Readings for orientation to research and scholarship

Compiled in this form January 2020

This short set of readings is intended to encourage the reader to be critical, to question, to doubt.

I wish had found sources for the following statements, but I did not find them said just the way I present them here:

Questioning is the beginning of wisdom.

Wisdom is knowing what you do not know and what you cannot know.
Inspired by Socrates as recorded in Plato, *Apology* 21d
https://en.wikipedia.org/wiki/I_know_that_I_know_nothing

Sources for the readings

Full text of required sections is in this document

Translation of first **quotation from Goethe's Faust** (with some changes by DS, putting faithful rendering of meaning ahead of rhyme and meter) from

Faust: A tragedy: Interpretive notes, contexts, modern criticism (Norton Critical Editions). 2. ed. by Johann Wolfgang von Goethe.
Walter W. Arndt (Translator), Cyrus Hamlin (Editor)
W.W. Norton; September 2000. 737 p. # ISBN: 0393972828

Second Faust quotation translated by DS.

How Lama Ted diverted a hurricane

from
Davis, Philip 3., 1923-
The thread : a mathematical yarn.
Boston : Birkhaeuser; 1983. 126 p. : ill. ; 21 cm.
ebook $74.99
https://www.springer.com/us/book/9780817630973?wt_mc=ThirdParty.SpringerLink.3.EPR653.About_eBook#otherversion=9781468467246

Diesing, Paul
Patterns of discovery in the social sciences.
Chicago: Aldine • Atherton; 1971. 350 p. Chapter 1. Introduction, p. 1-25
ebook published 2017, introduction in free preview
https://www.taylorfrancis.com/books/9781315126142

Faust. Der Tragödie erster Teil (The Tragedy's First Part)

Nacht.
In einem hochgewölbten, engen gotischen Zimmer. Faust, unruhig auf seinem Sessel am Pulte.

Night.
In a narrow, high-vaulted Gothic chamber, FAUST, restless in his armchair by the desk,

Faust
Habe nun, ach! Philosophie,
Juristerei und Medizin,
Und leider auch Theologie
Durchaus studiert, mit heißem Bemühn.
Da steh ich nun, ich armer Tor!
Und bin so klug als wie zuvor;
Heiße Magister, heiße Doktor gar
Und ziehe schon an die zehen Jahr
Herauf, herab und quer und krumm
Meine Schüler an der Nase herum-
Und sehe, daß wir nichts wissen können!
Das will mir schier das Herz verbrennen.
Zwar bin ich gescheiter als alle die Laffen,
Doktoren, Magister, Schreiber und Pfaffen;
Mich plagen keine Skrupel noch Zweifel,
Fürchte mich weder vor Hölle noch Teufel -
Dafür ist mir auch alle Freud entrissen,
Bilde mir nicht ein, was Rechts zu wissen,
Bilde mir nicht ein, ich könnte was lehren,
Die Menschen zu bessern und zu bekehren.
Auch hab ich weder Gut noch Geld,
Noch Ehr und Herrlichkeit der Welt;
Es möchte kein Hund so länger leben!
Drum hab ich mich der Magie ergeben,
Ob mir durch Geistes Kraft und Mund
Nicht manch Geheimnis würde kund;
Daß ich nicht mehr mit saurem Schweiß
Zu sagen brauche, was ich nicht weiß;
Daß ich erkenne, was die Welt
Im Innersten zusammenhält,
Schau alle Wirkenskraft und Samen,
Und tu nicht mehr in Worten kramen.

Faust
I have pursued, alas, philosophy,
Jurisprudence, and medicine,
And, help me God, theology,
With fervent zeal through thick and thin.
And here, poor fool, I stand once more,
No wiser than I was before.
They call me Magister, Doctor, no less,
And for some ten years, I would guess,
Going up and down, through tos and fros
Have led my pupils by the nose -
And see there is nothing we can know!
It sears my heart to find it so.
True, I know more than those imposters,
Those parsons and scribes, doctors and masters;
No doubt can plague me or conscience cavil,
I stand not in fear of hell or devil-
But then, all delight for me is shattered;
I do not pretend to worthwhile knowledge,
Don't flatter myself I can teach in college
To better people and to reform them.
Nor have I estate or moneyed worth,
Nor honor or splendor of this earth;
No dog would live out such wretched part!
So I resorted to Magic's art,
To see if by spirit's might and word
Many a secret might be revealed;
So I need toil no longer so,
Propounding what I do not know;
So I may learn what keeps the world
Together in its inmost core,
Behold all creative force and seed
And no more peddle in empty words

Part 2, Act 2, Gothisches Zimmmer (High-vaulted Gothic chamber). Mephistopheles

Wer kann was Kluges, wer was Dummes denken,
Das nicht die Vorwelt schon gedacht?

Who something wise, who something dumb can think
That generations past have not already thought?

How Lama Ted diverted a hurricane

Philip Davis, The Thread, p. 89 -95

The man whom I shall call Lama Ted came directly to Providence from Sikkim. One sees him occasionally on the street wearing saffron robes and a triangular saffron cap. He works in the kitchen of a sandwich shop popular with the art students. . . .

But the great story of his first days in Providence, said Sam, was how Lama Ted prevented a hurricane from devastating the East Coast of the United States - from Eastport to Sandy Hook. It came about in this way. Arriving safely in Providence after a long and tiring flight, Lama Ted spent several weeks resting and settling himself in. At the end of this time, which must have been around the beginning of October, he told Sam that he wanted to commemorate his safe trip by sacrificing to the Spirits of the Waters. For this purpose he would need convenient access to the ocean. Sam took him down to India Point Park which borders on the northern tip of Narragansett Bay. Technically, this is ocean with a tide of several feet whose rise and fall is tabulated in the Providence Almanac. There are sea gulls and all that, but Lama Ted decided that the ocean at Providence was not sufficiently extensive and open and that in any case he would need a place such as a bridge where he would be above an expanse of water. Sam told him about the Mount Hope Bridge about twenty miles south of the city. Lama Ted thought this place would be acceptable and told Sam he would go by bus the following week on a day to be determined ritually. His plan was to buy a round trip ticket to Newport, get off at the bridge, perform the ceremony and return.

Three or four days before the set day, the radio began reporting that a storm of hurricane proportions was making its way up the Atlantic Coast from the Caribbean. The storm was being watched carefully and given a name - let's call it Felicia. Its cyclonic wind velocity was determined to be very high indeed and its movement northward was carefully plotted and projected forward. Felicia was expected to pass over Long Island and Southern New England on precisely the afternoon that Lama Ted had selected for his sacrifice.

The storm warnings now extended from Cape Hatteras to Newfoundland. The owners of small vessels tied up in marinas were advised to secure their ships and householders along the south eastern coast of Long Island were alerted for possible evacuation. The morning brought a steady downpour of heavy warm rain and the winds rose steadily in strength. Travellers’ advisories were pronounced. By eleven o’clock in the morning, businesses and institutions in the Rhode Island area allowed their people to go home. Schools and scheduled events were cancelled. Radio Cassandras reminded us to lay in a supply of candles and canned goods and to fill our bathtubs with fresh water. The winds approached 45 with occasional gusting to 65. The barometer fell below twenty-nine inches. The streets were strewn with large branches snapped from trees. Flooding in cellars was widely reported. The keepers of the hurricane gates just below downtown Providence lowered this barrier against a possible tidal wave up the bay such as had inundated the city in 1938 and once again in 1954.

In the middle of this storm and meteorological apprehension, carrying an umbrella which the winds soon made useless, Lama Ted found his way to the central bus station in Providence to catch the 1:00 P.M. bus to Newport. Sam says that at that time the Lama had an English vocabulary of perhaps a dozen words and that he had drilled him in three particular words: "Providence," "Newport," and “Mt. Hope Bridge." The bus was strangely on time, Lama Ted bought his round trip ticket and told an incredulous driver, "Mt, Hope Bridge."

Acquidneck Island, formerly called Rhode Island - this is the original Rhode Island while the mainland portion of the state was known as Providence Plantations - is a triangular island about fifteen miles in length, on which there are three cities: Portsmouth, Middletown, and Newport. The island is reached by three bridges, the great Newport suspension bridge from the west and the Mount Hope and Tiverton Bridges from the north and east. The Mount Hope Bridge joining Bristol on the mainland and Portsmouth on Acquidneck is also a suspension bridge, the second largest in New England, more than a mile in total length with a central segment better than a hundred feet above the passageway joining Mount Hope and Narragansett Bays. The bridge has a toll barrier on the Bristol side and is not customarily open to pedestrians.

The bus made its way along the highway in lakes of water. Believing that Lama Ted could not conceivably have any business at the bridge itself, the driver let him off at the college just short of the bridge. Lama Ted found his way through the warm driving rain to the bridge entrance, past the toll booth and up the span to the highest position. There he performed the ceremony of sacrifice to the Spirits of the Waters. As a part of the ceremony, he cast off onto the waters long paper streamers and flags on which prayers and verses were written in Sikkimese and Tibetan characters.

A car proceeding northward to Bristol spotted him and reported at the toll gate that a nut dressed up in a costume was at the middle of the bridge doing some very strange things. Perhaps it was a case of suicide. The toll gate called up the Bristol police and within a few minutes a patrol car with a siren and a flashing red dome light picked up Lama Ted and brought him down to the Portsmouth side. Here was a strange bird indeed, but obviously no nut. Lama Ted now used his one relevant word over and over: "Providence, Providence." The Bristol police, as a courtesy to strangers - I doubt if they realized he had travelled from the foothills of Kanchenjunga, the second highest mountain in the Himalayas, to Mt. Hope, the second longest bridge in New England - were all set to drive him back to Providence in the storm and indicated as much by grunts and signs. The Lama caught their meaning and showed them the return portion of his bus ticket. The police then kept him in the Portsmouth station and flagged down the next bus back to Providence.

In the meanwhile, the prayer strips cast from the bridge were buffeted by the winds of gale strength but found their way down to the waters and lay upon the lee of Mt. Hope Bay. From there the message of the verses passed through Narragansett Bay to Rhode Island Sound, thence to the open Atlantic, possibly to the Antilles to the south or to Newfoundland to the north where the Spirits of the Waters may then have been residing. The winds veered and receded. The mechanism of the hurricane ground strangely to a halt. By the time Lama Ted was back in Providence, the rain had stopped and the sun was shining.
The next morning the Providence Journal reported that the hurricane had abruptly turned eastward, sooner than predicted, and had blown out to sea with only minor damage. The reason for this perturbation was not given, nor was the reason perceived as clearly as Sam had done.

What is reason? What is belief? [emphasis added]

Years ago, William James gave a public lecture and said, "Many of us in this hall believe in democracy, in liberal Christianity, and in the existence of the atom, all for reasons that are not worthy of the name."

Should he have listed the Yeti and the Spirit of the Waters?

Presumably **William James,** American philosopher

William James was an American philosopher and psychologist, and the first educator to offer a psychology course in the United States. James is considered to be a leading thinker of the late nineteenth century, one of the most influential philosophers of the United States, and the "Father of American psychology". Wikipedia
Born: January 11, 1842, New York, NY
Died: August 26, 1910, Chocorua, NH
Education: Harvard Medical School (1864–1869), MORE
Influenced by: Charles Sanders Peirce, Gustav Fechner, MORE

This might be the hurricane of the story
September 26, 1961 – **Hurricane Esther** moved within 35 miles of the south coast of Rhode Island and Massachusetts as a Category 1 hurricane, before subsequently making a sharp right turn and then making a loop, returning as a tropical storm five days later. Esther remained offshore, but produced hurricane-force wind-gusts from Block Island, Rhode Island, eastward across Cape Cod, Massachusetts, Nantucket, and Martha's Vineyard. There was less damage than in Hurricane Donna one year prior. Wind gusts of 75 mph (121 km/h) to 90 mph (145 km/h) occurred onshore.
https://en.wikipedia.org/wiki/List_of_New_England_hurricanes#20th_century

Two famous quotes on research methods

For each quote, there is a bit of **optional** context and history for those who might be interested.

First quote by *Niels Bohr*

> "Washing dishes works just like language. We have dirty water and dirty dishcloths, and yet we manage to finally get the plates and glasses clean. In language, too, we have to work with unclear concepts and [a form of] logic whose scope is restricted in an unknown way, and yet we use it to bring some clarity into our understanding of nature."

Niels Bohr (1885 - 1962), Danish physicist who made foundational contributions to understanding atomic structure and quantum theory, Nobel Prize 1922.
Bohr was also a philosopher and a promoter of scientific research. Wikipedia

as reported by *Werner Heisenberg* (1901-1976), German physicist, developed the first mathematical representation of quantum mechanics, discovered the uncertainty principle. Nobel Prize 1932.

From
Werner Heisenberg
Der Teil und das Ganze: Gespräche im Umkreis der Atomphysik.
[The part and the whole: Conversations around atomic physics]
Munich, Germany: Piper 1969. 334 p. (quote on p. 190)
English: Arnold J. Pomerans (Translator)
Physics and Beyond: Encounters and Conversations
New York, NY: Harper & Row 1971. 247 p. (quote on p. 137)
0061316229 https://en.wikipedia.org/wiki/Physics_and_Beyond

The book is a memoir with many conversations among famous physicists. The context for the quote is a group of physicists spending time in Heisenberg's mountain cottage.

Quote German. Nach dem Essen ergab sich bei der Verteilung der Pflichten, dass Niels [Bohr] das Geschirr waschen wollte, während ich den Herd sauber machte, andere Holz hackten oder sonst Ordnung schafften. Dass in einer solchen Almküche die hygienischen Anforderungen nicht denen der Stadt entsprechen können, bedarf keiner Erwähnung. Niels kommentierte diesen Sachverhalt, indem er sagte:

> „Mit dem Geschirrwaschen ist es doch genau wie mit der Sprache. Wir haben schmutziges Spülwasser und schmutzige Küchentücher, und doch gelingt es, damit die Teller und Gläser schließlich sauberzumachen. So haben wir in der Sprache unklare Begriffe und eine in ihrem Anwendungsbereich in unbekannter Weise eingeschränkte Logik, und doch gelingt es damit Klarheit in unser Verständnis der Natur zu bringen."

Quote English. After the meal, we established a roster of duties: Niels would wash up, I would clean the stove, the others would chop wood or sweep the hut. It goes without saying that our primitive kitchen would have caused a sanitary inspector’s hair to stand on end. Niels [Bohr] commented on this state of affairs as follows: [See box on top.]

Second quote, ascribed to *Kurt Lewin* as one of his signature quotes, which he repeated often.

> "There is nothing as practical as a good theory" or
>
> "Nothing is so practical as a good theory"

Kurt Lewin (/ləˈviːn/ *lə-VEEN*; 9 September 1890 – 12 February 1947) was a German-American psychologist, known as one of the modern pioneers of social, organizational, and applied psychology in the United States.[2] Exiled from the land of his birth, Lewin made a new life for himself, in which he defined himself and his contributions within three lenses of analysis: applied research, action research, and group communication were his major offerings to the field of communication. https://en.wikipedia.org/wiki/Kurt_Lewin

But this aphorism does not originate with Lewin. It has a long history that demonstrates its resonance and importance. I give a summary of this history from

Arthur G. Bedeian (2016),"
A note on the aphorism "there is nothing as practical as a good theory",
Journal of Management History, Vol. 22 Iss 2 pp. 236 - 242
http://dx.doi.org/10.1108/JMH-01-2016-0004.

Earliest mention found is in an 1873 book by German educator Friedrich W. Dörpfeld
Grundlinien einer Theorie des Lehrplans: zunächst der Volks- and Mittelschule
(An Outline of a Theory of the Curriculum: Initially for Primary and Middle Schools).
The following "motto", appears on its title page:

"Eine **richtige** Theorie ist das Praktischste, was es gibt."
"A **correct** theory is the most practical thing there is".

