequence the meaning of the law itself, tacitly assume a host of other laws. Furthermore, additional assumptions become evident when we consider what is done when evidence is adduced in support of the law. For example, in measuring the velocity of sound in a given gas, different numerical values are in general obtained when the measurement is repeated. And, if a definite numerical value is to be assigned to the velocity, these different numbers must be "averaged" in some fashion, usually in accordance with an assumed law of experimental error. Accordingly, the law about the velocity of sound in gases does not formulate relations between the immediate data of sense. It deals with things which can be identified only via procedures involving fairly complicated chains of inference and a variety of general assumptions.

It is pertinent to note, also, that reports of what are commonly regarded as experimental observations are frequently couched in language of what is admittedly some theory. For example, experiments on beams of light passing from a given medium to a denser one show that the index of refraction varies with the source of the beam. Thus, a beam issuing from the red end of the solar spectrum has a
different index of refraction than has a beam coming from the violet end. However, the experimental law based on such experiments is not formulated in unquestionably observational terms (e.g., in terms of the visible colors of light beams), but in terms of the relation between the index of refraction of a light ray and its wave frequency. The ideas of the wave theory of light are thus absorbed into the statement of the presumably experimental law.

On the other hand, although the commonly cited examples of theories are statements about things that in an obvious sense are unobservable, it is frequently possible to determine indirectly, via inferences drawn from experimental data in accordance with postulates of the theories, important characteristics of what is ostensibly not observable. Consider, for example, the theory of the atom devised by Niels Bohr in order to explain, among other things, experimental laws about the line spectra of various chemical elements. This theory has now been replaced by a more satisfactory theory, but it was successful in explaining a number of experimental laws of spectroscopy. In very brief outline, the theory postulates the following. It assumes that there are atoms, each of which is composed of a relatively heavy nucleus carrying a positive electrical charge, and a number of negatively charged electrons with smaller mass moving in approximately elliptic orbits with the nucleus at one of the foci. The number of electrons
circulating around the nucleus varies with the chemical elements. The theory further assumes that there is only a discrete number of permissible orbits for the electrons; that the electromagnetic energy of an electron in an orbit depends on the diameter of the orbit; and that, as long as an electron remains in any one orbit, its energy is constant and the atom emits no radiation. On the other hand, an electron may "jump" from an orbit with a higher energy level to an orbit with a lower energy level; and, when it does so, the atom emits electromagnetic radiation, the wave length of which is a function of these energy differences. On the face of it, the electrons, their circulation in orbits, and their jumps from orbits to orbits in the Bohr theory are all notions that do not apply to anything manifestly observable. Unless something further is added to the theory to indicate how its implicitly defined terms are related to matters which are observable, then the theory cannot be significantly affirmed or denied, and, in that case, it would be scientifically almost useless. Accordingly, connections between the implicitly defined notions of the theory and what can be identified via laboratory procedures are introduced somewhat as follows. On the basis of the electromagnetic theory of light, a line in the spectrum of an element is associated with an electromagnetic wave, the length of which can be calculated from experimental data on the position of the spectral line. Since the Bohr theory
associates the wave length of a light ray emitted by an atom with the jump of an electron from one orbit to another, it becomes possible to link the theoretical notion of an electron jump to the experimental notion of a spectral line. Once this link and other similar links are introduced, the experimental laws concerning the series of lines occurring in the spectrum of an element can be deduced from the theoretical assumptions about the transitions of electrons from their permissible orbits.

For these several reasons many analysts have concluded that the labels "experimental law" and "theory" do not signify laws fundamentally different in kind, but designate what is at best only differences in degree. To analysts who hold this view, the distinction is of little use.

However this may be, from the point of view of the present study, the distinction between experimental laws and theoretical laws is helpful, in that it allows us to articulate two requirements that the premises in any scientific explanation must satisfy if they are to be capable of empirical validation. In the first place, whether the premises of an explanation are formulated in terms of experimental or theoretical laws, it must be possible to infer from them determinate consequences, so that one can decide on the basis of logical considerations, and prior to the examination of any empirical data, whether
or not an alleged consequence of the explanatory premises is indeed implied by the latter. For unless this requirement is fulfilled, the explanatory premises have no definite content, and questions as to what the premises exactly assert cannot be settled except by recourse to some privileged authority or arbitrary caprice. Consider, for example, the rather extreme case of the view that the mutual gravitational attraction of physical bodies is a manifestation of certain "appetites or natural tendencies," closely related to love, inherent in these bodies, which makes their "natural movements intelligible and possible." What determinate consequences can be derived from this interpretation of gravitational phenomena? Considering some characteristic aspects of love, in the familiar sense of the word, this view seems to imply that gravitational affinity should be a selective phenomenon: not just any two physical bodies should attract each other. Nor should the strength of the affinity of one body to a second one always equal that of its converse; nor should it depend significantly on the masses of the bodies or on their distance apart. Since all of the consequences thus suggested are known to be false, the explanatory premises

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we are now considering evidently are not meant to imply them. However, in point of fact, these explanatory premises are so elusive as to preclude the derivation of any determinate consequences from them. Thus, no conceivable observational data can affirm or deny them.

In the second place, even if the premises of an explanation are formulated in a way so as to include theoretical notions that are not explicitly defined via overt empirical procedures and observable traits of things, nevertheless at least some of the theoretical notions must be tied down to fairly definite and unambiguously specified observational materials. For if this condition is not satisfied, the explanatory premises can have no determinate consequences about empirical subject matter.

An immediate corollary to these requirements is that since consistent explanatory assumptions cannot imply two incompatible consequences, credible explanatory premises must not only be capable of being confirmed by observational evidence, but they also must be capable of being negated by such evidence. In short, explanatory premises must not be formulated in such a manner that they can always be construed and manipulated so as to explain whatever the actual facts are, no matter whether controlled observation shows one state of affairs to obtain or its opposite.
Concluding Summary

The main task of the present chapter has now been accomplished. We have discussed in some detail three related epistemological canons of empirical scientific explanation. It is with respect to these canons that the cognitive psychology of Bruner and his associates needs to be evaluated. And, it is to such matters that we must now turn.
CHAPTER IV

EPISTEMOLOGICAL CRITIQUE OF BRUNERIAN
COGNITIVE PSYCHOLOGY

Introductory

The primary task of this chapter is the evaluation of the major explanations in Brunerian cognitive psychology (described in Chapter II) in terms of three epistemological canons (described in Chapter III). Two important groups of philosophical issues occupy us first. Then, the major Brunerian psychological explanations are identified and critiqued.