In an advertisement for his book, Dörpfeld used a variation

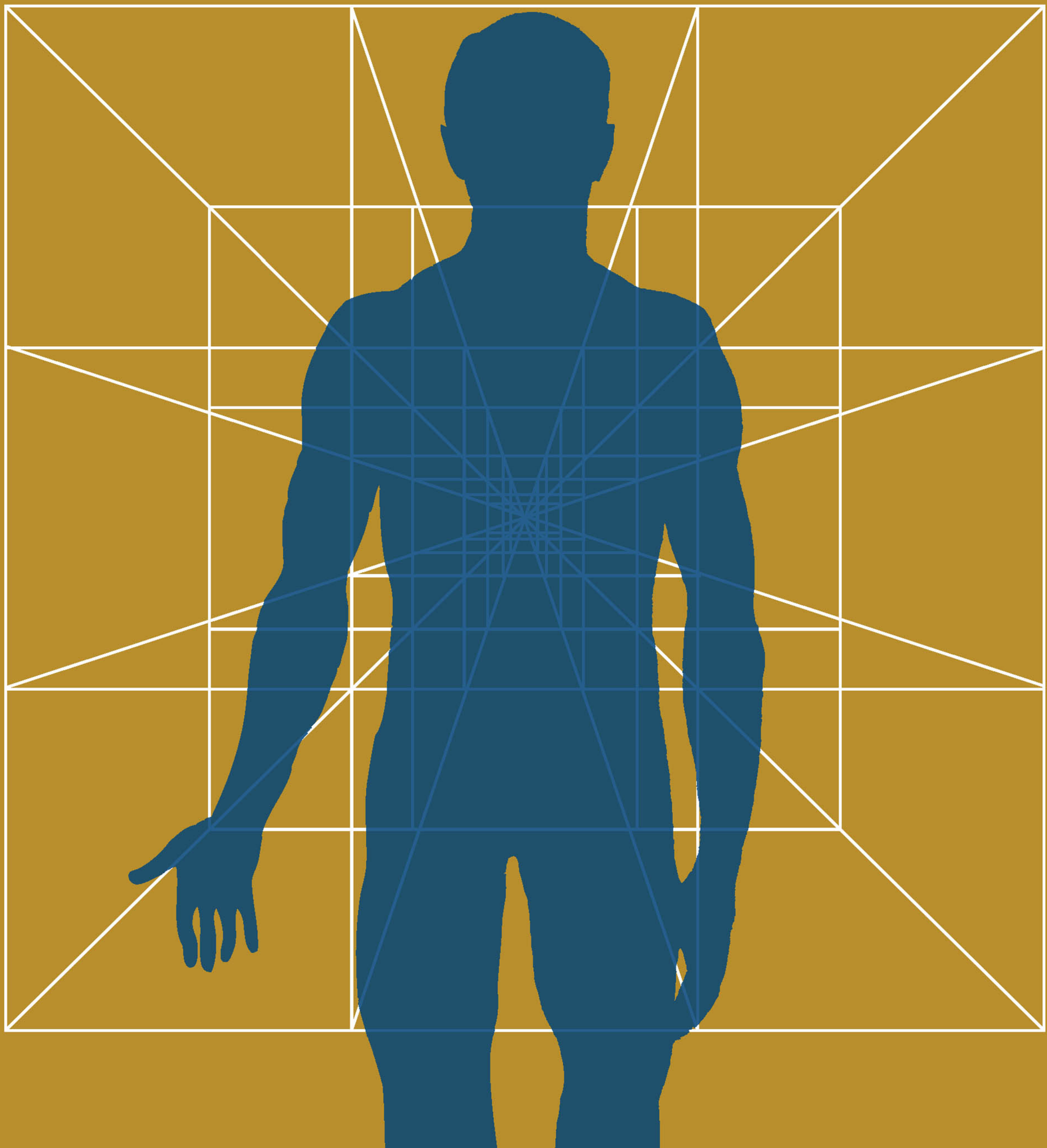
"Es gibt nichts Praktischeres als eine **gute** Theorie"
"There is nothing more practical than a **good** theory".

This version was highly quoted in Germany. The American psychologist and educator G. Stanley Hall often used "Nothing is so practical as a good theory" starting in 1882. It was repeated by many.

The form "Yet nothing is so practical as a good theory" appeared in an advertisement extolling their research placed by the General Electric Company in many newspapers in 1920.

PATTERNS of DISCOVERY in the SOCIAL SCIENCES

PAUL DIESING



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PAUL DIESING

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In memory of

ROBERT REDFIELD

Acknowledgments

My primary indebtedness is to the many social scientists who let me hang around and listen to them, showed me their experimental apparatus, invited me to their private discussion groups and endured my sometimes disruptive questions, listened and commented on my presentations in various seminars, colloquia, and discussion groups, tried to answer my questions, and read and commented on some portion of this work, Dean Pruitt and Morris Zelditch especially. I am also grateful to Richard McKeon for teaching me that there are several different modes of knowing.

I also wish to thank Sue Pidgeon and Lucille Peterson for their diligent typing and retyping of various drafts of the manuscript.

The following work was written over a period of time and unfortunately could not take account of the most recent developments in the areas investigated. The chapters on mathematical modeling were substantially completed in 1966, those on computer simulation in May 1967, and the rest in June 1969.

The publisher, Alexander J. Morin, is mainly responsible for the inclusion of the last chapter. I had earlier deleted it as superficial and sketchy, but he insisted I put it back in, perhaps because he wanted a happy ending.

Contents

1

Introduction

Books on social science methodology mostly fall into one of two classes. First, there are "methods" books, works which introduce the student to research techniques in some specialized area of the social sciences. Each field has its own methods: there are methods in social research, methods in cultural anthropology, research methods in human relations, and so on. These are "how-to-do-it" books. The student is taught how to write questionnaries, conduct interviews, calculate chi-squares, administer tests, write computer programs, and do whatever else is required in his special field of interest. Such books must be revised frequently, because new techniques are constantly appearing and old ones being modified. Then there are "method" or "scientific method" books. These are more abstract discussions of science in general, referring to specific fields only to illustrate what is true of all science at all times.

My approach is midway between these two. The methods I investigate are not the hundreds of particular scaling, testing, interviewing, and statistical techniques, nor the timeless logic of science in general, but rather the four or five different methods or modes of procedure, incorporating particular techniques as parts, that social scientists use today. By "method" or "mode of procedure" I mean the whole series of steps that a scientist or research team follows in the process of making a contribution to a field of knowledge. Not everything a scientist does is part of his method—teaching seminars in the subject, applying for research grants, politicking to get his theories accepted—but only those things that are an essential part of the achievement of knowledge. I call these methods "patterns of discovery," using the terminology of the late Norwood Hanson (1958), because I am dealing with the whole process of inquiry, the whole process of "discovering" or creating or developing knowledge, and not just the verification aspect.

To discuss "methods" rather than method does not imply that there is no one basic method of science. However, a premature interest in this one method forces one's attention to move to so abstract a level that much of what scientists do must be ignored as technical detail. Consequently, one's account tends to become thin and abstract, and attention shifts to philosophical puzzles of little interest to scientists; or, in the attempt to achieve richness of detail, one tends mistakenly to identify a particular method, say, the method of nuclear physicists or experimental psychologists, with the general method of science. General scientific method is best discussed only after one has begun to appreciate the variety that exists in methods now in use.

Types of Methods

If one's attention is directed to differences among methods, the most obvious difference is that between the clinical and the experimental method. This difference has often been noticed and has been accounted for in a great variety of ways. If one wishes to reduce differences to a minimum, one can say that there are only these two basic methods, the clinical and the experimental. However, with a bit of attention, one notices that survey methods are distinguishable from experimentation and that there are also variants of the clinical method, notably participant observation. One also finds that formal methods have characteristics that distinguish them from both clinical and experimental approaches, and that there are in turn several formal methods.

One could go on and make further distinctions, but let us provisionally stop here and say there are at present four main types of methods in use: experimentation, statistical survey research, participant-observer and clinical methods, and formal methods. For the time being, computer simulation can still be treated as a formal method, though perhaps in a few more years it may be more appropriately regarded as a fifth and distinct method.

Participant-observer and clinical methods can also be distinguished, but it is more convenient to group them together, to keep the list down to four. Such a list is not intended as a definitive classification of existing methods, but only as a set of initial distinctions useful for exploring the field. As one continues his investigations, it may become necessary to make further distinctions of varying degrees of sharpness and to notice continuities or overlapping between methods initially distinguished from each other.

Let us glance at each of these methods briefly to note their main characteristics.

The experimental method has been most fully developed among the social sciences in psychology and in social psychology. It has variables as its subject matter, that is, any natural occurrences that exhibit measurable variations in incidence, or rate of occurrence, or rate of change of occurrence. Its principal objectives are to discover variables that behave in a lawlike fashion and to discover the laws governing their variation. Presumably everything in nature changes somehow, but the experimentalist tries to find regular changes that can be described in relatively simple and precise terms. Originally variables were studied in pairs, but later Fisher's statistical work (1935) enabled experimenters to deal with three or more variables simultaneously. When a pair of variables is being studied, one is ordinarily treated as an independent variable ("cause") and the other as a dependent variable ("effect"). The correlations that may be found between the two serve as a first approximation or ingredient of some prospective law. More complex correlations and partial correlations among three or more variables point to more complex laws.

The experimental procedure, in outline, is to locate a potentially lawlike variable by examining previous experimental results and trying to find masking effects that disguised or covered over some hidden correlation. Theory is useful for suggesting possible masking effects and possible hidden correlations. It is also possible to examine a case study or even one's own experience with the help of theory, to locate a possible lawlike variable, but this approach is more difficult and less likely to succeed because of the chaotic appearance of ordinary experience. The searching of ordinary experience is likely to be a haphazard, hit-or-miss affair, while the searching of experimental results can be more systematic because of the regularity of the data.

Next, one imagines an experimental situation in which the masking effects are removed or controlled so that the hidden correlation can be plainly observed. Control can be achieved in a variety of ways, including holding the masking factors constant, eliminating them entirely, limiting their range of variation, counteracting them, and subtracting their presumed effects statistically from the results. Once the controls are set up, the next steps are to introduce the independent variable and then measure the change in the dependent variable. The results are then compared with previous experimental results to see whether one has moved closer to the presumed hidden correlation. If one has moved closer, one continues the search in the same direction; if not, one starts looking in a different direction. Tests of significance are used to determine whether it is worthwhile to continue the search in the same direction or advisable to try something different. Significance criteria are set at a level such that not too many promising leads are discarded prematurely and not too many blind alleys are preserved; however, such tests are advisory only.

It is also possible to begin one's work by examining plausible speculations on a subject, then operationalizing some of the key concepts and devising ground-breaking experiments. Such initial experiments cannot be expected to produce immediate success; they serve only to start the long search for hidden variables and correlations.

As the investigator gradually refines his variables and strengthens his correlations, he also tries to determine the limits of their validity. Do they hold only for college sophomores? For men only, or for women, too? For Japanese? Navahos? Frequently some speculation or theory can be used to suggest a class of subjects for whom the correlation might not hold, or for whom it holds very strongly. Such investigations not only uncover limits but also put one on the track of more general laws of which the original correlation was an instance.

Checking can occur throughout the search process. It is possible at any stage to repeat the experiment in a different place with a different experimenter and different instances of the variable to see whether the same results occur. However, most published instances of what are called "replication" are actually part of the search process, since small changes are made in the experimental setup in hopes of getting a slightly better correlation or of uncovering new limits on the original correlation. True replications, changing only experimenter, place, and specific subjects, are usually left to students, and their frequent failures to get the same results are explained as being due to inexperience.

Once the initial objective, a general law, is achieved and checked, attention shifts to the discovery of new laws. These may be supplementary, in that they limit the range of validity or applicability of the original law, or they may state the effects of the original dependent variable on other variables. The eventual result envisioned is a kind of network of linked variables, extending endlessly in all directions.

In the experimental method, definitions are always at least partly operational. Definitions of independent variables include a statement of the operations by which they are introduced and controlled, and definitions of dependent variables include a statement of the operations and measurements by which their presence can be determined. The reason is that the experimental discovery of laws depends on actual operations with the variables involved, which is possible only if the variables are reduced to operational terms. Similarly, replication is possible only if the original operations have been specified. It is not necessary to have a completely operational definition; in many cases it is thought that a single concept, for instance "group cohesion," can have several different operational definitions, all sharing a vague common core of meaning. However, each new operational definition produces some shift of meaning, perhaps a large shift. Consequently, widespread use of the experimental method tends to produce a proliferation of variables and laws, many vaguely overlapping, rather than the single clear network of laws originally anticipated. When attempts are made to collect and systematize large numbers of empirical laws, as in March and Simon's *Organizations* (1958), the results are suggestive rather than precise because of the shifting meanings of the central variables.

This difficulty in producing truly general laws is one of the chief problems in the experimental method in the social sciences, along with such problems of controlling variables as experimenter bias. Experimentation is effective in producing five-page reports in psychology journals, but these reports are consolidated only very gradually into a system of general laws. Consequently, scientists interested in developing general theory in a hurry sometimes shift to other methods, particularly the formal method, which are better adapted to the problems of general theory.

The survey method was devised to overcome another problem of the experimental method: the difficulty of dealing experimentally with large and complex subject matter. Experimentation always involves a considerable abstraction from natural complexity, and scientists who wanted to study complex sets of variables in their natural setting devised the survey method for this purpose. However, survey research has developed well beyond this original purpose and become a method in its own right, one that has been combined with and enriched other methods and has also produced its own kind of theory.

The experimental difficulty of dealing with large and complex subjects is met in the survey method by sampling and by substituting statistical controls for experimental ones. Similarly, correction and validation involve primarily the statistical manipulation of data. With the continuing development of statistical techniques it has become possible to devise quite complex research designs, involving many variables in a variety of relationships and yielding complex correlations. Thus the austere limits of the classical experimental method are transcended, and the complexity of actual societies can be more adequately handled.

Another advantage of the survey method is that it combines readily with all other methods. Experimentation has been enriched by statistical controls, for instance by using sampling techniques to select experimental subjects. Participant observers have used sample surveys to extend the range of their observations, while survey researchers have used a variety of clinical and quasi-clinical techniques, such as focused and unfocused interviews, various degrees of participant observation, and projective devices, to enrich their data. The variety of combinations in use is so great that survey research and participant observation can now be seen as two ends of a continuum rather than as two distinct kinds of methods. Formal methods have also used survey research data to provide interpretations and probable values of formal variables and to suggest new variables and relationships.

The participant-observer method was first developed by anthropologists, though it is also frequently used by sociologists, social psychologists, political scientists, and organization theorists. Its primary subject matter is a single, self-maintaining social system. The system may be a small community with its own culture, or a larger society with its culture, or a small and relatively isolated neighborhood, or a gang, clique, voluntary organization, or family, or a formal organization or institution, or a person (clinical method), or a historical period. In each case the emphasis is on the individuality or uniqueness of the system, its wholeness or boundedness, and the ways it maintains its individuality. The primary objective is to describe the individual in its individuality, as a system of rules, goals, values, techniques, defense or boundary-maintaining mechanisms, exchange or boundary-crossing mechanisms, socialization procedures, and decision procedures. In one important variant, the primary interest is in recurring processes within or around such individual systems.

The procedure is, first, to become socialized into the system, to learn a set of roles and normative elements, to form relationships, and thus to participate in the normal routines and occasional crises of the system. If the system is small, the researcher can gradually turn himself into an analogue of the system, so that he reacts as it reacts, feels as it feels, thinks and evaluates as it does. The next step is to make this implicit knowledge (Polanyi's "personal knowledge," *verstehen* in a sense) explicit. The researcher constructs hypotheses about parts of the system out of the recurrent themes that come to his attention and tests these hypotheses against a variety of data—what he sees, what others tell him, how he reacts, and how others react to his probing actions. Many detailed hypotheses are gradually combined into a model of the whole system, whose parts are tested by how well they fit together and how well they agree with the data.

The system model is continually checked against new data and revised. Since the researcher is part of the system he studies, new data are continually coming in and the model is never quite completed. Other researchers contribute further checks by providing their own models of the system, which are compared with one another for coherence as well as with the various sets of data.

All through this process the researcher is continually comparing his case with others familiar to him, looking for similarities and differences, and using one case to suggest things to look for in another. One eventual result of such a process of comparison is a typology, a classification of cases according to similarities and differences. Further study of a type should lead to hypotheses about which of its characteristics are particularly important in determining the rest and what are the dynamics of the type. Comparison of widely differing types enables one to search for still more general characteristics of many kinds of human systems—universal or nearly universal values, institutions, system problems, mechanisms, and the like. General theorizing of this sort tries to transcend the relativity inherent in the participant-observer method by looking for general characteristics of human systems, though it still recognizes that these characteristics vary considerably in detail.

At least three other methods similar to participant observation can be distinguished. First, the clinical method used in clinical psychology and psychiatry is basically the same in that it deals with a whole, unique, self-maintaining system—in this case a person—and aims at construction of a system model; it involves the intimate participation of the therapist in the functioning of his subject matter, so as to develop an intuitive understanding of it; it involves the development of specific hypotheses out of recurring themes and the testing of them against several kinds of data, including the clinician's own reactions and the responses to his probing actions; and it involves the continuous reconstruction of the system model in terms of internal coherence and of agreement with the continuing supply of data. It falls short of participant observation at its best in that the clinician cannot, in principle, get as complete an inside understanding of his subject as can a group of field workers. If the personality is, in part, a system of roles and role expectations, the clinician participates in it by taking one or two roles that are offered him. He can then participate in and observe the activity of his subject in those roles. But the subject's activity in other roles, as husband, father, employee, and the like, is not accessible to direct observation and must be reconstructed intuitively from the subject's reports. This makes for an incompleteness of observation that is not necessarily the case for field studies. A partial solution to the clinician's problem is to study a whole family, but this approach is likely to sacrifice some of the depth of knowledge that can be achieved by concentrating on a single subject or part of one.

Another similar method is used by some historians when they attempt to reconstruct a whole historical period out of available data and try to understand it, intuitively or "from the inside," as a kind of integrated system with its own unique character or spirit. This method falls far short of the clinical method in that the historian cannot participate in his subject matter at all but must experience it vicariously and imaginatively. Nor is a historical period actually a self-maintaining system with actual boundaries; even if it were, it would be much too large to reconstruct in all its inner workings.

Still another similar method is occasionally proposed by some institutional economists. In it the self-maintaining system to be studied is the total set of institutions in which a particular economy functions, seen in historical perspective. I have not succeeded in understanding this method adequately since it seems to have remained a proposal rather than an actuality for over a half century. However, it would seem to involve all the difficulties of the historical method and more, owing to the size and complexity of its subject matter. Just as the participant-observer method has been most successful in studies of simple nonliterate societies or small formal organizations, so the most successful institutionalist studies have been of small primitive economies (such as Polanyi, 1957). Attempts to study the U.S. or world economy have necessarily involved great reliance on statistics and thus have moved toward the survey research method, which is much better suited to a large subject matter. My impression is that there is no one institutionalist method predominant at the present time; some people who call themselves institutionalists use statistical surveys, some use elaborate econometric models, some use participant observation supplemented by numerous statistics, and some use historical reconstruction. Conversely, if a unified institutionalist method is ever fully developed, it will probably be some amalgam of clinical-historical, survey research, and even formal methods.

Formal methods have long been in use in economics, and in recent years have become important in a number of fields, including psychology, sociology, international politics, and some newer interdisciplinary fields. These methods are in particularly rapid development right now, so it is difficult to give an adequate description that will not soon be outdated. Nor is it easy to summarize all the varied and sometimes contradictory methodological devices that are being tried out.

The subject matter of a formal method is a formal system of logical relationships abstracted from all the varied empirical content it might have in the real world. For example, the classical economic theory of the firm dealt with the structure involved in any process of production using any materials at any set of relative prices with any technology. It is supposed that this formal structure is present in the real world in some way or to some extent, and that there it determines the course of events. The initial objective of the formal method is to construct a model of a system or process that can be exemplified empirically.

The first step in the procedure is to set up a first approximation or baseline model by laying down a minimum set of postulates and definitions. These may be derived from some empirical theory by abstracting from its empirical content and thus laying bare its implicit logical structure, as Simon did with Homans' theory (Simon, 1957, ch. 6). More frequently, they are derived by dividing an empirical process into its obvious parts and stating the necessary relations (or in some cases, all possible relations) between those parts.

A minimum set of postulates is one that is sufficient to generate roughly the kind of dynamics the scientist wishes to study. The next step is to deduce, either logically or mathematically or by computer simulation, the inherent dynamics of the system, that is, the set of changes that is determined by the system's internal structure, apart from external influences and apart from any empirical content such as particular values of the system's variables or parameters. (A variable here is some quantitative characteristic of the system that can change, and a parameter is some characteristic of the environment that is given for the system.) Some systems, such as neoclassical price theory, are equilibrium systems; that is, their inherent dynamics lead toward a steady state. In this case the factors that produce and maintain equilibrium can be determined, together with the way the equilibrium value depends on the value of each factor. Other systems fluctuate around an equilibrium point or line, as, for instance, business cycle models. In this case the shape and range of the fluctuations and the location of the equilibrium point can be deduced, together with the dependence of each on the system variables. Other systems, such as those of stochastic learning theory, approach a limit whose value is determinately related to structure and to initial values. Still others, such as population models, go off to infinity if left to themselves, at a determinate rate; and some simply fluctuate indeterminately. Many systems are compound; that is, they have multiple possible outcomes, depending on the initial values of certain variables. For example, in Simon's Berlitz model there is an indifference line; any state of the system lying above the line moves to infinity (the person learns the language), any state below it moves to zero (he stops studying). In some economic growth models any ratio of capital formation to population increase above a crucial rate leads to self-sustained growth (infinity), any lesser rate to stagnation (equilibrium), any rate less than a given minimum to bankruptcy (zero). In all these cases the formal theorist can deduce the way in which the system's logical structure determines its dynamics.

The next step is to interpret the model. Interpretation consists of providing a set of rules of correspondence that relate formal terms of the theory to empirical concepts; in this way the theory gets content and is related to the empirical world. Each formal theory may have a variety of empirical theories corresponding to it; the crucial requirement is that all the empirical theories have the same logical structure as the formal theory they interpret. If the initial definitions of the formal theory are derived from some empirical theory, the latter provides a ready-made interpretation, but even here other interpretations should be discoverable. In addition, the formal theorist may provide "heuristic interpretations" as he goes along, to help the empirically minded reader to think through the theory.

Once interpretations are available it is possible to criticize and correct the initial model. This proceeds by what is called the "method of successive approximations." Correction can begin at either of two places, the initial definitions and postulates or the derived system dynamics, and formal theorists have disagreed on which is the more appropriate (cf. MacEsich, 1961). If one corrects through system dynamics, one compares the path or outcome of the system with empirical paths and outcomes and notes the divergence. Then one modifies some postulate or definition, or adds a new variable, in such a way as to shift the system closer to the empirically observed paths. When the two paths are roughly similar (they cannot be identical because random factors always distort the empirical path away from its logical course) the theorist can assert in some fashion (depending on the theory of truth he believes in) that he has now discovered the logical structure in the world that produces the empirically observed paths or outcomes. If one corrects through initial postulates and definitions, one notes the divergence between the variables and relationships postulated in the formal system and those known to exist in empirical reality. Then, one by one, the missing variables are added and their effects on the system dynamics worked out. When the two sets of variables and relationships roughly correspond (again, they cannot be identical) the theorist can assert that the set of relationships present in that part of the world will of itself tend to produce the kind of dynamics expressed in the formal theory.

Formal methods do not normally produce laws relating pairs of variables; they produce models. However, parts of a model or deductions from a model can be selected and restated in the form of a lawlike sentence. Such "formal laws" are not to be confused with empirical generalizations, which describe factual, observable regularities, nor are they like functionalist laws, which state empirical compatibilities and incompatibilities for some type of empirical system. Rather, formal laws are a priori statements of necessary connections between abstract entities. The "iron laws" of economics are examples of such a priori necessities. These laws need not be exemplified in any particular instance because of empirical interferences and accidents, and they cannot be empirically falsified, as I shall argue in chapters 2-4.

This account is not intended as a definitive description and classification of social science methods; it is only a preliminary statement of some obvious differences among methods. It is intended to serve as an initial orientation, a set of guideposts that will enable the reader to plot his approximate position as he wanders deeper into the thicket of actual practice. I am *not* claiming that there are exactly four sharply distinct methods, rather than three, six, or eight; rather, I am picking out four prominent locations in the terrain and contrasting them with one another. Each of the locations can serve not only as a guidepost but also as a point of departure for exploring the whole field of social science methods, and the field will look different whenever one begins from a different point of departure.

Therefore, some of the broad generalizations made above will be qualified or even discarded as we go into more detail. Other generalizations will hold from some standpoints but not from others. One example will illustrate. As we study formal methods more carefully, we find that mathematical modelers also frequently undertake experiments, and when they do, their experiments differ in a number of important ways from the kind of experimentation I have summarized as "experimental method." I shall describe these differences in detail in chapter 4. These differences did not always exist; when mathematical modelers began experimenting about twenty years ago, they used the experimental techniques then current and only gradually made the modifications they found necessary for their purposes. Experimental methods in other hands were developing in a rather different direction or directions, so that by about 1965 one could say that two distinct kinds of experimentation were going on. I shall later call these two "formalist experiments" and "empiricist experiments." The description of experimental method above applies to empiricist experiments, apart from some recent developments, but not to formalist experiments.

When we study the distinction between formalist and empiricist experiments, we find that it sometimes wobbles and starts to disappear. When I describe the distinction to a formalist, he understands and agrees, but when I try it on an empiricist, he is puzzled and starts to argue. The distinction seems perverse and pointless or even unintelligible to him. All experiments are basically the same, he will say; the only distinction worth making is between good and bad experiments, and in the latter class belong a number of unfortunate attempts by people who are better at mathematics than they are at science. These attempts, he says, are characterized by crude, unimaginative experimental design, an insensitive and overly rigid experimenter, and utterly routine mechanical treatment of data. When I listen to such an argument, I am at first persuaded that the distinction I thought I saw was an illusion; but then I gradually notice the category of "formalist experiment" appearing in the argument in a distorted fashion. What shall we say, then? Are there two kinds of experiment or one? It seems to me we should say that from a formalist standpoint there are two kinds, while from the standpoint of an empiricist experimenter there is only one. This conclusion, of course, is subject to modification as I talk to more experimenters of various kinds.

The various social science methods—let us assume as a first approximation that there are about four—have developed to their present state gradually over the past fifty years and are still developing, some rapidly and some slowly. It may be that some of them are also instances of basic, timeless modes of human knowledge (Sacksteder, 1963b), but I shall not consider this possibility. As historical developments, they are all imperfect, incomplete, just as scientific theories are always developing and incomplete. On the other hand, they do not have any inherent, a priori shortcomings or limits that may not eventually be overcome. In their present state they represent solutions to past problems of method and contain tensions and difficulties that will induce future development.

The present differences among the methods described are both factual and normative. A clinician and an experimenter, or a formalist and a survey researcher, follow different procedures, evaluate their developing work by different standards, and aim at different goals. An adequate account of these methods should cover all three aspects—procedures, goals and standards; it should show how procedures and goals are related, describe the characteristic problems and typical solutions that arise out of the procedures, and discuss the criteria for solution that the problems require.

The boundaries between the methods cut across the traditional social science fields. Clinical or case study methods occur not only in psychology but also in anthropology, history, sociology, and political science. Statistical surveys are carried out by psychologists and political scientists as well as by sociologists, and formal methods appear in all the social science fields, now even in anthropology. Moreover, communication and co-operation occur primarily within the boundaries of a method, not within a field. Thus, clinical psychologists and anthropologists have co-operated closely for thirty years now, but clinical and experimental psychologists in the main maintain a cold reserve. Economists formerly were relatively isolated, but with the spread of formal methods to other fields have come to co-operate increasingly with other formalists. Formal and institutional economists have little polite to say to each other, but some institutionalists can work with anthropologists and sociologists who deal in problems of social institutions and cultures.

Differences of method are not only barriers to communication and co-operation, but frequently sources of outright hostility and disdain. Experimenters frequently regard clinicians as frauds and quacks, certainly not as scientists, and dismiss psychoanalytic theory as fiction; clinicians sometimes hold equally uncomplimentary attitudes toward the trivialities of experimental theory and the degrading manipulations of experimental method. Both, however, can agree in regarding formalists as prescientific spinners of abstractions; their models are called "toys" (Homans, 1961, pp. 164, 190, 226, 329), useless at best (Martindale, 1959, pp. 88-89), and usually misleading (Pollis and Koslin, 1962); and their mathematical constructions are regarded as deliberate attempts to disguise the triviality and even the falsity of their empirical assumptions.

Not all contenders are equal in this contest of mutual disdain; the experimentalists and survey researchers are dominant, perhaps becuase there are more of them or perhaps because it is easier to argue that all science is essentially experimental. The dominant view goes something like this: Science is the experimental (or experimental-statistical) search for general laws that relate two or more variables. Experimentation is defined with varying degrees of strictness; to the pure experimentalist even statistical work is suspect, while to the survey researcher, laboratory experimentation is too artificial and too limited to be very useful. Clinical work is not science; it is either a kind of history (case history), or a prescientific exploration for appropriate variables with which to experiment, or downright fraud. Formal work is mostly a misguided and premature aping of the "more advanced" sciences. Physicists or chemists (according to this viewpoint) can properly construct mathematical theories because so many empirical general laws have already been discovered in these fields, and their mathematics merely relates or summarizes the laws in a theory, but in social science very few laws (if any) have as yet been thoroughly verified, so there is nothing to summarize. Consequently, if one dissects one of those mathematical monsters that formalists are constructing, one finds its genuine empirical content to be either trivial or false. The only proper use for nonstatistical mathematics at present is in the deduction of hypotheses from other hypotheses for experimental verification.

This view, or something like it, is so pervasive that even some clinicians and formalists adopt it. Periodically one reads declarations by clinicians that psychoanalysis ought to become "scientific," and there are even a few misguided attempts to make it so—misguided because "science" is defined according to the experimental ideal rather than in a way appropriate to clinical experience. An occasional formalist or clinician will confess privately, "I'm not really a scientist at all, you know" (cf. Gladwin and Sarason, 1953, p. 438).

I think this view is false. It thoroughly distorts both clinical and formal methods and may even be misleading in its interpretation of experimental work. More generally, I think the widespread attitude that there is only one scientific method, usually one's own, is unfortunate. It produces a distorted view of what other scientists are doing, and as a result blocks much potentially fruitful co-operation on new methods and new theories. My main purpose in this book is to argue against a single-method ethnocentrism and to argue that each method is valid in its own way and has its own advantages and disadvantages. Insofar as one form of ethnocentrism is dominant, I wish to argue against that form specifically and defend the other methods against it. I wish to argue that social science is not at present, and ought not to be concerned solely with the experimental-statistical verification of hypotheses and the discovery of general laws.

My procedure will be to describe formal and case study methods—both participant-observer and clinical—in detail, exhibiting them as methods of discovery different from, but analogous to, experimental method. I shall show that their strengths and weaknesses do not spring from the closeness of their resemblance to experimentation but are an integral part of their own unique approaches to knowledge. I shall not describe experimentation and survey research in similar detail because these methods have already been thoroughly studied and described by methodologists. However, I shall from time to time summarize various aspects of these methods to contrast them with corresponding aspects of formal and case study methods.

My relative neglect of experimentation and survey research is not intended to disparage these methods, for which I have a high regard, bur rather to correct the unduly low regard that some social scientists have for formal and clinical and field methods. Nor do I think that further detailed study of experimentation and survey research is unnecessary; there have been some interesting recent developments in experimental methodology that make previous accounts partly obsolete. That task, however, I leave to others.

My neglect of the social context of current social science is not based on a belief that society has no impact on science, but only on the need to keep my subject within manageable limits. A careful study of the social context of American social science would undoubtedly lead to reinterpretations of the methodological developments I shall describe, and I hope such studies can build on mine.

The Logic of Discovery

My earlier statement that this book deals with patterns of discovery requires clarification. Philosophers of science have disputed the question whether there is a logic of discovery, and the present work is in part a contribution to that dispute. The dispute turns in part, but only in part, on the meaning assigned to the term "logic."

Some philosophers, defining "logic" as "deduction," have argued that there can be no logic of discovery, since if we could deduce new knowledge from old it would not really be new. They have further argued that there is no order or method in the process of discovery at all, that "the creative side of science *is* wild and undisciplined" (Jarvie, 1964, p. 49).

In this view science is divided into two quite distinct parts, discovery and justification, neither contributing anything to the other. Discovery proceeds according to no rules or regularities of any sort, so all the methodologist can do about it is to tell anecdotes, myths about such things as serpents and benzene rings, to illustrate the proposition that new hypotheses can pop up in the oddest ways. Justification, in contrast, is a regular process involving rules of evidence, rules of inference, and rules of confirmation, so this is the domain of logic and method.

Other philosophers, defining logic more broadly, have argued that there is a logic of discovery. Norwood Hanson (1958, 1963) has argued for this position in physics, while Abraham Kaplan has argued for it in the social sciences (1964, pp. 12-18). Kaplan, following John Dewey's lead, defines logic as the procedures scientists use when they are doing well as scientists (p. 8). The task of the methodologist is to reconstruct, that is describe and clarify, the logic or logics that scientists are using. The question of whether there is a logic of discovery thus becomes empirical; one answers it affirmatively by describing, reconstructing, one or more such logics, and one answers it negatively by disconfirming a proposed reconstruction.

The present work follows Kaplan's lead by attempting to describe or "reconstruct" several logics that social scientists are now using. I have called these logics "patterns of discovery," following Hanson, to indicate the tradition in which I am working. However, this phrase may be vague or misleading for readers not familiar with Hanson's or Kaplan's work, so I shall specify it a bit.

The term *discovery* is misleading inasmuch as it suggests that scientists are limited to finding something that is already there. The suggestion is that social reality is given for the scientist and his only task is to imitate what is there without changing it. But actually scientific knowledge is in large part an invention or development rather than an imitation; concepts, hypotheses, and theories are not found ready-made in reality but must be constructed. Further, scientific knowledge is part of the process of self-awareness by which societies and individuals "reconstruct" themselves, as I shall argue in chapter 18, so that knowledge necessarily changes what is given. The test of truth in the social sciences, as Dewey used to argue, is whether a theory succeeds in changing its social referent, in some fashion that remains to be specified.

Pattern is also a pretty vague term, as vague as *method* and *logic*. It refers here to a regular, systematic, step-by-step series of procedures used by some group of scientists. The procedures are not mechanical or automatic, nor do they constitute an algorithm guaranteed to give results. They are rather to be applied flexibly according to circumstances; their order may vary, and alternatives are available at every step. In this respect they are more like the search procedures incorporated in Newell and Simon's "General Problem Solver" (1963) and in Cyert and March's simulation of managerial decision procedures (1963). To be sure, they are not sufficiently formalized to be put into a computer program, but they resemble the complexity and susceptibility to unexpected results of a computer program more than they do the austere single-mindedness of symbolic logic.