Two Groups of Related Philosophical Issues

It seems hardly possible to begin an evaluation of the logical and empirical credentials of Bruner's psychology without first attempting to identify and to discuss some philosophical issues that, while they may have only an indirect bearing on the problems at hand, nevertheless, might generate some needless confusion and misunderstanding. Roughly speaking, these philosophical issues may be divided into two groups. The first group has to do with the
possibility of a scientific psychology—whether a science that seeks to explain human conduct is capable of realization. The second group has to do with the methodological proposition, broadly to the effect that any valid explanation of human behavior must always (as a practical matter, if not in principle) formulate its explanatory premises only in terms which refer to either the overt conduct of the human organism or to the publicly observable features of its environment. These two groups of issues intertwine to some extent and they are very complicated. A full discussion of them in the present study would be pointless. However, aspects of both groups of issues do merit brief attention.

At the center of the debate about the possibility of a scientific psychology seems to be the view that the behavior of man will always elude scientific explanation because, somehow in principle, scientific laws have no role to play in explaining human conduct. To those who hold this sort of view, the explanation of human behavior can and, indeed, must be accomplished in ways other than the scientific.

It is worth notice that the notion that human conduct is somehow beyond the scientific pale is one which has had a long and checkered history; no doubt it originated somewhere in remote antiquity; and it has found, from time to time, particular favor among the world's religionists. In any event, the notion was argued in a distinctively
forceful way toward the end of the nineteenth century, at
about the time of the inception of the modern social
sciences.\textsuperscript{1} Of course, social scientific inquiry was
attempted despite the views of those who held that it was
impossible. And, almost no one today would argue that the
modern social sciences have been totally unfruitful since
their initial days. Yet, so far as scientific explanation
is concerned, at any rate, the achievements of the modern
social sciences appear to have been very modest, especially
when they are contrasted with those of the physical sciences.
Moreover, this situation is particularly true of psychology.
Although undoubtedly millions of man-hours of research have
been devoted in psychology to the scientific study of human
behavior, few (philosophers of science, if not psycholo­
gists) will question the assertion that in psychology
neither has a body of knowledge been accumulated nor have
explanatory systems been achieved which compare favorably in
scope, precision, and degree of reliability with what the
physical sciences have to offer. The situation of psy­
chology is, as one philosopher aptly puts it, that

\begin{quote}
In many important respects . . . the current debate
about the explanation of human behavior lacks focus and
definition. There is no consensus even about those
\end{quote}

\textsuperscript{1}I have in mind, particularly, the arguments of
Wihelm Dilthey (1833-1911). See, e.g., Don Martindale,
\textit{The Nature and Types of Sociological Theory} (Boston:
analytical and pre-scientific issues on which the construction of a "disciplined" science of human behavior depends: e.g., about what kinds of things are puzzling about human behavior at all, what tests a fruitful new psychological concept must pass, or what a comprehensive theory of human behavior should seek to achieve. So long as this is the case, it can be no surprise that, on a more substantive level, psychology still lacks an agreed body of theoretical concepts, or that one psychologist's great discovery often strikes other psychologists as a waste of energy and breath. 2

This condition of the social sciences in general and of psychology in particular, together with other factors, has led to a reopening of the philosophical debate as to whether a science of human behavior is, in fact, possible. Many of the old arguments against the possibility of a scientific psychology have been revived and restated. Perhaps, the most interesting of these has to do with the belief in the freedom of the human will. It is argued that, since men are free to behave as they choose, they are always capable of nullifying any universal law about their conduct to which they are alleged to be subject. If any such law is produced, it is only to be expected that someone will proudly or perversely exercise his option of rendering it false.

The real trouble with this argument is that it simply assumes the falsehood of the position it seeks to

demolish. If the attribution of free will is construed in such a way that a man can be said to have acted freely only if his action is not susceptible of an explanation which contains within it a universal law, then there certainly will be no question that, if men ever act freely, their behavior is not explicable by appeal to universal laws. This still allows for the possibility of the explanation of human behavior by appeal to statistical laws. However, on the assumption which proponents of this view tacitly make—that a man is free on any given occasion to try anything whatsoever he believes to be feasible—the possibility of there being even statistical laws which would be of any scientific value is rather effectively excluded.

Surely, it is fair to ask for some justification of the very strong assumptions in this argument. What reason have we for believing that men ever do act freely in the sense in question? Without doubt, at least a prima facie case can be made for holding that men act freely, in some sense or other; but it is by no means clear why an action which can pass this test of freedom, whatever it may be, cannot also be the subject of an explanation, the premises of which contain scientific laws in them. Many philosophers have, in fact, held that what we ordinarily mean by speaking of an action as freely done is not incompatible with its being subject to universal or statistical law. Others have gone even further to the point of holding that, when we say
that a human action is free, we actually imply, or presuppose, that it is capable of scientific explanation. Still other philosophers, who grant that the scientific explanation of human behavior excludes the possibility of free will, as the notion of free will is ordinarily understood, conclude for just this reason that our ordinary notion of free will has no application. So the spirited controversy goes.

All this controversy notwithstanding, however, it appears that the issue as to the possibility of a science of human behavior is not a matter which one could hope to settle by strictly a priori conjectures about free will (or anything else, for that matter). For, it seems perfectly sound to say that we can never be in a position to show that any given piece of human conduct is scientifically inexplicable: the most that we can claim is that we have not, as yet, been able to find any scientific explanation for it. Such a claim does not necessarily imply that there is no scientific explanation, or even that such an explanation will always be beyond our power to discover. Although this is probably the sort of argument which seemed much more convincing in the incipient days of scientific psychology than it does today, it seems still to be valid.