Justification and verification are not treated as a separate set of procedures occurring *after* "discovery," but are included *within* the process of discovery. In some methods verification is scattered throughout the process, and in others it occurs at one definite point; in some methods there are two or more kinds of verification and in others there is only one; but in any case, verification is always a subordinate part of a larger process of discovery. It constitutes the check point or points in the process.

Most important, what is invented or developed is not just hypotheses but the whole conceptual apparatus of science—methods and techniques, scales and indices, variables and factors, concepts, hypotheses, and models. None of these are either given to scientists or arbitrarily created ("conjectured") by them; they are all worked out step by step in the regular procedure that constitutes scientific method.

I myself do not especially like the traditional phrase "logic of discovery," and prefer to describe scientific methods as "heuristics," but this term also may have inappropriate connotations. "Heuristic" means for some people a haphazard trial-and-error process, and I do not intend this connotation. For others it is a term of disparagement applied to scientific work so poor that it has no noticeable results; such work at least has the heuristic value of helping one avoid the same mistakes next time. Then there are the formalist's "heuristic interpretations," which help make his theory intelligible to simple-minded empiricists. These connotations are all exaggerations of a central core of meaning that may be roughly expressed as follows: a heuristic is a loosely systematic procedure for investigation or inquiry that gives good results eventually and on the whole, but does not guarantee them in any particular case and certainly cannot promise "optimum" results. Heuristic is opposed to algorithm and is similar to search (in Herbert Simon's sense), research, inquiry, and the like.

I shall illustrate these points with an example from survey research, a method not treated in detail in this book. One of the principal tasks of the survey research method is the "discovery" of concepts. These concepts should be related by operational definitions to variables that behave in a lawlike manner, and the variables in turn should be reliably measurable by indices, scales, or test scores. All these subsidiary entities are also developed, or in some cases adapted, in the process of developing a concept.

The concept I have chosen for an illustration is "intraception," and the history of its development has been reported by Levinson et al. (1966). Intraception was first discussed and defined by Murray (1938) as follows: "The dominance of feelings, fantasies, speculations, aspirations. An imaginative, subjective human outlook. Romantic action." Vague as this definition and its discussion by Murray may seem, he regarded it as a refinement of still vaguer concepts advanced by James and Jung. He felt that it singled out one component of the tender-mindedness and introversion complexes and made it available for measurement.

Murray proposed several measures of intraception, but his primary interest was in the clarification of the concept. In contrast, the primary methodological focus in the *Authoritarian Personality* studies (Adorno et al., 1950) was on measurement, and in particular on developing the so-called F scale. One of the components of authoritarianism, as measured by the F scale, turned out to be anti-intraception, which, however, was not always inversely correlated with intraception. The F scale could then be used for further study of intraception, anti-intraception, and extraception.

Further refinement of the concept was achieved by Levinson and his associates in their research on mental hospitals, in which they used the F scale among other instruments. They developed the following definition: "Intraception is the disposition, expressed through various modalities, to emphasize and differentiate psychological aspects of oneself and of the external world" (Levinson et al., 1966, p. 126). They also developed an intraception index with four indicators that intercorrelate in the .3 to .6 range. This moderate level of correlation indicates that intraception as revised is still a somewhat vague concept, perhaps multidimensional, and that still better indicators could be developed.

The line of development here seems to move, on the concept side, from vagueness and complexity toward explicitness and simplicity, and from imaginative description toward lawlike correlations. (I have omitted the discovered correlations from my summary.) On the measuring instrument side, the line of development seems to move from broad general scales and indices toward reliable, specific indices with high indicator intercorrelations. The method, at the most general level, moves back and forth between concept and measurement, with the results of each used to refine and improve the other. This back-and-forth movement appears both in the thirty-year history of this line of research and in the detailed work of Levinson and his associates, who describe their work as a continuing dialectic between concept and empirical findings (1966, p. 129).

I select a second contrasting example from experimental work, another method not discussed systematically in these pages. W. K. Estes, in a 1968 colloquium at Buffalo, reported some of his experimental work dealing with the effect of nonreinforced trials on learning. His problem was set by the fact that a number of experimental studies had shown that the learning curve continues to rise during a series of nonreinforced trials, while a number of other studies had reported a level curve for nonreinforced trials. His first step was to search both sets of studies to find, if possible, some characteristics that were uniformly present in one group but not in the other. He found one such characteristic: When the allowable time for response was 2 seconds or less, the learning curve rose during nonreinforcement; when response time was more than 2 seconds, the curve remained level. His next step was to search the experimental literature for findings about response time in the neighborhood of 2 seconds. He found a generalization that response latency (the amount of time it takes to respond) in that type of learning experiment begins at approximately 3 seconds on the average and, with practice, shortens toward approximately 1 second. Este's next step was to put these two findings into a deductive relationship. Together they implied that when allowable response time was 2 seconds or less, for many subjects there was not sufficient time to respond on initial trials; but as latency decreased with practice to below 2 seconds, the response rate would increase independently of whether any additional learning was occurring. This suggested the hypothesis that the rising learning curve during nonreinforcement was an artifact of the brief response time allowed; the rising curve did not measure learning but rather a decrease of response latency. The next step was to devise experiments to test this hypothesis (which, incidentally, was confirmed).

This example differs from the previous one in that it concerns the work of a single experimenter over several months, rather than groups of researchers over thirty years. Also a hypothesis rather than a concept was "discovered," and its development preceded testing. Nevertheless, the development phase was just as regular and systematic as the testing phase and had its own logic and its own check points. Estes did not dream up his hypothesis; he deduced it from propositions discovered by systematic search.

These two reports may not have accurately described what the researchers were actually doing, and their work may have been atypical in various ways and degrees. If I were to study their methods more systematically, I would have to investigate both these questions. I would also have to be more specific about how, in the first example, concept led to improved scale or index and how empirical findings led to improved concept. But the reports serve to illustrate what I mean by calling scientific methods "heuristics" or "search procedures" or "logics of discovery."

Perhaps the reader is now in a position to select his own name for the present account of scientific method.

Method of the Present Work

Anyone who discusses method must eventually face the question of what *his* method is. After some thought I have concluded that my method all along has been that of participant observation. My approach is essentially anthropological; I treat various methods as subcultures within the general culture of science, each subculture belonging to a community within the general society of social scientists. There are as many methods as there are distinguishable communities of scientists, and the boundaries of each method are those of the community that uses it.

A community is located by finding people who interact regularly with one another in their work. They read and use each other's ideas, discuss each other's work, and sometimes collaborate. They have common friends, acquaintances, intellectual ancestors, and opponents, and thus locate themselves at roughly the same point in sociometric space. Their interaction is facilitated by shared beliefs and values—goals, myths, terminology, self-concepts—which make their work mutually intelligible and valuable. Although they do not all use exactly the same procedure in their work, there is a great deal of similarity, and the differences are accepted as variant realizations of the same values.

Conversely, the boundary of a community is marked by noninteraction, and more definitely by interminable polemics and unresolved misunderstandings. Examination of the polemics reveals differences in beliefs, goals, and values that make rational discussion and collaboration difficult or even impossible.

A method consists of the actual procedures used by members of a community, and the variations of procedure illustrate the range of variants of the method. Each method is justified and explained by an ideology or philosophy of science which specifies the goals of science, the available and permissible means, the impermissible errors, the proper subject matter, the heroic exemplars, and the unfortunate failures or pseudoscientific villains. Needless to say, the actual method always deviates from the prescriptions of its associated ideology, and the successful deviations are the source of change in method.

Some deviants are marginal men, in the sense that they have absorbed parts of two different ideologies and have a diffuse or split identity, and these deviants may mediate between two communities of scientists. If they are successful, that is, accepted and imitated, they become the medium for collaboration between the communities. Collaboration may lead to a regular division of labor, to an interpenetration of ideologies, and sometimes eventually to a partial integration of the two communities. In this way two methods may become variants of a single method, though the original methods may also continue in use. Conversely, other deviants may develop variations in a method that eventually becomes a new method used by a new community. Still other deviants may move by stages into some other community and become accepted as members there.

This conception of method is historically oriented and relativistic. Methods change slowly and continually; they develop, combine, and separate. They have no timeless essence—or any essence they may have does not become apparent in this approach—and are not separated by any fixed boundaries. Some boundaries at some times are quite sharp, such as the boundary between the clinical and the experimental method, which nowadays is crossed or straddled by few people. The Murray group at the Harvard Psychological Clinic made such an attempt, but it does not seem to have caught on. Other boundaries are rather indistinct and are freely crossed or well populated, such as the boundary between statistical surveying and mathematical modeling in the 1950's and the boundary between statistical surveying and participant observation at present. In these cases one needs to use a good deal of care (or recklessness) in drawing a boundary, if indeed a boundary is needed. Some border areas may be regarded as belonging indifferently to one or the other adjacent method, and their true status may not become clear until they have developed further. There are at least two such border areas at present, each showing promise (and some achievement) of new methodological developments. One is the combination of experimentation and mathematical modeling that I shall discuss in chapter 4. The other is the combination of some aspects of particpant observation with survey research techniques in the comparative study of particular political systems; much of this work is still unpublished (for example, Frederick Frey's continuing work on Turkish politics). I shall discuss some earlier stages of this development in chapter 12.

The task of the participant observer is to describe methods as they actually exist in a certain time period, which in the present case is the last two to four decades. It is necessary to take account of practices, supporting ideologies, and ranges of deviation, and to relate these to each other.

A variety of techniques is available for this purpose. First, observation of a literate culture includes, but is not limited to, reading its written output. Published scientific work can be treated as artifacts of the culture, analogous to potsherds, and can be used to reconstruct some of its typical modes of behavior. Articles on methodology can be treated like informants' reports in work on nonliterate cultures. Like most such reports, they are likely to be idealized accounts of what happens at best rather than what happens typically (for example, Nagel, 1961, pp. 503-520). Some are outright myths (Lewin, 1936, ch. 1; Radcliffe-Brown, 1957), valuable as indicators of goals, values, and belief systems. Polemical articles are useful indicators of variance in belief systems and of the boundaries of scientific communities.

To find out what actually happens in science, direct observation is necessary in addition to reading. This means observation of work in progress, including the study of experimental apparatus, questionnaires, field notes and diaries, uncompleted models, and particularly the comparison of different stages in the development of an apparatus, questionnaire, or model. It means talking and listening, personally and in colloquia, about a scientist's own work and about the work of others, in order to discover not only actual procedures but also particular modes of thinking, approaches to problems, and critical standards. Direct participation in scientific work, experiencing at first hand the problems and the modes of solution in use, is indispensable if one is to infer the actual performance behind published work and to interpret the meaning of methodological discussions.

Values, beliefs, and attitudes can also be studied as they are being transmitted to new scientists. This socialization process can be observed by taking and visiting courses, attending lectures, and looking at textbooks. One can even learn something by taking part in those barbaric sacred rites called "scientific conventions," though there it is difficult for outsiders to gain proper entry to the mysteries.

I do not mean to imply that I have used all of these techniques to the fullest, though I have done some of each and a great deal of most of them. Like most accounts of method, the above is an idealization, constructed after the fact. My cry is the cry of all fledgling field workers: "If only I had known at the beginning of my field work what I know now, I could have done so much better!" Indeed, my own performance has been so far below the ideal that I fear it resembles more the earliest anthropological work, the reports of naive travelers to far-off lands. Like them, I journeyed to these strange cultures originally in search of the gold of truth, hoping to exchange my shiny philosophical trinkets for it, and stayed to marvel at the intricate customs, the weird rituals, the incomprehensible feuds and wars, the admirable human beings I found there. I hope, however, that I have at times succeeded in approximating what Gluckman calls "the method of apt illustration" (Epstein, 1967, p. xiii), which is a step higher up the ladder than the traveler's report. A still more adequate method, it seems to me now, would be one that makes extensive use of quantitative data and statistical techniques; but if I had adopted such a method this book would never have been finished.

Like all methods, the participant-observer method has its characteristic biases and defects, and it is well to take note of them at the outset. To begin, some biases: First, the observer of a living system expects to find both ideals and practices, norms and facts, in interplay with each other. He looks for mechanisms of social control whereby practice is kept within an acceptable range of ideals, and ideals are reinterpreted to remain relevant to practice. He expects to find ranges of deviation beyond the allowable limit and is interested in the deviants as both indicators of strain and sources of innovation and diffusion.

Second, the process of studying a living system from the inside leads one to identify with it and to accept its own standards and outlook. Among anthropologists this tendency is expressed in the doctrine of cultural relativism, the doctrine that each culture has its own problems and achievements, its own strengths and weaknesses, and that it must be sustained and improved on its own unique terms. However this doctrine may have been misinterpreted by eager philosophical critics and overextended by an occasional enthusiast, as a methodological bias it simply expresses a resolve to understand a culture in its own categories. It has no more metaphysical significance than the experimentalist's postulate of universal determinism, which expresses his resolve to keep looking for causes. Cultural relativism is not incompatible with a search for cultural universals, but it does carry with it a great skepticism about any proposed universals, a belief that there are probably exceptions or that the universal is usually described too narrowly. When an anthropologist does claim to have found a valid universal, it is likely to be something quite abstract, a functional prerequisite that can be satisfied in different ways by different cultures—and even then he would not be surprised to hear of an exception or two. In the present context this characteristic bias becomes a belief that all scientific methods must be understood in their own terms and improved in their own ways, and that any general characteristics of scientific method are likely to be requirements that can be satisfied in a variety of ways.

One difficulty of the participant-observer method is its tendency to draw a sharp boundary around its subject. Any scientific method has this tendency to some extent, since what is studied is treated differently from the surrounding material which is not studied. But the tendency is stronger in participant observation because its practitioners attempt to interpret their subjects as going systems, and systems have boundaries and boundary-maintenance functions. If one's subject is a Pacific island culture or a small jungle tribe, the tendency does not lead to appreciable distortions; indeed, the anthropologist who studies a whole isolated culture can correctly claim that he, of all social scientists, is most justified in drawing a boundary around his subject. But when the subject is a small town, a factory, a street corner gang, a subculture, or in general anything that is also a part of a larger system, the danger of distortion must be faced. In the present context the danger is in a tendency to think that there are four, or five, or some other definite number of methods in use, and to forget that there are also innumerable combinations, variations, and boundary cases. Some of the hybrids are misguided artificial creations that cannot survive, but others may be valuable improvements or precursors of future methods, and still others may turn out to be methods in their own right. There is a similar danger in thinking that the social sciences are themselves sharply defined, simply because we are treating them in that fashion. Actually they are an interdependent part of a larger system, Western society, and probably of other systems as well.

Another difficulty in the participant-observer method is separating the observer's contribution, or bias, from the contribution made by his subject matter. The account in the following chapters must be interpreted as partly an expression of my own biases and partly an account of what is actually there, and it will not be easy to separate the two, particularly if the reader has biases of his own. One solution to the difficulty (though not necessarily the best one) is for the observer to make his biases explicit, and I shall attempt to do this.

In the first place, my original unconscious drift into the participant-observer method expresses a preference for direct observation of particular fact and a self-critical suspicion of all generalization and abstraction. It also expresses a preference for complexity and disorder over clarity and simplicity, or, more accurately, an ambivalence on this point. I regard positively all social science methods and theories, but my admiration for logic and mathematics is a recent acquisition, so recent that I am still a novice rather than a fully initiated adept. I dislike anything that claims superiority, dominance, or orthodoxy, and I prefer to believe that all established truths, including my own, must be mistaken. My particular preference for psychoanalytic theory and my relative dislike for neoclassical economic theory will also soon become apparent. The reader will undoubtedly find additional biases as he goes along.

Preliminary View of the Social Sciences

Before beginning our detailed studies of particular methods, let us take a quick preliminary look at the social sciences as a whole.

The social sciences are a doubly segmented society, divided by two principles of grouping that cut across each other. In this they resemble various Plains Indian societies, such as the Cheyenne, whose members are divided both into clans and into voluntary soldier societies. One principle of grouping, the clan principle, is the professional field: for example, psychology, sociology, or economics. A person enters a field by taking the appropriate course of training (socialization) and by finding a job recognized as belonging to the field—teaching, research, clinical practice, etc. He remains in the field by holding that job or moving to other jobs of higher status. Each field is controlled by its elders, who decide on job offerings, advancement, and co-optation to the ruling group. Each main field has several subdivisions, but control is largely retained by the field elders rather than those in the subfield. Individual departments of a university may combine two or more fields, but control is still by field, as a member may move in and out of departments within his field.

The other principle of grouping is the method. A person adopts a method by engaging in supervised research (socialization) and continues by doing more research, either individually or in teams. Members consult and criticize each other, check each other's results or build on them, exchange techniques, and in general collaborate extensively. Control of a method is a more complex process (which I shall discuss presently), but personal prestige is more important than official position because of extensive personal collaboration. A method provides opportunities for achievement and influence, while a field with its primarily ascriptive values provides financial and emotional security, official advancement, power, and personal identity.