While the immediately foregoing argument may be perfectly sound, some analytic philosophers maintain that it also misses the point. They call attention to the fact that, when we ordinarily explain human behavior during the course of our everyday activities, we do not usually employ scientific modes of explanation. They often allow that explanations of the scientific type are sometimes appropriate in daily life, as when we account for some piece of human action by relating it to a disorder in the actor's body. But, cases of this kind are held to be the exception rather than the rule. In daily life, we usually explain a man's action in terms of his beliefs, his intentions, his motives, his values, or the social context in which the action takes place. Consider, for example, the action of drinking a glass of wine. As performed by different people under different circumstances, the drinking of a glass of wine may be a religious communication, a manifestation of loyalty, a piece of inadvertence, a proof of alcoholism, an expression of politeness, an attempt to seduce or corrupt another person, an attempt to summon up one's courage, the performance of a social rite, the sealing of a bargain, and

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5The example is suggested by A. J. Ayer.
many other things besides. All of these are usually accepted by us in daily life as adequate explanations: if the circumstances are right, they render the performance of the action intelligible. But, only in the case of the alcoholic does it seem intuitively plausible that the explanation (if made complete) is of a scientific character. In the other cases, we find the action intelligible because it is explained either in terms of the actor's reasons and intentions, or by reference to social expectations, or through some combination of these.

Taking as a leading point these familiar cases of more or less ad hoc explanations for human conduct that we accept in daily life, it is further argued by some analytic philosophers that the search for scientific laws with which to explain human behavior is a basically misguided enterprise. They contend, moreover, that the ad hoc explanations of common sense should serve as models for inquiry about human action, and that scientific models should not. Such contentions, accordingly, raise a number of quite difficult questions. Exactly how do explanations in terms of beliefs, intentions, and reasons operate, and in what specific ways do they appear to differ logically from scientific explanations? What kind of an explanation of an action, logically speaking, do we obtain when we are able to fit it into a social context, or see it as fulfilling a social expectation? Again logically speaking, even if reasons can be
found for saying that these *ad hoc* explanations are not, or are not wholly, of a scientific character, does it follow that the actions which they explain cannot also be explained in ways which *do* conform to scientific models? In other words, assuming that we do have to deal here with two or more radically different sorts of explanations, are we bound to conclude that they are mutually exclusive?

To attempt even a partial development of adequate answers to questions such as those just raised would lead us far beyond the scope of the present study. Suffice it here to say that what the correct answers are to such questions is a matter of significant and, to some extent, unresolved dispute among analytic philosophers. Yet, in fairness it should also be said that the present writer sees the dispute ultimately being resolved in favor of those who hold that the two different kinds of explanations are not mutually exclusive, and that, accordingly, a science of human behavior is within the realm of possibility. In any event, for purposes of the present study, the possibility of the scientific explanation of human behavior will simply be assumed.

Let us now turn our attention to a very brief consideration of the methodological proposition that any valid explanation of human behavior must have its premises formulated in such a way as to refer always to publicly observable (and preferably quantifiable) subject matter.
Some version or near-version of this methodological doctrine has been about in psychological circles for a long time. It is fairly clear from the history of psychology (as was remarked in Chapter II) that the early behaviorists had something like this doctrine in mind when they argued for the rejection of introspection and introspective techniques as research strategies in psychological inquiry. It is equally clear that among psychologists today the views of the early behaviorists are not as fashionable as they once were. Yet, the words written by two philosophically sophisticated psychologists in 1948 have a ring of truth to them even today.

... One can still "observe among "toughminded" psychologists the use of words such as "unobservable" and "hypothetical" in an essentially derogatory manner, and an almost compulsive fear of passing beyond the direct colligation of observable data. "Fictions" and "hypothetical entities" are sometimes introduced into a discussion of theory with a degree of trepidation and apology quite unlike the freedom with which physicists talk about atoms, mesons, fields, and the like.°

Moreover, there also seems to be a tendency in contemporary psychology to treat all terms that refer to unobservable subject matter as on the same footing merely because they share the attribute of having unobservable referents; so we

find people arguing that if neutrons are admissible in
physics, it must be admissible to talk about, say, the
damming up of libido and its reversion to earlier channels.

Philosophical matters related to this state of
affairs in psychology were discussed at length in Chapter
III, but it is now worth underscoring perhaps the crucial
point of that chapter. So far as the present study is
concerned, extensive debate among analytic philosophers has
made it plain that scientific inquiry cannot, on the basis
of the record of scientific achievement, be identified only
with the study of observable subject matter. To the
contrary, the debate has revealed that the introduction
into scientific discourse of terms which refer to unobservable
subject matter (and which are ultimately, but only
indirectly, linked with observation) deserves to be regarded
as a hallmark of scientific inquiry. Nevertheless, the
point here is not that a methodological program in
psychology which proposes to deal only with observable
subject matter should be ruled out on philosophical grounds
either as illegitimate or intrinsically unfruitful. The
point is, rather, that a methodological program in psychology which permits the positing of unobservable phenomena should not be dismissed as out of hand on philosophical grounds merely because it utilizes terms with unobservable
referents.

Methodological programs in a scientific psychology
probably should be judged from a philosophical standpoint mainly according to the product that emanates from them. It is the explanatory adequacy of that product that, philosophically speaking, matters, and not necessarily the means by which that product was obtained. In any event, Bruner's research strategy is one which considers the positing of unobservables as permissible. And, his cognitive psychology is richly laced with terms that refer to unobservable events and processes. It is the explanatory adequacy of the products of that research strategy to which we must now turn.

The Functions of Categorizing

Among the central notions of Bruner's early work on cognition are "category" (or "code" or "concept"), "categorizing" (or "coding"), and "category system" (or "coding system"). Each of these terms, of course, refer to some sort of unobservable phenomena in the human organism—in the sense that the existence of the event, process, or entity in question is inferred by the investigator from certain aspects of the organism's overt behavior. It may be recalled that, according to Bruner, the human organism's capacity to categorize enables it to operate successfully in its very complex environment.