The conflict between these two modes of grouping is a prominent feature of the society. The two are interdependent in that work achievement is demanded for membership and advancement in a field, while financial security is necessary for work. But the strengthening and consolidation of each system tends to weaken the other, because they cut across one another. Increased cohesiveness of a field cuts one off from colleagues in other fields using the same method and thus reduces the wide collaboration that is important for scientific advance. Conversely, wide-ranging collaboration reinforces methodological differences within a field and leads to increased strife and polemics within departments and at field conventions.

The conflict is functional for social science as a whole, because it preserves integration by preventing subsystems from becoming too cohesive. The fact that methods and fields largely cut across one another forces users of different methods together within a field, while it brings members of different fields together within a method. Whenever a field (such as experimental psychology recently or economics fifty years ago) achieves substantial unity of method and high internal cohesion, contact with other fields and with other methods is weakened and theory stagnates. The field moves into a scholastic phase in which attention is focused on smaller and smaller details within an essentially unchanging theoretical framework. A similar stagnation of method would occur if one method were completely isolated from others.

Conflict between the two subsystems, and therefore the unity of the social science system as a whole, can be maintained only insofar as each subsystem maintains its own unity against the disrupting influence of the other. How is this done? The unity of a field, I suspect, is essentially maintained by the job-placement system. The field elders must maintain extensive and close personal contacts to carry out their control task. They must continually exchange information about job applicants and openings, promotions, moves, and departmental politics affecting future openings and applicants. Newer field members develop extensive contacts for the same purpose. My guess is that a content analysis of intimate conversation at conventions and during visits would show a higher frequency of job conversation than of scientific discussion. These contacts unite proponents of different methods as friends and reduce methodological prejudices. If methodological commitment is too strong, contact is limited to those using similar methods, departments become specialized in a particular approach, and the field becomes fragmented. A corollary of this suggestion is that when jobs are plentiful, job control and field unity will be weakened and collaboration across field boundaries will increase.

A second and derivative basis of field unity is the personal identification of individual scientists within a field. A scientist will identify himself as an anthropologist, for instance, rather than as a survey researcher. This means that a field takes on some of the characteristics of a clan; one belongs to it, is accepted by it, and finds security in it. Other field members are brothers and one has an obligation of loyalty to them, even though they may use different methods. Contact with nonfield members may be adventuresome and exciting, but also carries a danger of disloyalty and betrayal and, in extreme cases, even loss of identity. Consequently, collaboration across fields on the basis of a shared method is usually, in the cases I have examined, a cautious affair marked by emotional reserve and frequent reaffirmation of personal differences.

Unity of method is maintained in a more complex way because methods are less institutionalized than fields. Socialization is important in transmitting the culture of a method, but it is often weakened by cross-socialization in a methodologically mixed department. After socialization is completed, deviation is controlled by the methodologists, who function as moralists prescribing canons of methodological purity. In addition, they create myths that dramatize the importance of correct behavior—for example, historical myths that describe the progress of science from error and superstition (false methods) to its present enlightenment (true method). Galileo is the hero of many historical myths, so much so that one turns to a new account of Galileo's work in the confident anticipation of enjoying a new myth. The empiricists tell how their hero dared to look for himself and associate him vaguely with the leaning tower of Pisa and the moons of Jupiter. The formalists point to his crude experimental apparatus—waterclocks and wooden planks—and argue that his infrequent "experimenting" was simply a device to give his formal models an empirical interpretation. Lewin in turn makes him out to be a proto-clinician in disguise (Lewin, 1936, ch. 1). In addition to historical myths, there are utopias showing how the golden future of science will be brought about through methodological correctness. Autobiographical accounts reveal that these myths and methodological prescriptions are taken seriously (Homans, 1962, ch. 1; Skinner, 1959b). (In Skinner's myth he himself is the hero). For those disposed to wander, there are myths about the lost sheep, the pseudo-scientists who use false methods, illustrating the horrible consequences of deviation (Gray, 1962). Here again one finds in conversation that (for example) the experimentalist's fears of sinking gradually into the clinicians' morass or the formalists' mathematical fantasies are real and strong. Against insidious moral danger, constant striving for purity of method is the only protection.

The gentle persuasion of the moralist-methodologist is supplemented by the stronger witchcraft of the journal editor. If one wonders about the remarkable uniformity of method displayed in certain journals and asks the authors of articles why they write that way, they answer, "It's always done that way, that's science, isn't it? You do A, B, C, etc. Besides, we couldn't get it published any other way" (cf. also Riesman and Watson, 1964, p. 311). Then there is the occasional anguished cry of the bewitched victim: "One year of research down the drain to satisfy an editor's pet theory!"

These specific unifying influences give guidance to a more pervasive influence, the diffuse sanctions inherent in widespread collaboration with one's peers. And, finally, the unity of a fellowship of work is strengthened by occasional polemics with misguided proponents of different methods.

The continuous interplay between method and field is, perhaps, occasionally affected by another character, this one an outsider, the philosopher of science. I learned early to avoid these missionary types; their continuous cry, "Repent! You aren't being truly scientific! That was only an explanation sketch, not an explanation!" was unedifying and wearying. Others of a different theological orientation, but equally unedifying, would say "Stop trying to act like scientists! The phrase 'policy science' is logically self-contradictory!" Their numerous theological arguments were also uninteresting; they seemed mainly to disagree over whether social scientists could in principle be saved and go to the heaven of physics or whether they were predestined to damnation. As for this heaven, I cannot tell whether it exists or is another myth, but at any rate I heard marvelously varied accounts of it. Their disputes always seemed to be phrased in terms of how it was in physics, a field which they knew well. Some of them talked in addition of a second heaven above physics, called "fully axiomatized science," a heaven with its own ideal language and method. The doings of social scientists interested them little, except as a basis for a catalog of error; if social scientists did well, they would eventually be in physics anyway, and their present earthly ways would be forgotten.

More recently I have come across a new kind of missionary, exemplified by Kaplan (1964) and perhaps Mischel (1966), who force me to revise my earlier stereotype. This new missionary does not speak of repentance and salvation, but says, rather, "If I am to be helpful to these people, I must first learn to understand them." Instead of making quick forays into the social sciences in pursuit of sinners, he goes to live there and seems actually to enjoy it. With these new missionaries I can be friends, as long as they avoid theology.

Still more recently I have come to appreciate Hempel's work, have seen its value for science, and have used some of his ideas in my own thinking. But it still is the case that my interest is different from that of most philosophers of science. I wish to be neither coach nor umpire, as Kaplan classifies the philosophers. I wish simply to understand and describe the methods of social scientists, to see what they are really up to, and to note both pitfalls and improvements as they appear. I do not wish to dispute about the timeless characteristics of an ideal science, only to describe present developments in all their variety and historical uniqueness. I do not wish to be a philosopher *of* science, where *of* means "different from and superior to." I wish only to participate in the scientific enterprise here and now, contributing those particular things of which I am capable.

References

Abelson, Robert , and M. Rosenberg . 1958. Symbolic psycho-logic, Behavioral Science, 3:1-13.
Abelson, Robert , and M. Rosenberg . 1963. Computer simulation of hot cognition, in Tomkins and Messick , eds., Computer Simulation of Personality. New York: Wiley.
Aberle, David . 1961. Matrilineal descent in cross-cultural perspective, in D. Schneider and K. Gough , eds., Matrilineal Kinship. Berkeley: University of California Press.
Aberle, David , et al. 1950. The functional prerequisites of a society, Ethics, 60:100-111.
Adams, Ernest . 1960. Survey of Bernoullian utility theory, in H. Solomon , ed., Mathematical Thinking in the Measurement of Behavior. Glencoe: Free Press.
Adorno, T. W. , et al. 1950. The Authoritarian Personality. New York: Harper.
Ake, Claude . 1967. A Theory of Political Integration. Homewood: Dorsey.
Alker, Hayward . 1965. Mathematics and Politics. New York: Macmillan.
Allen, R.G.D. 1938. Mathematical Analysis For Economists. New York: Macmillan.
Almond, Gabriel , and J. S. Coleman , eds. 1960. The Politics of Developing Areas. Princeton: Princeton University Press.
Almond, Gabriel , and S. Verba . 1963. The Civic Culture. Princeton: Princeton University Press.
Anderson, Alan , ed. 1964. Minds and Machines. Englewood Cliffs: Prentice-Hall.
Apter, D. 1965. The Politics of Modernization. Chicago: University of Chicago Press.
Archibald, K. , ed. 1966. Strategic Interaction and Conflict. Berkeley, Calif.: Institute of International Studies.
Arlow, Jacob . 1959. Psychoanalysis as scientific method, in S. Hook , ed., Psychoanalysis, Scientific Method, and Philosophy. New York: N. Y. U. Press.
Arrow, Kenneth . 1951. Social Choice and Individual Values. New York: Wiley.
Ashby, W. Ross . 1956. Introduction to Cybernetics. New York: Wiley.
Balandier, G. 1966. The colonial situation, in E. Wallerstein , ed., The Colonial Situation. New York: Wiley.
Balderston, F.E. , and Austin C. Hoggatt . 1963. Simulation models: analytic variety and the problem of model reduction, in Hoggatt and Balderston , eds., 326 Symposium on Simulation Models. Cincinnati: Southwestern Publishing Co.
Bales, R.F. 1950. Interaction Process Analysis. Cambridge: Addison Wesley.
Bales, R.F. 1953. The equilibrium problem in small groups, in Parsons , Bales , and Shils , Working Papers in the Theory of Action. New York: Free Press.
Bales, R.F. , A. Couch , and P. Stone . 1962. The interaction simulator, in Proceedings of a Harvard Symposium on Digital Computers and Their Applications, Cambridge: Harvard University Press.
Bandura, Albert . 1961. Psychotherapy as a learning process, Psychological Bulletin, 58:143-159.
Banfield, Edward . 1961. Political Influence. New York: Free Press.
Banks, Arthur S. , and Robert Textor . 1963. A Cross-Polity Survey. Cambridge: M. I. T. Press.
Banton, Michael , ed. 1965. The Relevance of Models for Social Anthropology. New York: Praeger.
Baruch, Dorothy W. 1952. One Little Boy. New York: Julian Press.
Bateson, Gregory . 1936. Naven. Stanford: Stanford University Press.
Beattie, John . 1965. Understanding an African Kingdom: Bunyoro. New York: Holt, Rinehart, and Winston.
Beattie, John . 1968 (1959). Understanding and explanation in social anthropology, in R. Manners and D. Kaplan , eds., Theory in Anthropology. Chicago: Aldine Publishing Co.
Becker, Howard S. , and Blanche Geer . 1960. Participant observation, in Richard Adams and J. Preiss , eds., Human Organization Research. Homewood, Illinois: Dorsey.
Becker, Howard S. , et al. 1961. Boys in White. Chicago: University of Chicago Press.
Bendix, Reinhard . 1963. Concepts and generalizations in comparative sociological studies, American Sociological Review, 28:532-539.
Benedict, Ruth . 1934. Patterns of Culture. Boston: Houghton, Mifflin.
Bernard, Jessie . 1945. Observation and generalization in cultural anthropology, American Journal of Sociology, 50:284-291.
Reprinted in R. Manners and D. Kaplan , eds., Theory in Anthropology. Chicago: Aldine.
Bettelheim, Bruno . 1950. Love is Not Enough. Glencoe: Free Press.
Bettelheim, Bruno . 1955. Truants From Life. Glencoe: Free Press.
Binder, Leonard . 1961. Iran. Berkeley: University of California.
Black, Max , ed. 1961. The Social Theories of Talcott Parsons. Englewood Cliffs: Prentice-Hall.
Blau, Peter . 1964. The research process in the study of the dynamics of bureaucracy, in Phillip Hammond , ed., Sociologists at Work. New York: Basic Books.
Blumer, Herbert . 1956. Sociological analysis and the variable, American Sociological Review, 21:683-690.
Borch, Karl . 1968. The Economics of Uncertainty. Princeton: Princeton University Press.
Bott, Elizabeth . 1957. Family and Social Network. London: Tavistock.
Boulding, Kenneth . 1953. The Organizational Revolution. New York: Harper.
Boulding, Kenneth . 1962. Conflict and Defense. New York: Harper Torchbooks.
Briefs, Henry . 1960. Three Views of Method in Economics. Washington, D. C: Georgetown University Press.
Brodbeck, May . 1959. Models, meaning, and theories, in L. Gross , ed., Symposium on Sociological Theory. Evanston: Row, Peterson.
Brody, Richard . 1963. Some systemic effects of the spread of nuclear weapons technology: a study through simulation, Journal of Conflict Resolution, 7:665-753.
Bronson, Gordon . 1965. The hierarchical organization of the central nervous system: implications for learning processes, Behavioral Science, 10:7-25.

327 Brown, Robert . 1963. Explanation in Social Science. Chicago: Aldine.
Brownell, Baker . 1950. The Human Community. New York: Harper.
Bruyn, Severyn . 1966. The Human Perspective in Sociology. Englewood Cliffs: Prentice-Hall.
Buck, R. C. 1956. On the logic of general behavior systems theory, in H. Feigl and M. Scriven , eds., Foundations of Science and the Concepts of Psychology and Psychoanalysis. Minneapolis: University of Minnesota Press.
Bunzel, Ruth . 1929. The Pueblo Potter. Columbia University Contributions to Anthropology. VIII. New York: Columbia University Press.
Bush, Robert R. , and F. Mosteller . 1955. Stochastic Models for Learning. New York: Wiley.
Bush, Robert R. , and F. Mosteller . 1960. Survey of mathematical learning theory, in R. D. Luce , ed., Developments in Mathematical Psychology. Glencoe: Free Press.
Byers, N.F. 1959. Economic, logical, and mathematical systems, Social Research, 26:379-402.
Caplow, T. 1956. A theory of coalitions in the triad, American Sociological Review, 21:489-493.
Caplow, T. 1959. Further development of a theory of coalitions in the triad, American Journal of Sociology, 64:488-493.
Catton, William . 1965. The concept of mass in the sociological version of gravitation, in Fred Massarik and P. Ratoosh , eds., Mathematical Explorations in Behavioral Science. Homewood: Irwin-Dorsey.
Christie, Richard . 1956. Eysencks treatment of the personality of Communists, reply by Eysenck and rejoinder by Christie, Psychological Bulletin, 53:411-451.
Churchman, C. W. 1957. Introduction to Operations Research. New York: Wiley.
Churchman, C. W. 1963. An analysis of the concept of simulation, in Hoggatt and Balderston , eds., Symposium on Simulation Models. Cincinnati: Southwestern Publishing Co.
Clark, Kenneth . 1965. Dark Ghetto. New York: Harper.
Cohen, Bernard P. 1963. Conflict and Conformity. Cambridge: M.I.T. Press.
Cohen, Kaiman . 1960. Computer Models of the Shoe, Leather, and Hide Sequence. Englewood Cliffs: Prentice-Hall.
Cohen, Kaiman , and Richard Cyert . 1961. Computer models in dynamic economics, Quarterly Journal of Economics, 75:112-127.
Cohen, Kaiman , and Richard Cyert . 1965. Theory of the Firm. Englewood Cliffs: Prentice-Hall.
Cohen, Ronald . 1964. Conflict and change in a Northern Nigerian Emirate, in George Zollschan and Walter Hirsch , eds., Explorations in Social Change. Boston: Houghton Mifflin.
Cohen, Ronald . 1970. Review of Swartz, Local-Level Politics . American Anthropologist, 72:112-115.
Coleman, James S. 1960. The mathematical study of small groups, in H. Solomon , ed., Mathematical Thinking in the Measurement of Behavior. Glencoe: Free Press.
Coleman, James S. 1964. Introduction to Mathematical Sociology. New York: Free Press.
Colson, Elizabeth . 1954. The intensive study of small sample communities, in Robert Spencer , ed., Method and Perspective in Anthropology. Minneapolis: University of Minnesota Press. Reprinted in Epstein, 1967.
Colvard, Richard . 1967. Interaction and identification in reporting field research, in G. Sjoberg , ed., Ethics, Politics, and Social Research. Cambridge: Schenkman.
Coser, Louis . 1955. The Functions of Social Conflict. Glencoe: Free Press.
Crimmel, W. Manuscript . An Introduction to Logics.
Cronbach, Lee , and Paul Meehl . 1955. Construct validity in psychological tests, Psychological Bulletin, 52:281-302.
328 Cross, John . 1965. A theory of the bargaining process, American Economic Review, 55:67-94.
Crossman, Richard , ed. 1950. The God That Failed. New York: Harper (Bantam paperback edition).
Cyert, Richard , and E. Grunberg . 1963. Assumption, Prediction, and Explanation in Economics, in Cyert and March , eds., A Behavioral Theory of the Firm. Englewood Cliffs: Prentice-Hall.
Cyert, Richard , and J. March . 1963. A Behavioral Theory of the Firm. Englewood Cliffs: Prentice-Hall.
Dahl, Robert . 1963. Modern Political Analysis. Englewood Cliffs: Prentice-Hall.
Dalton, Melville . 1959. Men Who Manage. New York: Wiley.
Dalton, Melville . 1964. Preconceptions and methods in Men Who Manage , in Phillip Hammond , ed., Sociologists at Work. New York: Basic Books.
Davis, Kingsley . 1967 (1959). The myth of functional analysis, reprinted in N. J. Demerath and W. A. Peterson , eds., System, Change, and Conflict. New York: Free Press.
Davis, Otto , 1969. Notes on strategy and methodology for a scientific political science, in Joseph Bernd , ed., Mathematical Applications in Political Science, IV. Charlottesville, U. P. of Virginia.
Deutsch, Morton . 1949. A theory of cooperation and competition, Human Relations, 2:129-151.
Deutsch, Morton . 1962. Cooperation and trust: some theoretical notes, in Nebraska Symposium on Motivation, pp. 275-319. Lincoln: University of Nebraska.
Deutsch, Morton . 1965. Some psychological aspects of social interaction, in B. Wolman , ed., Scientific Psychology. New York: Basic Books.
Deutsch, Morton . 1966. Rejoinder to Kelleys comments, in K. Archibald , ed., Strategic Interaction and Conflict, pp. 44-48. Berkeley: Institute of International Studies.
Deutsch, Morton , and R. Krauss . 1962. Studies of interpersonal bargaining, Conflict Resolution, 6:52-76.
Diesing, Paul . 1967. National self-determination and U. S. foreign policy, Ethics, 77:85-94.
Downs, Anthony . 1957. An Economic Theory of Democracy. New York: Harper & Row.
Dozier, Edward . 1954. The Hopi-Tewa of Arizona. Berkeley: University of California Press.
Easton, David . 1965. A Systems Analysis of Political Life. New York: Wiley.
Edwards, Ward . 1961. Probability learning in 1000 trials, Journal of Experimental Psychology, 62:385-394.
Eggan, Fred , ed. 1937. Social Anthropology of North American Tribes. Chicago: University of Chicago Press.

Eggan, Fred . 1954. Social anthropology and the method of controlled comparison, American Anthropologist, 56:743-763.
Eisenstadt, S. N. 1958. The study of Oriental despotisms as systems of total power, Journal of Asian Studies, 17:435-446.
Eisenstadt, S. N. 1961. Anthropological studies of complex societies, Current Anthropology, 2:201-210.
Eisenstadt, S. N. 1963. The Political Systems of Empires. New York: Free Press.
Eisenstadt, S. N. 1964. Processes of change and institutionalization of the political systems of centralized empires, in George Zollschan and Walter Hirsh , eds., Explorations in Social Change. Boston: Houghton Mifflin.
Elkin, A. P. 1953. Murngin kinship re-examined, American Anthropologist, 55:412-419.
Epstein, A. L. , ed. 1967. The Craft of Social Anthropology. New York: Barnes and Noble.
329 Erikson, Erik . 1950. Childhood and Society. New York: Norton.
Erikson, Erik . 1959a. The nature of clinical evidence, in Dan Lerner , ed., Evidence and Inference. Glencoe: Free Press. Revised in Erikson, 1964, ch. 2.
Erikson, Erik . 1959b. Identity and the life cycle, (selected papers), in George S. Klein , ed., Psychological Issues. New York: International Universities Press.
Erikson, Erik . 1964. Insight and Responsibility. New York: Norton.
Erikson, Erik . 1966. The concept of identity in race relations, Daedalus, 95 no. 1:145-171.
Erikson, Erik . 1968. Identity, Youth and Crisis. New York: Norton.
Estes, W. K. , and J. H. Straughan . 1954. Analysis of a verbal conditioning situation in terms of statistical learning theory, Journal of Experimental Psychology, 47:225-234.
Estes, W. K. , and J. H. Straughan . 1959. The statistical approach to learning theory, in Sigmund Koch , ed., Psychology, vol. 2, pp. 380-491. New York: McGraw-Hill.
Etzioni, Amitai . 1961. A Comparative Analysis of Complex Organizations. New York: Free Press.
Etzioni, Amitai . 1968. The Active Society. New York: Free Press.
Evans-Pritchard, E. E. 1940. The Nuer. Oxford: Oxford University Press.
Evans-Pritchard, E. E. 1962a. Essays in Social Anthropology. London: Faber.
Evans-Pritchard, E. E. 1962b. Social Anthropology and Other Essays. New York: Free Press.
Eysenck, H. J. 1952. The effects of psychotherapy: an evaluation, Journal of Consulting Psychology, 16:319-324.
Eysenck, H. J. 1965. The effects of psychotherapy, International Journal of Psychiatry, 1:97-178.
Fallding, Harold . 1967. The family and the idea of a cardinal role, in G. Handel , ed., The Psychosocial Interior of the Family. Chicago: Aldine.
Fallers, Lloyd . 1960. The role of factionalism in Fox acculturation, in F. Gearing , et al., eds., Documentary History of the Fox Project. Chicago: University of Chicago.
Feibleman, James K. 1956. The Institutions of Society. London: Allen and Unwin.
Feigenbaum, Edward , and J. Feldman . 1963. Computers and Thought. New York: McGraw-Hill.
Feldman, Julian , 1962. Computer simulation of cognitive processes, in H. Borko , ed., Computer Applications in the Behavioral Sciences. Englewood Cliffs: Prentice-Hall.
Feldman, Julian , Fred Tonge , and Herschel Kantor . 1963. Empirical explorations of a hypothesis-testing model of binary choice behavior, in Hoggatt and Balderston , eds., Symposium on Simulation Models. Cincinnati: Southwestern Publishing Co.
Festinger, Leon , H. Riecken , and S. Schachter . 1956. When Prophecy Fails. Minneapolis: University of Minnesota Press.
Firth, Raymond . 1939. Primitive Polynesian Economy. London: Routledge.
Firth, Raymond . 1951. Elements of Social Organization. London: Watts.
Firth, Raymond . 1955. Function, in W. Thomas , ed., Yearbook of Anthropology. New York: Wenner-Gren Foundation.
Firth, Raymond . ed. 1957. Man and Culture. London: Routledge and Kegan Paul.
Fisher, Ronald . 1935. The Design of Experiments. London: Oliver and Boyd.
Forrester, Jay . 1961. Industrial Dynamics. Cambridge: M.I.T. Press.
Fortes, Meyer . 1949. The Web of Kinship Among the Tallensi. London: Oxford University Press.
Fortes, Meyer , and E. E. Evans-Pritchard , eds. 1940. African Political Systems. London: Oxford University Press.
Fouraker, Lawrence E. , and Sidney Siegel . 1963. Bargaining Behavior. New York: McGraw-Hill.
330 Freud, Sigmund . 1925. Collected Papers, vol. 3. London: Hogarth.
Friedman, Milton . 1953. Essays in Positive Economics. Chicago: University of Chicago Press.
Fromm, Erich , 1941. Escape From Freedom. New York: Holt, Rinehart, and Winston.
Fromm, Erich . 1947. Man For Himself. New York: Holt, Rinehart & Winston.
Fromm, Erich . 1948. Individual and social origins of neurosis, in Clyde Kluckhohn and H.A. Murray , eds., Personality in Nature, Society, and Culture. New York: Knopf.
Fromm, Erich . 1949. Psychoanalytic characterology and its application to the understanding of culture, in S. S. Sargent and M. W. Smith , eds., Culture and Personality. New York: Viking Fund.
Fromm, Erich . 1955. The Sane Society. New York: Holt, Rinehart and Winston.
Gamson, W. A. 1964. Experimental studies of coalition formation, in L. Berkowitz , ed., Advances in Experimental Social Psychology, 1:82-110. New York: Academic Press.
Gans, Herbert . 1967. The Levittowners. New York: Pantheon.
Gearing, Fred , et al., eds. 1960. Documentary History of the Fox Project. Chicago: Department of Anthropology, University of Chicago.
Geer, Blanche . 1964. First days in the field, in Phillip Hammond , ed., Sociologists at Work. New York: Basic Books.

Geertz, C. 1967. Ritual and social change: a Javanese example, in N. J. Demerath and W. A. Peterson , eds., System, Change, and Conflict. New York: Free Press.
Gladwin, Thomas , and Seymour Sarason . 1953. Truk: Man in Paradise. New York: Wenner-Gren Foundation.
Glaser, B. , and A. Strauss . 1965. Discovery of substantive theory: a basic strategy underlying qualitative research, American Behavioral Scientist. 8:5-12.
Glaser, B. , and A. Strauss . 1967. The Discovery of Grounded Theory. Chicago: Aldine.
Gluckman, Max . 1963. Order and Rebellion in Tribal Africa, Collected Essays. New York: Free Press.
Gluckman, Max . 1968. The utility of the equilibrium model in the study of social change, American Anthropologist, 70:219-237.
Gluckman, Max and E. Devons , eds. 1964. Closed Systems and Open Minds. Chicago: Aldine.
Goffman, Erving . 1962. On cooling the mark, in A. Rose , ed., Human Behavior and Social Processes. Boston: Houghton Mifflin.
Goffman, Erving . 1967. Interaction Ritual. Chicago: Aldine.
Goldberger, Arthur . 1959. Impact Multipliers and Dynamic Properties of the Klein-Goldberger Model. Amsterdam: North Holland.
Golembiewski, Robert . 1962. The Small Group. Chicago: University of Chicago Press.
Goodenough, Ward . 1968 (1956). Residence rules, in R. Manners and P. Kaplan , eds., Theory in Anthropology. Chicago: Aldine.
Goodman, Leo . 1964. Mathematical methods for the study of systems of groups, American Journal of Sociology. 70:170-192.
Gouldner, A. W. 1950. Patterns of Industrial Bureaucracy. Glencoe: Free Press.
Gouldner, A. W. 1954. Wildcat Strike. Yellow Springs, Ohio: Antioch.
Gouldner, A. W. 1959. Reciprocity and autonomy in functional theory, in L. Gross , ed., Symposium on Sociological Theory. Evanston: Row, Peterson.
Grant, D. A. 1962. Testing the null hypothesis and the strategy and tactics of investigating theoretical models, Psychological Review, 64:54-61.
Gray, D. J. 1962. Sociology as a science, American Journal of Economics and Sociology, 21:337-346.
Greenberger, Martin . 1965. A new methodology for computer simulation, in James 331 Beshers , ed., Computer Methods in the Analysis of Large-Scale Systems. Cambridge: M.I.T. and Harvard.
Guetzkow, Harold . 1962. A use of simulation in the study of inter-nation relations, in Guetzkow , ed., Simulation in Social Science. Englewood Cliffs: Prentice-Hall.
Guetzkow, Harold . 1963. Simulation in International Relations. Englewood Cliffs: Prentice-Hall.
Gullahorn, J. and J. 1963. A computer model of elementary social behavior, in Feigenbaum and Feldman , eds., Computers and Thought. New York: McGraw-Hill.
Gusfield, Joseph . 1960. Field work reciprocities in studying a social movement, in Richard Adams and Jack Preiss , eds., Human Organization Research. Homewood: Dorsey.
Gusfield, Joseph . 1963. Symbolic Crusade. Urbana: University of Illinois Press.
Gusfield, Joseph . 1967. Tradition and modernity: misplaced polarities in the study of social change, American Journal of Sociology, 72:351-362.
Habermas, Jrgen . 1967. Zur Logik der Sozialwissenschaften. Tbingen: Mohr. Reprinted by Verlag Zerschlagt das Brgerliche Copyright.
Haldi, J. , and H. Wagner . 1963. Simulated Economic Models. Homewood: Irwin.
Halle, M. , and K. Stevens . 1962. Speech recognition, IRE Transactions on Information Theory, pp. 155-159.
Hallowell, A. I. 1955. Culture and Experience. Philadelphia: University of Pennsylvania Press.
Hamblin, Robert , et al. 1969. Changing the game from Get the Teacher to Learn, Transaction, 6:20-31.
Handel, Gerald , ed. 1967. The Psychosocial Interior of the Family. Chicago: Aldine.
Hanson, Norwood . 1958. Patterns of Discovery. Cambridge: Cambridge University Press.
Hanson, Norwood . 1963. The Concept of the Positron. Cambridge: Cambridge University Press.
Harris, Marvin . 1968. The Rise of Anthropological Theory. New York: Crowell.
Harsanyi, John . 1961. On the rationality postulates underlying the theory of co-operative games, Journal of Conflict Resolution, 5:179-196.
Hartmann, Heinz . 1959. Psychoanalysis as a scientific theory, in S. Hook , ed., Psychoanalysis, Scientific Method, and Philosophy. New York: N.Y.U. Press.
Hebb, D. O. 1949. The Organization of Behavior. New York: Wiley.
Hebb, D. O. 1955. Drives and the C.N.S., Psychological Review, 62:243-254.
Hegel, G. 1945, originally 1821. Philosophy of Right, trans, by Knox . London: Oxford.
Helmer, Olaf , and Rescher, N. 1959. On the epistemology of the inexact sciences, Management Science, 6:25-52.
Hempel, C. G. 1965. Aspects of Scientific Explanation. New York: Free Press.
Herskovits, Melville . 1948. Man and His Works. New York: Knopf.
Hiller, L. , and R. Baker . 1962. Computer music, in H. Borko , ed., Computer Applications in the Behavioral Sciences. Englewood Cliffs: Prentice-Hall.
Hirt, Michael , ed. 1962. Rorschach Science. New York: Free Press.
Hoggatt, A. C. , and F. E. Balderston , eds. 1963. Symposium on Simulation Models. Cincinnati: Southwestern Publishing Co.
Holland, Edward . 1963. Experiments on a Simulated Underdeveloped Economy. Cambridge: M.I.T. Press.
Holmberg, Allan . 1950. Nomads of the Long Bow. Washington, D.C.: Smithsonian Institute of Social Anthropology.
Homans, George C. 1950. The Human Group. New York: Harcourt, Brace.
332 Homans, George C. 1961. Social Behavior: Its Elementary Forms. New York: Harcourt Brace.

Homans, George C. 1962. Sentiments and Activities. New York: Free Press.
Homans, George C. , and D. Schneider . 1954. Marriage, Authority, and Final Causes. Glencoe: Free Press.
Hook, Sidney . 1940. Reason, Social Myths, and Democracy. New York: Day.
Hook, Sidney , ed. 1959. Psychoanalysis, Scientific Method, and Philosophy. New York: N.Y.U. Press.
Hopkins, T. , and I. Wallerstein . 1967. The comparative study of national societies, Social Science Information, 6/5:25-58.
Horton, D. 1943. The functions of alcohol in primitive societies, Quarterly Journal of Studies in Alcohol.
Reprinted in C. S. Ford , ed., 1967. Cross-cultural Approaches. New Haven: HRAF Press.
Horvath, William J. 1965. A mathematical model of participation in small group discussions, Behavioral Science, 10:164-166.
Huff, David . 1965. The use of gravity models in social research, in F. Massarik and P. Ratoosh , ed., Mathematical Explorations in Behavioral Science. Homewood: Irwin-Dorsey.
Hughes, Charles C. 1966. Social control among the St. Lawrence Island Eskimos, in Schwartz , Turner , and Tuden , eds., Political Anthropology. Chicago: Aldine.
Hull, Clark , et al. 1940. Mathematico-deductive Theory of Rote Learning. New Haven: Yale University Press.
Hull, Clark . 1943. Principles of Behavior. New York: Appleton-Century.
Hull, Clark . 1952. A Behavior System. New Haven: Yale University Press.
Hunter, Floyd . 1959. Top Leadership USA. Chapel Hill: University of North Carolina Press.
Hurwicz, Leonid . 1963. Mathematics in Economics: language and instrument, in James Charlesworth , ed., Mathematics and the Social Sciences. Philadelphia: American Academy of Political and Social Science.
Jacob, Philip , and J. V. Toscano , eds. 1964. The Integration of Political Communities. Philadelphia: Lippincott.
Janowitz, Morris , and D. Segal . 1967. Social cleavage and party affiliation, American Journal of Sociology, 72:601-618.
Jaques, E. 1952. The Changing Culture of a Factory. New York: Dryden.
Jarvie, I. C. 1964. The Revolution in Anthropology. London: Routledge and Kegan Paul.
Kaplan, Abraham . 1964. The Conduct of Inquiry. San Francisco: Chandler.
Kaplan, Bert . 1968 (1957). Personality and social structure, in Robert Manners and D. Kaplan , eds., Theory in Anthropology. Chicago: Aldine.
Kaplan, Harold . 1967. Urban Political Systems: A Functional Analysis of Metro Toronto. New York: Columbia University Press.
Kaplan, Morton . 1957. Balance of power, bipolarity, and other models of international systems, American Political Science Review, 51:684-695.
Kapp, K. W. 1961. Toward a Science of Man in Society. The Hague: Nijhoff.
Kardiner, Abram . 1939. The Individual and His Society. New York: Columbia University Press.
Kardiner, Abram , and L. Ovesey . 1951. The Mark of Oppression. New York: Norton.
Karsh, Bernard . 1958. Diary of a Strike. Urbana: University of Illinois Press.
Kelley, H.H. 1965. Experimental studies of threats in interpersonal negotiations, Conflict Resolution, 9:79-105.
Kelley, H.H. 1966. A classroom study of the dilemmas in interpersonal negotiations, in K. Archibald , ed., Strategic Interaction and Conflict. Berkeley: Institute of International Studies.
333 Kelley, H. H. , and Arrowood, A. J. 1960. Coalitions in the triad: critique and experiments, Sociometry, 23:231-244.
Kelly, George A. 1955. The Psychology of Personal Constructs. New York: Norton.
Kelman, Herbert C. 1965. Manipulation of human behavior: an ethical dilemma for the social scientist, Journal of Social Issues, XXI, 2:31-46.
Kemeny, John , J. L. Snell , and G. Thompson . 1957. Introduction to Finite Mathematics. Englewood Cliffs: Prentice-Hall.
Kemeny, John , and J. L. Snell . 1962. Mathematical Models in the Social Sciences. Boston: Ginn.
Kendall, P. , and P. Lazarsfeld . 1950. Problems of survey analysis, in R. Merton and P. Lazarsfeld , eds., Continuities in Social Research. Glencoe: Free Press.
Kerr, C. , J. Dunlop , F. Harbison , and C. Myers . 1960. Industrialism and Industrial Man. Cambridge: Harvard University Press.
Keynes, John M. 1936. General Theory of Employment, Interest, and Money. New York: Harcourt, Brace.
Kluckhohn, Clyde . 1944. Navaho Witchcraft. Cambridge: Harvard University Press (Beacon Press reprint).
Kluckhohn, Clyde . 1949. The Philosophy of the Navaho Indians, in F. Northrop , ed., Ideological Differences and World Order. New Haven: Yale University Press.
Kluckhohn, Clyde . 1967 (1939). On certain recent applications of association coefficients to ethnological data, American Anthropologist, 41:345-377.
Reprinted in Clellan S. Ford , ed., Cross-Cultural Approaches. New Haven: HRAF Press.
Kluckhohn, Clyde , and D. Leighton . 1946. The Navaho. Cambridge: Harvard.
Knopf, Irwin . 1956. Rorschach summary scores in differential diagnosis, Journal of Consulting Psychology 20:99-104. Reprinted in M. Hirt, ed., Rorschach Science.
Kbben, Andre . 1961. New ways of presenting an old idea: the statistical method in social anthropology, in Frank Moore , ed., Readings in Cross-cultural Methodology. New Haven: HRAF Press.
Kosok, Michael . 1966. The formalization of Hegels dialectical logic, International Philosophical Quarterly, 6:596-631.
Kroeber, Alfred . 1948. Anthropology. New York: Harcourt, Brace.
Krasnow, H. S. , and R. A. Merikallio . 1964. The Past, Present, and Future of General Simulation Languages, Management Science, XI, 2:236-267.
Krupp, Sherman . 1963. Theoretical explanation and the nature of the firm, Western Economic Journal, 1:191-204.