Early on in A Study of Thinking, Bruner inquires as to what the "act" of categorizing accomplishes for the
human organism. Bruner's remarks as regards this inquiry make it clear that he recognizes that such an inquiry is of a functional nature.\footnote{Jerome S. Bruner, Jacqueline J. Goodnow, and George A. Austin, \textit{A Study of Thinking} (New York: John Wiley & Sons, Inc., 1956), pp. 11-12.} And, in fact, his basic question may be likened to the query of a physiologist who asks: "What does the heartbeat accomplish for vertebrates?" Generally speaking, the kind of phenomenon that a functional inquiry aims to explain is typically a recurrent set of events or processes. Moreover, a functional inquiry seeks to explain a particular phenomenon by considering it as a component of a system of phenomena, and by determining the role that the component plays in keeping the system to which it belongs in proper working order or maintaining it as a going concern. The logical pattern of functional explanations was discussed at length in the last chapter (see pages 90-91, 98-101), but these matters now merit further brief re-examination. They are crucial in our first effort to evaluate aspects of Bruner's work.

Let us proceed by way of a simple illustration. Consider first the statement: The heartbeat in vertebrates has the \textit{function} of circulating blood throughout the organism. At first blush, it might be held that all the information conveyed by such a statement could be expressed just as well by substituting the word "effect" for the word
"function." However, this construal would force us to assent also to the statement: The heartbeat in vertebrates has the function of producing heart sounds; for the heartbeat has that effect. A physiologist would, of course, refuse to assert the latter statement on the ground that heart sounds are an effect of the heartbeat which is of no importance to the operation of the organism; whereas the circulation of the blood effects the transportation of nutriment to, and the removal of waste materials from, various parts of the organism—a process that is indispensable if the organism is to remain in proper working order. We should notice next that the heartbeat will perform the function attributed to it only if certain conditions are met by the organism and by its environment. For example, circulation will fail if there is a rupture in the aorta. Most of such conditions would be left unmentioned by a physiologist aiming functionally to explain the heartbeat in vertebrates, at least partially because they would be assumed to be satisfied as a matter of course in situations in which the organism normally finds itself. In any event, the considerations here outlined suggest that the basic pattern of a functional explanation is as follows: The object of the explanation is some item "i," which is a relatively persistent event, process, or disposition (e.g., the heartbeat) occurring in a system "s" (e.g., the body of a living vertebrate); and the explanation aims to show that
"s" is in a state or internal condition, and in an environment representing certain external conditions (hereafter jointly referred to as "c"), such that, given "c," "i" has effects which satisfy some operational requirement, "n," of "s," a requirement that is necessary for the system's remaining in adequate, effective, or proper working order. Furthermore, the statements that "i," in the specified setting "c," has effects that satisfy "n," and that "n" is a necessary condition for the proper functioning of "s," both involve general laws. The assertion that a condition "n" constitutes a functional requisite for a state of some specified kind (such as proper functioning) is tantamount to the statement of a law to the effect that whenever condition "n" fails to be satisfied, the state in question fails to occur. Bearing these notions in mind, let us return to our evaluation of Bruner's attempt functionally to explain the act of categorizing in the human organism.

In *A Study of Thinking*, Bruner explicitly mentions five effects (or, as he puts it, "achievements") that the act of categorizing has for the human organism: environmental complexity is reduced; objects of the world are identified; the necessity for constant learning is reduced; direction is provided for the organism's "instrumental activity"; and the ordering and relating of classes of events is permitted (see pages 38-39). The descriptions of these effects are given at a very high level of
abstraction, and, thus, they lack definite empirical referents. In fact, they are so abstractly described that Bruner in his discussion of the functions of categorizing is forced to devote most of his space to explicating exactly what he means by his descriptions of the effects. However, presumably in concrete situations it would be possible to formulate sentences descriptive of each effect with explicit empirical referents.

No explicit mention is made by Bruner in his discussion of the functions of categorizing regarding any relevant system of which the act of categorizing might be thought of as a component, although one strongly suspects that the system implicit in his discussion is the entire mind of the human organism. Furthermore, there is no explicit mention by Bruner in his discussion of hypotheses that would relate in law-like ways the process of categorizing and the effective operation of the human mind. This state of affairs stems, no doubt, from that fact that Bruner, in his attempt functionally to explain the act of categorizing, formulated his explanation at a very high level of abstraction, and used in it terminology that is fraught with ambiguity. In any case, even assuming that Bruner's explanation is enthymematic in form, it is not possible for the present writer to envisage precise, law-like hypotheses that would serve the purposes of Bruner's functional explanation.

In sum, then, what we seem to have here in Bruner's
formulation is only hints of the beginnings of a functional explanation of the categorizing process in the human organism. It is apparent, therefore, because of its incomplete and imprecise formulation, that Bruner's explanation is empirically untestable. In other words, it is very difficult to see how any empirical evidence could be brought to bear on the question of whether the explanation is a correct one.

It is worth recalling at this point that substantial amounts of experimental procedures and findings are reported and described by Bruner and his associates in A Study of Thinking. It is also worth recalling, however, that the Brunerians, themselves, list only two principal findings of their total investigation: (1) that it is possible to describe and evaluate concept attainment strategies, and (2) that it is possible to demonstrate the effect of relevant conditions on measurable aspects of concept attainment strategies. At best, these findings have little or no bearing on Bruner's attempted functional explanation of the categorizing process. Nevertheless, they do seem to lend credence to a proposition to the effect that the human organism can, from time to time, be conceived of as engaging in the process of categorizing its experience. However, the attempted functional explanation assumes the existence of the act of categorizing, and tackles the much more difficult problem of determining what systemic effects that phenomenon
has. In any event, perhaps the best thing that could be said about Bruner's attempted functional explanation of the act of categorizing might be summed up in the Scottish dictum: not proven.

The Conditions of Generic Categorizing

Let us turn now to an evaluation of an explanation that Bruner proffers in his early paper, "Going Beyond the Information Given." In this paper, it may be recalled, Bruner is mainly concerned with the conditions under which a person tends to learn or to formulate categories—generic categories—that have a wide applicability in his experience. In other words, Bruner is interested in the classic psychological problem of transfer of training.