Kuriloff, Arthur , and S. Atkins . 1966. T group for a work team, Journal of Applied Behavioral Science, 2:63-94.
Ladd, John . 1957. The Structure of a Moral Code. Cambridge: Harvard University Press.
Langner, Thomas , and S. T. Michael . 1963. Life Stress and Mental Health. Midtown Manhattan Study, vol. 2. New York: Free Press.
Lave, Lester . 1965. Factors affecting co-operation in the Prisoners Dilemma, Behavioral Science, X, 1:26-38.
Lawrence, W. E. , and G. P. Murdock . 1949. Murngin social organization, American Anthropologist, 51:58-65.
Lazarsfeld, Paul . 1959. Problems in methodology, in R. Merton et al., eds., Sociology Today. New York: Basic Books.
Leach, E. R. 1951. The structural implications of matrilateral cross-cousin marriage, Journal of the Royal Anthropological Institute, 81:23-56.
Leach, E. R. 1954. Political Systems of Highland Burma. Boston: Beacon Press.
Leach, E. R. 1957. The epistemological background of Malinowskis empiricism, in R. Firth , ed., Man and Culture. London: Routledge and Kegan Paul.
334 Leach, E. R. 1965. The nature of war, Disarmament and Arms Control, 3:165-183.
Leighton, Alexander . 1959. My Name is Legion. New York: Basic Books.
Leighton, Alexander , et al. 1960. People of Cove and Woodlot. New York: Basic Books.
Leighton, Alexander , et al. 1963. The Character of Danger. New York: Basic Books.
Lerner, Abba . 1965. Professor Samuelson on theory and realism: a comment, American Economic Review. 55:1153-1155.
Lesser, Alexander . 1933. The Pawnee Ghost Dance Hand Game. Columbia University Contributions to Anthropology, New York: Columbia University Press.
Levinson, D. J. , et al. 1966. Intraception: evolution of a concept, in G. Direnzo , ed., Concepts, Theory, and Explanation in the Behavioral Sciences. New York: Random House.
Levy, Marion . 1952. The Structure of Society. Princeton: Princeton University Press.
Levi-Strauss, C. 1963. Structural Anthropology. New York: Basic Books.
Lewin, Kurt . 1936. Dynamic Theory of Personality. New York: McGraw-Hill.
Lewin, Kurt . 1948. Resolving Social Conflicts. New York: Harper.
Lewin, Kurt . 1951. Field Theory in Social Science. New York: Harper.
Lieberman, Bernhardt . 1962. Experimental studies of conflict in some two-person and three-person games, in Joan Criswell et al., eds., Mathematical Methods in Small-group Processes. Stanford: Stanford University Press.
Lieberman, Bernhardt . 1964. i-trust: a notion of trust in three-person games and international affairs, Conflict Resolution, 8:271-280.
Lindesmith, Alfred . 1968. Addiction and Opiates. Chicago: Aldine.
Lindzey, Gardner . 1961. Projective Techniques and Cross-Cultural Research. New York: Appleton-Century-Crofts.
Linton, Ralph , ed. 1940. Acculturation in Seven Indian Tribes. New York: Appleton-Century-Crofts.
Llewellyn, Karl , and E. A. Hoebel . 1941. The Cheyenne Way. Norman: University of Oklahoma Press.
Lockwood, David . 1964. Social integration and system integration, in George Zollschan and Walter Hirsch , eds., Explorations in Social Change. Boston: Houghton Mifflin.
Lord, Edith . 1950. Experimentally induced variations in Rorschach performance, Psychological Monographs, 64, no. 316. Reprinted in Hirt, 1962.
Luce, R. D. 1959. Individual Choice Behavior. New York: Wiley.
Luce, R. D. , and Howard Raiffa . 1957. Games and Decisions. New York: Wiley.
Lumsden, Malvern . 1966. Perception and information in strategic thinking, Journal of Peace Research, 251-274.
Macesich, G. 1961. Current inflation theory: considerations on methodology, Social Research, 28:321-330.
Machlup, F. 1964. Professor Samuelson on theory and realism, American Economic Review, 54:733-736.
Machlup, F. 1967. Theories of the firm: marginalist, behavioral, managerial, American Economic Review, 57:1-33.
Malinowski, B. 1922. Argonauts of the Western Pacific. London: Routledge.
Malinowski, B. 1926. Crime and Custom in Savage Society. New York: Humanities Press.
Malinowski, B. 1927. Sex and Repression in Savage Society. London: Routledge.
Malinowski, B. 1944. A Scientific Theory of Culture and Other Essays. Chapel Hill: University of North Carolina Press.
Malinowski, B. 1945. The Dynamics of Culture Change. New Haven: Yale University Press.
Malinowski, B. 1954 (1948). Magic, Science, and Religion. Garden City: Doubleday 335Anchor Books (reprint).
Mandelbaum, David . 1954. Form, variation, and meaning of a ceremony, in R. Spencer , ed., Method and Perspective in Anthropology. Minneapolis: Minnesota University Press.
Manners, Robert . 1968 (1956). Functionalism, realpolitik, and anthropology in underdeveloped areas, in R. Manners and D. Kaplan , eds., Theory in Anthropology. Chicago: Aldine.
Mannheim, Karl . 1936. Ideology and Utopia. New York: Harcourt, Brace reprint HB3.
Maquet, Jacques . 1964. Objectivity in Anthropology, Current Anthropology, 5:47-55.
March, J. , and H. Simon . 1958. Organizations. New York: Wiley.
Marcuse, Herbert . 1966. Eros and Civilization; a Philosophical Inquiry into Freud. Boston: Beacon Press.
Marsh, Robert M. 1965. Comparative sociology, 1950-1963, Current Sociology, vol. 14, no. 2.
Martindale, Don . 1959. Sociological theory and the ideal type, in L. Gross , ed., Symposium on Sociological Theory. Evanston: Row, Peterson.
Martindale, Don . 1960. The Nature and Types of Sociological Theory. Boston: Houghton Mifflin.
Masling, Joseph . 1960. The influence of situational and interpersonal variables in projective testing, Psychological Bulletin, 57:65-85.
Massey, Gerald . 1965. Professor Samuelson on theory and realism: a comment, American Economic Review, 55:1155-1163.

McKeon, Richard . 1951. Philosophy and method, Journal of Philosophy, 48:653-682.
McKeon, Richard . 1952. Freedom and History. New York: Noonday.
McPhee, William . 1963. Formal Theories of Mass Behavior. New York: Free Press.
Mead, George H. 1934. Mind, Self, and Society. Chicago, University of Chicago Press.
Mead, Margaret , ed. 1955. Cultural Patterns and Technical Change. New York: New American Library.
Mead, Margaret . 1956. New Lives For Old. New York: Morrow.
Meehl, Paul . 1950. On the circularity of the law of effect, Psychological Bulletin, 47:52-75.
Meehl, Paul . 1954. Clinical vs. Statistical Prediction. Minneapolis: University of Minnesota Press.
Melitz, J. 1965. Friedman and Machlup on the significance of testing economic assumptions, Journal of Political Economy. 73:37-60.
Merton, Robert . 1949. Social Theory and Social Structure. Glencoe: Free Press.
Merton, Robert . 1965. On the Shoulders of Giants. New York: Free Press.
Messick, David . 1967. Interdependent decision strategies in zero-sum games: a computer-controlled study, Behavioral Science, 12:33-48.
Miller, George A. 1964. Mathematics and Psychology. New York: Wiley.
Miller, Neal . 1959. Liberalization of basic S-R concepts, in Sigmund Koch , ed., Psychology, vol. 2, pp. 196-292. New York: McGraw-Hill.
Miller, S. M. 1952. The participant-observer and over-rapport, American Sociological Review, 17:97-99.
Mills, C. Wright . 1951. White Collar. New York: Oxford University Press.
Milner, P. M. 1957. The cell assembly, Mark 2, Psychological Review, 64:242-252.
Milner, P. M. 1961. A neural mechanism for immediate recall of sequences, Kybernetik, 1:76-81.
Mischel, T. 1964. Personal constructs, rules, and the logic of clinical activity, 336 Psychological Review, 71:180-192.
Mischel, T. 1966. Pragmatic aspects of explanation, Philosophy of Science, 33: 40-60.
Mitchell, J. C. 1966. Theoretical orientations in African urban studies, in M. Banton , ed., The Social Anthropology of Complex Societies, ASA Monograph, #4. London: Tavistock.
Mitchell, William . 1962. The American Polity. Glencoe: Free Press.
Mitroff, Ian . 1967. A study of simulation-aided engineering design. Working Paper No. 66, Space Sciences Laboratory, University of California, Berkeley, California.
Mitroff, Ian . 1969. Fundamental issues in the simulation of human behavior, Management Science, 15:B-635-649.
Mooney, James . 1896. The Ghost Dance Religion and the Sioux Outbreak of 1890. Bureau of Ethnology, report 14, part 2, 1892-3. Washington: Government Printing Office.
Moore, Barrington . 1958. Political Power and Social Theory. New York: Harper Torchbooks.
Moore, Frank W. , ed. 1961. Readings in Cross-cultural Methodology. New Haven: HRAF Press.
Murdock, George . 1949. Social Structure. New York: Macmillan.
Murphey, Rhoads . 1957. New capitals of Asia, Economic Development and Cultural Change, 5:216-243.
Murphy, Robert . 1967. Tuareg kinship, American Anthropologist, 69:163-170.
Murray, Henry A. 1938. Explorations in Personality. New York: Oxford.
Myrdal, Gunnar . 1944. An American Dilemma. New York: Harper.
Nadel, S. F. 1935. Nupe witchcraft and anti-witchcraft, Africa, 8:423-447.
Nadel, S. F. 1942. Black Byzantium. London: Oxford University Press.
Nadel, S. F. 1951. The Foundations of Social Anthropology. Glencoe: Free Press.
Nadel, S. F. 1952. Witchcraft in Four African Societies, American Anthropologist, 54:18-29.
Reprinted in C. S. Ford , ed., Cross-cultural Approaches, New Haven: HRAF Press, 1967.
Nagel, Ernest . 1961. The Structure of Science. New York: Harcourt, Brace and World.
Nagel, Ernest . 1963. Assumptions in economic theory, American Economic Review, 53:211-220.
Naroll, Raoul . 1962. Data Quality Control. New York: Free Press.
Naroll, Raoul . 1968. Some thoughts on comparative method in cultural anthropology, in Hubert M. and Ann B. Blalock , eds., Methodology in Social Research, ch. 7. New York: McGraw-Hill.
Nash, Manning . 1955. Machine Age Maya. American Anthropological Association Memoir no. 87. Menasha: American Anthropological Association.
Needham, Rodney . 1954. Siriono and Penan: a test of some hypotheses, Southwestern Journal of Anthropology, 10:228-232.
Needham, Rodney . 1962. Structure and Sentiment. Chicago: University of Chicago Press.
Newell, Alan . 1962. Some problems of basic organization in problem-solving programs, in M. Yovits et al., eds., Self-Organizing Systems, pp. 393-423. Washington: Spartan Press.
Newell, Alan , and H. Simon . 1963. GPS, a program that simulates human thought, in E. Feigenbaum and J. Feldman , eds., Computers and Thought. New York: McGraw-Hill.
Olson, Mancur . 1965. The Logic of Collective Action. Cambridge: Harvard University Press.
Orcutt, Guy , et al. 1961. Microanalysis of Economic Systems. New York: Harper.
337 Orcutt, Guy . 1963. Views on simulation and models of social systems, in Hoggatt and Balderston , eds., Symposium on Simulation Models, pp. 221-236. Cincinnati: Southwestern Publishing Co.
Osgood, C. 1953. Method and Theory in Experimental Psychology. New York: Oxford.
Osgood, C. 1958. Behavior theory, in Roland Young , ed., Approaches to the Study of Politics. Evanston: Northwestern University Press.
Oskamp, S. , and D. Perlman . 1965. Factors affecting co-operation in a Prisoners Dilemma game, Conflict Resolution, 9:359-374.
Parsons, Talcott . 1937. The Structure of Social Action. New York: McGraw-Hill.