Factors of four kinds, Bruner tells us, affect the human organism's acquisition of generic categories or codes: set, need state, degree of mastery, and diversity of training. Set has a number of dimensions, according to Bruner, among the most important of which are the person's life history, the situational instruction the person is given at the time he is confronted with the task of acquiring a generic code, and whether a person is in the frame of mind to want or to intend to learn generically. Need state means the degree to which an individual is motivated to learn in a given confronting situation. Too much motivation or too little, Bruner argues, may tend to
thwart generic learning. Degree of mastery refers to the amount of repetition a person engages in during a particular learning situation. Bruner seems to be not quite sure, generally speaking, as to how much practice is necessary for generic learning, but he contends that an optimum level can be discovered in specific situations. Diversity of training refers to the number of different illustrations of a generic code a person needs to be exposed to in order to learn the code. Thus, for example, a parent teaches his child to apply the word "table" by pointing to several different sorts of tables, while, simultaneously, uttering the word. Again, Bruner seems not quite certain as to how much diversity of training is required in specific situations of generic learning, but he argues that this could be discovered through research. In total, this argument leads one to believe that what we have, here, is an enthymematic formulation of an explanation of generic coding—an explanation that could be reconstructed so as to conform to the covering-law model of explanation. For, the general statement that four sorts of factors affect the occurrence of a particular kind of phenomena has a law-like flavor. In any event, let us proceed on the assumption that an explanation of the covering-law type could be reconstructed.

A major difficulty in reconstructing Bruner's argument so as to fit the covering-law model of explanation
is finding a precise way to state the necessary covering law under the rest of the explanation could be subsumed. Consider Bruner's word: "affect." Just exactly what sort of relationship between generic coding and the properties of the organism and its environment to which Bruner refers (set, need state, degree of mastery, and diversity of training) is asserted by the proposition that the latter affect the former? Is it a causal relationship? Probably not, because Bruner seems to say simply that certain "values" of set, need state, degree of mastery, and diversity of training are conditions necessary but not sufficient to produce generic categorization in the human organism. It seems most likely that Bruner is asserting a relationship of functional dependence (in the mathematical sense of function). Laws of this type were discussed briefly in the last chapter (see pages 107-108). If, in fact, Bruner intends to state a relationship of functional dependence in this case, it at once becomes clear that a much more precise formulation (with accompanying mathematical notation) would be necessary if the empirical warrant of the asserted relationship were ever to be tested. Thus, it appears that, again, what we have here is only the very beginnings of an adequate explanation. These hints, to be sure, are stimulating and suggestive, but they are only hints. In its imprecise formulation, it is not
possible to bring empirical evidence to bear on the validity of Bruner's explanation.

**Early Work Contrasted With Later**

Bruner's attempted explanations of the functions of categorizing in general and of the factors that affect generic categorizing form the central core of his early work on human cognition. Since we have found that both of these explanations are so imprecisely formulated that the ascertaining of their empirical warrant is for practical purposes impossible, one could hope, or perhaps even expect, that Bruner in his later work might make some attempt to refine and to tighten up these explanations and subject them to serious empirical test. But, in fact, Bruner in his later work abandons his notions about categorizing almost entirely, in favor of a new notion: modeling or mode of representation. Exactly why Bruner makes this move is not clear. A careful examination of his accounts of his later work indicates that Bruner, himself, makes no attempt to give detailed reasons in this regard. In any event, it does seem apparent that Bruner's later work is not a systematic attempt to build upon his work done earlier.

The remarks in the preceding paragraph should not be taken as an argument that the content of the later work done by Bruner and his associates is entirely devoid of reference or similarity to Bruner's earlier work. Some of
the later work shows at least a slight influence of the earlier. A case in point is the Mosher-Hornsby Twenty Questions experiment reported in *Studies in Cognitive Growth* (see pages 72-76). The familiar notion of strategy plays a central role in that investigation. Yet, the closely related notions of concept attainment and categorizing seem to have been relegated to a back seat in relation to the notion of modeling in the interpretation of the findings of the study.

The dominant concepts of Bruner's later work are enactive, ikonic, and symbolic representation. These notions are used by Bruner to mean certain kinds of mental activity or mental achievements on the part of the human organism, and their existence is inferred from the organism's overt behavior. They are rather slippery concepts—particularly in view of Bruner's willingness to equate each mode of representation with a model or "modeling activity." For instance, take the equation of the symbolic representation of a radio with a model of a radio. Even on the most sympathetic reading of Bruner, it is nearly impossible to determine in what sense the word "radio" is a model of a radio. However, on the other hand, one could think of the words of a detailed description of a radio as being, in some sense or other, a model of a radio. In any case, the precise meaning of the expression "mode of representation" is difficult to pin down.
"Laws" of Representation

Typical of the research that ostensibly has been generated by the notions of enactive, ikonic, and symbolic representation are the two experiments on the topic of "equivalence," reported by Olver and Hornsby in Studies in Cognitive Growth (see pages 58-60). It is difficult to interpret the findings that the investigators report in these experiments. Beginning with an assumption which is beyond dispute—that people sometimes treat items of their experience as "the same" or "alike"—Olver and Hornsby expect to find that people sometimes group items of their experience together either because they envisage them as playing a common role in some action, or because they have in common some immediately sensible trait, or because they are connoted by the same (or nearly the same) expression. Moreover, Olver and Hornsby propose to explore the relationships, if any, which hold between the growth of the human organism and the ways in which it appears to group the items of its experience. Precisely what the investigators intend by the use of the word "growth" is not clear in the context in which they use it, but they seem to mean the age of the organism. In any event, the most interesting of the findings reported by Olver and Hornsby in their investigations is that, the younger the human organism is, the more likely it is to group items of its experience on the basis
of their immediately sensed traits. Of course, this finding has a law-like flavor. And, if precisely stated, it could serve as a covering law in an explanation, say, of the behavior of a given six-year-old youngster who appeared on one occasion to group together a Kleenex, a flashlight battery, and a coffee cup because they were all blue in color. But the proviso, "if precisely stated," is crucial in this case. In the formulation given to it by Olver and Hornsby, this finding gives only a hint of being the statement of a law. Accordingly, it seems reasonable to say that a first order of business for Olver and Hornsby should be an attempt to refine the statement of this finding so as to make it explicit. Then, an effort should be made to subject the finding to rigorous empirical test. It certainly is conceivable that the finding would not withstand such treatment. Nevertheless, any serious assessment of the validity of this finding must await such a procedure.

At this point, it seems worth remarking that many of the research reports contained in Studies in Cognitive Growth are similar to the Olver-Hornsby report in that they contain in them statements law-like in nature. All of these statements, but some more than others, suffer from the malady of imprecise formulation, and, thus, in their present form, lack the logical qualifications necessary for serious empirical test. It seems to the present writer that it would, indeed, be worth the time and effort required to
attempt the test of as many of these statements as is practicable.

Identity as a Condition of "Conservation"

In Studies in Cognitive Growth, Bruner and Nair attempt an explanation intended as a refutation or, at least, a refinement of Piaget's views. The specific problem with which they are concerned has to do with Piaget's notion of "conservation" in children. On Piaget's view, according to Bruner, the development in a child of the capacity to recognize the invariance of the quantity of some material in the face of changes in its constituent dimensions never precedes the development in the child of two other capacities: reversibility and compensation (see page 64). Reversibility means a child's capacity, in a situation in which he has observed changes in the constituent dimensions of some piece of material, mentally to reverse the process of change that he has witnessed, and, in effect, to imagine the piece of material with the dimensions it had before it was changed. Compensation means a child's capacity to recognize, when he is observing two equal quantities of the same kind of material with different constituent dimensions, that they are, in fact, equal in quantity by mentally changing one of the objects in view to see if it could be made to correspond roughly to the other object. Piaget's statement that "conservation" never precedes the advent of
reversibility and compensation in a child is, of course, the straightforward assertion of a developmental law.

Bruner seems to take issue with Piaget regarding these matters. It is Bruner's view that a conception of identity is a necessary ingredient in a child's representational system, if the capacity to "conserve" is to be achieved. Moreover, Bruner argues that the existence in a child's representational system of a conception of identity is a requirement even for the development of the capacities of reversibility and compensation. And, Bruner's views regarding these matters certainly seem to be intuitively plausible.

As evidence supporting his views vis-a-vis Piaget's, Bruner cites, among other things, a rather elaborate investigation conducted by Nair (see pages 65-66). On one interpretation of Nair's study, it appears that Bruner's views are, in fact, supported. However, on a second interpretation, support of Bruner's views is rendered somewhat uncertain. What generates the second possible interpretation of Nair's study is the question that she asks of her five-year-old subjects: "Is this the same water as the duck had there?" A difficulty is rooted in her use of the word "same." In principle, it is not possible for anyone unfamiliar with the theoretical rationale of Nair's study (let alone a five-year-old subject) to determine, in the context of her question, what she means by "same." Of
course, Nair intends by her use of the word "same" to mean: identical. But "same" is one of those words, of which there are so many in the English language, that are systematically ambiguous. *Webster's Seventh New Collegiate Dictionary* lists under "same" as its first meaning: "resembling in every relevant respect." Moreover, Webster lists a number of synonyms for "same," three of which are important for us to consider: identical, equivalent, and equal.

It seems fairly certain that five-year-olds do not spend much time reading dictionaries; so, no doubt, if they are consciously or unconsciously aware of the ambiguity of the word "same," their knowledge does not stem from such a source. However, it does seem likely that an ordinary five-year-old would have heard the word "same" uttered by adults in a multitude of contexts, and, thus, their awareness of the nuances of the word is quite possible. This creates a problem in interpreting the results of Nair's experiment.

Suppose, for the sake of argument, that a five-year-old subject understood Nair to mean Webster's first meaning of "same" when she asked her question: "Is this the same water as the duck had there?" The question would be understood to mean: Is this water similar in every relevant respect to the water that the duck had there? The water, when in the first tank, toward which Nair pointed upon asking the question, was, of course, in a different visual
configuration in comparison to how it looks in the second tank as our five-year-old observes it. And, under these circumstances, a perfectly legitimate response on the part of our five-year-old would be: "No." If our five-year-old were asked for reasons to support his judgment, he might say: "They are not the same because they look different." On the basis of this remark, presumably Nair would infer that the child was unaware that the water in the second tank was the identical water that was in the first tank. It is conceivable, of course, that Nair would be correct in making such an inference. On the other hand, it is also conceivable that the child, in responding negatively, was perfectly aware of the identity relationship, but that on this occasion he simply was employing a different meaning of the word "same" when he made his response to Nair's question. Accordingly, the delicate shades of meaning connoted by the word "same" confound the interpretation of Nair's findings.

The word "same" also confounds the interpretation of Nair's findings when the utterances of her subjects who, themselves, use the word are considered. For example, Nair reports that 50 percent of the children, who showed the capacity to "conserve" on her pretest and who correctly answer her question as to the amount of water in both tanks, use some variant of "It's the same water" as their main argument for maintaining invariance in amount. Bruner
interprets this fact as an indication that these children have an internalized notion of identity. And, his interpretation may be correct. However, at least some of these statements could be interpreted as meaning that the water in the one tank is similar (or equivalent, or equal) to the water in the other tank. Again, the multiple nuances of the word "same," coupled with the fact that the contexts in which the children use the word in this instance do not make it clear what sense they intend by it, render Bruner's interpretation somewhat uncertain.

Bruner seems to be unaware of the difficulty that attends the use of the word "same" when one attempts to interpret Nair's study. Or, in any event, he gives no attention to the difficulty anywhere in his discussions of the implications of Nair's experiment. Nor is there any indication in Bruner's discussion of the procedural arrangements of Nair's study that Nair, herself, is aware of the difficulty. This is an unfortunate state of affairs. For, the pesky difficulty of the multiple meanings of the word "same" is intertwined with the problem of assessing the validity of many aspects of the work of Bruner and his associates.

Representational Conflict and "Growth"

Perhaps, the most interesting of Bruner's explanations and supporting verification procedures have to do with
what Bruner calls "growth." Bruner seems to characterize "growth" as a person's capacity to keep himself aloof from the immediately sensible features that serve to stimulate him in a given confronting situation, and to hold in mind other features of the situation that are not immediately apparent in it but that are crucial in making proper judgments about it. Bruner hypothesizes that "growth," thus defined, can sometimes be induced by the manipulation of the confronting situation in which a person finds himself in such a way that conflict between modes of representation of the situation manifests itself.