Parsons, Talcott . 1949. Essays in Sociological Theory. Glencoe: Free Press.
Parsons, Talcott . 1951. The Social System. Glencoe: Free Press.
Parsons, Talcott , et al. 1955. Family, Socialization and Interaction Process. Glencoe: Free Press.
Parsons, Talcott . 1960. Structure and Process in Modern Societies. Glencoe: Free Press.
Parsons, Talcott . 1964. Social Structure and Personality. New York: Free Press.
Parsons, Talcott . 1966a. Societies: Evolutionary and Comparative Perspectives. Englewood Cliffs: Prentice-Hall.
Parsons, Talcott . 1966b. The political aspect of social structure and process, in D. Easton , ed., Varieties of Political Theory. Englewood Cliffs: Prentice-Hall.
Parsons, Talcott . 1967. Sociological Theory and Modern Society. New York: Free Press.
Parsons, Talcott . 1969. Politics and Social Structure. New York: Free Press.
Parsons, Talcott , R. F. Bales , and E. Shils . 1953. Working Papers in the Theory of Action. Glencoe: Free Press.
Parsons, Talcott , and Edward Shils , eds. 1951. Toward a General Theory of Action. Cambridge: Harvard University Press.
Parsons, Talcott , and N. Smelser . 1956. Economy and Society. New York: Free Press.
Peattie, Lisa . 1965. Anthropology and the search for values, Journal of Applied Behavior Science, 1:361-372.
Peattie, Lisa . 1968. The View from the Barrio. Ann Arbor: University of Michigan Press.
Pepper, Stephen . 1942. World Hypotheses. Berkeley. University of California Press.
Peters, R. S. 1960. The Concept of Motivation. London: Routledge.
Polanyi, Karl , ed. 1957. Trade and Market in the Early Empires. Glencoe: Free Press.
Pollis, A. , and B. Koslin . 1962. On the scientific foundations of marginalism, American Journal of Economics and Sociology, 21:113-129.
Polsky, Ned . 1967. Hustlers, Beats, and Others. Chicago: Aldine.
Popper, Karl . 1957. The Poverty of Historicism. Boston: Beacon Press.
Pruitt, Dean . 1967. Reaction systems and instability in interpersonal and international affairs, ONR Technical Report No. 2.
Radcliffe-Brown, A. R. 1922. The Andaman Islanders. Glencoe: Free Press. Reprinted 1948.
Radcliffe-Brown, A. R. 1935. Kinship terminology in California, American Anthropologist, 37:530-535.
Radcliffe-Brown, A. R. 1949. Functionalism: a protest, American Anthropologist, 51:320-323.
Radcliffe-Brown, A. R. 1951a. The comparative method in social anthropology, Journal of The Royal Anthropological Institute, 81:15-22.
Radcliffe-Brown, A. R. 1951b. Murngin social organization, American Anthropologist, 53: 37-55.
Radcliffe-Brown, A. R. 1952. Structure and Function in Primitive Society. Glencoe: Free Press.
Radcliffe-Brown, A. R. 1957. A Natural Science of Society. Glencoe: Free Press.
Radlow, R. 1965. An experimental study of cooperation in the Prisoners 338Dilemma game, Conflict Resolution, 9:221-227.
Radnitzky, G. 1969. Ways of looking at science, Scientia, 104:49-57.
Rank, Otto . 1947. Will Therapy. New York: Knopf.
Rapoport, Amnon . 1967. Optimal policies for the Prisoners Dilemma, Psychological Review, 74:136-148.
Rapoport, Anatol . 1957. Lewis Richardsons mathematical theory of war, Journal of Conflict Resolution, 1:249-299.
Rapoport, Anatol . 1963. Mathematical models of social interaction, in R. D. Luce , ed., Handbook of Mathematical Psychology, vol. 2, pp. 493-580. New York: Wiley.
Rapoport, Anatol . 1964a. Review of Computers and Thought, Management Science, 11: 203-210.
Rapoport, Anatol . 1964b. Strategy and Conscience. New York: Harper.
Rapoport, Anatol . 1966a. Strategic and non-strategic approaches to problems of security and peace, in K. Archibald , ed., Strategic Interaction and Conflict. Berkeley: Institute of International Studies.
Rapoport, Anatol . 1966b. Some system approaches to political theory, in D. Easton , ed., Varieties of Political Theory. Englewood Cliffs: Prentice-Hall.
Rapoport, Anatol . 1967. Exploiter, leader, hero, and martyr: the four archetypes of the 22 game, Behavioral Science, 12:81-84.
Rapoport, Anatol . 1968. Editorial comment. Journal of Conflict Resolution, 12:222-223.
Rapoport, Anatol , and A. H. Chammah . 1965. Prisoners Dilemma. Ann Arbor: University of Michigan Press.
Rapoport, Anatol , and M. Guyer , 1966. A taxonomy of 2 X 2 games, in L.V. Bertalanffy and A. Rapoport , eds., General Systems, vol. 11.
Rashevsky, Nicholas . 1951. Mathematical Biology of Social Behavior. Chicago: University of Chicago Press.
Redfield, Robert , 1941. The Folk Culture of Yucatan. Chicago: University of Chicago Press.
Redfield, Robert . 1947. The folk society, in American Journal of Sociology, 52:293-308.
Redfield, Robert . 1950. A Village that Chose Progress. Chicago: University of Chicago Press.
Redfield, Robert . 1953. The Primitive World and Its Transformations. Ithaca: Cornell University Press.
Redfield, Robert . 1960a (1955). The Little Community. Chicago: University of Chicago Press.
Redfield, Robert . 1960b (1956). Peasant Society and Culture. Chicago: University of Chicago Press.
Redfield, Robert . 1962. Human Nature and the Study of Society. Collected papers, vol. 1, M. P. Redfield , ed. Chicago: University of Chicago Press.
Redfield, Robert , and A. Villa Rojas . 1934. Chan Kom. Chicago: University of Chicago Press.
Reid, Russell . 1967. Marriage systems and algebraic group theory, American Anthropologist, 69:171-178.
Reik, Theodor . 1949. Listening with the Third Ear. New York: Farrar, Straus.
Richards, Audrey . 1939. Land, Labor, and Diet in Northern Rhodesia. Oxford: Oxford University Press.
Riesman, David , and J. Watson . 1964. The sociability project, in Phillip Hammond , ed., Sociologists at Work. New York: Basic Books.

Riker, William . 1962. The Theory of Political Coalitions. New Haven: Yale University Press.
Riker, William . 1967. Experimental verification of two theories about n-person games, in Joseph Bernd , ed., Mathematical Applications in Political Science III. Charlottesville: University Press of Virginia.
Rochester, N. , et al. 1956. Tests on a cell assembly theory of the action of the 339brain, in I.R.E. Transactions on Information Theory, vol. IT-2, 3:80-93.
Rogers, Carl . 1961. On Becoming a Person. New York: Houghton Mifflin.
Rokeach, Milton , and C. Hanley . 1956. Eysencks tender-mindedness dimension: a critique, reply by Eysenck and rejoinder by Hanley and Rokeach, Psychological Bulletin, 53:169-186.
Rome, Sydney and Beatrice . 1962. Computer simulation toward a theory of large organizations, in H. Borko , ed., Computer Applications in the Behavioral Sciences. Englewood Cliffs: Prentice-Hall.
Rosenzweig, Saul . 1949. Psychodiagnosis. New York: Grune and Stratton.
Rosenzweig, Saul . 1954. A transvaluation of psychotherapya reply to Hans Eysenck, Journal of Abnormal and Social Psychology, 49:298-304.
Ross, Nathaniel , and S. Abrams . 1965. Fundamentals of psychoanalytic theory, in B. Wolman , ed., Handbook of Clinical Psychology, ch. 14. New York: McGraw-Hill.
Rotter, Julian . 1954. Social Learning and Clinical Psychology. New York: Prentice-Hall.
Rotwein, E. 1959. On The Methodology of Positive Economics, Quarterly Journal of Economics, 73:554-575.
Saaty, Thomas . 1968. Mathematical Models of Arms Control and Disarmament. New York: Wiley.
Sacksteder, William . 1963a. Diversity in the behavioral sciences, Philosophy of Science, 30:375-395.
Sacksteder, William . 1963b. Structural variation in science, Synthese, 15:412-423.
Sacksteder, William . 1964. Inference and philosophic typologies, The Monist, 48:567-601.
Sampson, E. , and M. Kardush . 1965. Age, sex, class, and race differences in response to a two-person nonzerosum game, Conflict Resolution, 9:212-220.
Samuels, Ina . 1959. Reticular mechanisms and behavior, Psychological Bulletin, 56:1-25.
Samuelson, Paul A. 1963. Discussion, American Economic Review, 53:231-236.
Samuelson, Paul A. 1964. Theory and realism: a reply, American Economic Review, 54:736-739.
Samuelson, Paul A. 1966. Collected Scientific Papers. Cambridge: M.I.T. Press.
Sanford, Nevitt . 1966. Self and Society. New York: Atherton.
Sayre, Kenneth . 1965. Recognition. Indiana: University of Notre Dame Press.
Schachtel, Ernst . 1966. Experiential Foundations of Rorschachs Test. New York: Basic Books.
Schafer, Roy . 1954. Psychoanalytic Interpretation in Rorschach Testing. New York: Grune and Stratton.
Schapera, Isaac . 1953. Some comments on comparative method in social anthropology, American Anthropologist, 55:353-362.
Reprinted in C. S. Ford , ed., Cross-cultural Approaches. New Haven: HRAF Press, 1967.
Schneider, Louis . 1964. Toward assessment of Sorokins view of change, in George Zollschan and Walter Hirsch , eds., Explorations in Social Change. Boston: Houghton Mifflin.
Schubert, Glendon . 1959. Quantitative Analysis of Judicial Behavior. Glencoe: Free Press.
Schwab, Joseph . 1960. What do scientists do? Behavioral Science, 5:1-27
Schwab, William B. 1960. An experiment in methodology in a West African urban community, in Richard Adams and J. Preiss , eds., Human Organization Research. Homewood: Dorsey.
Schwartz, Jacob . 1961. Lectures on the Mathematical Method in Analytical Economics. New York: Gordon and Breach.
Sears, R. R. 1943. Survey of Objective Studies of Psychoanalytic Concepts. New York: Social Science Research Council.
340 Shneidman, Edwin . 1965. Projective techniques, in B. Wolman , ed., Handbook of Clinical Psychology. New York: McGraw-Hill.
Shubik, Martin . 1959. Strategy and Market Structure. New York: Wiley.
Shubik, Martin . 1963. Some reflections on the design of game theoretic models for the study of negotiation and threats, Journal of Conflict Resolution, 7:1-12.
Shubik, Martin . 1964. Game Theory and Related Approaches to Behavior. New York: Wiley.
Shubik, Martin . 1968. On the study of disarmament and escalation, Journal of Conflict Resolution, 12:83-101.
Siegel, B. , ed. 1963. Biennial Review of Anthropology. Stanford: Stanford University Press.
Siegel, Sidney , and L. E. Fouraker . 1960. Bargaining and Group Decisionmaking. New York: McGraw-Hill.
Simon, Herbert . 1954. The construction of social science models, in Paul Lazarsfeld , ed. Mathematical Thinking in the Social Sciences. Glencoe: Free Press.
Simon, Herbert . 1957. Models of Man. New York: Wiley.
Simon, Herbert . 1963. Discussion, American Economic Review, 53:229-231.
Simon, Herbert , and A. Newell . 1956. Models, their uses and limitations, in L. D. White , ed., The State of the Social Sciences. Chicago: University of Chicago Press.
Skinner, B. F. 1938. The Behavior of Organisms. New York: Appleton-Century.
Skinner, B. F. 1959a. Cumulative Record. New York: Appleton-Century.
Skinner, B. F. 1959b. A case study in scientific method, in Sigmund Koch , ed., Psychology, vol. 2, New York: McGraw-Hill.
Skinner, B. F. 1966. Contingencies of reinforcement in the design of a culture, Behavioral Science, 11:159-166.
Smelser, Neil . 1963. Sociology of Economic Life. Englewood Cliffs: Prentice-Hall.
Spence, Donald P. 1968. The processing of meaning in psychotherapy, Behavioral Science, 13:349-361.
Spicer, Edward H. 1940. Pascua, a Yaqui Village in Arizona. Chicago: University of Chicago Press.
Spicer, Edward H. 1954. Potam, a Yaqui Village in Sonora. American Anthropological Association memoir no. 77. Menasha: Banta.

Spicer, Edward H. , ed. 1952. Human Problems in Technological Change. New York: Russell Sage.
Stallknecht, Newton , and R. Brumbaugh . 1954. The Compass of Philosophy. New York: Longmans, Green.
Stephan, F. , and E. G. Mishler . 1952. The distribution of participation in small groups, American Sociological Review, 17:598-608.
Stephens, William N. 1968. Hypotheses and Evidence. New York: Crowell.
Steward, Julian . 1953. Evolution and process, in A. Kroeber , ed., Anthropology Today. Chicago: University of Chicago Press.
Stieper, Donald , and D. Wiener . 1965. Dimensions of Psychotherapy: An Experimental and Clinical Approach. Chicago: Aldine.
Stogdill, Ralph . 1959. Individual Behavior and Group Achievement. New York: Oxford.
Suppes, Patrick , and R. C. Atkinson . 1960. Markov Learning Models for Multi-person Interactions. Stanford: Stanford University Press.
Swartz, Marc , ed. 1968. Local-Level Politics. Chicago: Aldine.
Swartz, Marc , et al., eds. 1966. Political Anthropology. Chicago: Aldine.
Taylor, Charles . 1964. The Explanation of Behavior. New York: Humanities.
Thibaut, J. , and H. H. Kelley . 1959. The Social Psychology of Groups. New York: Wiley.
Thomas, W. I. , and F. Znaniecki . 1958 (1920). The Polish Peasant in Europe and 341America. New York: Dover.
Tolman, E. C. 1951. A psychological model, in Talcott Parsons and E. Shils , eds., Toward a General Theory of Action. Cambridge: Harvard University Press.
Tomkins, Silvan , and S. Messick , eds. 1963. Computer Simulation of Personality. New York: Wiley.
Toulmin, Stephen . 1961. Foresight and Understanding. Bloomington: Indiana University Press.
Tullock, Gordon . 1967. Toward a Mathematics of Politics. Ann Arbor: University of Michigan Press.
Turner, V. W. 1957. Schism and Continuity in an African Society. Manchester: Manchester University Press.
Turner, V. W. 1964. Symbols in Ndembu Ritual, in Max Gluckman , ed., Closed Societies and Open Minds. Chicago: Aldine.
Turner, V. W. 1969. The Ritual Process: Structure and Anti-Structure. Chicago: Aldine.
Tustin, A. 1953. The Mechanism of Economic Systems. Cambridge: Harvard University Press.
Udy, Stanley . 1959. Organization of Work. New Haven: HRAF Press.
Uesugi, T. K. , and W. E. Vinacke . 1963. Strategy in a feminine game, Sociometry, 26:75-88.
Vidich, Arthur , and Joseph Bensman . 1958. Small Town in Mass Society. Garden City: Doubleday.
Vidich, Arthur , and Joseph Bensman . 1960. The validity of field data, in Richard Adams and Jack Preiss , eds., Human Organization Research. Homewood: Dorsey.
Vinacke, W. E. 1959. Sex roles in the three-person game, Sociometry, 22:343-360.
Vinacke, W. E. , et al. 1966. The effect of information about strategy on a three-person game, Behavioral Science, 11:180-189.
Vinacke, W. E. , and A. Arkoff . 1957. An experimental study of coalitions in the triad, American Sociological Review, 22:406-414.
Vogt, Evon , and E. Albert , eds. 1966. People of Rimrock: A Study of Values in Five Cultures. Cambridge: Harvard University Press.
Von Mises, L. 1960. Epistemological Problems of Economics. Princeton: Van Nostrand.
Walker, Marshall . 1963. The Nature of Scientific Thought. Englewood Cliffs: Prentice-Hall.
Wallerstein, Immanuel . 1966. Social Change: The Colonial Situation. New York. Wiley.
Warner, W. L. 1941. The Social Life of a Modern Community. New Haven: Yale University Press.
Warner, W. L. , and Paul Lunt . 1942. Status System of a Modern Community. New Haven: Yale University Press.
Warner, W. L. , and Leo Srole . 1945. Social Systems of American Ethnic Groups. New Haven: Yale University Press.
Warner, W. L. , and J. O. Low . 1947. Social System of the Modern Factory. New Haven: Yale University Press.
Washburne, Chandler . 1961. Primitive Drinking. New York: College and University Press.
Wax, Rosalie . 1960. Reciprocity in field work, in Richard Adams and Jack Preiss , eds., Human Organization Research. Homewood: Dorsey.
Webb, Eugene , et al. 1966. Unobtrusive Measures: Nonreactive Research in the Social Sciences. Chicago: Rand, McNally.
Weil, R. L. 1966. The N-person Prisoners Dilemma: some theory and a computer-oriented approach, Behavioral Science, 11:227-233.
Weiss, Robert . 1956. Processes of Organization. Ann Arbor: Survey Research 342Center, University of Michigan.
White, Harrison . 1963a. An Anatomy of Kinship. Engelwood Cliffs: Prentice-Hall.
White, Harrison . 1963b. Review of Mathematical Methods in Small Group Processes , American Journal of Sociology, 64:304-306.
Whitehead, A. N. 1941. Mathematics and the Good, in P. Schilpp , ed., The Philosophy of Alfred North Whitehead. Evanston: Northwestern University Press.
Whiting, John , and I. Child . 1953. Child Training and Personality. New Haven: Yale University.
Whyte, W. F. 1943. Street Corner Society. Chicago: University of Chicago Press.
Winch, Peter . 1958. The Idea of a Social Science. London: Routledge.
Wittenborn, J. R. 1949. Statistical tests of certain Rorschach assumptions: Analyses of discrete responses, Journal of Consulting Psychology, 13:257-267. Reprinted in M. Hirt, 1962.
Wittenborn, J. R. 1950a. Statistical tests of certain Rorschach assumptions: the internal consistency of scoring categories, Journal of Consulting Psychology, 14:10-19. Reprinted in M. Hirt, 1962.
Wittenborn, J. R. 1950b. A factor analysis of Rorschach scoring categories, Journal of Consulting Psychology, 14:261-267.

Wittenborn, J. R. , and F. A. Mettler . 1951. A lack of perceptual control score for the Rorschach test, Journal of Clinical Psychology, 7:331-334.
Wittfogel, Karl . 1957. Oriental Despotism. New Haven: Yale University Press.
Wolman, Benjamin . 1965. Schizophrenia and related disorders, in B. Wolman , ed., Handbook of Clinical Psychology. New York: McGraw-Hill.
Yamane, Taro . 1962. Mathematics For Economists. Englewood Cliffs: Prentice-Hall.
Zelditch, Morris . 1955. Role differentiation in the nuclear family: a comparative study, in Parsons , et al., Family, Socialization and Interaction Process. Glencoe: Free Press.
Zinnes, Dina . 1968. An introduction to the behavioral approach: a review, Journal of Conflict Resolution, 12:258-267.