Bruner's hypothesis seems to be an attempt to formulate a causal law that could be used in a covering-law type of explanation of some particular instance of "growth." The word "seems" is underscored because the present writer is not entirely clear as to what Bruner means to assert. It seems plain that Bruner would not wish to assert that representational conflict is a condition sufficient to produce "growth" in the human organism. But, it does seem reasonable to interpret Bruner as saying that representational conflict might be a necessary condition for "growth" to occur. On the latter interpretation, Bruner's hypothesis is just a hint at the beginning of a formulation of a law. In such a formulation, it cannot be subjected to serious empirical test.

It is worth notice that the experimental procedure
from which Bruner's conflict-growth hypothesis appears to spring, Frank's study (see pages 67-70), suffers in some of its aspects from difficulties that confound interpretation of its findings. The purpose of Frank's study, in its initial phases, was, of course, to prompt its young subjects to represent symbolically the fact that a quantity of water remains the same in amount despite changes in its constituent dimensions. Then, in the last phase of the study, the subjects were confronted for the first time with an opportunity ikonically to represent their confronting situation. Bruner interprets the fact that most of the subjects of the experiment (except for the four-year-olds) could "conserve" in a posttest, whereas most of them could not in a pretest, as being induced by the prompting of symbolic representation that took place in the early phases of the experiment. When confronted with the opportunity ikonically to represent their situation, Bruner argues, most of the subjects resist it, or, at least, they disregard the visual presence of the water in making their judgments as to invariance of its amount. Bruner tells us that this is the case at least partially because the final phase of the experimental procedure is such that a blatant conflict takes place between the ikonic and symbolic modes in the representational systems of the subjects. But, the youngsters four years of age who participated in Frank's experiment constitute an exception to all this. Nine of them insist,
in the final phase, that the identical quantities of water which they see in different configurations are different in amounts. This behavior on the part of the four-year-olds casts some doubt on Bruner's interpretation of the responses of the other youngsters who served as subjects for Frank's study. Since the four-year-olds were placed by the first phases of the experiment in situations that ultimately would generate representational conflict in the final phase, it is not clear why they, too, did not "grow." All this suggests, at the very least, that some variable other than conflict between representational systems is involved in producing "growth." Indeed, one could argue, in view of the behavior of the four-year-olds, that representational conflict has no bearing at all on the occurrence of "growth." In any case, Frank's experimental procedures, elaborate as they are, do not provide us with even the slightest hint either as to what other variable (or variables) might be involved in producing "growth," or as to whether, in fact, representational conflict is a necessary condition for "growth" to occur.

**Concluding Summary**

We have now critiqued each of the major explanations of Brunerian cognitive psychology. We have found each of these explanations to be wanting, from an epistemological viewpoint.
Each of the Brunerian explanations (save one) suffers from the major malady of imprecise formulation. So imprecisely formulated are they, in fact, that in each case (save one) it is impossible to see clear relationships between them and the empirical data gathered by the Brunerians in connection with them. Moreover, each explanation (save one) in its present formulation is incapable of serious empirical test. Thus, from an epistemological viewpoint, the major Brunerian explanations (save one) deserve to be regarded as suggestive but rather ambiguous hypotheses in need of careful refinement and empirical verification.

Bruner's explanation regarding the relationships which hold between the human organism's conception of identity and its capacity to "conserve" constitutes an exception to all this. In this case, Bruner's explanation appears to be precisely formulated enough to be capable of serious empirical test. But, certain features of the testing procedures used by the Brunerians to verify this explanation render the results of the testing process somewhat uncertain. Thus, the truth status of this explanation is not clear.
CHAPTER V

SOME PEDAGOGICAL IMPLICATIONS OF THE STUDY

Introductory

Throughout the present study, our fundamental aim has been to decide in a rational way whether or not Brunerian cognitive psychology constitutes a promising guide for classroom teaching and curriculum building in the social studies. We have now reached a point in the investigation where it becomes appropriate to remark briefly on the extent to which this aim has been accomplished.

In this chapter, we first approach the pedagogical implications of this study in a general way. Then, we become more specific via a consideration of Man--A Course of Study, a social studies curriculum project with which Bruner, himself, has been directly associated.

General Pedagogical Considerations

Analytic philosophy has played a major role in the present study. It has provided us with a basis for making a judgment as to whether Brunerian cognitive psychology is true, from an empirical scientific viewpoint. Via the use
of canons for empirical scientific explanation that are articulated in the literature of contemporary analytic philosophy, we have been able to grasp vividly the most serious of the problems of the major Brunerian psychological explanations: imprecise and ambiguous formulation. But, at the same time, the use of these canons has thrown the achievements of the Brunerians into bold relief. Having analyzed their work in detail, it is almost impossible not to be impressed by the suggestive nature of their attempted explanations, and by the scope and difficulty of the problems the Brunerians set for themselves.

Since the philosophical critique here presented has found Brunerian cognitive psychology to be not without merit but to suffer from serious if not scientifically fatal flaws, what, then, is a proper pedagogical stance of a social studies teacher with regard to it? This is a demanding question. Let us approach a general answer to it by way of a rough analogy.

A meteorologist, when he is preparing weather forecasts for a particular locale, on some occasions finds it useful to conceptualize an approaching storm as a frontal wave cyclone. On other occasions, he may find it more fruitful to view an approaching storm, in a simpler fashion, as a large region of intensely unstable air. A weather forecaster can do this because he has at his disposal, in the body of theory that constitutes meteorology, two
distinctively different but related explanations of storms. The situation of the forecaster in relation to the science of meteorology is roughly analogous to the situation of the social studies teacher in relation to the body of explanations that constitute Brunerian cognitive psychology. For, the Brunerians offer two distinctively different but related perspectives with which to view the cognitive nature of the human organism. On the one hand, the Brunerians suggest that it may be useful to view a person as coding his experience into systems of contingently related nonspecific categories. On the other, they assert that sometimes it may be useful to conceive of a person as modeling his environment via enactive, ikonic, and/or symbolic modes of representation. Like the forecaster, the social studies teacher appears to have a choice of intellectual tools.

However, the analogy ultimately breaks down. Consider first that the two meteorological explanations of storms are precisely formulated, and that they both have been subjected to rigorous empirical tests. Consider next that, in contrast, the major explanations of Brunerian psychology are neither precisely formulated, nor have they been adequately tested. In fact, as we have seen, the main reason why the Brunerian explanations have not been adequately tested is because they are so indeterminate in their formulation.
In such a situation, then, what should the social studies teacher do? Should he completely dismiss the psychological work of Bruner and his associates as scientifically untestable—therefore, a hopeless muddle? The answer to the last question probably should be in the negative. An important fact to remember about the work of the Brunerians is that their way of analyzing cognitive phenomena in the human organism is suggestive of explanations which contain scientific laws in them. Needless to say, mankind's stock of scientific laws that can be used in explaining human behavior is very modest, indeed. The potential that the work of the Brunerians has for adding to scientific knowledge of human behavior is something to be excited about. However, the Brunerians need to be encouraged, in a friendly and sympathetic way, to reformulate their cognitive explanations in such a way as to render capable of serious empirical test. In short, social studies teachers should have fundamental reservations about the work of Bruner and his associates, but, in the present darkness, even a dim candle is something no one can afford to pass up.

Consideration of Man--A Course of Study

In his very busy, practical world of the everyday classroom, the social studies teacher is probably not likely to come face to face with the entire body of the Brunerian's work on cognitive psychology. So, the advice of the
preceding paragraph—that the social studies teacher should have fundamental reservations about the psychological explanations of Bruner and his associates—is not likely to have any immediate practical value for him. However, it is quite possible that a social studies teacher might confront what is ostensibly Brunerian cognitive psychology in the context of deciding whether or not to use *Man—A Course of Study*, created by the Educational Development Center, Inc. Thus, in considering the pedagogical implications of the present study, perhaps a few remarks are in order regarding *Man—A Course of Study*.

*Man—A Course of Study* is intended to be a fifth grade social studies course. It consists of more than sixty distinct preplanned lessons—each with an individual "conceptual aim" (an expression use in the course guide) and an associated array of teaching materials. It was created by a massively large number of curriculum builders, teachers, and social science scholars.

The early development of *Man—A Course of Study* was directed by Bruner, himself.¹ And, there is some evidence that Brunerian psychology had at least a salutary impact on the people who created it. For example, Peter B. Dow, in the course of study's introductory materials,

explicitly states that Bruner's ideas on structure of subject matter, which, as we have seen, derive from the early work on cognition of Bruner and his associates (see pp. 50-51), were considerably influential in the development of the course. Moreover, Bruner devoted a full chapter in his book, Toward a Theory of Instruction, to describing the content of the course, although the actual content of Man--A Course of Study as exhibited in the trial teaching edition is somewhat different from Bruner's description. In any event, Brunerian influence seems apparent.

However, one is struck immediately after perusal of sample materials of the trial teaching edition of Man--A Course of Study with the rather tenuous relationships that seem to hold between the course and Brunerian cognitive psychology.\(^2\) If the course was based on Brunerian psychology, at the very least one would expect an explicit statement of the concepts or models to be taught in the course to appear somewhere in the materials of the course. Yet, no such statement is to be found in the sample materials of the course's trial teaching edition. Moreover, it would seem reasonable that, in a course of study based on Brunerian psychology, the individual lesson plan objectives

\(^2\)At least one other writer notes this lack of connection. See Peter H. Martorella, Concept Learning in the Social Studies (Scranton, Pa.: Intext Educational Publishers, 1971), p. 25.
would be stated clearly enough that a reader could tell precisely what they are. But, this is not the case. This last point can be best illustrated, perhaps, by a detailed consideration of a typical lesson plan from the course.

The lesson we will use is part of a series of lessons, presumably devoted to helping students get an answer to the rather ambiguous question: "What is a man?" The lesson plan begins with a statement of the lesson's "conceptual aim."

We can learn much about man by observing his behavior, just as we learn about other animals by observation. But man is a symbolizing creature, and we must inquire into the meaning of his behavior if we are to understand humanness.

The lesson plan calls for the students to view a movie about Netsilik Eskimos, entitled "Fishing at the Stone Weir." The lesson plan advises the teacher to tell the students to watch for certain things in the movie: for example, to notice an Eskimo boy at play. The movie shows the Eskimos going about the normal routines of their daily life: erecting a tent, fishing, eating fish eyes, caring for children, etc. During the course of the movie the Eskimos do a number of things, the reasons for which are not clear to the students: for example, a woman covers the eyes of a fish with ashes. The lesson plan advises the teacher to

encourage the students, after the movie is over, to identify all of the Eskimo actions the reasons for which they do not understand. The plan then advises the teacher to tell the students to read a portion entitled "Ancient Rules of Life" of a booklet provided, called The World We Know, in order to find out the reasons for all the anomalous Eskimo actions. By reading, the students determine, for example, that Eskimos cover the eyes of fishes with ashes to ensure future good fishing.⁴

About this lesson and the many others like it in Man--A Course of Study, perhaps the most significant question that could be raised is "What exactly is its aim?" Is it to teach a concept, as the expression "conceptual aim" in the course guide implies? If so, what is the concept: man, symbol, behavior, meaning, or what? Or, is the aim of the lesson to teach the proposition that a man's behavior can be understood only if one knows his reasons for doing it? Or, is the aim of the lesson entirely different from all this?

The lack of explicit statement in the course materials of the concepts and/or models to be taught in the course, and the lack of clear statements of individual lesson aims in the course lesson plans, both suggest that

⁴Some details of the lesson plan are omitted in the interest of brevity.
the creators of *Man--A Course of Study* were either far from clear as to the nature of Brunerian psychology or chose not to utilize it in the development of the course.

In any event, a social studies teacher considering *Man--A Course of Study* for classroom use should realize that the research undertaken in the present study regarding the truth of Brunerian cognitive psychology will be of little value to him in making a judgment regarding the course's worth. The relationships between Brunerian psychology and *Man--A Course of Study* are not at all clear. *Man--A Course of Study* has a life of its own.

**Concluding Summary**

In this chapter, we have considered the pedagogical worth of Brunerian cognitive psychology and of a course of study ostensibly derivative from it. It has been our conclusion, regarding the major explanations of Brunerian psychology, that they are empirically untestable, and that, consequently, they do not in their present formulation constitute a promising guide either for teaching or curriculum building in the social studies. As regards *Man--A Course of Study*, our conclusion has been that, since the relationships which hold between Brunerian psychology and it are not clear, the findings of the present study have little relevance to questions of its pedagogical worth.
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